Getting Beneath the Hood of Effective Place-Based Investments: Job Impacts of the Community Development Block Grant[*]

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Abstract

I study the causal job impacts of the Community Development Block Grant, a federal program providing $3-4 billion in annual block grants for urban cities and counties to flexibly spend on low-income neighborhood development. First, I estimate the job impacts of a plausibly exogenous shock to the allocation process, which created a 20% gap in allocation generosity between "winning" and "losing" grantees. Using a synthetic difference-in-differences (SDID) framework, I estimate that job counts increased by 3.4% in winning localities relative to losing localities. Second, I estimate local public spending multipliers by exploiting the CDBG funding formula to construct a simulated instrument for block grant generosity. I find that each dollar of federal CDBG funding attracts roughly two dollars of additional public spending by local governments, suggesting that total public spending per job created is higher than my SDID estimates imply. I conclude by studying nearly 500 large-scale CDBG investments to uncover the comparative effectiveness of different policies in different places. Among low income neighborhoods eligible for the CDBG, job impacts are greater in comparatively affluent areas.

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1   Introduction

America is in the midst of a "Great Divergence" characterized by large and growing spatial disparities (Moretti, 2012). In many places, decades of stagnating or declining employment have led to the emergence of serious social problems—drug and alcohol abuse, disability, incarceration, and early mortality (Austin, Glaeser and Summers, 2018). The geographic distribution of economic decline has also become the backdrop to deep-seated political divisions across the country (Autor et al., 2017). In hopes of bridging these geographic disparities and bringing jobs to economically distressed places, federal, state, and local governments have increasingly relied on spatially-targeted place-based policies—spending $60 billion annually to bring jobs to specific places. The federal government's $1.2 trillion Infrastructure Investment and Jobs Act provided an enormous influx of funding for local economic development in the wake of the COVID-19 pandemic. Given these stakes, the playbook on effective place-based policymaking still remains underdeveloped. Decisions of what policies to fund and where to fund them are staggeringly open-ended. While some place-based investments have successfully reversed the fortunes of distressed places, many others—at great cost—have done little to promote lasting economic development. Efforts to study what makes certain place-based investments more effective than others are hindered by the fact that the same policy may have different impacts in different places.

This paper presents new insights on effective place-based policymaking via the Community Development Block Grant (CDBG), a federal program which has funded thousands of place-based investments in low-income neighborhoods across the country. While the CDBG's $3-4 billion in annual funding comes from the federal government, the program decentralizes the actual policymaking process to local governments through annual block grants—lump sum funds which local governments are entitled to and can be flexibly used to invest in low-income neighborhoods. The CDBG approaches federal place-based policymaking through the lens of fiscal federalism, combining the scale and reach of federal programs with the benefits (and potential pitfalls) of decentralizing decision-making to local governments, allowing federal funds to be tailored to local needs. The breadth of the CDBG also presents a unique opportunity to analyze and compare a variety of place-based policies—infrastructure, small business assistance, land acquisition and clearance, building/housing construction, and more—all across the country, and all within the same empirical and administrative framework.

I begin by estimating the causal impact of CDBG funding on local job counts, which I obtain

from Census LEHD Origin-Destination Employment Statistics (LODES) data. To do so, I exploit a sudden change in the data inputs used to calculate formula allocations which generated a persistent 20% swing in allocation between "winners" (treatment group) and "losers" (comparison group) from the shock. I argue that the data update, which occurred in 2012 and involved switching from the 2000 Decennial Census to the 2005-2009 American Community Survey, largely reflected changes in measurement error. Furthermore, the endogenous changes in labor market conditions captured by this update were already outdated: the update occurred in 2012 but reflected changes in the labor market from 2000 to 2009.

I use a synthetic difference-in-differences framework (Arkhangelsky et al., 2021) to show that in the decade prior to 2012, job counts between the treatment group and a weighted synthetic comparison group are nearly exactly parallel. This implicitly provides a test for whether the changes from 2000 to 2009 captured by the data update were actually endogenous. Job counts begin diverging after the data shock and imply a positive and significant 3.4 percent increase in jobs over the course of eight years post-treatment. Estimates from standard differences-in-differences are slightly larger and are also highly significant. I document small but insignificant differences showing that effect sizes are larger for low-income residents and places. I also find that job impacts appear driven by a disproportionate increase in low-wage jobs paying less than $1,250 per month.

The cost-per-job implied by these estimates for the median grantee is only $4,346, which is almost certainly an underestimate of the actual local public spending that generated these impacts. For reference, some of the most cost-effective place-based policies clock in at $13,000 per job created (Bartik, 2020b). The CDBG is not an enormous program at face value: the median grantee with roughly 100,000 residents receives approximately $1.15 million per year from the program. However, funds from the block grant are frequently used to attract other sources of public and private funding (Theodos, Stacy and Ho, 2017). Previous work from an evaluation of the Empowerment Zone program documented that each dollar from the federal block grant portion of the program was qualitatively linked to an additional $7 of external funding (Busso, Gregory and Kline, 2013). The CDBG also boasts a built-in mechanism known as the Section 108 Loan Guarantee program, providing grantees with federally-guaranteed below-market rate loans up to five times the size of annual CDBG allocations, using future CDBG allocations as collateral for the loans (Prunella, Theodos and Thackeray, 2014).

Given this, I next estimate local public spending multipliers from the CDBG. To estimate the causal impact of federal CDBG funding on local public spending, I exploit the structure of the CDBG fund-

ing formula to construct a simulated instrument for endogenous CDBG allocations. I measure public spending at the local government level with data from the Annual Survey of Local Government Finances. My findings suggest that every dollar of CDBG funding leads to an increase of $3.16 in public spending on community development and housing. A fiscal multiplier of this size suggests that the CDBG reduces frictions preventing local governments from investing optimally in local neighborhoods. Qualitative and anecdotal evidence support the notion that the CDBG specifically helps local governments to overcome liquidity constraints and coordination failures when conducting large-scale economic development projects (Prunella, Theodos and Thackeray, 2014). This finding suggests that the total public spending per job created is $13,733. This estimate omits any private investment that may have been attracted through the CDBG.

Finally, I attempt to uncover the types of place-based investments (e.g., infrastructure, small business assistance, etc.) which generate the largest job impacts, and the characteristics of places where job impacts are likely to be largest. The CDBG funds a wide variety of economic development activities and provides a unique setting to conduct a comparative analysis of different kinds of policies across the country. I use geocoded data on where and when CDBG investments were made and estimate tract-specific causal effects of nearly 500 large CDBG investments. To estimate tract-specific casual effects, I constructing a synthetic counterfactual tract for each treated tract and estimate SDID separately for each of these tracts. I then calculate the correlation between the resulting tract-specific job impacts and various characteristics of investments and places.

I find that the nearly 500 treatments in my sample increased tract-level jobs by 5.1% on average. Among the various investments in economic development allowed by the CDBG, land clearance and investments in microenterprises (businesses employing less than five employees) appear to have a stronger positive correlation with treatment effects. Notably, the CDBG does not fund financial incentives to attract firms to specific places, which have a mixed record of success and cost-effectiveness (Neumark and Kolko, 2010; Bartik, 2020a). The national scope of the CDBG also provides a useful setting to study characteristics of locations where place-based policies are likely to be effective. Among low-income tracts targeted by the CDBG, job impacts generally appear larger when investments are made in *less-disadvantaged* neighborhoods. The strongest correlation was for tracts trending positively in terms of job counts prior to treatment. Similar patterns emerge when assessing correlations with commuting zone-level characteristics. Overall, these results suggest that it is more difficult to turn around distressed places than it is to create jobs in comparatively prosperous places. These results

stand in contrast to previous predictions which argued that place-based policies would have larger impacts in distressed places.

This is the first paper to rigorously quantify the job impacts of the Community Development Grant. More broadly, this paper contributes in several ways to a large and growing literature on place-based policies which I describe in further detail in Section 2.2. Many empirical studies in this literature have focused on evaluating the impacts of particular programs in specific places. This approach to studying place-based policies faces two key problems. The first involves external validity: similar policies may have different effects in different places. A related problem is that large-scale policies—policies which have produced some of the most convincing evidence available—cannot feasibly be replicated in other settings. In short, data remains limited on effective place-based policymaking. Neumark and Simpson (2015) in their handbook on place-based policies assert that "to guide policy, we need to know more about what works, why it works, and, crucially for place-based policy, where it works and for whom it works." A recent study by Slattery and Zidar (2020) addresses these issues by estimating the job impacts of roughly 550 place-based firm incentive deals. This paper is closely related to recent work by Slattery and Zidar (2020), which estimates the job impacts of roughly 550 place-based firm incentive deals. This paper complements this work by studying a wide variety of other place-based policies which have

2 Place-Based Policies and the CDBG

2.1 Overview of the CDBG

The CDBG was created by the Housing and Community Development Act of 1974 to consolidate many existing programs for community development activities. Whereas each of these individual programs only provided funds for a narrow set of uses, the consolidated CDBG took provided local governments with annual block grants that could be flexibly used for a wide variety of community development activities. The CDBG's decentralized approach toward increasing federal investment in local communities received bipartisan support for decades after the program's inception; however, the program has recently come under scrutiny for the lack of evidence on its benefits in addition to allegations of waste, abuse, and fraud (Theodos, Stacy and Ho, 2017). The Trump administration proposed outright elimination of the program on multiple occasions.

Each fiscal year, the CDBG funding cycle begins with the congressional appropriations process to determine the program's annual budget. Figure 1 plots the size of the CDBG budget over time in both

nominal and real terms. While funding for the CDBG has been nominally stable between three and four billion dollars over the past three decades, program funding has declined substantially in real terms. After the program budget is finalized, the Department of Housing and Urban Development (HUD) allocates CDBG funds to grantees known as "entitlement communities". Each entitlement community represents a city or county government with a minimum population of 50,000 or 200,000, respectively.[1] The amount that each grantee is entitled to is determined by an allocation formula that is a function of population, poverty, and housing variables derived from Census data. I discuss the formula in greater detail in Section 3. Figure 2 shows that allocated amounts vary enormously across grantees and are roughly log-normally distributed. New York City receives approximately $150M each year whereas smaller, more affluent localities can receive as little as $60,000. On a per-capita basis, Cleveland receives approximately $60 per person, whereas Bowie City (a suburb in Maryland) receives under $3 per person.[2]

Grantees enjoy a large degree of discretion in terms of eligible activities to fund. All funded activities must meet one of three national objectives: 1) principally benefiting low- and moderate-income persons, 2) eliminating or preventing slum and blight conditions, or 3) meeting other urgent needs (such as natural disasters). At least 70 percent of a grantee's allocated CDBG funds must be spent toward the first objective, known as the low- and moderate-income (LMI) objective. Before funding is approved for an LMI project, grantees must quantitatively verify that least 51 percent of residents within the project's service area qualify as LMI—earning less than 80 percent of the Area Median Income (AMI).[3] Eligible activities are summarized in Table 1, which describes six of the largest activity categories and summarizes the 836,633 activities that were funded between 2000 to 2016.[4]

2.2 Place-Based Jobs Policies and the CDBG

Place-based policies encompass a broad range of spatially targeted public investments that aim to reduce poverty and improve local living conditions. On paper, the merits of place-based policies as a poverty reduction strategy are mixed (Austin, Glaeser and Summers, 2018). Efforts to improve places

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[1] If an eligible city is nested within an eligible county, separate grants are allocated to both the city government and the county government. However, the county funds can only be used in areas outside of the city's entitlement community boundaries.

[2] In 2017, the mean allocation was approximately $1.7M with a standard deviation of $5.6M; the median was approximately $800,000. The mean per-capita allocation was approximately $11 with a standard deviation of $8; the median was approximately $8.

[3] AMI is calculated separately for each metropolitan area or non-metropolitan county in the ACS. The service area calculation is automated via an internal database which calculates geographic LMI concentrations using the American Community Survey. An activity's service area is delineated at the census tract level.

[4] I note that what constitutes an "economic development activity" is broadly defined—many economic development activities are likely classified under other categories.

directly may distort optimal migration decisions and incentivize workers to stay in unproductive areas. Many costly place-based policies merely re-allocate jobs across space without generating any net increase in labor demand. Even policies that ultimately succeed in creating jobs may lead to gentrification and higher housing prices, to the detriment of lower-income residents.

Skepticism surrounding place-based policies has abated over time as permanent spatial differences in income, poverty, joblessness, and life expectancy have taken root across the country (Moretti, 2011). Inter- and intra-county migration has slowed dramatically, especially for low-wage workers facing a declining earnings premium for moving to high-productivity places (Austin, Glaeser and Summers, 2018; Ganong and Shoag, 2017; Autor, 2019). Low-income residents face disproportionate financial, information, and psychic barriers that impede "moving to opportunity" (Bergman et al., 2019). The role of place-based policies in standard spatial models has also been revisited to consider spatial market failures (such as involuntary unemployment), externalities (such as agglomeration), insurance against location-specific economic shocks, and equity gains in places with disproportionate concentrations of disadvantaged residents.[5,6]

Empirically, evidence on place-based jobs policies is not only mixed, but evaluations of such policies are inherently place- and context-specific.[7] Even the well-documented successes of federal programs such as the Empowerment Zones program (Busso, Gregory and Kline, 2013) and the Tennessee Valley Authority (Kline and Moretti, 2013) come with the caveat that they involved enormous investments in specific settings that are unlikely to be replicated. These empirical challenges leave many practical first-order questions about place-based policymaking unsatisfactorily answered.

The scope of the CDBG provides a unique opportunity to explore 1) how the federal government can support place-based policymaking on a national scale, and 2) what features of place-based investments are most likely to be effective at stimulating economic development. While previous efforts have been made to compare policies across different studies, comparisons are difficult when evaluations differ substantially across methods, locations, and time periods. The CDBG has funded thousands of place-based investments across the country, making it possible to systematically analyze

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[5]Neumark and Simpson (2015) and Austin, Glaeser and Summers (2018) provide excellent reviews of these arguments.

[6]For example, a recent working paper by Gaubert, Kline and Yagan (2020) argues that if mobility and earnings responses are small, the equity gains from place-based redistribution can exceed the efficiency costs of redistribution, making place-based redistribution a useful complement to traditional income-based redistribution.

[7]Three categories of federal place-based jobs policies that have received the most attention involve infrastructure investment, military spending, and place-based incentives to attract firms and investments. Examples of evaluations on each of these categories include: infrastructure (Kline and Moretti, 2013; Glaeser and Gottlieb, 2009; Garin, 2019), military spending (?Nakamura and Steinsson, 2014), and place-based firm incentives (Busso, Gregory and Kline, 2013; Neumark and Kolko, 2010; Chen, Glaeser and Wessel, 2022)

and compare the cost-effectiveness of different policies within a common administrative and empirical framework. The national scope of the program also makes it possible to study *where* place-based investments tend to produce the largest job impacts. Slattery and Zidar (2020) take a similar approach by studying roughly 500 different firm incentive deals to uncover what kinds of industries and places are more likely to receive these deals. Bartik (2015) and Austin, Glaeser and Summers (2018) use Bartik industry shocks to show that labor demand shocks tend to increase employment more in regions with higher baseline rates of nonemployment, concluding that place-based policies will likely have the largest impacts in the most distressed locations. Beyond this prediction, little is known about what other attributes of places influence policy effectiveness. Designing place-based policies at the federal level is further complicated by the widely diverse needs of places across the country. Austin, Glaeser and Summers (2018) argue that "the norm in U.S. politics is that national [place-based] policies need to be uniform, even when local heterogeneity argues strongly against such uniformity".

