Districting and Government Overspending

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Theories of government spending driven by a common-pool problem in the fiscal revenues pool predict that greater districting of a political jurisdiction raises the scale of government. This paper presents evidence on this and related predictions from a cross section of city governments in the United States. The main finding is that, when other plausible determinants of government spending are controlled for, greater districting leads to a considerably greater scale of government activity. The results also show that at-large electoral systems do not, and forms of government that concentrate powers in the office of the executive do, break this relationship.

I. Introduction

A central feature of the recent literature on the political economy of government spending is the prominence given to the role of distributive politics—the politics of policies that produce benefits concentrated to a particular group of people and costs that are disbursed over the entire political jurisdiction. Pork barrel projects are a prime example in which federally financed projects produce benefits for one geographical community. As discussed extensively in Weingast, Shepsle, and Johnsen (1981), such politics leads to a bias toward bigger project size and, in general, bigger government. Legislators, when making spending pro-

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posals, fully value the benefits of public spending in their district but internalize only a fraction of the taxation costs.[1] Other recent papers in which the same basic channel affects fiscal performance include Chari and Cole (1993*a*, 1993*b*), Chari, Jones, and Marimon (1997), Hallerberg and von Hagen (1999), and Velasco (1999).

A central prediction that emerges from this class of models is that the greater the number of districts, the greater the overspending bias and, hence, the greater the size of government. The purpose of this paper is to test this and related predictions from a cross section of city governments in the United States. These governments exhibit substantial variation in both their fiscal outcomes and political structures and constitute a good data set for testing theories relating political institutions to fiscal outcomes. City political structures are, and have been, difficult to change, and the resilience in these institutions bolsters our faith in making causal interpretations from the regression results. These data have the additional virtue that cities share a common national institutional setup, and problems in inference arising from unquantifiable historical and institutional factors, which in general plague cross-country studies, are likely to be less. The central empirical findings can be summarized as follows.

First, there is strong evidence that, when city population and other plausible determinants of government size are controlled for, bigger city councils are associated with considerably greater government expenditures per capita. Extensive sensitivity analysis of the main results indicates that the finding is robust to a variety of considerations. When possible concerns of endogeneity are addressed by instrumenting for council size using the size of the city council 30 years ago, the estimated effect becomes stronger in magnitude. The findings are also robust to alternative measures of the size of government: the share of total government expenditures in total city income and local government employment per capita. The results indicate an elasticity of 0.11 of government size with respect to the number of districts: one more political district in the average city (of seven districts) is associated with a 1.6 percent increase in government expenditures per capita.[2] In terms of aggregate government spending, this amounts to an increase of about $0.72 million in the average city. Given a median city budget in the sample of $17.5 million and given that when cities consider redistricting they generally consider changes of more than just one district, these amount to nontrivial effects of political districting on government size.

Given that an overspending bias may arise in legislatures and, more

[1] This is also referred to as the "common-pool" problem in the literature on environmental economics. At a more general level, the overspending bias arises because of districting and generalized taxation.

[2] These estimates are taken from the discussion in Sec. IV*D*.

important, that, ex post, each legislator may prefer a coordinated outcome that entails less spending for all, a central question that emerges is what political institutions, if any, we can put into place to achieve better outcomes. I consider the effects of two such institutions that are purported to limit the effect of districting on government size: (i) at-large electoral systems, in which candidates for office are elected from the entire jurisdiction; and (ii) forms of government that afford strong powers to the office of the executive in the government—the office of the city mayor in the case of cities. I am able to present evidence on both of these mechanisms since there is substantial variation in both of these institutions across cities in the sample and since evidence suggests that these institutions are hard to change.

It is commonly believed that at-large systems, compared to district systems, can curtail pork barrel–type spending by inducing council members to treat the entire city as their constituency. If at-large council members *did* cater to the good of the entire city, the asymmetry in sharing the benefits and costs of public expenditures would be removed and the overspending bias would disappear. The evidence I find contradicts this commonly held view. At-large cities are not less susceptible to pork barrel–type spending than district cities. Many cities in recent years have adopted mixed electoral systems—in which some council members are elected at large and some by district—in an effort to try to capture the best elements of both district and at-large systems. Results for these cities indicate that the effects of both district and at-large council members are slightly exacerbated in mixed electoral systems. One interpretation that these results admit is that in addition to the externalities that council members impose on each other within a group, there are also intergroup externalities that they fail to internalize, hence leading to greater sensitivity of the size of government to the size of the council.

The other institution I examine is the strong-mayor form of city government. Recent literature in the area of budget institutions—the study of how the rules of the game surrounding the budgetary process affect fiscal outcomes—indicates that political institutions that centralize decision-making authority in one figure in the government, as, for instance, in the president of a presidential government system, can reduce the overspending bias.[3] A strong executive can internalize the externalities inherent in legislators' spending proposals and enforce fiscal discipline on the legislature. City governments in the United States come in two predominant forms: (i) the *mayor-council* form, in which the city mayor is generally elected directly from the city population and is the head of the executive branch of the government; and (ii) the *council-*

[3] For a review of the empirical literature, see Alesina and Perotti (1999).

manager form, in which the legislative and executive functions of government are fused into the city council, which may appoint a city manager to administer city services. The relevant difference between the two is that the former concentrates powers in the city mayor, who cannot be fired by the city council and can therefore exert independent influence on the city council. In addition, cities vary considerably in how much power they concentrate in their mayors, for instance, by giving them agenda-setting powers and powers to veto council legislation. Using data on the form of city government and on indicators of mayor powers, I find suggestive evidence that city governments with strong mayors, particularly those that afford their mayors veto powers, are able to break the relationship between districting and the size of government spending.

The paper is organized as follows. Section II briefly discusses the theory and the existing empirical evidence and delineates the contribution of this paper. Section III describes the data used in the paper. Section IV presents the main results with respect to the impact of districting on government size. Section V presents the results from investigating the role of (*a*) electoral systems and (*b*) forms of government in mediating the relationship between districting and government size. Section VI concludes the paper.

II. Related Literature

Weingast et al. (1981) provided one of the earlier formalizations of the common-pool problem in the fiscal revenues pool. They considered the problem in which representatives from legislative districts propose public projects from the national tax revenue pool. One of their results was that, given district tax shares that are nonincreasing in the number of districts, "project scale for any district grows as the polity is more finely partitioned into districts" (p. 654).[4] The basic ingredients to Weingast et al.'s overspending result were districting, a legislative norm of universalism (under which legislators follow a policy of mutual support, making for a coalition of the whole), and generalized taxation.[5] A subsequent criticism of their approach, however, was the universalism assumption. Although in other work (e.g., Shepsle and Weingast 1981) they showed that the expected utility of a legislator running for reelec-

[4] Recent papers that have the same common-pool mechanism at their heart include Chari and Cole (1993*a*, 1993*b*), Chari et al. (1997), Hallerberg and von Hagen (1999), and Velasco (1999).

[5] They also have a different source of inefficiency, which they call the "politicization of expenditures" (that some project costs are politically beneficial as the public outlays provide employment, etc.), but it is not necessary for the result on government scale and districting.

tion is higher when the legislature has a norm of universalism than in the alternative environment of minimum winning coalitions, the lack of a clear voting game made the theory less appealing.[6] Subsequent work by Baron and Ferejohn (1989) and Baron (1991) on legislative bargaining helped to fill this gap. Their framework was later adopted by Persson, Roland, and Tabellini (1997, 1998, 2000) to provide a rich set of predictions relating political institutions to fiscal outcomes. Their work showed that overspending is more likely in parliamentary systems since members of the governing coalition are likely to have veto powers over budget legislation, making the environment like universalism. By comparison, presidential systems are expected to have less government spending because they rely on a separation of powers and afford more powers to an independent executive. Results in this paper shed light on both these sets of results. The basic prediction on the effect of the number of players is readily tested. The analogy between presidential and parliamentary systems on the one hand and mayor-council and council-manager forms of government on the other is used to present evidence on the latter type of issues on separation of powers.

The existing empirical literature is based mostly on cross-country and U.S. state data.[7] The general approach in these papers is to examine how constructed indices of the fragmentation of the budgetary process affect fiscal outcomes.[8] A common overall theme in this literature is that institutions that centralize decision-making authority are associated with smaller budget deficits and quicker fiscal adjustment to adverse shocks. This paper contributes in the following ways. First, I use a sample of local governments in the United States that allows me to greatly increase the degrees of freedom and complements the set of findings pertaining to countries and states. Second, I focus on providing evidence on a central prediction of common-pool models that has not received much attention: the effect of districting on government size. Of the two central predictions that common-pool type models make, that (i) more districts and (ii) a more decentralized legislative decision-making process worsen the outcome, it is the latter that has received most of the attention. One reason for this omission may be that direct tests of this

[6] Of immediate relevance for this paper, Cox and Tutt (1984) provide micro evidence from the study of the Los Angeles County Board of Supervisors for a norm of universalism in the board's budgetary decision making.

[7] Relevant papers in the cross-county literature include Roubini and Sachs (1989*a*, 1989*b*), von Hagen and Harden (1994), Alesina et al. (1999), Hallerberg and von Hagen (1999), Kontopoulos and Perotti (1999), and Bradbury and Crain (2001). For state-level studies, see Alt and Lowry (1994), Poterba (1994), Bayoumi and Eichengreen (1995), Gilligan and Matsusaka (1995, 2001), and Bohn and Inman (1996), among others.

