Measuring the Impact of Community Development Block Grant Spending on Urban Neighborhoods

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Abstract

Regression analysis of Community Development Block Grant (CDBG) spending in 17 large cities reveals strong statistical associations between spending from 1994 to 1996 and changes in three indicators of neighborhood conditions: the home purchase mortgage approval rate, the median amount of the home purchase loans originated, and the number of businesses. However, there is no consistent association between spending and indicators of subsequent neighborhood change unless CDBG spending is sufficiently spatially targeted that it exceeds a threshold of the sample mean expenditure and is measured relative to the number of poor residents. In addition, associations vary according to neighborhood trajectories before investment and changes in the local economy.

Nevertheless, even in the least hospitable contexts—highly concentrated neighborhood poverty, preexisting declines in home values, weak city job growth—our estimates are consistent with the hypothesis that above-threshold CDBG spending produces significant neighborhood improvements. We discuss the implications for such spatially targeted spending and connections between our work and the emerging literature on the dynamics of poor neighborhoods.

Keywords: Community development and revitalization; Federal government; Urban policy

Introduction

For 30 years, the Community Development Block Grant (CDBG) Program has been the primary vehicle through which the federal government funds improvements to America's low-income neighborhoods. Currently, cities, urban counties, and states are allocated more than $4 billion annually on the basis of a national CDBG formula that takes into account population, poverty rates, age of the housing stock, and other needs (U.S. Department of Housing and Urban Development [HUD] 2003).

The Community Development Act of 1974, which created the CDBG Program, noted in §101(a)(1)–(2) that cities faced critical social, economic, and environmental problems arising from population growth, increasing concentration of low-income residents, and inadequate public and private investment and reinvestment in housing and other physical facilities. The program was intended to create "viable urban communities as social, economic, and political entities" (§101(b)(1)) by systematic and sustained action by federal, state, and local governments to eliminate blight and conserve and renew older urban areas "to provide decent housing and a suitable living environment, principally for persons of low- and moderate-income" (§101(c)).

With the federal support authorized under the act, local governments were expected to accomplish these overall goals by arresting the deterioration of property and neighborhood and community facilities, removing conditions detrimental to health and safety, conserving the housing stock, improving community services, promoting income integration and neighborhood diversity through spatial deconcentration of assisted housing and revitalization of deteriorating neighborhoods, and stimulating private investment in areas with population outmigration and stagnating tax bases. The act did not require localities to adopt a specific mix of these activities, but rather allowed them to pick and choose those that, in their view, best met the program's intent.

Clearly, the overall thrust of the program as reflected in the authorizing legislation was to improve neighborhood economic and social conditions, as distinct from helping specific low-income individuals (although communities are free to emphasize this as they choose). Indeed, the CDBG Program is "an emphatically neighborhood-centered initiative" (Walker and Boxall 1996, 25). Therefore, the question of whether CDBG jurisdictions have managed to improve the quality of their neighborhoods by using the resources the act provides is of considerable public policy interest. Put differently, to what degree have local governments successfully carried out their federal mandate under the CDBG Program?

Much scholarly research has focused on where and how CDBG funds have been spent, which groups have been the prime beneficiaries, how efficient

the plans and their implementation have been, and what political forces lie behind these allocations (MKGK Incorporated 1980; Nathan et al. 1977; Rich 1993; Urban Institute 1994; Wong and Peterson 1986). By contrast, whether and under what circumstances these investments have produced any measurable changes in the trajectories of the affected neighborhoods has been the subject of only two inquiries, neither devoted primarily to measuring the program's impact.

Bleakly et al. (1983) examined 30 Neighborhood Strategy Areas (NSAs) in 20 cities from 1979 to 1981. The NSA demonstration arose from a set of 1977 CDBG Program amendments encouraging a coordinated strategy through the spatial targeting of CDBG funds in conjunction with the Section 8 Substantial Rehabilitation Program and other investments. They examined an index of neighborhood conditions based on the average of four components: the percentage of structures in very good condition and the percentage of blocks in the area with well-maintained streets, little litter, and landscaping in very good condition. Although no details of the analysis or statistical tests are presented, the authors report that "improvement in neighborhood conditions [1979–1981] appears related to … a higher-than-average concentration of CDBG expenditures per block" (1983, 3).

As one part of a comprehensive national assessment of the program, a team from the Urban Institute (1994) analyzed statistics from a random sample of 223 census tracts drawn from a nationally representative sample of 60 cities. In cross-tabulating per-capita CDBG expenditures and changes in poverty rates in these census tracts from 1980 to 1990, they found that tracts with declining poverty rates averaged $1,247 per capita, whereas those with stable and rising poverty rates evinced lower spending levels of $844 and $737 per capita, respectively (although no tests of statistical significance are reported). The Urban Institute's (1994) study also solicited from local experts 16 qualitative analyses of case studies of selected neighborhoods where CDBG funds were targeted. On the basis of this and other evidence, it concluded that the CDBG Program "has made an important contribution to city community development, including demonstrated successes in achieving local neighborhood stabilization and revitalization objectives" (Urban Institute 1994, i).

Neither the Bleakly et al. (1983) nor the Urban Institute (1994) study aimed to produce statistically conclusive answers regarding the neighborhood impact of CDBG spending, and, indeed, they did not. But, increasingly, policy makers are demanding better evidence that public funds have produced the intended results. The Government Performance and Results Act, passed by Congress in 1992, aims to increase effectiveness and accountability by requiring agencies to measure the results of their program expenditures. CDBG was

no exception, and HUD's 2001 Annual Performance Plan specifies that "neighborhoods with substantial levels of CDBG investment will show improvements in such dimensions as household income, employment, business activity, homeownership and housing investment" (163).

Therefore, for reasons of scholarship as well as sound policy, the effects of community development spending on urban neighborhoods are worth examining rigorously. In this research, we address two key questions.

1. Has CDBG spending made a measurable difference in low-income neighborhood trajectories—the pace and direction of economic and social change—as measured by three robust indicators?
2. Is the answer contingent on neighborhood context and the spatial concentration of CDBG spending?

We attempt to provide convincing answers to these questions by

1. Analyzing a large sample of census tracts in 17 cities
2. Conducting multivariate statistical tests of relationships using a variety of outcome indicators
3. Disaggregating CDBG expenditures by type
4. Searching for minimum thresholds of CDBG expenditures
5. Testing the sensitivity of relationships in different neighborhood and city economic contexts

Our article is organized as follows. We begin by presenting our conceptual and statistical model relating CDBG spending to changes in neighborhood conditions. We then describe our sample of 17 cities and the procedures we undertook to acquire usable information and robust neighborhood indicators. We next report regression results for the entire sample and results disaggregated by type of CDBG spending, then show how relationships vary by neighborhood and city context. After that, we discuss results as they relate to the policy debate over targeting investments spatially. Finally, we close with caveats in interpretation, suggestions for further research, and connections between our work and emerging literature on the dynamics of poor neighborhoods.

CDBG spending and neighborhood change: Conceptual and statistical frameworks

Conceptual model

In principle, CDBG investments could produce neighborhood improvements both directly and indirectly—directly by, for instance, renovating the housing stock, creating or upgrading community facilities and public infrastructure, and other activities that immediately create added value in neighborhoods. It could improve neighborhoods indirectly by investing in one or a series of projects that encourage private investors to view CDBG-funded neighborhoods as places where favorable economic returns can be generated.

