WP/01/208

IMF Working Paper

Government Spending, Legislature Size, and the Executive Veto

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INTERNATIONAL MONETARY FUND

 WP/01/208

IMF Working Paper

Research Department

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Authorized for distribution by Mr. Borensztein

December 2001

Abstract

Recent work on the political economy of fiscal policy has asked how budgetary institutions affect fiscal outcomes. But what determines the budgetary institutions? In this paper I consider one such institution: the executive veto. A simple theoretical framework predicts that jurisdictions with more political actors spending from a common pool of tax resources will choose to empower their executives. Using an econometric framework to identify the exogenous variation in the number of districts, I present evidence from a cross-section of local governments in the United States that jurisdictions with more electoral districts are likely to have executives with veto powers.

JEL Classification Numbers: D7, E6, H1, H3

Keywords: government spending, political institutions, executive veto

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[1] Presented at the "Democratic Institutions and Economic Policy" panel of the 2001 Annual Meetings of the American Political Science Association, August 30–September 2, 2001. I am grateful to Caroline Hoxby for providing some of the data used in this paper, and to Eduardo Borensztein, Dhammika Dharmapala, and Mark Hallerberg for helpful comments.

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I. Introduction

One proposition which the literature on budgetary institutions (the study of how the rules of the game governing the budget-making process in government affect budgetary outcomes) has tried to establish is that the size of government increases in the number of districts which get to spend out of a common pool of tax revenues.[2] Models of economic policy making which build on the assumptions that the benefits of public spending are concentrated to legislative districts and that the total tax bill is shared across districts predict such a relationship. With the total tax bill to be shared amongst a greater number of districts, the perceived marginal cost of one more dollar of public spending in any representative's district is smaller, leading to higher spending proposals by all representatives. Such models also imply that there is overspending—government spending is greater than what the elected representatives would have preferred if they could have coordinated on an equilibrium. If elected representatives, and more generally, their constituents, are worse off with such an outcome, they would prefer a budgetary institution which could get them to coordinate on an incentive compatible outcome which entailed less spending. One such budgetary institution is the executive veto authority for budget legislation. It is argued that such veto authority affords strong powers to the executive who can credibly threaten to veto large budgets.[3] We should then expect that jurisdictions with greater propensity for overspending—those with a large number of electoral districts—choose to give their executives authority to veto budget legislation.

Testing this prediction empirically is difficult. The first problem is that observed political systems which have executive veto may not be the result of choices made by the elected body, or more generally, by the electorate; they may simply represent the influence of history and other factors having nothing to do with the fiscal consequences of an executive veto. In industrialized countries, political institutions have evolved over a long period, influenced by a variety of factors at work. In many former colonies, the political institutions are heavily influenced by those of the parent country. Therefore, one needs a sample of jurisdictions where there is precedence of changing the powers given to the executive, there is variation across the sample in whether executives have veto powers, and there is a sufficiently large number of observations to get precision in estimation. Second, and more formidably, given a sample of jurisdictions with variation in the executive veto, it is difficult to identify the causal effect of the number of electoral districts on the probability of having a

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[2] See Kontopoulos and Perotti (1999) for evidence from a panel of OECD countries, Gilligan and Matsusaka (1995, 2001) for evidence from US states, and Baqir (forthcoming) for evidence from US cities. These papers show that, respectively, the number of political parties in coalition governments and the number of spending ministries in government, the size of the upper house in state legislatures, and the size of the city councils, raise scaled measures of government spending.

[3] See Carter and Schap (1987) and McCarty (2000).

veto. Both variables are aspects of the overall political system of the jurisdiction. If the political system changes for some other reason, both these variables can change, giving a spurious partial correlation. In the empirical work on budgetary institutions the justification traditionally given for regressing fiscal variables on institutional variables (and interpreting the coefficients causally) is that it is costly to change institutions.[4] Such an argument cannot be used for regressing one political institution on another since a priori it is unclear which institution is the more costly to change. Inference from simple partial correlations is likely to be highly problematic.

In this paper I present evidence on the question posed above from a cross-section of city governments in the United States. The data and methods I use address both these problems. City governments vary both in their political institutions (in particular, the number of elected representatives and whether the executive (city mayor) has veto authority) and in their fiscal outcomes. Two popular forms in which city political institutions are packaged are the mayor-council and the council-manager form of government. The former resembles a presidential system of government with separation of powers between the elected legislative body (the city council) and the executive (city mayor) elected directly from the city. Mayors in mayor-council cities typically have strong powers: appointing department heads, preparing the budget to be presented to the council and implementing it, and having veto authority over council-passed legislation. In contrast, council-manager forms are similar to parliamentary systems with legislative and executive authority fused in the council. The office of the mayor often exists but her role is largely ceremonial. The council appoints, and can fire, a city manager who administers the city and implements the council-passed budget. Cities have changed these institutions over time. According to the International City/County Management Association, an organization which promotes the council-manager form of government, an average of 44 cities adopted the council-manager form of government per year between 1980 and 1993. Therefore, at least some variation in executive veto authority across cities represents choices made by cities with respect to the political institutions governing them. This helps to address the first concern mentioned above.

The fact that institutions may come bundled in alternative forms of government, however, accentuates the importance of the second problem mentioned above. This may be particularly poignant for identifying the effect of number of electoral districts on the probability of having executive veto authority since the progressive reform movement in the earlier part of the 20th century, which articulated the council-manager form with weak mayors, also advocated small councils (Engstrom and McDonald (1986)). If the reformers were motivated by the fiscal commons problem and an appreciation of the role a strong mayor can play in breaking universalistic spending coalitions, such a simple empirical correlation between council size and executive veto may indeed be evidence for the prediction discussed above. The problem with such an interpretation, however, is that the alternative hypothesis that such a correlation is spurious, due to an unobserved factor, cannot

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[4] See Alesina and Perotti (1999) for a critical review.

be rejected. I use an empirical methodology to get at the causal effect of districting on the probability of having an executive veto. Some variation across cities in the number of electoral districts is likely due to natural divisions in the city, such as hills, valleys, rivers, streams, and other such features of the city's topography. Cities with a greater number of rivers and streams flowing through them are likely to—and do, in a statistically significant way—have a greater number of electoral districts. Electoral districts are often drawn to be contiguous tracts of land and to have some notion of identity or homogeneity for the district. Natural divisions due to topography therefore give natural boundaries along which district lines can coalesce. On the other hand, it is difficult to think of a reason why having an executive veto would be correlated with the number of rivers and streams. Since rivers and streams satisfy these two criteria for a good instrument, I use the method of instrumental variables to address the question: do cities with greater number of electoral districts choose to give their mayors veto powers?

The answer is yes. The effect is quantitatively important and statistically very significant. Results are based on a cross-section of 1676 cities pertaining to 1990. I first present a theoretical framework which illustrates the fiscal commons problem and the role an executive veto can play in limiting overspending. The argument builds on the work of Weingast, Shepsle and Johnsen (1981), Chari, Jones, and Marimon (1997), and Persson and Tabellini (2000). If the executive veto can limit overspending and the exercise of the veto is voluntary, the burden of the question falls on explaining why all cities don't give their executives veto powers. The tradeoff I focus on is based on the work of Shepsle and Weingast (1981). Universalistic spending coalitions have the cost that spending is excessive, but the advantage that each member of the legislature is assured of being in the majority coalition, and his district getting public projects financed by national tax dollars. When a legislature delegates authority in the form of a veto to the executive, who needs the electoral support only of a majority of the districts, it leaves itself open to the possibility that any given member of the legislature may not be in the winning coalition. This tradeoff between the uncertainty of being in the winning coalition and the benefits of cutting excessive spending forms the basis of deciding whether to delegate veto authority to the executive. The model shows that this decision depends on the size of the legislature: for small legislatures, the benefits of certainty outweigh the costs of the fiscal commons problem while for large legislatures the latter dominate.

I next discuss the empirical methodology to be used in taking this prediction to the data. The estimator used is a version of the Amemiya Generalized Least Squares estimator (Amemiya (1978)) for limited dependent variable models where some regressors are suspected of being endogenous. Newey (1987) shows that under quite general conditions this estimator is asymptotically at least as efficient as alternative two-stage estimators based on a first-stage instrumenting regression and a second-stage conditional maximum likelihood equation (as developed by Smith and Blundell (1986) for the Tobit model and Rivers and Vuong (1988) for the Probit model). Results of the estimation are presented in section III as follows.