3   The Impact of CDBG Funding on Job Counts

3.1   Empirical Strategy

I first estimate the impact of CDBG funding on job counts in eligible counties and cities ("grantees"). To do so, I exploit a sudden change in the data inputs used to calculate annual grant allocations. This generated a persistent 20% swing in allocation generosity between "winners" (treatment group) and "losers" (comparison group) from the shock. I compare job counts between winning and losing grantees using synthetic difference-in-differences (SDID) (Arkhangelsky et al., 2021), assigning weights to grantees in the comparison group to align the trajectory of pre-treatment jobs as closely as possible to that of the treatment group. The SDID framework also weights pre-treatment time periods: periods in which job counts are more similar to post-treatment job counts are considered better comparison periods and are given additional weight. The causal impact of CDBG funding is the difference between winners and losers, scaled by the difference in CDBG funding attributed to the data shock.

3.1.1   The CDBG Funding Shock

I begin by describing the shock, which revolves around the CDBG funding formula. Each fiscal year t, Congress decides on the size of the CDBG budget A_t which typically ranges from \$3-4 billion in

nominal terms. These funds are allocated to each grantee i using the following funding formula:

$$CDBG_{it}^{alloc} = f\left(A_t, \frac{Pop_{it}}{\sum_i Pop_{it}}, \frac{Pov_{it}}{\sum_i Pov_{it}}, \frac{Overcrowd_{it}}{\sum_i Overcrowd_{it}}, \frac{GrowthLag_{it}}{\sum_i GrowthLag_{it}}, \frac{OldHousing_{it}}{\sum_i OldHousing}\right) \quad (1)$$

where f is a nonlinear function of the six underlying inputs.[8] The formula inputs from Equation 1 include:

1. Pop_{it} represents total population living within the jurisdiction of grantee i

2. Pov_{it} calculates the population with household incomes beneath the federal poverty limit

3. $Overcrowd_{it}$ indicates the number of housing units where the ratio of occupants to rooms exceeds 1.01

4. $GrowthLag_{it}$ calculates the difference between the current population and what the population would have been if population growth had followed the national trajectory since 1960

5. $OldHousing$ counts the number of housing units constructed prior to 1940

From 2002 to 2011 (the pre-treatment period), formula inputs were obtained from the 2000 decennial census. While population and growth lag were updated annually with modeled census estimates, the other inputs were fixed to the 2000 Census and remained constant throughout this time. Variation in CDBG allocations over this period therefore arose from three factors: 1) annual fluctuations in congressional budget A_t, 2) annual changes in population and growth lag from modeled population estimates, and 3) the gradual influx of newly-eligible grantees, affecting denominator totals in Equation 1.

In 2012, the CDBG implemented two significant updates to the formula inputs. Inputs from the 2000 Census (poverty counts, overcrowding, pre-1940 housing) were updated to the corresponding inputs from the 2005-2009 American Community Survey (ACS). Inputs in subsequent years would also be updated with each annual iteration of the ACS. Population and growth lag, which used the 2009 modeled population estimates in 2011, switched to the 2010 official census counts and would continue to be use annual population estimates in subsequent years.

These changes profoundly affected the input values used to calculate CDBG allocations. For the average grantee, the 2005-2009 ACS overcrowding estimate was 46 percent lower than in the 2000

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[8]I review the explicit functional form of the formula in Section 4, which I use in my identification strategy for estimating fiscal multipliers.

Census estimate. Poverty counts were 16 percent higher, and pre-1940 housing counts were 8 percent higher (Joice, 2012). The correction from the 2009 modeled population estimate to the 2010 census was smaller—typically within 3% in magnitude for most jurisdictions—but still large enough to affect allocations for a variety of public programs where funding was tied to population counts (Serrato and Wingender, 2016).[9] These changes led to an unprecedented "scrambling" of grantee allocations: Figure 3 compares how the distribution of year-over-year changes in CDBG allocations differed between 2010-11 and 2011-12. There was almost no movement across the distribution in terms of grant allocation size prior to 2012: between 2010 and 2011, only three out of over 1,000 grantees had allocation changes exceeding 5% in magnitude, conditional on annual CDBG budget sizes. Between 2011 and 2012, 648 grantees experienced swings of over 5 percent.

Importantly, the data update is unlikely a purely exogenous event. The primary concern is that the data update partially reflects evolving local labor market conditions. For example, "winners" from the data shock may be more likely to be jurisdictions with declining labor markets, reflected by increases in poverty counts between the 2000 census and the 2005-2009 ACS. Post-treatment comparisons of winning and losing grantees may therefore be confounded by these underlying trends captured by the data update.

Several features of the data update mitigate these concerns. First, the data update captures outdated information. The 2012 data update reflects changes in poverty, overcrowding, and pre-1940 housing that occurred between 2000 and 2005-2009, as well as changes in population that occurred between 2009 and 2010. Second, much of the data update reflects changes in measurement that are unlikely to be correlated with changes in local labor conditions. Serrato and Wingender (2016) argue that the change from the 2009 population estimate to the 2010 census count was driven nearly entirely by measurement error. Among the ACS variables, the *increase* in the stock of pre-1940 housing by 8 percent after the data update seems entirely driven by measurement error: while the stock of housing built before 1940 can decrease through demolition, it is physically impossible for that number to have increased over time. The 46 percent decline in overcrowded housing also seems remarkable, especially over a period characterized by its housing market volatility.

Joice (2012) describes several key differences in measurement that arose from the switch from the decennial Census to the ACS. First, the decennial Census is administered on April 1 once every ten

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[9]While there is a risk of our job count estimates being confounded with other programs relying on the Census correction, we note that 1) the CDBG data correction happened several years after the launch of the census counts in 2012, 2) population counts receive the lowest weights in the funding formula, and 3) the size of the Census correction is small relative to the size of the ACS correction.

years whereas the 5-year ACS estimates are collected and averaged continuously over five years. The ACS estimates will therefore capture seasonal differences in poverty counts that the one-time Census did not. Second, the ACS changed the "residence rule" determining where respondents live. The ACS requires a household to respond if it has lived, or plans to live, for 2 months at the unit where the survey was mailed. By contrast, the Census required respondents to answer based on their "usual place of residence". This measurement change most directly impacts places with high concentrations of seasonal residents. Third, the ACS sample is substantially smaller than the sample that completed the Census long form, implying a greater degree of sampling error. Finally, while the decennial census was conducted almost entirely through mail-in responses, the ACS relied more heavily on telephone and in-person interviews. Interviewers were able to reduce respondent confusion over what counted as a room in the calculations for overcrowding (Woodward, Wilson and Chesnut, 2007) and help respondents determine their building's true age in the calculation of pre-1940 housing.

3.1.2 Difference-in-Differences and Synthetic Difference-in-Differences

My first approach for estimating the impacts of the CDBG funding shock follows Havnes and Mogstad (2011), who quantify the impact of child care reform in Norway by splitting municipalities evenly into into two groups based on each municipality's post-reform percentage in childcare uptake. The authors then use differences-in-differences to compare high-uptake municipalities (treatment group) and low-uptake municipalities (comparison group) before and after the reform. In a similar manner, I split CDBG grantees into a treatment and comparison group by evenly dividing grantees according to the percentage point increase in their CDBG allocations before and after the data update. Table 2 presents summary statistics and differences between treatment and comparison grantees across a variety of characteristics. Differences in socioeconomic, demographic, and neighborhood characteristics are generally small with the exception of comparison grantees having noticeably higher median home values. Job counts–our outcome of interest–are generally higher for treatment grantees than for comparison grantees.[10]

Figure 4 shows the trajectory of log CDBG funding for both groups relative to 2011, the last pre-treatment year.[11] Prior to 2012, the trajectory of CDBG funding was nearly identical between the two groups. Between 2011 and 2012, funding declined for both groups due to a smaller congressional

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[10]This approach also extends prior work by Gordon (2004) which studies how school spending responded to a sudden data update to the inputs to Title I funding.

[11]Figure A1 shows the raw trajectory of funding prior to centering on the last pre-treatment year.

budget, but allocations declined by 19 percentage points more for losers than for winners. The trends return to being parallel immediately after the shock, implying that the data update had a one-time effect on funding that persisted through the post-treatment period.

I employ this panel-based strategy to address remaining concerns about the endogeneity of the data update. If the changes between 2000 to 2009 captured by the data update were truly endogenous, then parallel trends should fail to hold prior to 2012. The pre-treatment trajectory of jobs depicted in Panel A of Figure 5 suggests the opposite: job counts were convincingly parallel between the treatment and comparison group, particularly after 2006. The parallel nature of the pre-trends over a fairly long pre-treatment horizon gives additional credibility that the discontinuous break in trends after 2012 can be attributed to the sudden data shock. Given this, I estimate the DiD effect using the following estimating equation:

$$Y_{it} = \mu + \tau(Treat_i \times Post_t) + \alpha_i + \beta_t + \epsilon_{it} \tag{2}$$

where i indexes grantees and t indexes years.[12] Additional covariates to improve precision were not available due to the length of the sample period and the nonstandard boundaries of CDBG grantees.[13]

The robustness of the baseline differences-in-differences approach can be improved through the synthetic difference-in-differences method, which differs in several key ways. In SDID, I observe a panel for $i = 1, ..., N$ grantees, N_{tr} of which comprise the treatment group and N_{co} of which comprise the comparison group. The panel extends over the years $t = 1, ..., T$ and there are $T_{pre} = 10$ pre-treatment periods. Treatment status W_{it} is zero for all grantees prior to 2012 and is one for treated grantees after T_{pre}. In standard differences-in-differences (DID), the estimated treatment effect τ solves the following optimization problem:

$$\left(\hat{\tau}^{did}, \hat{\mu}, \hat{\alpha}, \hat{\beta}\right) = \underset{\tau,\mu,\alpha,\beta}{\operatorname{argmin}} \left\{ \sum_{i=1}^{N} \sum_{t=1}^{T} (Y_{it} - \mu - \alpha_i - \beta_t - W_{it}\tau)^2 \right\} \tag{3}$$

where each unit i and time period t is given equal weight. Synthetic differences-in-differences (SDID)

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[12]I briefly note that there is no staggered adoption in this context, mitigating the need to incorporate new methods from Goodman-Bacon (2021), Callaway and Sant'Anna (2021), de Chaisemartin and D'Haultfœuille (2020), Sun and Abraham (2021), and others.

[13]The ACS 1-Year estimates begin in 2005, and the smallest available geographic unit—the Public Use Microdata Area—is generally larger than the jurisdiction of CDBG grantees and does not align well with respect to geographic boundaries.

allows these weights $\hat{\omega}_i^{sdid}$ and $\hat{\lambda}_{ti}^{sdid}$ to vary, leading to the modified optimization problem:

$$\left(\hat{\tau}^{sdid}, \hat{\mu}, \hat{\alpha}, \hat{\beta}\right) = \underset{\tau, \mu, \alpha, \beta}{\operatorname{argmin}} \left\{ \sum_{i=1}^{N} \sum_{t=1}^{T} \left(Y_{it} - \mu - \alpha_i - \beta_t - W_{it}\tau\right)^2 \hat{\omega}_i^{sdid} \hat{\lambda}_t^{sdid} \right\} \tag{4}$$

The weights $\hat{\omega}_i^{sdid}$ and $\hat{\lambda}_{ti}^{sdid}$ are determined algorithmically prior to solving Equation 4. Like in the traditional synthetic control method (SCM) (Abadie, Diamond and Hainmueller, 2010), the unit weights $\hat{\omega}_i^{sdid}$ are chosen to minimize pre-treatment differences in outcomes between the treatment group and the weighted comparison group. The overarching intuition is that a weighted combination of untreated tracts may represent a more appropriate comparison group than any single tract or unweighted combination of untreated tracts. Formal results show that improving pre-treatment fit reduces bias under several classes of data-generating processes including linear factor models (from which differences-in-differences is derived) and autoregressive models (Abadie, Diamond and Hainmueller, 2010; Ben-Michael, Feller and Rothstein, 2018). This further justifies the use of flexible unit weights to achieve better pre-treatment fit.

Unlike traditional SCM, SDID permits estimation with multiple treated units—much like DiD. The SDID calculation of these weights also differs from standard SCM in two key ways. First, SDID incorporates a regularization parameter ξ which increases dispersion of the weights and ensures a unique solution.[14] Second, SDID includes an intercept term ω_0 which relaxes the requirement that pre-treatment outcomes in the synthetic control group exactly match pre-treatment outcomes in the treatment group. Instead, parallel trends are sufficient—a weaker assumption which is made possible by the inclusion of fixed effects α_i in Equation (4). The weights $(\hat{\omega}_0, \hat{\omega}^{sdid})$ therefore solve the following optimization problem:

$$(\hat{\omega}_0, \hat{\omega}^{sdid}) = \underset{\omega_0, \omega}{\operatorname{argmin}} \sum_{t=1}^{T_{pre}} \left(\omega_0 + \sum_{i=1}^{N_{co}} \omega_i Y_{it} - \frac{1}{N_{tr}} \sum_{i=N_{co}+1}^{N} Y_{it} \right)^2 + \xi^2 T_{pre} \|\omega\|_2^2 \tag{5}$$

In short, SDID boasts the strengths of SCM by allowing comparison units to have flexible weights—reducing bias by improving pre-treatment fit—while also retaining the benefits of the DiD parallel trends assumption and allowing estimation with multiple treated units. On the other hand, the validity of SDID is closely tied to the number of pre-periods available and the goodness of pre-treatment fit.

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[14]I use the default regularization parameter proposed by Arkhangelsky et al. (2021) based on the size of a typical one-period change for comparison units prior to the event.

Unlike both SCM and DiD, the SDID estimator also applies weights to pre-treatment time periods. The weights are calculated using control units and attempt to equalize the weighted average of pre-period outcomes with the simple average of post-period outcomes. Weights are intuitively greater for pre-periods that are more similar to post-periods. The weights $(\hat{\lambda}_0, \hat{\lambda}^{sdid})$ solve the following optimization problem:[15]

$$(\hat{\lambda}_0, \hat{\lambda}^{sdid}) = \underset{\lambda_0 \lambda}{\text{argmin}} \sum_{n=1}^{N_{co}} \left(\lambda_0 + \sum_{t=1}^{T_{pre}} \lambda_i Y_{it} - \frac{1}{T_{post}} \sum_{t=T_{pre}+1}^{T} Y_{it} \right)^2 \qquad (6)$$

Arkhangelsky et al. (2021) suggest a variety of approaches to inference. With multiple treated units, the authors provide variations of the clustered bootstrap (Efron, 1979) and jackknife applied to SDID which both produce conservative confidence intervals. I report standard errors from the bootstrap approach which generally produces more conservative confidence intervals. The jackknife approach is more computationally feasible, and I present those confidence intervals in an Appendix.

