Examining the Local Economic Impacts of the Community Development Block Grant

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Abstract

This article provides preliminary evidence on the job impacts of the Community Development Block Grant (CDBG) program. The author uses a difference-in-differences (DiD) study design to leverage a one-time shock to the formula allocation process, which permanently reshuffled grant generosity, creating quasi-experimental variation. Job counts increased greatly in localities that received a large positive boost to their allocations but were unchanged in localities where allocations fell. For localities that benefited from the shock, cost-per-job estimates of the CDBG appear promising.

Introduction

The growing geographic concentration of poverty across the country has had widespread impacts on social and economic well-being. In response, governments at every level have increasingly relied on spatially targeted investments—or "place-based policies"—to revitalize local areas in decline. federal efforts such as the Opportunity Zones program and a recent $80 billion surge in place-based industrial initiatives (Muro et al., 2023) aim to attract economic investment to struggling regions. Local governments spend $60 billion annually on job creation efforts, with three-quarters of this investment spent on lucrative (often desperate) firm incentives to attract new employers to ailing places (Bartik, 2020).

Despite the growing stakes and urgent need for economic revitalization, policies such as the Community Development Block Grant (CDBG) program have seemingly faded into the background despite being a cornerstone of federal investment in low-income communities for the past 50 years. With more than $200 billion spent across the program's lifespan, the CDBG continues to provide local governments with a flexible funding mechanism to support a broad range of community development activities aimed at revitalizing neighborhoods, promoting economic development, and improving local living conditions. One potential reason for the CDBG's low salience is that few efforts have been made to rigorously evaluate its causal impacts. Despite the CDBG's historical bipartisan popularity, the lack of data-driven evidence likely hampers support for the program today.

This article presents preliminary causal evidence on the CDBG's economic impacts by examining its effects on local job counts through a natural experiment that introduced a permanent, one-time shock to the CDBG formula allocation process. The author uses a difference-in-differences (DiD) study design to quantify how the trajectory of jobs in local areas changed in response to this shock. This approach suggests that the CDBG increased job counts by an average of 7.2 percent among benefactors of the one-time shock over the subsequent 8 years at an estimated cost-per-job of $21,667. In a working companion article, the author finds similar results when applying more rigorous methods. A wider range of job outcomes are also explored, along with CDBG's impacts on local public finance and how different kinds of CDBG investments vary in job creation effectiveness (Zuo, 2024).

The CDBG offers important insights into effective federal support for declining areas. First, different places have different needs; investments of one kind might be effective in certain places but have limited impacts in other places. The flexibility of the CDBG enables federal dollars to be tailored to local use without pigeonholing funds to specific purposes for which certain places may have little need. Second, the decisionmaking process behind which place-based policies to fund and where to target them is staggeringly open-ended. Although some place-based investments have successfully led to sustained prosperity, many others—at great cost—have done little to generate lasting economic growth. Given this finding, researchers have struggled to provide guidance on what place-based investments local governments ought to pursue. The CDBG funds a broad range of investment activities—many of which are not strictly focused on economic development. Grantees have the discretion to choose from a wide spectrum of eligible activities, including activities related to housing, public services, public facilities, and more. The program's national coverage and broad uses provide strong potential for future research on effective place-based policymaking.

This article fills several important gaps in the existing literature on the CDBG. The author proposes a source of quasi-experimental variation in block grant generosity to causally estimate the CDBG program's impact and focuses explicitly on jobs as a measure of local economic vitality. Other existing studies propose using home values as a key outcome of interest—arguing that home values capitalize on the general appeal of an area and would thus change in response to effective CDBG investments (Galster et al., 2004; Pooley, 2014).

