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

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Abstract

Growing economic disparities across the U.S. have increased the need for effective place-based jobs policies. This paper seeks to uncover determinants of effective policies 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 jobs increase by 13% over ten years in census tracts where large CDBG investments occurred, without a corresponding increase in home prices. The increase in jobs is driven by low-income workers living in close proximity. The most effective place-based investments provided direct financial assistance to businesses or subsidized commercial/industrial construction. While the CDBG can only be deployed in lower-income neighborhoods, investments had greater job impacts in comparatively less-disadvantaged tracts. 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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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, 2020*b*). However, many practical and fundamental questions about effective place-based policy design remain unanswered, particularly with respect to how federal policies can be designed to meet the diverse needs of economically distressed communities across the country. Perhaps even more fundamentally, there remains little evidence on the types of place-based interventions that consistently generate sustained job growth, and where place-based policies should be targeted to maximize impacts.

This paper provides new insights on these issues by evaluating the jobs impact of the Community Development Block Grant (CDBG), a program which allocates $3-4 billion annually in federal block grants to local governments across the country to flexibly fund economic and community development activities in low-income neighborhoods. I use data on over 40,000 spatially-targeted investments funded by the CDBG for creating jobs in thousands of low-income census tracts nationwide. The CDBG provides a new perspective on federal place-based policies rooted in fiscal federalism, combining the scale of federal programs with the benefits (and pitfalls) of decentralization and tailoring policies to local needs. The breadth and flexibility of the CDBG also presents a unique opportunity to analyze and compare many different kinds of place-based policies within a unifying empirical and administrative framework.

I begin by studying the viability of decentralized place-based policymaking by estimating census tract-level job impacts of major CDBG investments. To do so, I link census block-level job counts from the LEHD Origin-Destination Employment Statistics (LEHD-LODES) to geocoded activity-level data from the CDBG. Tracts that receive large CDBG investments differ from untreated tracts in a variety of unobserved ways. Treated tracts will also differ from other treated tracts when local governments are given discretion to determine their own funding priorities. Comparison groups should therefore be flexibly chosen to account for varying sources of unobserved selection across treated tracts. A related problem is that the specific mix of chosen investments and the timing of treatment are likely endogenous to underlying tract attributes and trends. This further exacerbates issues highlighted by recent criticisms of two-way fixed effect methods where treated units with staggered adoption and heterogeneous treatment effects are pooled together in differences-in-differences and event study frameworks (de Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2018; Sun and Abraham, 2020; Callaway and Sant'Anna, 2019).

I instead opt to estimate causal effects for each treated tract separately before aggregating individual estimates together. I use the synthetic control method (SCM), an approach that generates causal estimates for individual treated units by constructing a counterfactual from a weighted combination of untreated units that closely reproduce pre-treatment outcomes (Abadie, Diamond and Hainmueller, 2010). For each treated tract, I construct a synthetic control tract using a weighted subset of untreated donor tracts from the same commuting zone, limiting donor candidates to a smaller subset of untreated tracts with characteristics that predict treatment status. 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, an approach which combines the strengths of both synthetic controls and standard differences-in-differences. The method 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 have 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.

I find that the top quartile of CDBG treatments by size generate positive and persistent average effects on tract-level job counts. Roughly 140 jobs are created ten years after the initial funding date, a 13% increase relative to baseline job counts. While the typical investment size associated with the top quartile of treatments is approximately $600,000, this measure does not include other public spending that the CDBG may have induced—a topic I return to in the final section of the paper. 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 trajectory of job growth in adjacent untreated tracts, suggesting that CDBG investments generate positive job spillovers in nearby areas. Finally, I find little to no change in median rents and home values. Taken together, the combination of more jobs for local low-income workers and a minimal welfare-offsetting response in housing costs suggests a net welfare increase in neighborhoods where the CDBG was implemented (Kline and Moretti, 2014).

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 job impacts are most positively correlated with investments that either provide direct financial assistance to businesses or subsidize construction for commercial and industrial use. Infrastructure, the category with the highest per-activity cost, appears to generate little discernible impact on jobs. While infrastructure is commonly perceived as a pillar of place-based policies, the types of infrastructure investments funded by the CDBG (e.g. parking, rail transport, streets) tend to be public improvements with little direct impact on firms. Taken as a whole, these findings suggest that investments which directly increase firm productivity 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, 2020*a*).

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 areas targeted by the CDBG, job impacts are larger in tracts scoring *highly* on socioeconomic, demographic, and neighborhood indices. This suggests that job growth in low income areas may be easier to accomplish in neighborhoods with better amenities and resident populations with greater workforce attachment. Conditional on neighborhood-specific characteristics, characteristics of local labor markets as a whole do not appear particularly predictive. This suggests that place-based policies can be successfully implemented in a wide variety of labor market settings so long as high-growth neighborhoods are strategically targeted.

I conclude by analyzing whether the block grant structure of the CDBG induces additional public spending or crowds out existing spending. One primary objective of block grants is to increase local public spending on specific activities that the federal government deems as desirable. Quantifying potential crowd-out is important because the CDBG should only generate impacts on spending that would not have occurred in its absence. On the other hand, the existence of fiscal multipliers may imply that local governments face liquidity constraints and funding gaps that the block grants are able to address. 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.

Understanding fiscal multipliers is also important for quantifying the cost-per-job of the CDBG given that each dollar of block grant could be tied to multiple dollars of public spending. Using these estimates, I conduct a back-of-the-envelope calculation suggesting that each job created requires 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, 2020*b*). These findings suggest that decentralizing place-based policies through flexible block grants appears to be a promising and cost-effective approach to stimulate job growth in a wide variety of economically disadvantaged places.

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 since its inception, the program currently faces scrutiny over its lack of transparency and accountability. Allegations of government waste, abuse, and fraud have grown increasingly common, and President Trump has notably omitted the program from his federal budget proposal in four consecutive years.

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 entitlement 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.[1]

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).[2] The service area calculation is automated via an internal database which calculates geographic LMI concentrations using the American Community Survey.[3]

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 place-based jobs policies. Economic development projects can be further categorized as follows:[4]

[1] 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.

[2] AMI is calculated separately for each metropolitan area or non-metropolitan county in the ACS.

