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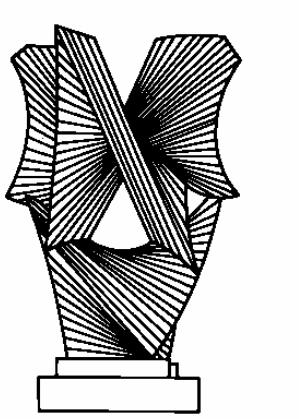

# The Fiscal Consequences of Electoral Institutions

*Christopher R. Berry and Jacob E. Gersen*

THE LAW SCHOOL
THE UNIVERSITY OF CHICAGO

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# The Fiscal Consequences of Electoral Institutions

Christopher R. Berry[*]

Jacob E. Gersen[**]

**Abstract**

There are more than 500,000 elected officials in the United States, 96 percent of whom serve in local governments. Electoral density—the number of elected officials per capita or per governmental unit—varies greatly from place to place. The most electorally dense county has more than 20 times the average number of elected officials per capita. In this paper, we offer the first systematic investigation of the link between electoral density and fiscal policy. Drawing on principal-agent theories of representation, we argue that electoral density presents a tradeoff between accountability and monitoring costs. Increasing the number of specialized elected offices promotes issue unbundling, reducing slack between citizen preferences and government policy; but the costs of monitoring a larger number of officials may offset these benefits, producing greater latitude for politicians to pursue their own goals at the expense of citizen interests. Thus, we predict diminishing returns to electoral density, suggesting a U-shaped relationship between the number of local officials and government fidelity to citizen preferences. Using a county-level dataset of all elected officials in the United States, we evaluate this theory along with competing theories from the existing literature. Empirically, we find evidence that public sector size decreases with electoral density up to a point, beyond which budgets grow as more officials are added within a community.

[*] Assistant Professor of Public Policy, The University of Chicago

[**] Assistant Professor of Law, The University of Chicago. We are grateful for very useful comments from Avinash Dixit, Nicole Garnett, Roger Myerson, and Eric Posner. Thanks to Peter Wilson for excellent research assistance. Financial support was provided by the John M. Olin Foundation, the Lynde & Harry Bradley Foundation, and the Robert B. Roesing Faculty Fund.

## INTRODUCTION

The ability of citizens in the United States to select their governing officials varies enormously from place to place. For instance, Lake County, Illinois is governed by 1,125 local officeholders, whereas the similarly populated Mecklenburg County, North Carolina has only 67 elected officials. This paper explores how variation in the structure of local electoral institutions exacerbates or mitigates agency problems between voters and elected officials in local fiscal behavior. Specifically, we model the taxing and spending of local governments as a function of the number and nature of elected officials within a jurisdiction. Our central claim is that the addition of elected officials often unbundles policy issues so as to produce greater voter control over all elected officials. However, because monitoring elected officials entails costs, having too many elected officials in a jurisdiction can sometimes worsen agency problems and produce greater slack in the voter-politician relationship. We suggest this dynamic produces a U-shaped relationship between the number of elected officials in an area and fidelity to voter preferences, for our purposes preferences over taxing and spending.

Our theory and data capture essential elements of the relationship between electoral institutions and public policy not previously emphasized or analyzed. For example, the existing literature contains conflicting views of the relationship between the size or structure of government and the level of taxing and spending. Different theoretical and empirical camps insist the relationship is positive, negative, or nonexistent. In part, we suggest this disagreement results from a lack of precision or nuance about the different types of elected officials.

Electoral institutions come in many shapes and sizes. There are more than 500,000 elected officials in the United States, or roughly one representative for every 600 inhabitants. The vast majority of these elected officials—96 percent—are in local governments. In addition to electing members of local governing bodies, such as city councils, county commissions, and school boards, voters choose myriad officials in the local executive and judicial branches, including mayors, judges, sheriffs, and treasurers, to name only a few. This paper models fiscal policy-making in local government as a function of the number and type of elected officials. We argue that the results have broad implications for the practice of democracy at all levels of government.

## I. Background and Theory

In textbook theories of democracy, elections ensure that policy outcomes are a rough match to majoritarian or median voter preferences (Dahl 1989; Sen 1983). Yet, this idealized view of elections as translating popular preferences into public policy has long-sine faltered, and it has done so for many reasons discussed extensively in the literature (Gailmard and Jenkins 2006). Voters may be ignorant or have worse information than legislators (Downs 1957, Arnold 1993). The whole notion of popular will might be either incoherent or nonexistent (Campbell et al. 1960, Riker 1981, Zaller 1992). And public choice theory in general provides no shortage of reasons to be dubious of the political process, including elections (Mueller 2003).

Perhaps most important, the voter-legislator relationship is riddled with agency problems (Lupia and McCubbins (1998:79)), and we therefore adopt the standard principal-agent framework. The agenda control exercised by elected officials may allow

legislators to enact policy that systematically diverges from voter preferences (Romer and Rosenthal 1982). So long as representatives propose a new policy that is far from voter preferences but less far than the status quo ante (existing policy), voters may not be able to sanction representatives effectively. Alternatively, because representatives will often have or develop expertise that voters lack, legislators will have a significant degree of discretion as well (Gailmard and Jenkins 2006). If voter information is worse than legislator information, voters will often not be able to tell whether a policy that diverges from their own preferences diverges for good reasons (legislator expertise) or bad reasons (divergent legislative preferences or self-interest).

Once the voter-politician relationship is located in the principal-agent framework, the role of elections in democracy becomes somewhat clearer. Elections are simply a mechanism for managing agency problems, and the efficacy of elections as a mechanism for controlling officials will vary with different institutional arrangements and political conditions. Elections provide a mechanism for voters both to select representatives that will take desirable actions (Fearon 1999), and sanction legislators who fail to enact policy consistent with voter preferences (Barro 1973; Ferejohn 1986; Banks and Sundaram 1998, 1993).

If so, then the risk of drift between voters and policy outcomes is real, but the extent of slack depends on the nature and specific structure of the principal-agent relationship in any given jurisdiction. As a simple example, elections might be more or less frequent. More frequent elections should provide greater control over elected officials, but also impose greater participation costs on voters. An official elected to, say, a twenty-year term might be able to ignore the will of voters for long stretches of time.

An official facing reelection each month would need to be more vigilant in pleasing voters, but at the same time voters would need to expend more effort on electoral participation.

To understand the impact of electoral institutions on policy outcomes, then, it is critical to distinguish those institutions that reduce slack in the voter-official relationship and those that do not. A promising recent perspective on this question is the idea of *issue unbundling* posited in a pair of recent papers by Besley and Coate (2000, 2003). The basic idea is as follows. Suppose in a given jurisdiction there are $j$ policy dimensions. On any given dimension, the government can choose either a special interest-friendly policy or a voter-friendly policy. A majority of voters prefers the voter-friendly policy on each dimension. However, there is an interest group in each domain that prefers the special-interest policy, and the group will provide a private benefit to the policymaker if the special interest's preferred policy is enacted. This benefit may be a campaign contribution that the policymaker can use to improve her lot at election time or a bribe that can be used for private consumption. The policymaker would like to receive the side payments from the interest groups, but only if doing so will not cost her the next election.

Consider a jurisdiction in which a single elected official has responsibility for all $j$ policy dimensions. This official will be ascribed all the blame and credit for policy outcomes, and voters must make a single reelect-reject decision in each election. The crudeness of the electoral sanction reduces voters' ability to control the single official along any particular policy dimension. In a sense, voters must make a decision on a bundle of policy dimensions. As a result, the official may be able enact special interest-friendly policies in some dimensions, as long as she enacts consumer-friendly policies on

a sufficient number of dimensions to secure reelection. For these "general purpose" officials, elections will not completely mitigate agency problems.

In contrast with the general-purpose policymaker, consider a jurisdiction in which a separate elected official makes policy in each of the *j* issue domains. The creation of specialized offices for particular policies facilitates issue unbundling. When an official is exclusively responsible for providing a single public good like water or sanitation, voters do not have to make aggregate judgments across multiple policy issues when evaluating that official. A vote for or against the "special purpose" official is a summary of voter preferences along only one policy dimension. An official who enacts an interest group-friendly policy in her single domain will not be able to placate voters with voter-friendly policies on other issues. Thus, for those issue dimensions in which there is a specialized official, elections should better ensure that policy outcomes are close to the preferences of voters. The greater the unbundling, the greater the mitigation of agency problems. In a jurisdiction with *j* elected officials, each of whom has authority to make decisions along a single policy dimension, the power of elections increases drastically. Besley and Coate (2003) provide empirical support for their issue unbundling argument by contrasting elected and appointed utility regulators. Using panel data for US states, they find that elected regulators systematically enact more consumer-friendly policies than appointed regulators.

The logic of issue unbundling for elected versus appointed offices has been developed in the context of a single office. But is there any theoretical limit to the unbundling benefits that can be achieved by converting more and more offices from appointed to elected positions? If issue unbundling gives citizens the opportunity to bring

policy outcomes closer to their preferences, should not all public positions, from district attorney to dog catcher, be elected offices with authority over a single policy dimension? We believe the answer is no, and the reason lies with the increased monitoring costs associated with the proliferation of elected offices. Each additional elected office added to the ballot requires additional work on the part of voters. As the number of offices grows, the costs to citizens of monitoring a legion of public officials may outweigh any marginal benefits associated with issue unbundling.

We conceive of electoral monitoring costs as having two basic components. The first component is a function of the number of public services provided in the jurisdiction. At the most basic level, the citizen must determine whether each policy has been set at the voter-friendly level or the interest-group friendly level. The second component of monitoring costs is a function of the number of elected offices. For each office, the citizen must be able to identify the incumbent and assess her responsibility for a particular service or services. To illustrate, consider the voter's experience at the polls. On the ballot, the citizen sees a list of offices, and for each office a list of names. The ballot does not identify the incumbent, and in most cases it does not even list a political party affiliation.[1] At a minimum, a voter must be able to identify the incumbent for each office and match the incumbent to an assessment of the service(s) performed by the office in question. Where there is only one general purpose office, all services can be attributed to one official. The voter needs only to know which candidate is the incumbent and to form an overall assessment of the incumbent's performance. Where there are many offices, the task becomes considerably more challenging. In practice, it is not at all unusual to find two dozen or more elected offices on a ballot. In the discussion that

[1] About three-quarters of local elections are nonpartisan.

follows, we use the term *monitoring costs* to refer to total effort required to evaluate all services in a jurisdiction and match them to the relevant incumbent officials.

