

# Getting Beneath the Hood of Effective Place-Based Policies: Evidence from the Community Development Block Grant\*

George W. Zuo<sup>†</sup>

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## Abstract

Growing economic disparities across the U.S. have increased the need for effective place-based policies. This paper seeks to uncover determinants of effective place-based investments by analyzing the job impacts of thousands of spatially targeted investments made by local governments to spur economic development in low-income areas, funded by \$3-4 billion in annual federal block grants from the Community Development Block Grant. Using a hybrid approach combining synthetic control methods with traditional differences-in-differences, I find that job counts increase by 7% over ten years in census tracts where large CDBG investments occurred, at a cost of approximately \$18,000 per job created. Investments disproportionately benefited low-income workers living within two miles of the investment location. Job impacts are greatest when investments are made to support local firms through financial and technical assistance, land clearance, and commercial/industrial construction. Investments appear to have larger impacts in commuting zones that are less affluent and less dense, but individual municipalities are best off targeting less-disadvantaged neighborhoods. I verify that block grants do not crowd out public spending and estimate that each dollar of block grant generates approximately three dollars of public spending.

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<sup>†</sup>University of Maryland, 3114 Tydings Hall, 7343 Preinkert Dr., College Park, MD 20742. Email: [gzuo@umd.edu](mailto:gzuo@umd.edu)

# 1 Introduction

America is in the midst of a “Great Divergence” characterized by large and growing disparities in jobs, income, poverty, and mortality across cities and communities nationwide (Moretti, 2012). Federal, state, and local governments currently spend \$60 billion annually on spatially-targeted “place-based” policies to create jobs in low-income and economically stagnant areas (Bartik, 2020b). However, the playbook on effective place-based policymaking still remains underdeveloped in a variety of dimensions. Many place-based policies continue to adopt one-size-fits-all approaches to economic development without considering the vast local heterogeneity across economically distressed communities throughout the country (Austin, Glaeser and Summers, 2018). Perhaps more fundamentally, little systematic evidence exists on what factors make a place-based investment more likely to succeed in creating jobs for local residents.

This paper presents numerous new insights on effective place-based policymaking by synthesizing causal job impacts across thousands of federally funded but locally implemented place-based investments. I do so by analyzing the Community Development Block Grant (CDBG), a federal program which distributes \$3-4 billion in annual block grants to local governments across the country. Local governments can flexibly use their annual block grants to fund a wide variety of economic and community development activities in low-income neighborhoods. The CDBG represents an approach to federal place-based policymaking that is rooted in fiscal federalism, combining the scale of federal programs with the benefits (and pitfalls) of decentralization and tailoring policies to local heterogeneity. The breadth and flexibility of the CDBG also provides an unprecedented opportunity to analyze and compare many different kinds of place-based policies within a common empirical and administrative framework.

I begin by estimating localized job impacts in census tracts where CDBG investments are made. As usual, treated tracts differ endogenously from untreated tracts across a variety of dimensions. However, in this setting, the investments themselves are also endogenous to the tracts receiving them. The size, mix, and timing of investments will likely depend on tract characteristics and preferences of individual local governments. This complicates the use of standard differences-in-differences and event study approaches. Differences in selection across treated tracts makes it difficult to justify any combination of comparison tracts as satisfying the parallel trends assumption, even if pre-trends appear parallel. Treatment effects are also likely to be heterogeneous across individual treatments,

greatly exacerbating issues highlighted by recent criticisms of two-way fixed effect methods with heterogeneous treatment effects and staggered adoption (de Chaisemartin and D’Haultfoeuille, 2020; Goodman-Bacon, 2018; Sun and Abraham, 2020; Callaway and Sant’Anna, 2019).

Given these issues, I attempt to construct a separate counterfactual comparison tract for each individual treated tract. I use the synthetic control method (SCM), an approach that generates a counterfactual for a single treated unit using a weighted combination of untreated units that reproduces pre-treatment outcomes as closely as possible (Abadie, Diamond and Hainmueller, 2010). In the CDBG context, I construct a synthetic control tract for each treated tract by using a weighted subset of untreated donor tracts from the same commuting zone, limiting to tracts with attributes that are similar to those of treated tracts. When the SCM produces a well-fitting comparison for a treated tract, the potential bias of the resulting estimate can be bounded under fairly conservative assumptions (Abadie, Diamond and Hainmueller, 2010; Abadie, forthcoming). Recent developments in the literature encourage the use of an *intercept-shifted* (or *de-meaned*) variant of the SCM to improve pre-treatment fit. This approach simultaneously extends the SCM by allowing pre-treatment imbalance between treated and synthetic control tracts to differ by a constant intercept shift, while also generalizing differences-in-differences by allowing control tracts to contribute via non-uniform weights (Doudchenko and Imbens, 2016; Ben-Michael, Feller and Rothstein, 2018; Ferman and Pinto, 2019). The estimator exhibits “double-robustness” properties (Ferman and Pinto, 2019; Arkhangelsky et al., 2019) and performs well if either differences-in-differences or synthetic controls provides a suitable counterfactual.

When focusing on the top 50% of CDBG treatments by size, federally-funded and locally-implemented place-based investments in economic development generate positive and persistent effects on tract-level job counts.<sup>1</sup> After 10 years, job counts in treated tracts are greater by roughly 107 jobs relative to job counts in counterfactual tracts (a 7.3% increase). The effect is mostly driven by jobs held by workers who live in lower-income tracts, and who live within five miles of the treated tract. While the initial increase in jobs is driven by low-wage jobs, higher-paying jobs begin materializing close to the ten-year mark. I also observe similar job growth in adjacent untreated tracts, suggesting that CDBG investments generate positive job spillovers in nearby areas. Finally, I find that home prices do not change and rents increase by roughly 3%. These findings suggest that CDBG investments tend to be welfare-improving for local workers (Kline and Moretti, 2014).

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<sup>1</sup>As I explain in Section 3.1, my sample is unlikely to be sufficiently powered to detect job impacts from treatments smaller than this (with a median size of \$80,000).

The CDBG funds a wide variety of economic development activities, providing a promising setting to conduct a comparative analysis of place-based policies. I find that a variety of investments are associated with positive job impacts. Among these investment types, the most impactful categories involve financial and technical assistance to for-profit businesses, or subsidizing construction for commercial and industrial construction. Infrastructure appears to produce relatively little impact on jobs despite being the most costly activity on a per-investment basis. Additionally, I verify that although the mix of investment types exhibits changes over time, CDBG impacts remained positive and significant among treatments throughout the panel. Taken as a whole, these findings suggest that investments which directly support local firms tend to generate the largest impacts. Notably, the CDBG does not fund local financial incentives to attract firms to specific locations, 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. Tracts with better amenities may be more conducive to commercial and industrial activity, and residents in these neighborhoods may be more responsive to changes in labor demand. I also show that CDBG investments produce positive benefits regardless of whether the tract had been on a positive or negative trajectory prior to treatment; however, the effects are notably larger in tracts on the rise. Finally, I analyze characteristics the effectiveness of the CDBG across different local labor market characteristics. CDBG investments tend to perform better in more disadvantaged and less densely populated commuting zones.

Importantly, these estimates cannot distinguish between CDBG investments and other endogenous public investments that may subsequently follow. This is an issue that is common in the place based literature (for example, see [Kline and Moretti \(2013\)](#)), and although it does not impact the internal validity of the empirical approach, it does change the assessment of costs and benefits associated with the CDBG. This motivates an analysis of how much total public spending is generated by each dollar of CDBG funding. This exercise is also useful for determining whether the CDBG could potentially crowd out existing public spending. My approach exploits exogenous variation in the CDBG's allocation formula to construct an instrument for each grantee's CDBG allocation. Using data on local public spending from the Annual Survey of State and Local Government Finances, I find that every dollar of CDBG funding leads to approximately \$3.16 of public spending on community development

and housing. A fiscal multiplier of this size suggests that the CDBG may be addressing frictions that prevent 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.

Using these estimates, I conduct a back-of-the-envelope calculation suggesting that each job created is associated with approximately \$25,000 in public spending. This suggests that the CDBG is relatively cost-effective compared to other programs that have been studied, including cash incentives for firms to locate in specific places (\$196,000 per job created), the Tennessee Valley Authority (\$77,000), customized job training (\$15,000), and cleanup of contaminated industrial sites (\$13,000) (Bartik, 2020b). While decentralized place-based policies funded by the CDBG are not strictly the most cost-effective among these programs, the flexibility and national scope of the block grants in conjunction with their ease of administration represents a uniquely promising opportunity to stimulate job growth in a wide variety of economically disadvantaged communities.

## 2 Place-Based Policies and the CDBG

### 2.1 A Brief Overview of the CDBG

The CDBG was created by the Housing and Community Development Act of 1974 in an effort to consolidate numerous existing categorical programs for community development activities. The program departed from a long tradition of federal programs dictating how states and localities could spend federal funds (Theodos, Stacy and Ho, 2017). Instead, the CDBG enabled individual localities to flexibly fund a broad range of activities for improving low-income neighborhoods. While the CDBG's decentralized and flexible approach to place-based redistribution has historically enjoyed bipartisan support, the program has faced recent scrutiny over its lack of transparency and accountability. The Trump administration proposed outright elimination of the program over frequent allegations of waste, abuse, and fraud by local governments.

Each fiscal year, the funding cycle begins with the congressional appropriations process to determine the program's annual budget. Figure 1 shows that while funding for the CDBG has been nominally stable at \$3-4 billion over the past three decades, program funding has declined substantially in real dollars. After the budget is finalized, the Department of Housing and Urban Development (HUD) is responsible for allocating CDBG funds to grantees known as "entitlement communities". Each en-

titlement community represents a city or county government with a minimum population of 50,000 and 200,000, respectively. The amount that each grantee is entitled to is determined by an allocation formula that is a function of local need, approximated by population, population growth, poverty, and housing. The largest recipients are typically dense cities with high poverty concentrations and limited housing supply, as well as cities in rapid decline. Although I do not directly use the allocation formula to identify the job impacts of the CDBG, I return to it when estimating public spending multipliers in Section 5.2. Allocated amounts vary enormously across grantees and are roughly log-normally distributed (see Figure 2). New York City receives approximately \$150M each year, whereas smaller, 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.<sup>2</sup>

After funds are allocated, local governments determine their own funding priorities and methods for distributing funds. The main restriction is that 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).<sup>3</sup> The service area calculation is automated via an internal database which calculates geographic LMI concentrations using the American Community Survey.<sup>4</sup>

The CDBG funds a wide variety of community development activities which include: 1) property and land acquisition/demolition, 2) economic development, 3) housing rehabilitation, 4) public services for youth and under-served populations, 5) public improvements (e.g. street/sewer improvements, parks, beautification, etc.), and 6) administrative costs. Expenditures for public services and administration are capped at and 15 and 20 percent of each grantee's annual allocation. In this paper, I focus on economic development activities, which map most directly to conventional definitions of

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<sup>2</sup>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.

<sup>3</sup>AMI is calculated separately for each metropolitan area or non-metropolitan county in the ACS.

<sup>4</sup>An activity's service area is delineated at the census tract level. Prior to the ACS, the calculations were conducted using the decennial census.

place-based jobs policies. Economic development projects can be further categorized as follows:<sup>5</sup>

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.<sup>6</sup>
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, 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.

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<sup>5</sup>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>.

<sup>6</sup>Land-related activities are substantially less common than building-related activities.



The first four columns in Table 1 provide a breakdown of these categories for CDBG activities funded between 2000 and 2016. For each category, Column (1) shows the average size in CDBG dollars spent per activity, and column (2) shows the total number of activities that were funded. The average cost of a CDBG activity 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. Multiple activities can also be funded within the same census tract; column (3) shows that the CDBG funded economic development activities in nearly 9,000 different tracts between 2000 and 2016 (out of roughly 73,000 census tracts nationwide).

## 2.2 Place-Based Policies and the CDBG

Place-based policies encompass a broad range of spatially targeted interventions that aim to improve local living conditions. This paper focuses on the subset of place-based *jobs* policies, which typically aim to stimulate local economic development and job growth in disadvantaged or underperforming areas (Bartik, 2020b).<sup>7</sup> Economists have traditionally been skeptical of place-based policies for a variety of reasons. Standard models predict that spatial economic differences should converge over time as people migrate to high-income places and capital is attracted to low wages in poorer places. Consequently, efforts to improve places directly may distort optimal migration decisions and incentivize workers to stay in unproductive areas. Many costly place-based policies also simply attempt to re-allocate jobs across space without generating any net increase in labor demand. Even policies that ultimately succeed in creating jobs may increase housing prices, to the detriment of lower-income residents.

Skepticism surrounding place-based policies has gradually abated as the spatial convergence predicted by standard models has slowed (and even reversed) in many regions, while permanent geographic differences in income, poverty, joblessness, and life expectancy have taken root and grown over time (Moretti, 2011). Inter- and intra-county migration has slowed dramatically (Austin, Glaeser and Summers, 2018), especially for low-wage workers facing a declining earnings premium for moving to high-productivity places (Ganong and Shoag, 2017; Autor, 2019). Poor residents also face disproportionate financial, information, and psychic barriers that impede “moving to opportunity” (Bergman et al., 2019). Meanwhile, empirical work on landmark federal programs such as the Empow-

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<sup>7</sup>An example of a place-based policy that does not focus on economic development is improving/increasing the affordable housing stock in a given area; see Koster and van Ommeren (2019), Diamond and McQuade (2019), and others.



erment Zones program ([Busso, Gregory and Kline, 2013](#)) and the Tennessee Valley Authority ([Kline and Moretti, 2013](#)) now provide examples of the transformative potential of place-based policies. Recent research also highlights the role of place-based policies as a policy instrument that is uniquely situated to correct spatial market failures (such as involuntary unemployment) and externalities (such as agglomeration), act as insurance against location-specific economic shocks, and generate equity gains in places with disproportionate concentrations of disadvantaged residents.<sup>8,9</sup>

The rich and growing economic literature on place-based policies still leaves many practical first-order questions unsatisfactorily answered. In their handbook chapter on place-based policies, [Neumark and Simpson \(2015\)](#) 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.” Designing policies at the federal level is further complicated by the widely diverse needs of places across the country. [Austin, Glaeser and Summers \(2018\)](#) observe 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”. Federal support is particularly important for economically distressed municipalities where needs are more severe and local tax bases are likely declining.

Intergovernmental grants such as the CDBG provide one potential solution via fiscal federalism. In the CDBG, the role of the federal government is to allocate block grants across municipalities and to set guidelines about the kinds of economic development activities that block grants can be spent on. Discretion over what activities to ultimately fund is ceded to local governments, who leverage place-specific knowledge about local needs and investing opportunities. Local governments also internalize incentives to invest in high-yield activities [Tiebout \(1956\)](#). The key empirical issue is whether local governments are sufficiently capable of “picking winners” and implementing policies that result in sustained local job growth. Local governments have also demonstrated that they can be skilled at diverting federal funds away from their intended uses ([Baicker and Staiger, 2005](#)).<sup>10</sup> Still, intergovernmental grants provide an important policy instrument for combining the scale of federal programs with the potential benefits of local control.

The scope of the CDBG also provides opportunities to explore qualities of effective place-based policies. While previous efforts have been made to compare policies across different studies, compar-

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<sup>8</sup>[Neumark and Simpson \(2015\)](#) and [Austin, Glaeser and Summers \(2018\)](#) provide excellent reviews of these arguments.

<sup>9</sup>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.

<sup>10</sup>[Baicker and Staiger \(2005\)](#) study federal matching grants for Medicaid, and argue that matching grants are more expensive and less effective than block grants when contributions can be misrepresented by local governments.

isons are difficult when evaluations differ substantially across methods, geographies, and time periods (Bartik, 2018).<sup>11</sup> The CDBG makes it possible to analyze how policy effectiveness varies across different types of funded activities. Place-based policies are perhaps most commonly associated with zone-based incentives for businesses to operate in specific locations (Neumark and Simpson, 2015) but encompass a wide variety of other activities for stimulating economic development. Without an understanding of the comparative effectiveness of different types of activities, policymakers may struggle to make data-driven decisions on how the marginal dollar for place-based policies should be spent.

The CDBG also benefits from its wide geographic reach. Context is crucial for understanding the impacts of place-based policies, and standalone evaluations are inherently limited by the fact that even rigorously-estimated effects may only reveal impacts of a specific policy implemented in a specific place. 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, and conclude that place-based policies will have the largest impacts in the most distressed locations. Beyond this prediction, little is known about what other attributes of places influence policy effectiveness. The literature has less yet to say about how the effects of place-based policies could differ *within* local labor markets, given the enormous economic disparities that exist even across neighborhoods within the same city (Chetty et al., 2018).

### 3 Estimating Tract-Level Job Impacts of the CDBG

#### 3.1 Empirical Strategy

I begin by investigating how effective local governments are at investing CDBG funds to create local jobs in low-income neighborhoods. Following previous empirical work on place-based policies, I focus on census tracts as the primary geographic unit of observation.<sup>12</sup> I then estimate tract-level estimates of CDBG investments on job counts.

Estimating tract-level causal effects in this setting is complicated by several issues. First, tracts that receive CDBG investments differ from untreated tracts in unobservable ways. This classical issue is further complicated by the fact that treatments themselves are also endogenous to the tracts receiving

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<sup>11</sup>In addition to the aforementioned evaluations of the TVA and Empowerment Zones, see Neumark and Kolko (2010) on employment zones, Koster and van Ommeren (2019) on housing development, and Collins and Shester (2013) on slum clearance, among others.