[3] An activity's service area is delineated at the census tract level. Prior to the ACS, the calculations were conducted using the decennial census.

[4] The language used for these descriptions are taken directly from the CDBG's "Matrix Code Definitions", which can be

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.[5]

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.

accessed here: https://files.hudexchange.info/resources/documents/Matrix-Code-Definitions.pdf.

[5] 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, 2020*b*).[6] 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-

[6] 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.[7,8]

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).[9] 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-

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

[8] 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.

[9] 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).[10] 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 the Jobs Impact of the CDBG

3.1 Empirical Strategy

I begin by investigating the viability of decentralizing place-based jobs policies via the CDBG. To do so, I estimate tract-level estimates of CDBG investments on job counts. Following previous empirical work on place-based policies, I focus on census tracts as the primary geographic unit of observation.[11] Estimating tract-level causal effects of the CDBG is complicated by two primary issues. First, tracts that receive CDBG investments differ from untreated tracts in unobservable ways, necessitating the choice of a proper comparison group. The issue is further complicated by the fact that treated tracts will also differ from other treated tracts when municipalities have different criteria for

[10] 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.

[11] 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.

choosing where to invest. A uniform comparison group would likely be inadequate for handling the unobserved differences across treated tracts. The second issue is that treatment timing and the mix of activities funded may themselves be endogenous to tract attributes and trends. This exacerbates issues that have recently been documented with traditional two-way fixed effect methods, which implicitly compare treated tracts to other treated tracts from other cohorts and groups, leading to contaminated estimates and negative weights when treatment effects are heterogeneous across cohorts or groups (Goodman-Bacon, 2018; Sun and Abraham, 2020).[12] 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,[13] 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.

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

[12] 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'Haultfœuille (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.

[13] 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.

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 $i = 1, ..., N$ census tracts over the years $t = 1, ..., T$. 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 $j = 1, ..., J$. I require treatment to be an absorbing state, so census tracts remain treated for the remainder of the panel after receiving a CDBG investment for the first time. In reality, an investment in one period may lead to an endogenous response of additional CDBG (or even external) investments in subsequent periods. My estimates therefore represent the overall effect of an initial CDBG investment while allowing for any future endogenous responses. I return to this issue in the "Implementation" section below. I denote T_j as the year in which census tract j becomes treated.

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

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

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

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

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:

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

where inference is conducted empirically using jackknife standard errors.[14,15]

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

$$\min_{W_j \in \Delta_j^{scm}} \frac{1}{2(T_j - 1)} \sum_{\ell=1}^{T_j-1} \left(Y_{j,T_j-\ell} - \sum_{d_j=1}^{L_j} w_{d_j} Y_{i,T_j-\ell} \right)^2 \tag{4}$$

where the weights in $\mathbf{W_j}$ are conventionally non-negative and sum to one.[16] This characterization of the optimization problem differs slightly from the original characterization proposed by Abadie, Diamond and Hainmueller (2010) in that it includes only lagged outcomes as predictors and does not use other covariates. This modification has become standard in many recent applications of the SCM and is necessary for the intercept-shift modification, as I describe further below.

The key empirical challenge underlying SCM is determining whether $\hat{Y}_{j,T_j+k}(0)$ is a reasonable estimate for $Y_{j,T_j+k}(0)$, the counterfactual outcome for tract j in the absence of treatment. Abadie, Diamond and Hainmueller (2010) and Abadie (forthcoming) show that the bias of the SCM estimate

[14] 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.

[15] Another method of conducting inference in the SCM setting is via permutation (or "placebo") tests. This approach assigns a placebo treatment status to each of the donor tracts and and re-computes the synthetic control algorithm for every donor tract. P-values are then calculated by comparing the size of the treatment estimate to the distribution of placebo treatment estimates. I opted not to use permutation-based inference for two reasons. The first is practical in nature: given that I compute SCM estimates separately for hundreds of treated tracts, the number of placebo calculations will increase multiplicatively as the number of donor tracts increases. To illustrate, with 700 treated tracts and an average of 50 donor units per tract, the analysis would require 35,000 separate runs of the SCM. The second reason is that the permutation test typically assumes that treatment is randomly assigned—which is not the case in the CDBG setting. I instead follow Arkhangelsky et al. (2019) and use the leave-one-unit-out jackknife approach to empirically quantify uncertainty.

[16] 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.

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:

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

where δ_t represents an unknown common factor with constant factor loadings, λ_t represents a $(1 \times F)$ vector of common factors, μ_i represents an $(F \times 1)$ vector of unknown factor loadings representing unobserved features of specific census tracts. This structure allows the outcome Y_{it} to depend on multiple unobserved components μ_i that may have time-varying impacts on outcomes via the coefficients λ_t.[17]

The basic justification for synthetic controls under this structure is as follows. If the synthetic control for a given treated unit j manages to closely reproduce the pre-treatment outcomes $Y_{jt}(0)$ for $t < T_j$ but does not reproduce the values of μ_j in Equation (5), then it must be the case that the individual transitory shocks ϵ_{it} are exactly compensating for the differences in unobserved factor loadings in each pre-treatment period. This scenario becomes more and more unlikely as the number of pre-treatment periods increases, or as the scale of transitory shocks decreases. Abadie, Diamond and Hainmueller (2010) derive a bound for the bias of the SCM estimate as a function of these two parameters. Taking into account that pre-treatment fit is rarely perfect, Ben-Michael, Feller and Rothstein (2018) bound the bias as an increasing function of 1) imbalance in the pre-treatment outcomes, and 2) approximation error from balancing lagged outcomes instead of the latent factors themselves, which decreases to zero as the number of pre-treatment periods approaches infinity. To summarize, the 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 ϵ_{it}.

Despite the crucial importance of excellent pre-treatment fit, achieving such fit can be difficult in practice. When pre-treatment fit is imperfect, a number of recent papers have suggested modifying traditional SCM by de-meaning the outcome variable for all units using their pre-treatment averages, which is referred to as "intercept-shifted" or "de-meaned" SCM (Doudchenko and Imbens, 2016; Fer-

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

man and Pinto, 2019; Ben-Michael, Feller and Rothstein, 2018, 2019; Arkhangelsky et al., 2019).[18] The basic intuition for this modification maps to the intuition of standard differences-in-differences: the SCM assumption requiring levels of the outcome to match is relaxed in favor of a common trends assumption by allowing treatment and control outcomes to vary by a constant intercept shift. With this modification, the intercept-shifted SCM only distinguishes itself from differences-in-differences in that the weights on control units are allowed to vary instead of being uniform across all units. The estimated treatment effect τ_{jk} using this approach is as follows:

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

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 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.

