American Economic Journal: Economic Policy 3 (February 2011): 1–24
http://www.aeaweb.org/articles.php?doi=10.1257/pol.3.1.1

The Cabals of a Few or the Confusion of a Multitude: The Institutional Trade-off Between Representation and Governance†

By Leah Brooks, Justin Phillips, and Maxim Sinitsyn*

Our model illustrates how political institutions trade off between the competing goals of representation and governance, where governance is the responsiveness of an institution to a single pivotal voter. We use exogenous variation from the 30-year history of the federal Community Development Block Grant program to identify this trade-off. Cities with more representative governments—those with larger city councils—use more grant funds to supplement city revenues rather than implementing tax cuts, thereby moving policy further away from the governance ideal. In sum, more representative government is not without cost. (*JEL* D72, H71, R50)

In *Federalist Paper 10*, James Madison frames the balancing of representation and governance in this way: "… however small the republic may be, the representatives must be raised to a certain number, in order to guard against the cabals of a few; and … however large it may be, they must be limited to a certain number, in order to guard against the confusion of a multitude ….” Designing this balance between representing the voices of many while retaining the ability to govern remains critical today, whether politicians are disputing the size and composition of a new Iraqi legislature or the number of representatives for the recently enlarged city of Montreal.

Like Madison, we hypothesize that government institutions moderate a trade-off between representation and governance. By representation, we mean a representation of the full distribution of preferences in the decision making process. The representative ideal is each individual voter having voice in each policy decision. By

* Brooks: Department of Economics, University of Toronto, 150 St. George St., Toronto, ON M5S3G7 Canada (e-mail: leah.brooks@utoronto.ca); Phillips: Department of Political Science, Columbia University, 7th Floor, International Affairs Bldg., 420 W. 118th Street, New York, NY 10027 (e-mail: jhp2121@columbia.edu); Sinitsyn: Department of Economics, McGill University, 855 Sherbrooke St., West, Montreal, QC H3A 2T7 Canada (e-mail: maxim.sinitsyn@mcgill.ca). We are extremely grateful to Housing and Urban Development staffers Todd Richardson for sending us the historical CDBG allocation data that made this project possible, and to Sue Miller, for exceedingly useful background information and speedy replies. For help assembling the data, we thank John Curry, Governments Division, Census Bureau for the quick and helpful access to the time-consistent Annual Survey of Governments Data, and Alon Axelrod, ICPSR, for advice on the older census data. We thank Janet Currie for encouragement at a critical juncture, Sandra Black, John Galbraith, Shigeo Hirano, Jenny Hunt, Robert Jarman, Yan Lee, Byron Lutz, Enrico Moretti, Daniel Parent, Michael Smart, Colin Stewart, Melissa Schwartzberg, Tymon Tatur, and many seminar participants for helpful comments and advice. Finally, this project has benefitted from the support of McGill and Columbia University start-up funds, as well as funding from the Russell Sage Foundation's program on inequality.

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governance, we mean responsiveness to a single voter pivotal. The governance ideal is a benevolent dictator who perceives and implements the needs of the pivotal voter on every issue. Democratic institutions fall somewhere between these two extremes, using mechanisms to limit voice and thereby facilitate agreement on policies.

We begin by building a model to illustrate this trade-off that combines elements from two classic political economy traditions. From David F. Bradford and Wallace E. Oates (1971) we take the intuition that voters enjoy both public and private goods, and so do not desire that an increase in public income via grants be spent entirely by the public sector. From Barry R. Weingast, Kenneth A. Shepsle, and Christopher Johnsen (1981), we take the intuition that in a district-based system of representation, where taxes are levied across all districts, politicians perceive the full benefits of a project in their district, but do not perceive the full costs. The Weingast et al. model suggests that total per capita spending increases in the size of the representative body. By combining these two strands of the literature, our model suggests that if there is a trade-off between representation and governance, governments with more representatives should translate more grant revenue into total revenue than governments with fewer representatives.

We test for evidence of this trade-off between representation and governance by examining the behavior of US cities. We interpret the extent of representation as the size of the city's council: the larger the city council, the more representative the government. As we discuss in more detail below, there is substantial empirical evidence that larger governing bodies are, on average, more likely to include a wider range of preferences. In addition, we offer evidence that council size changes only very infrequently, and that council size is unrelated to other key municipal institutions. Our measure of governance is the responsiveness of municipal revenues to grant receipt: as this response diverges from the choice of the single pivotal voter, governance decreases.[1]

In particular, we examine the responsiveness of municipal revenues to Community Development Block Grant (CDBG) funds as a function of city council size. CDBG is a lump sum annual entitlement grant from the federal government to cities, administered by a formula. Because of the nature of the formula, the grant provides ideal exogenous variation in revenue by which we can analyze the effect of council size. The formula awards funds as a function of a city's poverty, overcrowding, age of buildings, and lack of population growth relative to all other cities. However, these formula elements are not direct measures of local need. We exploit the difference between the relative measures of need and the local measures of need to identify exogenous variation in grant income.

For this analysis, we have assembled a 30-year panel of cities including information on city finances, demographics, grant receipts, and council size. Using these data, we find that the average city increases total revenues by about one dollar for every one dollar of grant received. The larger the city council, the more responsive

[1] We do not mean to suggest that adherence to the preferences of the median voter is the Madisonian governance ideal. In Federalist 51, Madison argues that the preservation of individual liberty requires insulating the government from the passions of majorities. As is common in the public finance literature, we treat the governance ideal as perfect responsiveness to the preferences of the median voter. Notions of ideal governance are widely debated within normative political theory.

total revenues are to grant receipts: revenues increase by ten cents for each additional council member. This effect is unique to council size: we do not observe the same trade-off with population or other municipal institutions. While these results are extremely suggestive of our posited trade-off between representation and governance, they are also consistent with plausible alternative models that explain governmental behavior as a function of legislature size.

This paper links two literatures in public finance and political economy. One is a long-standing interest in the effects of the size of legislatures, which frequently contends that politicians perceive the public budget as a common pool and thus overspend.[2] Shepsle (1988) discusses a trade-off between representation and governance in the US Congress, and offers qualitative evidence in support of this hypothesis. Brian Knight (2004) shows that individual Congressional representatives respond to both common pool problems in allocating funds and tax costs in their allocation, as our model also hypothesizes. Douglas Muzzio and Tim Tompkins (1989) examine the impact of council size qualitatively, looking over a century of changes in New York City's council size and structure. Empirical work examining the effect of the size of legislative institutions on cities shows that city spending increases in council size (Reza Baqir 2002); our paper improves on Baqir (2002) by estimating the effects of council size net of city fixed effects and trends. In contrast, Per Pettersson-Lidbom (2008) offers regression-discontinuity evidence from Sweden and Finland, suggesting that spending decreases in council size. We discuss explanations for these divergent results in Section V.[3]

The second literature to which we contribute is the public finance literature on the effect of intergovernmental grants on recipient government finances.[4] Until Knight (2002), most papers in this literature found that local government expenditures did not respond as the economic model predicts, instead turning almost all (or more than all) of grant revenues into total revenues (see James R. Hines, Jr. and Richard H. Thaler 1995 for a review). Knight (2002), and subsequently Byron Lutz (2010) and Nora Gordon (2004), which carefully controls for grant endogeneity, finds results consistent with the theoretical prediction that most grant revenue is returned via a tax cut. Still, other recent, careful empirical work, including William N. Evans and Emily G. Owens (2007) and Monica Singhal (2008), does find evidence that grants add to total revenue (while others, such as Katherine Baicker and Douglas Staiger 2005, fall somewhere in between). Thus, how and why local governments respond to grant funds remains an open empirical question to which our paper adds a partial answer.

[2] James M. Buchanan and Gordon Tullock (1962) is a seminal analysis of the common pool issues. Empirical articles include Thomas W. Gilligan and John G. Matsusaka (1995), Gilligan and Matsusaka (2001), John Charles Bradbury and W. Mark Crain (2001), and Lynn MacDonald (2008). Another related literature discusses the optimal size of a legislature: George J. Stigler (1976) defines the size of the legislature as one of the two key mechanisms for representation.