[8] In a different empirical approach, Inman and Fitts (1990) test the predictions of a common-pool model using time-series data for U.S. federal expenditures and revenues for the period 1795–1988. They do not directly test the relationship between districting and government size, but their findings are consistent with those in this paper.

relationship from cross-county or cross-state data are difficult since budgets at the national level are drafted by committees or cabinets and then submitted for approval to the full legislature. In the absence of an explicit theoretical model of these institutions, it is unclear whether by the number of districts we should mean the total strength of the legislature, the number of members in the federal cabinet (or the number of members of the relevant committee), the number of political parties in the government, or some combination of the three.[9] However, one can readily exploit the variation in the size of city councils across U.S. city governments to test this prediction. City councils are relatively cabinet- and committee-free. Hence, they offer a rather clean test of the relationship between districting and government size. The sample of city governments also has the advantage that these governments share a common national institutional environment. There is likely less variation in unobserved institutions across cities in one country than across countries in the world. Finally, I provide evidence on a question that has not yet received much attention: How does a city's electoral system affect the extent of the overspending bias in the legislatures?[10]

III. Data

The basic specification used in the paper is to regress measures of government size on the size of the city council and other determinants of government expenditures. The data have been combined from different sources. Fiscal data are taken from the 1992 Census of Governments conducted by the Census Department. Demographic and income data are taken from the 1990 Census of Population.[11] Data on the political structure of city governments have been combined from (i) a 1990 survey of city governments conducted by an association of local governments in the United States, the International City/County Management Association (ICMA), and (ii) the 1992 Census of Governments,

[9] Kontopoulos and Perotti (1999) look at the issue of the number of players as well as the fragmentation of the budgetary process in affecting fiscal outcomes. They measure the number of players alternatively as the number of political parties in a coalition government and as the number of spending ministries in a government. Using panel data on 20 OECD countries for the period 1960–95, they find that the number of players matters for fiscal outcomes but get some variation in which measure matters: for the 1970s they find that the number of spending ministries matters whereas for the 1980s the number of parties matters. Their results also cannot be compared directly since their dependent variable is the change in expenditures as opposed to the level of expenditures.

[10] However, see recent work by Persson and Tabellini (2001) and Milesi-Ferretti, Perotti, and Rostagno (2002) on national electoral systems and the level and composition of government spending.

[11] The fiscal and demographic data were obtained from the *County and City Compendium 1993* (Slater-Hall Information Products, Washington, D.C.), a data product similar to the Census Department's *County and City Databook 1994* but providing more comprehensive coverage of U.S. cities.

TABLE 1
ATTEMPTED AND APPROVED CHANGES IN CITY GOVERNMENT STRUCTURE, 1980–90

	ATTEMPTED		APPROVED	
TYPE OF CHANGE	Number	Percentage of Total	Number	Percentage of Total
Any change in structure of government	230	16.2	114	8.0
Increase council size	40	2.8	21	1.5
Decrease council size	20	1.4	11	.8
Change to district electoral system	82	5.8	35	2.5
Change to a mixed electoral system	34	2.4	17	1.2
Change the mix between the at-large and district council members	15	1.1	7	.5
Change the form of government	37	2.6	11	.8

SOURCE.—ICMA.
NOTE.—Total number of cities in the sample is 1,420. The sample is smaller than that in table 4 below because of data availability on questions of proposed and approved changes in city government structure.

Government Organization File. The latter source provides information for fewer variables but for a greater number of cities.

Size of the city council is measured as the number of officials elected to the chief governing body of the government (Csize) and varies from a minimum of three to a maximum of 50 in the data set. Central to the empirical analysis is the assumption that council size is costly to change. Both theoretical and empirical arguments support this assumption. Theoretically, a change in the number of districts almost always has to be approved by the *incumbent* legislators. The redistricting inherent in such a change reapportions the incumbent council members' constituencies and introduces uncertainty in their reelection prospects. In their influential study of the world's electoral systems, Taagepera and Shugart (1989) convey this point well when they discuss the resilience in electoral laws: "Reforms usually require the approval of current assembly members. But these are by definition the very people whom the current electoral system has served well. Why should they want to change a system that got them elected?" (p. 5). At a practical level, there are significant costs involved in changing a political institution such as the size of the council. Typically the process involves a proposal brought forward either directly by the voters if the city has a provision for initiative or by the council, extensive discussion of the merits and demerits of change in the size of the council and the likely impact of a change on representation (with a commission being appointed sometimes to consider the issue at length), and approval by the council or the city population (by a referendum) or both.

Direct evidence also shows that the size of the city council is difficult to change. Table 1 summarizes the information from the ICMA data on

TABLE 2
CORRELATION BETWEEN MEASURES OF GOVERNMENT SIZE (N=1,987)

	Government Expenditure per Capita	Government Expenditure as Share of Total City Income
Government expenditure as share of total city income	.886	
Government employment per capita	.774	.781

the number of attempted and successful changes in city political structure in the 10 years preceding 1990. In the table, any change in structure of government refers to any kind of reform that the council considers and includes measures unrelated to the size of the council and electoral laws. More relevantly, table 1 shows very few attempts (4.2 percent of the sample) to change the council size, and of these, there is roughly a one-half probability of success. Most of the variation in council size can therefore be assumed to be coming from historical reasons. The table also shows that the electoral system and the form of government display considerable inertia.

I measure the size of government in three alternative ways: (i) government spending per capita (Exppc), (ii) government spending as a share of total city income (Expsh), and (iii) government employment per capita (govempl). All three measures have been used in the empirical literature on the size of government. Consistent findings across these measures would indicate a general relationship, not sensitive to a particular measure of the size of government. These three measures are quite highly correlated as shown in table 2.

A number of other plausible determinants of city spending are used as control variables. City size, as measured by population, is used to address economies of scale considerations and is considered in detail in Section IV. I also control for the racial heterogeneity of the city population using an index of racial fragmentation (ethnic):

$$\text{ethnic} = 1 - \sum_i s_i^2,$$

where s_i denotes the share of population of race i in the total city population, and

$i \in \{\text{white, black, American Indian, Asian and Pacific Islander, other}\}$.

The index ranges from zero (complete homogeneity) to one (complete heterogeneity) and can be interpreted as the probability that a randomly selected person from the city population will belong to a different racial

group than another randomly selected person. The racial categories are taken from the 1990 census. In the sensitivity analysis, I also control for a similar measure of heterogeneity for the council, using data on council members by race. The additional control variables are per capita income (incomepc), educational attainment as measured by the percentage of population with a bachelor of arts or higher degree (BAgrad), and income inequality in the city as measured by the ratio of the mean to median household income (MMI90).[12] Income and educational attainment are likely determinants of the demand for public services. The inequality variable is included since the size of government may respond to redistributive pressure arising out of income inequality. Table 3 shows the summary statistics on all the variables used in the study.

It is useful to consider what drives the variation of council size in the sample. Theoretically, the most obvious determinant is city population. Bigger jurisdictions should, and do, have bigger councils. Regressing council size on city population (in millions) yields the following equation (standard errors are in parentheses):

$$\underset{}{\text{Csize}} = \underset{(0.07)}{6.62} + \underset{(0.29)}{5.36}\text{Pop90}, \quad R^2 = .15, \; N = 1{,}972.$$

The slope coefficient indicates that although bigger cities have bigger councils, the effect is considerably small in magnitude. An increase in the city population from 10,000 to 100,000 would be associated with an increase in the council size from 6.7 to 7.2—a fairly small effect.[13] The small magnitude of effect is consistent with the evidence on the infrequency of changes in council size: over time, while city populations may have changed considerably, council size has changed less frequently, leading to a small slope coefficient in the 1990 cross section. The other important sample correlates of council size are the state in which the city is located and the city's ethnic and income heterogeneity. State matters because city councils derive their authority from state governments, which vary in their laws governing local governments. Running the regression above with a complete list of state indicator variables yields an adjusted R^2 of .41, whereas the estimated coefficient on population remains virtually unchanged (and highly significant). To the extent that preferences for public services are correlated along ethnic

[12] For a subset of the sample I had data on the income-based Gini coefficient and on the city unemployment rate. The findings on inequality are robust to either measure used. The unemployment variable was used to control for government spending responding to unemployment for standard Keynesian reasons. The results on council size were robust to including the unemployment rate in the regression.

[13] The same equation estimated in log-log form yields an elasticity of council size with respect to city population of 0.11. Taagepera and Shugart (1989, chap. 5) estimate a similar equation for a cross section of countries in 1985 and report an elasticity of legislature size (lower house) with respect to country population of 0.33.

TABLE 3
SUMMARY STATISTICS

Variable	Units	Minimum	Maximum	Median	Mean	Standard Deviation	Number of Observations
City government expenditures per capita	$1,000 per capita	.020	7.836	.641	.791	.539	1,991
City government expenditures, as a percentage of total city income	Percentage	.078	44.660	4.933	5.973	4.123	1,991
City government employees per capita	Employees per 1,000 population	.429	98.873	9.746	12.101	8.835	1,996
Council size	Number of people	3	50	6	6.859	2.888	2,342
Council size, 1960	Number of people	5	9	7	6.505	1.517	465
Ethnic	Fraction	.004	.730	.187	.235	.173	3,146
Council-ethnic	Fraction	0	.720	0	.122	.180	1,779
City population	Number of people	10,005	7,322,564	21,099	45,540	173,103	3,146
Income per capita	$10,000 per capita	.438	6.330	1.386	1.528	.597	3,146
%BAgrad	Fraction	.007	.909	.188	.225	.129	3,146
Ratio of mean to median household income	Ratio	.986	4.777	1.213	1.248	.185	3,146
Council members elected by district	Number of people	0	50	0	2.856	4.048	2,342
Council members elected at large	Number of people	0	16	5	4.003	2.731	2,342
Mayor-council form of government	Indicator variable	0	1	0	.378	.485	1,696
Mayor-council form of government, 1960	Indicator variable	0	1	0	.359	.480	473
Mayor elected directly from city population	Indicator variable	0	1	1	.775	.417	1,696
Mayor proposed budget to council	Indicator variable	0	1	0	.173	.378	1,696
Mayor appoints department heads	Indicator variable	0	1	0	.232	.422	1,696
Mayor can veto council-passed measures	Indicator variable	0	1	0	.345	.475	1,696
Mayor can veto specific items of appropriations	Indicator variable	0	1	0	.086	.281	1,696

SOURCE.—The 1992 Census of Governments; 1990 Census of Population; 1992 Census of Governments, Government Organization File (all Census Department); a survey of city governments conducted by the ICMA; and the *County and City Compendium 1993* (Slater-Hall Information Products, Washington, DC).