<sup>12</sup>Census tracts are geographic subdivisions of counties that typically contain 1,200 to 8,000 residents and commonly represent the size of a typical neighborhood.

them. The size, mix, and timing of investments all depend on tract characteristics and preferences of the specific municipality responsible for allocating funds across neighborhoods. For example, the decision of the size and variety of investments made will likely take into consideration whether a tract is gentrifying or declining, whether it is commercial, industrial, or residential, and how the investment would potentially affect the existing composition of firms and residents in the area. Each municipality will also differ in their relative weighting of these factors.

Differences in selection across treated tracts makes it difficult to justify any combination of comparison tracts as satisfying the parallel trends assumption, even if pre-trends appear parallel. These issues also exacerbate recent criticisms of traditional two-way fixed effect (TWFE) methods, which implicitly compare treated tracts to other treated tracts. Under staggered treatment adoption, TWFE estimates can be decomposed into a weighted combination of cohort-specific treatment effects where weights can be negative. In the presence of heterogeneous treatment effects, negative weights can lead to highly misleading average treatment effects (Goodman-Bacon, 2018; Sun and Abraham, 2020).<sup>13</sup> Standard panel methods that pool together treated tracts and compare them to uniform comparison groups are therefore unlikely to be reliable in the CDBG context.

Rather than estimating an average treatment effect on a pooled sample of treated tracts, I estimate causal impacts for each treated tract separately. This allows for the selection of comparison tracts to be tailored to each treated tract, while also avoiding bias from comparing treated tracts to one another in a pooled approach. 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. The individually-estimated tract effects must also be aggregated together to summarize the overall effect of the CDBG. While there is no agreed approach for how this should be done,<sup>14</sup> weights for each individual estimate can be chosen in a way that is both transparent and excludes the possibility of negative weights. By contrast, most modern criticisms of traditional differences-in-differences and event study methods focus on the opaque weighting scheme of underlying average treatment effects that can often produce negative weights.

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<sup>13</sup>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 heterogeneous across groups or periods, leading to situations where (for example) the total effect is negative while all underlying ATEs are positive. See Sun and Abraham (2020), de Chaisemartin and D'Haultfoeuille (2020), Goodman-Bacon (2018), Callaway and Sant'Anna (2019), and Borusyak and Jaravel (2016). It is also worth noting that many of the conclusions in these papers require the treatment to be identical across treatment units.

<sup>14</sup>Ben-Michael, Feller and Rothstein (2019) propose one data-driven method for aggregating together multiple synthetic control estimates, but their approach assumes a uniform treatment with a common donor pool of control tracts shared by all treated tracts.

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., 2019; 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 certain non-restrictive assumptions. 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  $i = 1, \dots, N$  census tracts over the years  $t = 1, \dots, T$ . Some tracts, denoted by the time-invariant indicator  $W_i = 1$ , will receive a CDBG investment for economic development at some point during the panel. I separately index this set of ever-treated units using  $j = 1, \dots, J$ . 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:

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

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 with treated tract  $j$  as  $\mathcal{D}_j$  and indexing donor units for that tract as  $d_j = 1, \dots, D_j$ , the synthetic control unit for tract  $j$  is formed by applying an  $D_j \times 1$  vector of weights  $\mathbf{W}_j = (w_{1j}, \dots, w_{D_jj})'$  to the set of donor control units to form an estimate of the counterfactual outcome:

$$\hat{Y}_{j,T_j+k}(0) = \sum_{d_j=1}^{D_j} w_{d_j} Y_{d_j,T_j+k} \quad (2)$$

Note that I allow  $D_j$ , 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:

$$\hat{\tau}_{jk}^{scm} = Y_{j,T_j+k} - \hat{Y}_{j,T_j+k}(0) \quad (3)$$

where inference is conducted empirically using jackknife standard errors.<sup>15,16</sup>

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  $\ell = 1, \dots, T_j - 1$ ) between the treatment tract and the synthetic control:

$$\min_{\mathbf{W}_j \in \Delta_j^{scm}} \frac{1}{2(T_j - 1)} \sum_{\ell=1}^{T_j-1} \left( Y_{j,T_j-\ell} - \sum_{d_j=1}^{D_j} w_{d_j} Y_{d_j,T_j-\ell} \right)^2 \quad (4)$$

where the weights in  $\mathbf{W}_j$  are conventionally non-negative and sum to one.<sup>17</sup> This characterization of the optimization problem differs slightly from the original characterization proposed by [Abadie](#),

<sup>15</sup>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.

<sup>16</sup>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 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. \(2019\)](#) and use the leave-one-unit-out jackknife approach to empirically quantify uncertainty.

<sup>17</sup>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 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.

[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 \(forthcoming\)](#) 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. To illustrate, suppose that potential outcomes follow the following linear factor structure:

$$\begin{cases} Y_{it}(0) = \delta_t + \lambda_t \mu_i + \epsilon_{it} \\ Y_{it}(1) = \alpha_{it} + Y_{it}(0) \end{cases} \quad (5)$$

where  $\delta_t$  represents an unknown common factor with constant factor loadings,  $\lambda_t$  represents a  $(1 \times F)$  vector of common factors,  $\mu_i$  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  $\mu_i$  that may have time-varying impacts on outcomes via the coefficients  $\lambda_t$ .<sup>18</sup>

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  $\mu_j$  in Equation (5), then it must be the case that the individual transitory shocks  $\epsilon_{it}$  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 reliability 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

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<sup>18</sup>This general structure resolves to basic differences-in-differences when  $\lambda_t$  is constant over time.

$\epsilon_{it}$ .

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., 2019).<sup>19</sup> 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  $\tau_{jk}$  using this approach is as follows:

$$\hat{\tau}_{jk} = \frac{1}{T_j - 1} \sum_{\ell=1}^{T_j-1} \left[ (Y_{j,T_j+k} - Y_{j,T_j-\ell}) - \sum_{d_j=1}^{D_j} w_{d_j} (Y_{d_j,T_j+k} - Y_{d_j,T_j-\ell}) \right] \quad (6)$$

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., 2019), where the estimator performs well if either differences-in-differences or traditional SCM provides a suitable counterfactual.

*Implementation in the CDBG Setting:* The first practical consideration is how to define tract-level treatment. In each year  $t$ , I observe whether one or more CDBG economic development activities were funded in any given census tract  $i$ . One possibility is to define treatment as the combination of all activities funded concurrently within the same tract. Column (3) of Table 1 shows that nearly 9,000 tracts (roughly 12 percent of all tracts nationwide) received an economic development investment at any point in time during the panel. Following Kline and Moretti (2013), I define initial treatment as

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<sup>19</sup>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 used to construct the synthetic control, which introduces differences in scale that preclude the use of non-zero intercepts.



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.

Although treatment is a binary indicator, quantifying the magnitude of treatment is complicated by the fact that CDBG investments tend to be temporally clustered. Local governments often aim to revitalize neighborhoods through multi-year plans, or may simply need additional CDBG funds from future years. Figure A2 plots the fraction of treated tracts where additional investments were made one to ten years after initial treatment. Additional CDBG activities are funded in 25% of treated tracts one year after initial treatment, 21% after two years, and 19% after three years. This percentage falls until the six-year mark, where the probability of future investments 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 measure the policy size in a given tract as the three-year sum of investments beginning with an initial non-zero investment. Figure A1 presents the cumulative distribution function of treatments defined in this way.

One potential issue with this definition is that many treatments defined in this way are quite small, with a median size of \$80,000. To put this into context, cost-per-job estimates from existing place-based studies currently range from \$10,000 to over \$100,000 (Bartik, 2020b). Estimates from smaller treatments may therefore be insufficiently powered to detect job impacts. I consequently estimate job impacts for the top half of treatments by size and explore how effect sizes vary with treatment size within this subsample. While this restriction does not ultimately affect the internal validity of the synthetic control estimates, it will affect the interpretation of the cost-benefit estimates. 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).

I further omit treated tracts with fewer than five years of pre-treatment data to mitigate potential overfitting in the SCM estimates. Although it is possible that tracts may have been treated prior to the start of the panel (which I do not observe), excellent pre-treatment fit of at least five years during the panel suggests via the factor structure in Equation (5) that the synthetic control succeeds at balancing factor loadings of the treated tract, even if the treated tract had actually been treated in the past.<sup>20</sup> Given the importance of observing medium-run effects, I also remove treated tracts in which treatment begins with less than seven years remaining in the panel.

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<sup>20</sup>Similarly, I allow tracts that are treated within the first five years of the panel but are untreated for the next five years (and then are treated again) to be included in the sample of treated tracts, so long as they achieve sufficiently strong fit.

Column (4) of Table 1 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. I use this set of tracts as the analysis sample for estimating job impacts of the CDBG, running the intercept-shifted SCM separately for each tract.

Another practical consideration is defining the appropriate set of donor units for each treated tract  $j$ ,  $\mathcal{D}_j$ . In general, the potential for overfitting increases as the number of donor units increases. It is neither practical nor prudent to use the set of untreated tracts nationwide as the donor pool. For each treated tract  $j$ , I attempt to limit the set of donor control units to 50 or fewer. For each given treated unit, I restrict the set of potential donor units to those within the same commuting zone. This ensures that common local labor market shocks will be differenced out of the estimate.

Still, many commuting zones contain hundreds, if not thousands, of untreated census tracts. To further restrict the set of donor tracts for each treated tract, I conduct the following procedure. First, I predict the probability of treatment for each untreated tract by estimating a logit model based on a large vector of baseline census tract covariates.<sup>21</sup> I narrow the list of covariates using a lasso model selection procedure to determine the subset of covariates that best predict treatment status.<sup>22</sup> 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.<sup>23</sup> Each treated tract  $j$  is therefore paired with a donor pool  $\mathcal{D}_j$  of up to 50 untreated tracts within the same commuting zone that are selected based on characteristics that predict treatment status.

I run the intercept-shifted SCM on an analysis sample of 1,375 census tracts with at least 5 years of pre-treatment data and 7 years of post-treatment data.<sup>24</sup> Table 2 shows how treated tracts differ from untreated tracts and their synthetic tracts. Untreated tracts differ substantially from treated tracts. Treated tracts are less affluent, have greater concentrations of minorities and low-educated residents, have less desirable neighborhood amenities (as measured by housing values and rents), but exhibit

<sup>21</sup>The covariates include all variables underlying the indices in Table 2, 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.

<sup>22</sup>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.

<sup>23</sup>This includes tracts that were only treated within the first five years of the panel.

<sup>24</sup>To implement this, I rely on the [augsynth](#) package by Eli Ben-Michael, Avi Feller, and Jesse Rothstein.

far greater density in terms of jobs per square mile. This suggests that treated tracts do tend to be commercial and industrial hubs with high levels of job density. Synthetic tracts very closely match the characteristics of treated tracts despite matching only on pre-treatment job trajectories. The one outstanding difference is that synthetic control tracts are still less dense than treated tracts, especially in terms of jobs. Part of this difference can be attributed to the fact that the intercept-shift adjustment enables tracts with fewer baseline jobs to contribute to the synthetic control so long as pre-treatment trends match.

After implementing the intercept-shifted SCM, I evaluate the pre-treatment balance of the resulting estimates. Because SCM is only recommended when pre-treatment fit is excellent, I restrict the set of SCM estimates to those where pre-treatment imbalance between treated and synthetic control tracts is within 20% of the baseline pre-treatment outcome. In Section 3.3, I show that the results are ultimately not sensitive to thresholds between 10% and 30%. Given this, I drop 592 census tracts where the pre-treatment imbalance between the treatment and synthetic control exceeds 20% of the baseline treatment value. I additionally drop the top and bottom 1% of estimates to limit the influence of outliers and remove potentially errant estimates. Table A1 shows how treated tracts removed for poor fit differ from the remaining treated tracts. Tracts with poor fit do not appear to differ by much in terms of resident characteristics. However, tracts with poor fit have substantially many more jobs and appear to represent the densest of commercial and industrial areas. These tracts could suffer from poor fit simply because they are more likely to experience larger absolute fluctuations in job counts, even if the fluctuations are proportionally similar.

I calculate point estimates  $\hat{\tau}_{jk}^{scm}$  and jackknife standard errors for each treated tract  $j$  and each post-treatment period  $k$  up to ten years after the initial funding date. There are a variety of ways to aggregate individual point estimates. Ben-Michael, Feller and Rothstein (2019) describe tradeoffs of various methods, although their discussion assumes a uniform treatment and a single pool of donor units that is shared across all treated units. Given these key differences in the CDBG setting, I opt to aggregate the SCM estimates in a transparent and parsimonious way. For each post-treatment period  $k$ , I calculate a weighted average of SCM estimates, where weights are calculated using the inverse of the jackknife standard error for each estimate.<sup>25</sup> This amounts to averaging the individual SCM estimates, placing greater weight on estimates that are more precisely estimated. Additionally, I

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<sup>25</sup>This approach was used by Dobbie and Fryer (2013) to aggregate school-specific causal effects of attending charter schools in New York, where each causal effect was calculated using that school’s admissions lottery. Their approach is similar to the CDBG context in the sense that admission to each charter school reflects a substantively different treatment, which differs from the standard setting in Ben-Michael, Feller and Rothstein (2019).

control for year-of-treatment fixed effects to account for the fact that job impacts may differ for treated tracts depending on the year  $T_j$  when CDBG investments began. I therefore estimate the following regression:

$$\hat{\tau}_{jk}^{scm} = \alpha_k + \delta_{T_j} + \epsilon_{jk} \quad (7)$$

The parameter of interest is  $\alpha_k$ , which represents the average of SCM treatment effects for event time  $k$ , weighted by the inverse of each estimate's standard error and controlling for year-of-treatment fixed effects. I also adjust standard errors for clustering at the commuting zone level.

In addition to job counts, I estimate CDBG impacts on several other job-related outcomes. First, I determine whether low-income residents are the primary beneficiaries of CDBG investments. Second, I determine whether jobs created by CDBG investments are held by local residents. These two metrics are often viewed as core goals of the CDBG and place-based jobs policies as a whole. Finally, I estimate the impact on jobs in neighboring tracts to quantify the extent of spillovers (positive or negative) generated by the CDBG. I describe how I measure each of these outcomes in the proceeding section.

### 3.2 Data

*CDBG 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 1.<sup>26</sup> 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. I show in Figure A3 that the typical activity takes from 1 to 3 years to complete from the time that the project is initially funded. Time to completion varies by

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<sup>26</sup>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.

project type, with the typical project taking between 1 to 2 years to complete. Building/land projects take slightly over 2 years to complete, whereas infrastructure projects take over 3 years to complete. Note that job counts could still respond even before the project is officially completed. I therefore define the year of treatment as the year immediately following the *funding date*, regardless of whether the project had been completed.<sup>27</sup>

*Jobs Data:* 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.<sup>28</sup> 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. I aggregate all census block counts into their respective census tracts. The data extend from 2002 through 2018 for most states. To protect confidentiality, the data are “fuzzed” at the block level via noise infusion. For many years, the data also contain disaggregations of job counts by race, wage category, education, and more.

The structure of the LODES data allows me to measure the CDBG’s impact on nearby and low-income workers. I define nearby workers as those working in a treated tract and living in a tract whose centroid is within five 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. I define LMI tracts (in contrast to the official CDBG definition) as those where the poverty rate is above the 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 in the same commuting zone. Another measure of socioeconomic status is simply the worker’s earnings. The LODES provides the number of block-level jobs where monthly earnings are less than \$1,250, between \$1,251 and \$3,333, and greater than \$3,333. I focus on the lowest wage category; although this does not directly provide insight on total household income, it is likely a strong indicator of being low-income.

The bottom panel of Table 2 shows how each of the five outcome variables differ between treated

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<sup>27</sup>There are pros and cons to linking treatment to the year following funding as opposed to the year that the funding occurred. The main drawback is that anticipatory responses could occur in the months immediately following a project being funded, especially for projects funded early on during the year. The benefits to defining treatment in this way are twofold. First, it allows the synthetic control to match on changes in economic conditions surrounding a project being approved. Second, it effectively increases the number of pre-treatment years available for the SCM to match on, which is important for the reliability of the SCM estimator.

<sup>28</sup>Census blocks are the smallest geographic unit used by the Census. Each census tract is an amalgamation of underlying census blocks.

tracts, untreated tracts, and synthetic control tracts. Again, the differences are large between treated and untreated tracts within the same commuting zone, with treated tracts having two to three times more jobs across all outcomes. The gap between treated and synthetic control tracts is substantially smaller, although treated tracts still exhibit greater job density.

*Tract-level covariates:* I obtain pre-treatment tract-level covariates from the 1990 and 2000 decennial censuses. The full list of covariates are presented in Table 2 and include a wide variety of demographic, economic, and residential variables. I also calculate changes between 1990 and 2000 to represent neighborhood trends prior to the start of the LEHD-LODES data. Census tract boundaries change every decade, so I rely on a tract-level concordance between the 2010 boundaries used by the LEHD-LODES data and the 2000 and 1990 boundaries used by the decennial censuses (Logan, Xu and Stults, 2014). I use the covariates from these data sets for the lasso model selection procedure and for analyzing place-based correlates of project effectiveness.

