McGill University Working Paper

Inside the Gift Horse's Mouth: City Spending, Political Institutions and the Community Development Block Grant Program

Leah Brooks

Department of Economics

McGill University

Justin Phillips

Department of Political Science

Columbia University

September 2007

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 critical juncture, Shigeo Hirano, John Galbraith, Jenny Hunt, Yan Lee, Byron Lutz, Enrico Moretti, Daniel Parent, Sandra Black, 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.

Inside the Gift Horse's Mouth: City Spending, Political Institutions and the Community Development Block Grant Program

Since 1975, the Community Development Block Grant (CDBG) has transferred funds from the federal government to cities, with the goal of improving low- and moderate-income urban areas. Though this program is the U.S. federal government's single largest source of aid to cities, scholars and policymakers know relatively little about the program's effectiveness. Do cities treat these grants as an addition to total revenues? Or do they, as theory predicts, return most of the grant as a tax refund? Moreover, can political institutions cause cities to differ in the extent to which they use grant funds to supplement or supplant total revenues? We use the form of the CDBG allocation equation to identify the effect of grant changes which are exogenous to city characteristics. Using budgetary, demographic, and grant allocation data on cities from 1975 to 2004, we find that, for each additional dollar of CDBG funds, cities collect an average of an additional dollar of revenue. Of this additional dollar, almost fifty cents go toward spending categories targeted by the program. Furthermore, we show that the tendency to use grant funds as a supplement to total revenues increases in the number of elected municipal officials, consistent with political economy theory that posits a common pool problem in the provision of local public goods.

Leah Brooks
Department of Economics
McGill University
Leacock Hall, Rm. 439
855 Sherbrooke St., West
Montreal, QC h3a 2t7
CANADA
leah.brooks@mcgill.ca
http://132.206.230.4/leah_index.html

Justin Phillips
Department of Political Science
Columbia University
7th Floor, International Affairs Bldg.
420 W. 118th Street
New York, NY 10027
USA
jhp2121@columbia.edu

Since the Nixon Administration consolidated numerous categorical urban grants into the Community Development Block Grant (CDBG) in 1975, the program has been the U.S. federal government's largest single source of aid to cities, with the primary objective of transforming distressed urban areas into viable communities. The program provides cities, counties and states with unrestricted lump sum grants, with a specific emphasis on targeting the needs of low-income people and places.

Despite the fact that for thirty years the federal government has annually spent between four and ten billion 2006 dollars on CDBG, policymakers and economists have very little sense of how program funds have been and are being used.[1] Neoclassical theory suggests that CDBG funds should offset revenue collections recipient cities would have otherwise made. If CDBG transfers are monies cities would have already spent in the absence of the program, then federal policy cannot be credited for any positive effects of the program, and we would need to reinterpret the existing quantitative evaluations of the program (Galster et al., 2004; Walker et al., 2002). This general issue of how recipient governments treat transfers is of broad interest, since many countries, as diverse as Canada and Colombia, rely much more heavily than the US on intergovernmental grants to implement federal policy (Chaparro et al., 2005).

In this paper, we seek to evaluate three specific research questions, each of which tests political economy theory. First, do CDBG funds supplement or supplant locally collected revenues? Second, if CDBG funds do supplement locally collected revenues, are they spent in targeted categories? Third, can political institutions cause cities to differ in whether they use grant funds to supplement or supplant total revenues?

CDBG funds go directly to large cities, and through counties and states to smaller cities and unincorporated areas. In this paper, we focus on the city portion of CDBG, which is an

[1]For comparison, in fiscal year 2004, the federal government spent $4.9 billion on the State Children's Health Insurance Program and $3.7 billion on federal transit grants.

entitlement and which accounts for about half of current program expenditures. The city program is useful to analyze for a number of reasons. Because so many cities receive CDBG money, we have a large number of observations over which to examine the heterogeneous fiscal behavior of governments. Also, unlike most grant programs investigated in the literature, CDBG is a program in decline; most of the changes in grant awards are cuts, not increases. Though program cuts may not be as politically glamorous as increases, they are equally important to fiscal decisions. Furthermore, CDBG has been the subject of budgetary controversy. As recently as fiscal year 2006, the Bush Administration proposed eliminating the program, in large part because the Administration is not convinced that local elected officials are using grant funds properly. Our study sheds light on cities' grant use.

Our research questions bring together two literatures. The first is the wide literature testing Bradford and Oates (1971)'s contention that grant funds should substantially crowd out locally-collected revenues. Recent research on this question has returned mixed conclusions (Knight, 2002; Lutz, 2005; Evans and Owens, 2007; Singhal, 2006; Gordon, 2004), so understanding whether, when, and why local governments return grants via tax cuts remains a fertile ground for research.

Apart from understanding this average effect, our paper also tests the contention from the literature on government size and spending that cities with more elected officials spend more due to a common pool in the budget (Baqir, 2002; Weingast et al., 1981). This theory, which appeals to a norm of universalism among elected officials and the assumption that, within any given district, project benefits exceed project costs, also argues that cities' use of grant funds increases in the number of elected officials. This line of investigation offers a more fundamental explanation for the findings of Strumpf (1998), which shows that grant spending increases with overhead spending. Our results also suggest that Pettersson-Lidbom (2006), which finds no effect of the number of elected officials on the size of government, may be limited by examining only city councils with a minimum of 17 members; we find that

spending is substantially constrained only with 5 or fewer elected officials.

Any empirical examination of the CDBG program faces two major challenges. First, the researcher needs a comprehensive dataset of recipients, grants, and recipient fiscal and demographic characteristics. By putting together multiple separate publicly available datasets with internal Department of Housing and Urban Development (HUD) grant information, we are able to analyze CDBG over the life of the program, from its inception in 1975 through 2004.

Second, any examination of the impact of grants may be biased if grant receipt depends upon lobbying, since grant receipt would then be correlated with the taste for the public good provided by the grant. This concern is ruled out prima facie with the CDBG program, because grants are provided via a formula (which we verify). Furthermore, the measured impact of the grant on fiscal behavior could be distorted if elements that determine the grant also determine fiscal behavior. In the case of CDBG, funds are allocated via a formula that rewards need relative to all other entitled cities and counties; this targeting makes the formula-designated lump sum a poor instrument.

However, the formula does not measure need as an economist would. For example, relative poverty in the allocation equation is the total number of poor people divided by the total number of poor people in all metropolitan areas. The formula consists of five such "relative" measures: population, poverty, overcrowding, age of buildings, and lack of population growth. Because local fiscal behavior is a function of local rates – the number of poor people in a city divided by the population of that city – and not the relative rate (number of poor people in that city divided by the number of poor people in all metropolitan areas), we control for the local correlates of the relative factors in the formula, and let the identification come from the remaining variance and the nature of the formula.

We find that a one dollar increase in CDBG funds causes a roughly one dollar increase in total revenues. About thirty cents of every grant dollar go toward increased expenditure on

housing and community development, and another roughly twenty cents of every grant dollar go toward to expenditures on other areas related to goals of the program, such as building code enforcement or parks and recreation. However, we find that these average effects mask substantial heterogeneity in how cities use funds. In particular, cities are more likely to supplement total revenues with grant funds as the number of elected officials increases. For each additional elected official, a city adds almost ten more cents of each grant dollar to total revenues.

1 CDBG History and Rules

The primary objective of the CDBG is to transform distressed urban neighborhoods into viable communities (§101(b)1)). Funds can be spent on any one of three national objective categories: benefiting 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 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 funds to entitled cities, counties and states. Cities become entitled when they reach a population of 50,000 or more, or when they become the principal city of a metropolitan statistical area.[2] Counties become entitled when their population, excluding entitled cities, is larger

[2]At first, this 50,000 population discontinuity seems like a promising avenue for identification. However, a large share of entrant cities qualify by becoming the principal city of a metropolitan area. To use this discontinuity, cities on both sides of the discontinuity must be identifiable. Unfortunately, principal city discontinuity is not replicable with publicly available data because it requires municipal-level information on commuting flows from 1976 to 2004.

than 200,000 (Richardson et al., 2003). County funds are to be spent on unentitled or unincorporated jurisdictions within the county; state funds are to be spent on communities that qualify neither under the city or county programs. In this paper, we focus exclusively on the entitled city portion of the program, which accounts for roughly half of the total program budget.

At its peak in 1978, Congress allocated $10.4 billion (this and all dollar figures are in 2006 dollars) to the program, which is administered by the Department of Housing and Urban Development. Since then, the CDBG program has seen a steady decline in funds. In 1978, the largest recipient city, New York, received $685 million in funds, and the smallest recipient city, Colonial Heights, Virginia, received $462,000 in funds; for 2004, these numbers are now $231 million for New York and $102,000 for Punta Gorda, Florida. Figure 1 shows the total nominal amount of CDBG funds received by entitled cities over time; this amount is relatively unchanged from the inception of the program. Figure 1 also shows total real annual allocations, which have fallen by roughly half.

In per capita terms, grants to municipalities have fallen from $80 to less than $20, as demonstrated in the first column of Table 1. This decline is a function of both Congressional reductions in program funding and a near-doubling in the number of cities receiving funds, from 522 to 900 entitled recipients, as shown with the right vertical axis in Figure 1 (and Column 4, Table 1). These new entitled cities have fewer people than the original grantees, so the mean population of recipient cities falls from just over 150,000 people in 1975, to almost 130,000 people in 2004 (Table 1, column 5).