NOTE.—Cross-sectional city government data pertain to 1990, unless otherwise stated. Ethnic is an index of racial heterogeneity for the city population, ranging from zero (least heterogeneity) to one (most heterogeneity). Its construction is described in the text. Council-ethnic is a similar measure for the city council using data on council members by race. %BAgrad is the percentage population over 25 with a college or higher educational degree. For the indicator variables, the variable equals one (and zero otherwise) if the corresponding statement is true. The last five variables on the powers of mayors are classified further in table 9 below. The text provides further details on all variables.

and income lines, we should expect greater demand for political representation in more heterogeneous jurisdictions for a given population. Regressing council size on population and two measures of heterogeneity (ethnic, as defined above, and income inequality, measured by the ratio of the mean to median income) as well as a complete list of state indicator variables gives

$$\begin{aligned} \text{Csize} = 5.87 + \underset{(0.25)}{5.22}\text{Pop90} + \underset{(0.39)}{1.64}\text{ethnic} + \underset{(0.40)}{1.02}\text{MMI}, \\ \bar{R}^2 = .42, \quad N = 1{,}972. \end{aligned}$$

The regressions for government size reported below control for these measures of income and ethnic heterogeneity, and the coefficients on these variables can be interpreted as capturing their direct impact on government size, when the effects that may go through council size are controlled for.

IV. Results I: Districting and Government Size

Table 4 presents the results of ordinary least squares (OLS) regressions for the size of government on the size of the city council and other variables. The three measures of government size are used in log form because of the presence of large outliers in each of these series and because in this form the coefficients on the council size variable can conveniently be interpreted as the elasticity of government size with respect to the number of electoral districts. The first specification includes only city population as a control variable. Subsequent specifications play close attention to the following sets of factors: (i) other plausible determinants of government size, (ii) the nonlinear effects of city population on government size, and (iii) the presence of state-specific effects correlated with both council size and government size. I discuss each in turn below.

The first specification indicates that the magnitude of effect of council size on government size is close across the three measures of government size. When the control variables are added in the second specification, these magnitudes remain relatively unchanged, indicating, in particular, that the council size effect is not going through two other possible political factors: racial heterogeneity and income inequality. Coefficients on the control variables indicate that government size increases with the racial heterogeneity of the city and the ratio of mean to median income of the city. The first of these two findings is consistent with the results in Alesina, Baqir, and Easterly (1999) and is not explored further here except to note that since the index of racial fragmentation goes up with the effective number of racial groups in the population, it is

important to determine whether bigger councils might simply be proxying for a more heterogeneous population. Results discussed in subsection *E* below indicate that this is not the case. The positive coefficient on the measure of income inequality interestingly relates to a long-standing literature on the relationship between income inequality and redistributive spending.[14] The coefficient on per capita income is consistent with previous studies of local public goods that find the demand for local public services to be income inelastic.[15] Plausible coefficients on the control variables suggest that the empirical model is not grossly misspecified.

A. City Size

The variable with the greatest statistical significance is the logarithm of the city population. Log form was used to capture the presence of outliers in the series. Since city size is strongly correlated with government expenditures and council size, the third specification splits the 1990 population into five quintiles and allows for a different slope coefficient for each interval. The results are illuminating. In small and medium-sized cities, per capita government expenditures decline with city population, consistent with the presence of economies of scale. An increase of 10,000 people in the city population for cities at the smallest quintile is associated with approximately an 18 percent decrease in expenditures per capita—an effect of considerable magnitude. At higher population quintiles the estimated effect becomes weaker in magnitude, eventually turning positive for the very biggest cities, suggesting the yielding of economies of scale to diseconomies.[16] The pattern of coefficients suggests a U-shaped relationship between the size of government and city population. Figure 1, which makes a finer division of city sizes in the sample, confirms this. The vertical bars show average per capita government expenditures by decile of the city population.[17] The connected line shows the average residuals by population decile from a regression of per capita expenditures on all the other variables included in table 4. Both series show a similar pattern and suggest that economies of scale in government spending eventually yield to diseconomies of

[14] See Benabou (1996) for an excellent review of this literature. This relationship is not explored further here since it is not the focus of the paper, but the findings on this variable are consistent in all the specifications.

[15] See, e.g., the review provided by Mueller (1987).

[16] The *F*-tests for the equality of coefficients across the five population quintiles reject at *p*-values of less than .001.

[17] The pattern is not markedly different when median per capita expenditure for the interval is used instead of the mean. Figures for the other two measures of government size yield very similar results.

TABLE 4
OLS Regressions for Government Size

	(1)	(2)	(3)	(4)
	A. ln(Government Expenditures per Capita)			
ln(council size)	.2760***	.3021***	.3203***	.1127***
	(.0384)	(.0383)	(.0384)	(.0373)
ln(city population)	.1515***	.1307***		
	(.0133)	(.0139)		
Ethnic		.1920**	.2550***	.5099***
		(.079)	(.0783)	(.0911)
Income per capita		.2272***	.2339***	.1631***
		(.0355)	(.0359)	(.0349)
%BAgrad		−.6060***	−.5940***	−.3898***
		(.1537)	(.1557)	(.1425)
Mean/median income		.6613***	.6543***	.8524***
		(.0921)	(.0939)	(.0957)
City population:				
1st quintile			−1.8201***	−1.2740***
			(.3143)	(.2821)
2d quintile			−1.2652***	−.9433***
			(.2057)	(.1796)
3d quintile			−.7196***	−.5609***
			(.1425)	(.122)
4th quintile			−.2002**	−.1842***
			(.0808)	(.0691)
5th quintile			.0147***	.0188***
			(.0047)	(.0049)
Constant	−2.4819***	−3.3969***	−1.9632***	−.0401
	(.1436)	(.1825)	(.139)	(.1633)
State fixed effects	no	no	no	yes
Observations	1,972	1,972	1,972	1,972
Adjusted R^2	.10	.15	.14	.39
Standard error of regression	.536	.523	.525	.442
	B. ln(Government Expenditures as a Share of Income)			
ln(council size)	.2855***	.2746***	.2903***	.1100***
	(.0393)	(.0369)	(.037)	(.0367)
ln(city population)	.1225***	.1058***		
	(.014)	(.0136)		
Ethnic		.2791***	.3357***	.5955***
		(.0772)	(.0767)	(.0901)
Income per capita		−.2913***	−.2852***	−.3332***
		(.0338)	(.0339)	(.0343)
%BAgrad		−.7339***	−.7187***	−.5896***
		(.1497)	(.1515)	(.1415)
Mean/median income		.9025***	.8921***	1.0982***
		(.0917)	(.0937)	(.097)
City population:				
1st quintile			−1.3642***	−.7671***
			(.3061)	(.2778)
2d quintile			−1.0219***	−.6553***
			(.201)	(.1771)
3d quintile			−.6137***	−.4491***
			(.1377)	(.1194)
4th quintile			−.1694**	−.1437**
			(.0789)	(.0679)

TABLE 4
(*Continued*)

	(1)	(2)	(3)	(4)
5th quintile			.0123***	.0154***
			(.0047)	(.0045)
Constant	−.193	−.6216***	.5390***	2.2141***
	(.1494)	(.1804)	(.138)	(.1669)
State fixed effects	no	no	no	yes
Observations	1,972	1,972	1,972	1,972
Adjusted R^2	.07	.28	.28	.45
Standard error of regression	.578	.508	.510	.434
	C. ln(Government Employment per Capita)			
ln(council size)	.5082***	.4998***	.4950***	.1322***
	(.042)	(.0407)	(.0408)	(.0374)
ln(city population)	.0345**	.0226		
	(.0146)	(.0146)		
Ethnic		.0702	.0678	.3166***
		(.0889)	(.0887)	(.0892)
Income per capita		−.0375	−.0358	.0081
		(.0376)	(.0375)	(.037)
%BAgrad		−.5437***	−.5205***	−.4514***
		(.174)	(.1749)	(.1561)
Mean/median income		1.4238***	1.4157***	1.1636***
		(.1079)	(.1095)	(.1047)
City population:				
1st quintile			−.357	−.5887**
			(.3331)	(.2865)
2d quintile			−.2609	−.4923***
			(.2278)	(.1883)
3d quintile			−.3917**	−.4735***
			(.152)	(.1179)
4th quintile			−.1462	−.1513**
			(.0941)	(.0714)
5th quintile			.0032	.0159***
			(.0054)	(.003)
Constant	.9901***	−.5121***	−.2178	2.1450***
	(.1544)	(.1984)	(.1573)	(.1772)
State fixed effects	no	no	no	yes
Observations	1,977	1,977	1,968	1,968
Adjusted R^2	.08	.20	.20	.51
Standard error of regression	.590	.551	.552	.432

NOTE.—The dependent variable for each measure of government size is named in each panel. The first specification for each measure includes only council size and the natural logarithm of the city population. The last specification for each measure includes all listed covariates and a complete set of state fixed effects. Ethnic is an index of racial heterogeneity for the city population: higher values represent greater heterogeneity. %BAgrad is the percentage of population over 25 with a college or higher degree. All variables are described in table 3. Population quintiles have population data expressed in 100,000s. The data set is a 1990 cross section of city governments in the United States, and sources are described in Sec. III in the text. Robust standard errors are in parentheses.