[3] Though Martin J. Osborne, Jeffrey S. Rosenthal, and Matthew A. Turner (2000) and Turner and Quinn Weninger (2005) do not explicitly consider the size of legislatures, both consider the related problem of representation, in discussing the determinants of participation in meetings with costly participation. These authors also hypothesize that institutional structures which encourage representation may lead to inefficient outcomes. Our work complements the empirical test in Turner and Weninger (2005), which focuses more carefully on representative inputs; our analysis focuses more heavily on governance outputs.

[4] This is sometimes referred to as the "flypaper" literature.

I. Theory

To illustrate the trade-off between representation and governance, we combine features of two canonical models. For fiscal federalism, we draw on the work of Bradford and Oates (1971), which shows that municipal governments should add little to total revenues in response to a lump-sum grant. We add to this model the political effects of districting. For the effects of the size of legislatures, we turn to Weingast, Shepsle, and Johnsen (1981), which shows that spending increases in the number of members of a legislature. We add a framework where pivotal voters consider both public and private goods in choosing the level of public goods. Fundamentally, we are interested in how the legislature's behavior toward grants changes with legislature size.[5]

Consider a city that has N elected officials, each representing a district $i \in \{1, \ldots N\}$. We discuss the model as if these districts are geographically based. The predictions of the model remain unchanged as long as politicians represent the preferences of the pivotal voter in their constituency. Thus, if members of a legislature elected at-large respond to specific constituencies, this model applies to at-large legislatures as well.[6] The official in district i aims to satisfy the desires of the exogenously given pivotal voter in his district. For simplicity of exposition we assume that pivotal voters in all districts have the same utility $U(C_i, G_i)$, where C_i is the private consumption good and G_i is the local public good in district i. We assume that $U_{CC} < 0$, $U_{GG} < 0$, and $U_{CG} > 0$. The extent of representation here and throughout the paper is N, the size of the city council. We justify this measure in more detail in Section IIIB.

We assume that the city council's decision rule follows the norm of universalism, which "assures any interested district a project" (Weingast, Shepsle, and Johnsen 1981). Our result does not depend critically on this solution concept. Our main result is robust to the leading alternative minimum winning coalition solution concept (as in David P. Baron 1991) under certain parameter values of the utility function. To keep the presentation brief, we present the model here using universalism and refer readers to web Appendix A for details of the alternative solution. Thus, our results should not be interepreted as testing the norm of universalism.

Under the norm of universalism, each elected official independently determines the size of the project he wants for his district, knowing that the city council will approve his request. We believe that universalism is a good description of how decisions are made in city governments.[7] In fact, universalism is likely a better description of local government solutions than of state legislator behavior on which this theory has been frequently tested (see Jowei Chen and Neil Malhotra 2007 for a recent example). For the reasons detailed below, we believe this decision rule more closely approximates municipal behavior than the primary alternative, the minimum winning coalition, which assumes that most votes pass with a slim majority.

[5] Specifically, our model delineates the costs of representation and is silent on the benefits. In Section IIIB, we discuss benefits of representation.

[6] We discuss further theoretical and empirical evidence on at-large legislatures in Section IIIB.

[7] Theoretically, Anke S. Kessler (2007) shows that universalism can emerge endogenously in a federal system. Baron (1991) extends the Weingast, Shepsle, and Johnsen (1981) result, showing that total spending increases in the number of districts, without reliance on the norm of universalism.

In most US cities, council members are officially non-partisan, which facilitates universalistic decisions, rather than those assembled by partisan coalitions. In Chicago and Los Angeles, for which we have analyzed roll call votes in recent years, we find very few non-unanimous votes. Gary W. Cox and Timothy N. Tutt (1984) find that the five-member Los Angeles County Board of Supervisors has, in many cases, an official "rule of five" for dividing funds received from other levels of government equally among the five supervisorial districts. Thus, we model the total amount of public good approved by the city council as the sum of those public goods preferred by each district: $G = \sum_{i=1}^{N} G_i$.

The total bill for the public good in the city is $p_G G$, where p_G is the price of the public good. To finance the public good, we assume that all citizens are taxed equally: each citizen's tax share is equal to $1/L$, where L is the city population. This tax share is independent of the number of districts in the city. Thus, each citizen contributes $(1/L)p_G G$ to the city's public good bill.

The city also receives a federal grant. Our objective is to find out how responsive municipal revenues are to council size. A federal grant is essentially an increase in income of every citizen. Here we assume that the grant is exogenous, of amount A, and that each citizen has the same income I. The grant is equivalent to an increase in each citizen's income by A/L. In sum, the budget constraint facing the pivotal voter in district i is $I + (A/L) = p_C C_i + (1/L) p_G G$, where p_C is the price of the private good. Thus each voter spends his income on private goods and both the public good in his district and the public good for other districts.

The pivotal voter chooses C_i and G_i to maximize his utility subject to the budget constraint above. Since any one district's budget constraint depends on all other districts' public good choices, we search for the Nash Equilibrium values of private and public good consumption and label them C_i^* and G_i^* (the sum of the public goods in all districts is then G^*). Our first result is Proposition 1.[8]

PROPOSITION 1: *Spending on the public good increases with the size of the grant, but less than proportionally*, $0 < \partial(p_G G^*)/\partial A < 1$.

Intuitionally, an increase in income inspires voters to spend more on both public and private goods, regardless of council size. Thus, we do not expect tax receipts to increase by the full amount of the grant. In fact, estimates in the literature put the marginal propensity to spend on public goods ($\partial(p_G G)/\partial A$) at between five and ten percent (Hines and Thaler 1995). Given this preference for private goods, between 90 and 95 percent of each grant dollar should be returned to the pivotal voter as a tax cut.

We now turn to how the amount spent on the public good depends on the number of districts. The optimal choice by the pivotal voter in the single-district municipality maximizes total municipal welfare. Proposition 2 shows that as the number of districts goes up—as representation increases—the total amount of public good also

[8] Proofs for all propositions are in web Appendix A.

increases, moving away from the efficient level. This result is commonly referred to as the "Law of $1/n$."[9]

PROPOSITION 2: *As the number of districts*, N, *increases*, *the amount spent on the public good also increases*, $\partial(p_G G^*)/\partial N > 0$.[10]

We illustrate the intuition behind Proposition 2 with the following example. Imagine that each district would like to have its own swimming pool, but has to pay a share of the cost of all swimming pools in the city. The city chooses a level of revenue so that citizens may spend on both private goods and the public good of the swimming pool. As the number of districts increases—as representation increases, in our framework—there are two countervailing effects. First, there are more swimming pools in the city since each district builds it own pool. Each district does this since the public good is, by definition, district-specific. This effect increases the amount of income the city collects via taxation. Second, the pivotal voter in each district recognizes that as more pools are being built in the city, the total municipal pool bill increases. He responds by demanding a smaller pool in his district and using a smaller share of his income to finance it. For our restrictions on the utility function the first effect dominates, and an increase in the number of districts leads to a larger share of income being spent on public goods. Intuitively, as districts proliferate and are more closely representative of local needs, the more income is taxed, and the more public goods provision diverges from the efficient outcome.

Now we want to examine how the marginal propensity to spend on public goods from grant income depends on the number of districts. If we assume that the voters' preferences are homothetic (and, thus, the utility function is homogeneous of degree one), then the public good budget share does not depend on the level of income. Therefore, cities with larger city councils will spend more on the public good both from income (Proposition 2) and from additional grant income (Proposition 3). Proposition 2, which predicts that the share of the private budget devoted to public goods is larger in cities with more districts, would be difficult to test in the cross section, as fixed effects would not be identified. Proposition 3 provides a means to test this contention by examining cities' response to exogenous changes in grant funding.