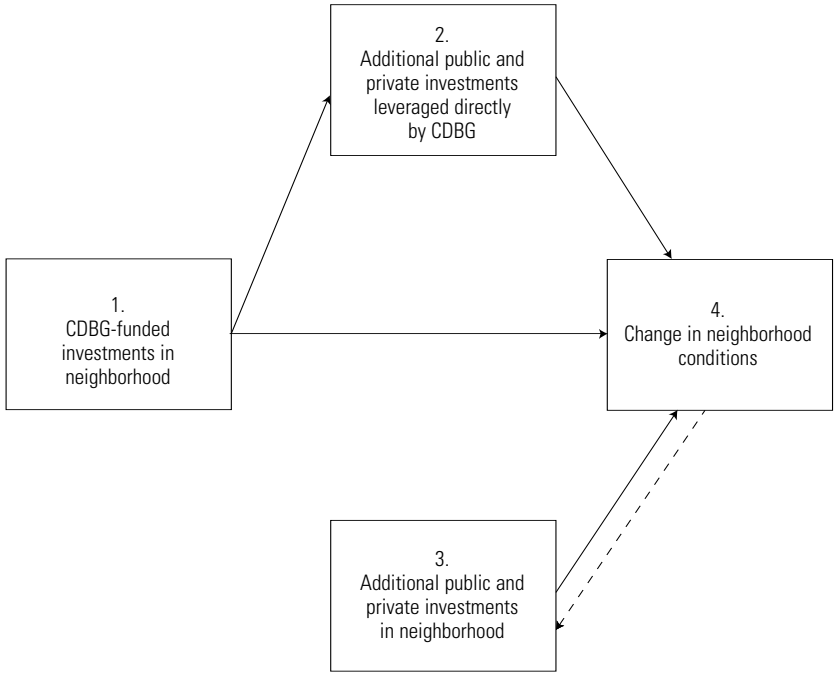
Many community development practitioners and scholars argue, however, that a critical mass of improvements is needed to trigger changes in the perception of investment prospects in a distressed neighborhood, but that once this critical mass is achieved, the pace of neighborhood improvement accelerates (Galster, Quercia, and Cortes 2000; Pollock and Rutkowski 1998; Quercia and Galster 1997, 2000; Taub, Taylor, and Dunham 1984; Thomson 2003). This critical mass of investment represents a threshold after which relationships between CDBG investment and neighborhood improvement alter dramatically for the better.

We expect, nevertheless, that the productivity of CDBG investments in creating neighborhood effects (whether above or below threshold) is contingent on two sets of factors: overall conditions in the city and preexisting conditions and trajectories of change in the neighborhoods targeted for CDBG investments. Depending on their initial inventory of assets and liabilities and their recent trends, neighborhoods may respond quite differently to the same intensity of CDBG investment. Analogously, the same amount of CDBG investment is less likely to produce improvements in neighborhoods located in cities where the larger economic, demographic, and social stimuli are weaker—that is, where unemployment, outmigration, and crime are increasing. Our point is that these factors provide a context that probably shapes the observed relationships among CDBG-funded investments, other private and public investments, and changes in neighborhood quality. This contextualization may take the form of altering the CDBG investment threshold or merely changing the relationship between such investment and neighborhood outcomes, either leading up to and/or exceeding the threshold. We will employ proxies for these contextual factors as bases for stratifying our regressions.

Figure 1 summarizes the relationships we have described. Of prime interest, changes in neighborhood conditions (box 4) are directly influenced by CDBG-funded investments (box 1) and indirectly influenced by the other public and

private investments that they may leverage in conjunction with the initial projects (box 2). Neighborhood conditions may also be influenced by other public and private investments that may initially have no connection to CDBG (box 3). In the best case when thresholds have been exceeded, CDBG investment–produced neighborhood improvements may trigger a "virtuous cycle" in which other public and private sources are subsequently induced to invest and improve neighborhood conditions, thereby inducing further public and private investment, and so on (the dotted feedback arrow in figure 1). The two key sets of encompassing contextual factors are shown at the bottom of figure 1.

Figure 1. A Model of CDBG and Other Influences on Neighborhood Conditions



Statistical model

Our statistical model builds on figure 1 as follows: For a sample of neighborhoods (census tracts) across 17 cities, we directly measure the annual amount of CDBG investment per poor resident (averaged over the 1994–1996 period) and corresponding changes in the values of several neighborhood indicators from 1994 to 1999. We regress the latter on the former to discern the basic relationship. We implicitly assume a lag structure because not all of the effects of CDBG spending should register immediately or completely during the year in which they occurred. We measure CDBG spending adjusted for the poor population in the neighborhood because theoretically the impact of a given amount of spending should differ depending on the depth of local needs and regulatory requirements that such spending primarily benefit low- and moderate-income people.

Unfortunately, there are no known cross-city data sources that would allow us to measure annually the private or public investment from nonfederal sources by neighborhood (local spending on water and sewer infrastructure, streets, public safety, parks and open space, or other municipal services). We do not believe, however, that failure to control for these non-CDBG investments will be problematic as we answer the research question at hand. First, if CDBG and other sorts of investments are not correlated, excluding the latter will not produce bias in the estimate of the relationship between CDBG spending and neighborhood changes.

Second, insofar as non-CDBG investments are correlated with *and induced by* CDBG spending, the observed relationship between such spending and neighborhood changes will constitute a reduced-form estimate of both direct and indirect CDBG effects. The estimated relationship between CDBG spending and neighborhood changes will prove biased only to the extent that such spending is *caused by* prior, non-CDBG investments. We proceeded under the assumption that these investments did not affect observed patterns of CDBG spending, which is consistent with previous research on the determinants of CDBG allocations (Rich 1993; Wong and Peterson 1986).

We explore the empirical importance of the theorized contextual factors by conducting sensitivity tests over various strata of city and neighborhood types. Finally, we investigate the existence of a threshold of CDBG expenditures by experimenting with cubic regression and by estimating regressions over samples of neighborhoods stratified by the intensity of CDBG spending. Cubic regressions permit the estimation of a wide range of nonlinear relationships, some of which may suggest thresholds.

In sum, our statistical model may be summarized symbolically as follows:

$$Y99_i = a + b_1(CDBG_i) + b_2(CDBG_i^2) + b_3(CDBG_i^3) + b_4(Y94_i) + \epsilon \qquad (1)$$

where

a = intercept

b = coefficient

Y99 = 1999 value for outcome indicator Y in neighborhood i

Y94 = 1994 value for outcome indicator Y in neighborhood i

CDBG = annual average amount of CDBG 1994–1996 expenditures, divided by the number of residents below the poverty line, 1990 (both measured in neighborhood i)

ϵ = a random error term with the usual assumed statistical properties

Sample of cities analyzed

We conducted our analysis on the central cities (municipalities) we selected to cover all U.S. regions, ensure differences in metropolitan-area job growth (a proxy for overall economic health), and provide wide variation in CDBG investments across census tracts within cities. We were tightly constrained in our selections by the adequacy of HUD's CDBG data in the locale and the availability of other databases providing annual observations of multifaceted local administrative data for census tracts, which served as the basis for our neighborhood indicators. Resource constraints in the face of costly cleaning of HUD's CDBG records (explained later) limited our final sample to 17 cities.