** Significant at the 5 percent level.

*** Significant at the 1 percent level.

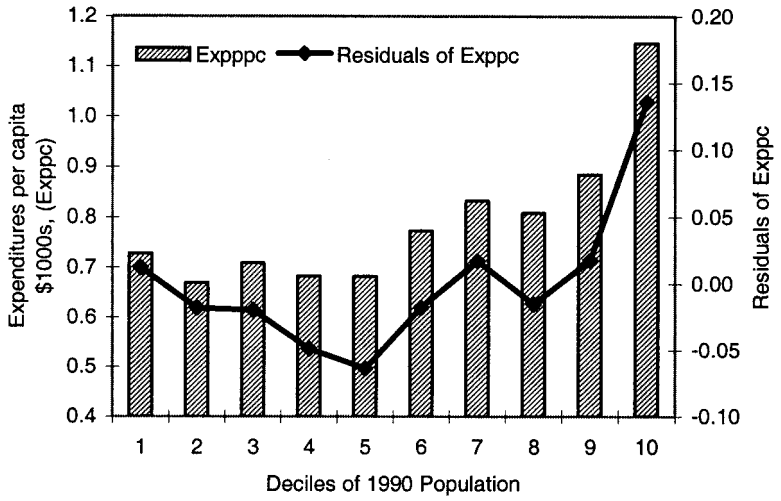

FIG. 1.—Government size by city size

scale. The estimated relationship with respect to council size is not affected when we allow nonlinear effects of population size.

B. State-Specific Effects

Since state-specific factors, such as differing degrees of state fiscal decentralization, are likely to affect both local government spending and local political structure, the fourth specification in table 4 controls for a complete set of state fixed effects. The estimated coefficient on council size drops to little over a third of its value. Figure 2 plots the logarithm of the median per capita city expenditure in a state against the median council size in the state and shows a strong positive correlation.[18] Note, however, the presence of influential observations: Washington, D.C. (with one local government) and the New England states are clustered on the upper-right side of the figure (fig. 3 excludes these states). Washington, being the nation's capital and having the highest level of expenditures per capita, can deserve special treatment. The New England states are the oldest states in the United States with a history of liberal and very democratic local government traditions. Some cities in the New England states also use the town meeting form of local government, which is unique to these states. A closer examination shows that the reduction in the council size coefficient in the fourth specification is

[18] Figures using the other two measures of government size look very similar.

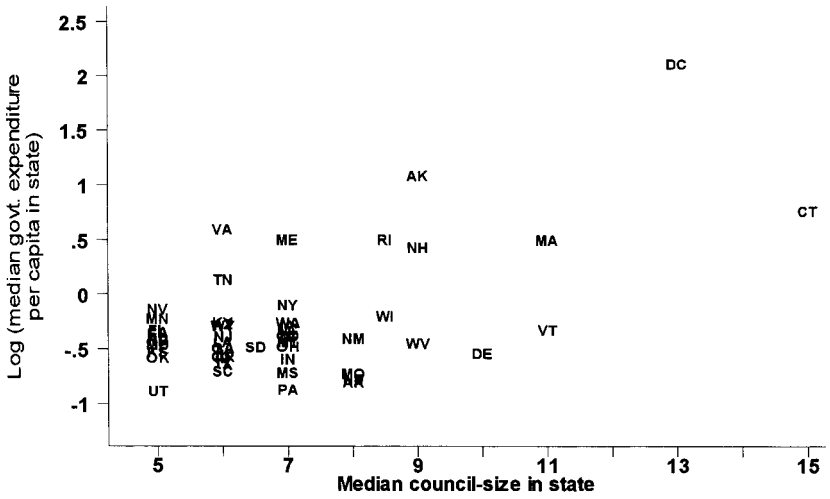

FIG. 2.—Council size and government expenditures: all states

accounted for by the New England effect. Running the fourth specification with only the indicators for the New England states gives close to the same reduction in the coefficient on council size.[19] The results are presented in row 1*a* of table 5. An *F*-test for the equality of the coefficients on the six state indicators does not reject at conventional levels, indicating that we could include one indicator variable for New England states. Row 1*b* of table 5, which reports the coefficients and standard errors on the council size variable when only this indicator is included in the equation, confirms this. Does the relationship between council size and government expenditures survive when we look only at the non–New England states? The subsequent two rows in table 5 show that it does. Row 1*c* reports the coefficients when only non–New England states are included in the sample and state indicators are *not* included. The estimated coefficient for each measure of government size is very close to the full sample regression with all state indicators (col. 4 of table 4). Moreover, inclusion of state indicators in this non–New England sample (row 1*d* of table 5) does not alter the coefficient much for two of the three measures of government size, consistent with the discussion above that the original reduction in the coef-

[19] The New England states are Connecticut, Maine, Massachusetts, New Hampshire, Rhode Island, and Vermont. The same holds if we include in addition an indicator for Washington, D.C.

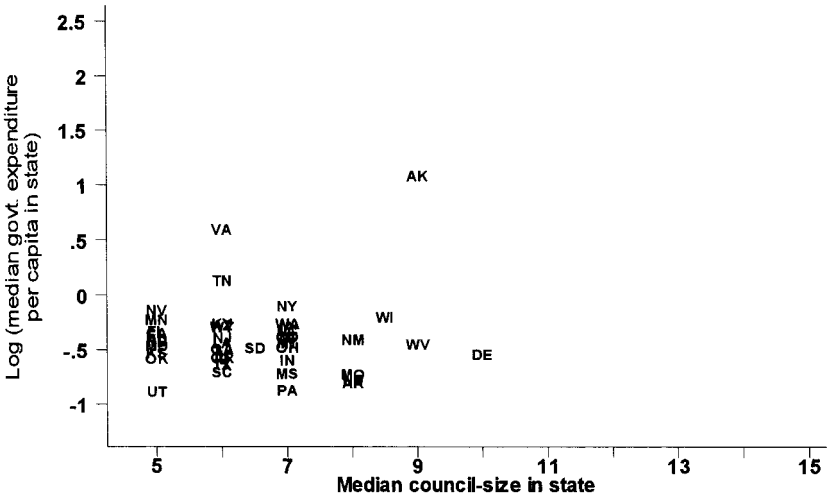

FIG. 3.—Council size and government expenditures: excluding Washington, D.C., and New England.

ficient was coming from the New England states.[20] Table 5 also pulls together results from other specification tests, which are discussed in detail below.

C. *Reverse Causality*

Evidence presented in Section III suggested that council size is costly to change, which should limit concerns that results are contaminated because of reverse causation. However, it is possible that over long periods, council size may have adjusted to incorporate spending preferences of cities.[21] To address these concerns, I present results with instrumental variables, using the size of the city council in 1960 as an instrument.[22] Since we are going a fairly long period back in time, this variable is likely exogenous to the spending decisions in 1990. However,

[20] There are only 92 observations if we look at the New England sample. Regressions for government size in this sample do not give any significant variable except racial heterogeneity. Note that even population is not significant, which indicates a not very good fit of the model.

[21] It is, though, not clear why greater government spending would require bigger councils. The council refers to the legislative function in government, whereas government programs typically fall under the executive branch. Getting more government programs is more likely to mean more government employees than more legislators.

[22] The data come from Aiken and Alford (1972) and were obtained electronically from the web site of the Inter-university Consortium for Political and Social Research, http://www.icpsr.umich.edu, study 0028.

TABLE 5
SENSITIVITY ANALYSIS

	COEFFICIENT ON ln(Council Size) WHEN THE DEPENDENT VARIABLE IS:		
	ln(Expenditure per Capita)	ln(Expenditures as Share of City Income)	ln(Government Employment per Capita)
0. Baseline coefficient	.1127	.1100	.1322
	(.037)	(.037)	(.037)
1. State-specific effects:			
a. Indicators only for New England states	.1284	.1115	.2982
	(.039)	(.038)	(.041)
b. One indicator for New England	.1323	.1147	.2937
	(.038)	(.038)	(.040)
c. Non–New England sample, without state indicators	.1410	.1224	.3218
	(.041)	(.040)	(.043)
d. Non–New England sample, with state indicators	.1294	.1255	.1496
	(.043)	(.042)	(.042))
e. State share in total revenue, with all state indicators	.1200	.1169	.1366
	(.038)	(.037)	(.038)
2. Population growth, 1980–90	.0971	.0972	.1051
	(.038)	(.037)	(.037)
3. Effective number of ethnic groups	.1088	.1114	.1378
	(.049)	(.047)	(.049)
4. Ethnic heterogeneity:			
a. Ethnic ≥ median (.20)	.1374	.1275	.1470
	(.039)	(.038)	(.039)
b. Ethnic < median	.0879	.0924	.1173
	(.039)	(.038)	(.039)
5. Big councils vs. small councils:			
a. Council size > 9	.1361	.1338	.1510
	(.04)	(.039)	(.041)
b. Council size ≤ 9	.1718	.1703	.1797
	(.049)	(.048)	(.052)
6. Big cities vs. small cities:			
a. City population ≥ median (25,555)	.1294	.1266	.1381
	(.038)	(.037)	(.038)
b. City population < median	.0756[a]	.0730[a]	.1191
	(.040)	(.039)	(.04)
7. Central vs. suburban cities (includes indicators for central and suburban cities)	.1464	.1402	.1955
	(.047)	(.045)	(.048)
8. Population density: controls for ln(population density)	.1114	.1084	.1308
	(.037)	(.037)	(.038)
9. Percentage voting for Democratic president	.1144	.1102	.1340
	(.039)	(.039)	(.039)

NOTE.— The table reports results from variations on the fourth specification in table 4 for each measure of government size. Row 0 reproduces the original specification for ease of reference. Only the coefficients (and robust standard errors in parentheses) on the council size are reported to conserve space. Results on the other variables are discussed in Sec. IV*E*. Each regression additionally includes all variables of table 4, including population quintiles and state indicators. Effective number of ethnic groups equals the reciprocal of one minus the racial heterogeneity variable for the city council. For rows 4–6, the council size variable is split according to the conditions in the two subrows. Numbers of observations range from 1,455 to 1,972 depending on data availability for the additional variables.