From 1975 to 2004, per capita municipal total revenues increased by about one-quarter (Table 1, Column 2) among recipient cities. Per capita expenditures on housing and community development (Table 1, Column 3), the category most closely targeted by CDBG, increased by almost fifty percent. At the peak of the program, CDBG funds account for almost 6 percent of city revenues; the 2004 figure hovers above 1 percent.

2 Theoretical Framework

How does a city respond to an exogenous change in grant revenue, all else equal? Bradford and Oates (1971) contend that, given fixed tax shares and a pivotal voter framework, a city will not increase its total revenues by the total amount of the grant. Because people like both public and private goods, the local government will return part of the grant as a tax refund for citizens to spend as they please. Similarly, when a grant is cut, municipal total revenues should be relatively unresponsive; if revenues are unchanged after grant receipt, they should be similarly little changed by grant decline.

A large literature has attempted to test the Bradford and Oates (1971) contention, and is well summarized in Gramlich (1989) and Hines and Thaler (1995). The first wave of articles attempting to test the Bradford and Oates contention found little evidence of tax cuts in response to grant increases; instead, researchers found that almost all grant funds were used as an addition to total revenues. Others found that grants are likely to be spent in the targeted category (see various articles in Mieszkowski and Oakland (1979)). Both of these effects are known in the literature as the flypaper effect, named for the idea that money sticks where it hits.

A second wave of literature on the flypaper effect using more rigorous empirical techniques has produced mixed conclusions. In the context of federal highway aid, once Knight (2002) controls for the endogeneity of these grants, he finds no evidence of any flypaper effect. Similarly, looking at grants to school districts in New Hampshire, Lutz (2005) finds that equalizing state government grants to local school districts for education are returned almost entirely as tax cuts. However, Evans and Owens (2007), analyzing the Clinton Administration Community-Oriented Policing Services program and Singhal (2006), analyzing the state tobacco windfall, both find evidence of some flypaper effect. Examining school district funding from Title 1, Gordon (2004) finds a flypaper effect that dissipates within two

years of grant receipt. Thus, the quantity of the flypaper effect under various circumstances remains an open question in the literature, and is the first question we address empirically.

To explain a sizeable change in total municipal revenues due to grant receipt, theory requires some wedge between voter desires and politician behavior. The literature offers some potential explanations, from fiscal illusion (Turnbull, 1992, 1998) to information costs that differ across jurisdictions. The lone paper to focus exclusively on heterogeneity in the flypaper effect is Strumpf (1998). Specifically, Strumpf (1998)'s theoretical predictive index for the flypaper effect depends upon the fact that jurisdictions vary in the gap between politicians' and voters' information about the budget. In jurisdictions where the information difference is large, jurisdictions spend more out of grants. Empirically, Strumpf (1998) measures the politicians' informational advantage with the amount of administrative spending by jurisdiction.[3] This is a compelling explanation for variable responses to grant receipt; however, it leaves open the question of why some cities have larger overhead costs than others. More fundamentally, the cause of the wedge between voters and politicians remains unidentified.

A separate literature on the behavior of legislatures (Weingast et al., 1981) hypothesizes that the quantity of government spending increases in the number of elected officials. The intuition for this hypothesis is that there is a common pool problem in spending, so that as the number of pool members increases, total spending does as well. In the context of this paper, the pool is the mayor and the city councilors. The Weingast et al result stems from three key assumptions. First, benefits are geographically targeted, while tax revenues come from the entire community. Thus, for each representative of a geographic district, localized spending benefits outweigh spending costs which are borne across the entire area. Second, legislatures – or city councils in the case of this paper – operate under a norm of universalism,

[3]Strumpf (2001) further argues that only credible governments are allowed to levy this tax. Lutz (2005) also studies the heterogeneity of the flypaper effect and finds that the flypaper effect increases in income.

such that each representative is guaranteed a piece of the spending pie.[4] Third, a given set of institutions is prohibitively expensive to change.

Thus, given a fixed set of institutions, where each councilor's aim is to spend so that benefits exceed costs, and a norm that allows all councilors to spend, spending increases in the number of councilors. Baqir (2002) tests this hypothesis and shows that per capita total spending increases in the number of officials across US municipalities.[5]

How does this theory apply to our analysis of the flypaper effect? First, assume grant funds are revenues that appear without cost. This is a reasonable assumption, since CDBG is funded via national taxation. Thus, given a new source of funds where geographic political benefits exceed the costs of taxation, and given a deliberative body that follows a norm of universalism, the propensity to spend out of grant funds increases in the number of elected officials. The wedge between the neoclassical voter preferences in the Bradford and Oates model and the actualized behavior is now the set of local institutions. Local institutions which are not optimal persist because the cost of modifying those institutions is very high (see Baqir (2002) for details on the infrequency of change in these local institutions). Thus, the Weingast et al. (1981) model provides a testable explanation for the divergence between voter preferences and political outcomes, based on institutional structure, rather than on realized institutional behavior.

In sum, political economy theory predicts larger changes in total revenues in response to grant receipt in cities with more elected officials. Theory is silent on whether the number of elected officials should affect expenditure in the grant's targeted categories. We evaluate these questions empirically.

[4]Baron (1991) extends this result, showing that total spending increases in the number of districts, without reliance on the norm of universalism.

[5]In contrast, Pettersson-Lidbom (2006) finds no effect of changes in council size for cities in Sweden and Finland.

3 Methodology

Recent 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 (Milligan and Smart, 2005; Lutz, 2005; Knight, 2002; Gordon, 2004). Our goal in this paper is to quantify the impact of grant funds on city behavior by estimating equations of the form

$$\text{expenditure pc}_{c,t} =$$

$$\beta_0 + \beta_1 \text{CDBG pc}_{c,t} + \beta_2 X_{c,t} + \beta_3 \text{city}_c + \beta_4 \text{city trend}_{c,t} + \beta_5 \text{year}_t + \epsilon_{c,t} \ , \tag{1}$$

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 city_c, city-specific trends are $\text{city trend}_{c,t}$ and year fixed effects are year_t. All coefficients with the exception of β_0, β_1 and β_2 are vectors. When estimating this equation, we first examine CDBG's impact on total per capita revenues, then on the targeted expenditure category, and finally on more detailed expenditure types.

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 ($\text{city trend}_{c,t}$). 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.

Two general concerns may arise in such an estimation. 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 expenditure, β_1 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 expenditures. 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. This targeting in the formula means that the formula-designated grant should not be used as an instrument, as it was designed with particular expenditures in mind.[6]

In contrast, our identification relies on the fact that local expenditures are determined by local, not relative, characteristics. That is, local per capita expenditures may well be a function of the municipal poverty rate, but, net of the local poverty rate, they are not a function of a city's poverty relative to all other cities. Colloquially, this argument says that spending 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 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).

What does it mean that the grant is a function of relative rates? Grants are awarded by a dual-formula system[7] as a function of a city's relative amounts of population, poverty, older structures, overcrowding, and growth. This dual formula applies to both entitled cities and entitled counties, and funds are awarded as a function of cities' and counties' relative

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

[7]The dual formula dates from 1978; before then all funds were awarded via formula A.

joint need.[8]

Formula A, in Equation 2, 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}).[9] Formula B in Equation 3 allocates a city's share of the total grant funds 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}), a city's number of buildings built before 1940 (age_c) relative to the total number of buildings built before 1940 (age_{MA}), and relative "growth lag".

$$\text{grant share}_{A,c} = \left((1/2)\frac{\text{pov}_c}{\text{pov}_{MA}} + (1/4)\frac{\text{pop}_c}{\text{pop}_{MA}} + (1/4)\frac{\text{ov crwd}_c}{\text{ov crwd}_{MA}} \right) \tag{2}$$

$$\text{grant share}_{B,c} = \left((2/10)\frac{\text{growth lag}_c}{\text{growth lag}_{EC}} + (3/10)\frac{\text{pov}_c}{\text{pov}_{MA}} + (1/2)\frac{\text{age}_c}{\text{age}_{MA}} \right) \tag{3}$$

To construct growth lag, HUD calculates the total growth of all entitled cities since 1960. If a city has grown more than this average rate, then growth lag_c is equal to zero. If a city has grown less than the average, then growth lag_c is equal to the extra population that city would have had, had it grown like the average since 1960.[10] The denominator for growth lag is the total of all growth lags for all entitled cities.

Each city's grant share is the maximum of its share from Formula A and Formula B, with

[8]Counties and cities share the same funding pot; for purposes of clarity, we omit counties from the discussion here. A detailed description of their role in the granting process is found in Appendix A.

[9]HUD's "metropolitan areas" in the formula are the variously-named Office of Management and Budget-defined metropolitan agglomerations. Entitled cities are a subset of all cities in metropolitan areas.