Five cities—Boston, Cleveland, Indianapolis, Oakland (CA), and Providence (RI)—were chosen with certainty because of their unusually robust data sets with annually observed neighborhood indicators. Four additional cities—Columbus (OH), Fort Lauderdale (FL), Houston, and Portland (OR)—were chosen because they were test sites for the American Community Survey (ACS) and thus also offered unusually rich neighborhood data.[1] The remaining eight cities—Birmingham (AL), Charlotte (NC), Denver, Los Angeles, Long Beach (CA), Milwaukee, Tulsa (OK), and Washington, DC—were selected to fill in gaps in the sampling frame, with preference given to cities with higher

[1] When we originally drew the sample, our intention was to use the early rounds of census tract–level information obtained from the ACS during the late 1990s, but delays in the ACS later made this not feasible.

CDBG allocations and the least amount of missing information in the HUD CDBG database. The final sample, stratified by region and metropolitan growth profile, is shown in table 1; respective CDBG allocations for 2000 are shown parenthetically.

Table 1. Cities Sampled, by Region and 1994–1997 City Job Growth Category

Percent Change in City's Jobs	Region			
	Northeast	Midwest	South	West
No growth –11.8 to 1.5	Providence, RI (7.3)	Milwaukee, WI (22.2)	Washington, DC (23.5)	Los Angeles, CA (89.8)
			Fort Lauderdale, FL (2.7)	
Low growth 1.6 to 6.0	Boston, MA (24.8)	Cleveland, OH (30.1)	Birmingham, AL (8.3)	Denver, CO (11.6)
				Long Beach, CA (9.3)
High growth 6.1 to 48.8	NA	Columbus, OH (8.5)	Charlotte, NC (4.7)	Oakland, CA (10.3)
		Indianapolis, IN (11.8)	Houston, TX (35.0)	Portland, OR (11.8)
			Tulsa, OK (4.8)	

Sources: Job change: HUD 2000; CDBG: HUD CDBG Data Tracking System (IDIS and its predecessors).
Note: CDBG allocations for 2000, in millions of dollars, are shown parenthetically.
NA = not applicable.

We recognize that our sample implies that findings may be generalizable only to those central cities receiving substantial allocations of CDBG funds. Nevertheless, we see this selection as appropriate, given the dearth of statistical studies on CDBG effects, combined with expectations of a minimum threshold of investment required before effects would be observed. If we were to find no effects in the cities where CDBG spending has been most intensive, we would not be likely to find them in participating suburban jurisdictions either.

Data sources

Data on CDBG expenditures

The success of any analysis of the impact of CDBG programs at the neighborhood level rests on an accurate depiction of the amounts and location of CDBG spending. Unfortunately, given the quality of the CDBG data available from HUD's record systems at the time of this study, such accuracy is

extremely difficult to achieve. Indeed, we believe that the sizable investment of resources required to clean data has heretofore been a major barrier to the systematic study of CDBG neighborhood effects. Despite our best efforts, the CDBG data we used contain measurement errors that weaken the precision of our statistical results. Moreover, we suspect that our algorithms for handling data with ambiguous spatial information, although reasonable, have the effect of biasing our regression coefficient on CDBG spending toward zero. Following is a brief description of the measures of CDBG activity developed for this research and the approach we used to construct as complete and accurate a database as possible; details and discussion of measurement errors and possible bias are presented in appendix A.

We used HUD's Integrated Disbursement and Information System (IDIS), other (older) administrative data maintained by HUD, and, as necessary, direct contact with CDBG grantees to construct a comprehensive database of CDBG neighborhood expenditures. Four main steps were involved:

1. *Compiling data from different administrative data sources*. Because of the timing of the IDIS phase-in, we were forced to combine disparate CDBG records for the 1994–1996 period from both IDIS and older administrative databases unique to each city. In several cities, there were gaps in the older data. Others created gaps as they switched systems, thus forcing us to interpolate data for some periods.

2. *Geocoding the locations of CDBG-funded activities*. The administrative records differed widely in the specificity and accuracy of the information about the location of site-specific CDBG-funded activities. The median percentage of cases across our sample in which we were able to assign site-specific expenditures to a particular census tract was 70 percent.

3. *Attributing certain expenditure types to particular neighborhoods*. For CDBG expenditures that were not site-specific, we adopted an allocation algorithm. Expenditures indicated for area benefit and direct benefit activities that occurred in more than one designated census tract were prorated equally among tracts; the median share of such expenditures in our sample was 16 percent. We assigned citywide expenditures that grantees targeted to eligible persons in their jurisdiction among census tracts in proportion to the share of the jurisdiction's 1990 population living below the poverty line.

4. *Distinguishing the type of expenditure*. Using codes provided in the databases, we grouped CDBG expenditures into four categories—housing, economic development, public facilities and social services, and property acquisition and disposition.

Data on neighborhood indicators

Local policy makers and community development practitioners have pursued a wide range of activities intended to improve neighborhoods, as measured by a comprehensive array of demographic, economic, and social indicators (Sawicki and Flynn 1996). Given the potential for an unwieldy set of indicators to analyze as dependent variables, we sought a parsimonious set that nevertheless would reflect a breadth of neighborhood outcomes most likely of interest to scholars and local policy makers alike.

Our method for selecting this set proceeded in four steps as follows. We first identified the five cities for which unusually rich, annually updated neighborhood (census tract) data could be assembled from local administrative records: Boston, Cleveland, Indianapolis, Oakland (CA), and Providence (RI). To these administrative data, we appended 1990 census tract demographic, socioeconomic, and housing stock characteristics, plus data from nationally available sources (Home Mortgage Disclosure Act [HMDA], Dun and Bradstreet, vendors of home sales records) for 1993–1994 and 1998–1999. Once completed, these data sets described mortgage market activity, home sale prices, jobs and firms, crime, public assistance, and vital population statistics for neighborhoods in these five cities during the 1990s.

Second, using these data sources, we developed three to four dozen (depending on the site) neighborhood outcome indicators that included a wide range of factors that closely correspond to the neighborhood socioeconomic and housing conditions that the CDBG Program was designed to address, such as poverty, crime, and inadequate housing.

Third, we conducted factor analyses on the full range of indicators in each of the five cities to ascertain whether a smaller number of distinct dimensions of neighborhood conditions could be consistently identified. Results showed remarkable cross-sectional and intertemporal comparability, especially considering the variety of locations, ages, demographics, and economic bases. In each of our five cities, five or six common clusters of indicators emerged that we believe correspond to intuitively plausible dimensions of neighborhood condition.[2] The most heavily weighted indicators in each factor provide a heuristic suggestion of a label signifying a dimension: social disadvantage, housing type and tenure, prestige, business and employment, crime, and housing vacancy. The legislative history in the Introduction makes it clear that these

[2] These factors had Eigen values greater than unity and explained 3 percent or more of the variance in the data set. Cumulatively, these six factors explained approximately two-thirds of the total variance, differing modestly by up to 5 percentage points depending on the city and the date. Details for each of our five cities are available from the first author.

common neighborhood dimensions closely correspond to the factors that CDBG investments should reasonably be expected to influence if they are efficacious.