[a] *p*-value <.06 (for all other estimated coefficients associated *p*-values < .05).

it is available for only 465 cities of the OLS sample. Since there is a considerable change in the sample size, table 6 first reports the OLS and then the two-state least-squares (2SLS) results (both with and without the state indicators). The OLS results show that the statistically significant estimated coefficients (on council size, racial heterogeneity, per capita income, and the ratio of mean to median income) are quite close to the estimates in the full sample of table 4. Thus the 465-observation sample is representative of the full sample. When we instrument for the 1990 value of council size, the estimated coefficient increases in magnitude. If reverse causality was contaminating the OLS results, we would have expected a reduction in the coefficient with the instrumental variable specification. One explanation that can account for the increase is that, over time, cities that have received net population inflows would have benefited from economies of scale in government spending while at the same time would have been under pressure to increase representation of the council. This would lead to a downward bias in the 1990 cross-sectional relationship between council size and government spending, and the instrumental variable results help to recover the causal effect of districting on spending.

D. Discussion

Given these results, it is useful to consider the magnitude of the effect of council size on government size. Focusing on per capita expenditures and using the baseline specification of column 4 of table 4, we can take 0.11 as an estimate of the magnitude of effect. An addition of one council member, then, to an average city council (of seven members) would be associated with a 1.6 percent ($\approx 0.11 \times \frac{1}{7}$) increase in per capita city government expenditures. Given average per capita city spending of \$792, this amounts to an increase of \$12 per capita. In aggregate terms, these coefficients imply that for an average city of 58,000 people, an addition of one political district in the city would be associated with an increase of \$0.72 million ($\approx 0.016 \times 792 \times 58{,}000$) in the city budget.[23] With a median city budget of \$17.5 million, and given that when cities consider changes in the number of seats in the

[23] If anything, this estimate is likely to be an underestimate of the effect on aggregate expenditures. Redoing the regressions with the log of total city government expenditures as the dependent variable gives an estimated coefficient (standard error) of (i) 0.294 (0.061) in the OLS regression of table 4 (with state indicators), (ii) 0.353 (0.191) in the 2SLS specification of table 6 (with state indicators), and 0.381 (0.090) in the OLS regression corresponding to panel B of table 10 below with indicators and interaction for cities with strong mayors. When the smallest of these coefficients (0.29) is used, the estimated effect of an increase of one councilman on the aggregate budget is \$3.1 million at the sample mean and \$0.73 million at the sample median of total expenditures.

city council they typically consider changes of more than one councilman, these are fairly substantial effects.

It is also useful to consider actual government spending in otherwise similar cities that have very different council sizes. Two of the key determinants of council size are state and population. Grouping big cities by state and population and looking for big differences in council sizes gives an illustrative example from Connecticut. Bridgeport and New Haven had very similar levels of population (142,000 and 130,000, respectively), racial heterogeneity (0.571 and 0.573), per capita income ($13,100 and $12,970), land area (16 and 19 square miles), and other indicators in 1990 used in the baseline regression. New Haven, however, has 30 council members, 10 more than Bridgeport. In 1990 it also spent $600 more than Bridgeport. The predicted difference from the regression in per capita spending is $200 for these two cities.

E. Additional Sensitivity Analysis

In the rest of this section I present results from other sensitivity analysis exercises carried out to check the robustness of the basic results. Table 5 pulls together the results from variations on the basic specification of column 4 of table 4.[24] For ease of reference, row 0 repeats the results from table 4. The subsequent four rows were discussed above in the context of state-specific effects. Row 1*e* presents one additional specification to control for state-specific effects: it controls for the share of total revenue coming from state government as a proxy for the degree of state influence in the fiscal affairs of the city. This share varies in the sample from zero to 88 percent with a mean (median) share of 16 percent (13 percent). Row 1*e* shows that the council size coefficients do not change much when we control for this variable. It is important to note that for a good fit between theory and data, most of the revenue needs to come from local sources. If most revenues came from, say, the state government and cities lobbied to get greater revenues transferred down, the appropriate measure of the "number of players" in the common pool would be the number of municipal governments in the state. For the average city, 78 percent of the revenue comes from local sources (median 81 percent). As a final check, I ran the basic regression of table 5 with the log of the locally generated per capita government revenue as the dependent variable. The estimated coefficient (standard error) on ln(council size) is 0.0856 (0.038) with a p-value of .024, very close to the original estimates of the effect on the size of government.

[24] Coefficients on the control variables are suppressed to conserve space. Significant differences in any of these variables are noted in the discussion. Complete results are available on request.

TABLE 6
2SLS Results for Government Size ($N=465$)

	(1)	(2)	(3)	(4)
	A. ln(Government Expenditures per Capita)			
Estimation	OLS	2SLS	OLS	2SLS
State fixed effects	no	no	yes	yes
ln(council size)	.4084***	.6448***	.1630**	.3173**
	(.0832)	(.1319)	(.0751)	(.147)
Ethnic	.2327*	.3013**	.4237***	.4188***
	(.1379)	(.1422)	(.1361)	(.1369)
Income per capita	.2496***	.2716***	.1788***	.1796***
	(.0627)	(.064)	(.0527)	(.053)
%BAgrad	.1634	.1233	.2781	.2754
	(.294)	(.2971)	(.2277)	(.2288)
Mean/median income	.4021**	.4069**	.6143***	.6093***
	(.1883)	(.19)	(.1558)	(.1566)
City population:				
1st quintile	−.7590**	−.7671**	−.5950**	−.5805**
	(.3126)	(.3154)	(.238)	(.2395)
2d quintile	−.2626	−.2675	−.1823	−.177
	(.2015)	(.2033)	(.1516)	(.1524)
3d quintile	−.4312***	−.4429***	−.2719***	−.2804***
	(.1395)	(.1408)	(.1042)	(.105)
4th quintile	−.1957**	−.2023**	−.0421	−.053
	(.0883)	(.0891)	(.0671)	(.068)
5th quintile	−.0012	−.0154	.0275*	.0182
	(.0192)	(.0203)	(.0148)	(.0167)
Constant	−1.7937***	−2.2838***	−1.6949***	−2.0302***
	(.3001)	(.369)	(.4203)	(.5036)
Adjusted R^2	.16	.15	.57	.57
Standard error of regression	.474	.478	.338	.340
	B. ln(Government Expenditures as a Share of Income)			
Estimation	OLS	2SLS	OLS	2SLS
State fixed effects	no	no	yes	yes
ln(council size)	.3956***	.6060***	.1684**	.2996**
	(.0799)	(.1265)	(.0739)	(.1444)
Ethnic	.2595*	.3205**	.4687***	.4645***
	(.1323)	(.1363)	(.1339)	(.1345)
Income per capita	−.2502***	−.2307***	−.3066***	−.3060***
	(.0602)	(.0613)	(.0518)	(.052)
%BAgrad	−.1809	−.2166	−.1271	−.1295
	(.2822)	(.2848)	(.224)	(.2249)
Mean/median income	.7334***	.7377***	.9781***	.9738***
	(.1807)	(.1821)	(.1533)	(.1539)
City population:				
1st quintile	−.6901**	−.6973**	−.5116**	−.4993**
	(.3)	(.3023)	(.2342)	(.2354)
2d quintile	−.2486	−.2529	−.161	−.1566
	(.1934)	(.1949)	(.1492)	(.1498)
3d quintile	−.4062***	−.4166***	−.2542**	−.2614**
	(.1339)	(.135)	(.1025)	(.1032)
4th quintile	−.1853**	−.1912**	−.0432	−.0525
	(.0847)	(.0854)	(.066)	(.0668)

TABLE 6
(*Continued*)

	(1)	(2)	(3)	(4)
5th quintile	−.0043	−.0169	.0209	.013
	(.0184)	(.0195)	(.0145)	(.0164)
Constant	.5793**	.1431	.6093	.3242
	(.2881)	(.3538)	(.4136)	(.4948)
Observations	465	465	465	465
Adjusted R^2	.20	.19	.57	.57
Standard error of regression	.455	.458	.333	.334
	C. ln(Government Employment per Capita)			
Estimation	OLS	2SLS	OLS	2SLS
State fixed effects	no	no	yes	yes
ln(council size)	.6965***	.9087***	.2104***	.3545**
	(.0894)	(.1413)	(.074)	(.1446)
Ethnic	.0629	.1244	.3039**	.2993**
	(.148)	(.1523)	(.134)	(.1347)
Income per capita	−.046	−.0263	.0396	.0403
	(.0673)	(.0685)	(.0519)	(.0521)
%BAgrad	.4991	.4631	.4785**	−.4759**
	(.3157)	(.3182)	(.2241)	(.2252)
Mean/median income	1.0453***	1.0496***	.7374***	.7326***
	(.2022)	(.2035)	(.1534)	(.1541)
City population:				
1st quintile	−.6291*	−.6364*	−.4531*	−.4396*
	(.3357)	(.3378)	(.2343)	(.2357)
2d quintile	−.0984	−.1028	−.0342	−.0294
	(.2163)	(.2177)	(.1492)	(.15)
3d quintile	−.3780**	−.3885**	−.1662	−.1741*
	(.1498)	(.1508)	(.1026)	(.1033)
4th quintile	−.2024**	−.2083**	.0177	.0075
	(.0948)	(.0954)	(.066)	(.0669)
5th quintile	−.0376	−.0504**	.0057	−.003
	(.0206)	(.0218)	(.0146)	(.0164)
Constant	−.1096	−.5494	.9063**	.5933
	(.3223)	(.3953)	(.4138)	(.4955)
Observations	465	465	465	465
Adjusted R^2	.19	.18	.65	.65
Standard error of regression	.509	.512	.333	.335

NOTE.—For each measure of government size and for specifications with and without a complete set of state indicators, the table compares the results of OLS and 2SLS regressions. Council size (in 1990) is instrumented with 1960 council size using historical data from Aiken and Alford (1972). The OLS results are reported since the common sample is much smaller than the regressions of table 4 because of availability of 1960 council size data. Population quintiles have population data expressed in 100,000s. Robust standard errors are in parentheses.