[10]For more information on these variables, see the appendix to this paper that describes how we match the formulas, and HUD's excellent reports that detail the formula (Neary and Richardson, 1995; Richardson et al., 2003; Richardson, 2005)

a correction so that the shares do not add up to more than one. The grant amount awarded, shown in Equation 4, is the city's grant share times the total allocation made available to entitled cities and counties by Congress. Since 1982, legislation guarantees entitled cities and counties 70 percent of the total CDBG allocation; before 1982, this share was 80 percent.

$$\text{grant}_c = \left(\frac{\max\left(\text{grant share}_{A,c}, \text{grant share}_{B,c}\right)}{\sum_{j=1}^{C} \max\left(\text{grant share}_{A,j}, \text{grant share}_{B,j}\right)} \right) * \text{allocation} \tag{4}$$

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 as a function of the log of 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 while all the local rates are significantly related to the per capita grant, with a R-squared of 0.39, there is still variance left to be explained. Adding city fixed effects increases the R-squared to 0.55.[11]

Our estimation is also aided by the fact that, while each city's grant is function of things possibly known to a city, such as the local poverty rate or local changes in population, each city's grant is also a function of things clearly difficult for any one city to observe. For example, an individual city's grant amount depends on the total poverty in all other cities and counties and the total number of cities and counties eligible for the program[12], net of information about the local poverty rate. Municipal officials in any given city are not likely

[11]Similarly, while local rates are correlated with relative rates, the correlation leaves variance left for estimation. Normalizing the relative rate by population (e.g., $((\text{pov}_c/\text{pov}_{MA})/\text{pop}_c)$, 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.

[12]This number depends heavily on the number of cities that enter the program because they become a principal city of a metropolitan area. It is actually not possible to calculate this number given publicly available information except for in 2003 and 2004.

to be well informed about these fine nuances.[13]

In addition to examining CDBG's impact on revenues and specific spending categories, we also investigate whether the flypaper effect varies by city characteristics – specifically, the number of elected officials – as suggested by theory. To do this, we estimate two specifications. The first allows a city characteristic $V_{c,t}$ to affect the use of CDBG funds linearly, as shown in Equation 5 below.[14] We also allow the control variables $X_{c,t}$ to have a differential impact with $V_{c,t}$ via an additional interaction term.

$$\text{expenditure pc}_{c,t} =$$

$$\beta_0 + \beta_1 \text{CDBG pc}_{c,t} + \beta_2 \text{CDBG pc}_{c,t} * V_{c,t} + \beta_3 V_{c,t} + \tag{5}$$

$$\beta_4 X_{c,t} + \beta_5 X_{c,t} * V_{c,t} + \beta_6 \text{city}_c + \beta_7 \text{city trend}_{c,t} + \beta_8 \text{year}_t + \epsilon_{c,t} \ .$$

We interpret β_2 as the additional propensity to spend out of grant funds associated with an additional unit of characteristic $V_{c,t}$.

Second, because the characteristic $V_{c,t}$ may not have a linear effect on the dependent variable, we allow the grant to have a differential impact on expenditures above and below the 25th percentile, the median and the 75th percentile of a given characteristic. Specifically, we estimate effects above and below a percentile of interest p, by estimating

$$\text{expenditure pc}_{c,t} =$$

[13]The structure of the data suggest two other estimation approaches. The first is a regression discontinuity approach, as in Gordon (2004), in which plausibly exogenous changes in grants are caused by the introduction of new information to the grant formula when updated census information is introduced into the grant calculation. Unfortunately, this is not a productive route to examine changes in CDBG funds, as the size of changes induced by census updates averages only 2 percent of the grant. Changes in years affected by census updates are, on average, smaller than changes in non-affected years. The second approach would be to analyze program entrants separate. This also turns out not to be a promising margin along which to find variation. As Table 1 shows, most entrants arrive in the later years of the program, when both average funding and variation in funding are low.

[14]Some of our characteristics do not vary over time; in such cases, the variable associated with β_3 is dropped as the variable is measured by the city fixed effect.

$$\beta_0 + \beta_1 \text{CDBG pc}_{c,t} * \{1 \text{ if } V_{c,t} \leq p\} + \beta_2 \text{CDBG pc}_{c,t} * \{1 \text{ if } V_{c,t} > p\} + \tag{6}$$

$$\beta_3 X_{c,t}\{1 \text{ if } V_{c,t} \leq p\} + \beta_4 X_{c,t} + \beta_5 \text{city}_c + \beta_6 \text{city trend}_{c,t} + \beta_7 \text{year}_t + \epsilon_{c,t} \ .$$

Again, we 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 $V_{c,t}$. We then test whether $\beta_1 = \beta_2$; if the coefficients are not equal, we interpret this as evidence that cities above the pth percentile of characteristic $V_{c,t}$ respond differently than cities below the pth percentile to changes in grant funds.

4 Data

To comprehensively understand the CDBG program, 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 available in Appendix B. From an internal HUD source, we have annual grant allocations from 1975 to 2001[15]; further annual allocation information from 1993 to 2004 comes from the HUD website.[16] 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.[17]

To these data we add demographic information – the same demographic information that HUD uses – municipalities from the 1960, 1970, 1980, 1990, and 2000 Censuses.[18]

[15] Courtesy of HUD Office of Planning, Development and Research Senior Economist Todd Richardson.

[16] See http://www.hud.gov/offices/cpd/about/budget/history/historical04to93.xls

[17] 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.

[18] 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

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.[19] We also 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.

In order to replicate the CDBG allocation calculations ourselves we need more sources of data. We add annual estimates for population, which the Census Bureau provides for cities since 1990, and which HUD uses in allocating funds. We include Census definitions for metropolitan areas (variously named Standard Metropolitan Areas, and Metropolitan Statistical Areas in different eras) from 1973, 1981, 1990, and 1999, and demographic information for all metropolitan areas and counties from the decennial censuses of 1970, 1980, 1990 and 2000, which we need to construct part of the denominator of the allocation formula.

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. Panels A and B of Figure 2 show the quality of the match for two years on a log scale so that all cities can be viewed. For both graphs, the line is the metaphorical 45-degree line, where all cities would lie if our constructed grant exactly matched the actual grant. The top panel of the figure shows our match in 1976, the year in which our constructed allocation is least correlated with the actual allocation. This is not entirely unexpected as this year – and the first 6 years of the program – included grandfathering from previous programs consolidated into the CDBG. Even so, in 1976 the correlation between the true and constructed grant is 0.88.[20] In 1995, shown in the bottom

areas.

[19]We use the same sources to gather a parallel dataset on counties and metropolitan statistical areas. Though these geographies are not the focus of this research, we need information on them to be able to compute CDBG allocations to cities.

[20]The points along the x-axis are cities to which HUD allocated funds, but for which we do not observe

panel, the correlation is even stronger, at 0.98.[21]

5 Results

5.1 Average Effects of CDBG on Revenues and Expenditures

We begin by estimating Equation 1 to investigate whether CDBG grants supplement municipal revenues, and present results in Table 2. The top panel shows CDBG's impact on total revenues, and the bottom panel shows CDBG's impact on housing and community development expenditures. Though CDBG funds could reasonably be spent in many categories (e.g., for parks or code enforcement), housing and community development is the only expenditure category in our data directly targeted by the program. The first column presents results controlling for year fixed effects, city fixed effects, and city-specific trends. The identification thus comes from within-city changes, net of city-specific trends in the outcome of interest and overall annual fluctuations.

In this specification, an additional dollar of CDBG funds is associated with an increase of $1.50 in total revenues and an addition of $0.30 spending on housing and community development. The third row of Panel A tests whether the estimated CDBG coefficient is equal to 1, as neoclassical theory never predicts a coefficient greater than 1. In this and all specifications we cannot reject that the coefficient is equal to 1. Throughout, we cluster standard errors at the city level.

Adding demographic and economic controls, as we do in columns 2 and 3, lowers the coefficients somewhat. Our first set of controls include the log of population, the poverty rate, the vacancy rate, the unemployment rate, the share of people under age 18, and the share of population of foreign origin; the second set contains the number of housing units per

information to construct an allocation.

[21] Further details on the quality of the match are in Appendix A.

capita, the real median family income, the share African American and the share Hispanic. This specification already controls for a number of variables that either directly affect the grant amount or are highly correlated with the grant amount. Because we express spending in per capita terms, this specification controls for the impact of relative population on grant receipt. We include the log of population in the likely event that per capita fiscal behavior is not linear in population. The poverty rate we include is the local rate correlate of the relative poverty that determines CDBG funding, and we include it in this basic specification because we believe that it is very likely to influence local spending decisions. With the addition of all these controls, we find that an additional dollar of CDBG revenue leads to a spending increase of \$1.24, which is insignificantly different from 1, and a \$0.27 increase in spending on housing and community development.

The fourth column of Table 2 adds controls for all the local rate correlates of the relative rates that determine funding. We add controls for the 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. This specification, which adds variables correlated with the variable of interest, causes the coefficient on CDBG grants per capita to increase slightly. Now a one dollar increase in CDBG is associated with a \$1.21 increase in total revenues and a \$0.27 increase in spending on housing and community development. This is our preferred specification.[22]

The final column of Table 2 presents a robustness check to show that the results are not driven by other intergovernmental revenue that co-moves with CDBG funding. In particular, US federal revenue sharing to local governments, another Nixon-era program that met its demise in the Reagan era, was initially roughly the size of CDBG. However, non-CDBG

[22]Results from the balanced panel sample are the same direction as the results for the analysis sample presented in the paper. When the outcome is housing and community development expenditures, the estimated $\hat{\beta}_1$ is nearly the same in both samples; when the outcome is total revenues, $\hat{\beta}_1$ is slightly larger, consistent with what we will show in the section on heterogeneous effects.

intergovernmental revenue is problematic as a control, as it contains matching grants and other politically sensitive grants that are endogenously related to spending, and may be correlated with CDBG. After adding this control, the grant coefficients increase modestly; the qualitative conclusions remain unchanged.