### 3.3 Estimates of Job Impacts

Figure 3 presents estimates for each event time  $k$  as calculated in Equation (7), as well as the corresponding 95% confidence intervals. The underlying estimates and standard errors are provided in tabular form in Table 3. Pre-treatment fit is excellent across all six outcome variables. Overall, the CDBG appears to have increased job counts in both treated and neighboring tracts, especially for low-income and nearby workers.

The effect of the CDBG on total jobs is positive, significant, and gradually increases over the first six years after treatment before stabilizing thereafter. Ten years after treatment, jobs in treated tracts increased by 107 relative to jobs in corresponding synthetic control tracts, a 7.3 percent increase off of a baseline of 1,451 pre-treatment jobs per treated tract.

The effect on jobs held by workers living in tracts with above-median poverty rates is also positive and significant. The effect on total jobs appears mostly accounted for workers from these tracts. By comparison, these workers comprise only 40% of pre-treatment jobs in treated tracts. This suggests that the increase in jobs from CDBG investments are disproportionately held by workers living in lower-income neighborhoods. Similarly, roughly half the effect on jobs is accounted for by workers living in tracts with poverty rates in the top quartile, despite the fact that these workers comprise approximately 20% of jobs in treated tracts.

Low-wage jobs also account for roughly half of the total effect. By comparison, these jobs com-

prise one-quarter of baseline jobs in treated tracts. This suggests that the CDBG tends to create lower-earning jobs. While higher pay is often an objective of place-based policies, many have argued that prioritizing joblessness over income inequality may have broader quality-of-life impacts (Winkelmann, 2014; Austin, Glaeser and Summers, 2018). Joblessness has notably been associated with mental health problems, opioid use, and suicide (Blakely, Collings and Atkinson, 2003; Krueger, 2017), particularly in economically distressed regions.

The CDBG also disproportionately benefits workers living within two miles of the treated tract. The effect size accounts for roughly 20 to 30 percent of the effect on total jobs, and these workers comprise only 12% of baseline jobs. While residents of targeted neighborhoods do benefit disproportionately, most of the overall effect on jobs appears to benefit workers living outside of the immediate neighborhood.

Finally, the CDBG generates positive and significant spillover effects on jobs in adjacent untreated tracts. The magnitude of the spillover by year 10 is similar to that of the main effect, but the increase is more gradual. The presence of positive effects in neighboring tracts mitigates the concern that the main effect is driven by displacement of nearby jobs. Jobs still may have been displaced from further away, but my analysis is unable to identify these effects. However, the CDBG prohibits grantees from funding activities that “assist directly in the relocation of any industrial or commercial plant, facility, or operation from one area to another area, if the relocation is likely to result in a significant loss of employment in the labor market area from which the relocation occurs.”<sup>29</sup> This provision appears to explicitly discourage the use of CDBG funds to displace existing jobs.

The results are also robust to the specific threshold used to limit tracts with strong pre-treatment fit. Figure A4 provides the SCM graphs for total jobs on the top row and jobs held by workers from LMI tracts on the bottom row. From left to right, the plots show how the effect trajectories change when restricting the analysis sample to treated tracts with pre-treatment imbalance less than 10%, 20%, and 30% of the baseline pre-treatment mean. By restricting the threshold to 10%, pre-treatment fit improves and reduces potential bias. By increasing the threshold to 30%, I am able to include many more of the larger tracts that would otherwise be omitted due to insufficient fit. The trajectory and significance of the effects remain similar across all three thresholds.

Finally, I explore how the estimates vary with the cutoff for treatment size. Figure A5 presents plots of the estimates when restricting to the top 25% of treatments by size (average: \$529,502) instead

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<sup>29</sup>See 24 CFR 570.482.



of the top 50% (average: \$258,348). Across the board, the trajectory of the impact appears quite similar, though the effect sizes appear to be moderately larger. The most noticeable difference occurs in the last two years on total jobs, where the effect is nearly 50% larger. The marginal increase in treatment size therefore appears to only modestly benefit local low-income residents.

## 4 Getting Beneath the Hood of Effective Place-Based Policies

In this section, I compare the effectiveness of different treatments across a variety of attributes. I focus on the nature of the investments as well as the kinds of neighborhoods are targeted. Throughout this analysis, I focus on jobs held by workers living in tracts with above-median poverty rates as the outcome variable of interest. Note that because of the lack of experimental variation in what activities are funded and where they are funded, these estimates are unlikely to be truly causal. Still, the CDBG provides an unprecedented opportunity to explore what makes an effective place-based policy.

### 4.1 What makes an effective investment?

To determine how various kinds of economic development activities differ in their job impacts, I begin by estimating Equation (7) separately for each of the eight activity categories described in Section 2.1.<sup>30</sup> Treatments can be comprised of multiple underlying activity types, so each regression includes the set of treatments where at least one investment in the activity category was included.<sup>31</sup> I again weight each estimate  $\hat{\tau}_{jk}^{scm}$  by the inverse of its standard error and I cluster standard errors from Equation (7) at the commuting zone level.

Figure 4 plots estimates and 90% confidence intervals, run separately for each of the eight different activity categories. Table 4 provides estimates in tabular form. At first glance, all eight investment types display roughly upward trajectories after the initial treatment, and many are statistically significant. Upon further inspection, the most productive place-based jobs policies tend to revolve around public services that directly benefit businesses—providing financial and technical assistance to firms, clearing land for economic development, and subsidizing commercial/industrial construction projects. The CDBG does not fund traditional incentive-based investments for attracting businesses to relocate to underperforming areas, which tend to have a mixed record of success (see Neumark and Simpson (2015) for an overview). Attempts to directly incentivize businesses to relocate to specific areas can also lead to costly bidding wars, severely dampening the cost-effectiveness of such policies

<sup>30</sup>Splitting the sample in this way greatly reduces sample size, so I restrict the post-treatment analysis to eight years after the initial funding date. I also exclude estimates with less than 20 underlying observations from the plot.

<sup>31</sup>Table A3 shows the fraction of each activity type  $a$  that coincides with an activity type  $b$  within the same treatment.

(Bartik, 2020a).

On the other hand, effects on infrastructure and exterior improvements appear comparatively limited. While infrastructure is commonly perceived as a crucial component of place-based policy, these findings suggest that job impacts of CDBG-funded infrastructure were small. The Tennessee Valley Authority notwithstanding, infrastructure has had a mixed record of spurring local job growth (Garin, 2019; Austin, Glaeser and Summers, 2018). The types of infrastructure funded by the CDBG also tend to be improvements to streets, water systems, transportation, and parking, which only marginally impact firm productivity. Benefits from infrastructure could potentially take longer to transpire, though even clearance projects begin producing impacts within eight years. Finally, estimates for non-profits and other improvements do not appear particularly insightful due to limited sample size and the ambiguity of what activities are exactly funded under this category.

I also compare the cost-effectiveness of these activity categories. Column (1) of Table 1 presents the average size of each activity category. On a per-dollar basis, financial assistance (\$118,257 per investment) is typically less costly than commercial/industrial construction (\$205,850 per investment) while generating similar impacts. Micro-enterprise (\$47,958) and clearance projects (\$50,000) generate more modest impacts but at substantially lower cost. Infrastructure, despite being the most expensive category (\$379,879), produces the smallest benefit per dollar.

To directly compare activity categories, I also run the following regression for each category  $c$ :

$$\hat{\tau}_{jk}^{scm} = \gamma(\text{Activity Category } c)_j + \delta_{Tj} + \epsilon_{jk} \quad (8)$$

where  $(\text{Activity Category } c)_j$  is an indicator for whether the treatment for tract  $j$  included an activity in category  $c$ .  $\gamma$  represents the effect size difference between treatments including category  $c$  versus all other treatments, conditional on year-of-treatment fixed effects. While the category-specific estimates for  $\alpha_k$  from Equation (7) represent the absolute effects for each activity category, the estimates for  $\gamma$  from Equation (8) capture each category's relative effectiveness. These estimates are plotted in A6 and are provided in tabular form in Table A2. Statistical significance (implying that one category generates statistically larger or lesser impacts compared to all other categories) occurs less frequently under this specification. The only activity type that appears to consistently outperform the others is financial assistance.

Taken together, these findings suggest that the first dollar is typically best spent on financial assis-

tance for businesses, though effects remain positive for a variety of other investment types, including clearance projects, commercial/industrial construction, micro-enterprise investments, and technical assistance. Exterior and cosmetic improvements to neighborhoods tend to generate temporary impacts, and the impact of infrastructure tends to be limited relative to its cost.

Finally, I consider whether investments had different impacts depending on when treatment occurred. The set of treatment years with more than 5 years of pre-treatment data and 7 years of post-treatment data includes 2007 through 2012. I analyze how treatment impacts differ across three periods: pre-recession (2007), recession (2008-2010), and post-recession (2011-2012). Figure A7 shows how the composition of investment types varies across these three periods. Between pre-recession and recession, clearance and infrastructure investments increase most substantially. Between recession and post-recession, direct investments in businesses decrease — both financial assistance and technical aid. Interestingly, the trajectory of job impacts remains quite similar across the three periods. The most notable difference is that the effect scales up much more quickly during the recession and beyond.

## 4.2 Where do place-based jobs policies generate the largest impacts?

I next analyze how different attributes of places contribute to the effectiveness of place-based investments. The literature currently provides limited guidance on this topic. At best, the existing evidence predicts that place-based policies will have larger impacts in places where nonemployment is high; Bartik (2015) and Austin, Glaeser and Summers (2018) show that shocks to local labor demand tend to produce the largest employment changes in regions with high baseline levels of nonemployment. I test and build upon this prediction by directly analyzing how the impacts of CDBG investments vary across various dimensions of the places where they are implemented.

Places can be characterized by a near-limitless number of variables. To avoid issues with multiple hypothesis testing, I begin by constructing four indices summarizing places by socioeconomic characteristics, demographic characteristics, neighborhood amenities, and urban density. The four indices and their underlying inputs are provided in Table 2. I create each index as follows. I begin by calculating two versions of each input variable: one in 2000 levels and one expressing the change between 1990 and 2000. The former represents the pre-treatment baseline and the latter captures pre-treatment trends. I then scale each input variable by its standard deviation, taking care to multiply certain input variables that are negatively associated with the corresponding index by -1.<sup>32</sup> I construct each index

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<sup>32</sup>These variables include: % in poverty, % HS grad or less, % single mother, % vacant housing. All corresponding

by summing across all scaled inputs and re-standardizing the resulting index to have a mean of zero and standard deviation of one. All re-scaling and standardization occurs only within the set of treated tracts eligible for the CDBG, not across the broader set of tracts nationwide. The indices therefore capture differences across treated tracts, which are comparatively disadvantaged to begin with.

I also calculate two geographic versions of each index: one at the tract level and one at the commuting zone level.<sup>33</sup> I do so in order to distinguish between tract-specific attributes and attributes of the broader local labor market. I then estimate the following equation for each index  $a$ :

$$\hat{\tau}_{jkz}^{scm} = \rho_1 \text{Index}_j^a + \rho_2 \text{Index}_z^a + \delta_{T_j} + \epsilon_{jkz} \quad (9)$$

where the outcome  $\hat{\tau}_{jkz}^{scm}$  again represents the causal effect of each treatment  $j$  on jobs held by workers from LMI tracts.  $z$  now indexes commuting zones.  $\rho_1$  represents the effect of a one-standard deviation increase in the tract-level measure of index  $a$  conditional on the corresponding commuting zone index.  $\rho_1$  captures how CDBG investments vary in their effectiveness between two tracts in similar commuting zones but with different tract-level measures of  $a$ .  $\rho_2$  captures how CDBG investments vary in their effectiveness between two similar tracts located in commuting zones with different levels of  $a$ . The two coefficients provide different insights on the spatial heterogeneity of place-based policies.

Figure 5 provides both pairs of estimates for the four indices, along with 90 percent confidence intervals. Table 5 provides tabular estimates. While not extremely precise, the tract-specific estimates suggest that place-based investments tend to produce larger effects in tracts scoring higher on the demographic/social and neighborhood indices. Tracts with high neighborhood index values represent places that may be more conducive to commercial and industrial development. Tracts with high demographic/social index values may have resident populations with greater workforce attachment higher levels of educational attainment. Given that a non-trivial portion of the overall jobs impact is driven by nearby workers, local workforce composition may influence the impact of place-based policies. Overall, these results suggest that the CDBG generated more jobs in comparatively less-disadvantaged tracts; however, this does not necessarily mean that residents from more disadvan-

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changes between 1990-2000 are also multiplied by -1.

<sup>33</sup>For commuting zone indices, I calculate each underlying variable at the commuting zone level by taking the average value across all underlying tracts, weighing by 2000 population. The only exceptions are initial jobs per square mile and population per square mile; for these variables, I sum total initial jobs and population across the commuting zone, and divide each by the total square mileage of the commuting zone.

tagged tracts did not benefit; workers residing in high-poverty tracts were still among the primary beneficiaries of the CDBG.

The role of commuting zone attributes is reversed. Effects appear larger in more-disadvantaged commuting zones.<sup>34</sup> There is also a clearly negative association with commuting zone density. The tract and commuting zone results suggest that to maximize job creation, federal funds should be prioritized for less affluent local labor markets, though municipalities should not necessarily target neighborhoods at the bottom of the distribution.

Finally, I consider how effects differ based on whether the neighborhood was on a downward or upward trajectory prior to treatment. I classify tracts based on whether jobs in the year prior to treatment was greater or less than jobs in 2002 (the initial period). I plot estimates separately for each set of tracts in Figure A10. Although CDBG investments appear to be more effective when targeted toward neighborhoods on the rise, impacts are positive for both sets of tracts.

## 5 Other Consequences of the CDBG

### 5.1 Are benefits capitalized into house prices?

While the CDBG appears to have had a positive impact on jobs, the benefits for low- and moderate-income residents could be mitigated by an accompanying increase in housing prices. In the extreme case, the rising tide of gentrification could eventually force residents of these neighborhoods to relocate to even further disadvantaged neighborhoods. I therefore attempt to estimate whether CDBG investments lead to short- and medium-run housing price responses.

The empirical approach I use differs from the main analysis due to the fact that national tract-level data on home prices and rent values is generally unavailable at an annual frequency. This greatly reduces the amount of pre-treatment data that is available. In lieu of this, I rely on self-reported housing price and rent data from the 1970, 1980, 1990, and 2000 decennial censuses to construct pre-treatment values for synthetic controls to match on (Logan, Xu and Stults, 2014). I also use American Community Survey (ACS) 5-Year Estimates from 2006-2010 through 2014-2018, which I obtain via the Integrated Public Use Microdata Series-National Historical Geographic Information System (IPUMS-

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<sup>34</sup>In contrast to predictions by Bartik (2015) and Austin, Glaeser and Summers (2018), Figure A9 shows that the employment-to-population ratio within a local labor market does not appear to correlate strongly 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.

NHGIS) ([Manson et al., 2020](#)).<sup>35</sup> Tract-level estimates are not available in the annual 1-Year estimates. For outcomes, I focus on median rent and the log of median home values. I adjust all prices to 2014 dollars.

In this version of the SCM, I use the four decennial censuses from 1970 to 2000 as universal pre-period training observations to match on. These are the only pre-period observations that tracts treated prior to 2010 will be able to utilize, given that the first year of ACS outcomes data begins in the five-year period spanning 2006 through 2010. For tracts treated in 2011, I allow the 2006-2010 ACS data to serve as an additional pre-treatment period. I similarly allow tracts treated in 2012 and 2013 to use prior ACS iterations as additional pre-period data.<sup>36</sup> Although the number of pre-periods is small, the actual time horizon that the SCM will attempt to match on will be large. This is a double-edged sword; on one hand, a close match based on three decades of housing prices is encouraging from the perspective of constructing a credible counterfactual. On the other hand, the long time horizon also increases the size of transitory shocks, increasing the likelihood of overfitting ([Abadie, Diamond and Hainmueller, 2010](#); [Abadie, forthcoming](#)). From a data perspective, a non-trivial number of treated tracts are missing data from the earlier decennial censuses; I restrict the sample of treated units to those with data beginning in 1970 or 1980. I also continue to restrict treated tracts to the 779 in the main sample to ensure that the housing price estimates correspond to the previously-estimate job impacts.

The effects on median log home values and rents are plotted in Figure 6 along with 90 percent confidence intervals. Estimates are provided in tabular form in Table 6. There is a clear zero effect on home values and a temporary increase in median rents that never exceeds \$20 (off of a baseline median rent of \$600). These findings suggest that economic development policies funded by the CDBG did not lead to a meaningful response in housing prices. One potential explanation is the increased prevalence of housing reserved for low-income tenants (e.g. units built via the Low-Income Housing Tax Credit), rent-controlled housing, and public housing. These housing sources tend to be less sensitive to upward price pressures. [Busso, Gregory and Kline \(2013\)](#) also find that the federal Empowerment Zones program, which also targeted similarly low-income census tracts, did not lead to a meaningful increase in housing costs.

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<sup>35</sup>The 5-Year ACS estimates begin in 2005-09, but data for approximately 10 percent of tracts is missing for this year (and this year only).

<sup>36</sup>I use the same set of treated tracts from the main sample, which only included tracts treated from 2006 to 2013 to maximize the number of available pre- and post-treatment outcomes.