First, I address the question of whether having veto authority affects city government spending? The regression results show a statistically significant and pronounced effect of veto authority on government spending. Cities with strong mayors don't exhibit a positive relationship between the number of electoral districts and the size of government spending, the central prediction of fiscal commons models. These regressions also show that of other available indicators of executive powers—including indicators for agenda-setting powers (the right to draft and propose the budget to the council) and for specific types of veto (e.g. the line item veto)—it is the overall indicator for whether the executive has any veto authority which makes the difference with respect to government spending. Second, as a benchmark, I present the regressions for the executive veto without instrumenting for council size. I next show that the instruments used are good predictors of the number of electoral districts in a city and present the results from the corrected probit. I conclude with a discussion of related issues not addressed by this paper which could lead to potentially interesting future research.

Existing work on veto authority has concentrated almost entirely on the effects of having a veto, and particularly, the line-item veto. The evidence, based heavily on US state level data, does not point to a strong effect of the line item veto on fiscal outcomes.[5] For example, in what is probably the most carefully executed study, Holtz-Eakin (1988) finds short-run effects of the line item veto under certain political composition of the legislature and governorship, but finds no long-run effects, even after allowing for political circumstances. To motivate the results in this paper, I will first present evidence that, for the city government data at hand, having some kind of veto authority in the office of the executive matters for government spending, and in particular, affects the relationship between districting and government spending.[6] There is, however, relatively little work on explaining why some jurisdictions give their executives veto authority. As such, a key intended contribution of this paper is to take the stage of inquiry one step higher and ask why do certain institutions get chosen. The paper which comes closest in spirit to this paper is de Figueiredo (2001). Using panel data on US states for the period 1866 to 1994, he explores why certain states decided to empower their governors with the item veto. He argues that the item veto is most likely proposed by fiscal conservatives who fear the loss of power in the future and as such the veto is a device to insulate policy outcomes from future liberal legislatures. Interestingly, he also finds that "fiscal strain does not increase the likelihood of adoption" (p. 3). In contrast, the results of this paper are consistent with the view that jurisdictions where fiscal strain is likely to be greater adopt the veto. I also focus on the overall veto instead of the item veto since the results I report show that it is the former which has a more pronounced effect on government spending.

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[5] See Carter and Schap (1990) for a comprehensive review of the empirical literature on the fiscal effects of the line item veto.

[6] I do not focus in this paper on the related interesting question of why one gets an effect of veto authority on city government spending while state studies do not find a statistically significant effect.

II. Theoretical Framework

The results for the choice of executive veto can readily be understood in the context of a simple theoretical framework. Consider a political jurisdiction consisting of J districts where the representative from district j has the following preferences:

$$U_j = v(g_j) + y - \frac{1}{J}\sum_{k=1}^{J} g_k, \tag{1}$$

where $v(\cdot)$ is a well-behaved, continuous, increasing, concave function satisfying $v(0) = 0,\ v'(0) = \infty,\ v'(\infty) = 0$. The representative benefits from government spending g_j in his district and from income (y) net of taxes. For convenience, the population of each district is normalized to unity so that the variables can be interpreted as per capita variables as well. Per capita taxes are assumed to determined simply by dividing the total tax bill equally among the J districts.[7] The utility function in (1) can be thought of directly as denoting the preferences of the representative from district j or the preferences of the median voter from district j. Consider first a decentralized approach to budgeting where each representative proposes spending in her district and the resulting budget is simply the aggregation of these spending proposals. This is the common pool problem discussed by Weingast, Shepsle, and Johnsen (1981), Chari, Jones, and Marimon (1997), and many others. The first order condition defining j's optimal spending proposal, given everybody else's spending proposals, is:

$$v'(g_j^U) = \frac{1}{J} \tag{2}$$

Without loss of generality, heterogeneity across districts has been suppressed so the above condition together with the concavity assumption on $v(\cdot)$ defines an increasing function $g(\cdot)$ which gives the relationship between per capita spending in the jurisdiction and the number of districts under universalism (denoted U): $g^U = g(J),\ g'(\cdot) > 0$. Indirect utility for a representative is therefore:

$$U^U(J) = v(g^U) - g^U + y$$

which is decreasing in the number of districts since the level of spending chosen in the aggregate is greater than the what would be socially optimally (denoted g^S given by $v'(g^S) = 1$).

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[7] This is not restrictive. As long as tax shares are exogenous and non-increasing in the number of districts, the same reasoning applies.

The way I model the effect of having an executive with a veto is by assuming that the executive needs the support only of a minimal majority of districts and that she maximizes average utility in these districts. Here I do not model the game fully but assume that an executive with veto authority is strong enough to attain her desired spending in the districts she caters to. Furthermore, which districts the executive will choose to become a part of her wining coalition is not known to the legislators ex ante. This gives the tradeoff with respect to having an executive with a veto: the tax rate will be less than an outcome under universalism but not all representatives will benefit from public spending in their districts. Suppose that J is an odd number—the minimal majority will therefore consist of $(J+1)/2$ members from the legislature. Then the first order condition with respect to the executive's problem of maximizing average utility in these districts is:

$$v'(g^M) = \phi$$

where g^M denotes spending in a majority district and $\phi = \phi(J) = (J+1)/2J$ is the marginal cost, in terms of per capita taxes, of one more unit of spending in each of the majority districts. Comparing with (2), g^M can be expressed as $g^M = g(\phi^{-1})$ and for $J > 1$, $g^M < g^U$. Minority districts get no public spending. Hence per capita taxes equal $\phi \cdot g^M$. The indirect utility for a representative is therefore $v(g^M) - \phi g^M + y$ if he is a member of the chosen coalition and $-\phi g^M + y$ if he is not a member of the chosen coalition. Ex ante all representatives are equally likely to be included in the majority coalition which implies that the probability of being in a winning coalition is given by ϕ. Hence the expected utility for a representative is given by:

$$U^V(J) = \phi\left[v(g^M) - g^M\right] + y$$

Note that for $i = U, M$, $g^i > g^S$, hence $v(g^i) - g^i$ is decreasing in g^i.

For representatives considering their welfare under alternative institutions of universalism and strong executives, the difference in expected utility is given by:

$$\Delta^U(J) \equiv U^U(J) - U^V(J) = \left[v(g^U) - g^U\right] - \phi\left[v(g^M) - g^M\right]. \tag{3}$$

Inspection of this expression gives the result that for a sufficiently large legislature, the benefits of capping the tax rate outweigh the costs of uncertainty.[8] Interestingly, in small legislatures, universalism would be preferred. Start by considering when J is very large. As

[8] Note that this is different from the Shepsle and Weingast (1981) result on the stability of universalism. They assumed that members of the winning coalition could not coordinate on a desired outcome. In their set-up the choice of spending by members of the winning coalition would also given by (2) which would yield $U^U - U^V = (1-\phi)\left[v(g^U) - g^U\right]$ which is positive as long as public spending produces net positive benefits.

$J \to \infty$, $\phi \to \frac{1}{2}$, and $g^M \to g(2)$. Thus the second term in (3) is bounded. The first term, however, continues to decline as J increases, eventually reaching zero. Let J^C denote the value of J at which $U^U(J) = 0$, that is $v(g(J^C)) - g(J^C) = 0$. Since $g^M < g^U \ \forall \ J > 1$ and $v(g) - g$ is decreasing in g, $v(g^M) - g^M > 0$ at J^C. From (3) then $\Delta^U(J^C) < 0$. That is, by the time J reaches J^C, Δ^U is already negative. Hence (3) turns negative before J reaches J^C, that is, before universalism turns so bad that there are not net benefits of public spending.[9]

Now consider (3) when J is close to 1. For $J = 1$, $\phi = 1$, and $\Delta^U(1) = 0$. Differentiating (3), one can show that $\Delta^{U'}(1) > 0$. The intuition is that at $J = 1$, $g^U = g^M = g(1)$ corresponds to the social planner solution characterized by $v'(g) = 1$. For infinitesimal changes in J the first order effects in changes in utility due to changes in g^i, $i = U, M$, are zero, since $v(g^i) - g^i$ has been maximized. However, $\phi'(1) = -1/2$, and $v(g(1)) - g(1) > 0$ which means that the effect which dominates close to $J = 1$ is that of the introduction of uncertainty of benefiting from public spending. Together, $\Delta^U(1) = 0$ and $\Delta^{U'}(1) > 0$ imply that Δ^U increases as J increases from 1 before eventually turning negative as J gets large. Since Δ^U is continuous in J we get

<u>Proposition 1</u>: $\exists \ J^*$, $1 < J^* < \infty$, such that $U^U(J) > U^V(J)$ for $J < J^*$ and $U^V(J) > U^U(J)$ for $J > J^*$.