This basic definition of treatment has several shortcomings. First, annual tract-specific investments are typically small in scale, with a median size of $50,000 (see Figure A1). The cost-per-job-

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

created of various place-based policies is known to range from $10,000 to over $100,000 (Bartik, 2020*b*). Given this, even estimates of the median treatment are unlikely to be sufficiently powered to detect job impacts. Focus on larger CDBG treatments is also consistent with the notion in the place-based literature that "big pushes" are needed to generate lasting local impacts (Moretti, 2012). I consequently focus my attention to the top 25% of treatments. While this does not ultimately affect the internal validity of the synthetic control estimates, the results may not necessarily extrapolate to smaller-scale treatments.

CDBG investments are also temporally clustered. Local governments may aim to revitalize neighborhoods through multi-year plans, or may need to spread out CDBG spending due to funding constraints. 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. The 75th percentile among treatments measured in this way is approximately $250,000. Again, these assumptions on treatment size ultimately do not affect identification, but may affect which treated tracts are included at the margin for individual estimation.

Finally, the SCM requires a sufficient number of pre-treatment periods to mitigate potential bias from overfitting. I choose to restrict the sample of treated tracts to those with at least five years of pre-treatment data. 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.[19] To ensure that I am able to estimate medium-run effects for each tract, I also remove tracts for which treatment begins with less than seven years remaining in the panel.

Column (5) of Table 1 shows that the average treatment size under these refinements is approximately $600,000. These treatments occurred in 779 census tracts and encompass nearly 3,000 underlying activities. I use this set of tracts as the analysis sample for estimating causal effects of the CDBG, running the intercept-shifted SCM separately for each tract.

[19]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.

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.[20] I narrow the list of covariates using a lasso model selection procedure to determine the subset of covariates that best predict treatment status.[21] 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.[22] 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 779 census tracts with at least 5 years of pre-treatment data and 7 years of post-treatment data.[23] 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 far greater density in terms of jobs per square mile. This suggests that treated tracts 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

[20]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.

[21]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.

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

[23]To implement this, I rely on the `augsynth` package by Eli Ben-Michael, Avi Feller, and Jesse Rothstein.

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 309 (40%) 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.[24] This amounts to averaging the individual SCM estimates, placing greater weight on estimates that are more precisely estimated. Additionally, I 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:

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

[24] 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).

The parameter of interest is α_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.[25] 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 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

[25]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.

the project had been completed.[26]

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.[27] 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 2017 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 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.

[26]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.

[27]Census blocks are the smallest geographic unit used by the Census. Each census tract is an amalgamation of underlying census blocks.

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 Regression (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 and increasing over time. The estimates are significant at the 10-percent level or better for most time periods. The effect becomes significant two years after the initial funding date at roughly 40 to 60 jobs created, a 4 to 5 percent increase off of a baseline median of 1,135 jobs for treated tracts in the analysis sample. The effect increases noticeably after year six and remains statistically significant thereafter. Roughly 140 jobs are created by year 10, representing a 13 percent total increase.

The effect on jobs held by workers living in LMI tracts is also positive and significant. At baseline, these jobs account for approximately half of all jobs in treated tracts (see Table 2). By comparison, the effect on total jobs appears completely driven by the increase in jobs for LMI workers. This suggests that CDBG investments disproportionately benefit workers commuting from LMI tracts. At the 10-year mark, the impact on LMI jobs represents a 20% increase relative to a baseline median of 530 jobs. The effect on workers specifically from low-income tracts is also positive and significant. Jobs increase by 14% at the ten-year mark (off a baseline of 227 jobs). The effect size is roughly half the total effect on jobs, whereas only one-quarter of jobs total jobs are held by these workers at baseline.

Low-wage jobs account for the majority of the total effect, especially during the first several years after treatment. By comparison, low-wage jobs account for only one-quarter of baseline jobs in treated tracts. In later years, the proportion of the total estimate represented by low-wage jobs decreases. This suggests that the CDBG primarily creates low-wage jobs in the short-term, while higher-paying jobs only begin to materialize in later years. While higher pay is an important objective of place-based policies, evidence suggests that joblessness has more adverse effects on life satisfaction than income inequality (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 benefits workers living within five miles of the treated tract. The effect size is more than half the total jobs effect, despite such workers representing only one-quarter of baseline jobs. The proportion of the effect accounted for by workers living nearby does appear to decrease in later years of the panel. This potentially coincides with the eventual arrival of higher-paying jobs, suggesting that the higher-paying jobs may be more likely to be held by non-local workers.

Finally, the CDBG generates positive and significant spillover effects on jobs in adjacent untreated tracts. The magnitude of the spillover is similar to that of the main effect. 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."[28] 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 A5 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% ($N = 402$), 20% ($N = 461$; the current threshold), and 30% ($N = 525$) of the baseline pre-treatment mean. The trajectory and significance of the effects remain similar across all three thresholds. 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.

[28] See 24 CFR 570.482.

4 Getting Beneath the Hood of Effective Place-Based Policies

In this section, I compare how treatments vary in their effectiveness based on what activities were funded and where they were implemented. Throughout this analysis, I focus on jobs held by workers living in LMI tracts 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, my estimates are unlikely to be causal in nature. Still, these findings provide one of the only direct sources of evidence on the heterogeneous impacts of place-based policies.

4.1 What kinds of activities are most effective?

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.[29] 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 funded.[30] 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, although many are noisy. Two categories consistently display positive and significant effects: commercial/industrial construction and financial assistance. The magnitudes of the trajectories are similar between the two, although financial assistance is more precisely estimated. The effects begin to materialize as early as two years after the project is funded.

The results suggest that the most productive place-based jobs policies tend to revolve around public services that directly benefit businesses—whether providing financial assistance or subsidizing commercial and industrial construction for businesses to eventually occupy. The CDBG explicitly forbids traditional zone-based subsidies and incentives for businesses to locate in underperforming areas, which tend to have a mixed record of success (see Neumark and Simpson (2015) for an overview). Instead, direct financial assistance can be used by new and existing businesses for a variety of productive purposes, including business space expenses (e.g. purchasing land or subsidizing building con-

[29]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.