## 5.2 Do Federal Block Grants Crowd Out Local Public Spending?

I also attempt to determine how much new public spending is generated by block grants. An exogenous lump sum transfer to local governments could serve as a necessary catalyst to pursue large, ambitious projects that would otherwise be financially infeasible. However, block grants may also crowd out existing spending; local governments could use block grants to substitute for spending that was previously allocated to fund economic development, and re-allocate the original funds toward other government functions. In the latter scenario, funds from federal place-based redistribution would be completely captured by local governments without generating any net increase in local public spending. Even if investments funded by the CDBG produce positive outcomes, the net benefit to local residents could be negligible if all CDBG spending is spending that would have occurred in the absence of the block grant. Indeed, [Baicker and Staiger \(2005\)](#) show that state and local governments are quite capable of diverting federal funds from their intended uses.

The empirical challenges here differ substantially from my analysis on job impacts. Local public spending is measured at the grantee (local government) level. Because grantees receive CDBG funds annually, there is no single “event” that can be used to estimate causal effects. I begin by describing a new source of identifying variation, which relies on the CDBG allocation formula.

### 5.2.1 How is the CDBG allocated?

Prior to the beginning of each fiscal year  $t$ , Congress determines the 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 measure the relative need of each grantee  $i$ . The allocation formula is as follows:

$$CDBG_{it} = s_t \times A_t \times \max\left( \underbrace{0.25 \times \frac{Population_{it}}{\sum_i Population_{it}} + 0.5 \times \frac{Poverty_{it}}{\sum_i Poverty_{it}} + 0.25 \times \frac{Overcrowd_{it}}{\sum_i Overcrowd_{it}}}_{\text{Formula A}}, \underbrace{0.2 \times \frac{GrowthLag_{it}}{\sum_i GrowthLag_{it}} + 0.3 \times \frac{Poverty_{it}}{\sum_i Poverty_{it}} + 0.5 \times \frac{Pre1940Housing_{it}}{\sum_i Pre1940Housing_{it}}}_{\text{Formula B}} \right) \quad (10)$$

$Population_{it}$  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<sub>it</sub>* 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.<sup>37</sup> 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  $A_t$ .<sup>38</sup>

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

$$CDBG_{it} = \max \left( \sum_{k=1}^3 v_t^k X_{it}^k, \sum_{k=4}^6 v_t^k X_{it}^k \right) \quad (11)$$

where  $X_{it}^k$  is the value of the  $k$ th input for grantee  $i$  in year  $t$  (e.g. *Population<sub>it</sub>*, *Poverty<sub>it</sub>*, etc.), and the  $v_t^k$  include the following terms:

$$v_t^k = \frac{c^k s_t A_t}{\sum_i X_{it}^k} = \frac{c^k s_t A_t}{\tilde{X}_t^k} \quad (12)$$

$v_t^k$  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 A11.

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

<sup>37</sup>These annual updates are not surveys, but rather modeled estimates based on decennial censuses.

<sup>38</sup>Without  $s_t$ , the formula typically allocates more money than what is actually available. As such,  $s_t < 1$ .

and slow growth tend to be overfunded.<sup>39</sup>

### 5.2.2 Empirical Strategy and Data

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:

$$\frac{Spend_{it}}{Population_{i0}} = \alpha + \rho \frac{CDBG_{it}}{Population_{i0}} + C'_{it}\beta + \theta_i + \lambda_t + \epsilon_{it} \quad (13)$$

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.

Next, recall that the allocation received by grantee  $i$  in year  $t$  can be expressed by the simplified Equation (11), a weighted combination of inputs  $X_{it}^k$  and their corresponding prices  $v_t^k$ . Equation (12) reveals that the prices  $v_t^k$  are determined externally from the perspective of grantee  $i$ . Federal appropriations  $A_t$  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 ( $\mathbf{X}_{it-1} \rightarrow \mathbf{X}_{it}$ ), but changes in prices from  $\mathbf{v}_{t-1} \rightarrow \mathbf{v}_t$  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.

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

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<sup>39</sup>In addition to redistributive shortcomings of the CDBG formula, Brooks and Sinitzyn (2014) also find that within localities, CDBG funding does not consistently reach communities with high levels of need.

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:

$$\widetilde{CDBG}_{it} = \max(\mathbf{X}_{i0}^A \mathbf{v}_t^A, \mathbf{X}_{i0}^B \mathbf{v}_t^B) \quad (14)$$

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.<sup>40</sup> [Goldsmith-Pinkham, Sorkin and Swift \(2020\)](#) show that the identifying assumption of this 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 [A11](#) 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

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<sup>40</sup>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 difference, most of the properties of traditional shift-share instruments apply to the simulated instrument.

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 (10), 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).

*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 every five years.<sup>41</sup> Samples are taken in non-census years, but most large governmental units are included with certainty in the annual sample.<sup>42</sup> 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 (10) from 2011 to 2017. The 2011 inputs were derived from the 2000 Census (except for population) and all subsequent years were derived from

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<sup>41</sup>The census is conducted in years ending in "2" and "7".

<sup>42</sup>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.

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.

### 5.2.3 Results and Discussion

I present OLS, reduced form, first stage, and IV estimates in Table 7. 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.

The estimate is consistent with 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 between \$0.25 and \$1, far greater than what an equivalent increase in tax revenue would generate (Inman, 2008). Indeed, the lower bound of the 95 percent confidence interval rules out multipliers as small as 0.57. A public spending multiplier greater than one is also consistent with findings from the Empowerment Zone program, which documented that each dollar of block grant provided by the program could be linked to an additional \$7 of outside money spent (Busso, Gregory and Kline, 2013).<sup>43</sup> Note however, that these spending multipliers do not include spending that may have been induced from private sources.

The CDBG also includes a provision known as the Section 108 Loan Guarantee Program. This

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<sup>43</sup>The block grant provided through the EZ program is known as the Social Services Block Grant, which provides funds for “essential social services that help achieve a myriad of goals to reduce dependency and promote self-sufficiency; protect children and adults from neglect, abuse and exploitation; and help individuals who are unable to take care of themselves to stay in their homes or to find the best institutional arrangements”. The authors also note that the accuracy of these data have been called into question and should be interpreted as a loose upper bound due to the fact that counterfactual spending is difficult to estimate.

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. [Prunella, Theodos and Thackeray \(2014\)](#) provide an in-depth summary of projects funded through Section 108 from 2002-2007. A total of 296 projects were funded at an average spend of \$4.6 million. 71% of funds were spent on economic development projects, 21% on public facilities, and 9% on housing. 79% of Section 108 projects also obtained funds from additional sources. Across all projects using Section 108 funds (including those that did not use outside funds), each dollar of Section 108 secured \$3.80 of other funds.<sup>44</sup> The authors also report that 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.” Furthermore, even without Section 108, 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 \(2017\)](#).

Using these estimates, I conclude by conducting a rudimentary back-of-the-envelope calculation of the public cost per job created by CDBG investments. I begin with the average cost per treatment of \$331,057 from column (4) in Table 1. This proxy for treatment amount includes only CDBG investments from the first three years of treatment (inclusive of the initial investment). Using Figure A2, I approximate that the typical CDBG spending in a treated tract over a 10-year period is \$632,636.<sup>45</sup> The public spending multiplier from the IV estimate in Table 7 suggests that the actual amount of public spending that occurred in the typical treated tract was approximately \$2M. At 108 jobs created per treatment (see Table 3, the typical public spending per job created was approximately \$18,500. By comparison, other cost-per-job estimates of prominent place-based policies include including financial incentives for firms to locate in specific municipalities (\$196,000 per job created), the Tennessee Valley Authority (\$77,000), customized job training (\$15,000), and cleanup of contaminated industrial sites (\$13,000) ([Bartik, 2020b](#)). While decentralized place-based policies funded by the CDBG are not strictly the most cost-effective among these programs, the flexibility and national scope of the block grants in conjunction with their ease of administration represents a uniquely promising opportunity

<sup>44</sup>Conditional on using other funds, each dollar of Section 108 secured \$4.69 of spending.

<sup>45</sup>To calculate this, I begin with the fact that the expected number of investment events within the first two years is 1.46 (1 in year 0, 0.25 in year 1, and 0.21 in year 2). The expected number of investment events in the remaining years is 1.33. I divide the \$331,057 cost per treatment by the 1.46 investment events to yield \$226,751 per investment event. I then multiply this by 2.79 expected investment events over the course of 10 years, which yields \$632,636.

to stimulate job growth in a wide variety of economically disadvantaged places.

## 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. This paper studies thousands of economic development investments funded by the CDBG in census tracts across the country. I estimate causal job impacts of these investments separately for each treated census tract using the intercept-shifted synthetic control method, a generalization of both differences-in-differences and traditional synthetic controls. I then correlate these estimates to potential determinants of effective place-based policies.

Large economic development projects funded by the CDBG increased jobs in low-income neighborhoods by roughly 7% ten years after the initial funding date. The increase was almost entirely driven by workers living in low- and moderate-income neighborhoods, and also disproportionately benefited workers living within two miles of the treated tract. Investments also generated similar rates of job growth in adjacent neighborhoods, while having minimal impact on housing values and rents. Investments were most effective when they directly supported local businesses—through financial and technical assistance, land clearance, and commercial/industrial construction. While the CDBG can only be used in low-income areas, job impacts were typically greatest in less disadvantaged tracts. On the other hand, CDBG investments were more effective in commuting zones that were less affluent and less dense. Finally, I verify that the block grant structure of the CDBG does not lead to crowd-out of public spending. Rather, the program appears to generate a fiscal multiplier of roughly three dollars of public spending per dollar of block grant.

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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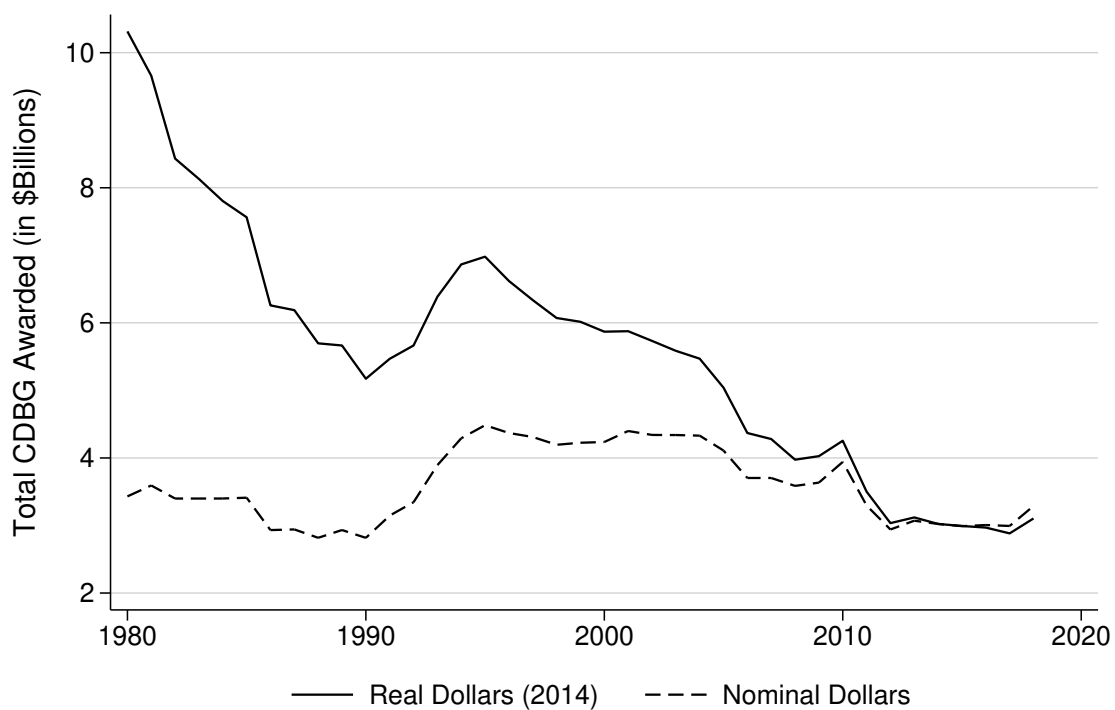
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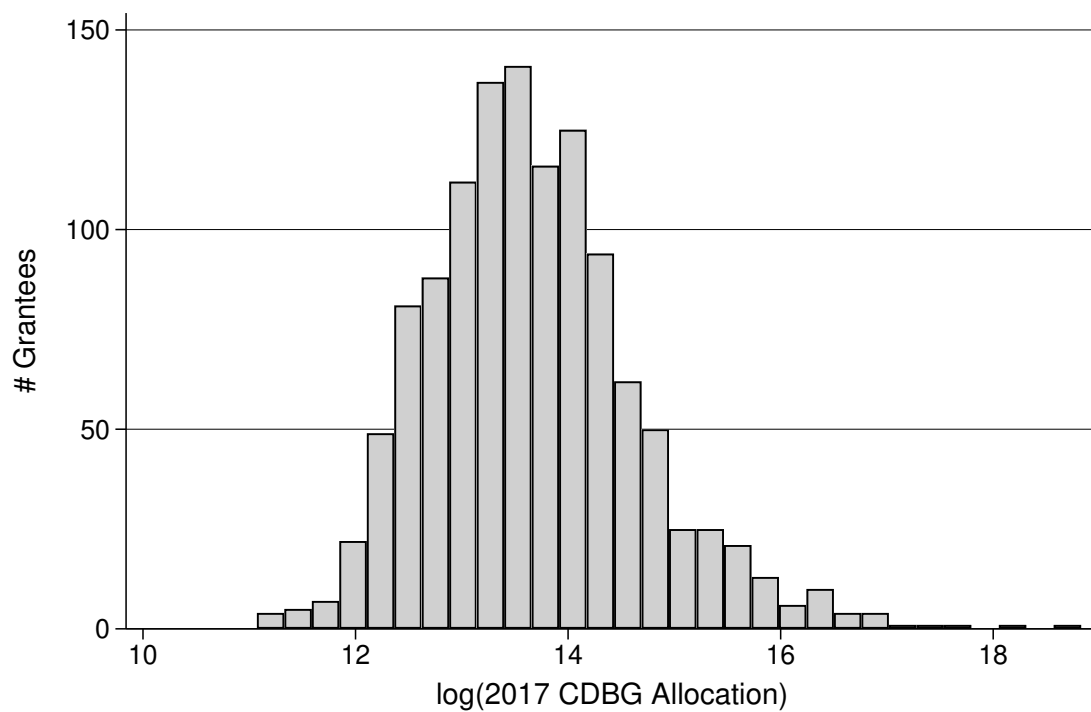
**Figure 1:** Historical CDBG Appropriations, Real vs. Nominal



*Note:* This figure shows how the total CDBG budget appropriated by Congress has changed over time. Data come from historical records which were obtained from the U.S. Department of Housing and Urban Development.