As a corollary $\Delta^U(J^*) = 0$, $\Delta^{U'}(J^*) < 0$.[10] Intuitively, at J close to 1, the effect of overspending on utility under universalism is small, since representatives are close to the maximal utility attainable, but the probability of being in the majority coalition declines rapidly. Thus the costs of uncertainty dominate. As J gets large, however, we know that

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[9] Such a case could be taken to mean that at that stage the districts prefer to break away from the jurisdiction since they get no net benefits from being a part of it.

[10] The case for J even is obtained in an analogous manner. Assume that the executive needs the support of half of the legislature—in case of a tie the executive can cast a vote to obtain the desired outcome. Then, $\phi = 1/2$, $g^M = g(2)$. Assume that $v(\cdot)$ is sufficiently concave so that $v(g(2)) - g(2) > 0$. This is a weak assumption since it says that a universalistic outcome in a 2 person legislature is not so bad that there are no net benefits of being part of the jurisdiction. The analogous expression for the difference in utility is $\Delta^U(J) = v(g(J)) - g(J) - \frac{1}{2}[v(g(2)) - g(2)]$. As $\Delta^U(2) > 0$ and $\Delta^{U'}(J) = U^{U'}(J) < 0$ for $J \geq 2$, by similar reasoning as above Δ^U turns negative before $U^U(J)$ reaches zero.

overspending associated with the universalistic outcome gets progressively bad. For J large enough ($J > J^C$), in fact, universalistic outcomes can produce net negative benefits for representatives (and agents) in the jurisdiction. The proposition shows that before one reaches this stage, the tradeoff swings in favor of limiting spending by endowing the executive with powers to choose minimum winning coalitions. That is , the result does not rely simply on the excessive net costs of universalism in large decentralized legislatures. Figure 1 illustrates what happens to expected utility under the two institutions as J increases.[11]

The way I operationalize this result is to assume that before the budget process the legislators get to choose the institution under which the budgetary decisions will be made. This can be modeled as a stage before the budget-making game where a legislator is chosen at random to bring a proposal before the legislature which would endow the executive with veto powers (and generally strengthen his position in the budgetary process). Given the symmetry in the model either all representatives will vote in favor or oppose it. The empirical prediction is that with a large number of districts it is likely that legislators would find it in their interest to choose an institution which empowers the executive.

III. Empirical Methodology and Data

To estimate the effect of the number of electoral districts on the probability of having an executive veto I consider the following model:

$$M_i^* = \beta J_i + X_{1i}\gamma + u_i \tag{4}$$

where $i = 1,\dots,N$ indexes the city, J is the size of the city council, X_1 is a $1 \times K$ row vector of assumed exogenous variables, and M_i^* denotes a latent variable which I take to be the excess utility from having an executive veto over not having one ($-\Delta^U$). The variable observed is M_i, an indicator for whether the city has executive veto, defined by

$$M_i = \begin{cases} 1 & \text{if } M_i^* > 0 \\ 0 & \text{otherwise.} \end{cases}$$

Council-size is suspected to be endogenous and assumed to be related to the X_1's and a $1 \times L$ row vector Z_i of instruments by:

$$J_i = Z_i\alpha + X_{1i}\delta + v_i \tag{5}$$

[11] The graphs are drawn for the function $v(g) = \frac{1}{\alpha} g^{\alpha}$ for $\alpha = 0.1$.

Let $X_i = [Z_i, X_{1i}]$ and conditional on X_i the disturbances u_i, v_i are assumed to be distributed joint normal, $u_i, v_i \sim N(0, \Sigma)$, where

$$\Sigma = \begin{bmatrix} \sigma_{uu} & \sigma_{uv} \\ \sigma_{vu} & \sigma_{vv} \end{bmatrix}.$$

If the cross-correlation term were zero, (4) could be estimated by a standard probit model. A positive correlation between v_i and u_i implies that, potentially due to an unobserved factor, when a city has an executive veto, the council-size is likely to be large.

To estimate the system formed by (4) and (5), one possibility is to use a two stage procedure, analogous to a two-stage least squares estimator if the second stage were linear. This is the method of Rivers and Vuong (1988). The joint density for M_i and J_i conditional on X_i can be factored into a conditional and a marginal component. First, one maximizes the marginal log likelihood for J_i, obtaining estimators for $(\alpha, \delta, \sigma_{vv})$. Second, one uses these estimators in the conditional log likelihood for M_i and maximizes it with respect to the remaining parameters. They show that this amounts to a simple procedure: a first stage OLS regression of J_i on the instruments and exogenous variables and a second stage probit regression of M_i on J_i, X_{1i}, and $\hat{v}_i$, the least squares residuals from the first stage, which yields consistent estimators of (β, γ). They provide the formulae for a consistent estimator of the asymptotic covariance matrix.

Newey (1987) shows that asymptotically more efficient estimation can be achieved by using a version of Amemiya's Generalized Least Squares (AGLS) estimator. The method of Amemiya (1978) is to start from a reduced form expression for (4) obtained by using (5) to substitute for J_i:

$$M_i^* = Z_i\theta + X_i\lambda + \varepsilon_i \tag{6}$$

where, $\theta = \beta\alpha$, $\lambda = \gamma + \beta\delta$, and $\varepsilon_i = u_i + \beta v_i$. The relation between the structural parameters (β, γ) and the reduced form parameters (θ, λ) can be expressed as:

$$(\theta', \lambda')' = D(\alpha, \delta) \cdot (\beta', \gamma')'$$

where D is a matrix which depends on the parameters in (5). Assume that the order condition is satisfied so that given estimates of $(\theta, \lambda)'$ one can obtain estimates of (β, γ). Let $(\hat{\alpha}, \hat{\delta})$ denote an estimator of the parameters in (5) and $\hat{D} = D(\hat{\alpha}, \hat{\delta})$. Then two estimates of (θ, λ) are available: those from direct estimation of (6), denoted $(\hat{\theta}, \hat{\lambda})$, and $\hat{D} \cdot (\beta', \gamma')'$ for some choice of (β, γ). Amemiya's method consists of choosing $(\hat{\beta}, \hat{\gamma})$ to minimize the generalized distance between these two estimates:

$$\min_{\beta,\gamma}\left[\begin{pmatrix}\hat{\theta}\\ \hat{\lambda}\end{pmatrix}-\hat{D}\cdot\begin{pmatrix}\beta\\ \gamma\end{pmatrix}\right]'\cdot\hat{\Omega}^{-1}\cdot\left[\begin{pmatrix}\hat{\theta}\\ \hat{\lambda}\end{pmatrix}-\hat{D}\cdot\begin{pmatrix}\beta\\ \gamma\end{pmatrix}\right]$$

where $\hat{\Omega}$ is a consistent estimator of the asymptotic covariance matrix Ω of $\sqrt{N}\left((\hat{\theta}',\hat{\lambda}')'-\hat{D}\cdot(\beta',\gamma')'\right)$. Note that in the case when the system is exactly identified the structural parameters can be obtained simply by inverting $\hat{D}$ in which case the Rivers and Vuong (1988) estimator discussed above is numerically equal to the limited information maximum likelihood estimator. It is in the over-identified case where the choice of the weighting matrix $\hat{\Omega}$ gives the efficiency gain. The solution to the minimization problem gives the AGLS estimators for (β,γ):

$$(\hat{\beta}_A',\hat{\gamma}_A')'=(\hat{D}'\hat{\Omega}^{-1}\hat{D})^{-1}\hat{D}'\hat{\Omega}^{-1}\cdot(\hat{\theta}',\hat{\lambda}')'$$

and $Asy.\ Var.\ (\hat{\beta}_A,\hat{\gamma}_A)=(D'\Omega^{-1}D)^{-1}$. Newey (1987) provides the methods for consistent estimation of $\hat{\Omega}$ which can be used to compute the estimator.