* Significant at the 10 percent level.

** Significant at the 5 percent level.

*** Significant at the 1 percent level.

Row 2 of table 5 includes as an additional regressor the growth in the city population, which can be an important factor because of the implications for city infrastructure. The estimated coefficient on the population growth variable is negative (−0.202 for the expenditure per capita regression) and statistically significant. When I split the population growth variable in a manner similar to the 1990 population variable as described above, I find that most of the effect is coming from the upper quintiles: the coefficients on the first three quintiles of population growth are not significant, whereas there is very little change in the coefficients on other variables. The negative estimated effect is therefore coming from rapidly growing cities in the sample.

Although we control for racial heterogeneity of the city, heterogeneity of the council may be an additional factor affecting government spending. On the one hand, one could envisage that it is not council size per se but the number of racial groups in the city council that, potentially through a similar common-pool type argument, is driving up government expenditures. Since bigger councils are likely to be more heterogeneous, council size may simply be proxying for the number of racial groups in the council. This would imply that government spending should not be related to the number of districts in racially homogeneous councils. On the other hand, one could argue that in addition to electoral districts, spending coalitions get formed along racial groups, so that spending is sensitive to both the number of districts and the number of racial groups in the council. Row 3 presents the results when we additionally control for a measure of the effective number of racial groups in the council.[25] There is very little impact on the coefficient for the council size variable. In addition, the coefficient on the effective number of ethnic groups variable ranges between 0.10 and 0.12 for the three specifications and is significant at 5 percent. These results are more consistent with the latter interpretation: it is not the case that the size of the council is simply proxying for the ethnic heterogeneity of the council. The next two rows (4*a* and 4*b*) show separate slope coefficients for heterogeneous and homogeneous cities and provide further evidence that the effect of districting is present in both.

In the government expenditure regressions, 1,777 of the 1,972 cities in the sample have councils composed of nine or fewer members. To

[25] This is in addition to controlling for the racial heterogeneity of the city population. The effective number of ethnic groups is the reciprocal of one minus the ethnic variable for the city council using data on council members by race. When racial groups are distributed equally, this equals the number of racial groups. When groups are not distributed symmetrically, as in one large group and several small groups, it is less than the number of groups to capture the "effective" number of groups. This is the same variable used by, e.g., Taagepera and Shugart (1989), Ordeshook and Shvetsova (1994), and Cox (1997) in their studies of the effects of electoral systems on the number of effective parties in the legislature.

see whether the relationship holds separately in big and small councils, I estimate separate coefficients on council size for these two groups. Row 5 shows significant effects for both large and small councils. I also estimate separate coefficients depending on whether the city population is less than or greater than the median for the sample (25,555). This is done partly to capture the commonly discussed idea that big cities have their special problems and may attain poorer outcomes for reasons other than the externalities inherent in distributive politics. Results show that (*a*) the magnitude of the effect is stronger in large cities, and (*b*) the statistical significance for the council size variable in small cities drops somewhat, but the coefficients are significant at 6 percent for two of the three measures and at 5 percent for the third measure of government size.

The next two regressions include other potentially omitted variables: inner-city versus suburban versus rural cities and population density. There may be systematic differences between inner cities and suburbs that are correlated with both desired government expenditures and council size. Central city residents typically favor greater public services and, because they are more heterogeneous, may also desire bigger city councils. Suburbs generally have the opposite characteristics. The same effect to some extent can be picked up in the population density variable. Results show that the council size effect is robust to these considerations. I also try to control directly for variation in political preferences by using data on percentages voting for a Democratic president in the 1992 presidential election, assuming that, though many factors are likely to affect a voting decision, residents with innate preferences for big government are, ceteris paribus, more likely to vote for a Democratic candidate. Such voting data are available only at the county level. I mapped each city in the sample to the county it is located in and used the county electoral data as a proxy for the city electoral variable.[26] The results after I control for this variable are shown in row 9 of table 5. There is little change for the council size coefficient, and, interestingly, the coefficient on the voting variable is not significant in any of the specifications, suggesting that other controls in the equation may already be accounting for the variation in political preferences.

V. Results II: Electoral Systems and Form of Government

Table 1 demonstrated that the city electoral system and form of government change relatively infrequently. The purpose of this section is

[26] For this to be a good proxy, it requires that there be a relatively high correlation across cities in a county on voting patterns. In the absence of direct information on how large or small this variation may be, the results on this variable should be interpreted with caution.

to examine whether the relationship between council size and government size varies systematically across these two sets of political institutions.

A. Electoral Systems

The key variation in electoral systems across cities is whether candidates are elected from the entire city or from districts within the city.[27] Of the total number of cities in the sample, 56 percent have at-large systems, 17 percent have district systems, and the remaining 27 percent have a mixed system in which some council members are elected by district and some at large.[28] Traditionally cities had district-based systems. At-large systems were introduced in some cities around the turn of the last century in part because it was believed that they would help to curtail pork barrel–type spending by inducing council members to treat the entire city as their constituency. For instance, Richard S. Childs, an early municipal reformer, noted the following as a criticism of ward systems (and a recommendation for at-large systems): "ward elections notoriously produced political small fry who intrigued in the council for petty favors and sought appropriations for their wards in reckless disregard of city-wide interests and the total budget" (1965, p. 37). In their review of the argument for adopting at-large systems in U.S. cities, Engstrom and McDonald (1986) note that council members elected at large were "expected to make decisions on the basis of what they perceived to be good for the entire city, not just one geographic or social segment of it" (p. 203).

Alternatively, at-large council members, despite running from the whole city, may have "home bases" or particular constituencies comprising subsets of the city population that they seek to distribute expenditures to in exchange for votes.[29] If so, we would expect the same effect from increasing at-large council members as from increasing district council members: an additional at-large council member represents

[27] I have so far been using the terms "council size" and "number of districts" interchangeably. The two need not be the same in cities in which some council members are elected at large. The results in this section will justify the use of council size as the relevant right-hand-side variable.

[28] For most cities, when council members are elected at large, they run from the entire city. A few cities, however, have several multimember districts. Although I do not have the data to distinguish between single-member and multimember district systems, Welch (1990) collected these data in a survey and found that 1.9 percent of her sample were such cities. For empirical purposes, therefore, I take the district electoral systems to mean single-member district systems.

[29] See Uslaner (1985) for a study of Israel's Knesset, an extreme example of an at-large system at the national level in which all representatives are elected from the entire country. He shows that legislators identify themselves with particular constituencies within the country along geographical, ethnic, and religious lines.

an additional player in the pool. Such home bases can develop along dimensions such as ethnicity, income, age, and any other characteristic that can segment the city population. Indeed, in at-large electoral systems, geography may be one of the dimensions along which constituencies for individual council members may form. It will be useful to explicitly state the two contrasting views on the role of at-large council members.

HYPOTHESIS I. At-large council members cater to the common good of the whole city, and district council members cater to the good of their respective districts.

HYPOTHESIS II. At-large council members cater to particular constituencies and face the same asymmetry between benefits and costs of their policy proposals as district council members.

The prediction to be tested is whether the relationship between council size and government size is nonexistent in cities with at-large council members. As a first look at the question, I estimate the following specification:

$$\ln(g) = \alpha_0 + \alpha_1 D_L + \alpha_2 \ln(J) + \alpha_3 D_L \cdot \ln(J) + \beta \cdot Z + \epsilon,$$

where g is a measure of the size of government, D_L is an indicator variable for a council with a majority of the council elected at large, J is the size of the council, and Z are all controls used in table 4. The predictions of hypothesis I are $\alpha_2 > 0$ and $\alpha_2 + \alpha_3 = 0$, and those of hypothesis II are $\alpha_2 > 0$ and $\alpha_3 = 0$.

The differing predictions rest on the estimated coefficient for α_3. Results, presented in panel A of table 7, show that the data reject the first hypothesis for all three measures of government size. For the government expenditure regressions, the estimate of α_3 is not statistically different from zero, indicating the same relationship between districting and government size in both district and at-large majority councils. For the government employment regression, an at-large majority council is associated with a negative intercept effect but a steeper positive relationship between council size and government size. At the sample mean, the net effect of switching to an at-large majority council is an *increase* in government size of 15 percent. The coefficients on the other variables are suppressed to conserve space, but they are close to the estimates in table 4.

Panel B of table 7 takes a closer look at the data. The specification above lumped together pure at-large systems with those mixed systems in which a majority of the council members were elected at large. This panel separates the coefficients across nonmixed and mixed electoral systems. The first three coefficients compare cities with district systems to at-large systems, and the next three coefficients compare mixed systems with an at-large majority to mixed systems with a district majority.