To re-phrase our overall results in terms of how the CDBG program has generally been functioning, a $1 cut in grant funds leads to a cut of equal magnitude in total revenues, and takes away about a quarter on the dollar from spending on housing and community development.

Our estimates are much higher than the near-zero effect of grants found in Knight (2002) and Lutz (2005), and more along the lines of the partial flypaper effect in Evans and Owens (2007). Why are our results so much larger than those recently found in the literature? We offer two potential explanations: constraints and differing demands.

As we explore in more detail in related work, if cities are constrained in their fiscal behavior by some mechanism prior to receipt of the grant, the local government should be more likely to treat this grant as an addition to total revenues (Brooks and Phillips, 2007). By constraints, we mean not those constraints that all politicians face from voters, but externally-imposed constraints on fiscal behavior. When a local government is constrained, and the constraint binds, the government cannot satisfy the desires of the pivotal voter. When a grant arrives, the city could potentially use the entire grant as a supplement to revenues and still leave the pivotal voter dissatisfied with the low level of spending. Indeed, most cities operate under authorizing legislation from states which restricts the type and amount of taxation and borrowing that cities may undertake.

Second, large cities generally tend to be more politically liberal than the country as a whole. If large cities have a greater taste for the redistribution that CDBG funds offer, their marginal propensity to spend on public goods out of additional income may be larger than that average propensity at the state level, as measured by Knight (2002). While these

differences in taste may explain some of the variation, our estimates are too large to be explained entirely by them.

If only thirty cents of every CDBG dollar is spent on housing and community development projects, as we show in Table 2, where do the remaining 70 cents go? Table 3 shows the impact of CDBG changes on a variety of expenditure categories by estimating Equation 1 using different expenditure categories as the dependent variables. Column 1 reports the coefficient on CDBG per capita from the specification without local rates in column 3 of Table 2, and column 2 reports our preferred specification from column 4 of Table 2. The first and third row repeat the coefficients from Table 2, columns 3 and 4. The second row of Table 3 shows that CDBG has a larger effect on expenditures than revenues.

When we combine all the expenditure categories that could potentially receive CDBG funds, save housing and community development – parks and recreation, code enforcement, sewers, and roads and streets – we find that a one dollar increase in CDBG increases expenditures on these targeted categories by almost 20 cents. However, when considered alone, no individual category shows any significant response to changes in CDBG allocations. We suspect we do not observe responses in individual categories because cities have such broad latitude in using funds, so that the noise in any one category is thus substantial.

We also find that a one dollar increase in CDBG funds is associated with a significant roughly 40 cent increase in education spending, and a small but significant 3 cent increase in health spending. Though CDBG funds are not to be used for education, money is fungible, and it is economically feasible that CDBG increases or decreases could cause changes in education spending. Changes in CDBG funds are unrelated to changes in spending on public buildings or welfare.

5.2 Does CDBG's Effect on Revenue and Expenditure Vary by Type of Political Institutions?

The previous section showed that municipal revenues change roughly one to one with changes in CDBG funds, and that roughly thirty cents of every CDBG dollar goes toward housing and community development spending. Do these basic results mask heterogeneity, driven by political institutions, in the use of funds?

Column 1 of Table 4 reports summary statistics for the number of elected officials. Panel A then reports the impact of CDBG funds in interaction with the number of elected officials on total revenues; Panel B examines the interaction's impact on the targeted category of housing and community development expenditures. The first row in each reports the impact of a linear change in municipal characteristics on block grant use, as specified in Equation 5. The next set of three rows explore whether the effect of this characteristic is nonlinear by estimating Equation 6. In other words, in the 50th percentile row, columns 2 and 3 report CDBG's impact in cities above the median (β_1) and cities below the median (β_2) respectively. Column 4 reports the p value for the test that these coefficients are equal. Panel B reports these same coefficients when the dependent variable housing and community development expenditures.

Table 4 shows evidence that cities with more elected officials are more likely to change total revenues with changes in block grant receipts. The mean number of total elected officials by city is 7.9, and the median is 7 (column 1). The interacted coefficient in column 3 is significant at the 5.4 percent level and suggests that adding one additional elected official leads to a 10 cent increase in the amount of grant funds supplementing total revenue. The above- and below-the-median coefficients (Panel A, columns 2 vs 3) differ at the 12 percent level and also suggest that cities with more elected officials pass through more of the grant to total revenues. Comparing the coefficients on elected officials in the following row, we see

that the effect is driven mostly by places with 5 or fewer elected officials behaving differently than places with 6 or more elected officials. These coefficients differ at the 4 percent level, while the coefficients comparing cities with 9 or fewer elected officials to cities with 10 or more elected officials differ at the 11 percent level.

In all cases, cities with more elected officials are more likely to change total revenues in response to changes in the grant. This effect is not driven by the share of at-large elected officials (not presented), as the effects for the share of at-large officials do not differ significantly from across any percentile boundary. Though we use a different testing framework, our findings – both with the number of elected officials and with the number of at-large elected officials – are very similar to what Baqir (2002) finds when examining the impact of the number of elected officials on city spending. Unlike Baqir (2002), however, we do not find any mitigating effects on this spending tendency in mayor-council cities where the executive frequently has a veto on proposed expenditures.

Is this effect of increased spending in the number of elected officials driven by some other municipal characteristic correlated with the number of elected officials? We present three pieces of evidence that it is not. First, city council sizes historically are a function of the region of the country, and city council size changes very infrequently (see Baqir (2002) for a more detailed analysis). Thus major determinants of council size are controlled for in our estimation with the municipal fixed effect. Second, our specification allows all of our covariates to have a differential effect by the number of elected officials. Thus, our results mechanically cannot be driven by the fact that cities with more elected officials have more unemployment or higher log population (see Table 2 notes for our complete list of covariates).[23] Third, when we do the analysis presented in Table 4 for both population and council members per capita, we see no significant differences in block grant use like those we

[23]In addition, adding controls for linear population, population squared, and population cubed only increases the standard errors in the interaction specification marginally (available upon request).

observe for the number of elected officials.

To bring this point home, the bottom panel of the Appendix C Table 1 reports results when the heterogeneity variable of interest is number of elected officials per 1000 people, rather than the total number of elected officials. Comparing those results to the ones in Table 4, the number of elected officials per capita is significantly worse in explaining differences: the interacted specification is far from significant, and the interacted coefficient does not capture as much of the main effect as in the specification using total number of elected officials. In addition, differences across quartile boundaries in the distribution of total number of elected officials per 1000 people are less informative about block grant use than the differences in the upper panel using total number of elected officials.

Our results reconcile Baqir (2002)'s findings of increases in spending as the number of elected officials increases with Pettersson-Lidbom (2006)'s finding of no changes in expenditures with changes 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 results in Table 4 show that only cities with five or fewer elected officials exhibit no flypaper effect. Thus, it may be that for a council to put substantial controls on spending, it must be quite small – so small as not to be observable in Pettersson-Lidbom's data.

In contrast to the overall effects of grant use, the impact of the number of elected officials on targeted block grant spending, as shown in Panel B, is quite different. Interestingly, we find that additional municipal officials have no effect on whether or not the municipal government targets the grant funds. This lack of effect echoes regardless of specification, except for weak evidence among cities with 9 or more elected officials to spend none of the grant funds on housing and community development. This suggests that the effect in the previous table on total revenues is driven specifically by the common pool problem as in

Baqir (2002), and not by some other difficulty of governance with large numbers of elected officials.

6 Conclusions

This paper has implications for both our understanding of governmental fiscal behavior and the efficacy of the Community Development Block Grant program. First, it sheds new light on the continuing debate over the existence and magnitude of the flypaper effect. Recent scholarship in public finance suggests that existing studies, by mistakenly examining grant programs for which receipts may be endogenously determined, have falsely concluded that intergovernmental revenue stimulates local expenditures. By focusing on a grant program for which we can control for the mechanism of receipt, our analysis is a good test for the flypaper effect. We find robust evidence that block grants supplement local expenditures and revenues. Stated differently, the results presented here provide some of the strongest evidence to date that the flypaper effect is indeed real, at least under certain circumstances.

The results further indicate that the magnitude of the flypaper effect is not constant. We show that there is substantial heterogeneity in the use of grant money by recipient governments and that the heterogeneity is a function of the number of elected officials. This result is additional evidence that the institutional context for making fiscal decisions matters greatly. It also suggests that the political institutions of recipient governments may play a strong causal role in the existence of the flypaper effect.

Finally, this paper presents a mixed review of the CDBG program. On the one hand, our results indicate that CDBG is stimulating local expenditures. We find that cities total revenue collections change roughly dollar-for-dollar with CDBG funds, and that almost fifty cents of each dollar is spent on activities targeted by the program. These targeted activities include housing and community development, parks and recreation, code enforcement, sew-

ers, roads, and streets. In other areas, however, the program appears to be falling short of its objectives. Our results show that CDBG funds are not supplementing municipal social welfare expenditures, despite the fact that this an expenditure category targeted by Congress. Since HUD records and reports indicate that social welfare expenditures are being made with program funds, we can only conclude that CDBG money is supplanting local expenditures that would have been made in this area. Overall, however, we find that affecting redistributional aims via intergovernmental grants is not the entirely hopeless proposition that neoclassical theory asserts.