[30]Table A3 shows the fraction of each activity type a that coincides with an activity type b within the same treatment.

struction, rehabilitation, purchases, and leases), capital purchases, inventory purchases, job training, and wage/fringe benefit increases. Compared to traditional zone-based incentives, assistance provided by the CDBG is more likely to create new jobs instead of reallocating jobs from other locations. 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 (Bartik, 2020*a*).

There are also positive, though less clear effects for clearance, micro-enterprise, and technical aid. The effect on clearance is marginally significant at the 10 percent level or better starting 6 years after the project is funded. The trajectory suggests that a recently-cleared site is not productive for years until new construction can occur in its place. For micro-enterprise, the impact on jobs becomes large and significant in years three and four before decreasing to non-significance. The magnitude of the effect at the peak is surprising given that micro-enterprise assistance is specifically intended for businesses with five or fewer employees. It is possible that these businesses tend to expand quickly after receiving CDBG aid while also have exhibiting lower survival rates. It is also worth noting that the estimates also tend to be noisier micro-enterprise investments. Finally, the point estimates on technical aid are quite large but are not precisely estimated. Technical aid generally involves consulting, workshops, and training for making business operations more effective. The target audience for technical aid typically involves fledgling businesses or distressed businesses on the brink of failure. Both types of businesses tend to be associated with volatile outcomes, potentially explaining the noisy estimates.

The effects on infrastructure and exterior improvements are largely insignificant. 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:

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

where $(\text{Activity Category } c)_j$ is an indicator for whether the treatment for tract j included an activity in category c. γ 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 α_k from Equation (7) represent the absolute effects for each activity category, the estimates for γ from Equation (8) capture each category's relative effectiveness. These estimates are plotted in A4 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 types with consistently positive estimates are commercial/industrial construction, financial assistance, and technical assistance.

Taken together, these findings suggest that the first dollar is typically best spent on financial assistance for businesses and subsidizing commercial/industrial construction. Technical assistance appears promising but remains inconclusive due to noisy estimates. Micro-enterprise produces short-term job growth, while clearance only generates job growth many years later. Infrastructure generates the smallest impact at the greatest cost.

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 activities 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.[31] I construct each index 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.[32] 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:

$$\hat{\tau}^{scm}_{jkz} = \rho_1 \text{Index}^a_j + \rho_2 \text{Index}^a_z + \delta_{T_j} + \epsilon_{jkz} \tag{9}$$

where the outcome $\hat{\tau}^{scm}_{jkz}$ again represents the causal effect of each treatment j on jobs held by workers from LMI tracts. z now indexes commuting zones. ρ_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. ρ_1 captures how CDBG investments vary in their effectiveness between two tracts in similar commuting zones but with different tract-level measures of a. ρ_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 six different place-based characteristics: employment-to-population ratio, initial jobs per square mile, and the four indices. Confidence intervals are plotted at the 90 percent level. Table 5 provides tabular estimates. The tract-specific estimates suggest that place-based investments tend to produce larger effects in tracts scoring higher on the socioe-

[31] These variables include: % in poverty, % HS grad or less, % single mother, % vacant housing. All corresponding changes between 1990-2000 are also multiplied by -1.

[32] 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.

conomic, demographic, and neighborhood indices. Tracts with high neighborhood scores represent places that may be more conducive to commercial and industrial development. Tracts with high socioeconomic and demographic scores may have resident populations with greater workforce attachment and higher levels of education and human capital. Given that the jobs impact of the CDBG is largely driven by nearby workers, local workforce composition likely plays a non-trivial role in determining 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 disadvantaged tracts did not benefit. Workers residing in high-poverty tracts were among the primary beneficiaries of the CDBG.

The role of commuting zone attributes is less clear. In fact, it appears that conditional on tract-specific characteristics, commuting zone characteristics are generally not predictive of whether a place-based policy will generate impacts. This suggests that the effects of place-based policies are tied more to neighborhood characteristics rather than characteristics of labor markets as a whole. In contrast to predictions by Bartik (2015) and Austin, Glaeser and Summers (2018), I find that the employment-to-population ratio within a local labor market does not appear to correlate 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.

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-NHGIS) (Manson et al., 2020).[33] 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.[34] 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

[33] 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).

[34] 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.

Zones program, which also targeted similarly low-income census tracts, did not lead to a meaningful increase in housing costs.

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:

$$
\begin{aligned}
CDBG_{it} = s_t \times A_t \times \max(&\underbrace{0.25 \times \frac{Population_{it}}{\sum_i Population_{it}} + 0.5 \times \frac{Poverty_{it}}{\sum_i Poverty_{it}} + 0.25 \times \frac{Overcrowd_{it}}{\sum_i Overcrowd_{it}}}_{\text{Formula A}}, \\
&\underbrace{0.2 \times \frac{GrowthLag_{it}}{\sum_i GrowthLag_{it}} + 0.3 \times \frac{Poverty_{it}}{\sum_i Poverty_{it}} + 0.5 \times \frac{Pre1940Housing_{it}}{\sum_i Pre1940Housing}}_{\text{Formula B}})
\end{aligned}
\tag{10}
$$

$Population_{it}$ 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. $GrowthLag_{it}$ represents the difference between the current population and what the population would have been if population growth had followed the national trajectory since 1960. $Pre1940Housing$ represents the number of housing units built prior to 1940. Prior to 2012, formula inputs were calculated using estimates from decennial censuses. Poverty, overcrowded housing, and pre-1940 housing were updated every ten years. Population and growth lag were updated annually via census estimates.[35] 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.[36]

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

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

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

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

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 A6.

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 for-

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

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

mula's redistributive shortcomings. In particular, Formula A communities tend to be underfunded relative to actual need, whereas many high-income Formula B communities with older housing stock and slow growth tend to be overfunded.[37]

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:

$$\frac{Spend_{it}}{Population_{i0}} = \alpha + \rho \frac{CDBG_{it}}{Population_{i0}} + C_{it}'\beta + \theta_i + \lambda_t + \epsilon_{it} \tag{13}$$

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

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

decompose $CDBG_{it}$ into an exogenous and endogenous component.