3.2 Data

Job Counts: I use public-use data from the LEHD Origin-Destination Employment Statistics (LODES), a dataset compiled and administered by the Longitudinal Employer-Household Dynamics (LEHD) program at the U.S. Census Bureau. The LODES data provide worker counts at the census block level for all combinations of residence and workplace census block pairs.[16] In other words, the data provide the number of jobs held by workers working in census block a and residing in census block b for every combination of blocks (a, b). I aggregate all census block counts to the geographic boundaries of their respective grantees. The data extend from 2002 through 2019 for most states. To protect confidentiality, the data are "fuzzed" at the block level via noise infusion. For some years, the data also contain disaggregations of job counts by race, wage category, education, and more.

The origin-destination structure of the LODES data allows me to quantify jobs held by nearby residents and low-income workers. I define nearby workers as those working in a treated tract and living in a tract whose centroid is within two miles of the treated tract's centroid. I use two different metrics to quantify jobs held by low-income workers. First, I observe the poverty rate of each worker's tract of residence and use this to determine the number of workers commuting from low- and moderate-income (LMI) tracts. In this setting, I define LMI tracts as those where the poverty rate is above the

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[15]Unlike unit weights, time weights are calculated without a regularization parameter.
[16]Census blocks are the smallest geographic unit used by the Census. Each census tract is an amalgamation of underlying census blocks.

median among tracts within the same commuting zone. I further define low-income tracts as those where the poverty rate exceeds the 75th percentile among tracts within the same commuting zone.

I also consider job counts based on worker earnings. The LODES provides the number of block-level jobs where monthly earnings are less than $1,250 (low-wage jobs), between $1,251 and $3,333 (mid-wage jobs), and greater than $3,333 (high-wage jobs). I focus on how job counts in the lowest wage category respond to CDBG investments.

CDBG Grantee-Level Allocations: I also use publicly-available data on grantee allocations from 1975 to 2018. I adjust all nominal allocations to 2018 real dollars. I include only grantees which were eligible for the CDBG from 2002 through 2018—roughly 80 percent of all grantees in the data. Because grantees qualify by crossing a population threshold, the 20 percent of excluded grantees are all smaller counties and cities which reached the population threshold at some point during the sample time frame.

3.3 Results

Panel A of Figure 5 provides a visualization of the raw difference-in-differences estimate, comparing log job counts for the treatment and comparison groups relative to 2011. The DiD estimate is 0.045 which is significant at the 1 percent level after adjusting standard errors for clustering at the grantee level. The DiD, which evenly weights all grantees in the comparison group, appears to exhibit approximately parallel trends in the pre-treatment period, particularly after 2006. The trajectories begin diverging in 2012 and continue to widen through 2019, which is consistent with the 20% gap in CDBG funding depicted in Figure 4 that emerges in 2012 and continues to persist thereafter. Under the assumption that the trajectory of job counts in the treatment group would have remained parallel absent the data shock, the DiD framework estimates that the persistent 20% gap in CDBG funding generated by the data shock led to an a 4.5 percentage point difference in job counts, averaged over the course of eight post-treatment years.

By comparison, the SDID method assigns unit weights to each grantee in the comparison group to make the pre-treatment job count trajectory in the comparison group as parallel as possible to the trajectory in the treatment group. Figure A2 plots the distribution of weights and how they differ from uniform weights. With 445 comparison grantees, uniform weights are 0.0022 for each grantee. No single synthetic weight exceeds 1.5 times the uniform weight, likely due to the contribution of the dispersion parameter ξ as well as how close to parallel the pre-trends already are for the unweighted comparison group. Comparing the Panel A of Figure 5 (DiD) to Panel B (SDID), the SDID adjustment

improves the fit prior to 2006 as well as in the years immediately preceding the data update. Table 2 shows that the SDID comparison group is very similar to the control group in terms of socioeconomic, demographic, and neighborhood covariates. The SDID comparison group is much closer to the treatment group in terms of job count outcomes, though differences still remain.

In terms of time weights, SDID assigned positive weights to only 2008 (0.14), 2010 (0.09), and 2011 (0.77). The weighted combination of job counts for comparison grantees across these years most closely matches average job counts for comparison grantees after the data shock. The distribution of the time weights suggest that labor market conditions in 2011 were by far the most similar to post-data shock labor market conditions.

The inclusion of time and unit weights decreases the estimate to 0.034, which remains significant at the 1% level. Figure 6 shows the SDID estimate for six different outcomes: total job counts, total jobs located in high poverty tracts (poverty rates higher than the national median), total jobs held by workers living in high poverty tracts, low wage jobs (monthly earnings less than $1,250), middle-wage jobs (monthly earnings between $1,251 and $3,333), and jobs held by workers living within five miles of the workplace. All six outcomes are statistically significant, and pre-treatment fit is fairly strong across the board. CDBG funding appears to have a slightly larger impact on jobs located in high poverty tracts as well as jobs held by workers living in high poverty tracts, although these differences do not appear to be significant. The CDBG does appear to have a larger proportional impact on low-wage jobs than for middle-wage jobs. Finally, the CDBG does not appear to disproportionately create jobs held by nearby workers.

How do the job impacts of the CDBG compare with those of other programs? Bartik (2020b) summarizes cost-per-job estimates of several prominent place-based policies which include include firm incentive policies ($196,000 per job created), the Tennessee Valley Authority ($77,000), customized job training ($15,000), and cleanup of contaminated industrial sites ($13,000). By comparison, our SDID estimates show that a 20 percent difference in annual CDBG funding over the course of eight post-treatment years led to a 3.4 percent increase in jobs. For the median grantee a $1.15M annual block grant from the CDBG and roughly 12,000 jobs, our estimates imply a cost-per-job estimate of approximately $4,346.[17]

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[17]The median block grant size and job count was obtained by computing distributions for each in 2011. The cost-per-job estimate was calculated by first multiplying pre-treatment jobs for the median grantee (11,853) by the SDID effect (0.034) to obtain 403 jobs created. I then multiply the median grant size ($1,152,156) by the CDBG shock size (0.19) to obtain a shock size of $218,910. I multiply the shock size by eight to reflect the total funding gap that occurred over the course of the pre-treatment period. Dividing the total funding shock ($1.75M) by average jobs created (403) yields the cost-per-job

This estimate is almost certainly an underestimate for the actual public spending required per job created. Although this would be a remarkable estimate in comparison to previously-studied policies, the CDBG cannot be evenly compared against these other policies. Certainly, there are clear advantages to the CDBG as a mechanism for creating jobs: local governments have agency and better information in identifying what spending needs will generate the largest impact, and there may even be important synergies between economic development activities and other activities funded by the CDBG including housing and public services. Regardless, we next explore the extent to which the cost-effectiveness estimates from this section can potentially be explained by local public spending multipliers.

4 Estimating Public Spending Multipliers

I now ask: how much of the CDBG job impact can be attributed to the direct effect of the block grant as opposed to additional local public spending attracted by an influx of federal funding? This exercise also represents an empirical test of the "flypaper effect", an empirical finding that each dollar of intergovernmental transfer between federal and state/local governments tends to trigger an increase in public spending far greater than what an equivalent increase in tax revenue would generate (Inman, 2008).

The CDBG is often used by governments as "seed money" to attract outside funding from public and private sources or fill funding gaps Theodos, Stacy and Ho (2017). A similar block grant program implemented through the Empowerment Zone program found descriptive evidence that each dollar of block grant funds could be linked to an additional $7 of outside money spent (Busso, Gregory and Kline, 2013). The CDBG also includes a provision known as the Section 108 Loan Guarantee Program. This program provides CDBG grantees with the option to pledge up to five times their annual CDBG allocation as security for a federally-guaranteed loan. The program provides CDBG grantees with an immediate source of short-term funding above and beyond their annual CDBG allocations, and allows costs to be spread out for up to 20 years. Across all projects using Section 108 funds (including those that did not use outside funds), each dollar of Section 108 obtained was also linked to $3.80 of other funds.[18] Many grantees indicated their projects could not have been completed without the aid of Section 108 funds. In a survey, 63 of 118 respondents stated that "without the Section 108, the projects would not have happened at all" (Prunella, Theodos and Thackeray, 2014).

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estimate of $4,345.

[18]Conditional on using other funds, each dollar of Section 108 secured $4.69 of spending.

4.1 Empirical Strategy

My objective is to estimate the effect of an exogenous shock to CDBG grants on local public spending. I focus on estimating the following regression, which relates per-capita local public spending to per-capita block grants from the CDBG:

$$\frac{Spend_{it}}{Population_{i0}} = \alpha + \rho \frac{CDBG_{it}}{Population_{i0}} + C'_{it}\beta + \theta_i + \lambda_t + \epsilon_{it} \tag{7}$$

I anchor population counts to a baseline-pre period to avoid division bias from endogenous changes in population. The regression also includes fixed effects for each grantee i and calendar year t. The outcome $Spend_{it}$ represents public spending on community development and housing. The vector of controls C_{it} contains the sum of state and local intergovernmental grants specifically tied to housing and community development, lagged by one year. The analysis time period runs from 2011 to 2017.

To estimate ρ, I use a simulated instrumental variables approach based on the specific structure of the funding formula. As stated in the introduction to the CDBG allocation formula, Congress determines the annual amount of funding A_t available for CDBG use, which typically ranges from \$3-4 billion nominally. The funds are then allocated to grantees via a funding formula which attempts to set block grant generosity based on the relative need of each grantee i. The allocation formula is as follows:

$$CDBG_{it} = s_t \times A_t \times \max\Big(\underbrace{0.25 \times \frac{Population_{it}}{\sum_i Population_{it}} + 0.5 \times \frac{Poverty_{it}}{\sum_i Poverty_{it}} + 0.25 \times \frac{Overcrowd_{it}}{\sum_i Overcrowd_{it}}}_{\text{Formula A}},$$

$$\underbrace{0.2 \times \frac{GrowthLag_{it}}{\sum_i GrowthLag_{it}} + 0.3 \times \frac{Poverty_{it}}{\sum_i Poverty_{it}} + 0.5 \times \frac{Pre1940Housing_{it}}{\sum_i Pre1940Housing}}_{\text{Formula B}} \Big) \tag{8}$$

$Population_{it}$ represents total population. $Poverty_{it}$ represents the number of people below the federal poverty limit. $Overcrowd_{it}$ represents the number of housing units where the ratio of occupants to rooms exceeds 1.01. $GrowthLag_{it}$ represents the difference between the current population and what the population would have been if population growth had followed the national trajectory since 1960. $Pre1940Housing$ represents the number of housing units built prior to 1940. Prior to 2012, formula inputs were calculated using estimates from decennial censuses. Poverty, overcrowded housing, and pre-1940 housing were updated every ten years. Population and growth lag were updated annually

via census estimates.[19] Starting in 2012, all formula inputs began updating annually using 5-Year ACS estimates. The 2012 inputs were determined using the 2005-09 ACS, the 2013 inputs were determined using the 2006-10 ACS, etc.

The contribution of each input k (e.g. population, poverty, etc.) to a grantee's allocation is based on that grantee's share of the national total for k. Each share is then multiplied by a constant c^k. These constants sum to one across the three inputs on either side of the formula. Grantees are then assigned an allocation based on the maximum of Formula A and Formula B. This amount is then multiplied by the federal appropriation A_t and then re-adjusted *pro-rata* by s_t such that the sum of allocations across all grantees matches the appropriated budget A_t.[20]

One way to interpret the formula is to collapse together inputs that do not vary at the grantee level as follows:

$$CDBG_{it} = \max \left(\sum_{k=1}^{3} v_t^k X_{it}^k, \sum_{k=4}^{6} v_t^k X_{it}^k \right) \tag{9}$$

where X_{it}^k is the value of the kth input for grantee i in year t (e.g. $Population_{it}$, $Poverty_{it}$, etc.), and the v_t^k include the following terms:

$$v_t^k = \frac{c^k s_t A_t}{\sum_i X_{it}^k} = \frac{c^k s_t A_t}{\tilde{X}_t^k} \tag{10}$$

v_t^k has the straightforward representation of the *dollar value* or "price" associated with an additional unit of input k. These prices vary over time, as depicted by Figure A3.

The defining feature of the allocation formula is its two-pronged categorization of grantees. "Formula A" favors grantees that are typically fast-growing cities with limited housing supply. "Formula B" typically favors grantees in older, deteriorating cities. While the two formulas should theoretically provide more funding to localities with greater needs, Collinson (2014) points out several of the formula's redistributive shortcomings. In particular, Formula A communities tend to be underfunded relative to actual need, whereas many high-income Formula B communities with older housing stock and slow growth tend to be overfunded.[21]

Next, recall that the allocation received by grantee i in year t can be expressed by the simplified Equation (9), a weighted combination of inputs X_{it}^k and their corresponding prices v_t^k. Equation (10) reveals that the prices v_t^k are determined externally from the perspective of grantee i. Federal ap-

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[19]These annual updates are not surveys, but rather modeled estimates based on decennial censuses.

[20]Without s_t, the formula typically allocates more money than what is actually available. As such, $s_t < 1$.

[21]In addition to redistributive shortcomings of the CDBG formula, Brooks and Sinitsyn (2014) also find that within localities, CDBG funding does not consistently reach communities with high levels of need.

propriations A_t and pro-rata adjustments s_t are determined at the national level. For each grantee, I recalculate national totals $\sum_i X_{it}^k$ to omit own-grantee contributions.

I use the external nature of the prices to construct a *simulated* instrument for $CDBG_{it}$. Between $t-1$ and t, changes in spending are correlated with changes in CDBG inputs ($\mathbf{X}_{it-1} \to \mathbf{X}_{it}$), but changes in prices from $\mathbf{v}_{t-1} \to \mathbf{v}_t$ are more likely to be exogenous. $\mathbf{X}'_{it-1}\mathbf{v}_t$ is the "simulated" CDBG that would have been received if exogenous prices changed but endogenous inputs $\mathbf{X}_{it-1}$ did not. Briefly abstracting from the nonlinear max function in the CDBG setting, another way to see this is to decompose $CDBG_{it}$ into an exogenous and endogenous component.

$$
\begin{aligned}
CDBG_{it} &= \mathbf{X}'_{it-1}\mathbf{v}_t + u_{it} \\
&= \mathbf{X}'_{it-1}\mathbf{v}_t + \mathbf{X}'_{it}\mathbf{v}_t - \mathbf{X}'_{it-1}\mathbf{v}_t \\
&= \underbrace{\mathbf{X}'_{it-1}\mathbf{v}_t}_{\text{Exogenous}} + \underbrace{[\mathbf{X}_{it} - \mathbf{X}_{it-1}]'\mathbf{v}_t}_{\text{Endogenous changes}}
\end{aligned}
$$

The exogenous $\mathbf{X}'_{it-1}\mathbf{v}_t$ is then used as a simulated instrument for endogenous $CDBG_{it}$. As is standard practice in the literature, I also fix the lagged inputs to an initial pre-period, which I denote as $\mathbf{X}_{i0}$. In this setting, I use inputs from the 2000 census, a decade before my analysis sample begins. $\mathbf{X}'_{i0}\mathbf{v}_t$ is plausibly exogenous due to the fact that lagged inputs are pre-determined from a decade prior and prices are externally determined. The final instrument which re-incorporates the max function is as follows:

$$
\widetilde{CDBG}_{it} = \max(\mathbf{X}_{i0}^A \mathbf{v}_t^A, \mathbf{X}_{i0}^B \mathbf{v}_t^B) \tag{11}
$$

This instrument represents how much of grantee i's CDBG allocation can be attributed to movements in the exogenous prices v_t^k, where each pre-period input X_{i0}^k determines how much grantee's allocation is tied to fluctuations in that input's corresponding price. Although the instrument operates using a nonlinear max function, it resolves to a typical linear combination if the inputs $\mathbf{X}_{i0}^A$ and $\mathbf{X}_{i0}^B$ are taken as given; the corresponding weights $\mathbf{v}_t^A$ and $\mathbf{v}_t^B$ will either be a vector of zeros or the actual vector of prices depending on which produces the greater allocation.