References

Baqir, Reza, 2002. "Districting and Government Overspending." *Journal of Political Economy* 110(6): 1318–54.

Baron, David, 1991. "Majoritarian Incentives, Pork Barrel Programs, and Procedural Control." *American Journal of Politicial Science* 35(1): 57–90.

Bradford, David F. and Oates, Wallace E., 1971. "The Analysis of Revenue Sharing in a New Approach to Collective Fiscal Decisions." *Quarterly Journal of Economics* 85(3): 416–439.

Brooks, Leah and Phillips, Justin, 2007. "An Institutional Explanation for the Stickiness of Federal Grants." McGill University Working Paper.

Chaparro, Juan Camile, Smart, Michael, and Zapata, Juan Gonzalo, 2005. "Intergovernmental Transfers and municipal finance in Colombia." In Richard Bird, James Poterba, and Joel Slemrod, (Eds.) "Fiscal Reform in Colombia: Problems and Prospects," MIT Press.

Evans, William N. and Owens, Emily, 2007. "COPS and Crime." *Journal of Public Economics* 91(2): 181–201.

Galster, George, Walker, Christopher, Hayes, Christopher, Boxall, Patrick, and Johnson, Jennifer, 2004. "Measuring the Impact of Community Development Block Grant Spending on Urban Neighborhoods." *Housing Policy Debate* 15(4): 903–34.

Gordon, Nora, 2004. "Do Federal Funds Boost School Spending? Evidence From Title I." *Journal of Public Economics* 88(9-10): 1771–92.

Gramlich, Edward M., 1989. "Intergovernmental Grants: A Review of the Empirical Literature." In Wallace Oates, (Ed.) "The Political Economy of Fiscal Federalism," Lexington, MA: Heath-Lexington.

Hines, Jr., James R. and Thaler, Richard H., 1995. "Anomalies: The Flypaper Effect." *Journal of Economic Perspectives* 9(4): 217–226.

Knight, Brian, 2002. "Endogenous Federal Grants and Crowd-Out of State Government Spending: Theory and Evidence from the Federal Highway Aid Program." *American Economic Review* 92(1): 71–92.

Lutz, Byron F., 2005. "Taxation with Representation: Intergovernmental Grants in a Plebiscite Democracy." Working paper series?

Mieszkowski, Peter and Oakland, W., 1979. *Fiscal Federalism and Grants-in-Aid*. Washington, D.C.: Urban Institute.

Milligan, Kevin and Smart, Michael, 2005. "Regional Grants as Pork Barrel Politics." CESifo Working Paper 1453.

Neary, Kevin and Richardson, Todd, 1995. *Effect of the 1990 Census on CDBG Program Funding*. Washington, D.C.: U.S. Department of Housing and Urban Development.

Pettersson-Lidbom, Per, 2006. "Does the Size of the Legislature Affect the Size of Government? Evidence from Two Natural Experiments." Stockholm University Working Paper.

Rich, Michael J., 1993. *Federal Policymaking and the Poor: National Goals, Local Choices, and Distributional Outcomes*. Princeton, NJ: Princeton University Press.

Richardson, Todd, 2005. *CDBG Formula Targeting to Community Development Need*. Washington, D.C.: U.S. Department of Housing and Urban Development.

Richardson, Todd, Meehan, Robert, and Kelly, Michael, 2003. *Redistribution Effect of Introducing Census 2000 Data Into the CDBG Formula*. Washington, D.C.: U.S. Department of Housing and Urban Development.

Singhal, Monica, 2006. "Special Interest Groups and the Allocation of Public Funds." NBER Working Paper 12037.

Strumpf, Koleman S., 1998. "A Predictive Index for the Flypaper Effect." *Journal of Public Economics* 69: 389–412.

Strumpf, Koleman S., 2001. "Government Credibility and Policy Choice: Evidence from the Pennsylvania Earned Income Tax." *Journal of Public Economics* 80: 141–167.

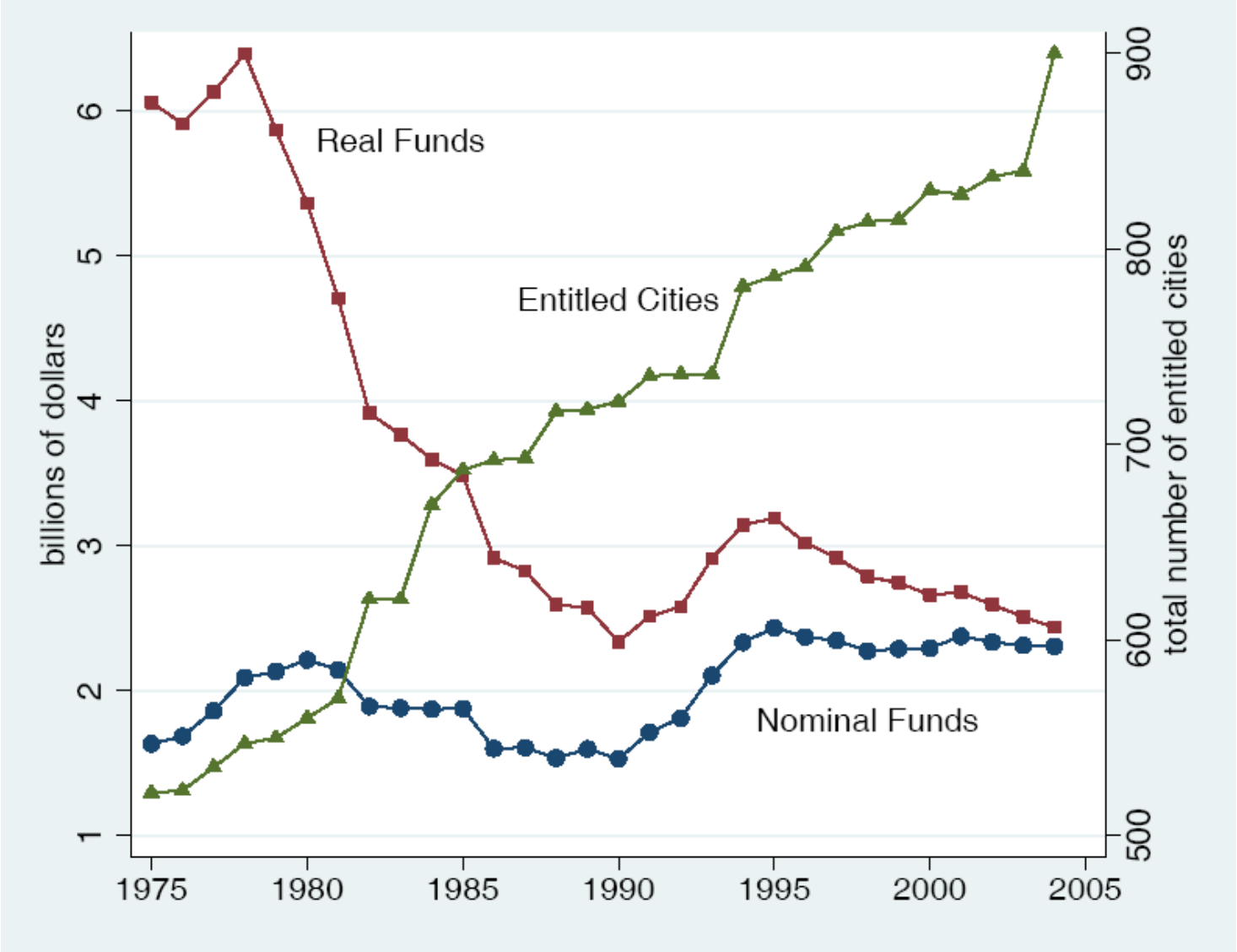
Turnbull, Geoffrey K., 1992. "Fiscal Illusion, Uncertainty and the Flypaper Effect." *Journal of Public Economics* 48: 207–223.

Turnbull, Geoffrey K., 1998. "The Overspending and Flypaper Effects of Fiscal Illusion: Theory and Empirical Evidence." *Journal of Urban Economics* 44(1): 1–26.

Walker, Chris Hayes, Chris, Galster, George, Boxall, Patrick, and Johnson, Jennifer, 2002. *The Impact of CDBG Spending on Urban Neighborhoods*. Washington, D.C.: U.S. Department of Housing and Urban Development.

Weingast, Barry R., Shepsle, Kenneth A., and Johnsen, Christopher, 1981. "The Political Economy of Benefits and Costs: A Neoclassical Approach to Distributive Politics." *Journal of Political Economy* 89(August): 642–64.