Data on council and city characteristics have been obtained from the "Municipal Form of Government 1991" survey conducted by the International City/County Management Association. These surveys, which are periodically conducted, report information on a variety of political characteristics of city governments, and the results are published in various issues of *The Municipal Yearbook*, an annual publication of ICMA. Data on city characteristics have been obtained from the County and City Compendium, a publication of Slater Hall Information Products (Washington, DC), and pertain to the year 1990 unless otherwise noted. This publication is similar to the US Census Bureau's County and City Databook but provides coverage for a greater number of cities and variables. Data on topography (number of larger and smaller streams) are provided by Caroline M. Hoxby (Hoxby (2000)). In a pioneering approach to estimating the effect of school choice on student outcomes she used these measures to get at exogenous variation in the number of school districts in metropolitan areas. The two variables I use are the number of large streams and the number of small streams in a county.[12] I match each city to the county it is located in. For the regression sample of 1676 cities, the median number of cities per county is 3—hence I get substantial variation at the city level in these variables. It would of course be better to have the topography variables at the city level but such data do not seem to be available. The first-stage regressions in the next section show that these variables are statistically significant predictors of the number of electoral districts in a city.

[12] Hoxby (2000) measures the number of large streams by hand using U.S. Geological Survey's 1/24,000 quadrangle maps, and checks them against the Geological Survey's *Geographic Names Information System* (GNIS). The number of small streams is taken directly from GNIS.

Table 1 (first five columns) shows the summary statistics for the variables used in the study for the entire sample of cities. There is large variation in the types of cities included in the sample. City population ranges from a minimum of about 10,000 to a maximum of 3.5 million. The strongest determinant of council-size is city population, as suggested by theory. A regression of council size on population gives a t-statistic of close to 10. Since a linear or log specification might not adequately control for the effects of city size, I use a 5-part spline function based on population quintiles in the regressions. Racial heterogeneity, measured by a Herfindahl index based on race shares of the city population, also ranges significantly: from a minimum of close to zero to a maximum of 0.72.[13] Other variables show considerable variation as well across cities.

The key explanatory variable in the study, size of the city council, varies from a minimum of 2 (Universal City, TX) to 30 (New Haven, CT). This variation allows me to test the predictions of the theory. City political structure is likely to be influenced by state laws and regulations since city governments derive their authority from state governments. States could regulate both the size of the city councils (or the ease with which they can be changed) and the choices made by cities with respect to the balance of power between the council and the mayor. In the empirical work it will be important to examine whether the relationship between executive veto and council-size holds within states as well—the existence of such a relationship would reduce concerns that any cross-state relationship is the result of omitted state specific covariates. To be able to identify the intra state relationship however one would need variation in the council size variable within states. Table 2 shows that this is indeed the case. It presents averages for council-size, population and the mayor veto indicator by state. There are on average 34.2 observations per state, although four of the states account for close to 600 of the cities in the sample. The standard deviation of state-level means of council-size is 1.60 (not reported in the table), while that of the within state deviations of council size from state means is 1.96. On the other hand, there is no variation in the mayor veto variable for four of the states (Vermont and Alaska, both of which contribute only one observation each for the sample, and Nevada and South Carolina, for which none of the cities in the sample have executive veto). For these states it will not be possible to identify separate state specific effects but since they constitute a small fraction of the sample, the problem is likely to be limited. More generally, however, for many states, either most of the cities have executive veto or don't, making it difficult to identify separate intercept effects. In the empirical section I will include a complete set of state indicators which are estimated significantly.

Of the 1676 cities in the sample, 580 (35 percent) give their mayors overall veto powers. The table breaks down information on the type of veto (when available) as well as other indicators of mayor power such as the authority to present the budget to the city council and to appoint department heads. The table also provides the means and standard deviations

[13] The index can be interpreted as the probability that a randomly selected person from the city will be of the same race as another randomly selected person.

for all variables separately for cities with and without the executive veto. Cities without the executive veto have on average 6.1 council member while cities which have veto powers have 8 members. Of the cities with overall veto power, 38 percent report the mayor to have veto powers specifically over appropriations while 25 percent have the line item veto. The other indicators of mayor powers are also quite strongly correlated with the overall veto. Given this correlation between alternative measures of veto powers I will start the empirical analysis by first asking which of these characteristics seem to be important for government spending, and in particular, for the relationship between the number of districts and government spending.

The separate statistics for veto and non-veto cities also show that council size, electoral system, and veto powers tend to be correlated. Non-veto cities tend to have small councils and primarily elect their councilmen at-large. This reflects the bundling of individual political characteristics and highlights the importance of being able to identify the exogenous variation in council size. I do not focus on the effects of council by the type of electoral system since the type of electoral system makes very little impact on the relationship of government spending with the number of council members (Baqir (forthcoming)). Including indicators for the type of electoral system in the regressions makes very little impact to the statistical significance of the results reported below but results in a substantial reduction of observations. Other variables, particularly city characteristics, have roughly similar means for veto and non-veto cities. In particular all indicators of racial heterogeneity (city heterogeneity, council heterogeneity, mayor race indicators) are very similar for cities with and without veto.

IV. Results

I present the results in the following order. First, to motivate the idea that having an overall veto matters for government spending, I present results of regressions for government spending where I include separate intercept and slope effects for cities with mayor powers. These results build upon Baqir (forthcoming). I use several candidates for indicators of mayor powers and find that the overall mayor veto is empirically the most important variable for the relationship between districting and government spending. Next I report the results from probit regressions for mayor veto where I do not address the potential problem of endogeneity. For reference I also report the estimates from the linear probability model. These regressions also show that controlling for state effects matters—most explanatory variables, except council size, lose their statistical significance when state indicators are included. Next I present the first stage results of the identification strategy. Finally, I report the results from the probit (and 2SLS, for reference) regressions using the number of streams as instruments.

Table 3 shows the results for estimating the following equation:

$$\log(g_i) = \alpha_1 V_i + \beta_1 \log(J_i) + \beta_2 (V_i \cdot \log(J_i)) + \alpha_2 M_i + \beta_3 (M_i \cdot \log(J_i)) + X_i \gamma + \varepsilon_i$$

where g_i denotes per capita government spending in city i, V_i is an indicator variable equal to 1 if the mayor has authority to veto council-passed measures (and zero otherwise), J_i is the council size, M_i is an additional indicator for mayor powers, and X_i are controls which include an index of racial heterogeneity of the city, per capita income, ratio of the mean to median household income in the city, percent of population aged 65 years or more, quintiles of city population, a constant, and $S-1$ state indicators, where S is the number of states represented in the sample. The first equation includes only the council size variable and the second adds the intercept and slope effects for a city where the mayor has veto authority. Theories of government spending based on a common pool problem in the fiscal revenues pool predict that the scale of government activity is increasing in the number of players spending from the pool ($\beta_1 > 0$). The interactions test whether the relationship between districting and government spending is broken in cities where mayors have veto authority ($\beta_1 + \beta_2 = 0$). The additional interactions test whether, given that the mayor some form of veto authority, the type of veto authority or other indicators of mayor powers affect the relationship with government spending. In terms of slopes, $\frac{\partial g}{\partial J} = \beta_1$ if $V = 0$. If $V = 1$, then $\frac{\partial g}{\partial J} = \beta_1 + \beta_2$ if $M = 0$, and $\frac{\partial g}{\partial J} = \beta_1 + \beta_2 + \beta_3$ if $M = 1$. That is, β_3 picks up on the incremental effect of the candidate *M* variable.

The variables I use for *M* are, firstly, the type of veto authority (veto over ordinances, over resolutions, over appropriations, and over specific items of appropriations), and, secondly, other indicators of mayor powers (the authority to prepare and present the budget to the city council, the authority to appoint heads of government departments, and whether the mayor is not a member of the council). The results are very consistent and show the following. First, column (1) shows that even when we ignore the separate relationship that may exist in veto and non-veto cities, there is a statistically significant positive relationship between districting and government spending with an implied elasticity of 0.1 of government size with respect to the number of electoral districts. Second, as column (2) shows, when we allow for separate intercept and slope effects for cities with executive veto, the relationship between districting and government spending is stronger in cities without an executive veto (β_1 becomes stronger in magnitude) and non-existent in cities with an executive veto. The bottom of the table shows the *F*-statistic for the null hypothesis: $\hat{\beta}_1 + \hat{\beta}_2 = 0$. The test does not reject, for any of the specifications. Third, once we control for an overall veto authority, it does not matter with respect to government spending the type of veto which the mayor has or other indicators of mayor powers—the hypothesis $\hat{\beta}_3 = 0$ never rejects. Thus these regressions show that it is the overall veto which matters for government spending and subsequent results focus on explaining the granting of this authority.

