

Legislative Professionalism and Government Spending: Do Citizen Legislators Really Spend Less?*

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Abstract. In this paper we consider the effect of legislative professionalism on state government spending. We examine arguments for why citizen legislatures should have systematically different spending patterns than professional ones. Using data from the US states, we find that state government expenditure per capita is significantly lower the less professionalized the state legislature. We conclude that reducing legislative professionalism is one of the instruments citizens may use to contain the growth of government.

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1. Introduction

Explaining the size of government is one of the perennial questions in public economics. While early research focused on the role of voter preferences, income, and the price of publicly provided goods, recent studies have turned to exploring the role of legislative institutions. In this paper, we consider the effect of one such institution, legislative professionalism. Members of professional legislatures receive relatively high pay, have large staffs, and tend to hold no other job outside of their political position. So-called citizen or amateur legislatures have members which have small staffs, receive relatively low compensation, and usually have another, non-political, income source. Citizen legislatures also meet for relatively short sessions. Our goal in this paper is to determine the effect of the professionalization of state legislatures on state government spending. It should be noted at the outset that the term professionalism as we use it refers to *resources*, not capability.

The variation in legislative professionalism of state legislatures in the U.S. provides a natural laboratory for analyzing this issue, which, to date, has not been studied. There is a high degree of variability in the resources which are available to state legislators. For instance, in 1992, state representatives in Indiana had a desk in one of two offices shared by all 100, no personal staff, received an annual salary of \$11,600, and met for one month-long session. Pennsylvania representatives had personal offices, one or two personal staffpersons, received \$47,000 in annual salaries, and met practically all year round (Mooney, 1995).

We should expect citizen legislatures to be different from professional ones for several reasons. The objective measures, such as compensation and staff, should change the incentives of legislators to provide legislation that is favorable to special interests. It is important to note that legislator turnover is much higher in citizen legislatures (Fiorina, 1996; Moncrief et al., 1996). Since we expect logrolling opportunities to be related to tenure, government spending should be different in citizen legislatures due to differing logrolling arrangements. These

The question we pose in this paper is purely positive. However, it is clearly relevant for policy debates. While in the 1960s, reforms were aimed at improving professionalism with the goal of improving policy output, today, much of the dissatisfaction with politics stems from a belief that full-time politicians attend too much to their own interests at the expense of their constituents. The movement for imposing term limits in part rests on the belief that term limits would make legislatures more like citizen legislatures, although Garrett (1996) challenges this hypothesis. However, since the effects of having a citizen legislature are complex and go beyond that of reduced tenure, we draw no conclusions or inferences about the effects term limits would have on government spending. The paper does provide evidence which can be used in assessing the likely effects of reforming legislative institutions to make them more or less professional in nature.

Our findings indicate that government spending in states with citizen legislatures is significantly lower than in states with professional legislatures. By reducing the professionalism of their legislators, citizens, if they so wish, can effectively constrain the size of government. In Section 2 we review the theory and evidence in regards to legislative tenure, interest groups, and government expenditures. Section 3 describes our research methodology and data. Our empirical results are presented in Section 4, and Section 5 concludes.

2. Legislative Professionalism and Government Spending

The relationship between government expenditures and legislative professionalism is, a priori, unclear. Much of what theory predicts depends on the hypothesized relationship between interest groups and legislators.

Interest groups, in addition to supplying campaign funds, provide legislators with information

not have the resources necessary to fully research each bill which comes before them. Citizen legislators, with smaller staffs and less experience, seem, if anything, more likely to depend upon interest groups for information and guidance. In this respect, citizen legislatures are more prone to lobbying influence than are professional legislatures.¹ Although, in general, it would be possible that special interest groups lobby for less spending, the presumption of the public choice literature has been, by and large, that more influence of special interests leads to higher spending. Reed et al. (1998) cite evidence that witnesses before congressional committees almost always favor higher spending. Thus, we would predict an increase in interest group influence to increase government spending. As a result, a citizen legislature's reliance on interest groups for information and research will predispose it to additional spending.