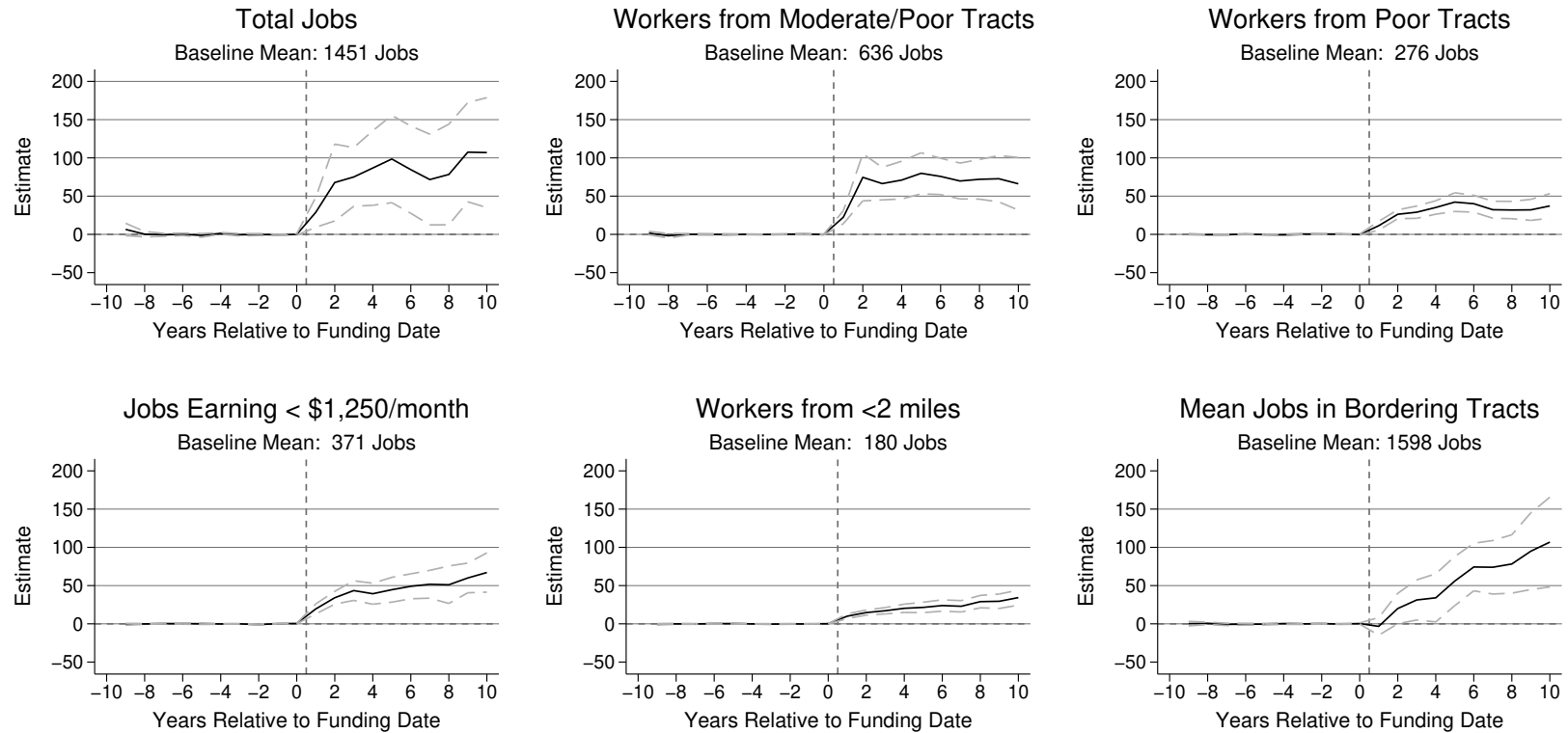


**Figure 2:** Distribution of Log CDBG allocation (2017)



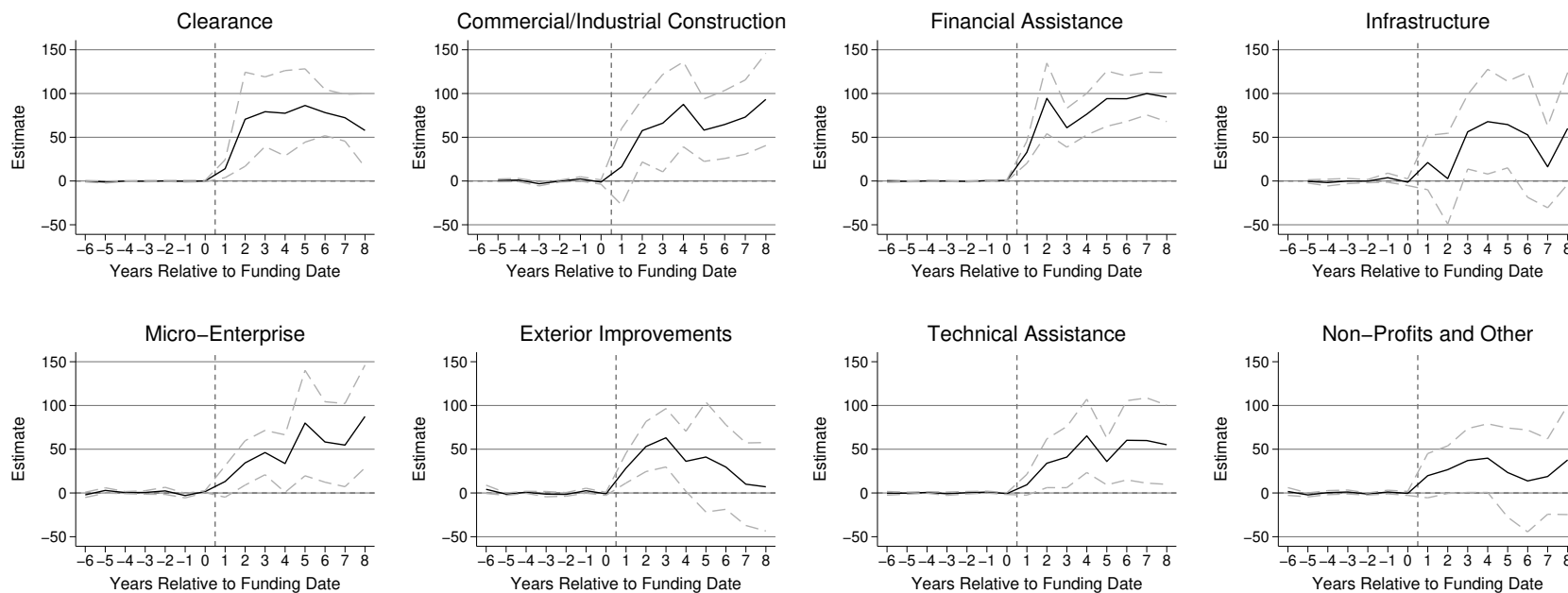
*Note:* This figure shows how the distribution of CDBG grantee allocations as of 2017 in nominal dollars. Dollar amounts are presented in logs. Data come from CDBG allocation spreadsheets provided by the U.S. Department of Housing and Urban Development.

**Figure 3: Effects of CDBG Investments on Jobs**



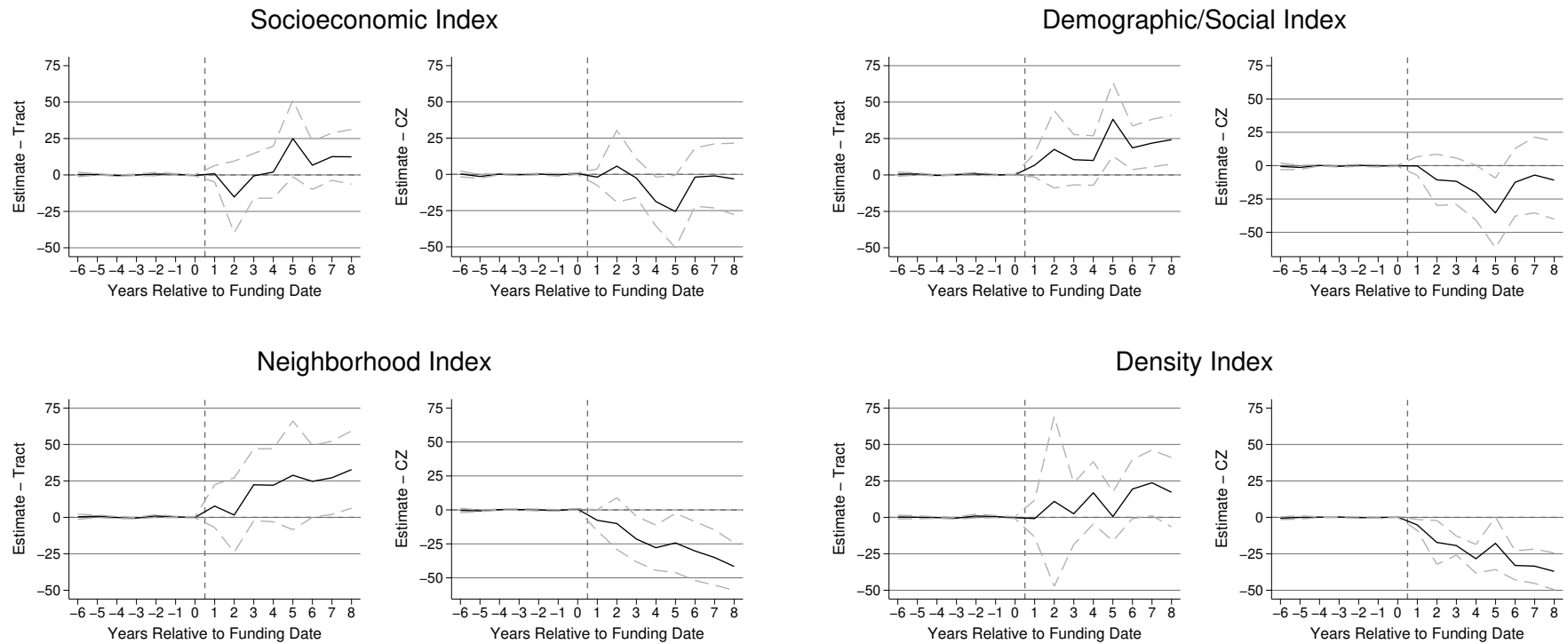
*Note:* This figure presents estimates of place-based investments funded by the CDBG across a variety of job outcomes. Each estimate is a pooled average of underlying synthetic control estimates. Synthetic control estimates were calculated for each of 1,375 treated census tracts; tracts with poor pre-treatment fit are removed as described in Section 3.3 and the remaining estimates are then pooled via Equation (7), which controls for calendar year fixed effects. Each estimate is weighted by the inverse of its synthetic control standard error. 95 percent confidence intervals are plotted using standard errors adjusted for clustering at the commuting zone level. Moderate/poor and poor tracts represent tracts with poverty rates greater than the 50th and 75th percentile for tracts within the commuting zone distribution. Low-wage jobs are jobs earning less than \$1,250 per month. Corresponding estimates are presented in tabular form in Table 3.

**Figure 4: Effects of CDBG Investments on Jobs, by Investment Type**

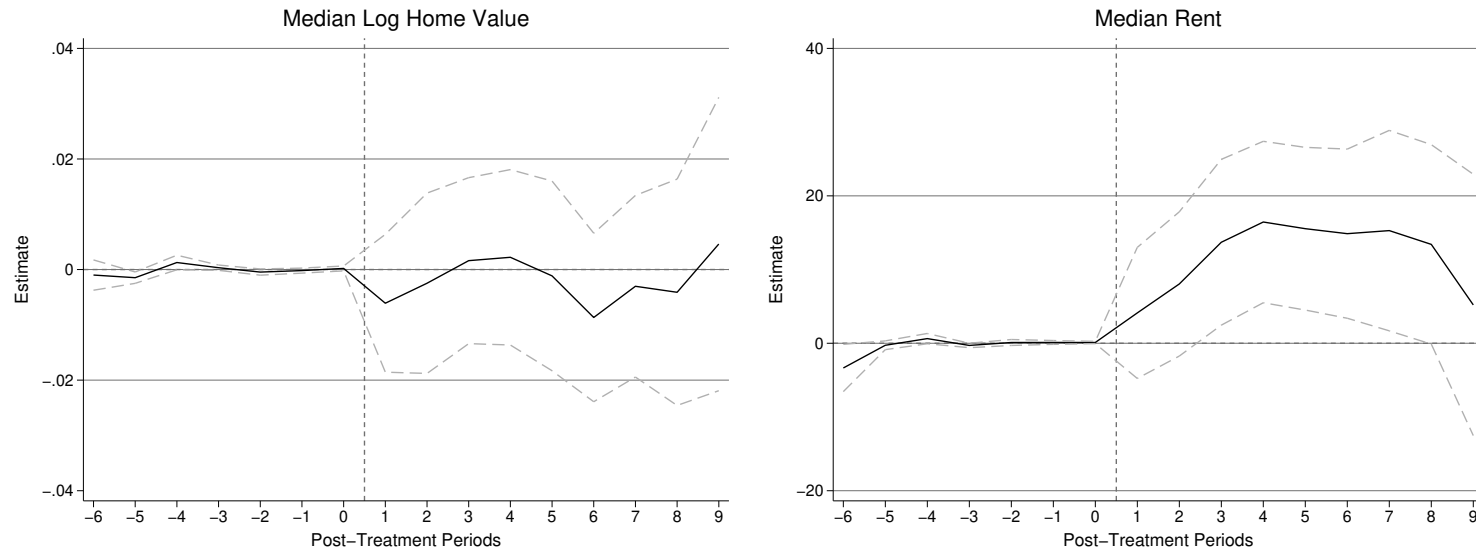


*Note:* This figure presents the impact of CDBG investments on jobs held by residents living in tracts with above-median poverty rates, shown across eight different activity categories funded by the CDBG. Estimates are obtained from Equation (7), estimated separately for the above eight activity categories. See Section 2.1 for a description of each category. Each plot above includes the set of treated tracts where treatment included at least one project associated with the category type. See Table 4 for estimates in tabular form. 90 percent confidence intervals are plotted using standard errors adjusted for clustering at the commuting zone level.

**Figure 5: The Correlation between Places and CDBG Effectiveness**



*Note:* This figure plots estimates from Equation (9). Each set of plots represents a separate index, described in Section 4.2. Each plot presents the association between job impacts of the CDBG for moderate/poor workers and a one standard deviation increase in each index, measured either at the tract level or the commuting zone level. Tabular estimates are provided in Table 5. 90 percent confidence intervals are plotted using standard errors adjusted for clustering at the commuting zone level.

**Figure 6: Effects on Housing**

*Note:* This figure presents estimates from Equation (7), using median log home prices and median rents as the outcomes of interest. See Section 5 for a description of the data collection process. This plot differs from the previous plots in that time periods are not uniform. The time periods correspond to the number of discrete periods relative to treatment, where periods include the decennial censuses from 1970, 1980, 1990, 2000, and the five-year ACS estimates from 2006-10 through 2014-18. Tabular estimates are provided in Table 6. 90 percent confidence intervals are plotted using standard errors adjusted for clustering at the commuting zone level.

**Table 1: Descriptive CDBG Statistics**

| <b>Project Type</b>      | <b>A. CDBG Economic Development Activity (2000-2016)</b> |                     |                 | <b>B. CDBG Treatments (3-Year Total)</b> |                          |                 |
|--------------------------|--|---------------------|-----------------|--|--------------------------|-----------------|
|                          | (1)<br>Avg. Activity (\$)                                | (2)<br># Activities | (3)<br># Tracts | (4)<br>Avg. Treatment (\$)               | (5)<br>Avg. # Activities | (6)<br># Tracts |
| 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. Column (6) shows the number of treated tracts with at least one project in each category.

**Table 2:** Baseline Tract Characteristics, Treated vs. Untreated vs. Synthetic Control

| <i>Census Tract Characteristics</i> | Treated<br>(1) | Untreated<br>(2) | Difference (T-U)<br>(3) | Difference (T-SCM)<br>(4) |
|-------------------------------------|----------------|------------------|-------------------------|---------------------------|
| <b>Socioeconomic Index</b>          | -0.48          | 0.03             | -0.51                   | -0.01                     |
| EPOP Ratio                          | 0.42           | 0.46             | -0.04                   | 0.00                      |
| HH Income                           | 45,064         | 64,844           | -19,779                 | 1,162                     |
| % in Poverty                        | 0.21           | 0.12             | 0.09                    | 0.01                      |
| % Professional Occupation           | 0.26           | 0.33             | -0.06                   | 0.00                      |
| <b>Demographic Index</b>            | -0.53          | 0.03             | -0.56                   | 0.02                      |
| % White                             | 0.57           | 0.69             | -0.13                   | -0.01                     |
| % Married                           | 0.43           | 0.55             | -0.11                   | 0.00                      |
| % College Educated                  | 0.18           | 0.25             | -0.07                   | 0.00                      |
| % HS Grad or Less                   | 0.58           | 0.48             | 0.10                    | 0.00                      |
| % Single Mother                     | 0.21           | 0.13             | 0.08                    | 0.01                      |
| <b>Neighborhood Index</b>           | -0.37          | 0.02             | -0.40                   | 0.05                      |
| Median Rent                         | 619            | 791              | -172                    | 10                        |
| Median Home Value                   | 133,824        | 192,133          | -58,308                 | 8,126                     |
| % Vacant Housing                    | 0.10           | 0.08             | 0.02                    | -0.01                     |
| <b>Density Index</b>                | 0.11           | -0.01            | 0.12                    | 0.07                      |
| Jobs per Sq. Mile                   | 4,095          | 2,003            | 2,092                   | 1,052                     |
| Population per Sq. Mile             | 5,835          | 5,416            | 419                     | 94                        |
| % Working Age (18-59)               | 0.58           | 0.58             | 0.00                    | 0.00                      |
| <b>Jobs</b>                         | 3,326          | 1,578            | 1,748                   | 559                       |
| From Moderate-Poor Tracts           | 1,592          | 691              | 901                     | 209                       |
| From Poor Tracts                    | 802            | 311              | 491                     | 138                       |
| Low-Wage Jobs                       | 965            | 492              | 473                     | 140                       |
| From Tracts within 5 Miles          | 430            | 200              | 230                     | 34                        |
| Mean Jobs in Bordering Tracts       | 2,352          | 1,578            | 2,858                   | -77                       |
| <b>N (Number of Tracts)</b>         | 1,375          | 60,215           | 61,590                  | 1,375                     |

*Note:* This table presents summary statistics for treated, untreated, and synthetic control tracts. Column (3) provides the difference between treated and untreated tracts, whereas Column (4) provides the difference between treated and synthetic control tracts. All variables represent baseline values and come from either the 2000 decennial census or the 2002 LEHD Origin-Destination Employment Statistics. Section 4.2 details how each of the four indices are constructed. Section 3.1 describes each of the job outcomes in detail.