The simulated instrument has a long history beginning with Currie and Gruber (1996) and Gruber and Saez (2002), and shares many properties of the widely-prevalent Bartik (or shift-share) instrument.[22] Goldsmith-Pinkham, Sorkin and Swift (2020) show that the identifying assumption of this

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[22]This instrument differs from the traditional shift-share instrument there is no "shift", given that the CDBG allocation formula is already written as a linear combination of inputs X_{i0} and prices v_{i0} in levels. Aside from this mechanical differ-

class of instruments is tied to the exogeneity of the fixed pre-period values $\mathbf{X}_{i0}$. The intuition for this maps to the identifying assumptions of basic differences-in-differences. In the example of a single input k, the evolution of v_t^k represents a national change in policy intensity and X_{i0}^k represents each unit's exposure to policy changes. The validity of differences-in-differences relies on the assumption that X_{i0}^k is not endogenous to subsequent changes in the outcome variable that are unrelated to policy changes in v_t^k.

As an instructive example, consider a grantee whose instrumented CDBG allocation is entirely driven by high baseline pre-1940 housing. Figure A3 shows that the dollar value of pre-1940 housing has monotonically increased over time. The identifying assumption would be violated if similar grantees with disproportionate exposure to pre-1940 housing all experienced rising public spending (relative to grantees with low levels of pre-1940 housing, and conditional on controls C_{it} and year fixed effects) for reasons unrelated to fluctuations in prices v_t^k.

While the exclusion restriction is ultimately not verifiable, several aspects of the instrument lend support to its validity. First, each grantee's CDBG allocation is determined by a combination of six inputs with varying trajectories for prices v_t^k, limiting the ability of any one endogenous factor to drive the estimate. Second, the non-linear max function creates a discontinuous change in the set of prices used (i.e. which "policy" is applied). For grantees near the boundary of the max function, a coin flip determines whether the instrument uses Formula A or Formula B prices. The instrument can even use both sets of prices for the same grantee in different years. Third, the analysis period begins in 2011 but the X_{i0}^k are pinned to baseline levels in 2000. If contemporaneous inputs X_t^k are endogenous to local public spending, then serial correlation will expose lagged values of the input to this endogeneity. Using a ten-year lag reduces exposure to this potential source of bias. Finally, with unit fixed effects in Equation (8), the identifying assumption requires that baseline X_0^k not be endogenous to *changes* in public spending as opposed to levels (Goldsmith-Pinkham, Sorkin and Swift, 2020).

4.2 Data

For spending outcomes, I rely on data from the Annual Survey of Local Government Finances. This data set is a comprehensive source of state and local government finance data that is collected and standardized on a national scale. Each year, surveys are administered to a sample of 90,000 governmental units throughout the United States, including counties, cities, townships. A census of all governments known as the Census of Governments Survey of Local Government Finances takes place

ence, most of the properties of traditional shift-share instruments apply to the simulated instrument.

every five years.[23] Samples are taken in non-census years, but most large governmental units are included with certainty in the annual sample.[24] Non-certain governmental units are sampled based on a "measure of size" calculated using total expenditures, taxes, and revenue from the prior census. The data from these surveys include narrowly-categorized breakdowns of revenue and expenditure sources for each government unit in the sample. I focus on spending categorized under "Housing and Community Development", which the CDBG specifically funds. I make several corrections to the data to account for potential reporting errors. First, I drop observations where expenditures on housing and community development were zero; this is unlikely to be true for grantees receiving CDBG dollars. Second, I drop observations where per-capita public spending is in the top and bottom 2.5%.

I also use data on the CDBG formula inputs underlying the grantee allocation calculations. This data set includes all inputs to the formula in Equation (8) from 2011 to 2017. The 2011 inputs were derived from the 2000 Census (except for population) and all subsequent years were derived from 5-Year ACS estimates, beginning with the 2005-09 ACS for the 2012 CDBG inputs. HUD did not retain data on formula inputs prior to 2011. I link each grantee in the CDBG data to its corresponding governmental unit in the Annual Survey of Local Government Finances. In total, I am able to link roughly 80 percent of grantees to their respective units in the survey. I keep only observations where the panel is not missing any data.

4.3   Results and Discussion

I present OLS, reduced form, first stage, and IV estimates in Table 3. I also provide 95 percent confidence intervals constructed from standard errors clustered at the grantee level. In the raw OLS, each per-capita dollar of block grant is associated with two dollars of per-capita public spending on housing and community development. The reduced form estimate is similar in magnitude and the corresponding first stage estimate suggests that variation in the instrument accounts for 59 cents of every dollar of CDBG funding. The first stage estimate suggests that changes in CDBG allocations are largely driven by national changes in the input values. The IV estimate indicates that a one dollar per capita increase in CDBG generates approximately 3.16 dollars of per capita local public spending. The increase relative to the OLS estimate suggests that the combined endogeneity of the six formula inputs leads to a downward bias in the raw relationship between per-capita CDBG allocations and public spending.

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[23]The census is conducted in years ending in "2" and "7".

[24]For example, the criteria for the 2016 sample were as follows: 1) all county governments with population over 100,000; 2) Cities with population over 75,000, 3) Townships with population of over 50,000.

These estimates suggest that the total amount of local public spending per job created for the CDBG is $13,733 per job, bringing it closer in line with existing cost-effectiveness estimates of other place-based policies. As an important caveat, this estimate does not take into account private investments which may have also been attracted through CDBG projects. As previously mentioned, large-scale projects using Section 108 reported securing over two dollars of private funding per dollar of Section 108 funding requested. Still, the CDBG appears to have done a remarkable job attracting other public funds to support local job creation efforts.

5   Getting Beneath the Hood of Effective Place-Based Policies

One of the most important features of the CDBG is that it allows local governments to use their block grants for a wide variety of community development purposes. In this section, I focus on investments in economic development with the focus of uncovering 1) what kind of economic developments tend to generate the largest job impacts, and 2) what characteristics of places tend to be most conducive for place-based policies.

Table 1 summarizes the various uses of the CDBG. In this section, I focus on investments in economic development activities, which include the following activities:[25]

1. Clearance, demolition, and cleanup of contaminated sites: clearance or demolition of buildings/improvements, or the movement of buildings to other sites. I also include activities undertaken to clean toxic/environmental waste or contamination.

2. Commercial and industrial construction: acquisition, construction, or rehabilitation of commercial/industrial buildings. I also include land acquisition/assembly for the purpose of creating industrial parks or promoting commercial/industrial development.[26]

3. Exterior improvements: exterior building improvements (generally referred to as "facade improvements"), and correction of code violations. The scope of the rehabilitation is generally more limited compared to "Buildings and Land" activities.

4. Financial assistance: Direct financial assistance to for-profit businesses, which can be used (for example) to acquire property, clear structures, build, expand or rehabilitate a building, purchase equipment, or provide operating capital. Forms of assistance include loans, loan guarantees,

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[25]The language used for these descriptions are taken directly from the CDBG's "Matrix Code Definitions", which can be accessed here: https://files.hudexchange.info/resources/documents/Matrix-Code-Definitions.pdf.

[26]Land-related activities are substantially less common than building-related activities.

and grants. These activities provide funding directly to private businesses, whereas other CDBG activities are typically undertaken by the local government or non-profit agencies. The CDBG does not fund tax incentives and subsidies for businesses to relocate.

5. Infrastructure: street, water, parking, rail transport, or other improvements to commercial or industrial sites.

6. Technical assistance: technical assistance to for-profit businesses, including workshops, assistance in developing business plans, marketing, and referrals to lenders or technical resources.

7. Microenterprise: financial assistance, technical assistance, or general support services to owners and developers of microenterprises. A microenterprise is a business with five or fewer employees, including the owner(s).

8. Non-profits and other: activities specifically designed to increase the capacity of non-profit organizations to carry out specific CDBG eligible neighborhood revitalization or economic development activities. This category also includes other uncategorized commercial and industrial efforts.

Crucially, the CDBG does not allow block grants to be spent on firm incentives, which are the most prevalent form of place-based policies and have been studied extensively (Bartik, 2020*b*; Slattery and Zidar, 2020). The first four columns in Table 4 provide a breakdown of these categories for CDBG activities funded between 2000 and 2016. Column (1) provides the average funding amount per activity type, and column (2) provides the total number of activities that were funded. The average CDBG investment is approximately $80,000 and nearly 43,000 such activities were funded during this time period. Infrastructure and building/land activities exhibit the highest cost-per-activity at $380,000 and $200,000. Clearance and financial assistance are the most commonly funded activities. Tracts often receive multiple CDBG investments; column (3) shows that the nearly 43,000 economic development activities were concentrated in only 8,812 different tracts (out of roughly 73,000 census tracts nationwide).

5.1 Empirical Strategy

I begin by estimating how tract-level job counts respond to large CDBG investments ("treatments") made within the tract.[27] Estimating tract-level causal effects in this setting is complicated by several

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[27]Following previous empirical work on place-based policies, I focus on census tracts as the primary geographic unit of observation. Census tracts are geographic subdivisions of counties that typically contain 1,200 to 8,000 residents and

issues. Tracts that receive CDBG investments differ from untreated tracts in numerous and often un-observable ways. This classical issue is further complicated by the fact that treatments (investments) themselves are also endogenous to the tracts receiving them. The size, mix, and timing of invest-ments made by local governments depend on neighborhood attributes and objectives of the govern-ment itself. These selection differences across treated tracts complicate the use of standard panel methods such as difference-in-differences, where treated tracts are pooled together and outcomes are compared over time against a comparison group of untreated (or not-yet-treated) tracts. Even in the unlikely case that such a comparison group provides a suitable counterfactual, estimates from stan-dard two-way fixed effect (TWFE) methods are likely biased in this setting due to treatment effects that are heterogeneous by groups and periods (de Chaisemartin and D'Haultfœuille, 2020), as well as staggered adoption and dynamic treatment effects (Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021; Baker, Larcker and Wang, 2021; Borusyak and Jaravel, 2016; Sun and Abraham, 2021).[28]

Rather than estimating an average treatment effect on a pooled sample of treated tracts, I take a case study approach and estimate causal impacts for each treated tract separately. This allows for the selection of comparison tracts to be tailored to each individual treated tract, while also avoiding many of the pitfalls of heterogeneous treatment effects and staggered adoption. The tradeoff is that it can be difficult and data-intensive to construct viable comparison groups for such a wide variety of treated tracts, and inference is less straightforward when there is only a single treated unit. I then examine the correlation between these tract-specific estimates and various characteristics of investments and places.

To estimate tract-specific causal effects, I re-purpose the synthetic difference-in-differences frame-work from Section 3 to conduct causal inference for a single treatment unit. This effectively reduces to the standard synthetic control method, but with two key differences. First, SDID relaxes the need for pre-treatment outcomes to match in levels; instead, parallel trends are sufficient for estimation. Second, SDID assigns weights to each pre-treatment time period, emphasizing pre-treatment time pe-riods where outcomes are more similar to the post-treatment period. Another minor difference is the

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commonly represent the size of a typical neighborhood.

[28] Standard TWFE estimates are a combination of group-by-period average treatment effects with weights that can be negative. Not only is the weighting somewhat opaque, but negative weights can be problematic when ATEs are heteroge-neous across groups or periods, leading to situations where (for example) the total effect is negative while all underlying ATEs are positive (de Chaisemartin and D'Haultfœuille, 2020). Under staggered treatment adoption, TWFE estimates can be decomposed into a weighted combination of cohort-specific treatment effects where weights can also be negative. Already-treated units are also used as a comparison for not-yet-treated units, which can be problematic when treatment effects vary over time. See Goodman-Bacon (2021), Callaway and Sant'Anna (2021), and Baker, Larcker and Wang (2021) for standard DID settings, and Borusyak and Jaravel (2016) and Sun and Abraham (2021) for event study settings. It is also worth noting that many of the conclusions in these papers require the treatment to be identical across treatment units.

incorporation of a regularization parameter which penalizes weighting schemes where the weights are concentrated on a small number of comparison units. Statistical inference in SDID is slightly different for a single treatment unit: the bootstrap is less reliable and the jackknife is not even defined. As such, we use a placebo variance estimation procedure for single treatment units proposed by Arkhangelsky et al. (2021), which is based on standard placebo-based inference tests used for traditional synthetic controls (Abadie, Diamond and Hainmueller, 2010). I run 100 placebo iterations per treated tract.

One practical consideration is how to define tract-level treatment. In any given year and census tract, I observe whether one or more CDBG economic development activities were funded. One possibility is to define treatment as the sum of all investments made in a given year within the same tract. Column (3) of Table 4 shows that nearly 9,000 tracts (roughly 12 percent of all tracts nationwide) received an economic development investment at any point from 2000 to 2016. Following Kline and Moretti (2013), I define initial treatment as an absorbing state and interpret my estimates as the effect of the initial CDBG investment, allowing for potentially endogenous future responses from other federal or local policies that may have been induced thereafter.

Assessing treatment size is complicated by the fact that CDBG investments in a given tract are often spread out over time. Local governments often aim to revitalize neighborhoods through multi-year plans or may simply not have enough CDBG funds in a single year to fully fund a project. Figure A4 plots how frequently additional investments were made one to ten years after the first time that an investment is observed in the data. Additional CDBG investments are made in 25% of treated tracts one year after initial treatment, in 21% of treated tracts after two years, and in 19% of treated tracts after three years. This percentage falls until the six-year mark, where the probability of additional investment remains stable at 15%. To approximate the size of the policy "push", I use the fact that the gradient is steepest during the first two years after initial investment and define treatment size as the three-year sum of investments. Figure **??** presents the cumulative distribution function of treatments defined in this way. T

One potential issue with this definition is that many treatments defined in this way are quite small. Table 4 suggests that many CDBG investments are small, with a median investment of only $80,000. Given that cost-per-job estimates from existing place-based studies currently range from $10,000 to over $100,000 (Bartik, 2020*b*), estimates from smaller treatments will likely be insufficiently powered to detect job impacts. I consequently focus on treatments that are $100,000 or greater. Focus on larger CDBG treatments is also consistent with a growing sentiment in the place-based literature that "big

pushes" are needed to generate lasting local impacts (Moretti, 2012). Column (4) of Table 4 shows that the average treatment size under these refinements is approximately $330,000. Columns (5) and (6) show that these treatments occurred in 1,375 census tracts and encompass more than 4,000 different investment activities.