Fiorina (1996) argues that imposing term limits would cause politicians to plan their post-political career right after moving to a public office. Thus, he argues, the power of special interest groups who can provide such jobs would be enhanced. The same kind of argument would hold for citizen legislators; professional politicians who – due to the resources they command – are relatively certain of reelection do not need to cater to special interest groups in order to advance their immediate career. This argument implies professional legislatures should spend less, other things being equal.

Although the above arguments may seem persuasive, they do not dominate the literature. Many believe citizen legislators are less prone to interest group influence. Fiorina (1994) makes the argument that a citizen legislator is much more likely to have a non-political career. This outside income acts to mitigate a citizen legislator's susceptibility to interest group pressures. Conversely, professional legislators have developed an expertise which is of value primarily in the political market; their skills are not highly valued by the private sector. As a result, professional legislators find reelection to be a financial imperative. Thus, interest

groups that support their re-election will be wooed and accommodated. The implication here is that a professional legislature will spend more, *ceteris paribus*.

Reed and Schansberg (1996) argue that legislators need time to identify potential partners for vote trading. Hence, logrolling should be more pervasive the higher the attained tenure of legislators. Thus, any legislative structure which results in increased turnover, such as that experienced by citizen legislatures, should reduce the opportunities for logrolling.² Another reason why lower tenure would lead to less logrolling is that it reduces the repeated game character of legislative relationships. Hence, it would become harder to enforce vote trading agreements which should reduce the prevalence of logrolling.

There is a long literature which ascribes an increase in government expenditures to logrolling (e.g., Tullock, 1959, and Weingast *et al.*, 1981). Again, legislators could in principle strike deals to reduce rather than increase spending. However, if the benefits of public projects are concentrated while the costs are dispersed, higher than optimal spending levels result (Weingast *et al.*, 1981). The implication is that any congressional structure which shortens tenure should result in decreased government spending.

Moreover, since the value of office is higher the higher the compensation, we would predict professional legislators invest more resources to get reelected. That is, we would predict they invest more time and money in pork barrel spending to secure support from their home district. This too would tend to increase the tendency of professional legislatures to have higher spending.³

¹ Fiorina (1996) makes this argument in connection with term limits.

² Moreover, short legislative sessions may prevent citizen legislators from finding many potential logrolling opportunities.

³ The professionalism measure in the empirical section uses annual compensation for legislators as one component. However, high compensation goes together with long sessions: Thus, the real compensation, i.e.,

Another argument is put forward by Reed et al. (1998). They argue that politicians with longer attained tenure become more favorably disposed over time toward government spending, and present some evidence to this effect. The same could be true of professional politicians. First, it might be that political careers attract individuals who are more inclined to be big spenders. Second, Fiorina (1994) makes the argument that since citizen legislators need a non-political income source, these are likely to be wealthy individuals, such as lawyers, who tend to be Republicans. Conversely, for people who are not as wealthy, the income offered to professional politicians tends to be an attractive alternative to other occupations.⁴ Hence, one may conjecture that citizen legislatures tend to be made up of individuals who are less prone to be big spenders than professional legislatures.

Finally, it may simply be that professional politicians are more efficient at their job if there is substantial on-the-job learning. Since higher productivity would lead to a lower price of legislation, this would tend to lower spending given that demand for public goods is inelastic. However, higher productivity might also lead to higher spending if career politicians are more efficient at targeting spending towards special interests.

On the other hand, if citizens thought professional legislators are more competent, they might entrust them with more decision making power, which would lead to higher spending. This, however, seems unlikely; public debate seems to indicate that citizens find professional politicians particularly untrustworthy.

In summary, while there are perhaps more arguments for why citizen legislatures should spend less than professional ones, this prediction is not unambiguous. Thus, we must determine empirically whether legislatures whose members have below average tenure, low pay, small staffs, and meet for short sessions really spend less. Following Gilligan and Matsusaka (1995), we investigate the determinants of state-level government expenditures. If the institutional

structure of legislatures matters, we should observe a change in government expenditures as legislative professionalism changes.