PROPOSITION 3: *If the utility function U is homogeneous of degree one*, *then the politician's marginal propensity to spend on public goods increases with the number of districts:* $\partial^2(p_G G^*)/\partial A \partial N > 0$.

Proposition 3 says that cities with more council members turn more additional grant revenue into public spending. Cities with fewer council members turn grant

[9] If having more than one district decreases welfare, why do we observe multi-district jurisdictions? Intuitively, we believe that representation secures governmental stability. Thus, the practicable political choice may be between dictatorship with the efficient amount of public goods, or an inefficiently large amount of public goods provided by a six-, seven- or eight-member city council.

[10] Here, and in Proposition 3, we treat N as a continuous variable. Both propositions also hold if, as in our empirical model, N is an integer.

revenue into private spending. This result holds regardless of whether we consider total revenues or, as we will in the empirical analysis, per capita revenues.[11]

Returning to the swimming pool example from Proposition 2, with homothetic preferences, the public good budget share does not depend on the level of income. When this is the case, and income is augmented by a grant, the share of grant revenue spent on the public good increases with the number of districts.[12]

Our model is not inconsistent with the interpretation that some feature of council size causes government to behave in a more Leviathan-like way (that is, maximizing rent extraction).[13] For example, cities with larger councils may have more bureaucrats who extract more rents.[14] Or it may be that cities with larger councils offer representatives more opportunities to shirk fiscally restrained behavior, and it is this shirking that increases the amount of grant funds that appear in municipal revenues. As an empirical matter, we are not able to distinguish what particular feature of large councils leads to an increased translation of grant funds into municipal revenues.

In sum, for a city of fixed population, as the number of council districts increases, we expect the share of grant revenue that translates to the purchase of the public good to increase. In the language of this paper, institutions promulgate a trade-off between representation and governance.

II. CDBG History and Rules

Our analysis focuses on the CDBG program, the primary objective of which is to transform distressed urban neighborhoods into viable communities (Community Development Act of 1974, §101(b)1)). CDBG is the federal government's single largest source of aid to cities. Funds can be spent on any one of three national objective categories: helping low- and moderate-income people, eliminating slums and blight, and meeting urgent community development needs. These categories cover a vast multitude of municipal activities. For example, in a recent year, the city of Chicago spent money on studying the establishment of a tax increment financing district, purchasing 26 properties with the goal of "sparking economic development," and supporting after-school tutoring, recreation and leadership-building opportunities, among many other activities (HUD IDIS database 2006; see Michael J. Rich 1993 for a detailed history of the program and qualitative analysis). As we discuss later in more detail, CDBG is a formula-based program that awards lump sums, as in our model, to entitled cities, counties and states. Cities become entitled when they reach a population of 50,000 or more, or when

[11] Specifically, $\partial^2(p_G G^*)/\partial A \partial N = \partial^2(p_G(G^*/L))/\partial(A/L)\partial N$.

[12] This model is specified to examine the effect of an increase in the number of districts in one city. However, the same results hold if we compare municipal responsiveness to grant receipt in two cities that differ only in the size of their city councils. It is this comparison that we make in our empirical tests.

[13] The Leviathan concept is due to Thomas Hobbes (2003), and is extrapolated on in Geoffrey Brennan and Buchanan (1980).

[14] Empirically, this does not seem to be the case. The raw correlation between the number of local government employees and council size is 0.38. When we examine the correlation net of population (by using the residuals of regressions of each variable on population and log of population), it is less than 0.005.

they become the principal city of a metropolitan statistical area (Todd Richardson, Robert Meehan, and Michael Kelly 2003).[15]

III. Specification and Identification

A. *Specification*

Our model suggests that total revenues are a function of income, federal grants, the size of the city council, and an interaction between the grant and council size. We start by examining the effect of grants on total municipal revenues, omitting the interaction term, in order to demonstrate our identification strategy and to given a baseline to which we can compare the interacted specifications. We estimate

$$(1) \qquad \textit{total revenue } pc_{c,t} = \beta_0 + \beta_1 \textit{CDBG}\,pc_{c,t} + \beta_2 \mathbf{X}_{c,t} + \beta_3 \mathbf{city}_c + \beta_4 \mathbf{city\ trend}_{c,t} + \beta_5 \mathbf{year}_t + \epsilon_{c,t},$$

where *pc* stands for per capita, *c* denotes city, *t* denotes years 1975 to 2004, and $X_{c,t}$ is a vector of demographic controls. City fixed effects are $\mathbf{city}_c$, city-specific trends are $\mathbf{city\ trend}_{c,t}$ and year fixed effects are $\mathbf{year}_t$. We do not include a separate control for council size as it does not vary over time in our data and is thus included in each city's fixed effect. All coefficients with the exception of β_0 and β_1 are vectors. Because we employ a relatively long panel of 30 years, and because many of the fiscal variables of interest change gradually (and thus cannot be accounted for by the city fixed effect), our specification includes city-specific trends ($\mathbf{city\ trend}_{c,t}$). To ensure that our results are driven by CDBG and not by other grants, as a specification check we add a control for all non-CDBG intergovernmental revenue. We use standard errors clustered at the level of the city. The coefficient of interest here is β_1, which describes how responsive municipal revenues are to grant receipt.

After establishing the baseline effect of grant receipt on total municipal revenues, we allow that effect to vary by city council size by estimating

$$(2) \qquad \textit{total revenue } pc_{c,t} = \delta_0 + \delta_1 \textit{CDBG}\,pc_{c,t} + \delta_2 \textit{CDBG}\,pc_{c,t} CS_c + \delta_3 \mathbf{X}_{c,t} + \delta_4 X_{c,t} CS_c + \delta_5 \mathbf{city}_c + \delta_6 \mathbf{city\ trend}_{c,t} + \delta_7 \mathbf{year}_t + \epsilon_{c,t}.$$

CS_c is the number of city council members in city *c*. We interpret δ_2 as the responsiveness of total revenue to grant funds associated with an additional city council member. As we detail below, identification in this estimation comes from the interaction of the

[15] At first, this 50,000 population discontinuity seems like a promising avenue for identification; we explain in web Appendix B why this does not turn out to be the case. The states and counties that are awarded CDBG funds are restricted from spending those funds on already-entitled cities. In this paper, we focus exclusively on the entitled city portion of the program, which accounts for roughly half of the total program budget. Means for these cities are presented in Appendix Table 3.

constant council size with exogenous changes in grant revenue. Proposition 3 of our theoretical model predicts that $\delta_2 > 0$. We estimate a complete interaction by also interacting CS_c with all demographic characteristics $\mathbf{X}_{c,t}$. This is a very demanding specification, which allows us to separate the effect of council size in interaction with the grant from all other possible interacted effects of council size. For example, this specification ensures that our results are not driven by differential revenue behavior by poverty rate in cities with large councils relative to cities with small councils.

Finally, because the theory offers no particular justification for a linear effect of council size on the responsiveness of revenues to grant funds, we estimate a model that provides for a non-linear effect of council size, allowing the grant to have a differential impact on revenues above and below the 25th percentile (cities with five or fewer members relative to cities with six or more members), the median (seven members versus eight or more) and the 75th percentile (nine members versus ten or more) of council size.[16] Specifically, we examine effects above and below a percentile of interest p, by estimating

$$(3) \quad \textit{total revenue pc}_{c,t} = \gamma_0 + \gamma_1 \textit{CDBGpc}_{c,t} \times \{1 \text{ if } CS_c \leq p\} + \gamma_2 \textit{CDBGpc}_{c,t} \times \{1 \text{ if } CS_c > p\} + \gamma_3 \mathbf{X}_{c,t} \times \{1 \text{ if } CS_c \leq p\} + \gamma_4 X_{c,t} \times \{1 \text{ if } CS_c > p\} + \gamma_5 \mathbf{city}_c + \gamma_6 \mathbf{city\ trend}_{c,t} + \gamma_7 \mathbf{year}_t + \epsilon_{c,t}.$$

We again allow the controls $X_{c,t}$ to have a differential effect on the dependent variable above and below the pth percentile of the distribution of CS_c (interacting each element of $X_{c,t}$ with $\{1 \text{ if } \mathrm{CS}_{c,t} \leq p\}$ and $\{1 \text{ if } \mathrm{CS}_{c,t} > p\}$). We then test the prediction of our model that $\gamma_1 < \gamma_2$. If the coefficient for the larger council size is greater than the coefficient for the smaller council, we interpret this as evidence that municipal revenues in cities above the pth percentile of council size are more responsive than cities below the pth percentile to changes in grant funds.