TABLE 7
REGRESSIONS FOR EFFECTS OF ELECTORAL SYSTEMS

	ln(Expenditures per Capita) (N=1,972)	ln(Expenditures as Share of City Income) (N=1,972)	ln(Government Employment per Capita) (N=1,968)
		Panel A	
Majority at large	−.2056	−.1848	−.4662***
	(.1358)	(.1316)	(.1442)
ln[council size (J)]	.1510***	.1475***	.1252**
	(.0507)	(.0493)	(.0504)
Majority at large × ln(J)	.075	.0701	.2051***
	(.0709)	(.0686)	(.0759)
Adjusted R^2	.33	.43	.45
		Panel B	
Pure × majority at large	−.1457	−.1324	−.3717**
	(.1399)	(.1358)	(.1474)
Pure × ln(J)	.1132**	.1107**	.0957*
	(.0495)	(.0483)	(.05)
Pure × majority at large × ln(J)	.0735	.0721	.1754**
	(.0737)	(.0716)	(.0781)
Mixed × majority at large	−.676	−.6248	−.7596
	(.457)	(.432)	(.4716)
Mixed × ln(J)	.1743***	.1700***	.1448***
	(.0502)	(.0489)	(.0504)
Mixed × majority at large × ln(J)	.2953	.2721	.3939*
	(.1961)	(.1856)	(.2192)
Adjusted R^2	.33	.43	.45
		Panel C	
Pure system	.2651*	.2599*	.1896
	(.1555)	(.1515)	(.1586)
Pure × district share × ln(J)	.1190***	.1136***	.1688***
	(.0438)	(.0429)	(.0461)
Pure × at-large share × ln(J)	.1139**	.1139**	.1510***
	(.0494)	(.0484)	(.0521)
Mixed × district share × ln(J)	.2620***	.2547***	.2561***
	(.0731)	(.0716)	(.0725)
Mixed × at-large share × ln(J)	.4129***	.3972***	.4461***
	(.0805)	(.0784)	(.0845)
Adjusted R^2	.33	.43	.45

NOTE.—Each panel corresponds to a different specification. All regressions include the complete set of controls of table 4, including state indicators and population quintiles. Majority at large is an indicator variable for a city council with a majority of at-large council members. J represents council size. Pure is an indicator for an electoral system in which either all council members are elected by district or all at large, and mixed is an indicator for an electoral system in which some council members are elected by district and some at large. District share (at-large share) is the share of district (at-large) council members in the council. See text for the interpretation on the transformed variables in panel C. Robust standard errors are in parentheses.

* Significant at the 10 percent level.

** Significant at the 5 percent level.

*** Significant at the 1 percent level.

Results are consistent with those in panel A. All at-large councils have the same relationship between council size and government spending as pure district councils, whereas mixed systems with a majority of at-large council members have the same relationship as those with a majority of district council members.

The results in panel B of table 7 also indicate that the magnitude of the relationship becomes stronger when we look at mixed systems. The *F*-tests for the equality of coefficients on the council size variable across mixed and nonmixed systems reject at *p*-values of less than .01 for all three measures of government size. This indicates that in addition to an intragroup externality, mixed systems may be associated with an intergroup externality, leading to even less internalization of the costs of spending proposals and hence greater sensitivity of government spending to council size.[30]

Finally, panel C of table 7 takes another look at the same question. The two sets of results discussed above focused on the at-large election of the majority of the council. It is possible that at-large council members may exert influence on spending decisions even when they do not constitute a majority. Such outcomes can come about in universalistic decision-making norms in the legislature when the aggregate decision reflects the desired outcomes of each member of the legislature. The specification estimated in panel C is

$$\ln(g) = \alpha_0 + (1 - D_M) \cdot \left[\alpha_1 + \alpha_2 \frac{J_D}{J} \ln(J) + \alpha_3 \frac{J_L}{J} \ln(J)\right] + D_M \cdot \left[\alpha_4 \frac{J_D}{J} \ln(J) + \alpha_5 \frac{J_L}{J} \ln(J)\right] + \beta \cdot Z + \epsilon,$$

where D_M is an indicator for a city with a mixed electoral system. The advantage of transforming the data on council members in this way is that (*a*) under the null of $\alpha_2 = \alpha_3$ (alternatively $\alpha_4 = \alpha_5$), the independent variable reduces to $\ln(J)$, allowing the estimated coefficients to be compared to the previous specifications; and (*b*) $\alpha_2 > \alpha_3$ (alternatively $\alpha_4 = \alpha_5$) if and only if $\partial g/\partial J_D > \partial g/\partial J_L$. Hence, a comparison of α_2 and α_3 (and α_4 and α_5, respectively) allows us to compare the magnitude of the effect for the two types of council members.[31] The inter-

[30] Chow tests to determine whether coefficients on all control variables should be freed across mixed and nonmixed cities did not reject.

[31] We can also run the regression $\log(g) = \alpha_1 + \alpha_2 J_D + \alpha_3 J_L + \beta \cdot Z + \epsilon$, separating the coefficients across mixed and nonmixed systems as above and testing whether $\alpha_3 = 0$. The magnitudes, however, cannot be compared with the previous set of results because of the loglinear specification. The results are qualitatively the same. The estimated coefficients (*p*-values) for expenditures per capita are the following, with the first two coefficients for nonmixed and the second two for mixed systems: 0.120 (0.029), 0.010 (0.088), 0.019 (0.013), and 0.050 (0.000).

action with D_M estimates the effects separately for mixed and nonmixed councils. The results in panel C of table 7 show (i) no qualitative difference between the effects of the two types of council members and (ii) a different pattern of coefficients for mixed versus nonmixed electoral systems. When districts are compared with at-large systems, there is very little difference in the effects of the two types of council members: F-tests for $\hat{\alpha}_2 = \hat{\alpha}_3$ do not reject at conventional levels for all three measures. In mixed systems, the estimated marginal effect of at-large council members is *greater* than that of district council members, and the difference is statistically significant: F-tests for $\hat{\alpha}_4 = \hat{\alpha}_5$ consistently reject at 5 percent for all three regressions. Comparing the estimated coefficients for each type of council members across mixed and nonmixed systems indicates stronger effects in mixed systems: F-tests for the joint hypothesis $\hat{\alpha}_2 = \hat{\alpha}_4$ and $\hat{\alpha}_3 = \hat{\alpha}_5$ reject at p-values of less than .001 for all three measures of government size.

1. Discussion

The results in table 7 indicate that although critics of district systems may have been right in thinking that district systems contribute to more government spending, they were likely wrong in supposing that at-large council members would not cater to particular constituencies within the jurisdiction. The result, however, is all the more surprising since the electoral system used in at-large city elections is a first-past-the-post system, where voters typically are allowed to cast as many votes as there are seats to be filled and candidates with the largest number of votes are declared the winners. As discussed elsewhere in the literature on electoral systems (e.g., Cox 1997), such systems tend to reduce the number of groups in the legislature. For instance, if there is a majority group and a racial minority group and people vote only for members of their own group, it is possible for the legislature to consist entirely of the majority group. The results show that even though at-large systems may reduce council heterogeneity, council members still seek to target government expenditure to particular groups, resulting in the continuation of pork barrel–type spending. This is consistent with evidence discussed above that the relationship between council size and government size exists in both homogeneous and heterogeneous councils.

The results in table 7 also show that the estimated effects are larger in cities with mixed electoral systems. One concern with this result might be the following: if mixed systems tend to have fewer numbers of at-large and district council members than pure at-large and pure district systems, respectively, and if government size is a concave function of council size, then we would automatically get bigger coefficients in the mixed sample. This turns out not to be the case. Although there is

TABLE 8
MEAN NUMBER OF COUNCIL MEMBERS BY ELECTORAL SYSTEM

	District System	At-Large System	Mixed System
Number elected by district	2.0		5.7
Number elected at large		4.5	2.5

suggestive evidence that government size is a concave function of council size, it is not the case that mixed systems have fewer numbers of both at-large and district council members, as table 8 demonstrates.[32] The table reports the mean number of council members elected by district and at large by electoral system. Mixed systems, on average, have a greater number of district council members (than district systems) and a smaller number of at-large council members (than at-large systems). The predicted changes in magnitude would therefore be in opposite directions; however, the results in table 7 show that for both district and at-large council members, the effects become stronger in mixed councils. The *t*-tests for the equality of means across these two types of observations reject strongly for both district and at-large council members.

B. Form of Government

Cities vary in their form of government and the powers they afford the office of the executive. Table 9 gives a breakdown. Of the 1,696 cities for which the form of government and mayor powers data are available, roughly one-third (641) have the mayor-council form of government. Subsequent rows show that this form is systematically associated with greater powers afforded to the city mayor. Of mayor-council form cities, 98 percent have directly elected mayors and 74 percent give their mayors veto powers, whereas of council-manager cities, 65 percent have directly elected mayors and 11 percent give them veto powers.

One theme in the existing cross-county literature on political institutions and budgetary outcomes is that presidential systems of government with strong executives can enforce fiscal discipline on a legislature otherwise prone to overspending.[33] The variation in political form of government across cities in the United States (mayor-council and council-manager systems) maps quite well to presidential and parliamentary systems, and evidence from cities can relate interestingly to the debate

[32] The regression corresponding to col. 4 in table 4, but replacing $\log(J)$ with J and J^2, yields the following coefficients (standard errors) on the linear and quadratic terms: 0.0263 (0.0086) and −0.0006 (0.00027), respectively. Both coefficients are statistically significant at 5 percent in this regression. However, the quadratic term loses significance in some of the sensitivity analysis regressions of table 5.

[33] See the review by Alesina and Perotti (1999) and other papers in the Poterba and von Hagen (1999) volume.

TABLE 9
DISTRIBUTION OF MAYOR POWERS

	All Cities	Mayor-Council	Council-Manager
Total	1,696	641	1,055
Mayor elected directly from city	1,315	629	686
	(78%)	(98%)	(65%)
Mayor proposes budget to council	293	286	7
	(17%)	(45%)	(1%)
Mayor appoints department heads	394	379	15
	(23%)	(59%)	(1%)
Mayor can veto council-passed measures	585	473	112
	(34%)	(74%)	(11%)
Mayor can veto specific items of appropriations	146	126	20
	(9%)	(20%)	(2%)

NOTE.—Numbers in parentheses are percentages of total. Sample consists of all available observations and, in particular, is slightly larger than the regression sample. There are no significant differences in the pattern if the sample is restricted to the regression sample.

at the cross-county level. In this subsection I examine whether cities with strong executives are able to break the link between districting and government spending. The basic specification I estimate is similar to the one estimated for majority at-large systems above:

$$\ln (g) = \alpha_0 + \alpha_1 D_M + \alpha_2 \ln (J) + \alpha_3 D_M \cdot \ln (J) + \beta \cdot Z + \epsilon,$$

where D_M is an indicator variable either for the mayor-council form of government or for one of the measures of mayor powers in table 9.[34] The hypothesis to be tested is $\alpha_2 + \alpha_3 = 0$.