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

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

$$\widetilde{CDBG}_{it} = \max(\mathbf{X}^A_{i0}\mathbf{v}^A_t, \mathbf{X}^B_{i0}\mathbf{v}^B_t) \tag{14}$$

This instrument represents how much of grantee i's CDBG allocation can be attributed to movements in the exogenous prices v^k_t, where each pre-period input X^k_{i0} 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 $\mathbf{X}^A_{i0}$ and $\mathbf{X}^B_{i0}$ are taken as given; the corresponding weights $\mathbf{v}^A_t$ and $\mathbf{v}^B_t$ 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.[38] 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^k_t represents a national change in policy intensity and X^k_{i0} represents each unit's exposure to policy changes. The validity of differences-in-differences relies on the assumption that X^k_{i0} is not endogenous to subsequent changes in the outcome variable that are unrelated to policy changes in v^k_t.

[38]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.

As an instructive example, consider a grantee whose instrumented CDBG allocation is entirely driven by high baseline pre-1940 housing. Figure A6 shows that the dollar value of pre-1940 housing has monotonically increased over time. The identifying assumption would be violated if similar grantees with disproportionate exposure to pre-1940 housing all experienced rising public spending (relative to grantees with low levels of pre-1940 housing, and conditional on controls C_{it} and year fixed effects) for reasons unrelated to fluctuations in prices v_t^k.

While the exclusion restriction is ultimately not verifiable, several aspects of the instrument lend support to its validity. First, each grantee's CDBG allocation is determined by a combination of six inputs with varying trajectories for prices v_t^k, limiting the ability of any one endogenous factor to drive the estimate. Second, the non-linear max function creates a discontinuous change in the set of prices used (i.e. which "policy" is applied). For grantees near the boundary of the max function, a coin flip determines whether the instrument uses Formula A or Formula B prices. The instrument can even use both sets of prices for the same grantee in different years. Third, the analysis period begins in 2011 but the X_{i0}^k are pinned to baseline levels in 2000. If contemporaneous inputs X_t^k are endogenous to local public spending, then serial correlation will expose lagged values of the input to this endogeneity. Using a ten-year lag reduces exposure to this potential source of bias. Finally, with unit fixed effects in Equation (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.[39] Samples are taken in non-census years, but most large governmental units are included with certainty in the annual sample.[40] 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

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

[40] 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.

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 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).[41] 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 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.[42] 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 $575,000 from column (4) in Table 1. This treatment 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 $1.1M.[43]. The public spending multiplier from the IV estimate in Table 7 suggests that the actual amount of public spending that occurred for the typical treatment was approximately $3.5M. At 142 jobs created per treatment (see

[41] 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.

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

[43] 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 $575,000 cost per treatment by the 1.46 investment events to yield $395,000 per investment event. I then multiply this by 2.79 expected investment events over the course of 10 years, which yields $1.1M.

Table 3, the typical public spending per job created was approximately \$25,000. This estimate places the CDBG among the more cost-effective programs that have been studied, 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, 2020*b*). These findings suggest that decentralizing place-based policies through flexible block grants appears to be a promising and cost-effective approach to stimulating 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 roughly 3,000 of the largest economic development projects funded by the CDBG in nearly 800 census tracts nationwide. 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 13% ten years after the initial funding date. The increase was almost entirely driven by workers living in low- and moderate-income neighborhoods, as well as workers living within 5 miles of the investment. Investments also generated similar rates of job growth in adjacent neighborhoods, while having minimal impact on housing values and rents. Effective investments typically involved direct financial assistance to businesses as well as subsidies for commercial and industrial construction. While the CDBG can only be used in low-income areas, job impacts were typically greatest in less disadvantaged tracts. Conditioning on tract-level characteristics, features of the broader local labor market do not appear predictive of job impacts. 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 paper provides crucial first steps for understanding how federal policies can be used to effectively create jobs in places facing diverse forms of economic decline.