The addition of monitoring costs to the unbundling framework suggests that the relationship between the number of elected offices and policy outcomes may not be linear or even monotonic. Rather, the addition of elected officials may lead to more voter-friendly policies up to a point because the marginal benefits of unbundling are greater than the marginal costs of monitoring. However, as more and more elected officials are added, marginal monitoring costs may exceed marginal unbundling benefits. That is, as monitoring costs increase, each elected official may receive less scrutiny from voters. If so, then officials governing specialized domains may be able to adopt special interest-friendly policies without suffering electoral reprisals. If marginal unbundling benefits decrease with the number of officials and marginal monitoring costs increase, then the overall relationship between the number of officials and policy outcomes should exhibit diminishing returns. At some point, marginal monitoring costs may outweigh marginal unbundling benefits, in which case we should find a U-shaped relationship between the number of elected officials and the prevalence of voter-friendly policy outcomes.

In the remainder of the paper, we explore these ideas in the context of local fiscal policy. To test our theory, we analyze the fiscal behavior of local governments as a function of the number of elected offices within the jurisdiction. Our assumption is that special interest-friendly policies entail greater government spending than voter-friendly policies. In other words, most interest groups want more government spending on the

policy they care about rather than less.[2] Therefore, we model government spending as a quadratic function of electoral density and expect the main effect to be negative and the squared term to carry a positive sign. We also model the relationship semi-parametrically. Whether actual values of electoral density are set at levels where the marginal costs of monitoring exceed the marginal benefits of unbundling—that is, whether the actual reduced form relationship is U-shaped—is an empirical question, which we return to after a brief literature review.

## II. Related Literature

Although the relationship between the number of elected offices and fiscal policy has not been studied, other literatures have explored the impact of size of government on spending. For example, a robust literature predicts that legislative bodies with more members will tend to overspend. Weingast, Shepsle, and Johnson (1981) showed that in a legislature with a norm of universalism, districted elections, and general taxation authority, budget project scale increases as the number of districts and therefore legislators grows. Because the benefits of pork-like spending projects tend to be concentrated in one district and the costs of paying (taxes) spread diffusely across all districts, the legislative body will exhibit an overspending bias. This class of models essentially treats the tax base as a common pool resource, producing standard problems of over-extraction. Given the assumptions of the model, increasing the number of elected officials in a jurisdiction produces an overspending bias—a gap between voter preferences and legislative outcomes. This effect has come to be known as the "law of

[2] While there are certain taxpayers groups that promote smaller government overall, we are aware of relatively few groups that fight for lower provision of services in particular policy areas such as education or policing.

1/n," which summarizes the share of tax costs internalized by any single district as the legislature grows. Although the assumption of "universality" has been criticized in the literature, at base it is merely an assumption of logrolling (Weingast and Marshall 1988), hardly an implausible working assumption for legislative behavior.

The Shepsle, Weingast, and Johnson model was developed in the context of the U.S. Congress, but its empirical support has come primarily from studies of other legislative bodies.[3] In particular, Gilligan and Matsusaka (1995) show that state level expenditures are positively related to the number of seats in a state legislature. Their findings support the hypothesis that increasing the number of elected officials leads to more spending than citizens would like. At the local level, Baqir (2002) finds that jurisdictions with more city council districts (more elected officials on the city council) spend more. Langbein, Crewson, and Brasher (1996) also find that per capita expenditures are positively related to the number of elected members of the city council (in a sample of cities with a council-manager form of government and a weak mayor). Similarly, Dalenberg and Duffy Deno (1991) argue that cities with ward elections tend to spend more than cities with at-large election systems, which they link to the problem of concentrated benefits and diffuse costs that underlies the law of 1/*n*.

Other political institutions like direct citizen initiatives or referenda can also reduce the severity of agency problems. For example, Matsusaka (1995) shows that states with a direct citizen initiative or referendum have lower spending than states without these institutions. He argues that initiatives allow voters to reduce the power of agenda control exercised by legislators in non-initiative states, and also to bring specific

[3] Knight (2006) provides a useful review and synthesis of the literature on common-pool problems in legislatures.

outcomes back in line with voter preferences in symmetric information settings. More direct voter control results in less state spending. Similarly, Gerber (1999) finds that states with the initiative produce social policies that more closely reflect voter preferences. On the other hand, Farnham (1990) finds only modest effects of recalls, initiatives, and referenda on local public expenditures, and Besley and Case (2003) find that inferences about the initiative's effect on fiscal outcomes are highly sensitive to how the model's standard errors are estimated.

It is not inevitable that electoral institutions will determine the relationship between voters and elected officials. The Tiebout (1965) model and its progeny,[4] suggest that (given certain assumptions) competition between local governments should result in the optimal provision of public goods. If citizens are mobile, jurisdictions that over-extract from their tax base will suffer an exodus of residents and capital. If local governments compete with each other for an increased tax base, the right bundle of public goods, taxes, and spending should be provided to each respective populus.[5] The literature on local government competition is expansive, and we wish to make only a gesture in its direction.[6] However, if it is correct, then institutional variation such as the number of elected officials or the structure of different governmental units should be largely irrelevant for determining fiscal outcomes. Neither the number of officials nor the structure of local government should be systematically associated with government taxation, spending, or borrowing in a perfectly competitive local government "market."

[4] See generally Sprunger and Wilson (1998); Taylor (1995); Sonstelie and Portney (1978); Rose-Ackerman (1983a; 1983b); Rauscher (1998); Perroni and Scharf (2001).
[5] But see, e.g., Epple and Zelenitz (1981) for an argument that Tiebout competition alone is insufficient to constrain government excesses.
[6] Wilson (1999) provides an excellent review of the literature on tax competition.

A prediction that electoral institutions will be largely irrelevant to public policy—fiscal or otherwise—is also supported by an assortment of scholarship relating to the median voter theorem. In a first-past-the-post winner-take-all political system, legislative outcomes will simply replicate the preferences of the median voter. (Borcherding and Deacon 1972; Bergstrom and Goodman 1973).[7] If so, then a legislature with 10 members will produce identical policy outcomes as a legislator of 100 members; both will match the preferences of the median voter and policy should be invariant to the number of legislators, votes, or elections.

Together, these various schools of thought produce clear but divergent predictions about the relationship between elected officials and government fiscal behavior. The common pool resource overextraction literature predicts that taxing and spending should increase with the number of legislators. A focus on elections as a mechanism for issue unbundling suggests a negative relationship, and both the Tiebout competition and median voter models predict a null effect. Our own framework predicts diminishing returns and possibly a U-shaped relationship between the number of elected officials and fiscal behavior.

If the theoretical literature offers competing arguments about the relationship between the size of governing and fiscal policy, existing empirical studies have done little to settle the question. The evidence on institutional variation and spending is mixed at the local level. A common approach is to ask whether cities that reformed their government structures spend more or less than cities that have not.[8] In this vein, some studies conclude that municipal governments of the council-manager form spend less than

[7] For an overview of these and related models, see Mueller (2003).
[8] See Jung (2006) for an overview.

mayor-council municipalities (Booms 1966; Lineberry and Folwoer 1967; Clark 1968; Stumm and Corrigan 1998). Other studies conclude that reformed municipalities spend more (Sherbenou 1961; Nunn 1996; Cole 1971; French 2004). Others find a null effect (Liebert 1974; Lyons and Morgan 1977; Dye and Garcia 1978; Morgan and Pelissaro 1980; Deno and Mehay 1987; Hayes and Chang 1990; Morgan and Watson 1995). While Baqir (2002) and Langbein, Crewson, and Brasher (1996) find a positive relationship between the number of seats on a city council and the level of expenditures, no one, so far as we are aware, has examined the broader question of the relationship between the number of local elected offices and taxing and spending in local government.

Although these literatures are often discussed together, making sense of the divergent predictions and findings requires a bit more precision. For example, scholarship on the law of 1/n is properly focused on *legislative* bodies like Congress or city councils with districted rather than at large seats. Cabined by its own terms, the law of 1/n literature applies not to all elected officials, but merely a subset of elected officials. Adding districts to a legislature should exacerbate the common-pool problem underlying the law of 1/n, but adding other nonlegislative elected offices should not. On the other hand adding specialized elected offices unbundles policy authority, while adding seats in the legislature does not. In other words, we suggest that two distinct forces are at work for these two different types of elected offices. It is therefore critical in empirical analysis to distinguish legislative body elected officials from nonlegislative body elected officials.

Moreover, even an increase in nonlegislative body elected officials does not inevitably reduce slack between voters and representatives. Increasing the number of elected officials should only reduce slack to the extent that there is a corresponding

unbundling effect. To wit, adding special purpose elected officials with exclusive authority over a single policy domain unbundles. Adding general purpose elected officials with nonexclusive nonunique responsibilities may or may not. Note, however, that on the margin, the addition of a special purpose elected official may also reduce the crudeness of a vote on the general purpose elected official. Before the addition of a new special purpose elected official, a voter would have to average across *n* policy dimensions when voting for a general purpose official. After the addition of an elected official (with exclusive authority over a single policy dimension), a voter must average across *n*-1 dimensions when voting for existing general purpose official. Although this increase in efficacy is unlikely to be large, there should be some positive movement at the margin. If so, adding special purpose government offices should increase the responsiveness of government as a whole, not only with respect to the new special purpose government officials.

To summarize, we conceive of the relationship between voters and politicians as a standard principal-agent problem. Elections provide more control over elected officials than would exist without elections. But as a mechanism of control, elections are imperfect. They are likely to be most effective when a single elected official controls a single policy dimension. In these settings, policy outcomes should be closer to voter preferences. However, at a certain point the costs of monitoring many government officials may outweigh the unbundling benefits, implying that the effect of electoral institutions is likely to exhibit diminishing returns. If the unbundling and monitoring costs theory of elections is correct, adding elected offices in a jurisdiction should bring policy outcomes closer to voter preferences until the costs of monitoring grow too great;

at that point, adding elected offices should produce policy outcomes that are marginally further from voter preferences. In the context of fiscal policy, we suggest that that some unbundling will reduce spending; but, too much elected officials will actually increase it.[9]

Our empirical strategy is analyze the link between what we informally term *electoral density*—the abundance of unbundling elected offices in a jurisdiction—and fiscal outcomes such as taxing and spending patterns. The main analysis models patterns of revenue raising by local governments as a function of variation in the number of elected offices. The data demonstrate that local governments with larger city councils do tax more (consistent with Baqir 2002), but that the relationship between other elected officials and taxing is indeed U-shaped. As a secondary test of findings, we pursue identical analysis, but with local government expenditures (rather than revenues) as the dependent variable. Throughout the analysis we rely on a mix of standard polynomial regression models and semi-parametric methods.