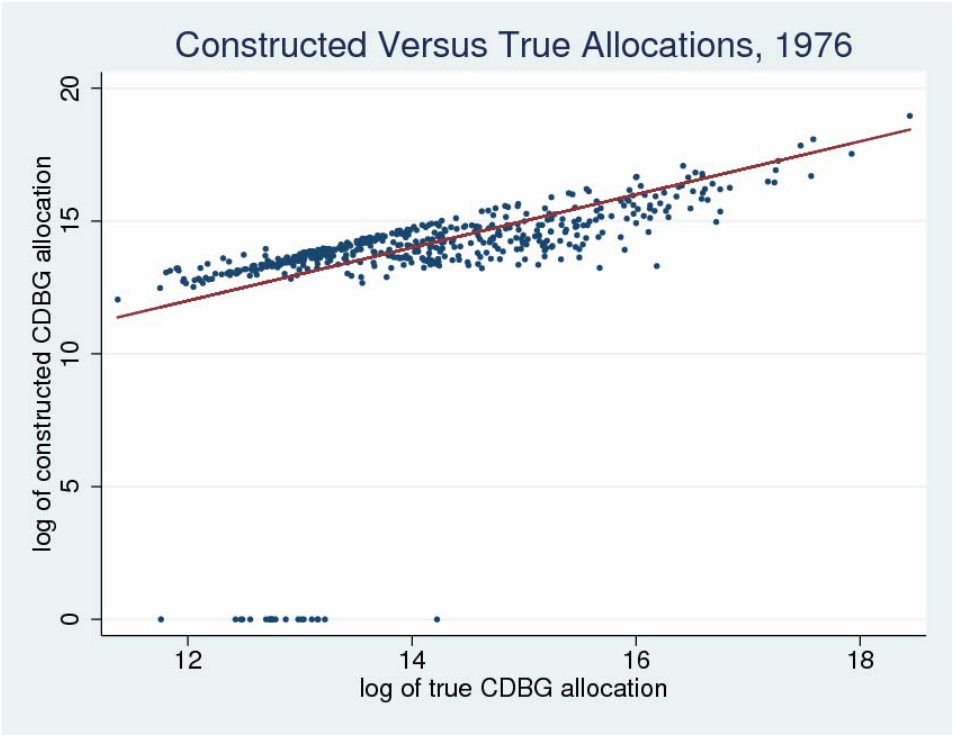
Figure 1: CDBG Allocation and Entitlement Status Over Time



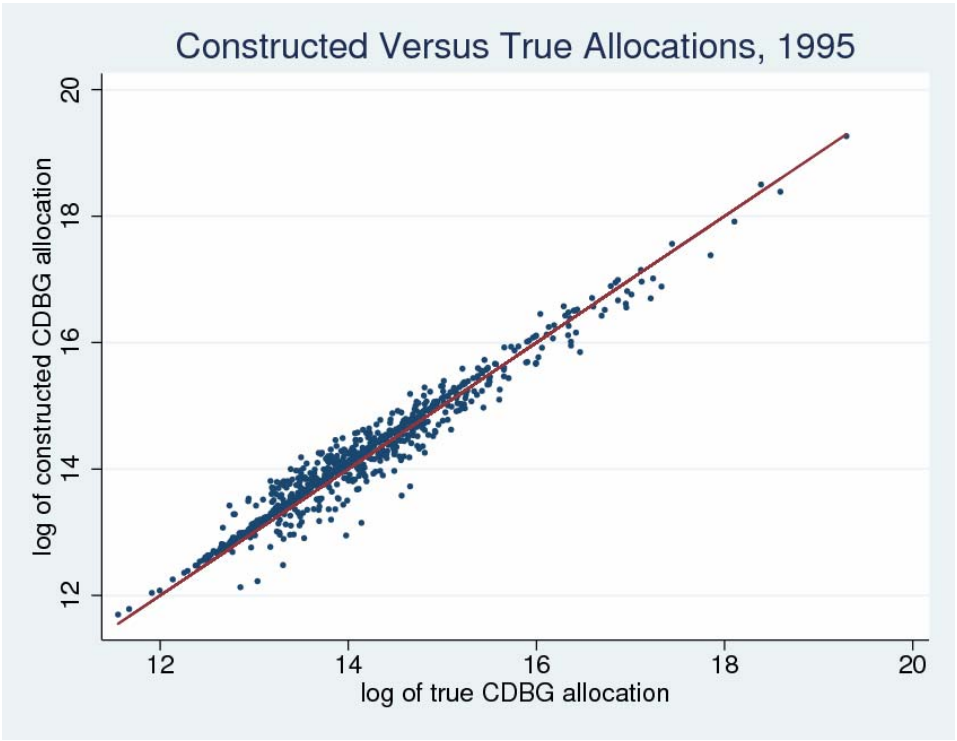
Notes: Funds are totals for the entitled city portion of the CDBG program.
Sources: Annual CDBG allocations from HUD website and internal sources; see Appendix B for more details.

Figure 2: Verifying that CDBG Allocations Follow HUD's Formula

A



B



Sources: Actual HUD allocations from HUD website and internal source. Constructed CDBG allocations based on decennial census data (1970, 1980, 1990, 2000) at the municipality, county and MSA level and population estimates at the city and county level from the Census Bureau's Population Estimates website.

Table 1: Characteristics of CDBG Recipient Cities

	(1)	(2)	(3)	(4)	(5)
	dollars per capita				
year	CDBG	Total Revenue	Hous & Cmty Dev	CDBG cities	Mean Population (1000s)
1975	79.91	1,433.54	46.25	522	151.4
1976	80.51	1,537.98	42.74	523	151.3
1977	81.51	1,614.77	50.49	535	149.4
1978	76.97	1,639.66	68.74	547	148.0
1979	67.45	1,574.81	52.11	550	147.6
1980	57.84	1,435.62	60.01	560	142.9
1981	49.91	1,429.66	59.93	570	142.5
1982	40.66	1,450.17	56.91	621	134.8
1983	38.73	1,473.00	52.71	621	135.7
1984	35.75	1,529.13	50.80	669	129.3
1985	34.37	1,594.30	53.80	687	127.9
1986	28.63	1,675.94	58.17	692	128.3
1987	27.65	1,704.85	51.88	693	129.0
1988	24.92	1,676.15	58.68	717	127.7
1989	24.67	1,710.39	61.03	718	128.4
1990	22.36	1,705.94	62.43	722	128.5
1991	23.65	1,684.22	62.55	735	128.5
1992	24.08	1,727.89	65.16	736	129.7
1993	27.23	1,736.78	66.34	736	130.9
1994	28.35	1,766.61	67.55	781	127.2
1995	28.39	1,764.11	65.64	786	128.0
1996	26.69	1,822.40	68.60	791	128.7
1997	25.32	1,786.69	64.43	809	128.5
1998	24.00	1,911.58	69.04	814	129.2
1999	23.54	1,941.49	71.05	815	130.4
2000	22.46	1,953.79	68.87	830	130.1
2001	22.63	2,088.93	74.25	828	131.7
2002	21.88	1,882.69	71.42	837	132.6
2003	20.96	2,063.15	86.80	840	133.2
2004	19.86	2,136.20	77.16	900	128.4

Notes: Results are means for all CDBG recipient cities in a given year.

Sources: See Appendix B.

Table 2: CDBG Adds to Total Revenue and to Economic Development Spending

	(1)	(2)	(3)	(4)	(5)
A. Dependent Variable: Total Revenues					
CDBG per capita	1.504***	1.325***	1.243***	1.210***	1.370***
	(0.329)	(0.322)	(0.310)	(0.304)	(0.252)
p-value, CDBG per capita =1	0.126	0.313	0.434	0.491	0.142
R-squared	0.897	0.897	0.899	0.899	0.918
Obs	21,531	21,531	21,531	21,531	21,531
Unique Cities	839	839	839	839	839
Year fixed effects	x	x	x	x	x
City fixed effects, city-specific trends	x	x	x	x	x
Demographic controls 1		x	x	x	x
Demographic controls 2			x	x	x
Local rates				x	x
Other intergovernmental revenue					x
B. Dependent Variable: Housing and Community Development					
CDBG per capita	0.295***	0.262***	0.271***	0.276***	0.295***
	(0.065)	(0.064)	(0.063)	(0.064)	(0.065)
R-squared	0.525	0.528	0.528	0.529	0.562
Obs	21,531	21,531	21,531	21,531	21,531
Unique Cities	839	839	839	839	839
Year fixed effects	x	x	x	x	x
City fixed effects, city-specific trends	x	x	x	x	x
Demographic controls 1		x	x	x	x
Demographic controls 2			x	x	x
Local rates				x	x
Other intergovernmental revenue					x

*** Significant at the 0.1% level. Standard errors in parentheses.

Notes: Standard errors are clustered at the city level. Demographic controls set 1 are the log of population, the poverty rate, the vacancy rate, the unemployment rate, the share of people under age 18, and the share of population of foreign origin; demographic controls set 2 are number of housing units per capita, the real median family income, the share African American and the share Hispanic. 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 Appendix B for details.

Table 3: CDBG Impact Across Spending Categories

	(1) city fixed effects, city trends, year fixed effects, controls set 1 & 2	(2) city fixed effects, city trends, year fixed effects, controls set 1 & 2, all local rates
Total Revenues	1.243***	1.210***
	(0.310)	(0.304)
Total Expenditures	1.611***	1.594***
	(0.314)	(0.312)
Housing & Community Development	0.271***	0.276***
	(0.063)	(0.064)
Combination of All Possible Non-H&CD CDBG Categories	0.185*	0.193*
	(0.092)	(0.093)
Parks & Recreation	0.044	0.045
	(0.027)	(0.028)
Code Enforcement	-0.002	-0.001
	(0.005)	(0.005)
Sewers	0.114	0.122
	(0.069)	(0.072)
Roads & Streets	0.029	0.027
	(0.038)	(0.037)
Education	0.395**	0.379**
	(0.126)	(0.127)
Total Health	0.030*	0.030*
	(0.015)	(0.015)
Total Long-Term Debt Issued During Fiscal Year	0.101	0.083
	(0.263)	(0.270)
Total Long-Term Debt Retired During Fiscal Year	0.102	0.1
	(0.155)	(0.154)

*** Significant at the 0.1% level. ** Significant at the 1% level. * Significant at the 5% level.
Standard errors in parentheses.