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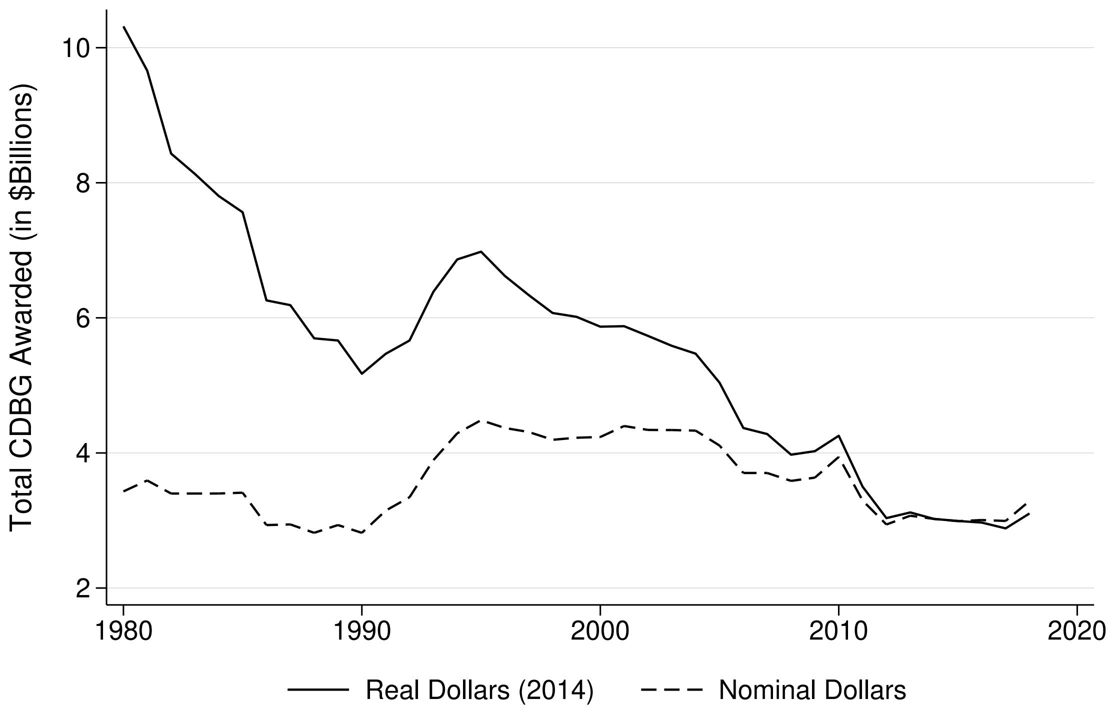
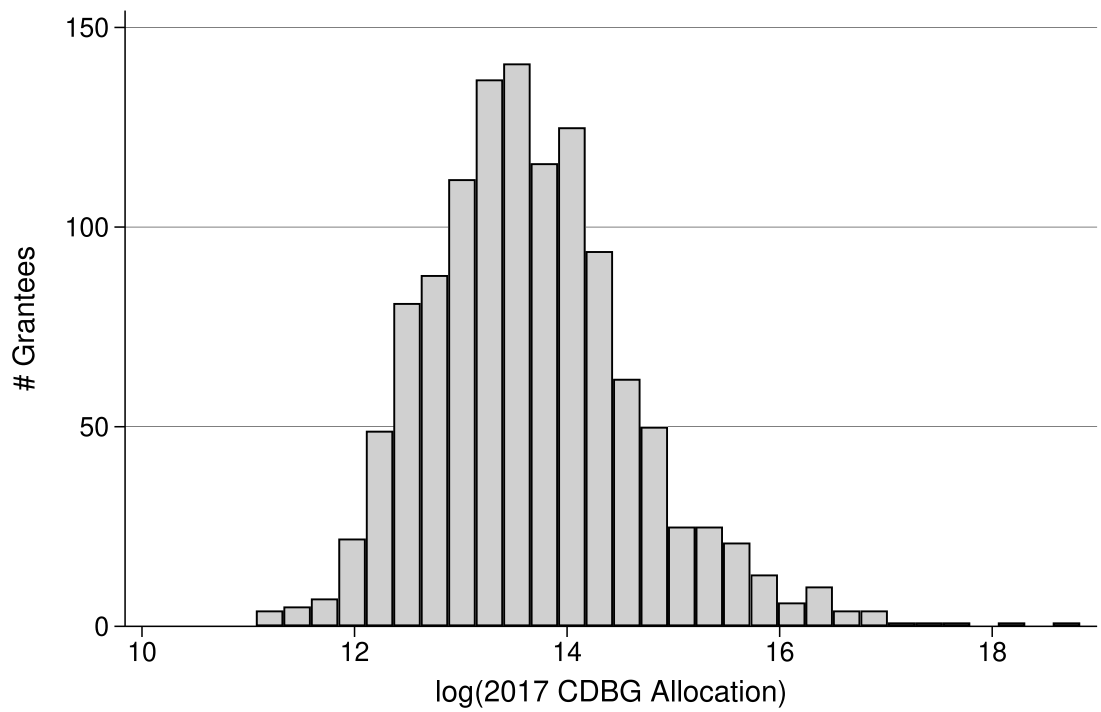
Figure 1: Historical CDBG Appropriations, Real vs. Nominal



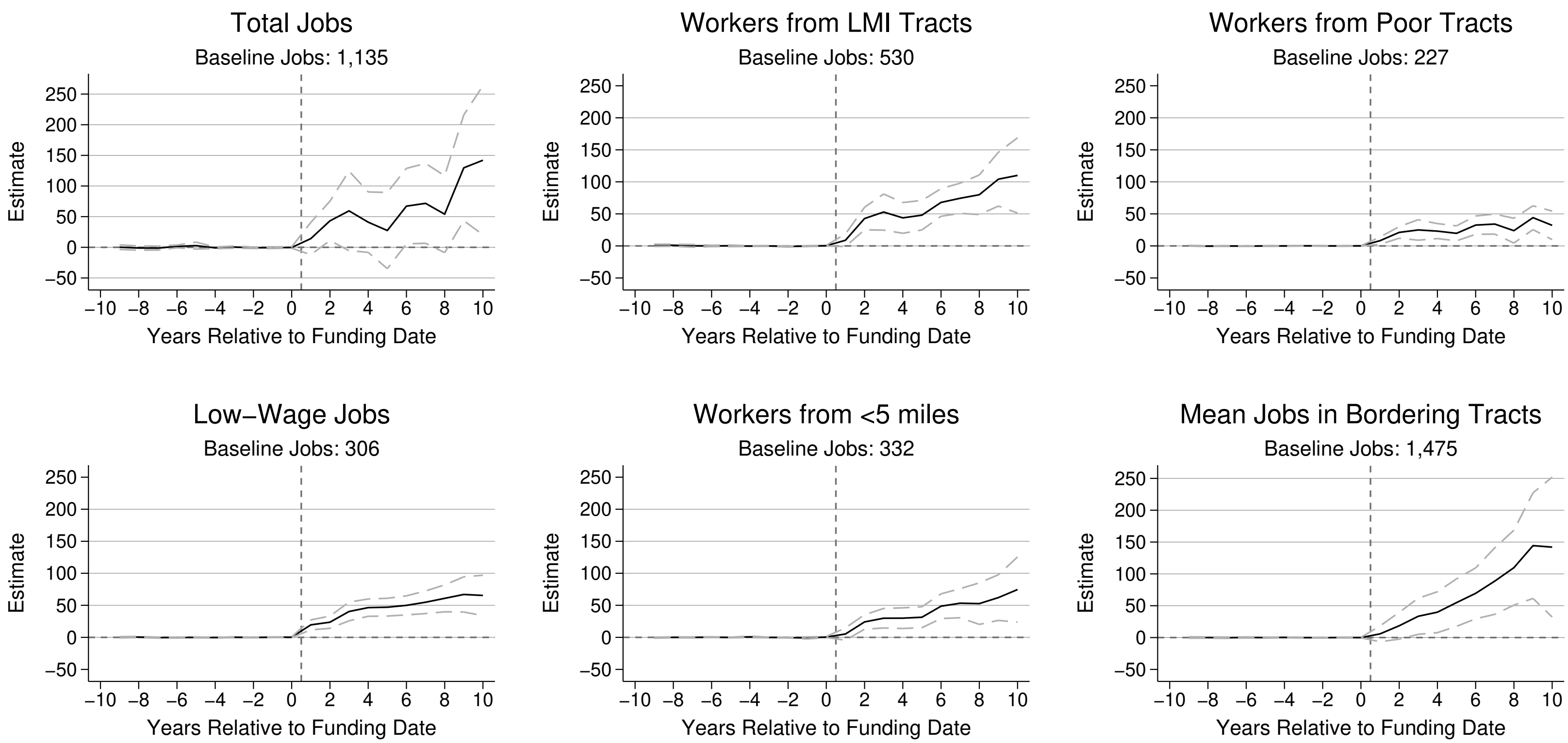
Note: This figure shows how the total CDBG budget appropriated by Congress has changed over time. Data come from historical records which were obtained from the U.S. Department of Housing and Urban Development.