## III. INSTITUTIONAL BACKGROUND

Because virtually nothing has been written on the local elected offices that are the subject of this paper, we begin by offering an overview of the institutional environment we seek to explore.[10] Table 1 contains some basic descriptive statistics about the number and distribution of elected officials. In 1992 there were over 500,000 elected officials in the United States in federal, state, and local government. The Federal elected officials are largely familiar: Senators, Representatives, the President and Vice-President.

[9] This intuition might be taken to be an alternative theoretical foundation for local overspending bias, distinct from the law of 1/n.

[10] The discussion is drawn from the 1992 Census of Governments (U.S. Census), the last to collect detailed data on locally elected officials.

State government elected officials are a substantially larger class, consisting of more than 18,000 elected officials. Across states, there is significant variation with respect to how many officials are elected. For example, Delaware has only 80 elected state officials, while Pennsylvania has 1,200. Forty percent of all State elected officials are members of State legislatures. The remainder consists of other elected officials (53 percent) including executive, administrative, and judicial functions; and elected members of State boards (7 percent) that include a handful of school board members in state-operated school systems (Alaska, Hawaii, Maine, and New Jersey), as well as soil conservation district boards in Arizona, Delaware, Louisiana, Missouri, and Washington.

The vast majority of elected officials—96 percent—serve in local governments. A staggering 343,000 elected officials are found on the governing boards of counties, municipalities, townships, special districts, and school districts. These governing bodies, such as city councils and school boards, represent legislative branch of local government. For the purposes of our analysis, we are especially interested in the other 120,000 elected officials who serve in specialized offices of the local executive and judicial branches. To get a sense of the non-governing body elected officials category, consider Table 2, which lists the number of various non-governing body elected positions by the different types of local government. For example, there are 324 county-executives in the United States, and 11,499 mayors of cities and towns.[11] Certain officials are associated exclusively or almost exclusively with certain levels of government. County-executives are of this sort. So too coroners and sheriffs, which are always county officials. There are 2,930 elected sheriffs

[11] For a useful recent summary of the structures of municipal governments, see DeSantis & Renner (2002).

(county) in the United States and 1,466 elected coroners. Road or Highway Commissioners are never county elected offices; surveyors always are.[12]

The tremendous variation in the number of elected offices from place to place is indicated in Table 3. We begin by created county-area summaries of the total number of elected offices in all governments. Cook County, Illinois, with a sum total of 370 total elected offices in all of its local governments, leads the nation. We then compute our two primary measures of *electoral density*: the number of elected offices per capita and per general-purpose government. The average county area has 1.7 elected offices per 1000 capita and 4.4 elected offices per government. At the low end, there are six counties where no local government has a non-governing body office, and these counties register a zero for both measures of electoral density. At the high end, Slope, North Dakota, has 75 elected offices per 1000 capita, meaning that nearly 10 percent of the population serves in a local office!

In sum, there is remarkable variation with respect to the size and structure of government in the United States. We are certainly not the first to make this observation, nor the first to analyze local government data. To our knowledge, however, no one has analyzed the impact of the number of elected offices on fiscal outcomes. The theoretical discussion emphasizes the critical, but ambiguous role of electoral institutions in the democratic political structure.

[12] Note that Table 2 is a summary only of *elected* offices. It says nothing about the number or distribution of *appointed* offices with the same functions. For certain offices that all governments at a given level must have, it is possible to infer the number of appointed officials. For example, if all counties had coroners, we could calculate the number of appointed-coroner officials by simple subtraction. As a general matter this will not be possible because not all counties, municipalities, or towns have identical slates of offices. However, even if precise figures cannot be obtained, the final column in Table 2 is a rough indicator for the prevalence of electing a given office. For example, only 317 counties elected county-executives while 1,177 elect a probate judge.

## IV. DATA & METHODS

Because the functional responsibilities of different types of local governments varies across states, we use county aggregates as our unit of analysis.[13] This allows us to ensure—to the greatest extent possible—that our local government units provide a similar bundle of public services. In some counties a given service will be provided by a special purpose government; in other counties, the same service will be provided by a general purpose government. However, at the level of county aggregates, we can be reasonably confident that a similar bundle of services is provided.

We begin by summing the number of elected offices in all governments within a county.[14] The number of elected offices is then normalized by the number of governments and also by county population to produce two explanatory variables of interest: elected offices per capita and elected offices per government. The elected offices variable is computed by summing the number of total elected offices in the county, excluding officials on governing bodies such as city or county councils. In other words, this variable captures all of the offices listed in Table 2. Each office is counted only once, regardless of the number officeholders. For instance, if there are 10 elected judges in a county, we consider this one elected office. We then divide the number of elected offices by the total number of general purpose governments within the county to calculate the per government measure. The elected offices per government variable is a rough measure of

[13] In states that do not officially have county governments, we use the *county area*, as designated by the Census of Governments.

[14] The number of elected offices is sometimes different than the number of elected officials. The difference between the two is mainly that some elected offices are occupied by multiple officials. The offices of judge and constable are common examples.

the degree of unbundling within a county.[15] The more functional elected offices within a county, the greater the degree of unbundling. Similarly, the greater the number of offices, the greater the total costs of monitoring. Both measures indicate what we call the *electoral density* of a county, and both capture the unbundling and monitoring costs theories.

To estimate the effect of legislative body elected officials, we calculate the average council size for general purpose governments within the county. If the law of 1/n literature is correct, the average city council size should be positively associated with spending. By disaggregating the elected officials data into legislative body and nonlegislative body officials, we are able to distinguish two potentially conflicting effects that could easily confound empirical estimates.

Our first dependent variable is general own-source revenue per capita. The numerator is the sum of own-source revenue across all governments in a county and the denominator is county population. Own-source revenue refers to all locally-raised revenue and excludes intergovernmental transfers. Own-source revenue accounts for 58% of all local government general revenue.[16] In addition, we model direct general expenditures per capita and a sample of expenditures on specific budget line items.

Electoral institutions are obviously not the only or even the primary determinants of local fiscal patterns. Therefore, we use a set of control variables with a strong foundation in the prior literature. The first control is income per capita. Following "Wagner's Law," the expectation is that demand for government services increases with

[15] We have experimented with other measures as well. Most alternatives have a simple correlation coefficient in excess of 0.95. No alternative that we have tried produces different conclusions.

[16] In principle, the aggregate tax rate is an ideal dependent variable. However, due to variation in assessment practices across jurisdictions and complexity of tax codes, calculating the effective tax rate in a county is prohibitively difficult.

income (Musgrave and Peacock 1958). Next, we control for several population characteristics that may reflect tastes for public goods (Cutler et al., 1993). We include the proportion of families with children to control for demand for education, a large component of local spending. We also include the fraction of the population over 65, as it is often argued that the older population prefers lower spending on education (Poterba, 1997). On the other hand, there may be additional costs associated with serving an elderly population. In an effort to control for the ideological orientation of the county, we use the Republican vote share in the 1992 presidential election. We also control for educational attainment, as measured by the percentage of adults with a college degree.

Alesina et al. (1999) argue that population heterogeneity leads to increased pressure for group-specific spending programs but fewer nonexcludable public goods. While their theoretical model is ambiguous as to the net effects, their empirical results show a positive association between ethnic heterogeneity and total expenditures and taxes. Following Alesina et al. (1999), we measure ethnic fragmentation as the probability that two randomly drawn people from a county belong to different ethnic groups.[17] Income heterogeneity is measured as the ratio of the mean household income to the median household income in a county. Along these lines, Meltzer and Richard (1981, 1983) argue that increasing inequality causes greater demand for redistribution, hence higher taxes.

[17] Specifically, ethnic fragmentation is computed as follows: $Ethnic = 1 - \sum_i (Race_i)^2$ ,

where $Race_i$ denotes the share of population identified as of race $i$ and $i$ = {White, Black, Hispanic, Asian and Pacific Islander, American Indian}. Note that Hispanic is identified as an "origin" rather than a race in the Census, so I count only non-Hispanic Whites, Blacks, Asian and Pacific Islanders, and American Indians for those categories. This same measure has been used in numerous prior studies; see the references in Alesina et al. (1999). For a theoretical interpretation of this index, see Vigdor (2001).

To address economies of scale considerations, we control for county population and land area.[18] In addition, we include a dummy variable indicating whether a county is the central county of a metropolitan statistical area (MSA), and another dummy for suburban counties within MSAs.[19] The omitted category is non-metropolitan counties. These central and suburban county indicators capture possible sorting by taste, as well as potential economies of scale in MSAs. Finally, States also vary in their assignment of fiscal responsibilities to local governments, as well as in unobservable historical, cultural, and institutional characteristics that may influence fiscal outcomes. For this reason, we include state fixed effects in all of the models reported below.[20]

Our main data sources are the 1992 Census of Governments (COG), the 1990 Census of Population and Housing (CPH), and the 1994 City and County Databook (CCD), all published by the U.S. Census Bureau. The data source for each variable is specified in Table 3A. We exclude Virginia (134 observations), Hawaii (4 observations), and Alaska (27 observations) from the analysis. Virginia is the only state whose municipalities are incorporated as *independent cities*, which are not part of any county. Hawaii has the only entirely state-run public school system. Alaska uniquely relies on boroughs rather than counties, and boroughs do not cover the entire land area of the state. Anomalously, the COG reports one record for New York City, but no records for its 5 component counties. Not being able to produce a county aggregate record, we drop the New York City observation.[21] In addition, we exclude Shannon county, South Dakota,

[18] One concern with this setup is that county population appears as both the denominator of the dependent variable and on the right hand side of the equation. Therefore, we have also run the analysis excluding county population. The coefficients change of course, but the substantive conclusions do not.
[19] In New England, the Census Bureau specifies central cities and towns rather than central counties of MSAs. In these states, we define any county containing a central city or town as a central county.
[20] The state fixed effects coefficients are not included in the tables, but are available from the authors.
[21] Alesina et al. (1999) also discuss this issue, and make the same decision.

population 10,490, which is the only county that has no elected officials outside the county governing body. Beginning with a total population of 3,136 counties in the 1992 COG, these case selection criteria produce an analysis sample of 2,965 counties.[22] In addition, for models that measure electoral density as offices per government, we exclude an additional 37 counties that have no general purpose governments, leaving an analysis sample of 2,928. Table 3 presents summary statistics for various measures of electoral density for all counties, while Table 3A presents summary statistics for all the variables based on the 2,965 counties selected for the analysis.