The validity of synthetic controls crucially relies on having a large number of pre-periods Abadie, Diamond and Hainmueller (2010); Ben-Michael, Feller and Rothstein (2019), and a short pre-period is susceptible to bias from overfitting. I therefore restrict the sample of treated tracts to those where treatment occurs from 2012 to 2015, ensuring a minimum of ten pre-treatment periods and five post-treatment periods. Although it is possible that tracts may have been treated prior to the start of the panel in 2002, synthetic controls may still recover unbiased estimates if the synthetic comparison group is convincingly parallel over the course of ten years between treatments. In total, my analysis includes 489 treated tracts.

The final task is to define the appropriate set of donor units for each treated tract. In general, the potential for overfitting increases as the number of donor units increases. It is therefore neither advisable nor computationally feasible to use the set of untreated tracts nationwide as the donor pool. I construct a national comparison group comprised of 25 donor tracts from each of the nine census divisions across the country. To limit the set of donor tracts in each division to 25, I conduct the following procedure. First, I predict the probability of treatment for each untreated tract by estimating a logit model on the entire sample of tracts, regressing treatment status (whether a tract was ever treated) on a large vector of baseline census tract covariates.[29] I narrow the list of covariates using a lasso model selection procedure to determine the subset of covariates which best predict treatment status.[30] With the final set of logit predictions, I rank untreated tracts within each individual commuting zone. I construct the donor pool of control tracts for each commuting zone by selecting up to 50 tracts with the highest predicted probabilities of being treated, excluding tracts that were ever treated as well as tracts bordering ever-treated tracts.[31] Each treated tract is therefore compared against a different weighted combination of the same 225 untreated tracts that are most comparable to treated

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[29]The entire sample of tracts includes all tracts within commuting zones where at least one CDBG investment occurred, which rules out tracts in most rural commuting zones. The covariates include all variables underlying the indices in Table **??**, which are collected from the 2000 decennial census or the first year of the outcomes data. For census variables, I also compute the change between 1990 and 2000 and include them in the model selection procedure as well. Census data were obtained via Logan, Xu and Stults (2014), which adjusts tract-level estimates to account for the fact that tract boundaries are slightly re-drawn each decade.

[30]To rule out regional differences in predictions and substantially reduce computing time, I conduct this procedure separately for tracts within each of the nine census divisions.

[31]This includes tracts that were only treated within the first five years of the panel.

tracts based on observable characteristics.

After estimating treatment effects for each of the 489 treated tracts, I measure bivariate correlations between these treatment effects and characteristics of investments and places. For investment characteristics, I focus on the total size of the investment, as well as the amount invested in each of the spending categories above. For place-based characteristics, I focus on residential density, the demographic and socioeconomic makeup of residents, housing prices, employment-to-population ratio, and the five-year trajectory of jobs leading up to treatment. I calculate each of these place-based measures at both the tract level and at the commuting zone level, noting that the former measures the importance of neighborhood-level characteristics and that the latter measures the importance of labor market-level characteristics. I normalize each place characteristic to have mean 0 and standard deviation 1. As a caveat, despite fact that my tract-level estimates are causal, the correlates I estimate need not reflect causal effects due to the lack of experimental variation in what and where local governments invest in. Despite this, the CDBG represents a unique setting in which correlates of effective place-based policies can be identified within a common empirical and administrative framework.

5.2 Data

I use administrative expenditure-level data from the Integrated Disbursement and Information System (IDIS), an online system for federal formula grant programs such as the CDBG. I use data on the universe of CDBG-funded activities from 2000 to 2018. The total data contain nearly 70,000 recorded activities; however, roughly 30% of the activities in the data were not associated with a specific address, leaving the roughly 43,000 activities shown in column (2) of Table 4.[32] Each record contains details on the funded amount, the date when funding for the project was approved, and the date when the project was ultimately marked as completed.

The data also include two levels of detail with respect to the type of activity that was funded. The "activity group" is one of eight categories: acquisition, administrative/planning, economic development, housing, public improvements, public services, repayments of Section 108 loans, and other. As previously mentioned, I focus on economic development activities, which most closely map to place-based jobs policies. The data also include indicators for the eight subcategories of economic development activities described in Section 2.1. Finally, the data include the date which the activity was approved, which is the date I use to define when treatment begins.

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[32]This could potentially be due to issues in reporting; however, it is also likely that the investment is simply not tied to a single location. For example, virtually all expenditures under the category "Administration and Planning" are not associated with an address.

5.3 Activity- and place-based correlates of effective place-based policies

Across all treated tracts in the sample, I find that jobs increased by an average of 5.1%, an estimate which is significant at the 1% level. This estimate is calculated by averaging tract-specific estimates for all 489 tracts, and weighting each treated tract by the inverse of the squared standard error associated with the estimate. This suggests that the typical large place-based investment generated large job impacts within the tract where the investment occurred.

Figure 7 presents bivariate correlations between the estimated treatment effects and activity types and and our estimated treatment effects with various area level characteristics. The correlations compare standardized treatment effects (mean 0 standard deviation 1) versus standardized covariates. This means that the reported correlations reflect *comparative* impacts across the distribution; the average tract-level impact is still large and positive. The top panel shows how various investment characteristics correlate with tract-level treatment effects. As expected, there is a positive (albeit somewhat small) correlation between tract-level treatment effects and the total size of the treatment.

At the tract level, the results overwhelmingly suggest that conditional on being a low-income tract eligible to receive CDBG funding, place-based investments tend to produce larger job impacts in neighborhoods with more affluent and well-educated residents, higher housing prices, and higher employment-to-population ratios. These neighborhoods may be more conducive to commercial and industrial development. The strongest correlation comes from the 5-year pre-treatment job change, suggesting that policies which target neighborhoods on a positive trajectory are most likely to have greater job impacts. Note that the 5-year job change is incorporated into the synthetic control matching process, mitigating the chance that the higher job impacts are confounded by the magnitude of the pre-period job trajectory. Similar patterns emerge at the commuting zone level, though residential density is a stronger correlate of job impacts. Overall, these findings suggest that among low-income places eligible for CDBG funding, CDBG activities tend to have greater job impacts in comparatively prosperous areas. This suggests that it may be more difficult and costly to reverse the fortunes of declining and distressed places compared to creating opportunities in places on a positive trajectory.

These findings stand in contrast to predictions by Bartik (2015) and Austin, Glaeser and Summers (2018), who argue that place-based policies will have the largest impacts in labor markets that are less well off. I find that the employment-to-population ratio within a local labor market appears to correlate *positively* with the impact of place-based investments funded by the CDBG. However, these previous predictions were based on state- and PUMA-wide regional industry shocks, which

may produce different impacts from more spatially-targeted place-based policies. CDBG investments also specifically target lower-income areas, whereas regional industry shocks impact entire local labor markets

6   Conclusion

This paper provides new evidence on the benefits of decentralizing place-based policy-making via federal block grants to local governments. This paper also provides some of the most direct evidence to date on the determinants of effective place-based policies, a topic with surprisingly few insights within an otherwise rich literature. The structure of the CDBG presents a unique opportunity to study a wide variety of place-based policies and their job impacts in low-income neighborhoods across the nation within a unified empirical and administrative framework.

I first show that CDBG funding has positive and significant effects on job counts in local labor markets. I do so by leveraging a shock to the process used to determine annual formula allocations, and estimate causal effects through synthetic differences-in-differences. Despite these effects, I find limited evidence that funding led to disproportionately better outcomes for low-income people and places. On the other hand, I find that the increase in jobs is disproportionately represented by low-paying jobs.

The cost-per-job implied by these estimates is quite low and suggests that funding from the CDBG may not have been the only source of public funding contributing to the job impacts above. I therefore estimate local public spending multipliers: the amount of local public spending induced by each additional dollar of CDBG funding. Using a simulated instruments approach exploiting the specific structure of the CDBG funding formula, I find that each additional dollar of CDBG funding generates roughly three total dollars of public spending. These estimates bring the implied cost-per-job much closer to cost-effectiveness estimates from other studies on place-based policies.

Finally, I leverage geocoded data on specific activities funded through the CDBG. I use synthetic difference-in-differences to estimate tract-specific causal job impacts where large CDBG investments were made. Among the 500 treatments in my sample, I find large and positive effects of CDBG investments on tract-level job counts. I find that activities focusing on land clearance and investing in microenterprises generated comparatively larger job impacts. For places, I find that conditional on being a low-income neighborhood eligible for CDBG investment, investments tended to produce larger job impacts in comparatively prosperous places. My findings suggest that it may be easier for

place-based policies to create jobs in relatively more affluent places than it is to turn around distressed places.

These results shed light on how the block grant structure can be used as a bridge between the scale of federal programs and the diverse, individual needs of localities across the nation. Federal funding for place-based policies may be particularly valuable to local governments in distressed and financially constrained jurisdictions. As economic disparities across the nation continue to grow, place-based policies will likely play a prominent role in ensuring that all Americans have access to economic opportunity wherever they live. This chapter provides practical insights for understanding how federal policies can be used to effectively create jobs across a wide variety of communities.

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Figure 1: Historical CDBG Appropriations, 1980-2018
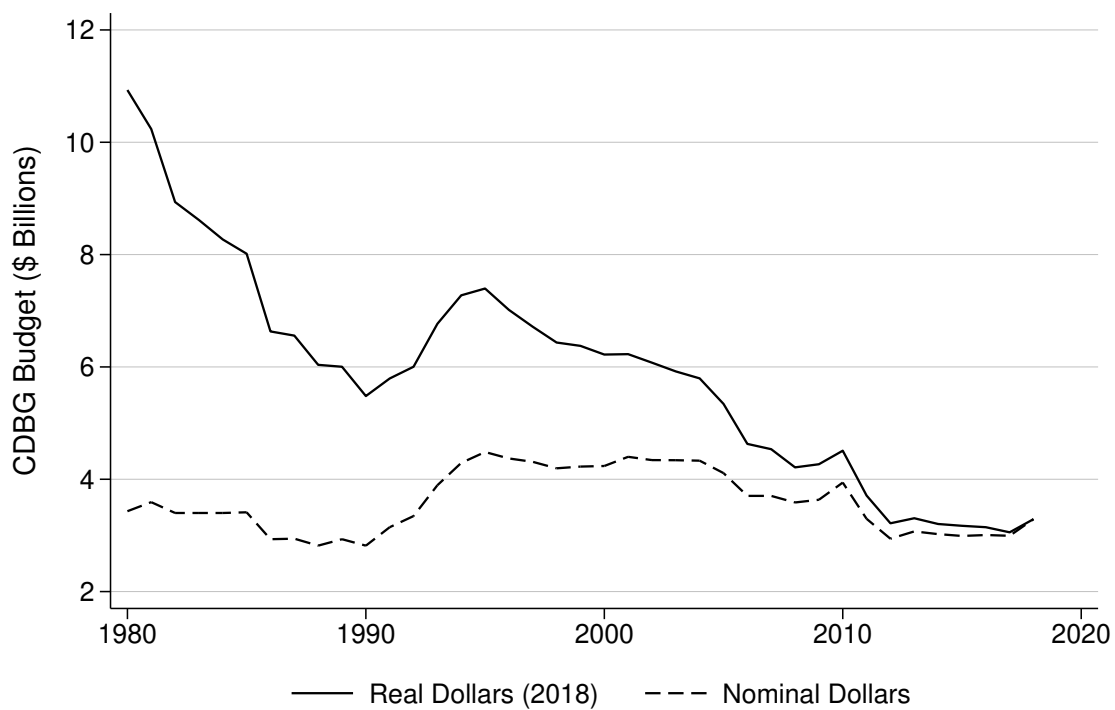
Note: This figure presents the evolution of the CDBG budget appropriated by Congress over time. CDBG budget in real dollars was adjusted relative to the 2018 CPI. Data were obtained from the U.S. Department of Housing and Urban Development.
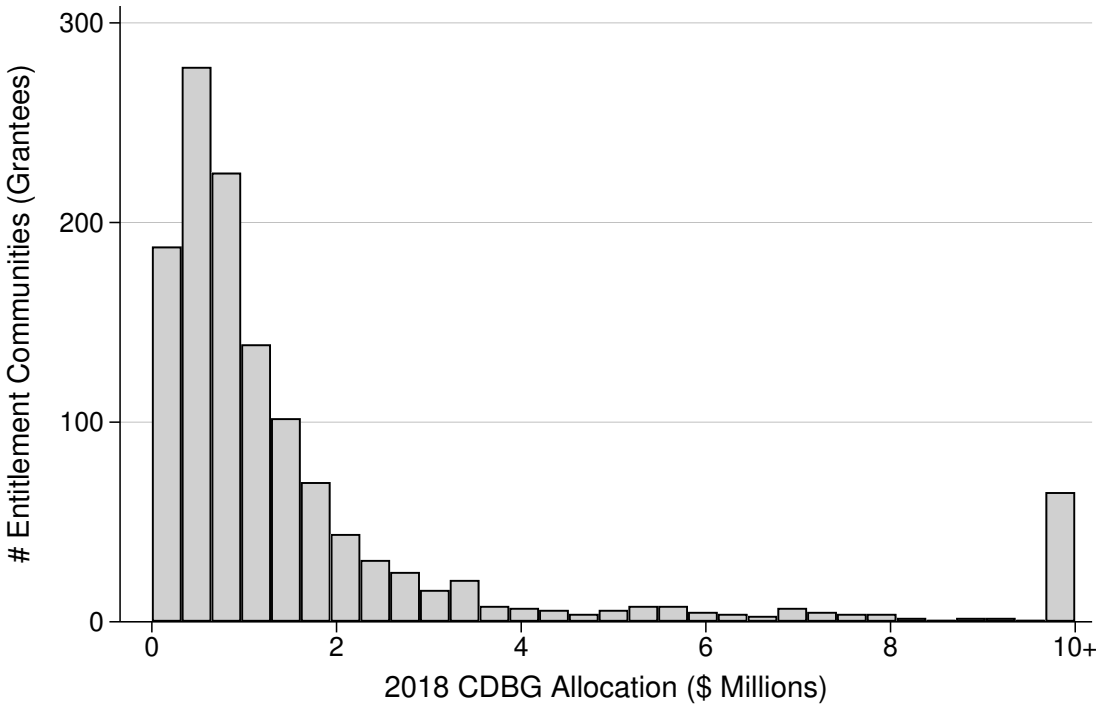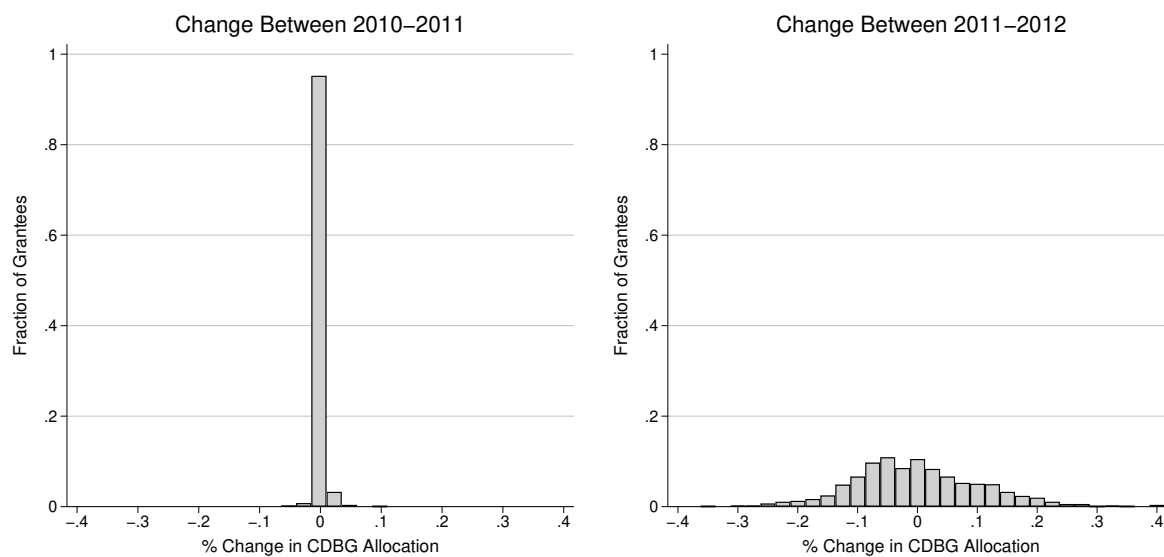
Figure 2: Distribution of Grantee-Level Allocations, 2018

Note: This figure presents the distribution of grantee-level CDBG allocations in 2018. Data come from CDBG allocation spreadsheets provided by the U.S. Department of Housing and Urban Development.