Figure 2: Distribution of Log CDBG allocation (2017)



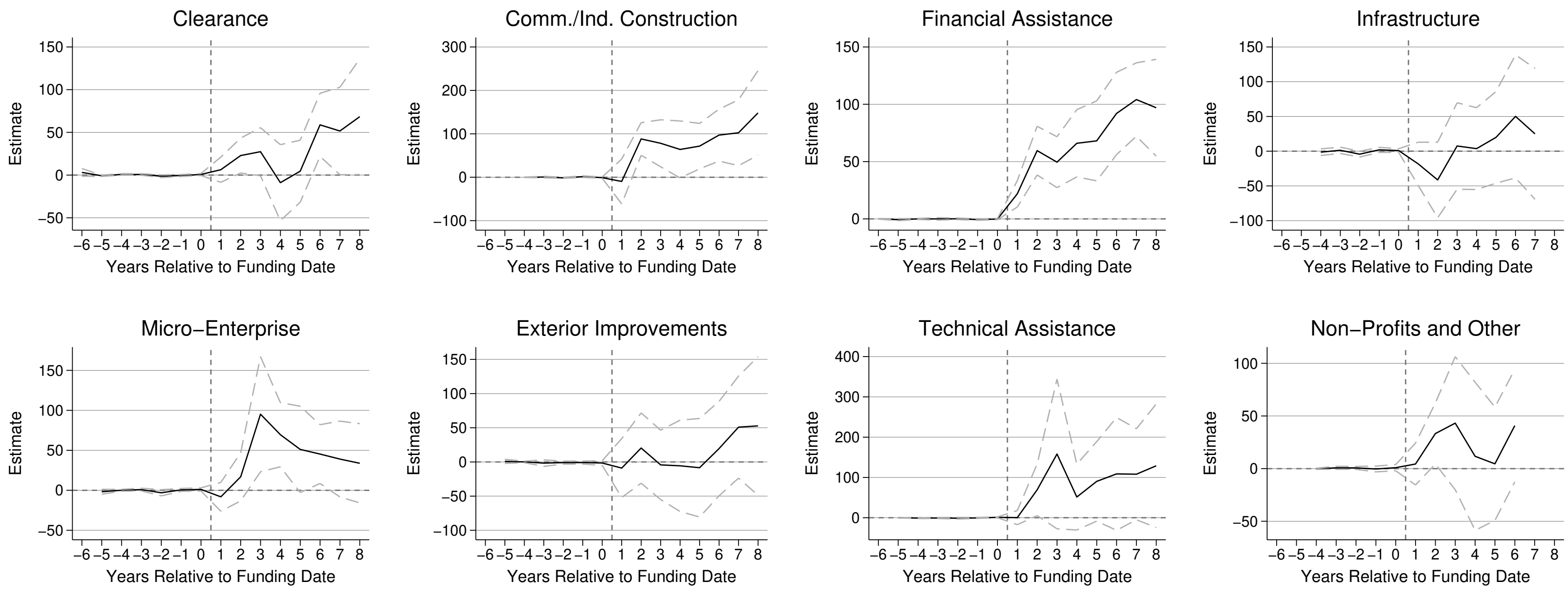
Note: This figure shows how the distribution of CDBG grantee allocations as of 2017 in nominal dollars. Dollar amounts are presented in logs. Data come from CDBG allocation spreadsheets provided by the U.S. Department of Housing and Urban Development.