Methods

To estimate the CDBG's impact on local job counts, the author uses a DiD approach, leveraging a one-time change in the data sources used to calculate annual CDBG allocations. This shock caused annual CDBG allocations to suddenly change for many grantees—and never revert. In 2012, the data inputs used to calculate CDBG allocations changed from the 2000 decennial census data to the 2005–2009 American Community Survey (ACS) (Joice, 2012). Before 2012, each grantee's percentage claim of the total CDBG budget remained relatively stable. The transition to the ACS in

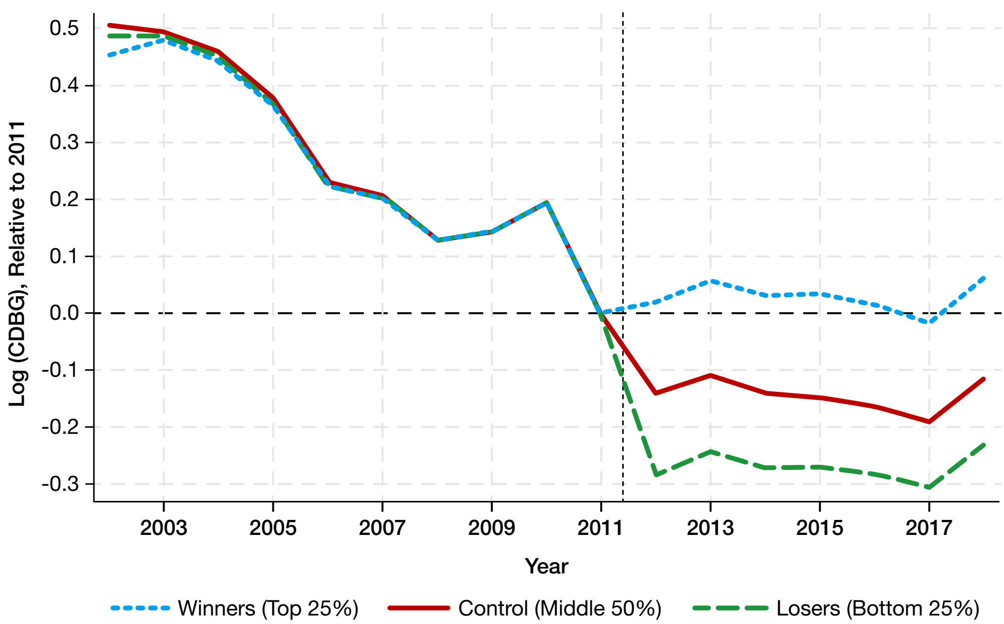
2012 led to a widespread "reshuffling" of grantee CDBG allocations, generating quasi-experimental variation in grant generosity and creating a natural experiment for evaluating the program's impact.

Grantees are categorized into three groups based on this reshuffling: *winners*, representing the top 25 percent of grantees, receiving the largest positive changes; *losers*, who represent the bottom 25 percent, experiencing the largest negative changes; and *control* grantees, representing the middle 50 percent, whose allocations remained relatively stable.

Exhibit 1 illustrates how the funding trajectories of these three groups evolved. Each line represents the natural logarithm of each group's average CDBG allocation, and for visual comparison, each line represents the average allocation size *relative to 2011*. Before the data shock in 2012, all three groups exhibited nearly identical trajectories in their CDBG funding allocations. After the data shock, the three groups diverge substantially in terms of their future funding trajectories.

Exhibit 1

CDBG Funding Trajectories Before and After 2012 Data Shock



Notes: This exhibit presents the trajectory of average CDBG allocations for winners, losers, and control grantees based on the percent change in allocation size before and after the 2012 data shock. Allocation trajectories over time are centered to show how grant allocations compare with their baseline values in 2011.
Source: U.S. Department of Housing and Urban Development Open Data

Exhibit 2 provides summary statistics comparing these three groups of grantees. The socioeconomic characteristics appear similar, but differences appear in terms of racial composition (63 percent White for winners, 54 percent for losers, 61 percent for control), property values (in thousands of dollars, 205 for winners, 305 for losers, 239 for control), and population/jobs (in thousands of people, 235 for winners, 204 for losers, 254 for control).