3. Empirical Methodology

Basic Model

Suppose the provision of government services were determined by voting with simple majority. Assume that all citizens within a state have identical preferences over public and private goods, and that the costs are shared equally among the population. Then the equilibrium provision level is the preferred level of the voter with median income Y .⁵ We assume that demand is of the constant elasticity form:⁶

$$X = AN^\alpha [tPN^\alpha]^\beta Y^\delta M^\phi e^u, \quad (1)$$

where X is the amount of public services demanded by the median voter as a function of the price of those services, P , which is assumed to be the median voter's tax share, t , times the constant marginal cost of the public good, P . The population size is N . η is the price elasticity of demand and β is the income elasticity. δ is supposed to measure the "publicness" of public services; pure public goods are nonrival in consumption, hence the cost of providing a given level is independent of the population. If this is the case then $\delta = 0$. Conversely, for pure private goods, rivalry means that provision costs and hence individual demand increase with the population. In addition to the demographic variables, we include a set of political variables, M . Under the null hypothesis that political institutions do not matter, we would have $\phi = 0$. u is an error term to be defined below.

⁴ In order to separate out this indirect effect, we control for the partisan composition of legislatures.

⁵ In the empirical section we use mean instead of median income, which would correspond to the so-called traditional or ad hoc approach to estimating government spending. We prefer to use median income here for the presentation, since it allows us to distinguish between direct and representative democracy. Furthermore, if the distribution of income is the same across states, mean and median income are correlated so the distinction is inconsequential.

⁶ This equation is derived by assuming that the amount of public good consumed by the median voter, O , is

Suppose public goods are financed by a head tax, i.e., $t = 1/N$. Letting state expenditure per capita be denoted $G = pX/N$, we get:

$$G = AN^{(\alpha-1)(1+\eta)} P^{1+\eta} Y^\delta M^\phi e^u. \quad (2)$$

Letting lower case letters denote natural logarithms of the variables, we then get our estimation equation:

$$g_{it} = a + (1 + \eta)p_{it} + (\alpha - 1)(1 + \eta)n_{it} + \delta y_{it} + \phi m_{it} + u_{it}, \quad (3)$$

where subscripts it refer to variables for state i at time t .

When estimating equation (3), several concerns need to be addressed. First, since we use panel data on the US states for several years, the error term is assumed to take the form $u_{it} = \mu_i + \lambda_t + \varepsilon_{it}$. Here, μ_i is the unobservable state effect, λ_t the unobservable time effect, and ε_{it} the stochastic disturbance with mean zero. That is, we will estimate an error components model, where the intercept is allowed to vary by state and time period. This method allows us to capture any possible differences in states' latent demand for government. For variables that can take on values of zero or one only, if the variable does not vary over time, using state fixed effects does not provide any additional information. In that case, we will let $u_{it} = \lambda_t + \varepsilon_{it}$. In addition, we use White standard errors to correct for heteroscedasticity.

Variables

Ours is a panel study which considers data for the US states in the years 1964, 1974, 1984, and 1994. (The choice of time periods is dictated by data availability). Alaska is dropped from the sample as an outlier. Nebraska is dropped because of its unicameral, non-partisan legislature; so is Minnesota in 1964 when its legislature was elected on a non-partisan basis, which leaves a total of 191 observations. All financial figures are expressed in constant, 1982-84 dollars per capita, using the consumer price index.

period we consider, mean government spending increased steadily, from a low of about 670 dollars per capita in 1964 to 1812 dollars per capita in 1994. To explain this trend, we include a number of obvious candidates.

Our independent and dependent variables with summary statistics are shown in Table 1.. We include several demographic variables which are commonly used in empirical studies of government spending. Among these, population is included to measure scale economies in the provision of public goods. If the demand for public goods is inelastic and publicly provided goods are not purely private, then government spending per capita should decrease with population.