The coefficients on the control variables have the expected signs. Racial heterogeneity in a city is associated with greater spending. One way in which this correlation can be interpreted is that if district spending has spillover benefits for residents of other districts, this curtails the common pool problem. In fact, in the limit, if everybody benefits from spending in each district, the common pool problem can completely vanish. If different districts have different racial concentrations and different racial groups benefit from different types of public spending, the extent of spillovers is likely to be less. Thus total spending would be higher in more heterogeneous cities. Per capita income is positively correlated with government spending, as has been documented in other empirical studies of local government spending (Bergstrom and Goodman (1973), Bergstrom et. al. (1982)). The ratio of mean to median income is added to proxy for the skewness of the income distribution, as models of income inequality imply, and the coefficient implies that cities where the distribution is more skewed have greater spending. A greater proportion of retirees is associated with greater spending and the coefficients on the population quintiles indicate that per capita spending goes down with the size of city in small to medium sized cities (because of economies of scale) but increases with city size in very large cities (eventual diseconomies of scale). All controls are significantly estimated.

The regressions include an intercept effect for the indicator of mayor veto. Existing theories do not have strong predictions for the sign of the intercept effect. One interpretation consistent with a positive intercept effect for executive veto is that mayors with these powers are able to break universalistic spending coalitions in the council, but their increased position of strength allows them to spend more on their priority items. There is no reason to think this spending on mayor's key priorities should be increasing in the number of councilmen. Hence, small councils, which have small overspending pressures to begin with, may end up with greater spending with a mayor veto. It is also interesting that agenda-setting powers (as measured by the right to present budget to the council) is not a significant determinant of government spending, once we control for veto powers, as a number of theories argue for the importance of agenda setting in determining the size of spending.

Table 4 reports the regressions for the effects of mayor veto authority where concerns of endogeneity have not been addressed. The first regression estimates a linear probability model and the second shows the estimates from a probit regression. I use the same set of controls as I used for the government spending regressions. The first two columns indicate a strong positive partial correlation between the size of the council and probability of having a veto. In addition, the index of heterogeneity and per capita income are also statistically significant predictors of having a veto. Interestingly, racial heterogeneity has a negative sign implying that more heterogeneous cities are less likely to give their mayors veto authority. *A priori* one would have expected that if more heterogeneous cities have similar problems of coordination as cities with more political districts, they would choose to give their mayors veto authority to curtail spending. Replacing the city heterogeneity index with the heterogeneity index constructed from council-member racial shares data also gives a negative coefficient. However, when I include both city heterogeneity and council heterogeneity, I get a negative coefficient on the former but a positive coefficient on the latter. Thus cities with more heterogeneous populations are less likely to have an executive with a veto, but in two cities with the same degree of heterogeneity in the population, the one with a more heterogeneous council is more likely to have a mayor with a veto. I do not go deeper into the causes of these differences as the effects of racial heterogeneity on having a veto is not the focus of the paper and because the results on heterogeneity are not robust to controlling for state specific effects, as discussed below.

City political structure is likely to depend on the state in which it is incorporated, reflecting state laws and regulations concerning local governments. It is therefore important to estimate the above equation while controlling for state specific fixed effects to examine if the relationship exists within states as well. If results were coming purely from the cross-state variation we could not rule out the possibility that the results were coming from omitted state level covariates. Figure 2 shows that indeed there is a significant cross-state relationship between the fraction of cities in the state which have veto authority and the average council size in the state. The most flexible way to address state specific effects is to include a set of state indicator variables in the regression. I estimate the above equation with a complete set of state indicators. Some state indicators however do not come out significant, in either the OLS or the probit specification. I therefore drop them (11 state indicators) from the specification to minimize reduction in the degrees of freedom and reduce possible problems of colinearity with the independent variables, including all state indicators in the regression which are estimated significantly.[14] The 3rd and 4th columns of the table show the results. Inclusion of the state indicators drives out the statistical significance on all explanatory variables except the income and council-size variables. Comparison with the specifications without the state indicators shows that it is the estimated coefficient which changes more than the standard error. This is a pattern more consistent with omitted variable bias (which biases the coefficient) rather than limited intra-state variation in the dependent variables (which would have resulted in bigger standard errors).

Table 5 shows estimation results using the number of streams to instrument for the size of the city council. The first three columns show the results excluding state indicators and the next three with state indicators. In each case, the first stage regressions show that topography is a statistically significant predictor for council size. Since the streams data are available at the county level, and the sample has on average 2 cities per county, the table reports standard errors calculated using a robust covariance matrix, to allow for correlation of errors within a county. The negative coefficient on the smaller streams variable is the result of colinearity since both the stream variables are very strongly correlated (p-value of the correlation coefficient is less than 0.0001). Joint tests for coefficients on both the variables being equal to zero reject strongly for both specifications. The second column shows the estimation from two-state least squares, ignoring for the moment that the predicted probabilities for the second stage need to be constrained to the unit interval. The third column shows the estimation using the Amemiya Generalized Least Squares (AGLS) estimator discussed in the previous section. Comparing the coefficients to the previous table the coefficients become stronger in magnitude when we instrument. The standard errors also increase due to the relative inefficiency of instrumental variables methods. The last three columns show that the same pattern holds in the specification with state indicators.

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[14] Test statistics for the joint hypothesis of excluding these indicators from the specification are reported in Table 4. As shown the tests do not reject.

These results show that controlling for state specific effects and using the exogenous variation in council size there is a positive and statistically significant effect of council size on the probability of having an executive veto. The pattern of change in coefficients when IV methods are used suggests that by not addressing endogeneity considerations we are likely *under*-estimating the impact of council size on the probability of having a veto. One interpretation consistent with this pattern is that the simple observed partial correlation between council size and executive veto is the reduced form of two underlying structural relationships. First is the effect of council size on the probability of having a veto for the reasons discussed in this paper. Second, is the opposite relationship: having a strong executive can curtail the proliferation of districts. The identification strategy then isolates the causal effect of districting on executive veto which is stronger than the reduced form relationship.

The table also shows the results of several specification tests. The test of over identifying restrictions corresponds to an auxiliary regression of the residuals from the IV estimation on the set of instruments. If the joint test of significance of the instruments in this regression rejects it casts doubt on the validity of the instruments. The table shows that for the specification without the state indicators the test statistic is on the margin for a 5 percent size, and does not reject for the specification with the state indicators. Since the test is a joint test that the equation is specified correctly and that the instruments are valid, the borderline significance in the specification without the state indicators may be implying that the specification with state indicators is more appropriate. In either event, the specification with the state indicators is the more appropriate specification a priori and failure to reject indicates the suitability of instruments used. The Davidson-MacKinnon (see Davidson and MacKinnon (1993)) is similar to the Hausman test and is based on a regression for the dependent variable (veto) where additionally the residuals from the instrumenting equation are included as regressors. Failure to reject implies that OLS would have produced consistent estimates while rejection implies that it is appropriate to instrument for the suspect endogenous variable. The test is based on whether the residuals have explanatory power in the regression for executive veto. The Smith and Blundell (1986) test is based on a similar principle for the probit model, where the residuals from the first stage are included as additional regressors in the second stage probit and one tests whether the coefficients on the residuals are jointly zero. Both tests of exogeneity show a similar pattern for the regressions with and without state indicators. When we are not controlling for state effects the test statistics indicate that we should be instrumenting for council size. However, once we have the state effects in the model the test statistics imply that the model might be correctly specified and there may not be a need for instrumenting. This implies that the sources of endogeneity in the first case may be due to state specific reasons, such as state regulations which affect both council size and the powers afforded to the executive. Since local governments derive their authority from state governments, the pattern of test statistics suggests this is a plausible interpretation.

The next table (Table 6) shows a similar pattern for the breakdowns of the overall veto variable: veto specifically over appropriations and the line-item veto. The corresponding regressions without instrumenting show a similar pattern as above. The results in Table 3 showed that given that the executive possessed some kind of veto authority there wasn't a statistically significant incremental effect of the type of veto authority. If those specifications were run excluding the overall veto variable (both the intercept and the slope effects) but including an interaction for just the type of veto authority (such as veto over appropriations or the line item veto), the results are similar but not as strong. The regressions in Table 6 show that districting also has predictions for the type of veto authority even though the results on the latter do not come out as clear in the regressions for government spending. In line with the regressions in Table 3, however, the results are strongest for the overall veto and become weaker when one considers solely the line item veto. Consistent with the empirical evidence discussed in the Introduction on the effects of the line item veto these results taken together imply that jurisdictions with large number of districts give their executives some kind of veto authority but not necessarily always the line item veto.