B. *Identification*

Our specification raises two major identification issues: the exogeneity of council size and the exogeneity of the grant amount. We deal with each of these in turn.

Council Size and Representation.—We identify variation in council size through a cross-sectional comparison of cities. Whether this is a valid strategy depends on two issues: how and when council size is determined, and whether council size correlated with other key institutional variables. As a matter of practice, city councils

[16] An alternative method is to allow a separate effect of the grant for each council size, or for small groupings of council size. Results from this type of specification generally have coefficients on the grant by size dummies that increase in council size. However, the standard errors increase substantially, and the coefficients fluctuate much more with the full interaction, suggesting that this specification may suffer from multi-collinearity.

change size very infrequently. Baqir (2002) writes that between 1980 and 1990, 4.2 percent of cities attempted to change their council size, and of those cities, only roughly half were successful. For example, Los Angeles had a major charter reform in 1925, switching from a nine- to a fifteen-member council. Despite the fact that its population has tripled since 1925, the city retains a fifteen-member council. At the small end of the scale, the city of Temple, TX's 1970 population of about 30,000 people roughly doubled by 2000, but the city retains a four-member council.

Web Appendix Table 4 presents the full distribution of council sizes, showing that the substantial majority of cities have councils of between 4 and 13 people. To more generally probe the determinants of council size, including its correlation with other institutional features, we estimate a cross-sectional regression of council size as a function of regional dummies (the nine census divisions), population, population squared and cubed, population rank in 1987 (the year of the council size data), year of incorporation, the at large share of the council, a dummy for whether the mayor is directly elected, a dummy for whether the city has the council-manager form, and a dummy for whether the city has home rule powers. In this estimation, linear population, two of the regional dummies, and two of the institutional variables (at large share of council, and a dummy for the council manager form of government) are significant predictors of council size. Results are very similar when we replace the continuous council size variable with dummy variables for whether a city has a council above or below key thresholds in the council size distribution. This motivates our careful controls for population when we examine the effect of council size on revenue responsiveness to grant receipt; we will control not just for linear population, but other functions of population as well. This finding also cautions us to ensure our results are not driven by institutional features correlated with council size; when we discuss the robustness of our estimates we show that this is not the case.

Our model assumes that larger city councils are more representative of the populations they serve. The idea that more views are represented when there are more voices is intuitive, and is supported by empirical evidence. If preferences are correlated with geography, and council districts are geographic, then more districts implies a broader representation of tastes. Nicholas O. Alozie and Lynne L. Manganaro (1993) find that the probability that a city council has at least one woman increases in council size, and Jessica Trounstine and Melody E. Valdini (2008) show that the share of women and Latinos on city councils increases with council size. Our data confirm the finding that the probability that a council has at least one woman or minority increases in size.[17, 18] Our theoretical model does not specifically lay out

[17] Specifically, we do a probit regression of presence of woman or minority as a function of population, log of population, share hispanic, share black, share at-large council members, median family income, poverty rate, share of persons 25 and over with a 4-year college degree, and share of persons 25 and over with at least a high school education. The regression covariates are from 1987 to match the year in which we observe council size and composition.

[18] Of course, increasing the size of a deliberative body is just one way to facilitate representation in budgetary policy. How else might a city increase the representativeness of its decision-making process? One obvious method is to extend the franchise to groups previously denied the right to vote. While of central importance in the past, this is much less relevant in US cities today as a result of the Voting Rights Act of 1965. Additionally, existing legislative districts could be re-drawn as to maximize diversity through, for instance, the creation of majority-minority districts (Gilligan and Matsusaka 2006, Gary King and Andrew Gelman 1991 both discuss how redistricting may influence political outcomes). Limiting incumbency advantage is yet another mechanism for extending or

the benefits of representation, but we believe that Western societies have developed many mechanisms for ensuring "diversity" (or representation) across a variety of institutions. The existence of such mechanisms argues that society places a premium on representation.

The last issue to consider, in examining council size, is the issue of by-district versus at-large elections. Our model describes cities where council members are elected by district. In the simplest model of an at-large election, council members are elected by the city as a whole and each represents the median voter. If this is the case, our model does not hold. Research shows, however, that the simple model holds only under very specific conditions which are not likely to be met. For example, Cox (1990) shows that this simple model is likely incorrect when there are twice as many candidates as seats, and Laura I. Langbein, Philip Crewson, and Charles Niel Brasher (1996) suggests that the simple model is incorrect if candidates band together into slates (as they commonly do). If council members garner support, as these two arguments suggest, from a sub-section of the electorate, the predictions of our model continue to hold. Shigeo Hirano (2006) offers evidence showing that support for candidates in at-large elections in Japan is strikingly geographically concentrated. In our sample, roughly half (54 percent) of cities have councils elected entirely at large, and a further 30 percent of cities have some at large representation on the council. Of the latter, the average at large share is 31 percent. We return to this issue in the empirical analysis.

Grants.—Research has shown that many grant programs are politically motivated, and thus that it is critical to control for the endogeneity of grants if we wish to correctly understand how grants affect fiscal outcomes (Kevin Milligan and Michael Smart 2005; Lutz 2010; Knight 2002; Gordon 2004).

Two general concerns may arise in an estimation of the effect of grant receipts on total revenues. First, if the grant amount is determined by a combination of lobbying expertise and tastes that are correlated with unmeasured attributes of a city that determine revenues, the coefficient on grant receipt will be biased upward. For example, cities that lobby for and receive grants for public housing demonstration projects, such as Hope VI, should be more interested in spending on public housing than cities that do not. This lobbying pathway cannot cause problems for our estimates of the effect of the CDBG program, because CDBG funds are determined strictly by formula, as we will show later in this paper. Since the inception of the program in 1976, this formula has changed only once.

However, even a formula grant could pose estimation problems if the elements that determine the formula grant also determine municipal revenues. In the case of CDBG, grants are awarded via a function of five variables, in an attempt to calculate a city's need relative to all other cities.[19] Our identification relies on the fact that local revenues are determined by local, not relative, characteristics. That is, local per capita revenue may well be a function of the municipal poverty rate, but, net of the

limiting the representativeness of a deliberative body (see Gelman and King 1990a and Gelman and King 1990b). Unfortunately, we are unable to measure incumbency advantage at the city level.

[19] For simplicity, this section omits many details of the granting process. See web Appendix B for complete details.

local poverty rate, it is not a function of a city's poverty relative to all other cities. Intuitively, our assumption means that per capita total revenue in Chicago should not be affected by changes in the number of poor people in Los Angeles. Thus, in our estimation, we control for all relevant local rates and let the identification come from the difference between local and relative rates and the nature of the formula. For example, instead of controlling for relative poverty (total number of poor people in city c/total number of poor people in all metropolitan areas) which determines the grant, we control for the local poverty rate (total people in poverty in city c/total population of city c).[20]

The grant has a "dual formula" and the Department of Housing and Urban Development calculates funds under both Formula A and Formula B. Formula A, in equation (4), allocates a city's share of the total grant monies as a function of the number of poor people in a city (pov_c) relative to the number of all poor people in metropolitan areas (pov_{MA}), the number of people in a city (pop_c) relative to the number of all people in metropolitan areas (pop_{MA}), and the number of overcrowded housing units in the city ($ov\ crwd_c$) relative to the number of all overcrowded housing units in metropolitan areas ($ov\ crwd_{MA}$). For brevity, we leave the specifics of Formula B to Appendix B; the formula includes poverty, age of structures, and the number of people slow-growing cities would have had had they grown at the average rate (growth lag).[21]

$$(4) \qquad grant\ share_{A,c} = \left((1/2)\frac{pov_c}{pov_{MA}} + (1/4)\frac{pop_c}{pop_{MA}} + (1/4)\frac{ov\ crwd_c}{ov\ crwd_{MA}} \right).$$

Each city's grant share is the maximum of its share from Formula A and Formula B, with a correction so that the shares do not add up to more than one.[22]

Our estimation is also aided by the fact that, while each city's grant is a function of things possibly known to a city, such as the local poverty rate or local changes in population, the identifying variation comes from things that are clearly difficult for any one city to observe. For example, an individual city's grant amount depends on the

[20] Fundamentally, this means that identification comes from temporary grant revenue shocks; we are silent on how cities respond to permanent grant revenue shocks.