Fourth, we ascertained through regression and correlation analyses the degree to which a more limited set of indicators available and annually updated in *all* cities (not just those with special administrative databases) might capture significant variation in most of these key neighborhood dimensions. We found that HMDA-generated data on mortgage approval rates and loan amounts, and Dun and Bradstreet data on businesses did so and thus were selected as the parsimonious set for our analysis. For more details, see appendix B and Galster, Hayes, and Johnson (2004).

To summarize, our research suggested that three indicators based on widely available data sources offer robust proxies for several broader dimensions of neighborhood in which the Community Development Act was interested and therefore offer reasonable (if not exhaustive) indicators for measuring the impact of CDBG expenditures on neighborhoods. Changes in these indicators serve as alternative dependent variables in the statistical model summarized in equation (1): the home purchase mortgage approval rate, the median amount of the home purchase loans originated, and the number of businesses.

Results

Thresholds of CDBG spending

Our principal measure of neighborhood CDBG spending was the annual average program expenditure, by census tract, from 1994 through 1996.[3] Given assumed lags between recorded expenditures, completion of funded projects, and the recognition of them by market forces in the neighborhood, we specified a three-year lag between the investment and the measurement of the cumulative changes in outcome indicators (1994 through 1999). CDBG expenditures were averaged over three years to help ensure that longer-term investment patterns, instead of possibly atypical transitory measures, were captured. The measure was developed for all census tracts in each of the 17 sampled cities and included expenditures on activities that directly benefited low- and moderate-income families, such as housing rehabilitation, service provision, or economic development, as well as neighborhoodwide activities

[3] The period was defined by program year, which, depending on the grantee, sometimes starts midway through the calendar year.

such as infrastructure and improvements in public facilities.[4] Our measure excluded CDBG general program administration and planning expenditures, since this type of spending could not be associated with particular neighborhoods.

We experimented with a variety of operationalizations of the independent variable—annual average CDBG spending in the census tract over the 1994–1996 period—alternating between absolute dollar amounts and standardizations by total tract population and total tract population living below the poverty line. CDBG expenditures per poor resident in the census tract consistently produced the strongest results across our various outcome measures and will be reported here. This finding suggests that CDBG effects are most appropriately measured in relation to the scale of local needs. Put differently, the ability of a marginal increase in CDBG spending in a tract to change neighborhood indicators appears to be inversely related to the concentration of poverty in that neighborhood.

Even using CDBG spending per poor resident, however, was typically not enough to yield a statistically significant, positive relationship between spending and changes in our neighborhood indicators when we analyzed either the full sample of census tracts or only those with non–zero values of CDBG spending. Results changed dramatically, however, when we confined our analysis to those tracts evincing above-sample-average CDBG spending: an annual average of $86,737 or more from 1994 to 1996.[5] Descriptive statistics for key variables for this set of tracts, by city, are presented in table 2.

For this stratum of neighborhoods, we observed statistically significant, positive relationships between CDBG spending per poor resident and subsequent changes in all three neighborhood indicators that suggested improved conditions overall (see table 3). Each additional $100 of CDBG spending per poor resident annually from 1994 to 1996 was associated with cumulative increases over five years of $2,358 (4 percent of the 1993–1994 sample mean) in median mortgage loan originated, 0.44 percentage points (less than 1 percent of the 1993–1994 sample mean) in mortgage application approval rates, and 1.77 businesses (1 percent of the 1993–1994 sample mean) in the neighborhood. Although for some indicators the squared and cubed CDBG variables also proved statistically significant, they were always substantively trivial in shaping what essentially proved to be a linear relationship (above the mean).

[4] While expenditure categories were tracked in HUD databases, direct benefit versus area benefit expenditures were not explicitly separated.

[5] This is the mean produced after a few extreme outliers were excluded from the sample.

Table 2. Descriptive Statistics for Key Variables for Census Tracts above the Threshold, by City

City	Mean 1990 Tract Population	Mean 1990 Tract Poverty Rate (%)	Mean Change in Home Prices, 1990 to 1994 (%)	Mean Annual CDBG Spending per Tract, 1994 to 1996 ($)	Neighborhood Outcome Indicators					
					Median 1993–1994 Loan Amount (Thousands of Dollars)	Median 1998–1999 Loan Amount (Thousands of Dollars)	Mean 1993–1994 Loan Approval (%)	Mean 1998–1999 Loan Approval (%)	Mean Businesses, 1995	Mean Businesses, 1999
Birmingham, AL	4,581	35.3	81.8	250,290	32.65	41.15	59.6	46.3	302	265
Boston, MA	3,491	23.1	–6.7	265,962	73.30	114.30	64.5	61.3	89	85
Charlotte, NC	2,582	34.2	–4.0	471,667	43.83	63.00	62.0	50.7	196	221
Cleveland, OH	2,439	37.9	27.8	173,307	25.84	48.88	57.2	50.8	90	82
Columbus, OH	3,695	32.4	9.5	182,356	50.43	68.68	61.5	57.5	218	219
Denver, CO	3,228	27.5	71.8	305,061	65.93	103.58	71.4	67.5	367	341
Fort Lauderdale, FL	4,425	45.4	–23.3	285,666	50.50	60.75	66.6	50.7	368	332
Houston, TX	3,918	29.9	0.2	230,123	40.85	57.93	60.9	54.2	230	217
Indianapolis, IN	2,796	35.2	21.3	199,117	25.89	47.33	59.9	48.2	83	72
Long Beach, CA	7,419	25.4	–30.2	156,985	115.48	108.24	58.6	59.2	227	200
Los Angeles, CA	5,519	28.4	–24.9	294,311	130.08	131.98	57.6	56.6	197	176
Milwaukee, WI	2,831	36.8	3.3	180,148	21.56	37.37	61.2	48.6	72	55
Oakland, CA	2,866	24.2	87.8	205,005	96.15	103.50	51.9	49.2	311	263
Portland, OR	3,549	21.0	41.8	221,681	70.75	110.08	73.5	68.1	244	254
Providence, RI	5,478	29.2	33.5	169,814	67.92	76.42	64.8	53.9	317	280
Tulsa, OK	2,196	30.3	4.6	283,258	47.19	37.50	67.1	50.3	99	80
Washington, DC	3,267	19.9	–27.1	279,033	99.35	110.16	67.1	63.6	129	117
Mean (unweighted)	3,781	30.4	15.7	244,340	62.22	77.70	62.7	55.1	208	192

Note: The threshold is defined as 1994–1996 average CDBG spending per tract greater than $86,737; outliers are excluded.