Notes: Standard errors are clustered at the city level. Demographic controls are as described in the notes for Table 2. All regressions use 21,531 observations; results are robust to the maximal possible sample.

Sources: See Table 2.

Table 4: Heterogeneous Effect of Number of Elected Officials on Total Revenues & Targeted Spending

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		A. Total Revenues			B. Housing and Community Development Expenditures		
Interaction Specification							
		CDBG	CDBG*HV		CDBG	CDBG*HV	
Mean	7.9	0.392	0.096		0.375***	-0.013	
		(0.433)	(0.050)		(0.101)	(0.008)	
Coefficient Comparison Specification							
		CDBG Coeff if		p-value,	CDBG Coeff if		p-value,
		<= xth perc	> xth perc	difference	<= xth perc	> xth perc	difference
Median	7	0.838**	1.824**	0.126	0.186	0.319***	0.325
		(0.315)	(0.579)		(0.107)	(0.077)	
25th Percentile	5	0.269	1.523***	0.037	0.203	0.272***	0.677
		(0.494)	(0.354)		(0.152)	(0.069)	
75th Percentile	9	0.892**	2.392**	0.107	0.329***	0.047	0.112
		(0.280)	(0.904)		(0.067)	(0.163)	

*** Significant at the 0.1% level. ** Significant at the 1% level. * Significant at the 5% level. Standard errors in parentheses.

Notes: The dependent variable in panel A is total revenues; dependent variable in panel B is housing and community development expenditures. All regressions use 21,531 observations, and standard errors are clustered at the city level.

Sources: See Table 2 and Appendix B.

Appendix A: Verifying the Block Grant Formula

In order to verify that the CDBG program follows the legislated formula, we replicate annual grant allocations using the same publicly available data the Department of Housing and Urban Development (HUD) uses in its own calculations.

We compare our constructed allocations to the "actual" data, both the annual designation of entitlement and the annual allocation for entitled cities and counties from the beginning of the program in 1975 to 2004. These actual data come from HUD: from 1975-2001 courtesy of Todd Richardson, and from 1993-2004 from a file on the HUD website.[1]

First we attempt to identify entitlement jurisdictions. A city becomes an entitled city if it either (a) is in a metropolitan area and has a population over 50,000 in a given year[2], (b) is the principal city of a metropolitan area, or (c) has ever been an entitled city in the past for two consecutive years (after 1989 only).[3] This first population and metropolitan area criteria is measurable using decennial census data from 1975 to 1990 (see below for information on when data become available to HUD), combined with MSA status by county.[4] From 1990 to the present, the population and MSA status cutoff is measurable using Census population estimates for cities, which are publicly available. We do not use these data before 1990 because annual population estimates for cities are not available before 1990. The second condition, whether a city is a primary city of a metropolitan area, is not verifiable with publicly-available data. The census does not publish primary cities by metropolitan areas historically (they are defined by county for most of the country), nor are employment data by city public; both of these are necessary to replicate the designation.[5]

[1] See http://www.hud.gov/offices/cpd/about/budget/budget01/index.cfm.

[2] As does HUD, we use "metropolitan area" to refer to the variously-named Office of Management and Budget-defined metropolitan agglomerations, variously known as Metropolitan Statistical Areas, Core-Based Statistical Areas, New England Town Areas, etc.

[3] In practice, cities that receive grants once only very very rarely lose their entitlement status (email from Miller).

[4] For New England, MSAs are defined by town.

[5] Counties are entitled when they have a population of 200,000, excluding the population of entitled

Using only the population criteria, we can correctly identify roughly three-quarters of actual entitled cities across all the sample years. Appendix Table 1 presents annually our ability to verify entitlement status for cities and counties. Though the total number of entitled cities has grown from 525 in 1975 to 913 in 2004, we consistently identify roughly 75 percent using the population-metro area criteria. Over time, our ability to identify entitled counties with the population criteria decreases from from 98 to 73 percent. For the rest of the verification process, we take entitlement status (as given by HUD) as given, and construct allocations only for entitled jurisdictions.

To calculate the amount of the allocation for each jurisdiction, we begin with the total amount allocated by Congress (from Richardson (2005) for 1975-2002; HUD online data for 2003 and 2004). From 1982 onward, seventy percent of the total allocation was legislated for entitlement jurisdictions. Before 1982, the share mandated for entitlement jurisdictions was eighty percent.[6] Our task is then to divide up this total allocation for entitled jurisdictions among entitled cities and counties. Though our paper does not focus on counties, we cannot calculate city shares without also calculating county shares as they both take pieces from the same pie.

Each entitled city and county's share is assigned via a formula. From 1975 to 1977, there was a single formula that allocated each entitled jurisdiction's share of the pie as

$$\text{grant share}_c = \left((1/2)\frac{\text{pov}_c}{\text{pov}_{MA}} + (1/4)\frac{\text{pop}_c}{\text{pop}_{MA}} + (1/4)\frac{\text{ov crwd}_c}{\text{ov crwd}_{MA}} \right)$$

The index $c \in \{1, ..., C\}$ denotes a city (though this formula is identical for counties), and MA denotes all metropolitan areas (sum of values for all MSAs). The variables are *pov*, the total number of people with income less than the poverty line, *pop*, the total population, and

cities. In addition, there are six counties that received entitlement designation through special tweaks to the designation rules (Richardson 2000 p. C-1).

[6]Following Richardson et al. (2003), page 14.

ov crwd, or the number of people living in housing with less than 1.01 rooms per person.

Also from 1975 to 1977, actual allocations included grandfathered receipts from the prior program of application-based grants. For the first three years of the program (1975-1977), no city received less money than it received under the previous system; for the next three years (1978-1980), grants declined to the levels allocated by the formula. We do not include these grandfathered amounts in our constructed allocations.

Before 1978, allocations were made only on the basis of Formula A. Starting in 1978, and continuing to the present, cities and counties were assigned grants based on the maximum of two formulae:

$$\text{grant share}_{A,c} = \left((1/2)\frac{\text{pov}_c}{\text{pov}_{MA}} + (1/4)\frac{\text{pop}_c}{\text{pop}_{MA}} + (1/4)\frac{\text{ov crwd}_c}{\text{ov crwd}_{MA}} \right)$$

$$\text{grant share}_{B,c} = \left((2/10)\frac{\text{growth lag}_c}{\text{growth lag}_{EC}} + (3/10)\frac{\text{pov}_c}{\text{pov}_{MA}} + (1/2)\frac{\text{age}_c}{\text{age}_{MA}} \right).$$

Here EC denotes all entitled cities.[7] The new variables are *age*, the number of housing units built before 1940, and *growth lag*, which is the lack of growth since 1960s. During the two-formula era, a city's share is the maximum of the two shares above: $\max(\text{grant}_{A,c}, \text{grant}_{B,c})$.

HUD detests the growth lag variable because it is difficult to calculate and relies on information that must sometimes be estimated. It is meant to capture how much a city has deviated from the mean growth of all cities since 1960. We make our best approximation from publicly available data without reconstructing municipal border changes (which is what HUD does). In any given year, the numerator *growth lag*$_c$ for a city c is calculated by

$$\text{difference}_{c,t} = (1960\ \text{pop}_c * \frac{\sum_{c=1}^{C} \text{pop}_{c,t}}{\sum_{c=1}^{C} \text{pop}_{c,1960}}) - \text{pop}_{c,t}, \text{ and}$$

[7] Counties use the same formula, with the exception of the denominator for growth lag, which is replaced by the total growth lag in all entitled jurisdictions (cities and counties) (Richardson et al. (2003), p. 5).

$$\text{growth lag}_{c,t} = \begin{cases} \text{difference}_{c,t} & \text{if difference}_{c,t} \geq 0 \\ 0 & \text{if difference}_{c,t} < 0 \end{cases} .$$

If a city's population in a given year is larger than its 1960 population times the average growth rate, it receives a growth lag value of zero. If a city's population in a given year is smaller than its 1960 population times the average growth rate, growth lag measures the number of extra people the city would have had, had it grown at the average rate since 1960.

The denominator for the growth lag variable for cities, $growth\ lag_{EC,t}$, is

$$\sum_{c=1}^{C} 1960\ \text{pop}_c * \frac{\sum_{c=1}^{C} \text{pop}_{c,t}}{\sum_{c=1}^{C} \text{pop}_{c,1960}} - \sum_{c=1}^{C} \text{pop}_{c,t} .$$

If a city has zero population in 1960, it has zero growth lag. Cities with no growth lag – those with no population in 1960 – do not go into calculating the denominator of the growth lag equation.