Exhibit 2

CDBG Grantee Summary Statistics

	Winners		Losers		Control	
	Mean	**SD**	**Mean**	**SD**	**Mean**	**SD**
Socioeconomic Characteristics						
EPOP Ratio	0.462	0.058	0.460	0.048	0.460	0.050
	61,395	16,910	64,418	22,487	60,523	20,402
HH Income	5	0	8	7	3	2
% In Poverty	0.164	0.081	0.152	0.075	0.169	0.079
% College Educated	0.287	0.116	0.291	0.143	0.280	0.124
% HS Grad or Less	0.405	0.116	0.428	0.130	0.426	0.120
% In Professional Occupation	0.343	0.083	0.349	0.111	0.341	0.094
Demographic Characteristics						
% White	0.631	0.215	0.540	0.256	0.609	0.226
% Married	0.492	0.078	0.460	0.072	0.456	0.082
% Single Mother	0.579	0.046	0.583	0.043	0.583	0.039
% Working Age	0.143	0.059	0.145	0.054	0.158	0.066
Neighborhood Characteristics						
Median Rent	878	234	1,033	363	906	330
Median Home Value	205,151	102,069	305,373	191,487	239,363	154,603
% Vacant Housing	0.085	0.041	0.079	0.043	0.088	0.039
Population and Jobs						
	234,903	240,597	203,970	382,535	254,335	603,798
Population	03	97	70	35	35	98
	33,217	49,134	26,670	67,351	41,599	146,236
All Jobs	7	4	0	1	9	36
Grantees	223		223		444	

CDBG = Community Development Block Grant. EPOP = employment-to-population ratio. HH = household. HS = high school. SD = standard deviation.

Note: This exhibit summarizes CDBG grantee characteristics across winning, losing, and control grantees—as defined based on the change in grantee allocation due to the CDBG shock.

Sources: U.S. Department of Housing and Urban Development; U.S. Census

Despite these differences, the study's empirical strategy relies on the three groups exhibiting similar *trends* in job counts—not static differences in *levels*. More specifically, DiD is used to compare outcomes for winners (or losers) against counterfactual control grantees. The sample is restricted to only winners (or losers) and control grantees, and the following regression equation is used for estimation:

$$Y_{it} = \alpha + \beta\,(\text{Winner}_i \times \text{Post}_t) + \phi_i + \theta_t + \epsilon_{it}$$

where Y_{it} denotes the outcome variable (e.g., job counts) for grantee i in year t, $Winner_i$ is a binary indicator for grantees in the group of winners (or, $Loser_i$ when comparing losers with control grantees), $Post_t$ is a binary indicator for the posttreatment period, and ϕ_i and θ_t are grantee and time fixed effects, respectively. Standard errors are clustered at the grantee level to account for potential serial correlation within grantees over time. Additional control variables were not included due to frequency and geographic irregularity of the data. To date, no publicly available data source compiles annual covariates at the grantee level (e.g., a mix of cities and urban counties) from

2002 through 2019. Other grantee characteristics that do not change over time are controlled for through the grantee fixed effect ϕ_i.

The DiD coefficient of interest, β, measures the causal impact of the CDBG shock on winners relative to the control group. A similar regression, including losers and control grantees, would estimate the causal impact of "losing" from the CDBG shock. As exhibit 1 indicates, winners and losers experienced a permanent 15- to 20-percent shock to their allocations in opposite directions.

The validity of the DiD approach hinges on the assumption that in the absence of treatment, the treatment group (either winners or losers) and the control group would have followed parallel paths over time. This assumption is inherently unverifiable because one cannot observe how treated grantees would have responded if the data shock had not occurred. The "Results" section below explains two simple checks that provide evidence in support of this assumption.

A major concern with this approach is that the data shock might reflect endogenous changes in local labor market conditions between 2000 and 2009. For instance, winners might be disproportionately composed of grantees experiencing declining labor markets, leading to an increase in poverty counts between the 2000 census and the 2005–2009 ACS. Thus, the division of grantees into three groups could potentially incorporate the actual changes that occurred between 2000 and 2009, complicating the argument that the "treatment" was entirely exogenous.

Several factors mitigate these concerns. First, the data update includes outdated information. The shock occurred in 2012, but the update reflects changes in poverty, overcrowding, and pre-1940 housing from 2000 to 2005–2009 and population changes from 2009 to 2010. Second, much of the variation from the update likely stems from measurement changes rather than actual labor conditions. Serrato and Wingender (2016) argue that the shift from the 2009 population estimate to the 2010 full count was largely due to measurement error. In the CDBG data shock, housing units built before 1940 increased by 8 percent, which should be impossible and points to measurement differences between the census and ACS being the driving factor. Overcrowded housing counts fell by 46 percent despite population growth outpacing housing starts, especially during the Great Recession, when housing construction rates halved (USAFacts, 2021).