Table 3. Regression Estimates for the Relationship between CDBG Expenditures Divided by Poor and Neighborhood Indicators, for a Sample of All Census Tracts with 1994–1996 Average CDBG Expenditures Greater than Mean

	Neighborhood Indicator		
Independent variables	Median Loan Amount	Loan Approval Rate	Number of Businesses
Neighborhood indicator at start of period (1993 to 1994)	0.807 (0.021)**	0.574 (0.037)**	0.958 (0.53E-2)**
CDBG spending/poor in tract (average/year, 1994 to 1996)	0.024 (0.0057)**	0.438E-2 (0.211E-2)**	0.018 (0.56E-2)**
(CDBG spending/poor in tract)**2 (average/year, 1994 to 1996)	–0.835E-5 (0.222E-5)††	–0.726E-6 (0.82E-6)	–0.636E-5 (0.218E-5)††
(CDBG spending/poor in tract)**3 (average/year, 1994 to 1996)	0.517E-9 (0.1452E-9)††	0.364E-10 (0.537E-10)	0.404E-9 (0.143E-9)††
Constant	24.24 (2.30)††	19.81 (2.23)††	–11.43 (1.89)††
Adjusted R^2	0.75	0.37	0.99
Sample N	512	509	502
Dependent variable mean	94.11	56.17	163.21

Note: Standard errors are shown parenthetically.

**$p < 0.01$; one-tailed test.

††$p < 0.01$; two-tailed test.

As we will amplify later, these findings suggest a threshold effect in terms of a critical mass of CDBG expenditures that must be attained before noticeable effects ensue. Unfortunately, our database was not robust enough or sufficiently free of measurement errors to permit a precise identification of this threshold, despite our attempts to do so. Thus, we are more confident in claiming that below roughly $87,000 in annual average expenditure, significant neighborhood payoffs from CDBG are unlikely to be observed, rather than in stating the precise level above this amount at which sizable effects begin to ensue.

Results for different types of CDBG spending

We replicated this analysis, except that we used independent variables measuring four different types of CDBG spending by categories delineated in the HUD databases—property acquisition and clearance, housing, economic development, and public facilities and services. Results (estimated for tracts with above-mean CDBG spending for that category) were intuitively plausible; see table 4, which presents only the statistically significant coefficients of the CDBG spending per poor resident variable.

CDBG spending on economic development was significantly correlated with three indicators, but was the only type that was significantly correlated

Table 4. Summary of Regression Results of CDBG Impacts, by Type of Spending, in Census Tracts with Above-Mean Amounts of CDBG Spending in a Given Type

Category of CDBG Expenditure Average/year 1994 to 1996	N	Change in Neighborhood as Indicated by		
		Median Loan Amount	Loan Approval Percentage	Number of Businesses
Acquisition and clearance	55	NS	NS	NS
Economic development	175	0.04	0.02	0.05
Housing	229	0.05	0.02	NS
Public facilities and services	246	0.11	0.04	NS

Note: Coefficients of CDBG spending divided by poor residents were shown only if statistically significant ($p < 0.05$).

NS = not significant.

with neighborhood employment (not shown) and business formation. Housing-related and public facility and service expenditures were positively correlated with changes in median mortgage loan amount and loan approval rates. Spending on acquisition and clearance was not associated with any indicator in a statistically significant way. Although relatively few tracts in our sample had that kind of expenditure (see table 4), the results may indicate that acquisition and clearance alone do not influence our neighborhood indicators.

Results for different neighborhood contexts

As previously noted and illustrated in figure 1, we expected neighborhood and city conditions to influence the productivity of CDBG investments in generating impacts. To test this, we reestimated the basic regression specification for different categories of neighborhoods and cities. We stratified all census tracts into three sets according to their 1990–1994 trends in single-family home median sales prices, which we believed to be the best overall measure indicating the neighborhood's trajectory before the period under investigation.[6]

Results (again, estimated for above-mean CDBG spending tracts) are summarized in table 5. An *F*-test confirmed that the stratification by neighborhood home price trajectory was statistically justified. Although there is a good deal of imprecision introduced into our stratified results by the reduction in sample sizes, patterns of results that are consistent with our expectations

[6] The paucity of our sample size constrained us to use three groups (approximately terciles), instead of more finely grained strata of home price changes. The apparent break points distinguishing the groups were –21 percent and 8.8 percent.

emerge. For all three outcome indicators, CDBG investments yielded the highest per-dollar payoffs (as evinced by the size of the coefficients) in neighborhoods already experiencing a strong upward trajectory of housing prices. Having noted this, however, table 5 also indicates that CDBG spending nevertheless had positive payoffs (at least above its threshold) in neighborhoods experiencing severe declines in home prices, payoffs that were robust across all three of our neighborhood outcome indicators.

Table 5. Summary of Regression Results of CDBG Impacts, by Neighborhood Type, in Census Tracts with Above-Mean Amounts of CDBG Spending

Neighborhoods Categorized by Home Price Change 1990 to 1994	N	Change in Neighborhood as Indicated by		
		Median Loan Amount	Loan Approval Percentage	Number of Businesses
Over 21% decline	210	0.04	0.01	0.02
–21% to +8.8%	166	NS	0.01	NS
Over 8.8% increase	99	0.09	0.03	0.12

Note: Coefficients of CDBG spending divided by poor residents were shown only if statistically significant ($p < 0.05$).

NS = not significant.

Results for different types of city economic contexts

To investigate the sensitivity of results to simultaneous city economic performance, we used the same three breakdowns presented in table 1 to stratify regression runs according to the amount of job growth in the city between 1994 and 1997.[7] Results (again estimated for above-mean CDBG spending tracts) are summarized in table 6. An *F*-test confirmed that the stratification by city job change was statistically justified. As expected, we found that the per-dollar payoffs from CDBG spending in terms of neighborhood business development proved least in cities experiencing job loss or stagnation, although the coefficients were still significantly positive even in these contexts. (See the last column of table 6.)

Discussion

Spatial targeting of CDBG spending

History suggests that both federal policies and local actions have been inconsistent over time in terms of the degree to which CDBG spending should

[7] We would have preferred to use the 1994–1999 period, but 1999 job data were not available.

Table 6. Summary of Regression Results of CDBG Impacts, by City Economic Context, in Census Tracts with Above-Mean Amounts of CDBG Spending

Neighborhoods Categorized by City's Job Change 1994 to 1997	N	Change in Neighborhood as Indicated by		
		Median Loan Amount	Loan Approval Percentage	Number of Businesses
Decline or no growth	238	0.01	0.01	0.02
Low growth	138	0.11	0.03	0.03
High growth	99	NS	NS	0.04

Note: Coefficients of CDBG spending divided by poor residents were shown only if statistically significant ($p < 0.05$).

NS = not significant.

be, or actually is, spatially targeted. Research on spending patterns during the early years of the CDBG Program indicated a widely dispersed spatial allocation (Dommel and Rich 1987; Dommel et al. 1980; Rich 1993). The 1977 amendments to the CDBG Program, however, encouraged communities "to define areas for strategic investment … where concentration of public resources would produce a demonstrable difference over a 'reasonable' period of time" (Urban Institute 1994, 4–23). The NSA Program was designed to put this targeting principle into effect, but most cities designating NSAs made them too large to be effective (Thomson 2003). In the early 1980s, all federal guidance about targeting fell victim to "local pressures to widely distribute investments across urban neighborhoods" (Walker and Boxall 1996, 25) and was eliminated. Even so, in the early 1990s, neighborhood-oriented (as opposed to communitywide) CDBG spending strategies were used by a majority of cities, and more than 90 percent concentrated at least some of their CDBG spending in particular areas (Urban Institute 1994).