We also include the percent of the state population residing in a metropolitan statistical area. States with many big cities might well have different spending patterns than those which are predominantly urban.. For instance, they might provide a larger range of public goods for which a certain population threshold must be reached, such as zoos, theaters, and so on. Further, urbanization may create unique public goods problems caused by crime, pollution, and the like. Another notable effect may be that tight labor and land markets lead to higher prices for production inputs, which would increase government spending if the demand for public goods is price inelastic. We also use the growth rate of population over the previous four years, since states whose population is growing fast may find themselves confronted with an especially high demand for infrastructure services such as highways or schools.

Personal income per capita is expected to positively affect expenditures per capita. If publicly provided goods are normal goods, we would expect government spending to rise with income.

Revenue from federal government per capita is included to capture the effects of intergovernmental grants. Lump-sum transfers should have a pure income effect while

services. This implies an expected positive coefficient for federal aid.

States with significant mineral reserves raise operations revenue by using severance taxes. If mineral reserves are partly owned by nonresidents, this shifts some of the costs of expenditures onto non-state residents and, as a result, is expected to increase spending per capita. We thus include state mineral production per capita.

To measure the effect of political parties, we include a variable intended to capture the amount of Democratic control of state politics. This variable takes on the value 1 if both houses and the governorship were controlled by the Democrats, minus 1 for complete Republican control, and zero otherwise. The reason for including this variable is twofold. One is the potential occurrence of partisan cycles, i.e., a systematic difference in the parties' propensity to use the government sector. Another reason is to capture the fiscal conservativeness of voters. This is important because we want to isolate the effect of legislative professionalism on government spending. States with low demand for government are also likely to have non-professional legislators. Therefore, we have to control for the states' preferences for government spending. This should be accounted for by the inclusion of the demographic and party variables. Another variable we use to capture latent conservativeness is a dummy variable equal to one if a state's voters can initiate and approve laws by direct voting. Matsusaka (1995) found that initiative states spend significantly less than non-initiative states; hence controlling for the initiative should allow us to isolate the effect of professionalism.

The primary focus of this paper is on the effect of legislative professionalism. Since there is no obvious way of measuring professionalism, we have to rely on proxy variables. One such variable is a simple dummy variable which takes on the value one if the state in question has a citizen legislature. This classification follows Kurtz (1992) and has later been adopted by the

National Conference of State Legislatures.⁷

Another approach was followed by Squire (1992) who used information on legislative compensation, staff, and length of legislative session to construct a continuous measure of professionalism that varies between 0 and 1. The index is constructed by taking the average of legislator compensation, staff members per legislator, and length of session, all measured relative to the US Congress.⁸ King (1998) constructed a measure similar to Squire's for the years that we consider. The indices differ in that King uses expenditures on staff, whereas Squire uses the number of staff members. However, since the two are highly correlated, so are the indices. We use King's index as our measure of professionalism, because it is available for a number of years. This allows us to exploit the variation of professionalism over time in addition to the cross sectional variation.

The 1960's are commonly viewed as the onset of a movement for the professionalization of state legislatures. Accordingly, the mean professionalism score rose between 1964 and 1974 from 0.16 to 0.22, or by 37.5 percent – during the same period state spending rose by 68 percent. Professionalism continued to rise during the 1970's, but in the 80's this increase leveled off. The mean scores for the periods 1984 and 1994 were 0.26 and 0.25 respectively. The maximum score also has risen considerably, while the minimum has remained roughly constant. Consequently, the variance of the professionalism scores also rose until 1984. California, Illinois, Massachusetts, Michigan, and New York are consistently among the most professional legislatures, while Arkansas, the Dakotas, New Hampshire, New Mexico, Utah, and Wyoming are among the least. There is thus considerable consistency with the qualitative score.