[21] This dual formula applies to both entitled cities and entitled counties, and funds are awarded as a function of cities' and counties' relative joint need. Counties and cities share the same funding pot; for purposes of clarity, we omit counties from the discussion here.

[22] The general concern of grant and expenditures being co-determined is allayed in the present case, because variables in the grant formula are lags of current conditions.

Whether the strategy of controlling for local rates leaves any variation to be identified depends on how much of the variance in grant distribution is left after controlling for local, as opposed to relative, rates. To evaluate this, we regress per capita CDBG on log population, the municipal poverty rate, the municipal overcrowding rate, the local share of pre-1940 housing over all housing, the share of the municipal "growth lag population" over the total municipal population, and year fixed effects, which control for the total allocation available in each year. This regression finds that all the local rates are significantly related to the per capita grant. However, with a R^2 of only 0.39, the regression shows that there is still variance left to be explained. Adding city fixed effects increases the R^2 to 0.55. This suggests that while there is a substantial city-specific component to the grant, there is also time variation left to be exploited.

Similarly, while local rates are correlated with relative rates, the correlation leaves variance left for estimation. Normalizing the relative rate by population (e.g., $((pov_c/pov_{MA})/pop_c)$, we find the correlation between local poverty and relative poverty to be 0.83; for overcrowding, growth lag and age, these correlations are 0.81, 0.35, and 0.97, respectively.

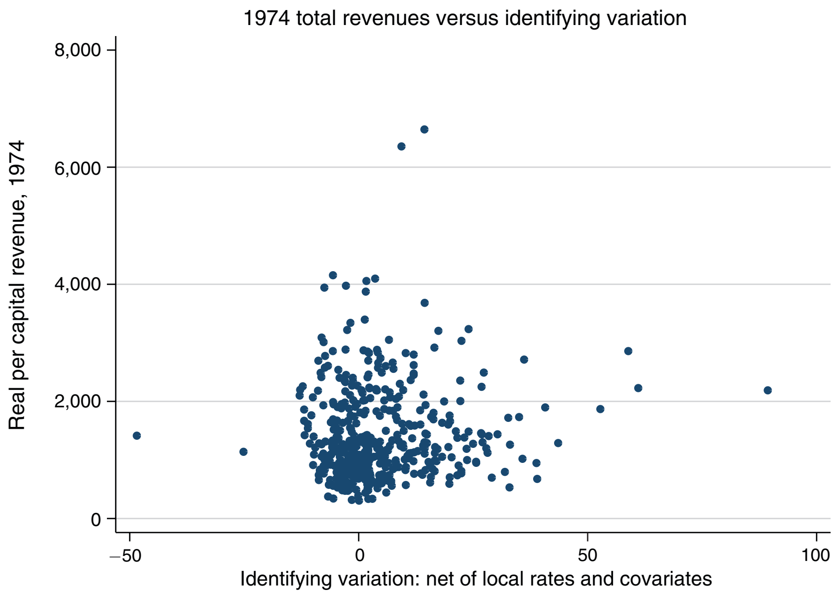

Figure 1. Identifying Variation

Notes: The *x*-axis reports predicted residuals from 1978 from a regression of CDBG per capita on local rates (poverty, growth lag, overcrowding, age of structures), the maximal set of covariates (column 4) in Table 2, city fixed effects, year fixed effects, and city-specific trends.

Source: See web Appendix B.

total poverty in all other cities and counties and the total number of cities and counties eligible for the program, net of information about the local poverty rate. Municipal officials in any given city are not likely to be well informed about these nuances.[23] For our framework to make analytical sense, municipal officials must also be able to make budgetary changes mid-year; John P. Forrester and Daniel R. Mullins (1992) document that this is the case for the overwhelming majority of cities.

Another way to evaluate the identifying variation in the data is to compare it to pre-CDBG era total revenue per capita. If the identifying variation is unrelated to current municipal revenues, it seems likely that it should not be related to municipal revenues just before the program. Figure 1 plots the last pre-CDBG year's (1974) total revenue on the *y*-axis. The *x*-axis reports estimated residuals for 1978 for a regression of the per capita CDBG grant on the local formula rates, city fixed effects, city specific trends, year fixed effects and the full set of covariates we will present in our regressions. There is no obvious correlation between these estimated residuals—the identifying variation—and per capita revenues, which is what we would expect if our identifying assumption is correct.[24]

[23] The structure of the data suggest two potentially useful discontinuities for estimation: the introduction of new data in the formula, and the entry of new cities into the program. We explain in Appendix B why these did not turn out to be promising margins.

[24] To be precise, there is a small positive and significant relationship between these two variables. However, when we drop the six (out of 474) outliers above the 99th percentile and below the first percentile of the grant residual distribution, we cannot at all reject ($p = 0.296$) that the coefficient is equal to zero. When we drop these

IV. Data

We study cities that have ever received CDBG funds. To do this, we combine data from a number of different sources; complete documentation on all datasets is in web Appendix C. From an internal HUD source, we have annual grant allocations from 1975 to 2001; further annual allocation information from 1993 to 2004 comes from the HUD Web site. We join these allocations by city to the Census's Historical Finance Database, which is a time-consistent series of Annual Surveys of Government data for cities and townships, 1970 to 2004. These data contain revenue, expenditure and debt patterns for local governments. This survey samples cities over 75,000 people with certainty in all years, and covers 90 percent of CDBG cities in all but four years, and over 85 percent of CDBG cities in all but two years. [25]

To these data we add demographic information—the same demographic information that HUD uses—on municipalities from the 1960, 1970, 1980, 1990, and 2000 Censuses.[26] In states where townships or towns also qualify as CDBG recipients, which is generally when such places have the powers of incorporated municipalities, we include them as well. We linearly interpolate all decennial census variables between survey years for use in the analysis. We include information on municipal political structure from the 1987 Census of Governments Organization File. This file describes municipal institutional features such as the size of the city council and whether or not the mayor is directly elected.[27] In order to replicate the CDBG allocation calculations ourselves we use additional sources of data described in web Appendix B.

The first goal of our data assembly is to verify that CDBG allocations follow HUD's formula. This test also demonstrates the strength of the estimation strategy, since identification relies on the formula nature of the grant. The average correlation between the true allocation and our constructed allocation across the 30 years of our sample is 0.98; the correlation is more than 0.97 in all but two years of the sample. web Appendix B presents complete details.

V. Results

We begin by estimating equation (1) to examine whether the receipt of CDBG funds increases total revenues. Table 1 presents the coefficient on CDBG per capita, β_1. In the first specification in column 1, which includes just city fixed effects and year fixed effects, we do not find that CDBG funds are related to revenues. Here and throughout, we cluster standard errors at the city level. When we add city-specific trends, we find that total revenues increase by \$1.50 for each additional grant dollar. When we add demographic controls, this estimated value falls to a relatively precisely estimated \$1.25 increase in total revenue for each grant dollar. Our first

outliers in estimating equation (1), the coefficient increases slightly and remains significant—the opposite of what we would expect if these outliers drove the results.