Our research strongly supports the proposition that spatial targeting influences the efficacy of CDBG spending in changing a battery of indicators correlated with a wide range of neighborhood conditions. Put differently, we found that CDBG spending had *no* demonstrable relationship with these neighborhood indicators (changing from 1993–1994 to 1998–1999 averages) if it failed to reach roughly $87,000 annual average expenditure in a neighborhood over the 1994–1996 period. Although our study was unable to identify this threshold more precisely because of data limitations, we urge this as a focus of future work.

Nevertheless, this ambiguity should not obscure the quantitative significance of our estimates of relationships above the threshold. For illustration, consider a stylized census tract with three alternative levels of 1994–1996 annual average CDBG investment: the threshold amount ($86,737), the mean

amount for all tracts above threshold ($244,340), and the highest mean amount for all tracts above threshold in any sampled city ($471,667 for Charlotte, NC). Using the parameters reported in table 2, we calculated how much higher each of our indicators would be at the threshold mean and maximum compared with the threshold. These differences are presented in the last two columns of table 7. We repeat this exercise for three different levels of poverty in the stylized neighborhood: 750, 1,125, and 1,500 poor people.[8] These variations lead to differences in the measured CDBG spending per poor resident and are portrayed in table 7.

The first thing to observe in table 7 is that above the threshold, CDBG spending has a strong association with our battery of neighborhood indicators. Compared with CDBG spending at the threshold level, spending at roughly five times that level over three years in a neighborhood with 750 poor residents is estimated to yield cumulative increases (over the same three years and the subsequent two years) of

1. $12,112 (19 percent higher than the threshold level)[9] in the median dollar amount of mortgages originated
2. 2.26 percentage points (4 percent higher than the threshold level) in the mean rate at which mortgage applications are approved
3. 9.08 (4 percent higher than the threshold level) more businesses located in the neighborhood

The first indicator is particularly dramatic and deserving of further discussion. Not only did our aforementioned work (appendix B; Galster, Hayes, and Johnson 2004) reveal that median amount of mortgage originations correlates well with a wide variety of neighborhood economic, social, and demographic indicators, but it is almost perfectly correlated (0.95) with the median price of homes sold when both are averaged over a two-year period. (See Galster, Hayes, and Johnson 2004 for more details.) So assuming with confidence that changes in our indicator accurately track changes in residential property values, we can model a representative CDBG neighborhood that contains 1,500 residential properties and 1,125 poor people and calculate what our parameter estimates imply. An additional $1,155,000 ($385,000 per year—the difference between minimum and maximum CDBG levels shown in table 7)

[8] With a census tract of average population, these numbers translate into approximately 20, 30, and 40 percent poverty rates.

[9] This is calculated based on the mean of the indicator at the beginning of the period; see table 2.

Table 7. Estimated Marginal Impacts of CDBG Spending above Threshold by Number of Poor in Simulated Neighborhood

	Minimum CDBG[a] $86,737	Mean CDBG[b] $244,340	Maximum CDBG[c] $471,667
Number of poor in the tract: 750			
CDBG spending/poor person	$115.65	$325.79	$628.89
Increase over threshold:			
Mortgage amount		$4,959.30	$12,112.46
Mortgage approval rate		0.92	2.26
Number of businesses		3.72	9.08
Number of poor in the tract: 1,125			
CDBG spending/poor person	$77.10	$217.19	$419.26
Increase over threshold:			
Mortgage amount		$3,306.12	$8,074.98
Mortgage approval rate		0.62	1.51
Number of businesses		2.48	6.06
Number of poor in the tract: 1,500			
CDBG spending/poor person	$57.82	$162.89	$314.44
Increase over threshold:			
Mortgage amount		$2,479.65	$6,056.23
Mortgage approval rate		0.46	1.13
Number of businesses		1.86	4.54

[a] At threshold (mean CDBG spending for all tracts).

[b] Unweighted average (above mean CDBG spending tracts only) across 17 sample cities.

[c] Highest per-tract average annual CDBG spending: Charlotte, NC.

Note: CDBG spending is expressed as a three-year annual average.

invested in such a neighborhood between 1994 and 1996 is associated with an increase in aggregate residential property values from 1993 to 1994 to 1998 to 1999 of $12,112,500. This is obviously no trivial relationship.[10]

The second point to extract from table 7 is that the relationship between levels of CDBG spending and changes in our indicators is contingent on the number of poor people in the neighborhood. Because the most robust econometric specification by far involved adjusting CDBG spending by the number of poor residents, the implication is that the same amount of spending will produce smaller changes in neighborhood outcomes (by the factor $1/\delta$) the greater the number of poor in the neighborhood (by the factor δ). Taking any

[10] It is interesting to note that this 11:1 ratio of property value to community development subsidy approximates a typical capitalization rate at which future rental income from property can be used to compute a current capital valuation. Galster, Tatian, and Wilson (1999) find that cap rates for multifamily properties average 9 percent nationally.

outcome of interest and comparing the 750 and 1,500 poverty segments of table 7, we see that the increase in any neighborhood outcome indicator predicted by raising CDBG spending from its threshold falls by half if the neighborhood in question has twice the number of poor people.

Caveats and directions for future research

Even though our research makes what we believe is a quantum leap in terms of scope and statistical sophistication in the analysis of CDBG effects, we know that it nevertheless requires some qualifying commentary and leaves significant areas for improvement. First, we investigated only an incomplete sphere of potential neighborhood effects. Not all CDBG expenditures, even above threshold amounts, were invested in ways intended to produce a visible neighborhood improvement. For example, investments to the underground infrastructure (water and sewer lines, for example) may be critically important to sustaining urban services to a poor neighborhood, but private investors may not see them. We have no way of distinguishing between these investments and others (say, in urban parks, affordable housing, and commercial strip facades) that might have an obvious and positive effect on investor perceptions. Moreover, our indicators do not constitute exhaustive measures of every conceivable aspect of neighborhood that might be visibly changed by CDBG spending.[11] Clearly, replicating our analysis with a more comprehensive battery of neighborhood indicators should prove instructive.

Second, the quality of CDBG data available for this analysis is not perfect. As noted in appendix A, information on CDBG spending for some years is incomplete or missing entirely for nearly all cities, and our procedures for allocating some CDBG expenditures across neighborhoods, however reasonable, tend to bias measured effects toward zero. The limited time span for which HMDA data were available meant that we could not experiment with alternative structures of lags between CDBG spending and neighborhood changes. We hope that future research will be able to replicate our analysis when additional years of fine-tuning HUD's IDIS CDBG data collection system has yielded a more complete and accurate database.

Third, information that ideally would help us apply more control variables in our analysis did not exist in usable form. We had no measures of other public or private investment that contemporaneously could potentially

[11] Our factor analysis identified six dimensions of neighborhood quality that explained 65 percent of the variance among our collection of neighborhood indicators. These factors were, in turn, measured by indicators that explained only a portion of their variance.

complement CDBG spending in some neighborhoods. Similarly, we lacked pre-1994 measures of CDBG spending and thus could not tell the degree to which investments had been sustained over the long run.[12] Also, measures of supportive or inhibiting neighborhood-, city-, or metropolitan-wide social, economic, and demographic influences on neighborhood conditions have been only partially controlled by our use of preexisting neighborhood home price trends and contemporaneous city job trends. Additional efforts to develop more control variables and operationalize the various contexts in which the payoffs from CDBG spending might vary are warranted.