## V. FINDINGS

Our main empirical contribution is to test the unbundling model in more general institutional settings, looking at *all* local elected offices, and to estimate the potentially conflicting effect of the law of 1/n in local government. Our main theoretical contribution is to extend the unbundling theory of political institutions to include monitoring costs. This simple revision significantly alters the empirical implications. Rather than suggesting a negative and largely linear effect of adding elected officials in a jurisdiction, the monitoring costs revision predicts a quadratic or U-shaped relationship. Spending should decrease initially as unbundling produces greater control over elected officials and subsequently increase as the marginal costs swamp any unbundling gain.

To test this proposition, we begin by estimating polynomial regression models of taxing and spending in local government. Because of the restrictive functional form assumptions inherent in the polynomial regression context, we also use a semi-parametric

[22] There are some minor discrepancies in how counties are counted in the COG versus the CPH, primarily in Virginia and Alaska, which explain why the former tallies 3,135 counties and the latter 3,034.

generalized additive model (GAM), in which we estimate the effect of electoral density with thin plate regression splines and allow all of the other covariates to enter the model linearly.[23] Both methods produce similar results. The relationship appears to be U-shaped and the turning point is at a reasonable location in the actual data.

An obvious concern with any study of the fiscal effects of political institutions is endogeneity; namely, the possibility of simultaneous causation between institutional form and fiscal policy (see Persson and Tabellini 2003). In other words, measures of electoral density may be correlated with the errors in an OLS regression, leading to biased estimates. To a large degree, concerns about reverse causation in this case should be allayed by the fact that electoral institutions are enshrined in longstanding provisions of state constitutions and city charters. For example, a set of state dummy variables explains more than half of the variation in electoral density across counties.[24] Moreover, the correlation between county area elected offices per government in 1992 and 1987 is 0.97. Thus, it is unlikely that electoral institutions change quickly in response to local spending preferences. In this sense, we believe it is safe to consider electoral institutions as predetermined, at least in the short-run. However, we return to this issue below.

A. Elected Offices and Revenue

To estimate the effect of electoral institutions on fiscal outcomes, we regress a log transformed measure of each county's own source revenues per capita—a standard measure of taxation in the public finance literature—on measures of elected offices per unit of government and per capita and their square. This is a straightforward polynomial

[23] The seminal reference on GAMs is Hastie and Tibshirani (1990). Beck and Jackman (1998) provide an accessible introduction. Our implementation follows Wood (2006) and the associated *R* package, *mgcv*.
[24] A regression of county aggregate elected offices per government on a set of state dummy variables yields an adjusted *R*-squared of 0.54, with 2,928 observations in our analysis sample.

regression model. The main results indicate that adding elected offices increases taxing at the low end of the distribution, but increases taxation after a certain point in the data. That is, the relationship between elected offices and taxing appears to be roughly U-shaped. This is true regardless of how electoral institutions are measured and the result is robust to a range of alternative specifications. In addition, we find that jurisdictions with larger average council sizes do tax more than jurisdictions with smaller councils. Each of these results is explored in greater detail below.

The first and third columns of Table 4 present coefficients for a simple polynomial equation without controls. The second and fourth columns present the coefficient estimates with the full controls included. The substantive conclusions are not sensitive to the inclusion of controls. We therefore focus our discussion on the full equations. Because the estimated model is a log-log regression, the coefficients represent elasticities, or the percentage point change in the dependent variable of interest produced by a percentage point change in the independent variable of interest. Note that in each of the models, the coefficient on elected offices is negative. And the coefficient on the squared variable is positive and statistically significant in all the models as well.[25]

First consider the model using offices per government. Both the linear and squared version of the variable are statistically significant ($p<0.05$). The main variable has a negative effect, while the squared term has a positive effect. The range of the main explanatory variable (log of elected offices per 10 general purpose governments) has a

[25] Including a squared version of another exogenous variable in the regression generates significant colinearity. The simple correlations are approximately 0.97. This does not produce biased coefficient estimates; however, it does affect the standard errors of the coefficients. To address this issue, we have mean deviated the explanatory variable of interest before squaring. In models not shown, this linear transformation produces identical coefficients, but reduces colinearity significantly. The relevant coefficient standard errors fall substantially, and therefore the relevant t-values increase as well.

range of 1.82 to 5.5 in the data. The observed turning point is at 3.2, approximately the 40th percentile of the data. This corresponds to a turning point of approximately 2.6 offices per government; a value for which about 1750 counties have a higher value. That is, forty percent of the counties in the sample have fewer elected offices per government than the turning point. Adding elected offices to these counties would reduce, rather than increase, overall revenue raising. For sixty percent of the counties, however, adding elected offices would actually increase spending.

The estimates from the per capita measures support similar conclusions,[26] and if anything are stronger. Whereas the *t* statistic values for the coefficients on the offices per government measures are between two and three, the *t* statistics on the offices per capita measures are both greater than five. Again, the coefficient on the offices per capita measure is negative, and the coefficient on the squared term is positive. The range of log of offices per million capita is 2.75 to 9.8 and the estimated turning point is at 5.46, approximately the 20th percentile. In untransformed terms, the turning point is at 0.24 offices per 1000 capita (corresponding to the same 20th percentile of the data). In sum, the polynomial regression models of own source revenues support the hypothesis of a U-shaped relationship between elected officials and taxing.

To get a better sense of the magnitude of these effects, we estimate the difference in predicted mean revenues, setting continuous covariates to their means, dummy variables to their modes, and allowing the measures of electoral density to vary. Table 5

[26] Both the offices per capita and offices per government variables contain values less than one;. When these negative values are squared the ordering of the underlying data is not maintained because both negative and positive numbers are treated identically after transformation. To preserve the original ordering, we simply express the first variable as offices per one million capita and the second as offices per 10 governments. The addition of a constant produces identical coefficient estimates on the untransformed term, but avoids changing the ordering of the original data.

lists predicted means at various points in the distribution of elected officials,[27] and lists the marginal effect of adding elected officials to the jurisdiction. The left hand side of Table 5 summarizes the effect of elected officials using the per government measure; the right hand side summarizes the effects of elected officials per capita. The first difference columns indicate the difference in per capita revenues raised shifting from the row *i* in the table to row *i*+1. To illustrate, the value corresponding to the fifth percentile of the number of elected offices per government is 2.42, which produces a predicted level of per capita revenues of approximately $952. The value of the elected offices per government variable at the tenth percentile is 2.64, which produces a predicted mean level of per capita revenues of $846. Moving from the fifth percentile to the tenth percentile, therefore produces a net decrease in own source revenues of approximately $106. Again, the estimated turning point in the polynomial regression model is at approximately 3.27, which is at the 40th percentile. At this point in the distribution of elected offices per government, the predicted mean of own source revenues is $838. Shifts at or around the 40th percentile of the data produce virtually no changes in revenues. The results are essentially identical for the per capita measure of elected offices. Moving along the distribution produces decreases in spending until around the thirtieth percentile of the data, at which point more elected offices produce increases in spending. Shifting from the 50th percentile to the 90th percentile produces an increase in revenues raised of approximately $125 per capita.

Our theory suggests that costs of monitoring additional elected offices may at some point offset the benefits of issue unbundling, leading to a roughly U-shaped

[27] The values of the distribution points are summaries of the log of elected offices per ten general purpose governments and the log of elected offices per million people respectively.

relationship with spending. But we have no particular reason to expect that this relationship is quadratic per se. Therefore, to test the sensitivity of our results to functional form assumptions, we next estimate the effects of elected offices semi-parametrically. Specifically, we use a generalized additive model (GAM), in which we estimate the effect of elected offices with thin plate regression splines and allow all of the other covariates to enter the model linearly.

The results are graphically summarized in Figure 1. The graphs represent the relationship between the measures of electoral density (log of elected offices per 1000 capita) and general own-source revenue per capita.[28] Solid lines represent point estimates; dashed lines represent 95 percent confidence intervals. In each of the graphs, the curve is downward sloping and turns upward at a point well within the data. The U-shape is clearly evident in the estimated effects of elected offices per capita. Put simply, the results from the GAM model lend further support to the conclusions of the simple polynomial regression model. The relationship between electoral institutions and fiscal policy is not linear; rather, increasing the number of elected officials reduces revenue-raising in counties with few elected officials, but increases spending in counties with many elected officials. Based on these results, we conclude that the quadratic fit in the linear models achieves a satisfactory approximation to the underlying relationship between electoral density and own-source revenue.

Returning to Table 1, our other main result is that the coefficient on the average council size is also positive and statistically significant ($p<0.05$). That is, as the average size of legislative bodies in a county increases, so too does spending. In all the models,

[28] The results are similar for both the per capita and per government measures. We therefore only present the per capita figures herein.

the coefficient hovers at approximately 0.10. A percentage point increase in the average size of the governing body produces roughly a one-tenth percentage point increase in revenue raised from own sources. This result is consistent with prior findings from Baqir (2002), who finds an elasticity of 0.11 in a comparable model. When we experiment with adding a quadratic term for council size (not shown), it is never significant in any of the models. Putting the results together, the data show that increasing the size of legislative bodies increases taxing, but that adding other nonlegislative body elected officials can reduce or can increase taxing, depending on how many elected officials are already present in the jurisdiction. These findings are consistently with the law of 1/n, as well as our theory of unbundling and monitoring costs.

The other control variables are fairly standard in the literature. However, a few coefficients are noteworthy. First, income is an important determinant of own-source revenue, and the elasticity is greater than one, as predicted by Wagner's Law (b=1.3 in the equation including elected offices per government and b=1.4 in the equation including elected offices per capita). In addition, the degree of ethnic fractionalization is positive and statistically significant, while the ratio of mean to median income, a rough measure of the degree of economic heterogeneity in the jurisdiction, shows a significant negative relationship with own-source revenue. These estimates are consistent with prior work (Alesina, Baqir, and Easterly 1999).