V. Conclusion

This paper used evidence from local governments to ask why the institution of executive veto varies across political jurisdictions. Given the problems in testing theories which predict the effect of one political institution (legislature size) on another (executive veto), as discussed at the beginning, these data together with the identification strategy provide a clean way to shed evidence on this question. These findings are important for budgetary institutions at the state/provincial and national levels as well since similar forces are likely at play in legislatures at different levels of government. To summarize, the theoretical framework predicted that given a choice between a decentralized universalistic environment and an extreme form of majority-minority environment, which institution would render higher expected utility to representatives in the legislature depends on the size of the legislature. For small legislatures the costs of uncertainty dominate and for large legislatures the costs of overspending dominate. The empirical results showed that, first, of the available indicators of executive powers in city governments in the US, the indicator for having some veto authority mattered the most for spending outcomes. Second, using instrumental variables to identify the relationship, cities with larger legislatures are more likely to have executives with veto authority. Comparison to the non-instrumented results showed that the reduced form relationship likely under-estimates the effect of legislature size on probability of having veto authority.

Overall, the argument in this paper has two key components. First, that expected utility for the players in the political game is higher in a majority-minority environment when there is a large number of actors, and second, that those institutions which yield higher net utility to the actors operating under them get chosen. It is possible to deviate from both of these points and come up with a theory that predicts the same reduced form relationship. However, given the dearth of formal work on modeling the choice of budgetary institutions, a ready alternative does not exist against which the predictions based on the argument in this paper can be tested. Further work in the area of the choice of budgetary institutions would provide additional empirical predictions which can be used to discriminate amongst competing theories.

Figure 1. Expected Utility

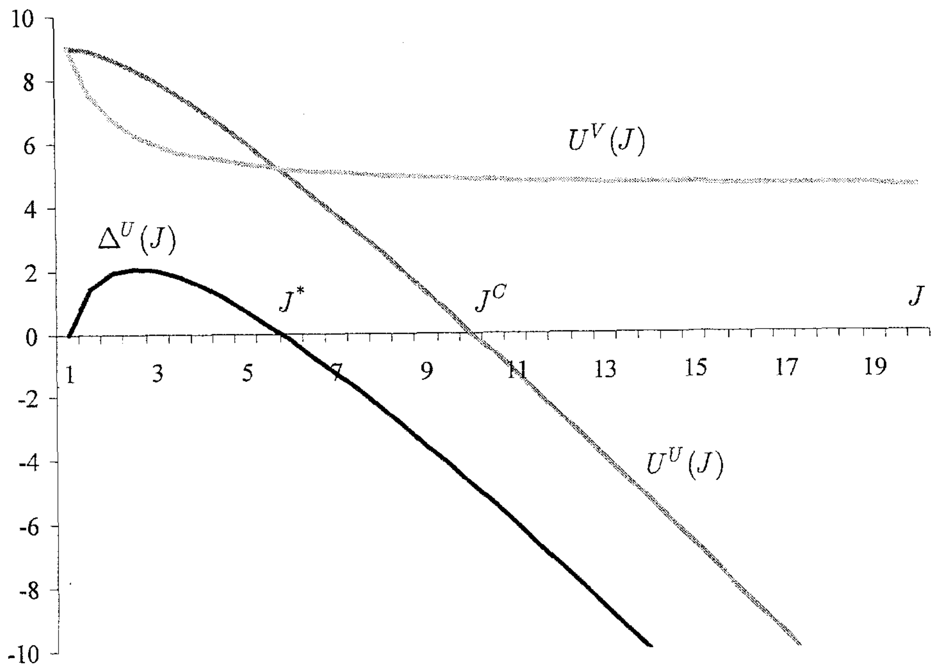

Figure 2. State level relationship between Executive Veto and Council Size

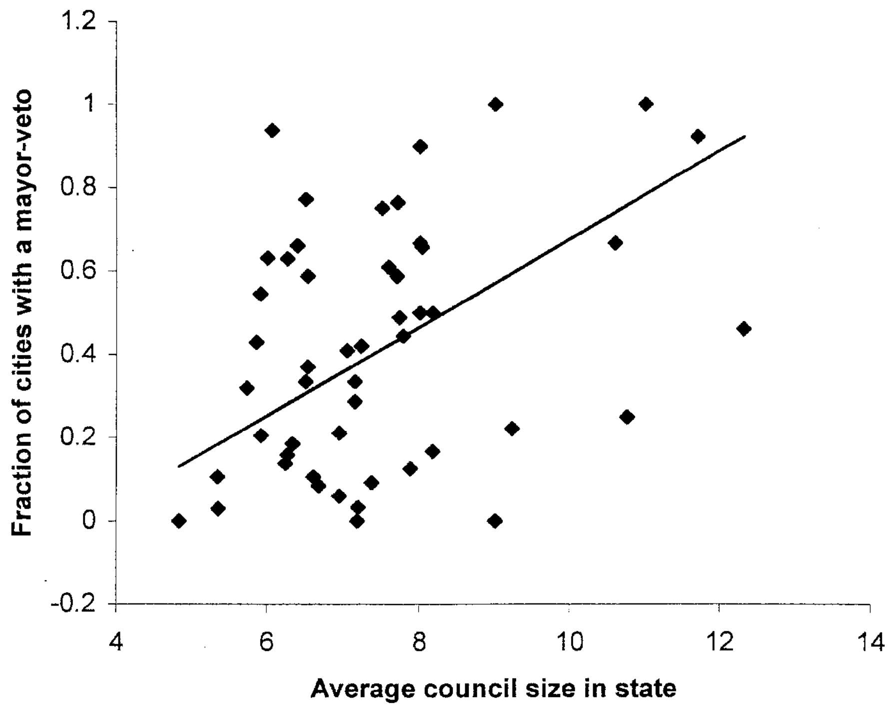

Table 1. Summary Statistics

		All cities					Mayveto = 0			Mayveto = 1		
Variable		Obs.	Mean	Std. Dev.	Min	Max	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.
Mayor characteristics:												
mayveto	Mayor has authority to veto council-passed measures	1676	0.346	0.476	0	1	1096	0.000	0.000	580	1.000	0.000
vetoapp	Mayor can veto appropriations	1675	0.130	0.337	0	1	1095	0.000	0.000	580	0.376	0.485
vetosapp	Mayor can veto specific items of appropriations	1675	0.087	0.281	0	1	1095	0.000	0.000	580	0.250	0.433
budgetm	Mayor prepares and presents budget to the council	1647	0.174	0.379	0	1	1075	0.042	0.200	572	0.421	0.494
headsm	Department heads are appointed by the Mayor	1655	0.233	0.423	0	1	1079	0.079	0.270	576	0.521	0.500
mmember	Mayor is a member of the council	1658	0.639	0.480	0	1	1085	0.870	0.336	573	0.202	0.402
mselpop	Mayor is elected directly from the city population	1674	0.786	0.411	0	1	1094	0.677	0.468	580	0.990	0.101
mwhite	Mayor is white	1676	0.936	0.246	0	1	1096	0.928	0.259	580	0.950	0.218
mblack	Mayor is black	1676	0.033	0.178	0	1	1096	0.032	0.176	580	0.034	0.183
Council characteristics:												
csize	Size of city council	1676	6.794	2.372	2	30	1096	6.131	1.619	580	8.045	2.986
ciwar	All councilmen elected by district	1390	0.131	0.337	0	1	919	0.061	0.239	471	0.268	0.443
cilar	All councilmen elected at-large	1390	0.633	0.482	0	1	919	0.746	0.435	471	0.412	0.493
cethnic	Index of council racial heterogeneity	1676	0.125	0.180	0	0.72	1096	0.126	0.182	580	0.123	0.178
City characteristics:												
pop90	City population	1676	46,498	111,703	10,019	3,485,398	1096	42,262	66,067	580	54,504	166,563
ethnic90	Index of city racial heterogeneity	1676	0.240	0.172	0.008	0.720	1096	0.257	0.173	580	0.207	0.166
bagrad90	Fraction of population aged 25 or more w/ college education	1676	0.218	0.123	0.017	0.792	1096	0.225	0.125	580	0.206	0.118
ipc90	Income per capita ($)	1676	14.616	5.837	5.237	63.302	1096	14.905	6.298	580	14.068	4.804
mmi90	mean/median household income	1676	1.258	0.142	0.986	2.240	1096	1.263	0.144	580	1.249	0.138
pop65up	Percent population aged 65 years or more	1676	13.317	5.571	1.700	56.100	1096	13.070	6.023	580	13.786	4.565
exppc92	Government expenditures per capita ($1,000s)	1430	0.770	0.477	0.022	4.199	936	0.746	0.438	494	0.816	0.541
expsh92	Government expenditures as % of city income	1430	5.841	3.818	0.172	44.660	936	5.622	3.534	494	6.257	4.277
Other characteristics:												
maycou	Form of government is "mayor-council"	1676	0.379	0.485	0	1	1096	0.154	0.361	580	0.803	0.398
commit	City council has standing committees	1616	0.584	0.493	0	1	1051	0.521	0.500	565	0.699	0.459
initiat	City has provision for initiative	1676	0.517	0.500	0	1	1096	0.567	0.496	580	0.424	0.495