**Table 3: Averaged Synthetic Control Estimates**

| Years After Funding Date             | 1                | 2                 | 3                 | 4                 | 5                 | 6                 | 7                 | 8                 | 9                  | 10                 |
|--------------------------------------|------------------|-------------------|-------------------|-------------------|-------------------|-------------------|-------------------|-------------------|--------------------|--------------------|
| <b>Jobs</b>                          |                  |                   |                   |                   |                   |                   |                   |                   |                    |                    |
| Estimate                             | 28.8***<br>(9.9) | 67.8***<br>(25.4) | 75.1***<br>(19.4) | 86.7***<br>(24.7) | 98.5***<br>(28.9) | 84.7***<br>(28.9) | 71.7**<br>(30.0)  | 78.3**<br>(33.3)  | 107.5***<br>(32.8) | 106.9***<br>(36.3) |
| N                                    | 781              | 781               | 781               | 781               | 781               | 781               | 781               | 683               | 564                | 467                |
| Baseline Mean                        | 1,451            | 1,451             | 1,451             | 1,451             | 1,451             | 1,451             | 1,451             | 1,451             | 1,451              | 1,451              |
| <b>Workers from Mod./Poor Tracts</b> |                  |                   |                   |                   |                   |                   |                   |                   |                    |                    |
| Estimate                             | 22.5***<br>(4.3) | 74.6***<br>(15.6) | 66.4***<br>(10.8) | 71.0***<br>(12.5) | 79.8***<br>(13.6) | 75.8***<br>(12.0) | 69.8***<br>(11.9) | 72.0***<br>(13.1) | 72.8***<br>(15.3)  | 66.1***<br>(17.4)  |
| N                                    | 761              | 761               | 761               | 761               | 761               | 761               | 761               | 660               | 542                | 450                |
| Baseline Mean                        | 636              | 636               | 636               | 636               | 636               | 636               | 636               | 636               | 636                | 636                |
| <b>Workers from Poor Tracts</b>      |                  |                   |                   |                   |                   |                   |                   |                   |                    |                    |
| Estimate                             | 11.3***<br>(3.0) | 26.3***<br>(2.9)  | 29.0***<br>(4.1)  | 35.2***<br>(4.4)  | 42.2***<br>(6.2)  | 40.1***<br>(5.6)  | 32.2***<br>(5.6)  | 31.8***<br>(5.7)  | 32.0***<br>(6.9)   | 37.2***<br>(8.1)   |
| N                                    | 707              | 707               | 707               | 707               | 707               | 707               | 707               | 616               | 512                | 430                |
| Baseline Mean                        | 276              | 276               | 276               | 276               | 276               | 276               | 276               | 276               | 276                | 276                |
| <b>Low-Wage Jobs</b>                 |                  |                   |                   |                   |                   |                   |                   |                   |                    |                    |
| Estimate                             | 19.5***<br>(3.3) | 34.1***<br>(4.2)  | 43.5***<br>(6.5)  | 39.4***<br>(6.9)  | 44.6***<br>(8.2)  | 49.1***<br>(8.2)  | 51.8***<br>(9.2)  | 51.1***<br>(12.4) | 60.0***<br>(9.8)   | 67.1***<br>(13.0)  |
| N                                    | 716              | 716               | 716               | 716               | 716               | 716               | 716               | 629               | 535                | 448                |
| Baseline Mean                        | 371              | 371               | 371               | 371               | 371               | 371               | 371               | 371               | 371                | 371                |
| <b>Workers from Nearby Tracts</b>    |                  |                   |                   |                   |                   |                   |                   |                   |                    |                    |
| Estimate                             | 10.0***<br>(1.5) | 14.7***<br>(1.7)  | 17.1***<br>(2.1)  | 20.3***<br>(2.7)  | 21.5***<br>(3.4)  | 24.0***<br>(3.8)  | 23.0***<br>(3.8)  | 29.1***<br>(4.1)  | 29.5***<br>(4.8)   | 34.3***<br>(5.0)   |
| N                                    | 666              | 666               | 666               | 666               | 666               | 666               | 666               | 599               | 516                | 436                |
| Baseline Mean                        | 180              | 180               | 180               | 180               | 180               | 180               | 180               | 180               | 180                | 180                |
| <b>Jobs in Adjacent Tracts</b>       |                  |                   |                   |                   |                   |                   |                   |                   |                    |                    |
| Estimate                             | -3.3<br>(5.8)    | 20.0**<br>(10.1)  | 31.3**<br>(13.3)  | 34.0**<br>(16.0)  | 55.9***<br>(16.3) | 74.3***<br>(15.8) | 74.0***<br>(17.7) | 78.3***<br>(19.4) | 95.0***<br>(25.4)  | 106.9***<br>(29.7) |
| N                                    | 1006             | 1006              | 1006              | 1006              | 1006              | 1006              | 1006              | 881               | 713                | 577                |
| Baseline Mean                        | 1,598            | 1,598             | 1,598             | 1,598             | 1,598             | 1,598             | 1,598             | 1,598             | 1,598              | 1,598              |

*Note:* This table presents the average of synthetic control estimates for each event time  $k$ . Averages are estimated via Equation (7), controlling for the year when treatment first occurred. Individual synthetic control estimates are weighed by the inverse of their respective standard errors. Standard errors in this table are adjusted for clustering at the commuting zone level. A graphical version is presented in Figure 3. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

**Table 4: Averaged Synthetic Control Estimates, by Activity Category**

| Years After Funding Date       | 1                 | 2                 | 3                 | 4                 | 5                 | 6                 | 7                  | 8                 |
|--------------------------------|-------------------|-------------------|-------------------|-------------------|-------------------|-------------------|--------------------|-------------------|
| <b>Clearance</b>               |                   |                   |                   |                   |                   |                   |                    |                   |
| Estimate                       | 14.1**<br>(6.1)   | 70.6**<br>(32.3)  | 79.2***<br>(23.9) | 77.5**<br>(29.3)  | 86.3***<br>(25.1) | 78.2***<br>(15.9) | 72.3***<br>(16.1)  | 57.8**<br>(25.1)  |
| N                              | 164               | 164               | 164               | 164               | 164               | 164               | 164                | 129               |
| <b>Comm./Ind. Construction</b> |                   |                   |                   |                   |                   |                   |                    |                   |
| Estimate                       | 16.3<br>(25.7)    | 57.6**<br>(21.1)  | 66.2*<br>(32.8)   | 87.6***<br>(28.6) | 58.2***<br>(21.1) | 64.6***<br>(22.9) | 73.1***<br>(25.0)  | 93.3***<br>(31.0) |
| N                              | 44                | 44                | 44                | 44                | 44                | 44                | 44                 | 40                |
| <b>Financial Aid</b>           |                   |                   |                   |                   |                   |                   |                    |                   |
| Estimate                       | 32.8***<br>(7.6)  | 94.5***<br>(24.2) | 60.9***<br>(13.3) | 76.4***<br>(14.3) | 94.1***<br>(19.0) | 94.0***<br>(15.6) | 100.0***<br>(14.7) | 95.9***<br>(16.8) |
| N                              | 344               | 344               | 344               | 344               | 344               | 344               | 344                | 305               |
| <b>Infrastructure</b>          |                   |                   |                   |                   |                   |                   |                    |                   |
| Estimate                       | 21.1<br>(18.1)    | 2.8<br>(30.1)     | 56.2**<br>(24.7)  | 67.8*<br>(34.8)   | 64.5**<br>(28.7)  | 52.7<br>(41.4)    | 16.3<br>(27.2)     | 60.1<br>(36.8)    |
| N                              | 27                | 27                | 27                | 27                | 27                | 27                | 27                 | 23                |
| <b>Micro-Enterprise</b>        |                   |                   |                   |                   |                   |                   |                    |                   |
| Estimate                       | 13.2<br>(10.7)    | 34.4**<br>(15.2)  | 46.2***<br>(15.1) | 33.6*<br>(19.6)   | 80.0**<br>(36.0)  | 58.3**<br>(27.5)  | 54.8*<br>(28.4)    | 87.5**<br>(35.0)  |
| N                              | 113               | 113               | 113               | 113               | 113               | 113               | 113                | 98                |
| <b>Exterior Improvements</b>   |                   |                   |                   |                   |                   |                   |                    |                   |
| Estimate                       | 28.4***<br>(10.1) | 53.1***<br>(17.0) | 63.1***<br>(19.8) | 36.2*<br>(20.4)   | 41.1<br>(37.2)    | 29.7<br>(28.6)    | 10.0<br>(28.0)     | 7.1<br>(29.8)     |
| N                              | 79                | 79                | 79                | 79                | 79                | 79                | 79                 | 64                |
| <b>Technical Aid</b>           |                   |                   |                   |                   |                   |                   |                    |                   |
| Estimate                       | 9.4<br>(7.0)      | 34.0**<br>(16.4)  | 41.0*<br>(20.5)   | 65.3**<br>(24.7)  | 36.0**<br>(15.7)  | 60.2**<br>(26.6)  | 59.9**<br>(28.7)   | 55.2**<br>(26.5)  |
| N                              | 101               | 101               | 101               | 101               | 101               | 101               | 101                | 94                |
| <b>Non-Profits and Other</b>   |                   |                   |                   |                   |                   |                   |                    |                   |
| Estimate                       | 19.8<br>(14.8)    | 26.7<br>(15.9)    | 37.1*<br>(21.4)   | 39.9*<br>(23.0)   | 23.4<br>(29.8)    | 13.8<br>(34.1)    | 18.9<br>(25.3)     | 37.7<br>(36.2)    |
| N                              | 41                | 41                | 41                | 41                | 41                | 41                | 41                 | 35                |

*Note:* This table presents averaged synthetic control estimates computed via Equation (7), calculated separately for each category type underlying CDBG economic development projects. The outcome of interest is jobs held by workers living in low- and moderate-income tracts. Treatments are included so long as one or more underlying project includes an activity of the given category. Individual synthetic control estimates are weighed by the inverse of their respective standard errors. Standard errors in this table are adjusted for clustering at the commuting zone level. A graphical version is presented in Figure 4. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

**Table 5: Place-Based Determinants of CDBG Effectiveness**

| Years After Funding Date        | 0               | 1               | 2                 | 3                  | 4                 | 5                 | 6                  | 7                  |
|---------------------------------|-----------------|-----------------|-------------------|--------------------|-------------------|-------------------|--------------------|--------------------|
| <b>Socioeconomic Index</b>      |                 |                 |                   |                    |                   |                   |                    |                    |
| Estimate - Tract                | 0.9<br>(3.4)    | -15.1<br>(14.9) | -0.6<br>(9.3)     | 2.0<br>(10.8)      | 25.0<br>(15.9)    | 6.7<br>(10.1)     | 12.6<br>(9.8)      | 12.5<br>(11.3)     |
| Estimate - CZ                   | -1.9<br>(3.4)   | 5.7<br>(15.0)   | -2.5<br>(8.1)     | -18.7*<br>(10.2)   | -25.6*<br>(15.1)  | -1.9<br>(12.1)    | -1.0<br>(13.4)     | -3.1<br>(14.9)     |
| N                               | 761             | 761             | 761               | 761                | 761               | 761               | 761                | 660                |
| <b>Demographic/Social Index</b> |                 |                 |                   |                    |                   |                   |                    |                    |
| Estimate - Tract                | 6.7<br>(5.0)    | 17.5<br>(16.0)  | 10.5<br>(10.4)    | 9.9<br>(10.3)      | 38.2**<br>(15.3)  | 18.6**<br>(9.0)   | 21.9**<br>(9.9)    | 24.2**<br>(10.1)   |
| Estimate - CZ                   | -0.2<br>(4.2)   | -10.6<br>(11.6) | -11.7<br>(10.5)   | -20.3<br>(12.4)    | -35.4**<br>(15.8) | -12.5<br>(15.3)   | -7.0<br>(17.2)     | -10.8<br>(17.7)    |
| N                               | 761             | 761             | 761               | 761                | 761               | 761               | 761                | 660                |
| <b>Neighborhood Index</b>       |                 |                 |                   |                    |                   |                   |                    |                    |
| Estimate - Tract                | 7.8<br>(8.9)    | 1.6<br>(15.4)   | 22.4<br>(14.9)    | 22.0<br>(15.2)     | 28.9<br>(22.6)    | 24.7*<br>(14.9)   | 27.2*<br>(15.2)    | 32.8**<br>(16.0)   |
| Estimate - CZ                   | -7.6<br>(4.7)   | -10.0<br>(11.5) | -21.4**<br>(10.1) | -27.8***<br>(10.1) | -24.3*<br>(13.2)  | -30.3**<br>(13.1) | -35.0***<br>(12.3) | -41.6***<br>(10.6) |
| N                               | 761             | 761             | 761               | 761                | 761               | 761               | 761                | 660                |
| <b>Density Index</b>            |                 |                 |                   |                    |                   |                   |                    |                    |
| Estimate - Tract                | -0.8<br>(7.7)   | 11.0<br>(35.2)  | 2.5<br>(12.7)     | 16.9<br>(12.9)     | 0.6<br>(10.2)     | 19.4<br>(12.3)    | 23.7*<br>(13.6)    | 17.3<br>(14.4)     |
| Estimate - CZ                   | -5.2**<br>(2.3) | -17.2*<br>(9.1) | -19.3***<br>(4.0) | -28.4***<br>(5.9)  | -17.8<br>(10.9)   | -33.0***<br>(6.0) | -33.5***<br>(7.1)  | -37.0***<br>(7.6)  |
| N                               | 761             | 761             | 761               | 761                | 761               | 761               | 761                | 660                |

*Note:* This table presents estimates from Equation (9), which correlates attributes of places to impacts of individual CDBG projects. The outcome of interest is jobs held by workers living in low- and moderate-income tracts. For each of the above attributes, Regression (9) includes both a tract-level measure and a commuting zone-level measure to differentiate between attributes of neighborhoods versus attributes of local labor markets. Individual synthetic control estimates are weighed by the inverse of their respective standard errors. Standard errors in this table are adjusted for clustering at the commuting zone level. A graphical version is presented in Figure 5. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

**Table 6: Effects of CDBG Investments on Housing Prices**

| Periods After Funding Date   | 1                 | 2                 | 3                | 4                | 5                 | 6                 | 7                 | 8                 | 9                |
|------------------------------|-------------------|-------------------|------------------|------------------|-------------------|-------------------|-------------------|-------------------|------------------|
| <b>Log Median Home Value</b> |                   |                   |                  |                  |                   |                   |                   |                   |                  |
| Estimate                     | -0.006<br>(0.008) | -0.002<br>(0.010) | 0.002<br>(0.009) | 0.002<br>(0.010) | -0.001<br>(0.010) | -0.009<br>(0.009) | -0.003<br>(0.010) | -0.004<br>(0.012) | 0.005<br>(0.016) |
| N                            | 462               | 462               | 462              | 462              | 462               | 462               | 418               | 370               | 297              |
| <b>Median Rent</b>           |                   |                   |                  |                  |                   |                   |                   |                   |                  |
| Estimate                     | 4.1<br>(5.4)      | 8.0<br>(5.9)      | 13.7**<br>(6.8)  | 16.4**<br>(6.6)  | 15.5**<br>(6.7)   | 14.9**<br>(6.9)   | 15.3*<br>(8.2)    | 13.4<br>(8.2)     | 5.2<br>(10.7)    |
| N                            | 499               | 499               | 499              | 499              | 499               | 499               | 458               | 400               | 326              |

*Note:* This table presents averaged synthetic control estimates, using log median home values and median rent as outcome variables. Housing price data come from the 1970-2000 decennial censuses and the 2006-10 through 2014-18 5-Year Estimates of the American Community Survey. Because of the staggered time periods, event time is measured in periods rather than in years. All estimates are computed via Equation (7). Individual synthetic control estimates are weighed by the inverse of their respective standard errors. Standard errors in this table are adjusted for clustering at the commuting zone level. A graphical version is presented in Figure 6. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

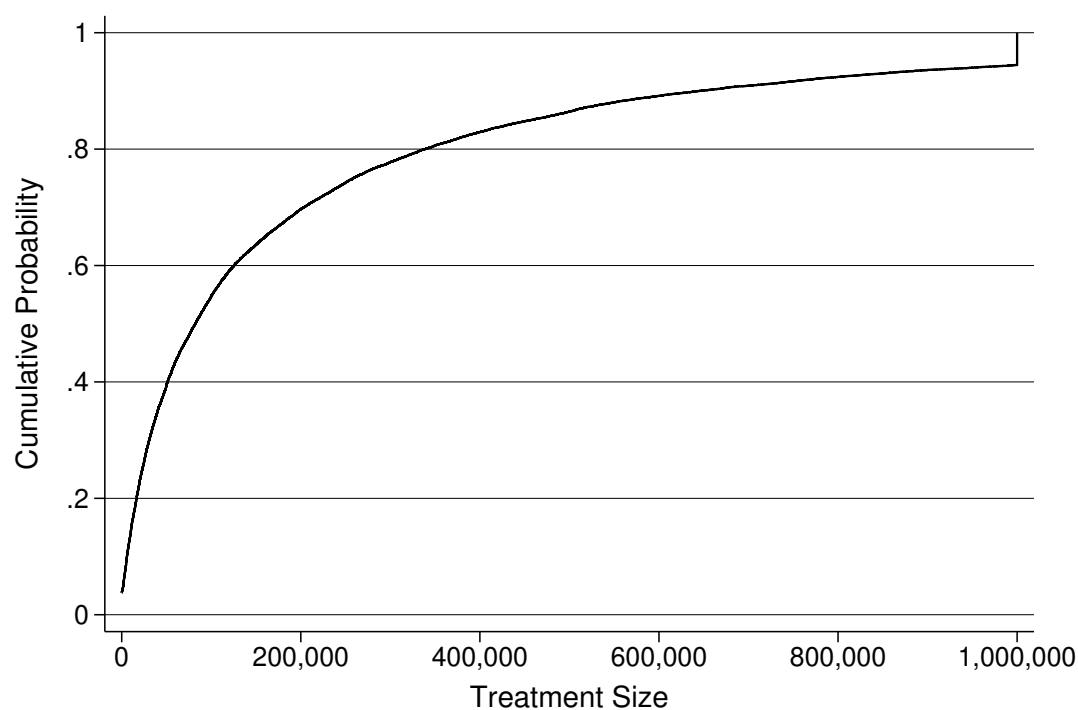
**Table 7: Estimates of CDBG Public Spending Multipliers**

|                              | OLS                   | Reduced Form          | First Stage            | Simulated IV          |
|------------------------------|-----------------------|-----------------------|------------------------|-----------------------|
| <i>Outcome (Per Capita):</i> | HCD Spend             | HCD Spend             | CDBG Allocation        | HCD Spend             |
| Per-capita CDBG              | 2.00**<br>[0.37-3.64] |                       |                        |                       |
| Per-capita Simulated CDBG    |                       | 1.86**<br>[0.29-3.44] | 0.59***<br>[0.50-0.68] | 3.16**<br>[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 (13). 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, as described in Section 5.2.1. 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 (14) and is derived in Section 5.2.2. 95 percent confidence intervals are presented in brackets, based off of standard errors that are adjusted for clustering at the grantee level. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