[25] In order to insure that our results are not driven by sample selection, we analyze both the full sample and the balanced panel samples separately.

[26] City codes change wholesale from 1980 to 1990, and we assembled a crosswalk to make this linking possible. By municipalities, we mean census places, and other census local governments that are incorporated areas.

[27] These data have superior coverage relative to the frequently used ICMA data.

Table 1—Grant Receipts and Total Revenues

	(1)	(2)	(3)	(4)
CDBG per capita	0.085	1.504***	1.247***	1.210***
	(0.611)	(0.329)	(0.310)	(0.304)
p-value, CDBG per capita equal 1	0.135	0.126	0.426	0.491
R^2	0.838	0.897	0.898	0.899
Observations	21,531	21,531	21,531	21,531
Year fixed effects	x	x	x	x
City fixed effects	x	x	x	x
City-specific trends		x	x	x
Demographic controls			x	x
Local rates				x

Notes: Standard errors in parentheses. Standard errors are clustered at the city level. Demographic controls are log of population, vacancy rate, unemployment rate, share of people under age 18, share of population of foreign origin, number of housing units per capita, real median family income, share African American, and share Hispanic. All regressions contain 839 unique cities. To be consistent with the following tables, we use the smallest sample that contains all variables of interest; results are robust to using the entire available sample of 23,012 for this regression.

Sources: Internal HUD datasets on annual CDBG allocations; online HUD dataset with annual CDBG allocations, Annual Census of Governments 1975–2004; Decennial Censuses of 1960, 1970, 1980, 1990, and 2000. See web Appendix B for details.

***Significant at the 1 percent level.

set of controls include the log of population, the poverty rate, the vacancy rate, the unemployment rate, the share of people under age 18, the share of population of foreign origin, the number of housing units per capita, the real median family income, the share African American and the share Hispanic. We include the log of population in the likely event that per capita fiscal behavior is not linear in population.

The coefficient falls further when we control for the local rate correlates of the relative rates that determine the grant amount (column 4). Specifically, we add controls for the local share of housing units accounted for by pre-1940 units, the share of units with overcrowding (the share of units with more than 1.01 persons per room), and the growth lag population as a share of total population (total population is also in the formula, but is netted out with our per capita specification, which is standard for this literature). In the final specification in Table 1, identification of the grant's impact on revenues now comes exclusively from variation in the grant attributable to differences between a city's local rates and its relative rates, net of city fixed effects, city-specific trends, year fixed effects, and demographic controls. We find that total revenues increase \$1.21 for each additional dollar of grant revenue. In our estimations, this coefficient is frequently slightly greater than one, and never statistically greater than one (the p-value for this test is reported below the standard error in Table 1).

Identification of the β_1 coefficient in Table 1 relies on assumptions about municipal behavior. Appendix Table 5 shows that this result is robust to the relaxation of many of these assumptions. The results are robust to additional functions of population (second, third and fourth powers) and the inclusion of intergovernmental revenue. The results are little changed if we use the maximal possible sample, or limit the sample to only cities that have always received CDBG. We control for the ability of cities to respond to the median voter—a key assumption of the Bradford and Oates (1971) model—by including the time-varying pattern of adoption of local tax and expenditure limits and find the coefficient little changed (see Brooks and Phillips 2010 for complete documentation on sources). Finally, our estimation in equation (1) is identified from the difference between the local and relative rates. In our baseline specification, we assume that local rates affect cities only linearly. This is a strong, and not necessarily plausible, assumption. When we control for second, third, and fourth powers of the local rates that determine grant funding, the coefficient is virtually unchanged from the final column in Table 1.

While our model explains why β_1 may be substantially larger than zero, it does not motivate why we might find a coefficient greater than one. In almost all the specifications reported in this paper, the coefficient of interest is larger than, but not statistically different from, one. That is, in virtually all cases, the estimated coefficients are well within the confidence interval including one. It would be possible to generate a true coefficient greater than one if projects are lumpy. If a city is determined to spend all of its grant funds, and projects are not easily subdivided, cities could increase revenues by more than one dollar for each grant dollar received. A strand of the literature analyzing the impact of grants on public revenues has also argued that coefficients near one are generated when voters mistakenly use the average tax price rather than the marginal tax price in their public good decisions (Oates 1979; Ronald C. Fisher 1982; Geoffrey K. Turnbull 1992; Stanley L. Winer 1983).

While our results are at the high end of the range in the literature, there are many other well-specified empirical papers which also find coefficients as large as the ones here. Gordon (2004) finds that in the first year of grant receipt, schools turn all of their Title I grants into public revenues, though this effect dissipates over time. In a result most similar to our finding, Matz Dahlberg et al. (2008) use a discontinuity in the formula that awards grants to Swedish municipalities and finds coefficients slightly greater than one for the impact of grant funding on municipal revenues. A recent review article attributes conversion of grant revenues to total revenues to institutional features, consistent with what we show below (A. Abigail Payne 2009).

We now turn to examining whether the effect of grant receipt on revenues varies by council size by estimating equation (2). Column 1 of Table 2 reports summary statistics for the number of council members. In the interaction specification of Table 2, column 2 presents results from the simple interaction model—equation (2) without the covariate interaction terms ($\mathbf{X}_{c,t} CS_c$). Column 1 reports that the mean number of council members per city is 7.9. If there is a trade-off between representation and governance, then we expect the coefficient on $CDBG_{c,t} CS_c$ to be positive, or $\delta_2 > 0$. The interaction result in column 2 confirms this hypothesis: for each additional council member, an additional 10 cents of each grant dollar contribute to total revenues.

According to the model, this effect should be binding at all points in the distribution, so we chose to look at coefficients above and below three percentile margins. Pairs of rows in column 2 in the "Coefficient Comparison" section of the table report results from the simple interaction version of equation (3) (without the terms $\gamma_3 X_{c,t} \times \{1 \text{ if } CS_c \leq p\} + \gamma_4 X_{c,t} \times \{1 \text{ if } CS_c > p\}$). Results for the 25th percentile show that for cities with five or fewer council members (column 1), on average, grant receipts do not increase total revenues; in contrast, cities with six or more council members do turn grant receipts into total revenues. The first p-value reported below tests $\gamma_1 > \gamma_2$, or the contention that cities with more council members turn grant revenues

Table 2—Council Size in Interaction with Grant

Panel A			Panel B		Panel C		
					Importance of at-large members		
			Impact of council size overall		Total number of council members		
Council size			Simple interaction	Fully interacted	At-large	By-district	At-large share of city council
(1)			(2)	(3)	(4)	(5)	(6)
Interaction specification (*Equation* 2)							
Mean	7.9	CDBG pc (δ_1)	0.349	0.392	1.324*	0.693*	2.317**
			(0.415)	(0.433)	(0.514)	(0.331)	(0.829)
		CDBG × CS (δ_2)	0.097*	0.096+	−0.034	0.174	−1.669
			(0.045)	(0.050)	(0.063)	(0.109)	(0.942)
Coefficient comparison specification (*Equation* 3)							
25th percentile	5	<= 25th percentile (γ_1)	−0.038	0.269	2.240*	0.715*	2.839*
			(0.434)	(0.494)	(0.869)	(0.330)	(1.124)
		> 25th percentile (γ_2)	1.605***	1.523***	0.816**	1.869**	0.831**
			(0.358)	(0.354)	(0.289)	(0.597)	(0.279)
		p-value, H_0: $\gamma_1 > \gamma_2$	0.001	0.018	0.058	0.042	0.04
		p-value, H_0: $\gamma_1 = \gamma_2$	0.002	0.037	0.116	0.084	0.08
Median	7	<= 50th percentile (γ_1)	0.673*	0.838**	1.701**	0.715*	1.210***
			(0.321)	(0.315)	(0.584)	(0.330)	(0.304)
		> 50th percentile (γ_2)	2.083***	1.824**	0.836*	1.869**	n/a
			(0.553)	(0.579)	(0.330)	(0.597)	see notes
		p-value, H_0: $\gamma_1 > \gamma_2$	0.009	0.063	0.094	0.042	
		p-value, H_0: $\gamma_1 = \gamma_2$	0.019	0.126	0.189	0.084	
75th percentile	9	<= 75th percentile γ_1)	0.749**	0.858**	1.127**	0.878**	1.194***
			(0.289)	(0.272)	(0.422)	(0.277)	(0.309)
		> 75th percentile (γ_2)	2.871***	2.477**	1.595***	2.432*	n/a
			(0.797)	(0.901)	(0.349)	(0.981)	see notes
		p-value, H_0: $\gamma_1 > \gamma_2$	0.004	0.040	0.187	0.059	
		p-value, H_0: $\gamma_1 = \gamma_2$	0.009	0.079	0.373	0.117	