Table 2. Council Size, City Population, and Executive Veto by State

		Council size			
State	No. of obs.	Mean	Standard deviation	Mean population	Fraction of cities with veto
California	243	5.30	1.05	73,226	0.025
Texas	130	6.43	1.35	56,631	0.100
Illinois	116	7.49	2.75	28,315	0.664
Ohio	106	7.55	1.27	39,252	0.660
Florida	96	5.33	1.01	43,604	0.125
Michigan	69	6.83	1.39	51,637	0.203
New Jersey	58	6.38	1.42	28,230	0.655
New York	58	6.90	2.40	36,588	0.431
Minnesota	53	5.91	1.24	32,213	0.170
Wisconsin	46	10.28	4.82	46,556	0.652
Missouri	45	7.67	1.94	35,573	0.489
Pennsylvania	40	6.50	1.47	31,731	0.600
Indiana	38	7.50	1.48	29,121	0.658
North Carolina	37	7.00	1.87	51,021	0.027
Oklahoma	32	6.28	1.78	50,686	0.125
Georgia	31	7.13	3.07	41,317	0.419
Oregon	31	6.52	1.26	38,149	0.323
Alabama	29	6.24	1.98	39,232	0.655
Washington	29	7.14	1.33	40,635	0.310
Kansas	28	6.29	2.17	46,059	0.179
Massachusetts	27	11.59	4.04	59,269	0.889
Tennessee	25	6.24	1.85	29,224	0.160
Iowa	24	6.46	1.14	38,993	0.708
Kentucky	22	6.68	3.03	32,797	0.364
Mississippi	20	6.00	1.17	32,224	0.600
Utah	20	5.60	0.94	36,235	0.300
South Carolina	20	7.05	1.54	31,429	0.000
Arizona	19	6.95	0.62	124,604	0.053
Louisiana	18	6.50	2.43	63,933	0.944
Colorado	18	7.78	1.44	48,036	0.111
Arkansas	16	7.81	1.72	34,132	0.813
Virginia	14	6.57	1.34	37,399	0.071
Maryland	13	6.23	2.01	26,524	0.538
Connecticut	13	12.31	6.24	66,286	0.462
Nebraska	10	8.00	0.82	72,057	0.900
New Mexico	10	7.40	1.96	61,417	0.100
Maine	9	7.78	1.64	22,817	0.444
West Virginia	9	9.22	2.05	23,766	0.222
New Hampshire	8	10.75	3.11	25,927	0.250
Idaho	7	5.86	0.90	29,120	0.429
South Dakota	7	7.14	2.12	27,539	0.286
Wyoming	6	8.00	1.55	26,825	0.667
North Dakota	6	8.17	4.92	39,681	0.500
Montana	6	8.17	2.79	37,642	0.167
Nevada	6	4.83	0.41	73,115	0.000
Rhode Island	4	7.50	1.91	63,082	0.750
Delaware	2	8.00	2.83	26,364	0.500
Vermont	1	11.00		18,230	1.000
Alaska	1	9.00		26,751	0.000

Table 3. Regressions for log of government expenditures per capita

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Additional Indicator (*M*)			Veto over ordinances	Veto over resolutions	Veto over appropriations	Line item veto	Present budget	Appoint dept. heads	Not member of the council
Log (council size)	0.100**	0.176***	0.174***	0.175***	0.176***	0.175***	0.164**	0.170**	0.178**
	(0.048)	(0.066)	(0.067)	(0.067)	(0.067)	(0.067)	(0.069)	(0.070)	(0.071)
Veto		0.430**	0.658*	0.539**	0.479**	0.379**	0.526***	0.397**	0.479**
		(0.168)	(0.344)	(0.234)	(0.205)	(0.192)	(0.201)	(0.199)	(0.223)
Veto x log (council size)		-0.202**	-0.336**	-0.260**	-0.225**	-0.174*	-0.252**	-0.183*	-0.214*
		(0.085)	(0.168)	(0.119)	(0.104)	(0.097)	(0.102)	(0.101)	(0.113)
M			-0.275	-0.177	-0.139	0.189	-0.227	0.026	-0.058
			(0.349)	(0.260)	(0.271)	(0.303)	(0.225)	(0.201)	(0.217)
M x log (council size)			0.163	0.093	0.064	-0.101	0.114	-0.024	0.009
			(0.168)	(0.126)	(0.129)	(0.143)	(0.109)	(0.099)	(0.110)
ethnic90	0.647***	0.641***	0.648***	0.645***	0.644***	0.647***	0.646***	0.637***	0.635***
	(0.102)	(0.102)	(0.098)	(0.098)	(0.098)	(0.098)	(0.099)	(0.099)	(0.099)
ipc90	0.013***	0.013***	0.013***	0.013***	0.013***	0.013***	0.012***	0.013***	0.012***
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
mmi90	0.518***	0.530***	0.520***	0.522***	0.525***	0.528***	0.545***	0.516***	0.533***
	(0.101)	(0.100)	(0.093)	(0.093)	(0.093)	(0.093)	(0.094)	(0.093)	(0.093)
pop65up	0.015***	0.015***	0.015***	0.015***	0.015***	0.015***	0.015***	0.015***	0.015***
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
popqti1	-0.994***	-0.971***	-0.945***	-0.940***	-0.957***	-0.955***	-0.968***	-0.979***	-0.986***
	(0.354)	(0.353)	(0.336)	(0.336)	(0.336)	(0.336)	(0.342)	(0.338)	(0.336)
popqti2	-1.010***	-1.009***	-1.000***	-0.989***	-1.003***	-0.998***	-0.953***	-1.026***	-0.978***
	(0.233)	(0.233)	(0.230)	(0.231)	(0.231)	(0.231)	(0.234)	(0.232)	(0.231)
popqti3	-0.525***	-0.519***	-0.504***	-0.498***	-0.502***	-0.499***	-0.516***	-0.539***	-0.487***
	(0.164)	(0.164)	(0.144)	(0.144)	(0.145)	(0.144)	(0.147)	(0.146)	(0.145)
popqti4	-0.096	-0.091	-0.093	-0.092	-0.096	-0.092	-0.083	-0.095	-0.087
	(0.088)	(0.088)	(0.085)	(0.085)	(0.086)	(0.085)	(0.087)	(0.086)	(0.085)
popqti5	0.023	0.024	0.023**	0.025**	0.024**	0.024**	0.024**	0.024**	0.025**
	(0.018)	(0.018)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Constant	-1.632***	-1.771***	-0.097	-0.102	-0.103	-0.104	-1.792***	-1.719***	-0.109
	(0.176)	(0.188)	(0.462)	(0.462)	(0.462)	(0.462)	(0.463)	(0.461)	(0.466)
Observations	1445	1445	1444	1444	1444	1444	1420	1429	1431
R-squared	0.39	0.39	0.39	0.39	0.39	0.39	0.39	0.39	0.39
F statistic		0.16	1.05	0.66	0.33	0.00	0.98	0.02	0.10
p-value		0.688	0.306	0.417	0.566	0.994	0.322	0.888	0.754

Dependent variable is the log of per capita government exenditures. "Veto" is an indicator variable equal to 1 if the mayor has authority to veto council-passed measures. In addition to the veto variable, other indicators, denoted by *M*, are included to test for their incremental effect. Columns (3) to (6) test for the incremental effects of specific types of vetoes, while the remaining columns test for incremental effects of other indicators of mayor powers. See Summary Statistics table for definitions of other variables. The F-statistic reports the test for the hypothesis that the sum of coefficients on log(councl-size) and veto x log(council-size) is zero. Popqti1 to Popqti5 are population quintiles (expressed in 100,000s). Regressions additionally include a complete set of state indicators. Robust standard errors are in parenthesis. * denotes significance at 10%, ** at 5%, and *** at 1%.