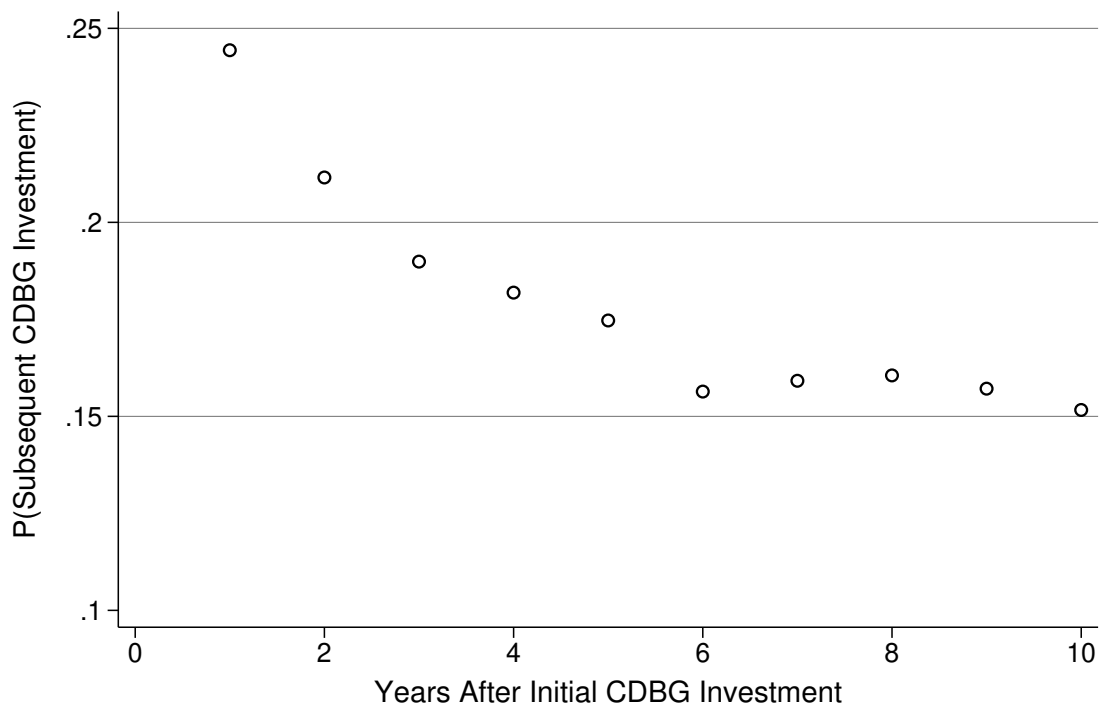
Appendix A: Appendix Tables and Figures

Figure A1: Cumulative Distribution of CDBG Treatment Amounts



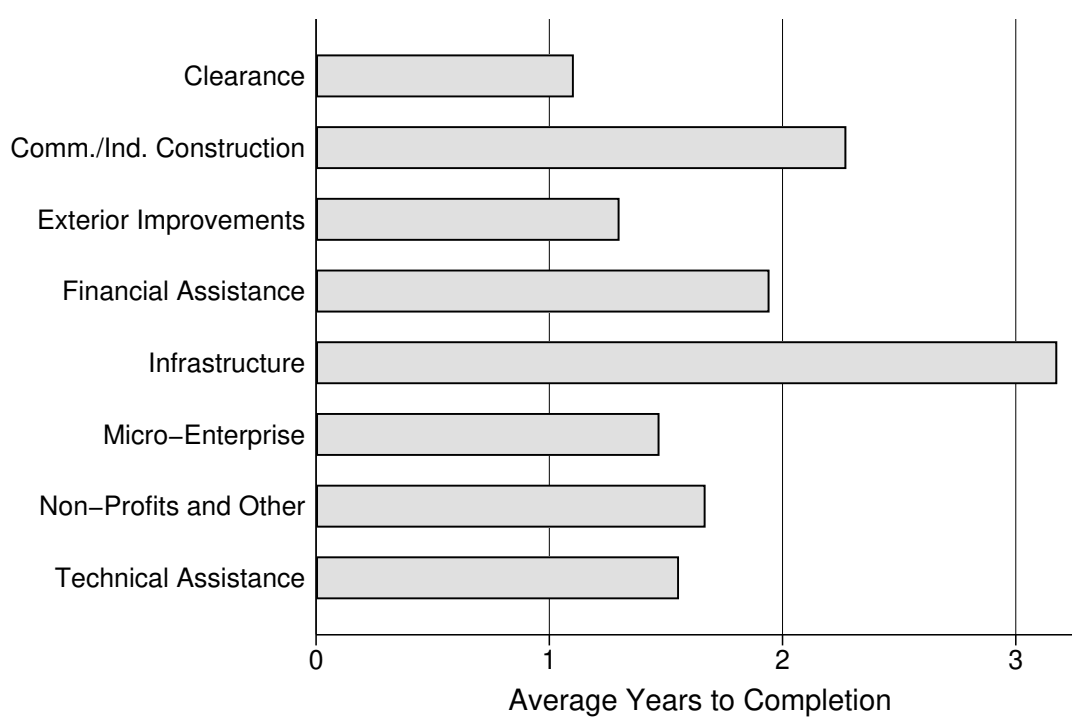
*Note:* This figure presents the cumulative distribution function of CDBG treatment amounts, defined as the three-year total of CDBG investments beginning with the first observed investment in a given tract. Treatments greater than \$1,000,000 have been top-coded for scaling purposes.

**Figure A2: Temporal Correlation of CDBG Investments**



*Note:* This figure shows the proportion of treated tracts that receive a subsequent investment  $t$  years after initial investment.

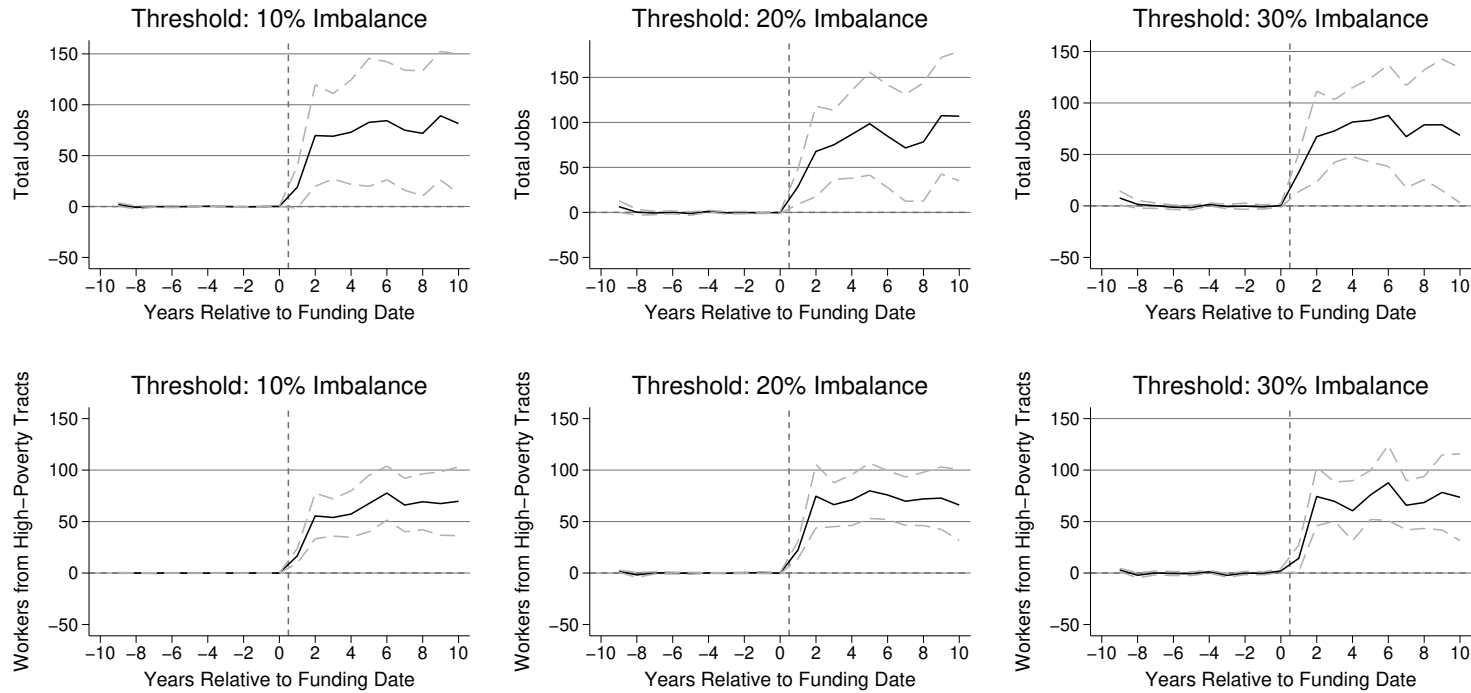
**Figure A3: Average Elapsed Time from Funding Date to Completion Date**



*Note:* This figure plots the average duration of time elapsed between the date when a project was funded by the CDBG and when the project was ultimately marked as completed within the CDBG's internal records.



**Figure A4: Sensitivity to Pre-Treatment Balance Threshold**



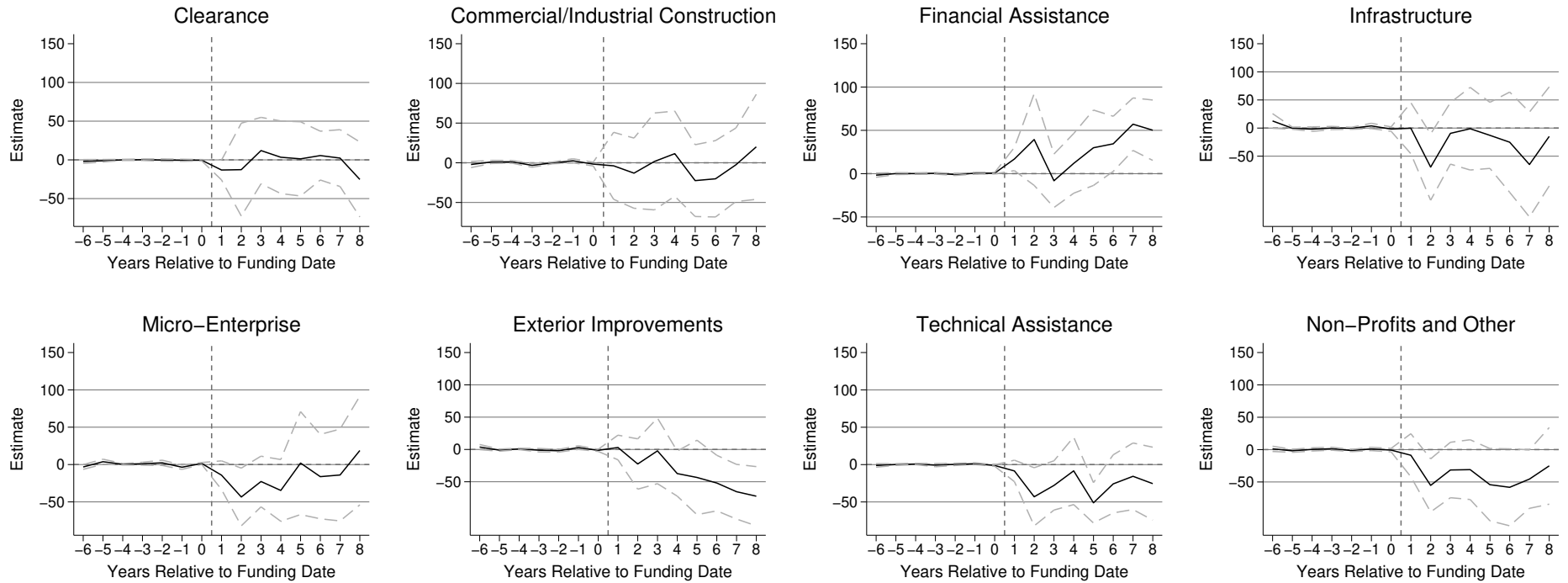
*Note:* This figure reproduces Figure 3, allowing the threshold for pre-treatment imbalance to fluctuate from 10% to 30%. Higher thresholds allow treated tracts with worse pre-treatment fit to enter into the analysis sample; lower thresholds restrict the analysis sample to tracts with excellent pre-treatment fit. 95 percent confidence intervals are plotted using standard errors adjusted for clustering at the commuting zone level.

**Figure A5: Effects of Large CDBG Investments on Jobs, by Investment Type**



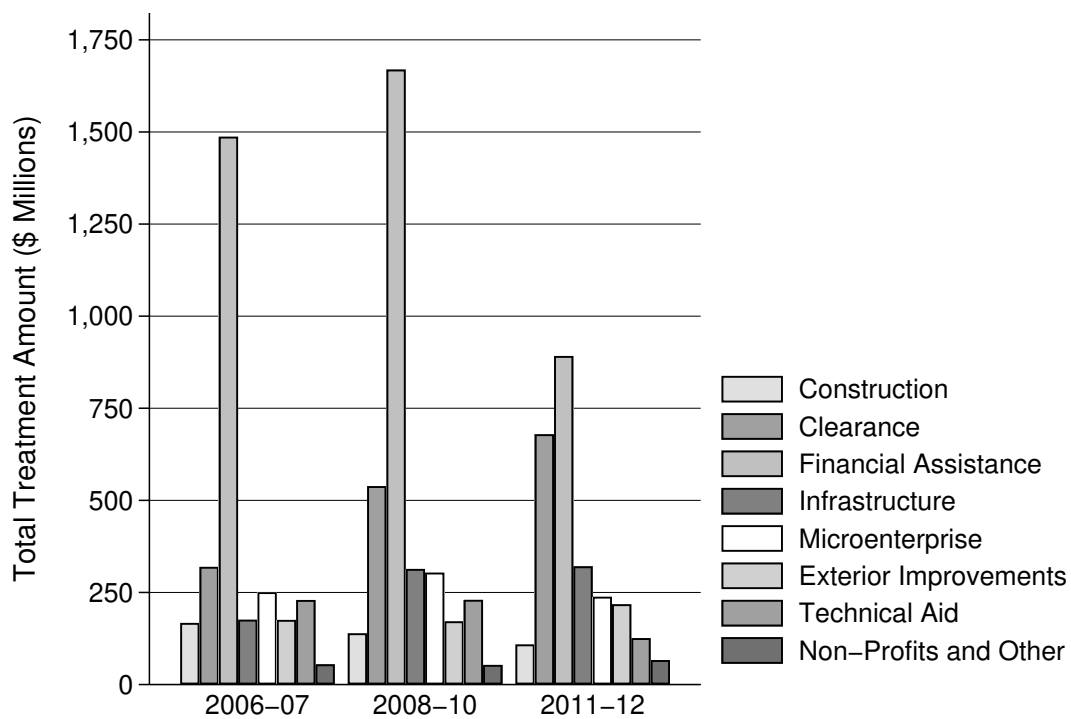
*Note:* This figure reproduces Figure 3, restricting the sample of treatments to the top 25% by treatment size (instead of the top 50% by size).

**Figure A6: Comparative CDBG type**



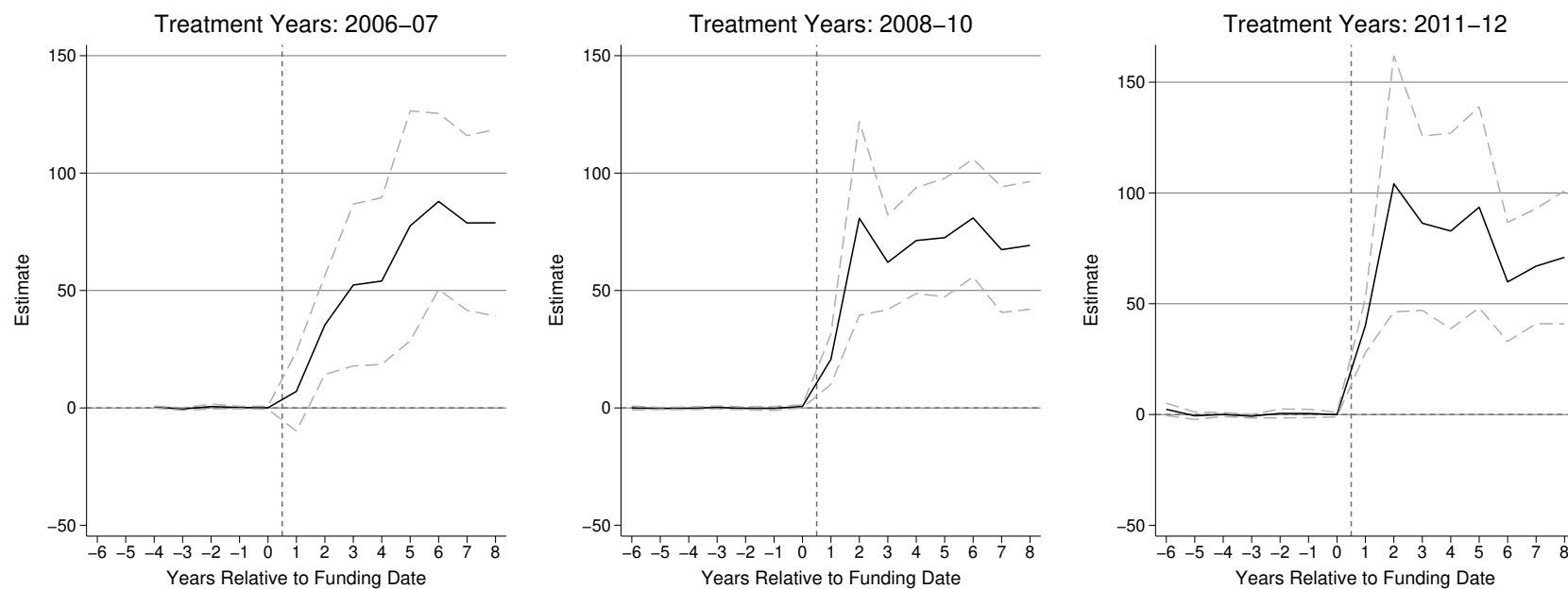
*Note:* This figure plots the correlation between each activity category and individual estimates of the CDBG on jobs held by workers from moderate/poor tracts. Estimates are obtained via Regression (8). 90 percent confidence intervals are plotted using standard errors adjusted for clustering at the commuting zone level.