Notes: Standard errors in parentheses. The dependent variable is total revenues per capita in $1,000s. In the "Interaction Specification," each set of two coefficients comes from one regression. In the "Coefficient Comparison Specification," each pair of coefficients by distribution point is one regression; below each pair of coefficients is the p-value for the test that the coefficients are equal. All regressions use 21,531 observations, and standard errors are clustered at the city level. The results across the twenty-fifth and fiftieth percentiles of column 5 (number of seats elected by district) are the same because the median number of councilors elected by district is zero (seventy-fifth percentile is 6).

*** Significantly different from zero at the 0.1 percent level.

** Significantly different from zero at the 1 percent level.

* Significantly different from zero at the 5 percent level.

\+ Significantly different from zero at the 10 percent level.

Sources: See web Appendix C.

into municipal revenues less frequently than cities with smaller councils. Consistent with our model's prediction that $\gamma_1 < \gamma_2$, the p-value of 0.001 rejects this hypothesis. We also report the p-value for a more stringent test for inequality of the two coefficients. This test finds that the effects above and below the 25th percentile of council size are different at the 0.002 percent level. The same pattern of an increasing tendency to turn grant receipts into total revenues increases across all percentile borders.

This result is robust to the demanding fully-interacted specification, in which we interact all control variables with dummies for being above or below the *p*th percentile of council size. We report these results in column 3. In the specification from equation (2), an additional council member is again associated with an additional ten cents of total revenue from grant funds (significant at the 5.5 percent level). In the coefficient comparison specifications, the general pattern is preserved. Again cities with five or fewer council members show little evidence of using grant funds to increase total revenues. In contrast, cities with six or more council members do use grant funds to supplement total revenues. Because we now allow the effect of all covariates to vary with council size, the difference in the effect of grant receipt on total revenues by council size cannot be accounted for by, for example, the fact that poorer cities have larger councils and are might be more likely to add grant receipts to total revenues.

Figure 2 demonstrates that the effects in Table 2 are not artifacts of the distribution points chosen. This figure plots the coefficients and 95 percent confidence intervals above and below any council size that constitutes at least one percent of all observations. The light shaded box is the confidence interval for γ_2 with the letter "B" placed at the point estimate, and the dark shaded box is the confidence interval for γ_1 with the letter "S" placed at the point estimate. In all cases, the coefficient for bigger councils is larger than the coefficient for smaller councils. The 95 percent confidence intervals are completely non-overlapping for council sizes of six and seven. The chart shows that other cut-offs are close to being significant at this level.

Our results are consistent with Baqir's (2002) findings of increases in spending with the number of elected officials, but are at odds with Pettersson-Lidbom's (2008) finding that spending decreases in the number of elected officials. Pettersson-Lidbom uses panel data and a regression discontinuity design for Finland and Sweden in which cities increase council size when they hit a population threshold. The minimum council size is 17 in Finland and 31 in Sweden. Our analysis differs in three key ways from Pettersson-Lidbom (2008): we use council size in interaction with grant receipt (as opposed to council size alone), we look at substantially smaller councils, and we do not rely on a discontinuity for identification. Any one of these three possibilities could explain why our results differ, and the divergence offers interesting avenues for future research. Finally, it may be that there are critical, unobserved institutional differences between Sweden and the US that lead to these divergent outcomes; such differences are also fertile ground for future research.

It is natural to ask whether the differing effect of grant receipt on total revenues by council size is replicable with other measures correlated with council size. We first discuss other municipal institutional arrangements and then municipal demographic characteristics. Panel C of Table 2 examines the importance of council structure on the translation of grant revenue to municipal revenue by replacing the council size variable in equations (2) and (3) with:

(i) the total number of council members elected at large,

(ii) the total number of council members elected by district, and

(iii) the at large share of the city council.

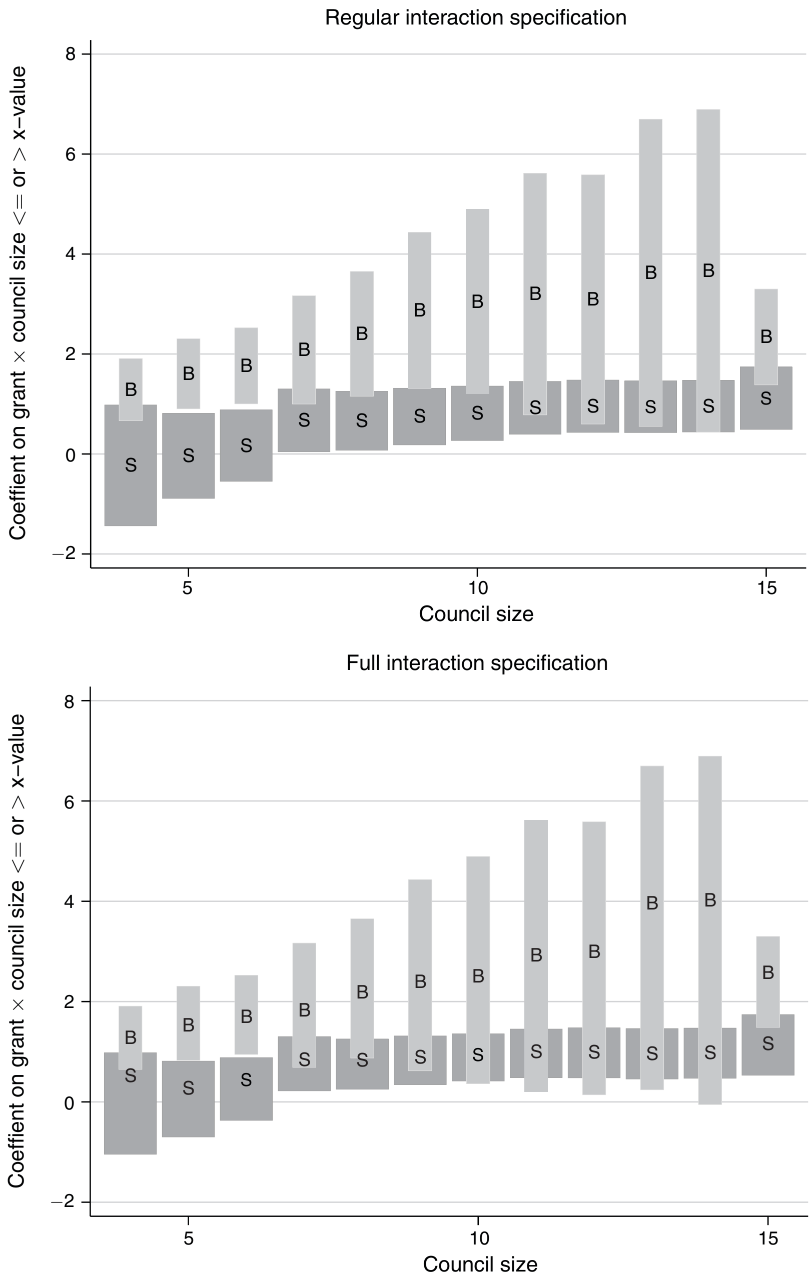

Figure 2. Impact of Grant on Municipal Revenues by Council Size Cut-offs

Notes: Each vertical line in these figures reports the coefficients and 95 percent confidence intervals from an individual regression. The dark shaded box is the confidence interval for γ_1 with the letter "S" (small) placed at the point estimate, and the light shaded box is the confidence interval for γ_2 with the letter "B" (big) placed at the point estimate. For example, the first vertical line in the first table estimates equation (3), modified so that the interaction terms are $\gamma_1\, CDBG\, pc_{c,t} \times \{1 \text{ if } CS_c \leq 4\} + \gamma_2\, CDBG\, pc_{c,t} \times \{1 \text{ if } CS_c > 4\}$.