Figure 3: CDBG Year-over-Year Allocation Changes, Before and After Shock

Note: This figure presents the distribution of year-over-year changes in allocation generosity between 2010-2011 and between 2011-2012. Data come from CDBG allocation spreadsheets provided by the U.S. Department of Housing and Urban Development.

Figure 4: How the CDBG data input shock affected CDBG grant allocations
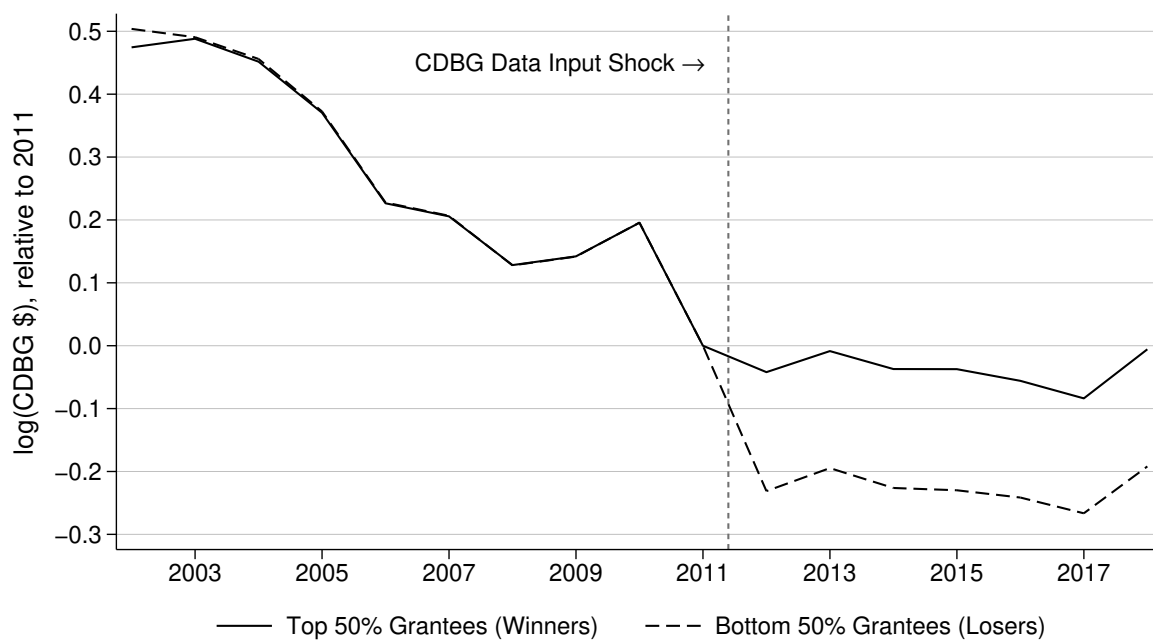
Note: This figure presents the trajectory of CDBG funding for "winning" and "losing" grantees as delineated in Section 5.1. Funding trajectories over time are centered to a baseline of 0 in 2011.

Figure 5: Comparison between DiD and SDID

A. Difference–in–Differences
Estimate: 0.045 (0.009)

B. Synthetic DiD
Estimate: 0.034 (0.006)

Top 50% Grantees (Winners) — — · Bottom 50% Grantees (Losers)
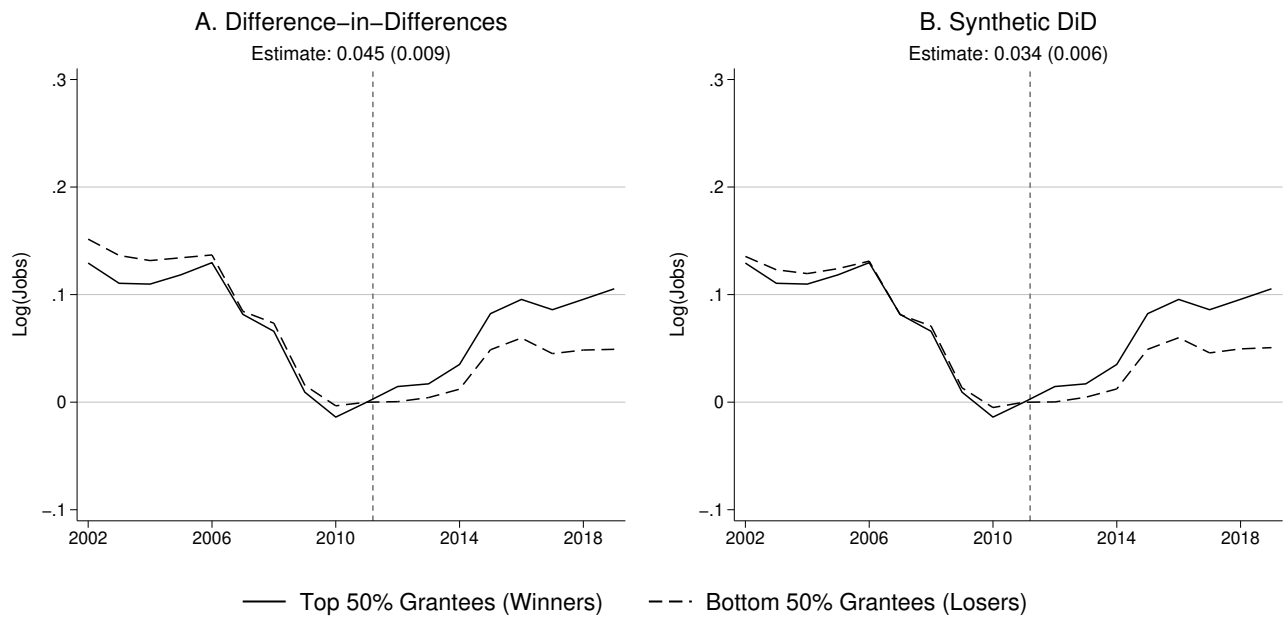
Note: This figure presents difference-in-difference and synthetic DiD estimates of the CDBG data shock on job counts. Job counts reflect the change in job counts relative to levels in 2011.

Figure 6: Effect of CDBG Funding Shock on Local Job Counts
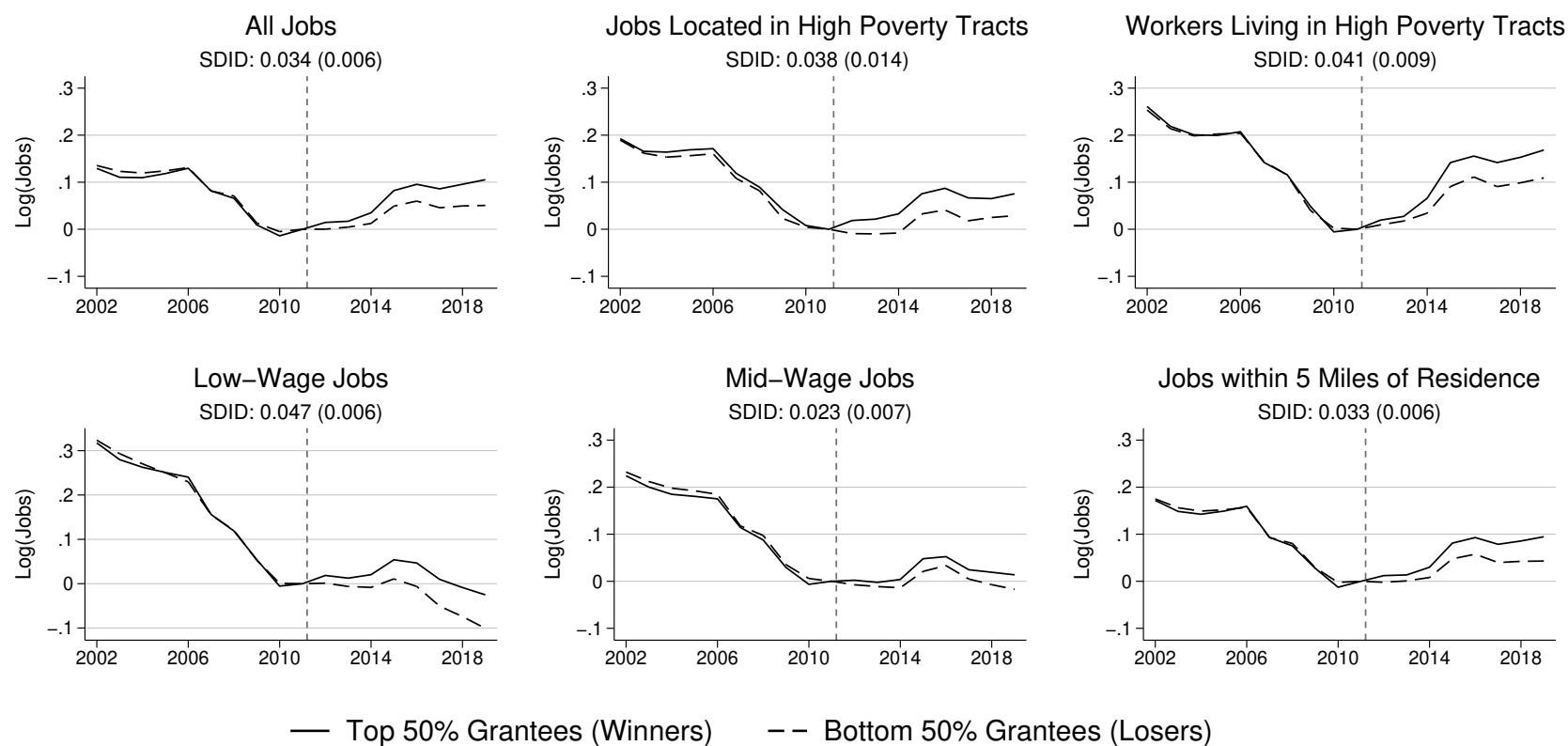
Note: This figure presents the impact of CDBG investments on six different job count outcomes. High poverty tracts denote census tracts where poverty rates exceed the national median. Low-wage jobs reflect jobs where monthly earnings are less than $1,250. Mid-wage jobs reflect jobs where monthlhy earnings are between $1,250 and $3,333.

Figure 7: Correlates of Impactful Investmens
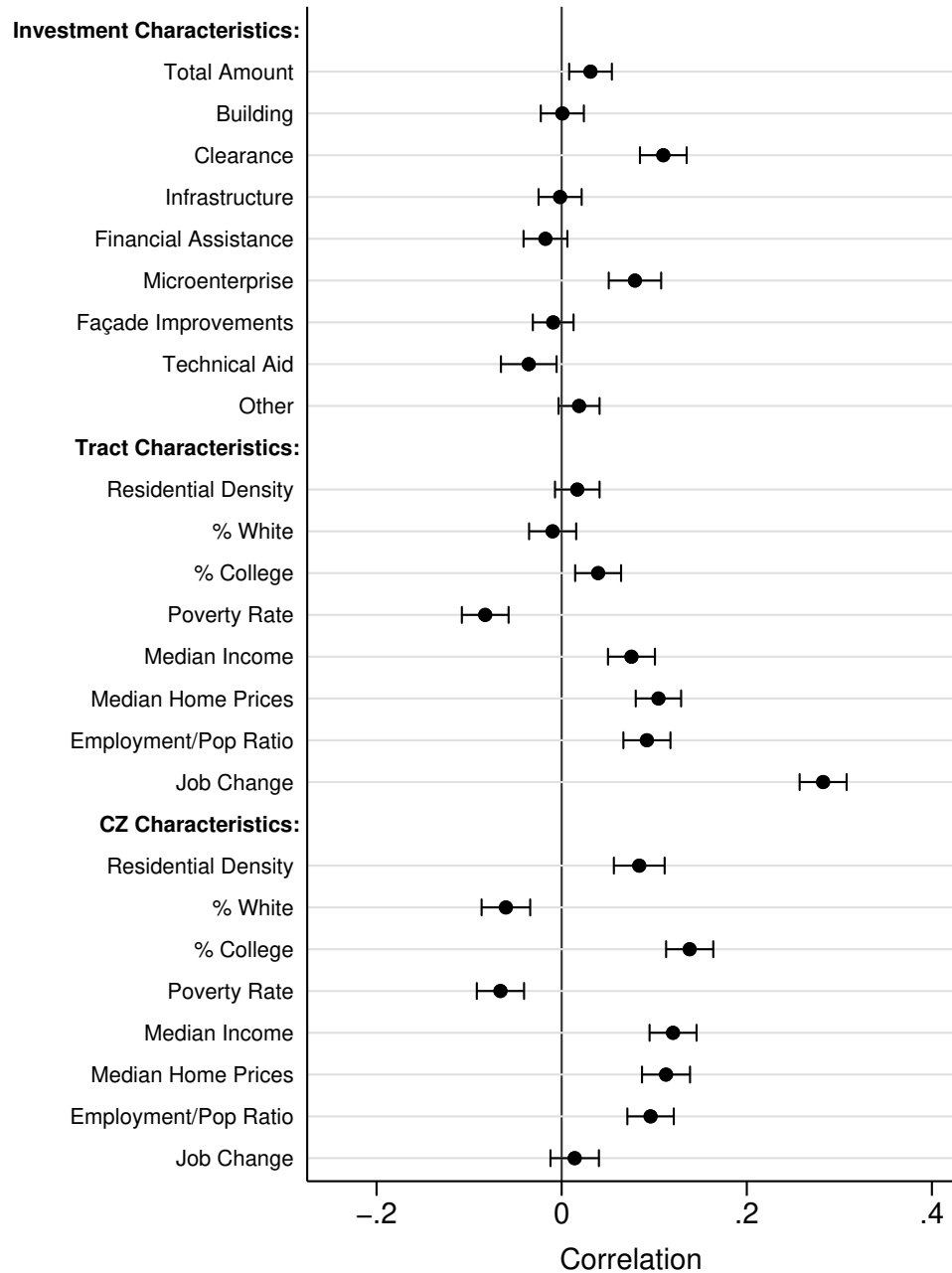
Note: This figure plots variance-weighted least squares regression results between normalized z-scores of tract-level treatment effects and investment/place characteristics.regressions are weighted by the inverse variance of each tract-level estimate, and 95 percent confidence intervals are plotted as well. The sample includes 489 treated tracts for which treatment (i.e., a large CDBG investment) first occurs between 2012 and 2015.