Counties with more children proportionally raise slightly more own source revenues than counties with fewer children proportionally. So too counties with a higher proportion of college graduates. In addition, suburban counties spend significantly less then central or rural counties, which could reflect sorting by preferences or greater

interjurisdictional competition (Schneider, 1988). We also find a quadratic relationship between county population and own-source revenue, consistent with Baqir (2002). Population growth is negatively associated with own-source revenue, which may suggest that it takes time for spending to catch up with population in rapidly growing areas.

The coefficients on Federal Intergovernmental Revenue per capita and State Intergovernmental Revenue per capita are positive and statistically significant in both models. Counties with governments that receive more intergovernmental revenue per capita also raise more per capita from own source revenues, consistent with the "flypaper effect" (e.g., Hines and Thaler, 1995).[29] Lastly, note that in both sets of equations, partisanship appears to matter relatively little. The proportion of the county that voted for the Republican presidential candidate in 1992 produces virtually no change in the level of own source revenue per capita. All of these findings are, of course, of secondary interest to our work. However, the findings are largely consistent with the existing literature.

Using either method of standardizing elected offices, the same central results hold. The relationship between elected nonlegislative offices and taxation is roughly summarized by a U shape. At the same time, making legislative councils larger increases taxation. The results provide support for the law of 1/n, as well as the unbundling and monitoring costs theory.

B. Electoral Institutions and Expenditures

To this point, we have focused predominantly on revenue raising or taxation, asking how electoral institutions affect the generation of revenue in local government.

[29] One concern with these results is that intergovernmental revenue may be jointly determined with own-source revenue. In results not show, we reestimated all the models in the paper excluding the intergovernmental revenue variables. The results for electoral density did not change notably.

Taxing, however, is only half the story. If governments with more elected officials tax differently, they should also spend differently. In this section, we analyze the relationship between electoral institutions and expenditures by local government. Our main results regarding expenditures provide further support for the findings on electoral density and revenue raising. Areas with more elected offices spend less up to a point, beyond which adding elected officials produces more spending. However, the size of the legislative body is not statistically significant in the spending models, although it retains a positive coefficient. This same basic pattern of results is replicated not just at the level of overall expenditures, but also on a majority of tested line-item expenditures as well.

1. Aggregate Expenditures

To ascertain whether local government spending varies as a function of electoral institutions, we replicate the earlier analysis of own source revenues, replacing the dependent variable with a measure of overall spending by all units of government within each county. The results are presented in Table 6. The independent variables of interest are logged versions of the number of elected offices, both normalized by the number of governments and citizens. Overall government spending is calculated per capita and logged in all models.

As above, the same substantive conclusions are supported by the simple equation and models with full controls. Once again, we focus our discussion the full control estimates (columns (2) and (4) in Table 6). To start with, the estimated effect of both offices per government and offices per capita appears to be U-shaped as indicated by the results from the polynomial regression model. The coefficients on elected offices per government and elected offices per capita are negative and the coefficients on the squared

versions of those variables are positive; all are statistically significant at conventional levels.

Figure 2 contains the GAM estimates with expenditures replacing revenue raising as the dependent variable. Again, the semi-parametric methods provide further support for the polynomial regression models, as the figure exhibits an obvious U-shaped relationship. Together, the polynomial regression estimates and the GAM estimates provide strong evidence that the relationship between the number of elected offices and fiscal behavior is U-shaped.

The results from our analysis of expenditures diverge from the analysis of revenues in one key sense. In the models of expenditures, the effect of average council size is positive (as before), but it is not statistically significant at conventional levels. Thus, the council size result appears sensitive to the choice of dependent variable. We will have more to say about this issue in section II.D.

2. Functional Expenditures

If the above results are correct, then a natural next stage of analysis is ask whether the results on aggregate expenditures apply to specific categories of spending. To explore this question, we estimate a series of models regressing the amount of money spent in specific functional categories on electoral institutions and controls. To conserve space, we report only the coefficients for elected offices per government.[30] The results presented in Table 7 correspond to the coefficient on elected offices per government in the model of

[30] Complete results are available from the authors on request.

the listed spending variable, including all the control variables used above.[31] In essence, we use expenditures on specific budget items as a further check on the validity of our earlier findings.

First, note that the budget lines include all detailed spending categories contained in the Census of Governments, which covers a diverse range of policies including hospitals, education, sewers, and interest on debt. Second, note that ten spending categories show the predicted quadratic relationship with offices per government and at a statistically significant level. Another 16 categories demonstrate the predicted relationship—negative effect for the electoral density and positive for its quadratic—although the relationships fall short of statistical significance. Indeed only 8 of the 35 spending categories show a relationship with offices per government that is not of the predicted shape, and none of these is statistically significant at conventional levels. In other words, all coefficients that are statistically significant are negative on offices per government and positive on its square. We do not want to make too much of these findings. However, the disaggregation suggests that the U-shaped relationship between elected officials and expenditures is present for many, though certainly not all, individual spending line items as well as for total spending.

C. Debt

The analysis of taxing, general spending, and functional spending all suggest a U-shaped relationship between electoral density and fiscal behavior in local government. However, own source revenue and aggregate expenditures are closely related. Aggregate

[31] We take the natural log of elected offices per government but leave the dependent spending variables untransformed in the models. As a consequence, effects are changes in the actual level of dollars spent.

local expenditures are the sum of own source revenues, intergovernmental transfers, and debt. Does the presence of more elected officials generate a similar effect on the use debt in local governments? The answer to this question is yes though with a few caveats.

To test this hypothesis, we ran a series of models of long-term debt outstanding per capita against our measures of electoral density. Table 8 presents the results of both simple regressions and models with full controls. In large part, the results mirror those of the earlier sections. As columns two and four indicate, in the full equations, the coefficients on elected offices per government and per capita are positive and statistically significant; the coefficient on square of those variables is positive and statistically significant. So too in the simple model for elected offices per government. The caveat is that in the simple model of elected offices per capita, the coefficient on the square is negative, though small. In that equation, there is not turning in the data after which the effect of electoral density is positive. Given the robustness of the findings across all our other models, we are not particularly troubled by this one model. Nonetheless, we report the result for the sake of transparency. The turning points in the two full equations are at approximately the 89th (offices per government) and 82d (offices per capita) percentiles respectively.

D. First-Differences Analysis

The results presented thus far are based on cross-sectional county aggregate data for 1992. We have argued that electoral institutions can be considered predetermined in the short run, thus mitigating some of the usual concerns with cross-sectional analysis. Nevertheless, in this section we test the robustness of our results by estimating the main

findings in first-differences. Differencing the data strips away the effects of any observable or unobservable variables that do not change over time. Thus, this strategy addresses any lingering concerns about omitted variables that may influence both electoral institutions and fiscal outcomes.

Data on elected offices in local governments are available in electronic form from the COG for 1987 and 1992. We merge these two years of data to create a short panel of county aggregate data. Consistent with our argument that electoral institutions do not change quickly, we note that we do not have a great deal of between-year variation in our measures of electoral density. The correlation between elected offices per government in 1987 and 1992 is 0.97; for elected offices per capita it is 0.96. The lack of cross-year variation should, if anything, bias against finding effects of electoral density in first-differences models. Because most of our demographic variables are from the 1990 Census, we are not able to include them in the first-difference models; we do not have independent values for 1992 and 1987. However, the effects of these and other variables that do not change significantly over the 5 year period will be washed out in the first differencing. We do include as predictors a smaller set of variables for which we are able to measure changes between 1987 and 1992. These include average council size, population and its square, and the number of governments of different types in the county. In addition, we include a functional performance index (FPI), which sums nationwide median spending for each service provided in the county.[32] The FPI should

[32] The FPI is defined as follows. For each functional spending category in the COG, we create a 0/1 variable for each county indicating whether the county has positive spending for that function. Next we compute median spending on each function among those counties in which the function is provided. For each county, we then sum nationwide median spending on each function it provides. This summary index indicates the amount a county would spend if it spent the nationwide median amount on each service it provides. Formally, the index is defined as:

capture changes in spending over time that are associated with changes in functional performance.

Table 9 presents results of the first-differences models,[33] in which we regress *changes* in own-source revenue between 1987 and 1992 on *changes* in the independent variables. The results for both the per capita and per government measures of electoral density are consistent with our cross-sectional models. In all specifications, we find a statistically significant quadratic relationship between electoral density and own-source revenues. We can, therefore, be reasonably confident that the cross-sectional results presented above are not being driven by omitted variable bias.

Interestingly, however, the results for council size change notably in the first-differences model. There is a significant negative effect of average council size in both models, which is at odds with the positive coefficient from the cross-sectional models. In models not shown, we also find the positive council size effect when we exclude the other independent variables. The most natural interpretation of these results is that the positive council size effects in the cross-sectional analyses are due to omitted variable bias. As council size is not our main variable of interest, we do not pursue the issue further here.

## VI. DISCUSSION

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$$FPI_j = \sum_i \alpha_{ij} \mu_i ,$$

where $i$ indexes functional spending categories and $j$ indexes counties; $\alpha_{ij}$ is one if county $j$ provides service $i$ and zero if it does not, and $\mu_i$ represents nationwide median spending on service $i$ among all counties that provide the service. Thus, a county's FPI will increase whenever it adds a new service and whenever nationwide median spending on its existing services increases. This is a variation on the method of Clark and Fergusson (1976).

[33] Because we have only two time periods, fixed effects and first-differences models produce identical results.

The theoretical and empirical literature in economics and political science contains divergent predictions about the relationship between electoral institutions and the fiscal behavior of governments. One collection of scholarship predicts that over-spending bias will increase with the number of elected officials. Another predicts increasing the number of elected offices improves the ability of voters to manage the principal-agent problem of representation. A third predicts that policy outcomes will be largely invariant to the number or nature of electoral institutions.

Against this backdrop, we have sought to make two theoretical contributions. First, we have emphasized that all elected officials are not identical. Adding elected officials serving in districted general purpose legislative bodies may well produce increases in spending and greater slack between voters and politicians. However, when new elected offices generate unbundling, this should increase voter control over politicians. The precise form of electoral institutions matters. Second, while we find nascent work on unbundling to be extremely promising, we also suggest that it is incomplete in its current form. Unbundling should help manage agency problems, but it will often also produce new monitoring costs. A theory of electoral institutions must account for both.