Panel A of table 10 reports the results when D_M equals one (and zero otherwise) for the mayor-council form of government. First, note that when variation in the form of government is taken into account, the coefficients on the council size variable become stronger than the ones in table 4. This is not due to the change in sample size since running the regressions of table 4 for this sample gives coefficients very close to those reported in table 4. Second, for two of the three measures of government size, the coefficient on the interaction is negative and statistically significant at p-values of less than 8 percent. Coefficients on the other variables are suppressed, but the pattern is very similar to the ones in table 4. Formal tests for the hypothesis $\alpha_2 + \alpha_3 = 0$ do not reject at conventional levels, but the test is not very powerful because of the statistically weak coefficients estimated on the interaction term.

I next ran similar regressions using the indicators of actual mayor powers using the four indicators listed above. The only indicator of mayor powers that was associated with statistically significant results was the overall mayor veto indicator: in all other cases, log council size was

[34] Chow tests for estimating a separate set of coefficients for the control variables do not reject at conventional levels for all three measures of government size.

TABLE 10
REGRESSIONS FOR FORM OF GOVERNMENT

	ln(Expenditures per Capita)		ln(Expenditures as Share of City Income)		ln(Government Employment per Capita)	
	Panel A					
Mayor-council form	.2619		.2165		−.0144	
	(.1716)		(.166)		(.1771)	
ln[council size (J)]	.2213***		.2171***		.1758**	
	(.0701)		(.0673)		(.0687)	
Mayor-council form × ln(J)	−.1601*		−.1407*		−.0156	
	(.0871)		(.0842)		(.0881)	
Observations	1,451		1,451		1,449	
Adjusted R^2	.35		.45		.47	
	Panel B					
Mayor veto	.4665***		.4348***		.2978*	
	(.1746)		(.1684)		(.1772)	
ln[council size (J)]	.2051***		.2008***		.2019***	
	(.0669)		(.0655)		(.0734)	
Mayor veto × ln(J)	−.2222**		−.2060**		−.1391	
	(.0878)		(.085)		(.0895)	
Observations	1,432		1,432		1,430	
Adjusted R^2	.35		.45		.47	
	Panel C					
Specification	OLS	2SLS	OLS	2SLS	OLS	2SLS
Mayor veto	.469	1.411	.500	1.358	.806*	1.295
	(.405)	(1.056)	(.393)	(1.021)	(.426)	(1.104)
ln[council size (J)]	.263**	.624**	.268**	.612***	.678***	.858***
	(.131)	(.225)	(.126)	(.217)	(.137)	(.235)
Mayor veto × ln(J)	−.208	−.668	−.219	−.646	−.359*	−.559
	(.204)	(.513)	(.198)	(.496)	(.214)	(.536)
Observations	343	343	343	343	343	343
Adjusted R^2	.11	.08	.18	.15	.19	.17

NOTE.—Each panel corresponds to a different specification. Mayor-council form is an indicator for whether the government has declared a mayor-council form of government. J represents council size. Mayor veto is an indicator for whether the city mayor has powers to veto council-passed measures. Regressions include all other controls of table 4, except panel C, where state indicators are not included. 2SLS specifications in panel C use 1960 values of council size and mayor-council form as instruments. Robust standard errors are in parentheses.

* Significant at the 10 percent level.

** Significant at the 5 percent level.

*** Significant at the 1 percent level.

close in magnitude to the coefficients reported in panel A of table 10 and was statistically significant, but the interaction term was not. The results with the mayor veto variable are presented in panel B of table 10. For the expenditure measures of government size the results are quite striking: government expenditure does not increase with council size in city governments in which mayors have veto powers. For the government employment regression, the p-value associated with the coefficient on the interaction term is .12.

It is also interesting to note that the coefficient estimated on the indicator for a mayor veto is positive and statistically significant. When the number of districts is held constant, the effect of switching to a strong-mayor form is positive for small councils and negative for big

councils. At the sample median of a seven-member council, the switch from a weak- to a strong-mayor form entails a 3 percent increase in expenditures per capita. Note that the prediction of common-pool models refers to the slope effect—government size increases with districting—and it continues to be the case that governments with executive veto do not exhibit this relationship.

A potential problem with the results might be the possible endogeneity of the mayor veto variable. If cities that are more prone to having fiscal problems choose strong-mayor forms of government, then the coefficient on the mayor veto is likely to be biased upward. Available evidence, however, indicates that this is not likely. The Aiken and Alford (1972) study, which was used to instrument for council size, also has information on the form of city government in 1960, which is a good predictor for whether the city's mayor in 1990 has veto powers. For the common sample of 473 observations, regressing 1990 mayor veto on 1960 form of government, all control variables used in the regressions above, and a complete set of state indicators yields highly statistically significant positive coefficients on the 1960 variable for both the linear probability model and the probit model. A switch to a mayor-council form of government in 1960 is associated with a 0.40 increase in the probability of having mayor veto in 1990 for the linear model and a 0.53 increase in probability for the probit model. I next instrument for the mayor veto variable using the 1960 form of government and run the same regression.[35] These results are displayed in panel C of table 10. For all three measures of government size, the results indicate that the coefficient on the mayor veto variable becomes stronger in magnitude in the instrumental variable specifications. The standard errors on the mayor veto variable are also bigger, but the coefficient increases by about three times for the per capita expenditures regression.[36] If reverse causality was accounting for the positive coefficients in the OLS results, we would have expected the opposite. Although this finding is not pursued further here, exploring the positive coefficient on the mayor veto indicator would make for interesting future research.[37] For the present purposes, I note that addressing endogeneity considerations

[35] The sample size drops to 343 from 473 as government expenditure variables are additionally included.

[36] These regressions include all variables in the standard specification, including population quintiles, but do not include state indicators. With the considerably smaller degrees of freedom, regressions with a complete list of state indicators do not give any significant coefficients except per capita income. The direction of the change in the coefficient on the mayor veto variable, however, is the same when state indicators are included.

[37] One possibility is that although strong mayors are able to enforce fiscal discipline on the council, they may themselves indulge in patronage-related spending by virtue of their strong position.

using the form of government 30 years ago as an instrument strengthens the results on the council size variable as well as the interaction.

VI. Conclusion

The purpose of this paper was to test whether the size of government is sensitive to the number of people making spending proposals out of a common revenue pool. Evidence from U.S. cities shows that scaled measures of government size do indeed go up with the number of legislators in a city government. The finding is robust to consideration of a number of possibly omitted variables and specifications. When concerns of potential reverse causality are addressed by instrumenting with the size of the city council 30 years go, the estimated magnitude of the effect becomes stronger. An estimate for the elasticity of government size with respect to the number of districts is 0.11. The findings also show that government size increases with the racial heterogeneity of the city, with a measure of the skewness of the income distribution, and that it decreases with city population in small cities but increases in large cities. Given the basic finding, the paper also looked at whether councils (*a*) dominated by legislators elected at large and (*b*) with strong mayors are able to break this relationship between the number of players and government size. The evidence suggests "no" to the first question and "yes" to the second.

Given the relatively good fit between theory and the data at hand, these findings are important for policy. They argue, first of all, that for any government, keeping the fiscal house in order depends in large part on how many people get to spend out of the tax revenue pool. When more people are added, everybody, including the incumbents, raises his or her spending decisions. Second, they show the relative fruitlessness and relative usefulness, respectively, of the following two ways to contain this spending tendency: electing council members at large and giving executives strong powers. Given that these city governments share many features with national governments, these findings have interesting implications for countries or states considering changes in the political process to address chronic fiscal problems. They imply that whether legislators run for office from the entire jurisdiction or from geographical districts within it, their decisions with respect to government spending do not differ: giving the executive veto powers may be a better way to enforce discipline on the legislature.

These findings, however, have left certain areas unaddressed. First is the hard issue of welfare consequences. Lower spending may be beneficial for all if the point of departure is a state of overspending arising from a common-pool problem and if it is brought down for all districts. It may not be beneficial if existing spending is too low or the distribution

of reductions across districts is skewed. When some districts lose more spending than others, the welfare consequences are unclear. Thus strong executives may reduce the distortion on the size of the budget, yet they might introduce distortions in the distribution of spending if they cater to a minimum winning coalition. These issues are hard to address with the data at hand since city government budgets are not broken down by district. Such analysis may be possible, however, at the federal level looking at the relationship between the changes in the size of the federal budget and its distribution across congressional districts.

Another important area of research is to take one step further back and examine the question, Why do some cities choose strong-mayor forms of government? Such an exercise addressing the important question of why certain political institutions get chosen is admittedly difficult to address using cross-country data because of the role of history and unquantifiable factors, but it can be usefully addressed using the city government data at hand. Over the course of their history, cities have occasionally changed their form of government. If the arguments presented in this paper have some bearing on reality, one factor that would be important for a city deciding the form of government would be the size of the council. If the size of the council is relatively exogenous because it may depend in part on the city population and if it is the case that bigger councils are going to have greater spending pressures, cities with large legislatures should choose strong-mayor forms of government. Although OLS and probit regressions show a positive and significant effect of council size on alternative indicators for strong-mayor forms of government, the empirical challenge is to identify the effect of council size on the form of government since both may be the result of some third outside factor (such as a movement for "reform" that entailed changes to both council size and form of government). Further work in this area would shed additional important light on these issues.

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