For counties, the growth lag situation is somewhat more complicated. Each county's initial 1960 population is the county's 1960 population minus cities that would have been entitled in 1960. The current year population is the county population minus the population residing in entitled cities. The mean growth rate is the growth rate of all entitled communities (cities and counties), unlike for cities, which just uses the mean city growth rate. Parallel to the cities, if a county grows more than the mean of all entitlement communities, it receives a growth lag value of is zero (Richardson et al. (2003), p. 5 and p. 56-7 for details).

These formulae assign a share of the grant pie in each year. In the years with the dual formula system, this system assigns more than the entire pie, so HUD reduces each entitled community's share, keeping the relative shares constant. We multiply this pro-rated share with the total amount allocated for entitled jurisdictions to arrive at an annual allocation for each entitled jurisdiction.

Following the description in Richardson et al. (2003), we use census data in the third

year after the decennial census with which it is associated. For example, allocations in 2000, 2001 and 2002 are based on 1990 census data; only in 2003 are allocations updated with the 2000 census data. Because this accords with the majority of allocation updates (though not all), we keep this method. This method leaves less than one percent of actual entitled jurisdictions without data.

Our constructed allocations give a quite good match to the actual allocations for entitled cities, as shown in Appendix Table 2. Only in the first three years, which include some grandfathered allocations, is the correlation between the constructed allocation and the actual allocation less than 0.99. The average correlation across the thirty years of the sample is 0.982. We do not do quite as well for matching county allocations, but this is not a challenge to the estimation as the county allocations merely change the amount of funds available to entitled cities, not cities' relative shares.

In rare cases, some cities choose to decline entitled city status in order to receive funds with an entitled urban county – usually this occurs when the county would fail to receive funds without the city's population. In general, cities are loath to do this, because there is no guarantee the county will allocate the city as much money as it would have gotten on its own. Six cities which would otherwise be entitled and receive grants choose to be part of entitled urban counties: Palm Bay, FL; Duluth, MN; Pharr, TX; West Jordan, UT; Bremerton, WA; Vancouver, WA; and Rapid City, SD. We calculate grants for these cities when they are entitled cities, but we drop them in all of our analytical work.

Consolidated cities (e.g., Athens-Clarke County, GA or Nashville-Davidson, TN) receive funds as entitled cities.

Appendix B: Data Sources

Our dataset is at the city-year level, with observations from 1975 to 2004. Data comes from the sources listed below.

The decennial census data serve as the frame to which all other data are added.

- Census
  - Decennial Censuses: City- and County-Level Data

    1970 Census , ICPSR 8109, 8107, 8129

    1980 Census Summary File 3A, ICPSR 8071

    1990 Census Summary File 3A, ICPSR 9782, save CA which is damaged; used file from UCLA ATS

    2000 Census Summary File 3, ICPSR 13342-13392

    Demographic information by Census place, and county
  - Decennial Censuses: Metropolitan Area-Level Data

    1970 Census , ICPSR 8109, 8107, 8129

    1980 Census Summary File 3C, ICPSR 8038

    1990 Census Summary File 3C, ICPSR 6054

    2000 Census Summary File 3 National, ICPSR 13396

    Demographic information by metropolitan area
  - Decennial Census, via City and County Databook

    1960 Census City-Level Data, cities 25000+ population, ICPSR 7735

    Demographic information by city and county
  - Annual Survey of Government Finances, Census of Government Finances

    Consistent-definition file received from Governments Division, Census Bureau
  - Population Estimates

Population estimates for cities (1990 onward) and counties (1975-2004), Census Bureau Population Estimates Division, http://www.census.gov/popest/estimates.php

  - Metropolitan Area Definitions

    Used definitions (counties/town for each MSA) used to report decennial census data

    Definitions dated April 27, 1973 (for 1970 Census), June 30, 1981 (for 1980 Census), June 30, 1990 (for 1990 Census), June 30, 1999 (for 2000 Census)

  - 1987 Census of Governments: Government Organization File, ICPSR 9388

    Institutional information for municipalities

- Consumer Price Index

  Bureau of Labor Statistics, All Urban Consumers

- Community Development Block Grant Data for Entitlement Jurisdictions

  - Annual Allocations, 1975-2001

    File from Todd Richardson, HUD

  - Annual Allocations, 1993-2004

    http://www.hud.gov/offices/cpd/about/budget/budget01/index.cfm

Conversations and emails with Sue Miller, Director, Entitlement Communities Division, Office of Block Grant Assistance, HUD, proved invaluable in understanding the workings of the CDBG program.

Appendix B Table 1: Verifying Entitlement Status

	Entitled Cities			Entitled Counties		
year	Predicted	Actual	Predicted/Actual	Predicted	Actual	Predicted/Actual
1975	390	525	0.743	73	74	0.986
1976	393	526	0.747	74	76	0.974
1977	395	538	0.734	76	79	0.962
1978	397	550	0.722	77	82	0.939
1979	398	553	0.720	79	85	0.929
1980	397	564	0.704	79	86	0.919
1981	400	574	0.697	80	87	0.920
1982	401	625	0.642	80	97	0.825
1983	469	626	0.749	87	99	0.879
1984	474	678	0.699	90	105	0.857
1985	484	694	0.697	94	108	0.870
1986	490	697	0.703	96	117	0.821
1987	497	698	0.712	96	116	0.828
1988	503	722	0.697	98	122	0.803
1989	512	723	0.708	99	122	0.811
1990	520	727	0.715	99	126	0.786
1991	526	743	0.708	101	126	0.802
1992	537	743	0.723	101	132	0.765
1993	576	741	0.777	102	134	0.761
1994	580	785	0.739	102	136	0.750
1995	593	791	0.750	102	139	0.734
1996	600	798	0.752	104	140	0.743
1997	611	816	0.749	104	142	0.732
1998	619	821	0.754	107	146	0.733
1999	627	822	0.763	109	148	0.736
2000	632	837	0.755	111	150	0.740
2001	639	839	0.762	112	153	0.732
2002	645	844	0.764	113	159	0.711
2003	693	853	0.812	121	160	0.756
2004	701	913	0.768	121	165	0.733

Sources: See text and Appendix B.

Appendix B Table 2: Evaluating the Match

	Cities				Counties			
Year	Entitled	Allocation Constructed	Share Calculated	Corr: Constructed & Actual Allocation	Entitled	Allocation Constructed	Share Calculated	Corr: Constructed & Actual Allocation
1975	525	504	0.960	0.869	74	74	1	0.628
1976	526	505	0.960	0.882	76	76	1	0.751
1977	538	515	0.957	0.958	79	79	1	0.878
1978	550	550	1	0.990	82	82	1	0.928
1979	553	553	1	0.992	85	85	1	0.936
1980	564	564	1	0.993	86	86	1	0.926
1981	574	574	1	0.994	87	87	1	0.923
1982	625	625	1	0.994	97	97	1	0.923
1983	626	626	1	0.988	99	99	1	0.978
1984	678	678	1	0.993	105	105	1	0.985
1985	694	694	1	0.993	108	108	1	0.986
1986	697	697	1	0.993	117	117	1	0.986
1987	698	698	1	0.992	116	116	1	0.986
1988	722	722	1	0.993	122	122	1	0.984
1989	723	723	1	0.993	122	122	1	0.984
1990	727	727	1	0.992	126	126	1	0.984
1991	743	743	1	0.992	126	126	1	0.982
1992	743	743	1	0.992	132	132	1	0.981
1993	741	741	1	0.986	134	134	1	0.977
1994	785	785	1	0.985	136	136	1	0.974
1995	791	791	1	0.990	139	139	1	0.980
1996	798	798	1	0.990	140	140	1	0.981
1997	816	816	1	0.990	142	142	1	0.979
1998	821	821	1	0.990	146	146	1	0.978
1999	822	822	1	0.990	148	148	1	0.978
2000	837	837	1	0.990	150	150	1	0.977
2001	839	839	1	0.990	153	153	1	0.978
2002	844	844	1	0.989	159	159	1	0.977
2003	853	852	0.999	0.988	160	160	1	0.977
2004	913	910	0.997	0.988	165	165	1	0.977

Sources: See text and Appendix B.

Appendix C Table 1: Effect of Number of Elected Officials Per Capita on Total Revenues & Targeted Spending

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		A. Total Revenues			B. Housing and Community Development Expenditures		
Interaction Specification							
		CDBG	CDBG*HV		CDBG	CDBG*HV	
Mean	0.137	0.597	3.459		0.245	0.138	
		(0.972)	(6.513)		(0.151)	(0.781)	
Coefficient Comparison Specification							
		CDBG Coeff if		p-value,	CDBG Coeff if		p-value,
		<= xth perc	> xth perc	difference	<= xth perc	> xth perc	difference
Median	0.106	1.051*	1.213***	0.768	0.096	0.334***	0.126
		(0.477)	(0.348)		(0.131)	(0.074)	
25th Percentile	0.061	0.728	1.286***	0.44	0.058	0.309***	0.261
		(0.689)	(0.318)		(0.212)	(0.064)	
75th Percentile	0.185	1.350***	0.935	0.486	0.288**	0.259**	0.826
		(0.318)	(0.528)		(0.091)	(0.091)	

*** Significant at the 0.1% level. ** Significant at the 1% level. * Significant at the 5% level. Standard errors in parentheses.

Notes: The dependent variable in panel A is total revenues; dependent variable in panel B is housing and community development expenditures. All regressions use 21,531 observations, and standard errors are clustered at the city level.

Sources: See Table 2 and Appendix B.