Table 1: Summary of Eligible CDBG Activities

Activity Type	Description	# Investments	Total Invested ($M)	Avg. Investment ($)
Acquisition and Clearance	Acquiring or disposing of land, air rights, easements, water rights, rights-of-way, and buildings	38,438	3,980	103,542
Administrative/Planning	Administration costs related to planning/execution of community development activities. Capped at 20% of annual allocation.	106,136	10,747	101,255
Economic Development	Activities aimed at job creation/retention, establishing/ stabilizing/expanding small businesses; see Section 6	45,848	5,754	125,492
Housing	Construction or rehabilitation of housing, homeownership assistance, inspections/code enforcement	250,629	17,921	71,505
Public Improvements	Acquiring, constructing, or rehabilitating public improvements or facilities (e.g., streets, parks, sewer lines, parking lots, beautfication)	162,788	24,693	151,688
Public Services	Covers labor, supplies, materials, and other costs of various public services (e.g., public safety, homelessness programs, job training, health care, recreation, education, etc.). Spend is capped at 15% of annual CDBG allocation.	223,282	7,888	35,326

Note: This table summarizes the different activities that are eligible for CDBG funding. Activity-level data were obtained from the U.S. Department of Housing and Urban Development.

Table 2: Descriptive CDBG Statistics

	Treatment (Winners)		Comparison (Losers)	
			Unweighted	Synthetic
	Mean	SD	Difference	Difference
Socioeconomic Characteristics				
EPOP Ratio	0.460	0.055	0.002	0.002
HH Income	60,847	18,194	1,740	2,255
% In Poverty	0.169	0.081	-0.011	-0.012
% College Educated	0.286	0.217	-0.032	-0.032
% HS Grad or Less	0.412	0.083	-0.019	-0.017
% In Professional Occupation	0.343	0.121	-0.003	-0.001
Demographic Characteristics				
% White	0.613	0.117	0.019	0.017
% Married	0.475	0.088	0.001	0.003
% Single Mother	0.582	0.043	0.001	0.001
% Working Age	0.152	0.043	-0.004	-0.005
Neighborhood Characteristics				
Median Rent	890	279	82	90
Median Home Value	217,817	123,599	59,028	62,588
% Vacant Housing	0.087	0.043	-0.004	-0.005
Jobs				
All Jobs	39,355	138,078	-7,194	-5,103
Low-Wage Jobs	9,886	30,208	-1,551	-1,036
Mid-Wage Jobs	14,399	44,439	-2,862	-2,146
High-Wage Jobs	15,071	63,883	-2,781	-1,922
Jobs Held by Workers from High-Poverty Tracts	9,437	65,245	-2,210	-1,762
Jobs Located in High-Poverty Tracts	12,518	51,118	-2,079	-1,454
Jobs Held by Residents Living < 5mi Away	21,181	63,914	-2,916	-1,961

Note: This table summarizes differences across grantees in the treatment and comparison group in Section 5.1. The number of grantees in each group is equal, and grantees are assigned a group based on the percentage change in CDBG allocation generosity between 2011 and 2012. Columns 3 and 4 show two different aggregations of control units: the former column takes an unweighted average of all comparison grantees. The latter column weights comparison groups based on the unit weights assigned by synthetic difference-in-differences.

Table 3: Estimates of CDBG Local Public Spending Multipliers

Outcome (Per Capita):	OLS HCD Spend	Reduced Form HCD Spend	First Stage CDBG Allocation	Simulated IV HCD Spend
Per-capita CDBG	2.00** [0.37-3.64]			
Per-capita Simulated CDBG		1.86** [0.29-3.44]	0.59*** [0.50-0.68]	3.16** [0.57-5.76]
N	3,990	3,990	3,990	3,990
F-Stat				166
Average Per-Capita CDBG	11.29	11.29	11.29	11.29
Average Per-Capita HCD Spend	74.76	74.76	74.76	74.76

Note: This table presents estimates of the fiscal multipliers generated by the CDBG. I relate per-capita public spending on housing and community development with per-capita CDBG allocations via Regression (7). For reduced form, first stage, and IV estimates, I instrument for per-capita CDBG allocations using a simulated instrument derived from the CDBG funding formula. The instrument interacts formula inputs pinned to an initial pre-period with input "prices" representing the dollar value of additional unit of input. The formula for the instrument is shown in Equation (11) and is derived in Section 4.1. 95 percent confidence intervals are presented in brackets, based off of standard errors that are adjusted for clustering at the grantee level. $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4: Descriptive Activity-Level CDBG Statistics

| Project Type | A. CDBG Economic Development Activity (2000-2016) | | | B. CDBG Treatments (3-Year Total) | | |
	(1) Avg. Activity ($)	(2) # Activities	(3) # Tracts	(4) Avg. Treatment ($)	(5) Avg. # Activities	(6) # Tracts
Land Acq. & Clearance	50,000	13,387	2,792	62,277	1.09	314
Comm./Ind. Construction	205,850	745	452	17,628	0.06	70
Exterior Improvements	51,626	4,680	1,385	19,898	0.22	138
Financial Assistance	118,257	10,462	3,658	140,082	0.74	612
Infrastructure	379,879	467	316	30,892	0.06	76
Micro-Enterprise	47,958	5,827	1,980	28,973	0.44	232
Technical Assistance	105,178	2,233	714	12,214	0.11	85
Non-Profits and Other	82,729	5,142	1,245	19,092	0.25	157
All Economic Development	79,609	42,943	8,812	331,057	2.98	1,375

Note: This table summarizes economic development projects funded by the CDBG. Panel A summarizes all CDBG activity across tracts in the sample. Column (1) calculates the size of an average project for each category. Column (2) provides the total count of each project category throughout the sample. Column (3) provides the number of unique tracts that received CDBG investments. Panel B characterizes treatments used in the synthetic controls analysis. Treated tracts represent the set of tracts where the 3-year running sum of investments falls within the top 50 percent of all similarly defined treatments. Treated tracts are also restricted to tracts where the treatment date allows for sufficiently many pre- and post-treatment observations. Column (4) describes the average treatment size for tracts in this sample. Column (5) provides the total number of projects represented by treatments defined in this way. Columm (6) shows the number of treated tracts with at least one project in each category.
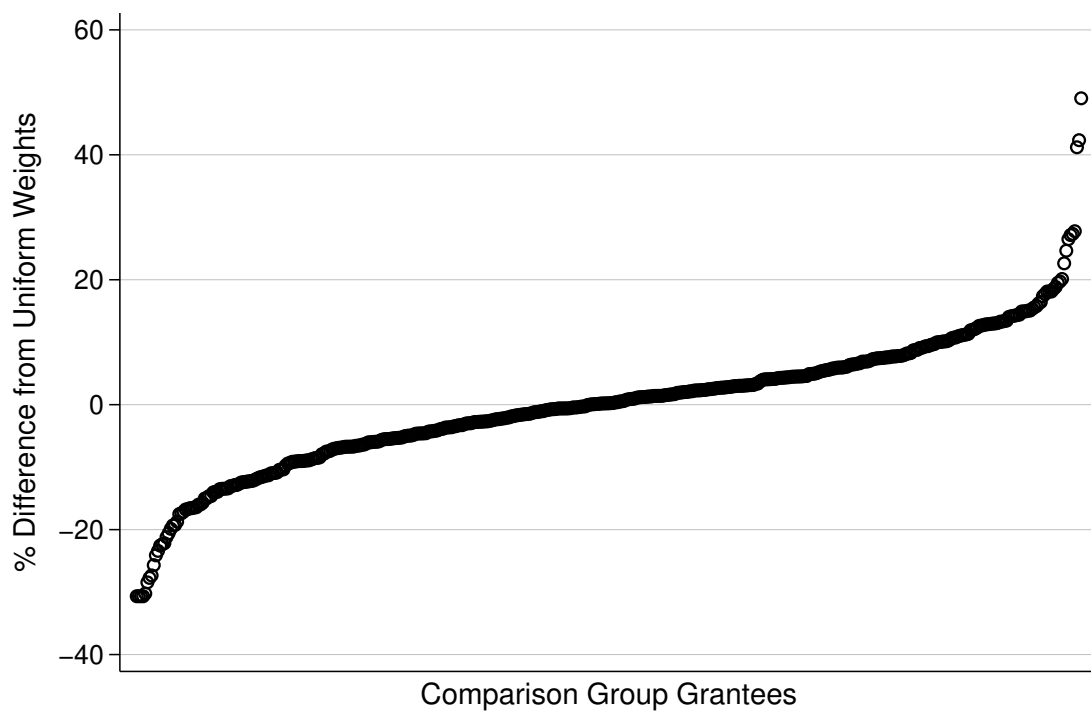
Appendix A: Appendix Tables and Figures

Figure A1: Changes in CDBG funding, uncentered

CDBG Input Shock →

Top 50% Grantees (Winners) --- Bottom 50% Grantees (Losers)
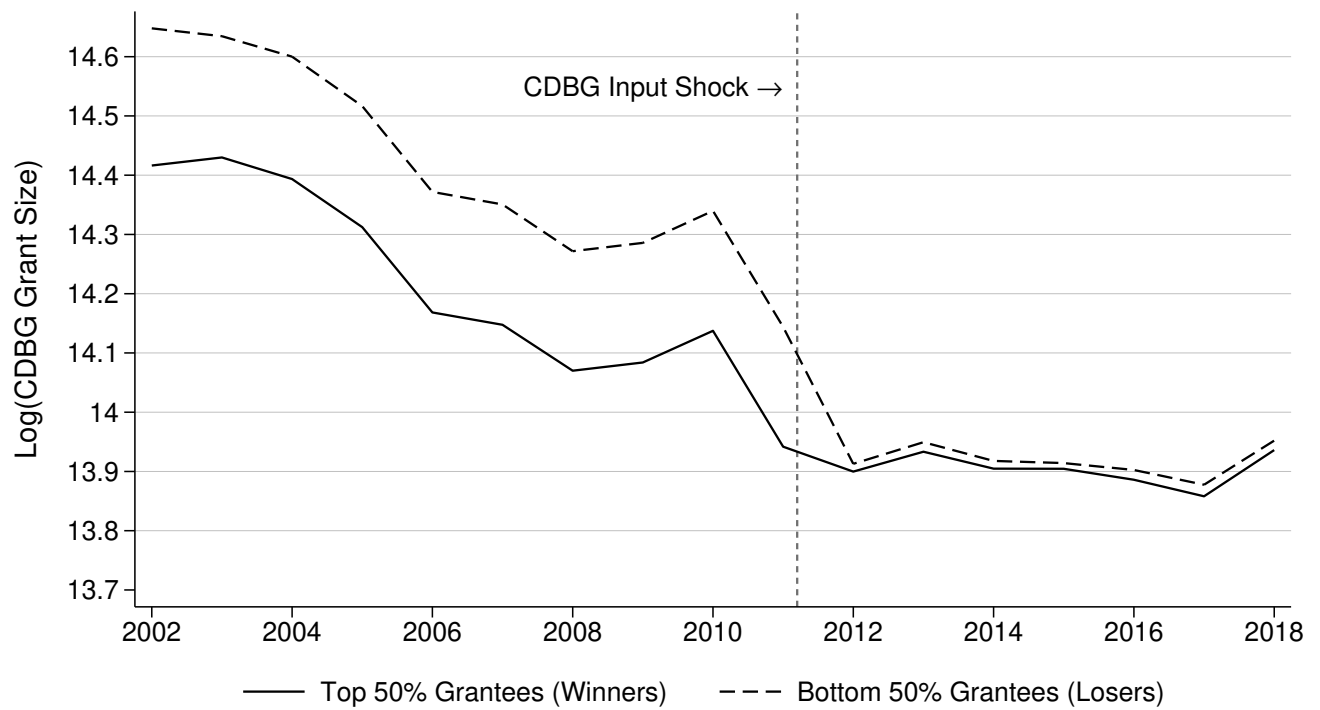
Note: This figure provides the uncentered version of Figure 4, minus the centering to show differences relative to 2011 levels.

Figure A2: SDID Comparison Group Weights

Note: This figure plots the weights underlying the synthetic control, relative to what uniform weights would imply.

Figure A3: CDBG Formula Weights, 2012-2017
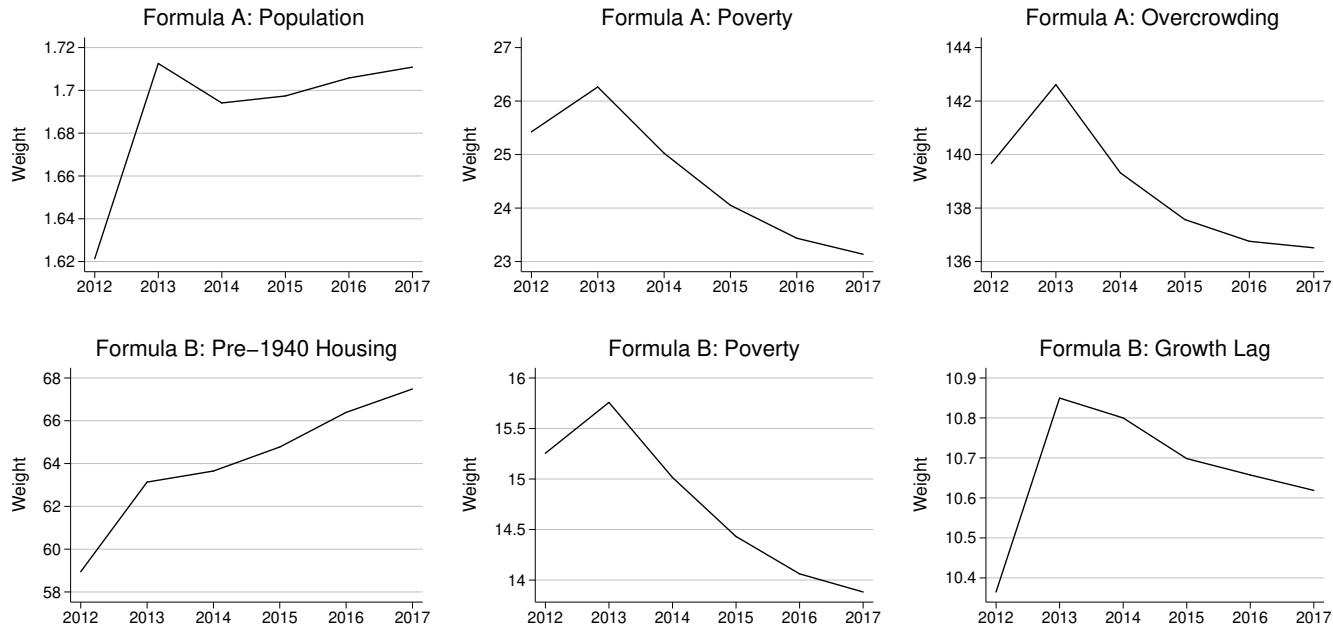
Note: This figure presents the evolution of national input values v_t^k from Equations (9) and (10). The input values represent the CDBG dollar value of an additional unit of each corresponding input. Calculations were made using spreadsheets with data on annual CDBG calculations from 2011-2017, provided by the U.S. Department of Housing and Urban Development.