**Figure A7: Breakdown of CDBG Activities by Treatment Year**



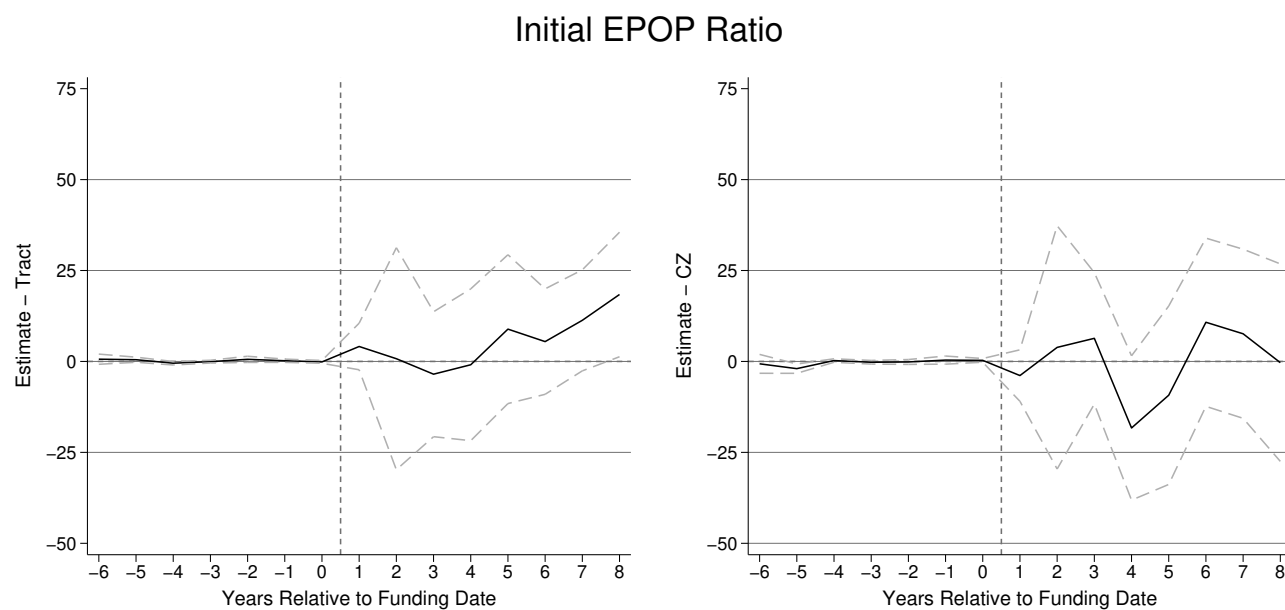
*Note:* This figure presents how CDBG expenditures were allocated across different investment activities during pre-recession (2006-07), recession (2008-10), and post-recession years (2011-12).

**Figure A8: CDBG Job Impacts, by Treatment Year**



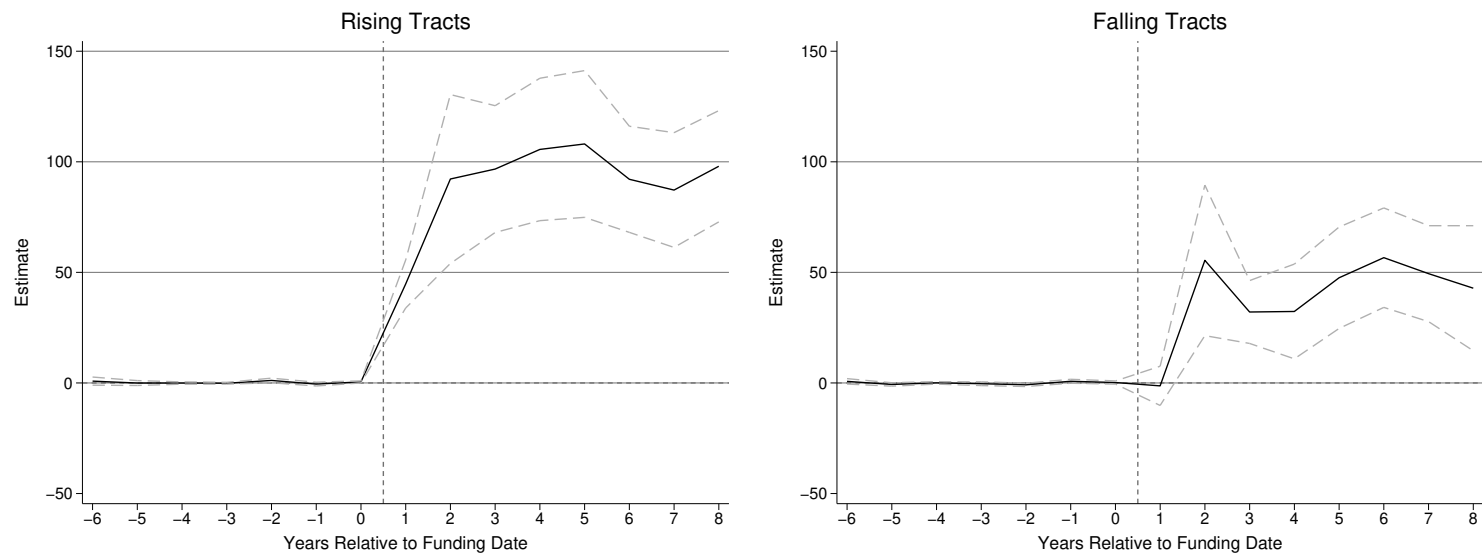
*Note:* This figure presents CDBG impacts on total job counts, estimated separately for the sample of treatments in pre-recession, recession, and post-recession years.

**Figure A9:** Association between CDBG Impacts and Initial EPOP Ratio



*Note:* This figure replicates Figure 5, focusing on employment-to-population ratio as measured in the 2000 decennial census.

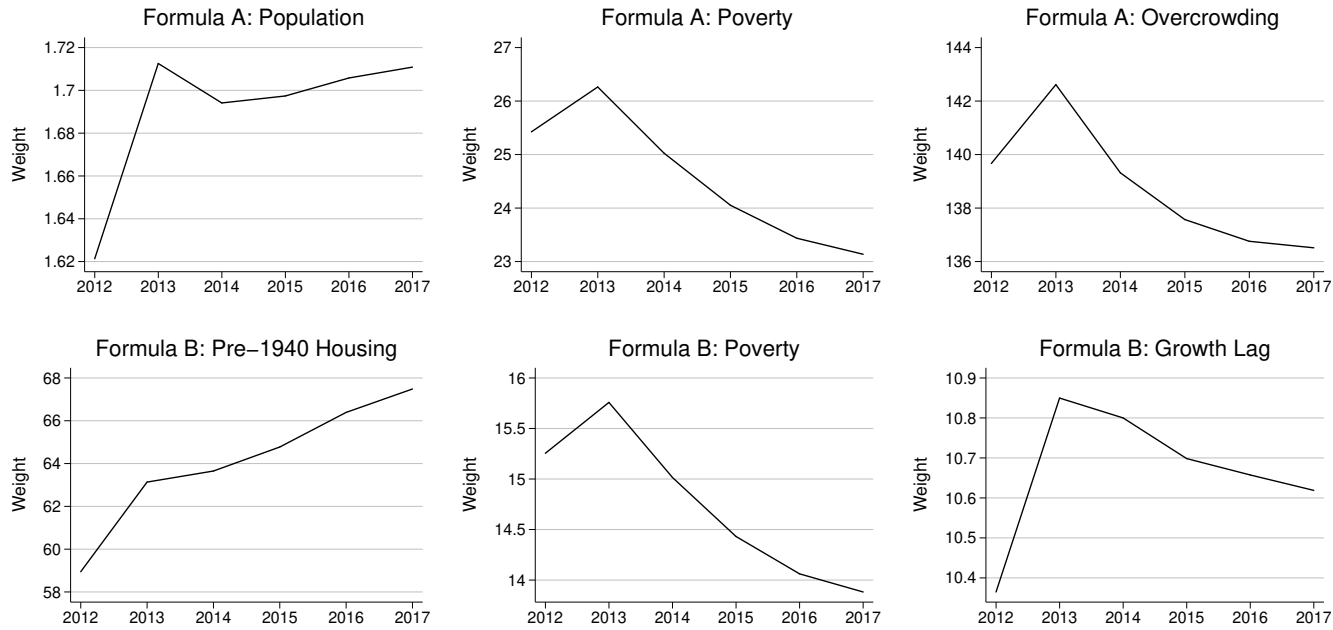
**Figure A10: CDBG Job Impacts, by Rising vs. Declining Tract**



*Note:* This figure presents CDBG job impacts separately for treated tracts depending on whether tract jobs increased or decreased between the initial period and the year prior to treatment.



**Figure A11: CDBG Formula Weights, 2012-2017**



*Note:* This figure presents the evolution of national input values  $v_t^k$  from Equations (11) and (12). 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.

**Table A1: Descriptive Statistics for Treated Tracts with Poor Fit**

| <i>Census Tract Characteristics</i> | Treated<br>(1) | Treated - Good Fit<br>(2) | Treated - Poor Fit<br>(3) |
|-------------------------------------|----------------|---------------------------|---------------------------|
| <b>Socioeconomic Index</b>          | -0.48          | -0.53                     | -0.42                     |
| EPOP Ratio                          | 0.42           | 0.42                      | 0.43                      |
| HH Income                           | 45,064         | 45,652                    | 44,289                    |
| % in Poverty                        | 0.21           | 0.21                      | 0.21                      |
| % Professional Occupation           | 0.26           | 0.25                      | 0.28                      |
| <b>Demographic Index</b>            | -0.53          | -0.62                     | -0.42                     |
| % White                             | 0.57           | 0.52                      | 0.62                      |
| % Married                           | 0.43           | 0.44                      | 0.43                      |
| % College Educated                  | 0.18           | 0.17                      | 0.19                      |
| % HS Grad or Less                   | 0.58           | 0.59                      | 0.56                      |
| % Single Mother                     | 0.21           | 0.22                      | 0.19                      |
| <b>Neighborhood Index</b>           | -0.37          | -0.38                     | -0.36                     |
| Median Rent                         | 619            | 631                       | 604                       |
| Median Home Value                   | 133,824        | 131,415                   | 136,998                   |
| % Vacant Housing                    | 0.10           | 0.10                      | 0.10                      |
| <b>Density Index</b>                | 0.11           | 0.02                      | 0.23                      |
| Jobs per Sq. Mile                   | 4,095          | 2,032                     | 6,807                     |
| Population per Sq. Mile             | 5,835          | 7,152                     | 4,103                     |
| % Working Age (18-59)               | 0.58           | 0.57                      | 0.59                      |
| <b>Jobs</b>                         | 3,326          | 1,411                     | 5,844                     |
| From Moderate-Poor Tracts           | 1,592          | 710                       | 2,752                     |
| From Poor Tracts                    | 802            | 353                       | 1,392                     |
| Low-Wage Jobs                       | 965            | 463                       | 1,624                     |
| From Tracts within 5 Miles          | 430            | 252                       | 664                       |
| Mean Jobs in Bordering Tracts       | 2,352          | 1,968                     | 2,858                     |
| <b>N (Number of Tracts)</b>         | 1,372          | 780                       | 592                       |

*Note:* This table presents the same summary statistics as in Table 2, presented separately for treated tracts and treated tracts with pre-treatment imbalance greater than (poor fit) and less than (good fit) 20%. Only tracts with good fit are ultimately used in the calculation of average treatment effects.

**Table A2: Relative Job Impacts, by Activity Category**

| Years After Funding Date       | 1                | 2                 | 3                | 4                | 5                | 6               | 7               | 8               |
|--------------------------------|------------------|-------------------|------------------|------------------|------------------|-----------------|-----------------|-----------------|
| <b>Clearance</b>               |                  |                   |                  |                  |                  |                 |                 |                 |
| Estimate                       | 3.3<br>(9.2)     | -23.7*<br>(13.8)  | -28.0<br>(25.3)  | -54.9*<br>(31.9) | -46.2*<br>(27.5) | -9.6<br>(30.3)  | -31.2<br>(34.1) | -17.3<br>(47.9) |
| N                              | 453              | 453               | 453              | 453              | 453              | 409             | 344             | 275             |
| <b>Comm./Ind. Construction</b> |                  |                   |                  |                  |                  |                 |                 |                 |
| Estimate                       | -19.9<br>(28.9)  | 46.2*<br>(23.8)   | 33.8<br>(31.4)   | 33.0<br>(62.3)   | 20.0<br>(40.5)   | 22.9<br>(35.6)  | 19.1<br>(44.7)  | 63.6<br>(57.1)  |
| N                              | 453              | 453               | 453              | 453              | 453              | 409             | 344             | 275             |
| <b>Financial Aid</b>           |                  |                   |                  |                  |                  |                 |                 |                 |
| Estimate                       | 26.3**<br>(10.6) | 34.2**<br>(16.6)  | -4.1<br>(28.6)   | 46.7*<br>(28.0)  | 42.4<br>(31.0)   | 42.8<br>(33.1)  | 61.6*<br>(32.1) | 31.9<br>(38.0)  |
| N                              | 453              | 453               | 453              | 453              | 453              | 409             | 344             | 275             |
| <b>Infrastructure</b>          |                  |                   |                  |                  |                  |                 |                 |                 |
| Estimate                       | -30.2<br>(22.2)  | -88.2**<br>(34.3) | -49.2<br>(34.8)  | -51.2<br>(34.4)  | -35.5<br>(38.5)  | -14.8<br>(53.2) | -42.2<br>(47.7) | -42.0<br>(62.8) |
| N                              | 453              | 453               | 453              | 453              | 453              | 409             | 344             | 275             |
| <b>Micro-Enterprise</b>        |                  |                   |                  |                  |                  |                 |                 |                 |
| Estimate                       | -16.7<br>(11.5)  | -20.8<br>(19.7)   | 66.7<br>(71.5)   | 37.4<br>(28.5)   | 19.4<br>(33.8)   | -17.5<br>(28.1) | -36.5<br>(39.3) | -43.4<br>(35.2) |
| N                              | 453              | 453               | 453              | 453              | 453              | 409             | 344             | 275             |
| <b>Exterior Improvements</b>   |                  |                   |                  |                  |                  |                 |                 |                 |
| Estimate                       | -21.3<br>(26.1)  | -17.9<br>(33.8)   | -56.7*<br>(30.9) | -47.5<br>(38.6)  | -54.2<br>(39.1)  | -40.7<br>(38.4) | -20.0<br>(45.1) | -15.6<br>(57.0) |
| N                              | 453              | 453               | 453              | 453              | 453              | 409             | 344             | 275             |
| <b>Technical Aid</b>           |                  |                   |                  |                  |                  |                 |                 |                 |
| Estimate                       | -5.2<br>(10.7)   | 30.8<br>(39.4)    | 110.8<br>(97.2)  | -2.2<br>(45.0)   | 36.2<br>(53.3)   | 36.1<br>(69.1)  | 34.5<br>(57.0)  | 65.1<br>(87.7)  |
| N                              | 453              | 453               | 453              | 453              | 453              | 409             | 344             | 275             |
| <b>Non-Profits and Other</b>   |                  |                   |                  |                  |                  |                 |                 |                 |
| Estimate                       | -0.9<br>(14.8)   | -19.2<br>(19.6)   | -14.0<br>(37.7)  | -29.0<br>(43.6)  | -35.1<br>(36.7)  | -12.5<br>(32.4) | -14.3<br>(45.6) | -5.2<br>(98.3)  |
| N                              | 453              | 453               | 453              | 453              | 453              | 409             | 344             | 275             |

*Note:* This table presents estimates from Equation (8), which attempts to correlate the effectiveness of treatments across different policy types. Individual synthetic control estimates are weighed by the inverse of their respective standard errors. Standard errors in this table are adjusted for clustering at the commuting zone level. A graphical version is presented in Figure A6. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

**Table A3: 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  $j$ .