Joice (2012) outlines key measurement differences between the decennial census and ACS. The census occurs every 10 years on April 1, whereas the ACS averages data collected continuously over 5 years, capturing seasonal differences the census misses. The ACS also changed the census' "residence rule," affecting areas with many seasonal residents. The smaller ACS sample has more sampling error than the larger census long-form sample. Lastly, the census used mostly mail-in responses, whereas the ACS relied more on phone and in-person interviews, reducing respondent confusion about room counts and building age (Woodward, Wilson, and Chesnut, 2007).

Two simple empirical checks assessed this potential threat. First, after 2012, CDBG inputs were updated annually with each ACS iteration. Changes in CDBG inputs from 2012 to 2019, which reflect how local conditions changed and did *not* involve measurement changes, do not produce any notable reshuffling in exhibit 1. A lack of movement suggests that the data shock did not primarily reflect endogenous labor market changes. Second, the author directly observed how pre-2012 job trajectories differed across the three groups. Exhibits 2 and 3, which are described in further detail below, indicate that the three groups were on highly similar job trajectories before 2012, mitigating potential concerns that the results in this study were driven by the composition of the different groups. Although the DiD estimate provides estimates for how the CDBG affected jobs, determining cost effectiveness further necessitates an examination of how much public spending was required to achieve those effects. A naïve approach would be to approximate the amount of CDBG funding associated with the 2012 data shock. However, this estimate would not reflect the total change in local public spending that occurred due to the shock. The CDBG—like other block grants—is commonly used as "seed money" to attract other sources of funding (Theodos, Stacey, and Ho, 2017); thus, a sudden increase in CDBG funding could potentially generate additional spending multipliers that must be explained. To estimate these potential multipliers, the author conducts the same DiD analysis using local public spending on housing and community development as an outcome variable. Using this estimate, the author approximates the total public spending that was induced by the 2012 data shock to use as the denominator in cost-per-job calculations.

Data

Job Counts: To calculate the number of jobs associated with each grantee, the author uses data from the Longitudinal Employer-Household Dynamics (LEHD) Origin-Destination Employment Statistics (LODES), a public dataset administered by the LEHD program at the U.S. Census Bureau. The LODES data provide worker counts at the census block level, although these counts are typically infused with a small amount of noise to protect anonymity. All census block counts were aggregated to the geographic boundaries of their respective grantees. For most states, data extend from 2002 through 2019.

CDBG Allocations: To identify winners, losers, and control grantees, publicly available data on grantee-specific allocations since 1975 were used. Using the Consumer Price Index, nominal allocations were adjusted to real dollars. Only the grantees who were continuously eligible for the CDBG from 2002 to 2019 were included, encompassing roughly 80 percent of all grantees in the data. Jurisdictions qualify for CDBG inclusion by crossing a population threshold; therefore, the 20 percent of excluded grantees are small counties and cities that crossed the population threshold (50,000 for cities; 250,000 for counties) at some point during this timeframe.