Our main empirical contribution has been to offer evidence of a U-shaped relationship between elected offices in local government and patterns of government taxing and spending. An important, if secondary, empirical contribution is to demonstrate that the relationship between council size and spending is sensitive to the inclusion of unit-level fixed effects, although more work is clearly warranted to explain why. In any case, we find little evidence that electoral institutions are irrelevant to the fiscal behavior

of local government. Our analysis then, supports the basic idea that elections matter, but adds significant nuance to this claim.

If our theoretical apparatus is correct, it suggests a number of potential future research questions. Perhaps most importantly, we have treated institutional variation as exogenous for purposes of our analysis, but it is clear that institutional choices—perhaps made long ago—shape the local political and fiscal landscape in important ways. Investigating the sources of these institutional choices is at the top of our future research agenda.

## CONCLUSION

Our analysis links several strains of literature in economics, law, and political science on the relationship between political institutions and policy outcomes. Our central finding is that differences in the number of elected officials in local government produce significant differences in level of taxing and spending. With respect to nonlegislative body elected officials, adding officials to jurisdictions with few existing officials produces spending and taxing decreases. Adding officials to jurisdictions with lots of elected officials actually increases taxing and spending. This manifests empirically as U-shaped relationship between the number of elected officials and fiscal behavior.

With respect to theoretical models of politics, our findings suggest the importance of better theorizing not just about elections writ-large, but also with respect to how variation in local political and institutional arrangements might facilitate or undermine the use of elections to control legislators. In this sense, our work fits into a long-standing tradition of scholarship that uses economic, demographic, and political characteristics to explore taxing and spending patterns by state and local government. However, by relying

on county-level data, and the nonlinear functional form, we are able to provide a novel perspective on politics, institutional structure, and public finance.

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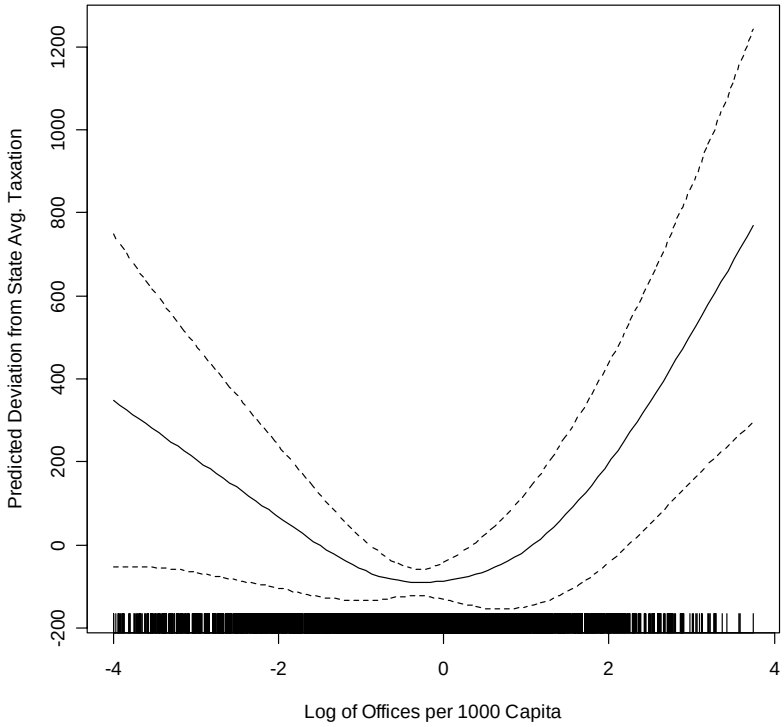


**Figure1: GAM Estimates of Own-Source Revenue**

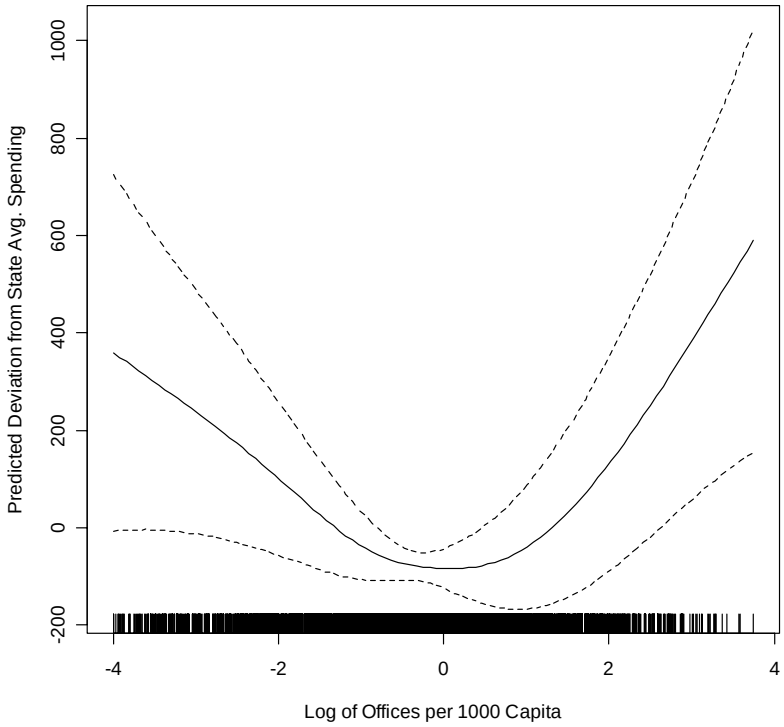


**Figure 2: GAM Estimates of Expenditures**

**Table 1. Elected Officials and Governments in the United States**

| | Total | Members of governing boards | Other elected boards | Other elected officials | Number of governments |
|---|---|---|---|---|---|
| Federal Government | 542 | 540 | - | 2 | 1 |
| State Governments | 18,828 | 7,461 | 1,331 | 10,036 | 50 |
| All local governments | 493,830 | 342,812 | 40,922 | 110,096 | 85,955 |
| General Purpose | | | | | |
| County | 58,818 | 17,274 | 10,835 | 30,709 | 3,043 |
| Subcounty | | | | | |
| Municipal | 135,531 | 107,542 | 4,157 | 23,832 | 19,279 |
| Town or township | 126,958 | 51,770 | 25,930 | 49,258 | 16,656 |
| Special Purpose | | | | | |
| School districts | 88,434 | 83,596 | - | 4,838 | 14,422 |
| Special districts | 84,089 | 82,630 | - | 1,459 | 31,555 |
| TOTAL | 513,200 | 350,813 | 42,253 | 120,134 | 86,006 |

*Source: Census of Governments, 1992, Vol. 1, No. 2, "Popularly Elected Officials"*

**Table 2. Non-Governing Body Elected Officials**

| Elected Office | County officials | Municipal officials | Township officials | Total elected officials | Counties where present |
|---|---|---|---|---|---|
| County Executive | 324 | - | - | 324 | 317 |
| Mayor | - | 11,380 | 119 | 11,499 | 2,699 |
| Assessor | 1,703 | 636 | 4,907 | 7,246 | 1,997 |
| Attorney | 1,842 | 425 | 67 | 2,334 | 2,018 |
| Auditor | 815 | 1,367 | 4,998 | 7,180 | 853 |
| County Clerk | 1,648 | - | 7 | 1,655 | 1,649 |
| Clerk | - | 3,735 | 12,046 | 15,781 | 1,334 |
| Clerk of the Court | 1,812 | 138 | 35 | 1,985 | 1,807 |
| Constable | 3,100 | 176 | 2,830 | 6,106 | 904 |
| Justice of the Peace | 2,862 | 482 | 3,161 | 6,505 | 826 |
| County or Probate Judge | 1,901 | - | - | 1,901 | 1,177 |
| Municipal Judge | - | 1,360 | 201 | 1,561 | 569 |
| Coroner | 1,466 | - | - | 1,466 | 1,386 |
| Sheriff | 2,930 | - | - | 2,930 | 2,930 |
| Police Chief | - | 649 | 52 | 701 | 358 |
| Recorder | 1,040 | 351 | 29 | 1,420 | 1,181 |
| Collector | 295 | 4 | 55 | 354 | 326 |
| Treasurer | 2,126 | 2,221 | 8,054 | 12,401 | 2,289 |
| Road or Highway Commissioner | - | 120 | 2,423 | 2,543 | 271 |
| Superintendent of Schools | 460 | - | 1 | 461 | 459 |
| Surveyor | 562 | - | - | 562 | 562 |
| Other Miscellaneous | 5,716 | 470 | 10,184 | 16,370 | 1,691 |

*Source: Census of Governments, 1992, Vol. 1, No. 2, "Popularly Elected Officials"*

**Table 3. Aggregate County-Area Elected Offices**

| | Average | Minumum | 25th percentile | Median | 75th percentile | Maximum | Minimum County | Maximum County |
|---|---|---|---|---|---|---|---|---|
| Elected Offices | | | | | | | | |
| Total | 26.3 | 0 | 9 | 13 | 33 | 370 | Maui, HI* | Cook, IL |
| Per 1,000 capita | 1.7 | 0 | 0.3 | 0.7 | 1.6 | 74.7 | Maui, HI* | Slope, ND |
| Per government | 4.4 | 0 | 2 | 3 | 5.4 | 90 | Maui, HI* | Rutland, VT |

*Not a unique minimum. Six counties have zero non-governing body elected officials.