Table 4. Regressions for Executive Veto

	(1) OLS	(2) Probit	(3) OLS	(4) Probit
csize	0.075***	0.095***	0.055***	0.083***
	(0.005)	(0.007)	(0.005)	(0.008)
ethnic90	-0.292***	-0.306***	-0.057	-0.118
	(0.071)	(0.084)	(0.073)	(0.107)
ipc90	-0.005***	-0.006**	-0.003*	-0.005**
	(0.002)	(0.002)	(0.002)	(0.003)
mmi90	-0.114	-0.116	-0.133*	-0.170
	(0.081)	(0.095)	(0.073)	(0.104)
pop65up	0.003	0.003	0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.003)
popqti1	0.224	0.345	-0.263	-0.307
	(0.321)	(0.397)	(0.283)	(0.438)
popqti2	0.450**	0.551**	0.002	0.083
	(0.228)	(0.279)	(0.202)	(0.308)
popqti3	0.288*	0.345*	0.076	0.178
	(0.153)	(0.190)	(0.134)	(0.209)
popqti4	0.051	0.060	-0.014	-0.028
	(0.090)	(0.115)	(0.079)	(0.127)
popqti5	0.010	0.012	0.018**	0.033*
	(0.011)	(0.018)	(0.009)	(0.020)
State indicators	No	No	Yes	Yes
Observations	1676	1676	1676	1676
R-squared	0.17		0.39	
F-stat (14, 1621)			0.56	
p-value			0.89	
Chi-squared (14)				13.07
p-value				0.52

Dependent variable = 1 if Mayor of city government has veto authority. OLS refers to the Linear Probability Model. Probit estimates report the slope effects implied by the estimated coefficients estimated at the sample means--the reported significance is with respect to the underlying coefficient being statistically different from zero. The F-statistics and Chi-squared statistic refer to the joint test of the coefficients on indicators for excluded states being equal to zero for each specification. Standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5. Instrumental Variables Estimation for Executive Veto

	Without state indicators			With state indicators		
	(1)	(2)	(3)	(4)	(5)	(6)
Estimation	OLS	2SLS	AGLS Probit	OLS	2SLS	AGLS Probit
Dependent variable	Csize	Veto	Veto	Csize	Veto	Veto
Council size		0.135***	0.156***		0.121**	0.156**
		(0.029)	(0.032)		(0.057)	(0.075)
Number of large streams	0.070***			0.036**		
	(0.014)			(0.015)		
Number of small streams	-0.003***			-0.001*		
	(0.001)			(0.001)		
ethnic90	-2.482***	-0.137	-0.152	-1.092***	0.012	-0.047
	(0.354)	(0.104)	(0.115)	(0.395)	(0.097)	(0.135)
ipc90	-0.050***	-0.002	-0.003	-0.039***	-0.001	-0.003
	(0.008)	(0.002)	(0.003)	(0.008)	(0.003)	(0.004)
mmi90	0.771*	-0.144*	-0.144	0.918**	-0.190**	-0.231*
	(0.397)	(0.086)	(0.097)	(0.368)	(0.091)	(0.127)
pop65up	0.009	0.002	0.002	0.026***	-0.001	-0.001
	(0.009)	(0.002)	(0.002)	(0.009)	(0.003)	(0.004)
popqti1	-12.254***	1.007**	1.148**	-11.241***	0.494	0.532
	(1.748)	(0.497)	(0.570)	(1.564)	(0.710)	(0.965)
popqti2	-5.655***	0.821***	0.934***	-5.572***	0.378	0.503
	(1.302)	(0.295)	(0.343)	(1.164)	(0.385)	(0.532)
popqti3	-4.175***	0.557***	0.623***	-3.709***	0.326	0.456
	(0.877)	(0.204)	(0.238)	(0.783)	(0.256)	(0.356)
popqti4	-1.255**	0.133	0.145	-1.205**	0.070	0.067
	(0.569)	(0.102)	(0.125)	(0.495)	(0.110)	(0.164)
popqti5	0.325***	-0.009	-0.007	0.313***	-0.002	0.011
	(0.065)	(0.014)	(0.020)	(0.059)	(0.020)	(0.031)
Constant	7.621***	-0.432*		6.284***	-0.477	
	(0.490)	(0.251)		(0.470)	(0.372)	
Observations	1676	1676	1676	1676	1676	1676
Test of overidentifying restrictions (Chi^2 (1))	4.02 [0.05]			2.17 [0.14]		
Davidson-MacKinnon test of exogeneity (F(1,1664))		5.02 [0.03]			1.57 [0.21]	
Smith-Blundell test of exogeneity (Chi^2 (1))			4.05 [0.04]			1.10 [0.30]

Notes: AGLS is the Amemiya Generalized Least Squares estimator. Details are provided in text and in Newey (1987). Standard errors for the first stage regressions are calculated using a robust variance matrix. Probit estimates report the slope effects. Test of overidentifying restrictions is a a lagrange multiplier test which corresponds to an auxilary regression of residuals from the IV specification on the instruments. It tests the joint hypothesis that the equation is properly specificied and the instruments are valid instruments. The Davidson-MacKinnon test (see Davidson and MacKinnon (1993)) corresponds to a regression of the dependent variable on the regressors and additionally the residuals from the instrumenting equation. It tests the null that OLS would be consistent--rejection indicates the need for instrumenting. The Smith-Blundell test is the analogous test for the probit model (see Smith and Blundell (1986)). P-values for the test statistics are reported in brackets below the test statistics. Standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6. Additional Regressions for Veto Indicators

	Regressions for Veto over Appropriations				Regressions for Line Item Veto			
	Without state indicators		With state indicators		Without state indicators		With state indicators	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Estimation	2SLS	AGLS Probit	2SLS	AGLS Probit	2SLS	AGLS Probit	2SLS	AGLS Probit
csize	0.133***	0.097***	0.101**	0.090**	0.081***	0.056***	0.067*	0.046*
	(0.024)	(0.020)	(0.049)	(0.041)	(0.019)	(0.015)	(0.040)	(0.027)
ethnic90	0.065	-0.029	0.042	0.008	0.041	-0.026	0.027	-0.006
	(0.089)	(0.079)	(0.084)	(0.075)	(0.069)	(0.061)	(0.069)	(0.051)
ipc90	0.002	0.000	0.001	0.000	0.001	0.000	0.001	0.000
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)
mmi90	-0.137*	-0.111	-0.128	-0.115*	-0.083	-0.067	-0.079	-0.057
	(0.074)	(0.069)	(0.079)	(0.070)	(0.057)	(0.055)	(0.065)	(0.048)
pop65up	-0.001	-0.001	-0.002	-0.002	-0.001	-0.001	-0.001	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)
popqti1	1.162***	0.778**	0.779	0.621	0.581*	0.359	0.329	0.168
	(0.426)	(0.379)	(0.614)	(0.525)	(0.330)	(0.281)	(0.503)	(0.342)
popqti2	0.638**	0.453**	0.458	0.371	0.286	0.196	0.142	0.083
	(0.253)	(0.226)	(0.334)	(0.289)	(0.196)	(0.167)	(0.273)	(0.185)
popqti3	0.470***	0.335**	0.300	0.237	0.168	0.092	0.057	-0.001
	(0.175)	(0.157)	(0.222)	(0.192)	(0.135)	(0.118)	(0.182)	(0.122)
popqti4	0.194**	0.136*	0.136	0.089	0.057	0.026	0.018	-0.008
	(0.088)	(0.080)	(0.096)	(0.086)	(0.068)	(0.059)	(0.078)	(0.057)
popqti5	-0.015	-0.009	-0.003	-0.008	-0.017*	-0.010	-0.011	-0.008
	(0.012)	(0.011)	(0.017)	(0.015)	(0.009)	(0.007)	(0.014)	(0.009)
Observations	1675	1675	1675	1668	1675	1675	1675	1640

For the first four regressions, depedent variable = 1 if Mayor has veto authority over appropriations. For the last four regressions, depdendent variable = 1 if Mayor has line item veto. AGLS refers to Amemiya Generalized Least Squares estimator--see text and Newey (1987) for details. Probit estimates report the slope effects. Standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

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