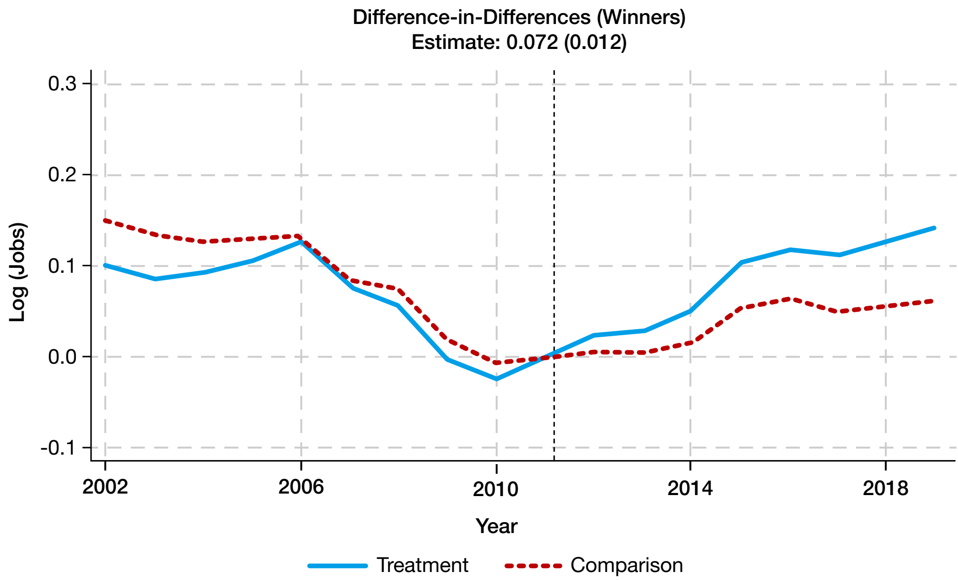
City/county-level public spending data: Local public spending was analyzed using Annual Survey of Local Government Finances data, which are detailed annual data on state and local public finances. Because these data come from surveys of local government administrators, their accuracy is less reliable than administrative data. The survey includes detailed revenue and expenditure categories for each government unit. The author focuses on "Community Development and Housing" spending, which aligns with CDBG uses. Given the complexity of cleaning and standardizing these data, a cleaned version available via Pierson, Hand, and Thompson (2023) was accessed. One noteworthy issue with these data is that some grantees report missing or zero spending on housing and community development—an impossibility if the jurisdiction is a CDBG grantee. This finding suggests that some survey respondents may be either misreporting or miscategorizing the spending. Given this realization, only grantees with five or fewer missing values throughout the study period were kept; the missing values were linearly interpolated when possible.

Results

Exhibit 3 shows the trajectory of job counts (in natural logarithm form) between winning and control grantees relative to 2011 values. Before the data shock, the two groups trended similarly, especially between 2006 and 2011. Shortly after the data shock, the two groups diverge gradually over time. The widening jobs gap is consistent with the accumulating funding gap caused by the data shock. The difference-in-differences estimate indicates that winning grantees experienced a large and significant 7.2-percentage-point increase in job counts relative to comparison grantees.[1] This estimate is significant at the 1-percent level.

Exhibit 3

The Impact of the CDBG Shock on Jobs (winners vs. control)



CDBG = Community Development Block Grant.

Notes: This exhibit presents difference-in-differences (DiD) estimates depicting the impact of the CDBG data shock on job counts, restricting the sample of grantees to winners and control grantees. The DiD estimate and standard error are presented in the graph subtitle. The y-axis denotes job counts relative to baseline levels in 2011. N=12,006, based on 18 years of data and 667 grantees (223 winners and 444 controls).

Sources: U.S. Census Longitudinal Employer-Household Dynamics; U.S. Department of Housing and Urban Development Open Data

As an additional test for the robustness of the DiD approach, a "placebo-in-time" test was conducted. This test introduces a "fake" event before the actual event to probe whether the baseline estimates in this study can be reproduced in a smaller placebo study—providing evidence as to whether the baseline estimates can be attributed specifically to the data shock. Data from 2002 to 2011 were used, and placebo treatment dates in 2007–09 and 2010 were assigned. In exhibit 4, the baseline estimates are presented in the leftmost column. No evidence that any of the placebo studies yield large or significant effects was found, supporting a causal interpretation of the baseline findings in this study.

[1] This estimate represents the average effect over 8 years after the data shock.

Exhibit 4

Placebo-in-Time Estimates

	(1)	(2)	(3)	(4)	(5)
DiD Estimate	0.072** (0.012)	0.020 (0.011)	0.014 (0.010)	0.014 (0.010)	0.017 (0.010)
Specification	Standard	Placebo	Placebo	Placebo	Placebo
Timeframe	2002–2019	2002–2011	2002–2011	2002–2011	2002–2011
Event Year	2012	2007	2008	2009	2010
N	12,006	6,670	6,670	6,670	6,670

DiD = difference-in-differences.

$^*p < 0.05$. $^{**}p < 0.01$.