*Source: Census of Governments, 1992, Vol. 1, No. 2, "Popularly Elected Officials"*

**Table 3A. Summary Statistics**

| Variable | Source | Mean | Std. Dev. | Min. | 25th pctile | Median | 75th pctile | Max. |
|---|---|---|---|---|---|---|---|---|
| ln(own-source revenue per capita) | COG | 6.87 | 0.56 | 4.58 | 6.54 | 6.91 | 7.22 | 9.72 |
| ln(direct general expenditures per capita) | COG | 7.47 | 0.37 | 5.11 | 7.22 | 7.45 | 7.71 | 9.18 |
| ln(long-term debt outstanding per capita) | COG | 6.20 | 1.42 | 0.00 | 5.64 | 6.36 | 7.01 | 11.32 |
| ln(elected offices per government) | COG | 3.50 | 0.74 | 1.27 | 3.00 | 3.40 | 4.01 | 6.80 |
| ln(elected offices per 1 million capita) | COG | 6.53 | 1.43 | 0.25 | 5.72 | 6.63 | 7.41 | 11.22 |
| ln(average council size) | COG | 1.62 | 0.36 | 0.08 | 1.43 | 1.61 | 1.73 | 4.80 |
| ln(population) | CPH | 10.13 | 1.37 | 4.67 | 9.25 | 10.01 | 10.92 | 16.00 |
| ethnic fractionalization index | CPH | 0.20 | 0.18 | 0.00 | 0.04 | 0.13 | 0.35 | 0.67 |
| mean to median income ratio | CPH | 1.27 | 0.10 | 1.02 | 1.20 | 1.25 | 1.32 | 2.01 |
| percent population 65 and over | CPH | 15.01 | 4.33 | 1.40 | 12.20 | 14.60 | 17.50 | 34.00 |
| ln(income per capita) | CPH | 2.38 | 0.22 | 1.42 | 2.25 | 2.36 | 2.49 | 3.35 |
| percent families with children | CPH | 48.04 | 4.91 | 25.10 | 45.30 | 47.90 | 50.70 | 79.60 |
| percent adults with college degree or higher | CPH | 13.34 | 6.32 | 3.70 | 9.20 | 11.70 | 15.40 | 53.40 |
| dummy = 1 if central county of MSA | CPH | 0.16 | 0.37 | 0.00 | 0.00 | 0.00 | 0.00 | 1.00 |
| dummy = 1 if suburban county | CPH | 0.10 | 0.30 | 0.00 | 0.00 | 0.00 | 0.00 | 1.00 |
| ln(land area) | CPH | 6.54 | 0.74 | 3.85 | 6.10 | 6.45 | 6.84 | 9.91 |
| federal intergovernmental revenue per capita, $1000s | COG | 0.05 | 0.08 | 0.00 | 0.01 | 0.03 | 0.06 | 1.03 |
| state intergovernmental revenue per capita, $1000s | COG | 0.73 | 0.31 | 0.00 | 0.54 | 0.67 | 0.84 | 3.48 |
| Republican vote share in 1992 presidential election | CCD | 39.60 | 8.51 | 12.90 | 33.80 | 39.00 | 45.10 | 75.00 |
| percent population growth, 1980-1992 | CCD | 5.97 | 20.25 | -34.40 | -6.30 | 2.50 | 12.70 | 207.70 |
| ln(number of municipalities) | COG | 1.75 | 0.69 | 0.00 | 1.39 | 1.79 | 2.20 | 4.80 |
| ln(number of townships) | COG | 0.90 | 1.34 | 0.00 | 0.00 | 0.00 | 2.48 | 4.28 |
| ln(number of special districts) | COG | 1.99 | 0.90 | 0.00 | 1.39 | 1.95 | 2.56 | 6.14 |
| ln(number of school districts) | COG | 1.41 | 0.80 | 0.00 | 0.69 | 1.39 | 1.95 | 5.03 |

Notes: COG = 1992 Census of Governments; CPH = 1990 Census of Population and Housing; CCD = 1994 City and County Data Book.
The unit of observation is the county area.
*N* = 2965 for all variables except offices per government, for which *N* = 2928, as explained in the text.

## Table 4. Polynomial Regression Models of Own Source Revenue

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| ln(offices per government) | -0.286 | -0.149 | | |
| | (0.095)*** | (0.083)* | | |
| ln(offices per government)$^2$ | 0.033 | 0.023 | | |
| | (0.013)** | (0.011)** | | |
| ln(offices per capita) | | | -0.445 | -0.198 |
| | | | (0.041)*** | (0.051)*** |
| ln(offices per capita)$^2$ | | | 0.033 | 0.018 |
| | | | (0.003)*** | (0.003)*** |
| ln(avg. council size) | | 0.096 | | 0.101 |
| | | (0.045)** | | (0.043)** |
| ln(income per capita) | | 1.336 | | 1.386 |
| | | (0.081)*** | | (0.079)*** |
| ln(population) | | -0.349 | | -0.220 |
| | | (0.070)*** | | (0.097)** |
| ln(population)$^2$ | | 0.015 | | 0.009 |
| | | (0.003)*** | | (0.004)* |
| ethnic fractionalization | | 0.228 | | 0.219 |
| | | (0.080)*** | | (0.079)*** |
| Income ratio | | -0.379 | | -0.329 |
| | | (0.131)*** | | (0.127)*** |
| Pct pop 65+ | | 0.026 | | 0.020 |
| | | (0.005)*** | | (0.005)*** |
| Pct kids | | 0.019 | | 0.018 |
| | | (0.004)*** | | (0.004)*** |
| Pct BA | | 0.008 | | 0.006 |
| | | (0.002)*** | | (0.002)*** |
| Central County | | -0.071 | | -0.071 |
| | | (0.034)** | | (0.034)** |
| Suburban County | | -0.137 | | -0.141 |
| | | (0.026)*** | | (0.026)*** |
| ln(land area) | | 0.094 | | 0.106 |
| | | (0.018)*** | | (0.018)*** |
| Federal revenue | | 0.418 | | 0.421 |
| | | (0.119)*** | | (0.115)*** |
| State revenue | | 0.128 | | 0.094 |
| | | (0.047)*** | | (0.046)** |
| Republican presidential vote | | -0.001 | | -0.001 |
| | | (0.001) | | (0.001) |
| population change 1980-1992 | | -0.003 | | -0.003 |
| | | (0.001)*** | | (0.001)*** |
| ln(municipalities) | | -0.042 | | -0.062 |
| | | (0.025)* | | (0.022)*** |
| ln(townships) | | -0.010 | | -0.038 |
| | | (0.018) | | (0.022)* |
| ln(special districts) | | 0.068 | | 0.076 |
| | | (0.016)*** | | (0.016)*** |
| ln(school districts) | | -0.056 | | -0.033 |
| | | (0.021)*** | | (0.022) |
| Adj R-squared | 0.01 | 0.25 | 0.05 | 0.27 |
| N | 2928 | 2928 | 2965 | 2965 |
| Number of counties above turning point | 401 | 1753 | 1322 | 2368 |

Robust standard errors in parentheses. Electoral density is measured as ln(offices per 10 general purpose governments) and as ln(offices per 1 million capita). The dependent variables is ln(own-source revenue per capita). All other variables are defined as shown in Table 3A.
*** p<0.01, ** p<0.05, * p<0.1

**Table 5. Predicted Own-Source Revenues at Different Values of Electoral Density**

| Offices per Government | | | | | |
|---|---|---|---|---|---|
| Percentile | ln(offices per 10 governments) | Offices per government | Predicted Revenue | First Difference | Second Difference |
| 1 | 1.83 | 0.63 | 878.65 | | |
| 5 | 2.42 | 1.12 | 952.26 | 73.61 | |
| 10 | 2.64 | 1.40 | 846.04 | -106.21 | -32.61 |
| 25 | 3.00 | 2.00 | 839.90 | -6.14 | -112.36 |
| 50 | 3.40 | 3.00 | 838.85 | -1.05 | -7.20 |
| 75 | 4.01 | 5.50 | 849.03 | 10.18 | 9.13 |
| 90 | 4.50 | 9.00 | 868.01 | 18.98 | 29.17 |
| 95 | 4.70 | 11.00 | 878.64 | 10.63 | 29.61 |
| 99 | 5.44 | 23.00 | 933.43 | 54.79 | 65.42 |

| Offices per Capita | | | | | |
|---|---|---|---|---|---|
| Percentile | ln(offices per 1 million capita) | Offices per 1000 capita | Predicted Revenue | First Difference | Second Difference |
| 1 | 2.76 | 0.02 | 917.56 | | |
| 5 | 3.94 | 0.05 | 838.60 | -78.95 | |
| 10 | 4.64 | 0.10 | 814.26 | -24.35 | -103.30 |
| 25 | 5.72 | 0.31 | 805.44 | -8.82 | -33.16 |
| 50 | 6.63 | 0.76 | 824.73 | 19.30 | 10.48 |
| 75 | 7.41 | 1.66 | 861.98 | 37.24 | 56.54 |
| 90 | 8.27 | 3.92 | 928.29 | 66.31 | 103.56 |
| 95 | 8.82 | 6.79 | 987.02 | 58.73 | 125.04 |
| 99 | 9.77 | 17.47 | 1125.47 | 138.45 | 197.18 |

This table presents predicted values of own-source revenue for different values of electoral density based on the coefficients from Table 4. The first column denotes various points in the empirical distribution of electoral density. The second column shows the corresponding value of electoral density in the natural log form used in the regression models. The third column translates the value of electoral density into its original form for easier interpretation. The fourth column shows predicted mean spending at each percentile in the electoral density distribution. The final two columns present marginal shifts in revenue, equivalently, the difference between the given and immediately prior listed percentile (or two prior listed percentiles) of electoral density. For computing the predicted values, all covariates are set at means except dummy variables which are set to their mode.

## Table 6. Polynomial Regression Models of Local Government Expenditures

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| ln(offices per govt) | -0.109 | -0.127 | | |
| | (0.054)** | (0.048)*** | | |
| ln(offices per govt)$^2$ | 0.015 | 0.016 | | |
| | (0.008)* | (0.007)** | | |
| ln(offices per capita) | | | -0.240 | -0.146 |
| | | | (0.025)*** | (0.033)*** |
| ln(offices per capita)$^2$ | | | 0.020 | 0.011 |
| | | | (0.002)*** | (0.002)*** |
| ln(avg. council size) | | 0.040 | | 0.041 |
| | | (0.029) | | (0.028) |
| ln(income per capita) | | 0.695 | | 0.709 |
| | | (0.053)*** | | (0.052)*** |
| ln(population) | | -0.277 | | -0.235 |
| | | (0.053)*** | | (0.077)*** |
| ln(offices per capita)$^2$ | | 0.011 | | 0.008 |
| | | (0.002)*** | | (0.004)** |
| ethnic fractionalization | | 0.141 | | 0.141 |
| | | (0.049)*** | | (0.048)*** |
| Income ratio | | -0.098 | | -0.081 |
| | | (0.095) | | (0.091) |
| Pct pop 65+ | | 0.015 | | 0.013 |
| | | (0.003)*** | | (0.003)*** |
| Pct kids | | 0.015 | | 0.015 |
| | | (0.003)*** | | (0.003)*** |
| Pct BA | | 0.005 | | 0.004 |
| | | (0.001)*** | | (0.001)*** |
| Central County | | -0.038 | | -0.039 |
| | | (0.024) | | (0.024) |
| Suburban County | | -0.090 | | -0.093 |
| | | (0.016)*** | | (0.016)*** |
| ln(land area) | | 0.048 | | 0.057 |
| | | (0.012)*** | | (0.011)*** |
| Federal revenue | | 0.662 | | 0.640 |
| | | (0.067)*** | | (0.065)*** |
| State revenue | | 0.435 | | 0.403 |
| | | (0.035)*** | | (0.034)*** |
| Republican presidential vote | | -0.001 | | -0.001 |
| | | (0.001) | | (0.001) |
| population change 1980-1992 | | -0.002 | | -0.001 |
| | | (0.000)*** | | (0.000)*** |
| ln(municipalities) | | -0.042 | | -0.025 |
| | | (0.015)*** | | (0.014)* |
| ln(townships) | | -0.006 | | -0.015 |
| | | (0.012) | | (0.013) |
| ln(special districts) | | 0.036 | | 0.042 |
| | | (0.010)*** | | (0.010)*** |
| ln(school districts) | | 0.008 | | 0.020 |
| | | (0.014) | | (0.014) |
| Adj R-squared | 0.00 | 0.28 | 0.04 | 0.30 |
| N | 2928 | 2928 | 2965 | 2965 |
| Counties above turning point | 1039 | 764 | 2035 | 1295 |