Figure A4: Temporal Correlation of CDBG Investments
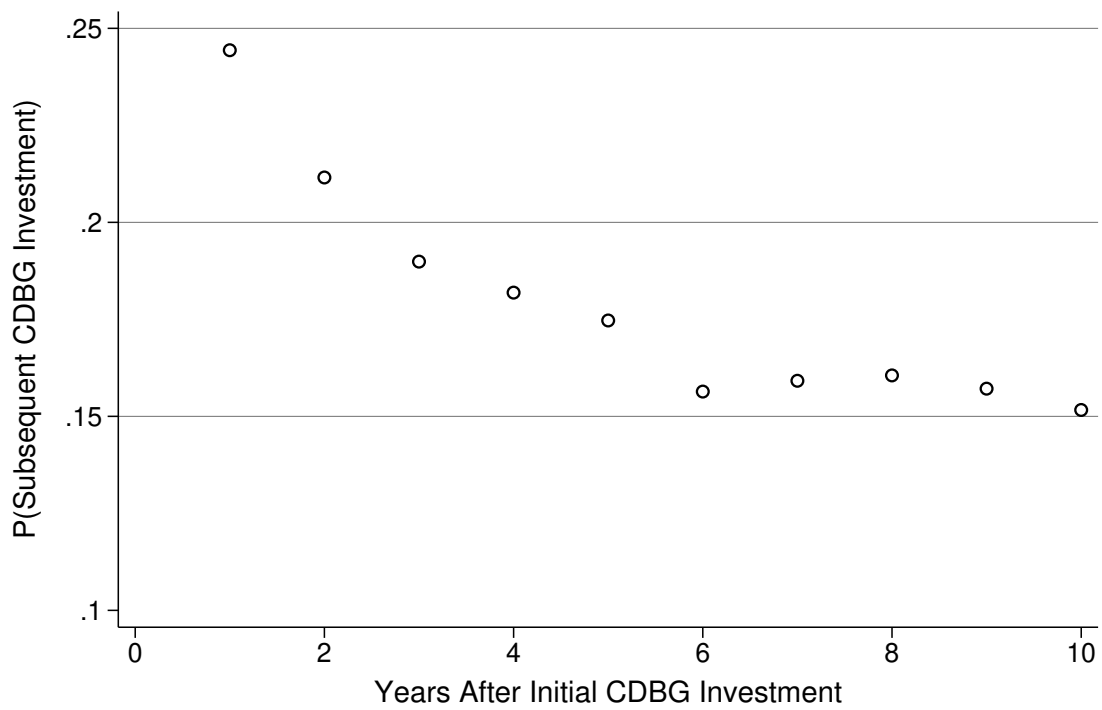
Note: This figure shows the proportion of treated tracts that receive a subsequent investment t years after initial investment.

Table A1: Shared Occurrences of Project Categories

	Clearance	CI Construction	Financial Assistance	Infrastructure	Micro-Enterprise	Exterior Improvements	Technical Aid	Non-Profit and Other	Tracts
Clearance	1.00	0.04	0.18	0.02	0.09	0.07	0.05	0.05	168
CI Construction	0.13	1.00	0.31	0.04	0.15	0.09	0.15	0.04	54
Financial Assistance	0.08	0.04	1.00	0.02	0.12	0.04	0.08	0.03	386
Infrastructure	0.06	0.03	0.11	1.00	0.02	0.00	0.05	0.02	66
Micro-Enterprise	0.11	0.06	0.35	0.01	1.00	0.07	0.19	0.07	132
Exterior Improvements	0.14	0.06	0.20	0.00	0.11	1.00	0.21	0.07	84
Technical Aid	0.09	0.08	0.30	0.03	0.24	0.17	1.00	0.16	104
Non-Profit and Other	0.20	0.05	0.30	0.03	0.23	0.15	0.43	1.00	40
Tracts	168	54	386	66	132	84	104	40	1,034

Note: This table shows the proportion of treatments with at least one project category in row i that also has a project category in column j.

Appendix B: Synthetic Control Justification

My approach to estimating tract-specific effects of the CDBG is to use the *intercept-shifted* synthetic control method (Doudchenko and Imbens, 2016; Arkhangelsky et al., 2021; Ben-Michael, Feller and Rothstein, 2019). When applied to a single treatment unit, the traditional synthetic control method (SCM) estimates the trajectory of the counterfactual untreated outcome by taking a weighted (synthetic) average of outcomes from untreated units, where the weights are chosen to match the treated unit's pre-treatment outcomes as closely as possible (Abadie, Diamond and Hainmueller, 2010). The overarching intuition is that a weighted combination of untreated tracts may represent a more appropriate comparison group than any single tract or unweighted combination of untreated tracts. If pre-treatment outcomes closely match, the bias of the SCM estimate can even be bound under the assumption that outcomes are generated by a linear factor model — a generalization of assumptions made by standard differences-in-differences. This makes good pre-treatment fit crucial and justifies the *intercept-shift* adjustment to traditional SCM, which can improve fit in settings where excellent fit is otherwise unobtainable. I begin by describing the general intercept-shifted SCM setup before moving to data assumptions specific to the CDBG setting.

SCM Setup: I observe a panel for $i = 1, ..., N$ census tracts over the years $t = 1, ..., T$. Some tracts, denoted by the time-invariant indicator $W_i = 1$, receive a CDBG investment for economic development at some point during the panel. I separately index this set of ever-treated units using $j = 1, ..., J$. I require treatment to be an absorbing state, so census tracts remain treated for the remainder of the panel after receiving a CDBG investment for the first time. In reality, an investment in one period may lead to an endogenous response of additional CDBG (or even external) investments in subsequent periods. My estimates therefore represent the overall effect of an initial CDBG investment while allowing for any future endogenous responses. I return to this issue in the "Implementation" section below. I denote T_j as the year in which census tract j becomes treated.

I estimate effects separately for each year after treatment begins. For a given treated tract j, I index "event time" k relative to the treatment year T_j, where $k = t - T_j$. Event time is negative prior to treatment and is zero in the year when treatment begins. Using potential outcomes notation, the treatment effect for treated tract j at post-treatment event time $k \geq 0$ is:

$$\tau_{jk} = Y_{j,T_j+k}(1) - Y_{j,T_j+k}(0) \tag{12}$$

The treated potential outcome $Y_{j,T_j+k}(1)$ is observed for all treated units after treatment. The empirical challenge is to estimate $Y_{j,T_j+k}(0)$, the unobserved value of the outcome that would have occurred in the absence of treatment. The SCM attempts to approximate the counterfactual outcomes of a treated unit by using a weighted average of untreated units with similar pre-treatment characteristics. Denoting the set of potential donor control units associated associated with treated tract j as $\mathcal{D}_j$ and indexing donor units for that tract as $d_j = 1_j, ..., D_j$, the synthetic control unit for tract j is formed by applying an $D_j \times 1$ vector of weights $\mathbf{W_j} = (w_{1_j}, .., w_{D_j})'$ to the set of donor control units to form an estimate of the counterfactual outcome:

$$\hat{Y}_{j,T_j+k}(0) = \sum_{d_j=1}^{D_j} w_{d_j} Y_{d_j,T_j+k} \tag{13}$$

Note that this notation allows D_j, the total number of donor candidates, to vary for each treated tract j. The estimate of the treatment effect k years after treatment for treated tract j is then:

$$\hat{\tau}_{jk}^{scm} = Y_{j,T_j+k} - \hat{Y}_{j,T_j+k}(0) \tag{14}$$

where inference is conducted empirically using jackknife standard errors.[33,34]

In this application of the SCM, I calculate weights to minimize the squared imbalance across the $T_j - 1$ lags of the outcomes (indexed by $\ell = 1, ..., T_j - 1$) between the treatment tract and the synthetic control:

$$\min_{W_j \in \Delta_j^{scm}} \frac{1}{2(T_j - 1)} \sum_{\ell=1}^{T_j-1} \left(Y_{j,T_j-\ell} - \sum_{d_j=1}^{L_j} w_{d_j} Y_{i,T_j-\ell} \right)^2 \tag{15}$$

where the weights in $\mathbf{W_j}$ are conventionally non-negative and sum to one.[35] This characterization

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[33]I use the "jackknife+" procedure proposed by Barber et al. (2019), which improves upon traditional jackknife standard errors by constructing the confidence interval around the *median* of all leave-one-out estimates, as opposed to centering the interval around the estimate only. While usually very similar in practice, this modification performs better when the SCM estimate is unstable.

[34]Another method of conducting inference in the SCM setting is via permutation (or "placebo") tests. This approach assigns a placebo treatment status to each of the donor tracts and and re-computes the synthetic control algorithm for every donor tract. P-values are then calculated by comparing the size of the treatment estimate to the distribution of placebo treatment estimates. I opted not to use permutation-based inference for two reasons. The first is practical in nature: given that I compute SCM estimates separately for hundreds of treated tracts, the number of placebo calculations will increase multiplicatively as the number of donor tracts increases. To illustrate, with 700 treated tracts and an average of 50 donor units per tract, the analysis would require 35,000 separate runs of the SCM. The second reason is that the permutation test typically assumes that treatment is randomly assigned—which is not the case in the CDBG setting. I instead follow Arkhangelsky et al. (2021) and use the leave-one-unit-out jackknife approach to empirically quantify uncertainty.

[35]A number of papers including Ben-Michael, Feller and Rothstein (2018) and Doudchenko and Imbens (2016) have recently suggested that allowing weights to be negative while incorporating a penalty function to constrain the optimization

of the optimization problem differs slightly from the original characterization proposed by Abadie, Diamond and Hainmueller (2010) in that it includes only lagged outcomes as predictors and does not use other covariates. This modification has become standard in many recent applications of the SCM and is necessary for the intercept-shift modification, as I describe further below.

The key empirical challenge underlying SCM is determining whether $\hat{Y}_{j,T_j+k}(0)$ is a reasonable estimate for $Y_{j,T_j+k}(0)$, the counterfactual outcome for tract j in the absence of treatment. Abadie, Diamond and Hainmueller (2010) and Abadie (2021) show that the bias of the SCM estimate can be bound when the outcome follows a linear factor model structure, which is a generalization of the structure assumed in standard differences-in-differences. Specifically, suppose that potential outcomes follow the following linear factor structure:

$$\begin{cases} Y_{it}(0) = \delta_t + \lambda_t \mu_i + \epsilon_{it} \\ Y_{it}(1) = \alpha_{it} + Y_{it}(0) \end{cases} \tag{16}$$

where δ_t represents an unknown common factor with constant factor loadings, λ_t represents a $(1 \times F)$ vector of common factors, μ_i represents an $(F \times 1)$ vector of unknown factor loadings representing unobserved features of specific census tracts. This structure allows the outcome Y_{it} to depend on multiple unobserved components μ_i that may have time-varying impacts on outcomes via the coefficients λ_t.[36]

The basic justification for synthetic controls under this structure is as follows. If the synthetic control for a given treated unit j manages to closely reproduce the pre-treatment outcomes $Y_{jt}(0)$ for $t < T_j$ but does not reproduce the values of μ_j in Equation (16), then it must be the case that the individual transitory shocks ϵ_{it} are exactly compensating for the differences in unobserved factor loadings in each pre-treatment period. This scenario becomes more and more unlikely as the number of pre-treatment periods increases, or as the scale of transitory shocks decreases. Abadie, Diamond and Hainmueller (2010) derive a bound for the bias of the SCM estimate as a function of these two parameters. Taking into account that pre-treatment fit is rarely perfect, Ben-Michael, Feller and Rothstein (2018) bound the bias as an increasing function of 1) imbalance in the pre-treatment outcomes, and 2) approximation error from balancing lagged outcomes instead of the latent factors themselves, which decreases to zero as the number of pre-treatment periods approaches infinity. To summarize, the reli-

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problem will improve the fit of the synthetic control; negative weights may not be normatively undesirable, especially if the outcome of the treated unit lies beyond the convex hull of the potential donors.

[36]This general structure resolves to basic differences-in-differences when λ_t is constant over time.

ability of the SCM estimate increases with excellent pre-treatment fit, and is adversely affected when the chance of overfitting is high due to a small number of pre-treatment periods and high variance in ϵ_{it}.

Despite the crucial importance of excellent pre-treatment fit, achieving such fit can be difficult in practice. When pre-treatment fit is imperfect, a number of recent papers have suggested modifying traditional SCM by de-meaning the outcome variable for all units using their pre-treatment averages, which is referred to as "intercept-shifted" or "de-meaned" SCM (Doudchenko and Imbens, 2016; Ferman and Pinto, 2019; Ben-Michael, Feller and Rothstein, 2018, 2019; Arkhangelsky et al., 2021).[37] The basic intuition for this modification maps to the intuition of standard differences-in-differences: the SCM assumption requiring levels of the outcome to match is relaxed in favor of a common trends assumption by allowing treatment and control outcomes to vary by a constant intercept shift. With this modification, the intercept-shifted SCM only distinguishes itself from differences-in-differences in that the weights on control units are allowed to vary instead of being uniform across all units. The estimated treatment effect τ_{jk} using this approach is as follows:

$$\hat{\tau}_{jk} = \frac{1}{T_j - 1} \sum_{\ell=1}^{T_j - 1} \left[(Y_{j,T_j+k} - Y_{j,T_j-\ell}) - \sum_{d_j=1}^{D_j} w_{d_j}(Y_{d_j,T_j+k} - Y_{d_j,T_j-\ell}) \right] \tag{17}$$

This "intercept-shifted" or "de-meaned" SCM estimator has a number of attractive properties. First, Ben-Michael, Feller and Rothstein (2018) interpret the estimator as "augmenting" SCM with unit fixed effects, and show that the resulting estimator has far better performance in terms of pre-treatment fit than traditional SCM estimates. Second, Ferman and Pinto (2019) show how the de-meaned SCM, a generalization of standard differences-in-differences, dominates differences-in-differences in terms of variance and bias in many settings. Finally, the de-meaned SCM exhibits "double-robustness" properties (Arkhangelsky et al., 2021), where the estimator performs well if either differences-in-differences or traditional SCM provides a suitable counterfactual.

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[37]It is worth noting that Doudchenko and Imbens (2016) show that the reason this modification cannot occur in the standard SCM presented by Abadie, Diamond and Hainmueller (2010) is due to the presence of auxiliary covariates aside from pre-treatment outcomes that are uesd to construct the synthetic control, which introduces differences in scale that preclude the use of non-zero intercepts.