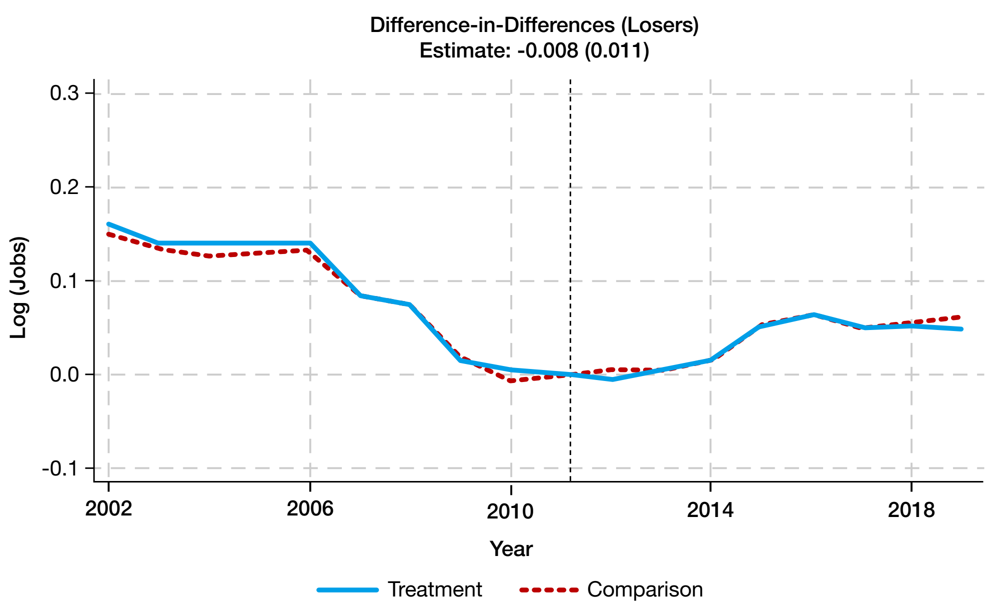
Notes: This table presents results from a placebo-in-time robustness check. Column (1) presents the baseline difference-in-differences estimate. Columns (2) through (5) present placebo estimates using only pretreatment data ranging from 2002 to 2011. Each placebo estimate is obtained by assigning the placebo treatment date noted in the bottom panel.

Sources: U.S. Census Longitudinal Employer-Household Dynamics; U.S. Department of Housing and Urban Development Open Data

Exhibit 5 shows that losing grantees experienced virtually no impact on job counts, with an insignificant difference-in-differences estimate of -0.8 percentage point. This result suggests that the funding shock potentially led to asymmetric outcomes between winners and losers. This finding potentially suggests that grantees who lost funding due to the data shock were largely able to reallocate spending away from low-impact activities to mitigate the economic impacts of the negative shock.

Exhibit 5

The Impact of the CDBG Shock on Jobs (losers vs. control)



CDBG = Community Development Block Grant.