Fourth, one of the most provocative findings from our work relates to threshold levels of CDBG spending. Due to data limitations, we were able to identify in only the coarsest terms where this threshold occurred—the sample mean level of annual average CDBG spending. Given the crucial policy significance of this parameter, we urge future researchers to validate this notion of a threshold and, if possible, to identify more precisely its value and the degree to which it depends on neighborhood and city context. To do so will require a larger sample of cities and more geographically precise CDBG data than are currently available.

Conclusion

Our empirical investigation of CDBG spending in 17 large cities during the 1990s has revealed several strong statistical associations with a battery of indicators that capture a wide range of neighborhood conditions. The evidence offers support for the claim that CDBG spending has made a considerable difference in the conditions in lower-income neighborhoods, although the magnitude of this impact is heavily contingent.

First, it appears that there is no consistent association between CDBG spending levels and indicators of subsequent neighborhood change unless expenditures (1) are sufficiently spatially targeted that they exceed the threshold (estimated here as the CDBG mean expenditure level per census tract) and (2) are standardized for the number of poor residents.

Second, implied effects vary according to neighborhood trajectories before investment and simultaneous changes in the local economy. Nevertheless, even in the least hospitable contexts—highly concentrated neighborhood poverty, preexisting declines in home values, weak city job growth—our estimates

[12] This omission is not damaging so long as these previous expenditures were on roughly the same scale as the ones we did measure, in which case the relative annual average across neighborhoods is an adequate proxy for spending in earlier years.

suggest that CDBG spending at above-threshold amounts produces significant improvements (both statistically and in practical terms) in multiple measures of neighborhood conditions.

Our research represents the first multisite statistical investigation focusing on the impact of CDBG spending on low-income urban neighborhoods. As such, it may be seen as a complement to case studies that may provide richer, more nuanced portraits of individual neighborhood redevelopment efforts, although with an associated loss of generality (Bright 2000; Keating and Krumholz 1999; Keating, Krumholz, and Star 1996). Our findings thus contribute to the ongoing debate about the degree to which federal place-based policies have noticeable effects, a debate that thus far has been waged as a battle of contrasting case studies (e.g., Grogan and Proscio 2000; Lehmann 1992).

In a broader sense, our research offers new perspectives on the following fundamental questions on the prospects for low-income neighborhoods, especially given new research indicating that it is typically not their destiny to remain poor forever (Galster et al. 2003; Jargowsky 2003).

1. What are reasonable expectations for improvements in poverty-stricken neighborhoods, and what are the dimensions?
2. How much investment from CDBG and other sources is required to produce these improvements, and what circumstances are necessary?
3. Where have neighborhoods and cities performed better than expected, and what can we learn about the strategies and supporting factors that produced this result?

This research only begins to answer these questions, but we hope that it is a promising beginning.

Appendix A

Details of methods for generating the CDBG database

Compiling program data. One of the major challenges with any analysis of the CDBG Program for the years covered by the study is the problem of missing data. Because of the phase-in of the IDIS system starting in 1996, no single system has comprehensive information on CDBG expenditures for 1994 through 2000. Most CDBG grantees went online with IDIS in the 1997 program year, but some communities did not begin until 1998. Gaps in data coverage sometimes appeared with the phase-in, compounding the problems

of missing data that hampered IDIS's predecessor system. (Indeed, 7 of the 17 cities in the analysis did not have complete data for the 1994–1996 period.) Despite supplemental coding of program documents, where available, a number of gaps remained in the final study database. All but two of the sampled grantees (Fort Lauderdale, FL, and Tulsa, OK) lacked some CDBG program data for the period immediately preceding their transition into IDIS, with the gap ranging from 3 to 18 months.

Geolocating CDBG activities. IDIS and HUD's earlier information system provide a range of geographic data at the activity level, but not always in a form that is easily geocoded to census tracts. We extracted all geographic information with a view to determining the census tract location of *all* of a grantee's expenditures. However, our ability to do so varied: The median share of expenditures that could be coded was 70 percent, and two cities had shares below 60 percent.[13] Nevertheless, our inability to locate where some share of CDBG spending occurred in every sample city undoubtedly introduces random measurement error into our data set.

Attributing expenditures to particular tracts. While some CDBG-funded activities occur in a single census tract and therefore can be unambiguously assigned, others are more difficult to attribute because they span tract boundaries. For example, a commercial establishment or community recreation facility might have a service area that encompasses more than one neighborhood, so the benefits may have accrued beyond the tract where the facility was located. Or an area-based CDBG-funded housing rehabilitation program might involve a target area comprising more than one tract. We therefore used the following procedures to assign expenditures to particular tracts. We admit that these procedures are ad hoc, but we cannot think of equally reasonable alternatives.

1. Expenditures coded as area and direct benefit activities that occurred inside a single census tract were assigned entirely to that tract, even if the direct beneficiaries did not necessarily reside in the same tract.

2. Expenditures for area and direct benefit activities that occurred in more than one census tract were prorated equally among tracts, inasmuch as we

[13] In some instances, most notably Portland (OR), geographically targeted expenditures were put into the residual category because there was not enough information in IDIS or earlier HUD databases to determine the census tracts encompassed by the target area(s).

had no other information on which to base an allocation. Prorated expenditures accounted for between 4 and 44 percent (median of 16 percent) of the grantees' total geocoded spending (that is, spending for which a tract location was available).

3. We assigned citywide expenditures that grantees targeted to eligible persons across their jurisdiction in proportion to the expected level of demand in each tract, as indicated by its share of the jurisdiction's 1990 population below the poverty line. This has the effect of adding a constant value to all tracts in a given city's measure of CDBG spending per poor resident.

4. Finally, residual expenditures from activities that lacked sufficient information to determine a census tract were not assigned to particular tracts. Therefore, these expenditures were effectively excluded from the analysis.

The net effect of the above allocation algorithms was to reduce the apparent cross-tract variation in CDBG spending compared with what it actually was. The only algorithm that may have worked counter to this is the first one. We suspect that our algorithms for handling missing data, although reasonable, have the effect of biasing our regression coefficient on CDBG spending toward zero.

Defining type of expenditure. One of the defining characteristics of CDBG is the discretion that local grantees have in deciding which projects derived from a range of eligible activities they should pursue. Because we anticipated that the characteristics of funded activities might have an important bearing on the nature of the program's impact, we grouped CDBG expenditures into four categories of eligible activities—housing, economic development, public facilities and social services, and property acquisition and disposition. These categories were flagged on the IDIS system, and we provided no further modification.

Appendix B

Details of methods for developing neighborhood outcome indicators

Our method of selecting a parsimonious set of indicators to use as dependent variables in this study proceeded in the following steps:

Step 1: Assembling comprehensive data on annual changes in neighborhoods. We first used unique local data sets assembled by the Urban Institute as a part

of the National Neighborhood Indicators Partnership (NNIP) program.[14] The five partners with the most complete information—Boston, Cleveland, Indianapolis, Oakland (CA), and Providence (RI)—were selected. These databases contain annual census tract information that allowed us to create indicators for such factors as violent and property crime, births to teens, low-weight births, welfare and food stamp use, and land use for the mid-1990s.