Sources: See text and web Appendix.

In none of the specifications do any of these institutional features appear to impact the translation of grant revenue to total revenue. Of all the variables, the closest to being significant is the number of council members elected by district, which is consistent with our hypothesis. Appendix Table 6 repeats these same specifications for additional institutional variables, one of which (dummy for council-manager form of government) is significantly related to council size in a cross-sectional regression. Specifically, we examine whether the mayor is directly elected, the form of government (council-manager or mayor-council), whether the city has powers of home rule, and, for home rule cities only, the year of first home rule, the at-large share of the council and whether the city has the mayor-council form. As in panel C of Table 2, none of these institutional variables are associated with the translation of grant revenues to total revenues.

Table 3 considers one additional institutional variable and a number of demographic variables. Column 1 repeats the results of the fully interacted specification from Table 2. Columns 2 through 6 repeat the same estimation from equations (2) and (3), but replace the council size variable with institutional and demographic characteristics. We again report p-values for tests of $\gamma_1 > \gamma_2$ and $\gamma_1 = \gamma_2$. When we replace council size with council members per capita, the interaction term between council members per capita and grant receipts is not significant, nor is the effect of grant receipts on total revenues significantly different across any of the three distributional points of the council members per capita distribution. However, for two of the three distribution points, the pattern of larger impacts with increasing number of council members per capita is consistent with our hypothesis. Our model suggests that revenues increase with grant funds in both council size and per-capita council members. Why the divergence between these two measures—with similar patterns but a larger standard error for the interaction term with council members per capita? Absolute council size and council size per capita are correlated (correlation coefficient of 0.25), but distributed rather differently. The distribution of absolute council size has a longer tail, and more concentration in the center of the distribution.

One might be concerned that the difference between the result with absolute council size and council size per capita could be driven entirely by the outliers in the absolute council size distribution. In fact, when we drop cities in the top one percent and the top five percent of the council size distribution, the results for the partial interaction specification in Table 2 are entirely robust.[28] In the demanding full interaction specification, the qualitative results persist, though the significance of the coefficients is not as strong. However, Figure 2 also suggests that the effect is not driven by any particular point in the size distribution.

The remaining columns of Table 3 show that we do not see any differential effect of grant receipt on total revenues by population, poverty or median income. In all cases, the interaction terms and percentile differences across coefficients are insignificant. In further results not presented, but available upon request, we find qualitatively similar results for council size when we add other additional controls. When we control for whether the local government is constrained, by including the

[28] Results available upon request.

Table 3—Alternative Hypotheses for the Effect of Council Size on Grant Responsiveness

	Council size (1)	Council members per capita (2)	Population (3)	Poverty rate (4)	Median family income (5)
Interaction specification (*Equation 2*)					
CDBG pc (δ_1)	0.392	0.597	1.297***	0.832	3.464
	(0.433)	(0.972)	(0.332)	(0.949)	(1.896)
CDBG × interaction variable (δ_2)	0.096+	3.459	-0.001	2.201	-0.049
	(0.050)	(6.513)	(0.001)	(6.853)	(0.036)
Coefficient comparison specification (*Equation 3*)					
<= 25th percentile (γ_1)	0.269	0.728	0.878	0.282	1.794**
	(0.494)	(0.689)	(0.562)	(0.653)	(0.685)
> 25th percentile (γ_2)	1.523***	1.286***	1.428***	1.189***	0.946***
	(0.354)	(0.318)	(0.302)	(0.315)	(0.238)
p-value, H_0: $\gamma_1 > \gamma_2$	0.018	0.220	0.185	0.090	0.086
p-value, H_0: $\gamma_1 = \gamma_2$	0.037	0.440	0.371	0.180	0.172
<= 50th percentile (γ_1)	0.838**	1.051*	1.029**	0.706	1.308***
	(0.315)	(0.477)	(0.396)	(0.394)	(0.354)
> 50th percentile (γ_2)	1.824**	1.213***	1.414***	1.369***	0.762
	(0.579)	(0.348)	(0.413)	(0.378)	(0.465)
p-value, H_0: $\gamma_1 > \gamma_2$	0.063	0.384	0.238	0.095	0.145
p-value, H_0: $\gamma_1 = \gamma_2$	0.126	0.768	0.476	0.189	0.289
<= 75th percentile (γ_1)	0.858**	1.455***	1.315***	0.968***	1.232***
	(0.272)	(0.321)	(0.313)	(0.260)	(0.304)
> 75th percentile (γ_2)	2.477**	1.266*	1.556*	1.718*	0.446
	(0.901)	(0.538)	(0.739)	(0.712)	(0.670)
p-value, H_0: $\gamma_1 > \gamma_2$	0.040	0.380	0.378	0.140	0.124
p-value, H_0: $\gamma_1 = \gamma_2$	0.079	0.759	0.757	0.280	0.248

Notes: Standard errors in parentheses. The dependent variable is total revenues per capita in $1,000s. In the "Interaction Specification," each set of two coefficients comes from one regression. In the "Coefficient Comparison Specification," each pair of coefficients by distribution point is one regression; below each pair of coefficients is the p-value for the test that the coefficients are equal. All regressions use 21,531 observations, and standard errors are clustered at the city level.

*** Significantly different from zero at the 0.1 percent level.
** Significantly different from zero at the 1 percent level.
* Significantly different from zero at the 5 percent level.
+ Significantly different from zero at the 10 percent level.

Sources: See web Appendix C.

presence of a binding tax and expenditure limit, results are unchanged. Additional controls for population also do not impact the results.

The literature suggests two other interesting relationships with council size: councils elected predominantly at large and strong mayors may be able to mitigate the power of free-spending councils. Indeed, Baqir (2002) finds that in cities where more council members are elected at large, council size is not as strongly related to spending. When we examine whether revenue responsiveness to grant receipt by city council size varies with the share or presence of at-large members, our results are routinely insignificant for all relevant coefficients. We take from this that we are simply not able to identify whether at-large membership mutes the effects of council size. Similarly, when we estimate whether the presence of a strong mayor (measured by the presence of a mayor-council system) inhibits the translation of grant revenues into total revenues, we do not find any conclusive evidence that this is the case – nor that it is not the case.

VI. Conclusion

By examining when and why cities convert grant funds into total revenues, we illustrate the trade-off between representation and governance present in a democratic representative government. Our model suggests that municipal revenues are increasingly responsive to grant receipt as a function of council size. We use exogenous variation in CDBG funds to test this contention and find that cities with large councils are more likely than cities with smaller councils to treat grant funds as an addition to total revenues.

The nature of a trade-off between representation and governance is of interest not just to economists, but also to policymakers charged with creating new institutions, whether those are national governments like Iraq, supra-national governments like the European Union, or local governments, such as new municipal incorporations. More broadly, our work suggests that a policy rule mandating representation can lead to an outcome divergent from the efficient one.

The discussion in this paper focuses on when governance outcomes consistent with the desire of the pivotal voters are achieved. We do not mean to imply by this that the benefits of representation are not of critical importance. Indeed, understanding when and how representation allows political systems to mediate fundamental conflict is fertile ground for future research. While we have delineated the costs of representation, a normative recommendation must compare the costs of representation to its benefits.

Representative governance is of clear and present importance for the legitimacy and ultimate success of democratic institutions. In other words, depending on the cost of representation, it may frequently be a cost worth paying.

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