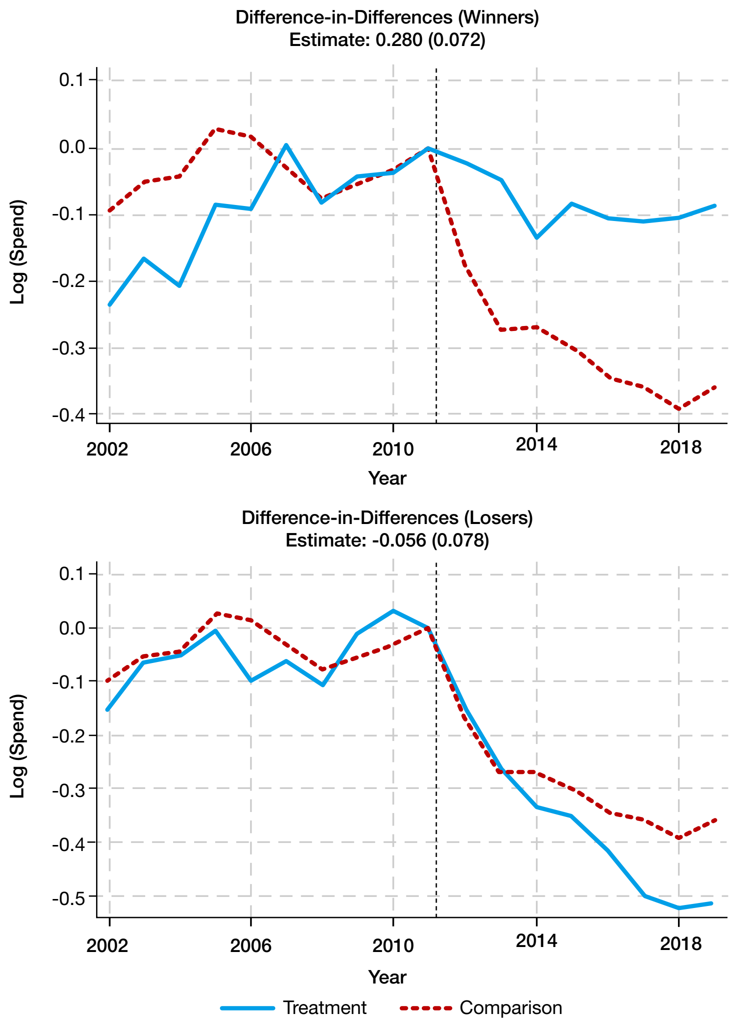
Notes: This exhibit presents difference-in-differences (DiD) estimates depicting the impact of the CDBG data shock on job counts, restricting the sample of grantees to losers and control grantees. The DiD estimate and standard error are presented in the graph subtitle. The y-axis denotes job counts relative to baseline levels in 2011. N=12,006, based on 18 years of data and 667 grantees (223 losers and 444 controls).

Sources: U.S. Census Longitudinal Employer-Household Dynamics; U.S. Department of Housing and Urban Development Open Data

How much extra public spending did the CDBG shock generate? Exhibit 6 illustrates the DiD estimate using local public spending on housing and community development from the Annual Survey of Local Government Finances as the outcome of interest. Despite data quality caveats, evidence suggests that winners increased their total local spending on housing and community development by 28 percent over the course of the post-period relative to control grantees. Before 2007, job counts between the two groups appear on different trajectories. After 2007, the two groups begin to move in lockstep, a promising development in support of the parallel trends' assumption.

Exhibit 6

Impacts on Local Public Spending on Housing and Community Development



CDBG = Community Development Block Grant.

Notes: This exhibit presents difference-in-differences (DiD) estimates depicting the impact of the CDBG data shock on total local public spending on housing and community development, as measured by the Annual Survey of Government Finances. DiD estimates and standard errors are presented in each panel's subtitle. The y-axis denotes total spending relative to baseline levels in 2011.

Sources: U.S. Census Longitudinal Employer-Household Dynamics; U.S. Department of Housing and Urban Development Open Data

The same exhibit indicates that losers experienced a small decline (though statistically insignificant) in local public spending. Both of these public spending results stand consistent with the asymmetric job results presented in exhibits 3 and 5. This result also hints that despite the negative funding shock, losing grantees ultimately spend similar amounts to control grantees on housing and community development, potentially through reducing spending on activities other than community development (such as administration and staffing costs).

To calculate cost-per-job, the median number of jobs among CDBG grantees before the CDBG shock (11,853) was determined then multiplied by the average percentage difference in jobs between winning and control grantees at the end of the sample period (an 8.1-percent gap) to determine the total net jobs created for the typical grantee after 8 years (960). To calculate the denominator, the median pretreatment public spending on community and housing development ($6.6 million in 2011 dollars; $9.2 million in 2024 dollars) is multiplied by the DiD estimate (28 percent) to obtain $2.6 million in induced public spending from the CDBG shock. Over 8 years, the accumulated difference in public spending amounts to 20.8 million real dollars. Therefore, the CDBG shock led to a cost-per-job estimate of approximately $21,667 per job for winners.