⁷ States identified as having citizen legislatures are: Arkansas, Idaho, Louisiana, Maine, Mississippi, Montana, Nevada, New Hampshire, New Mexico, North Carolina, North Dakota, Rhode Island, South Dakota, Utah, Vermont, West Virginia, and Wyoming. There are other qualitative indices of this kind. However, the correlation between these measures is very high, so using any one of them should produce similar results. For a comparison of those indices see e.g. King (1998)

It can be seen that the states which have citizen legislatures tend to be small states, which is to be expected. Thus we must control for population to ensure that we are not measuring the smaller spending needs implied by a small population. In this way, we are sure that holding population constant, having a citizen legislature changes government spending.

Most of the data were taken from various issues of the Statistical Abstract; mineral production was taken from the Minerals Yearbook, and the professionalism index was taken from King (1998).

4. Regression Results

We begin with a baseline regression which excludes political variables and state fixed effects. The results are displayed in column (1) of Table 2.⁹ The fit of the regression is good: 90% of the total variation are explained by the included variables. We find that government expenditures respond positively to federal aid, mineral production, metropolitan population, and income. Government spending was negatively related to population size. All these coefficients are significantly different from zero, except the one for mineral production. According to the point estimates, a one percent increase in federal aid increased government spending by 2/3 percent on average. An increase in mineral production by one percent led to an increase in government spending of 0.02 percent. While the coefficient is not significant in this specification, it does become significant when political variables or state fixed effects are included, so there is some support for the hypothesis that states export taxes through the use of severance taxes. An increase of population by one percent reduced expenditure by about 0.07 percent. Assuming a price elasticity of -0.4, this would translate into a congestion parameter of $\delta \cong 0.83$, indicating a substantial amount of congestion.¹⁰ An increase of per capita income

⁹ Except for the professionalism score, population change, democratic control, and the initiative dummy, all variables are measured in natural logarithms.

by one percent was associated with an increase in spending by about 0.45 percent. As expected, government provided goods are normal goods. A one percent increase in the share of the state's population living in metropolitan areas increased spending by 0.18 percent.

To this specification, in column (2) we add the political variables. The explanatory power of the model increases, and a Wald test of the null hypothesis that the coefficients on all three political variables are zero can be rejected at the one percent level. The table shows that states with an initiative had spending that was significantly lower than states without the initiative. This supports the finding of Matsusaka (1995). The table also shows that democratic control led to an increase in government spending, but the coefficient is insignificant.

The coefficient on the professionalism score is positive and significant at one percent. According to the estimate, an increase in the professionalism score by one percent was associated with an increase in government spending by approximately 0.18 percent. Given that we control for fiscal conservativeness by including party variables and the initiative dummy, this seems non-trivial. Matsusaka (1995) includes more sophisticated variables to measure ideology, and finds that these have no explanatory power. Thus, we feel confident that the effect we measure is due to professionalism itself.

As a further test, in column (3) of Table 2, we include state fixed effects which should capture states' latent demand for government services. The results clearly indicate that the effect of professionalism on government spending survives the inclusion of fixed effects: the coefficient estimate is a bit smaller (the implied elasticity is 0.12), but still significantly different from zero at one percent. This lends additional support to the hypothesis that the effects of professionalism do not occur because states with professional legislatures have a high demand for government services.

We also ran regressions using Squire's (1992, 1998) professionalism index for the years 1986 and 1995, with essentially unchanged results. Additionally, we used a dummy variable for citizen legislatures, and found that states with citizen legislatures spent approximately 350 dollars less per capita than states with professional or hybrid legislatures. This amounted to about 12% of mean expenditures.¹¹

A final concern in the estimation is the assumed exogeneity of the regressors in equation (2). First, since some federal aid programs include matching provisions, federal aid may depend on state expenditures and thus itself be endogenous. Second, the professionalism score might also be endogenous. We therefore reran the regression with two-stage least squares, treating federal revenue and professionalism as endogenous. We used Democratic control and the percentage of the population enrolled in primary or secondary schools as instruments. The results – reported in column (4) of Table 2 for the case without fixed effects – are essentially unchanged.