Robust standard errors in parentheses. Electoral density is measured as ln(offices per 10 general purpose governments) and as ln(offices per 1 million capita). The dependent variables is ln(own-source revenue per capita). All other variables are defined as shown in Table 3A.
*** p<0.01, ** p<0.05, * p<0.1

## Table 7. Effect of Electoral Density on Budget Line Items

| Spending Function | Elected Offices per government | | Elected Offices per government squared | | |
|---|---|---|---|---|---|
| | Coefficient | Std Error | Coefficient | Std Error | $R^2$ |
| Air transportation | -23.31*** | (6.90) | 2.578*** | (0.90) | 0.13 |
| Misc commercial activities | 0.156 | (0.22) | -0.0234 | (0.032) | 0.01 |
| Corrections | -1.595 | (5.48) | -0.155 | (0.81) | 0.02 |
| Elementary & secondary education | -83.23* | (42.5) | 6.245 | (5.93) | 0.34 |
| Higher education | -58.29** | (25.7) | 7.701* | (4.24) | 0.10 |
| Financial administration | -8.196 | (5.58) | 1.243** | (0.60) | 0.19 |
| Fire protection | -5.570 | (5.06) | 0.901 | (0.71) | 0.36 |
| Judicial | -5.036** | (2.41) | 1.145*** | (0.32) | 0.13 |
| Central staff | -10.59** | (4.54) | 1.613*** | (0.61) | 0.16 |
| Public buildings | -5.952*** | (2.27) | 0.967*** | (0.32) | 0.09 |
| Health | -12.29 | (13.1) | 1.669 | (1.83) | 0.06 |
| Own hospitals | 17.61 | (46.4) | -1.355 | (6.40) | 0.05 |
| Other hospitals | -1.417 | (1.06) | 0.147 | (0.13) | 0.02 |
| Regular highways | -32.10 | (25.8) | 5.798 | (3.60) | 0.30 |
| Toll highways | -0.0622 | (0.084) | 0.00860 | (0.012) | 0.03 |
| Public transit | 0.255 | (0.17) | -0.0191 | (0.022) | 0.02 |
| Housing and community development | -8.397 | (9.51) | 0.354 | (1.07) | 0.26 |
| Libraries | -1.760 | (2.15) | 0.217 | (0.27) | 0.11 |
| Natural resources | 2.086 | (9.23) | -0.682 | (1.17) | 0.04 |
| Parking | -0.271 | (1.00) | 0.0151 | (0.13) | 0.16 |
| Parks and recreation | -18.58** | (7.80) | 2.831*** | (1.09) | 0.22 |
| Police | -6.922 | (6.82) | 1.582* | (0.90) | 0.23 |
| Protective inspection | -1.310 | (0.83) | 0.194* | (0.10) | 0.23 |
| Welfare, categorical assistance | 1.702 | (4.29) | -0.247 | (0.56) | 0.10 |
| Welfare, cash assistance | -0.392 | (2.18) | 0.149 | (0.28) | 0.02 |
| Welfare, medical payments | -0.765 | (1.35) | 0.148 | (0.17) | 0.02 |
| Welfare, vendor payments | -0.453 | (0.74) | 0.0428 | (0.089) | 0.02 |
| Welfare, insurance | -0.601* | (0.34) | 0.115** | (0.051) | 0.04 |
| Welfare, other | 9.077 | (12.7) | -0.875 | (1.92) | 0.04 |
| Sewerage | 9.840 | (12.8) | -0.780 | (1.69) | 0.12 |
| Solid waste management | -16.32** | (8.18) | 2.164* | (1.17) | 0.06 |
| Water transit | -3.870 | (3.04) | 0.315 | (0.36) | 0.03 |
| Interest on general debt | -100.7** | (51.3) | 16.90** | (7.95) | 0.03 |
| Other expenditures | -15.98 | (15.3) | 2.714 | (2.33) | 0.08 |

Models include all control variables shown in Table 6 (not shown). Robust standard errors are in parentheses. Electoral density is measured as ln(offices per 10 general purpose governments). Line-item spending is measured in dollars per capita.

## Table 8. Polynomial Regression Models of Long Term Debt

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| ln(offices per govt) | -1.316 | -0.599 | | |
| | (0.223)*** | (0.206)*** | | |
| ln(offices per govt)$^2$ | 0.112 | 0.067 | | |
| | (0.031)*** | (0.027)** | | |
| ln(offices per capita) | | | -0.285 | -0.372 |
| | | | (0.117)** | (0.150)** |
| ln(offices per capita)$^2$ | | | -0.018 | 0.024 |
| | | | (0.010)* | (0.011)** |
| ln(avg. council size) | | -0.035 | | 0.026 |
| | | (0.122) | | (0.122) |
| ln(income per capita) | | 1.612 | | 1.461 |
| | | (0.245)*** | | (0.260)*** |
| ln(population) | | 1.706 | | 2.051 |
| | | (0.287)*** | | (0.349)*** |
| ln(offices per capita)$^2$ | | -0.063 | | -0.084 |
| | | (0.013)*** | | (0.015)*** |
| ethnic fractionalization | | 0.142 | | 0.154 |
| | | (0.247) | | (0.248) |
| Income ratio | | -0.302 | | -0.499 |
| | | (0.398) | | (0.411) |
| Pct pop 65+ | | 0.055 | | 0.065 |
| | | (0.016)*** | | (0.016)*** |
| Pct kids | | 0.041 | | 0.049 |
| | | (0.013)*** | | (0.013)*** |
| Pct BA | | 0.007 | | 0.012 |
| | | (0.007) | | (0.007)* |
| Central County | | -0.108 | | -0.110 |
| | | (0.074) | | (0.075) |
| Suburban County | | -0.099 | | -0.090 |
| | | (0.064) | | (0.065) |
| ln(land area) | | -0.134 | | -0.124 |
| | | (0.056)** | | (0.056)** |
| Federal revenue | | 0.796 | | 0.796 |
| | | (0.383)** | | (0.388)** |
| State revenue | | 0.481 | | 0.488 |
| | | (0.155)*** | | (0.155)*** |
| Republican presidential vote | | -0.007 | | -0.007 |
| | | (0.003)** | | (0.003)** |
| population change 1980-1992 | | 0.005 | | 0.005 |
| | | (0.002)*** | | (0.002)*** |
| ln(municipalities) | | -0.115 | | 0.054 |
| | | (0.083) | | (0.065) |
| ln(townships) | | -0.032 | | -0.068 |
| | | (0.070) | | (0.070) |
| ln(special districts) | | 0.144 | | 0.135 |
| | | (0.047)*** | | (0.047)*** |
| ln(school districts) | | -0.119 | | -0.118 |
| | | (0.065)* | | (0.068)* |
| Adj R-squared | 0.04 | 0.23 | 0.17 | 0.24 |
| N | 2928 | 2928 | 2965 | 2965 |
| Counties above turning point | 10 | 322 | NA | 523 |

Robust standard errors in parentheses. Electoral density is measured as ln(offices per 10 general purpose governments) and as ln(offices per 1 million capita). The dependent variables is ln(own-source revenue per capita). All other variables are defined as shown in Table 3A.
*** p<0.01, ** p<0.05, * p<0.1

## Table 9: First-Differences Estimates, 1987-1992

| | Dependent Variable: ln(own-source revenue per capita) | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| ln(offices per govt) | -0.256 | -0.206 | | |
| | (0.121)** | (0.103)** | | |
| ln(offices per govt)$^2$ | 0.049 | 0.039 | | |
| | (0.020)** | (0.017)** | | |
| ln(offices per capita) | | | -0.153 | -0.162 |
| | | | (0.071)** | (0.077)** |
| ln(offices per capita)$^2$ | | | 0.014 | 0.017 |
| | | | (0.006)** | (0.007)*** |
| ln(avg. council size) | | -0.080 | | -0.067 |
| | | (0.024)*** | | (0.025)*** |
| ln(population) | | 1.105 | | 1.565 |
| | | (0.403)*** | | (0.401)*** |
| ln(population)$^2$ | | -0.042 | | -0.061 |
| | | (0.019)** | | (0.019)*** |
| ln(functional performance index) | | 1.233 | | 1.227 |
| | | (0.090)*** | | (0.090)*** |
| ln(municipalities) | | 0.097 | | -0.030 |
| | | (0.109) | | (0.092) |
| ln(townships) | | -0.031 | | -0.055 |
| | | (0.026) | | (0.032)* |
| ln(special districts) | | 0.013 | | 0.013 |
| | | (0.015) | | (0.015) |
| ln(school districts) | | 0.096 | | 0.098 |
| | | (0.047)** | | (0.046)** |
| Adj R-squared | 0.01 | 0.14 | 0.01 | 0.14 |
| N | 2931 | 2931 | 2968 | 2967 |

All variables are expressed as first-differences between 1987 and 1992. Robust standard errors are in parentheses. Electoral density is measured as ln(offices per 10 general purpose governments) and as ln(offices per 1 million capita). All other variables are defined as shown in Table 3A. * p<.10, ** p<.05, *** p<.01

Readers with comments should address them to:

Professor Jacob E. Gersen
University of Chicago Law School
1111 East 60th Street
Chicago, IL 60637
jgersen@uchicago.edu

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