Discussion

How do the CDBG's job impacts compare with those of other programs? Bartik (2020) summarizes cost-per-job calculations for a variety of place-based policies, including firm incentive policies ($196,000 per job created), customized job training ($15,000), the Tennessee Valley Authority ($77,000), cleanup of contaminated industrial sites ($13,000), and customized services to businesses ($34,000). Whereas the CDBG does not rank at the very top of this list, its effectiveness is notable given that it is not explicitly a jobs program. Although some funds are used directly for economic development, the program frequently supports housing, infrastructure, public improvements, and public services. These flexible uses could potentially synergize with other existing economic development efforts by supporting population and business growth. The CDBG appears vastly more effective than firm incentives to attract employers to local areas—incentives that outpace CDBG spending by fifteenfold each year.

The CDBG stands out among federal programs due to its flexible and decentralized approach, allowing local governments to tailor their investments on the basis of specific community needs. Over its lifetime, the CDBG has allocated more than $200 billion to support place-based investments in low-income neighborhoods across the United States. The program's structure combines the scale and reach of federal funding with the adaptability of local decisionmaking, embodying the ideal principles of fiscal federalism. This flexibility has enabled municipalities to effectively address diverse local challenges, from housing and infrastructure improvements to public services and economic development projects. By contrast, other federal programs have been criticized for funneling federal dollars into one-size-fits-all programs with mixed benefits across widely different municipalities (Austin, Glaeser, and Summers, 2018).

The CDBG also appears to have high potential for generating public spending multipliers. Funds from the block grant frequently attract other sources of public and private funding (Theodos, Stacy, and Ho, 2017). The findings of this study align with a previous evaluation of the federal

Empowerment Zone program in the 1990s, which found that each dollar from a federal block grant generated an additional $7 of external funding (Busso, Gregory, and Kline, 2013). Further enhancing its potential for generating spending multipliers is the Section 108 Loan Guarantee program, which allows grantees to leverage their annual CDBG allocations to secure below-market-rate federal loans up to five times the size of the original allocation (Prunella, Theodos, and Thackeray, 2014). The CDBG therefore provides a source of liquidity and leverage, further allowing winners to “crowd in” large amounts of additional spending from a positive funding shock.

In a working companion article (Zuo, 2024), similar cost-per-job estimates are found using more state-of-the-art causal methods ($25,042 per job). The author also further contextualizes the magnitude of these findings and explores mechanisms behind the asymmetric effects for winners and losers. In one analysis, itemized CDBG spending data were used to assess how specific categories of CDBG expenditures responded to the shock. The article shows that for winners, CDBG expenditures increased across a wide variety of potentially high-impact spending categories. For losers, spending remained unaffected across most of these categories except for a notable decline in spending on public services—which are intended to encompass a wide variety of health and social programs but appear primarily used to fund miscellaneous activities labeled as “other public services.” Together, these facts suggest that losers were potentially able to absorb the negative funding shock by reducing spending on marginal public services, providing a potential explanation for the CDBG’s asymmetric job impacts.

Conclusion

This article sheds light on how block grants can be used to bridge the scale of federal programs with the diverse, individual needs of localities across the nation. The author finds that in jurisdictions that received a positive shock to their CDBG allocations, new jobs were created at a moderately low cost of $21,667 per job. Jobs were surprisingly unaffected in jurisdictions that experienced negative shocks to their allocations; the author finds evidence suggesting that affected jurisdictions were able to trim spending on marginal public services to offset this decrease in grant generosity. Because jurisdictions possess deep knowledge of their local labor markets, and because they face strong incentives to enhance economic opportunities and local living quality, a fiscal federalism-based approach to place-based policymaking appears to generate outsized job impacts. In addition to its flexibility, the CDBG provides local governments with powerful tools for coordinating other funding sources, unlocking investments that might otherwise have been difficult to achieve.

These findings relate to one important aspect of CDBG—its role as a form of de facto revenue sharing. Regardless of how they used the funds, grantees experienced an increase in jobs when they received a positive shock to their CDBG grant amount, indicating a general positive effect of federal spending in distressed communities. More work is needed, though, to better understand the relative effectiveness of different place-based investments supported by CDBG. One could assess impacts in specific neighborhoods that have received substantial CDBG investments and perhaps even determine the relative effectiveness of different types of CDBG-funded activities. This topic remains an important area for further research.

Authors

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