Altogether, the results indicate that professionalism is a significant determinant of government spending. Perhaps because of greater susceptibility to interest group pressure, or due to more extensive lobbying, legislatures tend to spend more the more professional their members. Advocates of term limits might view this as support for their position, but we should stress that since we have not separated out the effects of reduced tenure from the other potential effects of reduced professionalism, this inference would seem unwarranted.

5. Conclusion

This paper considered determinants of state government expenditures. We discovered state legislatures whose members received low pay, had small staffs, and met for short sessions spent less per citizen. While we have not identified any causal relationships, it seems clear that

the incentives for professional legislatures to legislate to the benefit of special interest groups and to strike logrolling deals differ from the incentives faced by citizen legislators.

The recent reform movements for imposing term limits stemmed in part from widespread dissatisfaction with professional legislatures. Part of this dissatisfaction, in turn, seems to be due to excessive government spending. This paper suggests an alternative to term limits, namely a reduction in the level of legislative professionalism.

We should stress, however, that it is difficult to draw normative conclusions from our finding. First, we have not shown that professional legislatures spend more than citizens want (although this is one possible interpretation). More importantly, assessing the normative properties of institutional structure obviously involves a number of aspects, including the quality of legislation, which is notoriously hard to measure. Hence, simply comparing spending levels does not allow inferences about the overall efficiency of an institution.

Table 1. Descriptive statistics.

	Mean	Std. Dev.	Minimum	Maximum
State General Expenditure	1292.65	719.71	377.66	7173.70
Professionalism	0.22	0.12	0.05	0.74
Democratic control	0.57	0.56	-1.00	1.00
Initiative	0.42	0.50	0.00	1.00
Revenue from federal government	345.55	163.73	87.70	1122.72
Mineral production	759.58	2472.02	1.76	28829.32
Population	4591.74	4881.17	338.00	31408.00
Personal income	11054.04	2806.57	4816.13	19597.84
Population growth	9.28	11.94	-7.20	78.70
Percent metropolitan population	48.10	33.84	0.00	100.00

Table 2. Regression results.

Variable	(1)	(2)	(3)	(4)
Professionalism		0.81 (4.12) ***	0.56 (3.38) ***	0.56 (3.12) ***
Democratic control		0.01 (0.41)	-0.01 (-1.17)	
Initiative		-0.08 (-3.64) ***		-0.09 (-3.79) ***
South	0.22 (0.55)	0.03 (0.75)		
Revenue from federal government	0.67 (10.33) ***	0.53 (11.09) ***	0.40 (11.62) ***	0.64 (7.83) ***
Mineral production	0.02 (1.43)	0.03 (2.41) **	0.02 (2.09) **	0.03 (2.71) ***
Population	-0.07 (-3.72) ***	-0.12 (-4.59) ***	-0.12 (-4.21) ***	-0.11 (-4.51) ***
Personal income	0.45 (2.22) **	0.46 (4.44) ***	0.23 (2.14) **	0.40 (3.51) ***
Population growth	-3.53E-04 (-0.29)	4.52E-04 (0.46)	4.46E-04 (0.55)	5.55E-04 (0.52)
Percent metropolitan population	0.18 (3.61) ***	0.08 (3.88) ***	0.14 (3.70) ***	0.13 (3.18) ***
Constant	-1.29 (-0.64)	0.18 (0.19)		0.55 (0.59)
Year dummies	Yes	Yes	Yes	Yes
State dummies	No	No	Yes	No
R^2	0.90	0.92	0.97	0.93
Adjusted R^2	0.89	0.91	0.96	0.92

Note: T-values, computed using White heteroscedasticity consistent standard errors, in parentheses. Asterisks indicate statistical significance at 10% (*), 5% (**), and 1% (***). State and year fixed effects not reported. No.

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