Next, we assembled annually updated census tract data from three sources that are available for most cities: business directories, property deed and tax offices, and federal regulators of financial institutions.

Business directories. Dun and Bradstreet produces a database that contains information on 10 million business establishments nationwide and can be used to group businesses at the ZIP code level. We drew three indicators from 1995 and 1999 Dun and Bradstreet data at this level: number of jobs, number of firms, and total dollar volume of sales annually. We converted the data to census tract values by approximating from ZIP code geography using the MABLE/Geocorr Geographic Correspondence Engine available from the University of Missouri.

Home price data. Information on parcels of property in a community is maintained by local property tax assessors and auditors to levy taxes on owners. At a national level, there are several commercial sources that gather and sell this information; we purchased ours from DataQuick. Because we encountered census tracts for which no sales were reported in 1994 or 1999, the median sales price of homes sold was calculated from 1993 and 1994 data combined and 1998 and 1999 data combined.

HMDA data. In 1975, Congress enacted HMDA, which requires that depository institutions (banks, savings and loans, thrifts, credit unions, and others) and for-profit, nondepository institutions (mortgage companies) report information on all mortgage applications and originations. Information that lenders must submit includes the type, purpose, and amount of a loan; the location of the property; occupancy; action taken; type of purchaser; reason for denial (optional); and the applicant's race, sex, and income. Although some sorts of institutions are exempt from reporting, HMDA data provide the best portrait of annual mortgage market activity available at the census-tract level.

[14] The NNIP is a collaborative effort by the Urban Institute and local partners to further the development and use of neighborhood-level geographic information systems in local policy making and community building.

Our analysis database contained a tract-level summary of the 1993 and 1994 HMDA reports and a similar summary for 1998 and 1999. To reduce problems associated with few or no observations in a single year, we averaged tract-level data for 1993 and 1994 together, and 1998 and 1999 data together. We calculated over both two-year periods the number of loan applications, the percentage of applications that resulted in originations, the percentage of all mortgage applications intended for home purchase and for home improvement, and the median loan amount originated.

Finally, to supplement the above information, we developed 26 indicators from 1990 census tract data (U.S. Bureau of the Census 1993). They include such factors as female household headship and marriage rates; racial, immigration, and demographic characteristics; incomes and unemployment; education and occupational status; and the ages, vacancy rates, values, and structure types of the housing stock. Even though annual updates of such indicators were not available during the 1990s, we nevertheless thought it important to see how they correlated with those from the other data sources.

Step 2: Ascertaining dimensions of neighborhood conditions. We analyzed this set of indicators for each of our five cities with a principal components analysis using varimax rotation, a common procedure used for social indicators (Ross, Bluestone, and Hines 1979; Wong 2002). For each site, we replicated the analysis for 1994 and 1999 indicators developed from the NNIP, Dun and Bradstreet, DataQuick, and HMDA databases; indicators based on 1990 census data were employed in both cases.

The results showed remarkable cross-sectional and intertemporal comparability, especially considering the wide range of locations, ages, demographics, and economic bases. In each of our five cities, six common clusters of indicators that we believe correspond to intuitively plausible dimensions of neighborhood condition emerged.[15] The most heavily weighted indicators in each factor provide a heuristic suggestion of a label signifying a dimension: social disadvantage, housing type and tenure, prestige, business and employment, crime, and housing vacancy.

The first factor, social disadvantage, heavily weights such indicators as female headship rates, teen birth rates, welfare usage, and percentages of black

[15] These factors had Eigen values greater than unity and explained 3 percent or more of the variance in the data set. Cumulatively, these six factors explained approximately two-thirds of the total variance, differing modestly by up to five percentage points, depending on the city and the date. Details for each of our five cities are available from the first author.

and (negatively) white populations. The second factor, housing type and tenure, consists predominantly of the percentages of structures that are single-family homes and that are owner-occupied. The third factor, prestige, loads heavily on the percentage of those with college degrees and those in managerial, professional, or technical occupations, and median home values. The fourth factor, business and employment, is heavily composed of the number of businesses and the number of jobs, and less so of the volume of sales. The fifth factor, crime, typically involves rates for both property and violent crime. The last factor, housing vacancy, loads heavily on residential vacancy rates in all cities, although in several it also involves the percentage of units lacking some minimal plumbing. For each city, there is remarkable stability in the factor loadings of the indicators between the two years.

Step 3: Ascertaining the parsimonious set of indicators. After we identified six common dimensions of neighborhood conditions, our next task was to ascertain the degree to which any individual indicators based on data sources available for *all* cities could serve as strong proxies for these dimensions. For this part of our investigation, we regressed each factor produced for a particular city and period on each of the indicators based on Dun and Bradstreet, DataQuick, and HMDA information. The resultant R^2 values provide an easily interpretable measure of how well each indicator explains the variation in the six factors.

We found, first, that several HMDA-based indicators proved to be especially strong, consistent predictors of the social disadvantage and prestige factors, and to a lesser degree the housing type and tenure factor and the crime factor. In particular, the mortgage approval rate seemed most robust, predicting the social disadvantage and prestige factors at R^2 values of 0.38 and 0.45, respectively, on average across the sites. The mortgage approval rate was reasonably predictive of the crime factor as well (average R^2 of 0.22). The median dollar amount of mortgages issued proved to be a strong predictor of the prestige factor (average R^2 of 0.74) and social disadvantage factor (average R^2 of 0.28).[16] Second, the Dun and Bradstreet–based indicators of business or jobs were extremely predictive of the business and jobs factor, with R^2

[16] The number of home purchase loan application records is the only generic-based indicator that is even modestly predictive of housing type and tenure (average R^2 of 0.27). The share of mortgages intended for home purchase or the share for home improvements is modestly predictive of the social disadvantage and prestige factors (average R^2 values of 0.22 and 0.28, respectively), but in both cases the explanatory power is less than that provided by the mortgage approval rate indicator.

typically exceeding 0.95. Finally, the DataQuick-based indicator of mean sales price of single-family homes proved to be a good predictor of the social disadvantage and prestige factors, with average R^2 of 0.25 and 0.72, respectively.

Next we identified which of these indicators provided redundant information by correlating them with all others, using all census tracts with available information from our entire sample of five cities. Two pairs were clearly redundant: median loan amount–median home sales price, and number of businesses–number of jobs. Both indicators in each pair were highly correlated in identical fashion in both years, 0.95 for the former and 0.86 for the latter. However, because median home sales prices and number of jobs had slightly less explanatory power for neighborhood dimensions than their correlated counterpart, they were not maintained in the parsimonious indicator set. By contrast, the (HMDA-based) mortgage approval rate and median loan amount originated and the (Dun and Bradstreet–based) number of businesses did not prove to be sufficiently intercorrelated to render any of them redundant.

To summarize, our experiments suggested that three indicators based on widely available data sources offer robust proxies for several broader dimensions of neighborhood and therefore offer reasonable (if not exhaustive) indicators for measuring the impact of CDBG expenditures on neighborhoods. Changes in these indicators serve as alternative dependent variables in the statistical model summarized in equation (1): home purchase mortgage approval rate, median amount of home purchase loans originated, and number of businesses.

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