Figure 3: Effects of CDBG Investments on Jobs



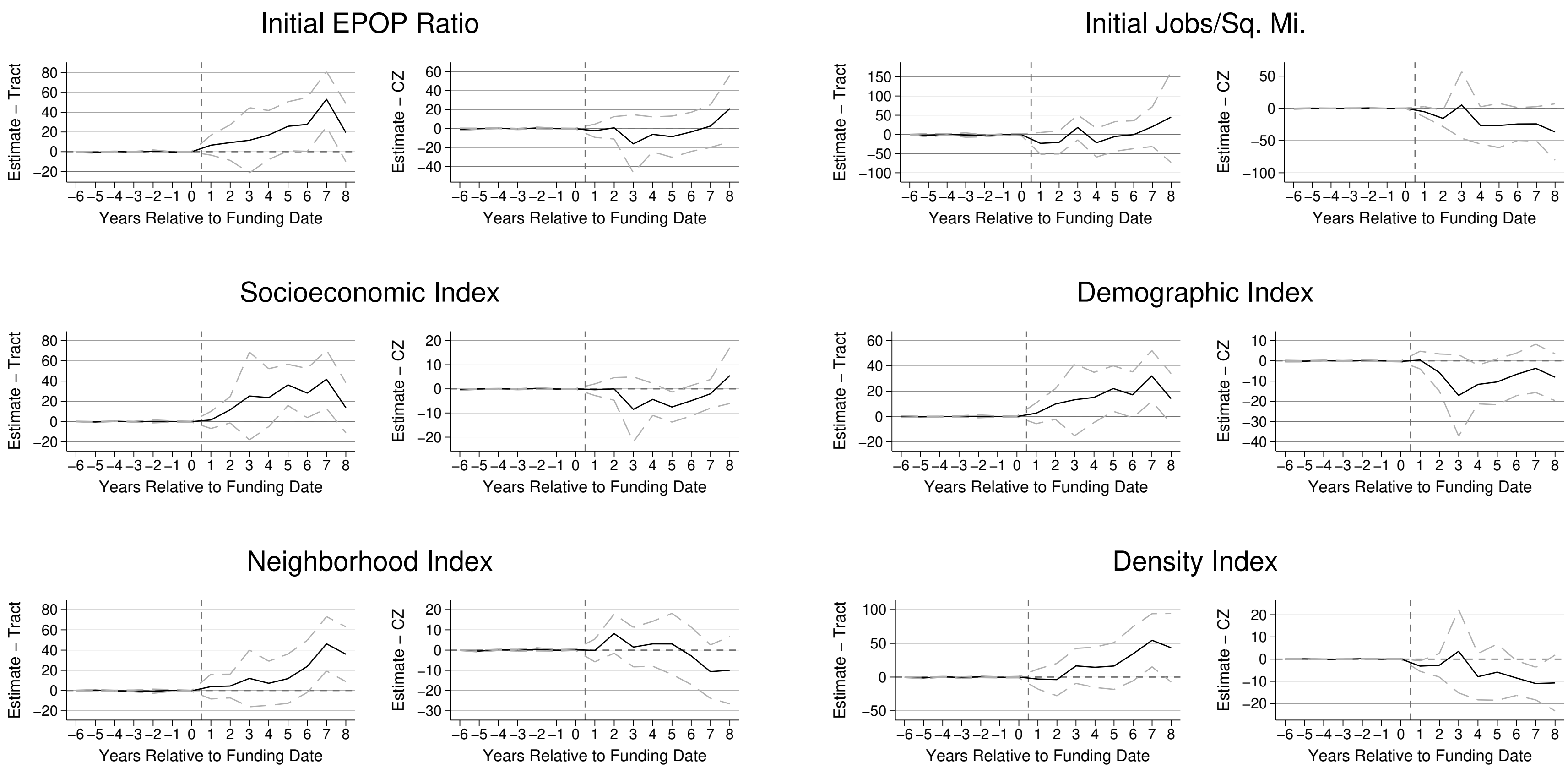
Note: This figure presents estimates of place-based investments funded by the CDBG across a variety of job outcomes. Each estimate is a pooled average of underlying synthetic control estimates. Synthetic control estimates were calculated for each of 779 treated census tracts; tracts with poor pre-treatment 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. LMI and poor tracts represent tracts with poverty rates greater than the 50th and 75th percentile for tracts within the same commuting zone. Low-wage jobs are jobs earning less than $1,250 per month. Baseline medians: jobs (1,135), workers from LMI tracts (530), workers from low-income tracts (227), low-wage jobs (306), workers from nearby tracts (332), mean jobs in adjacent tracts (1,475). Corresponding estimates are presented in tabular form in Table 3.

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



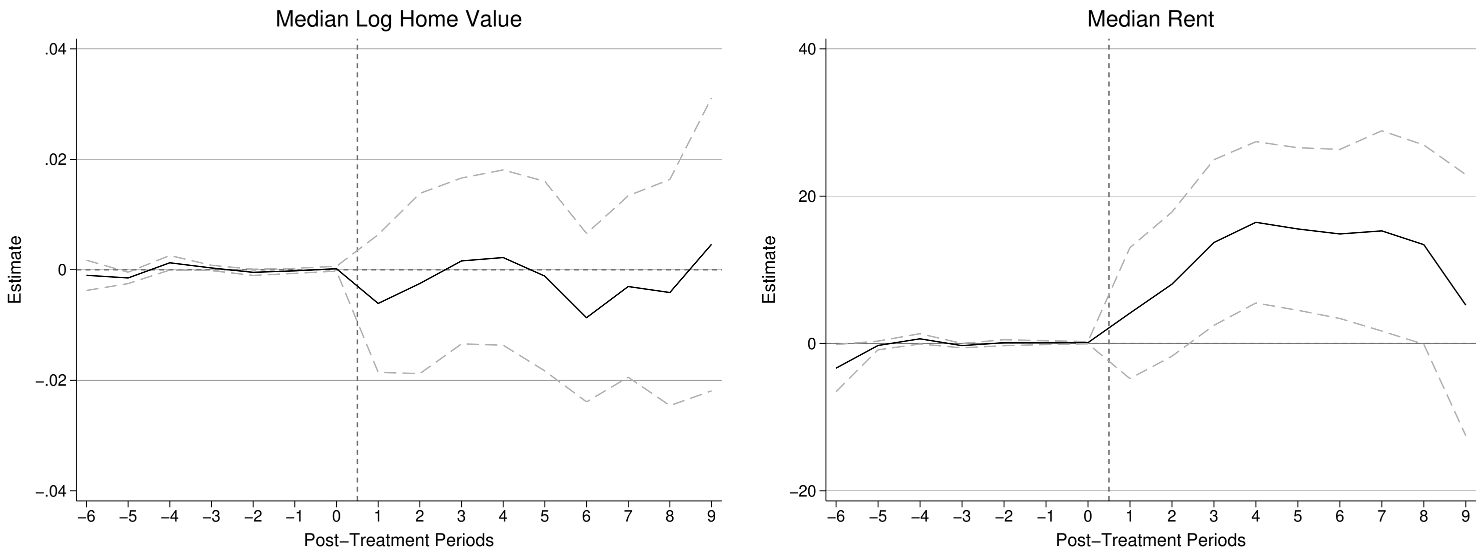
Note: This figure presents the association between jobs generated for LMI residents (baseline median: 530 jobs) and eight different activity categories funded by the CDBG. Estimates are obtained from Equation (7), estimated separately for the above eight activity categories. See Section 2.1 for a description 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 attribute of places. Each plot presents the association between the effectiveness of CDBG investments and the place-based attribute measured either at the tract level or the commuting zone level. Each attribute has been standardized to mean zero and standard deviation one. Estimates represent the association between jobs generated for LMI residents (baseline median: 530 jobs) and a one standard deviation increase in the corresponding attribute. 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

	A. CDBG Economic Development Activity (2000-2016)			B. CDBG "Treatments" (3-Year Total)		
Project Type	(1) Avg. Activity ($)	(2) # Activities	(3) # Tracts	(4) Avg. Treatment ($)	(5) Avg. # Activities	(6) # Tracts
Clearance	50,000	13,387	2,792	102,437	1.16	168
Comm./Ind. Construction	205,850	745	452	32,151	0.09	54
Exterior Improvements	51,626	4,680	1,385	31,230	0.26	84
Financial Assistance	118,257	10,462	3,658	244,349	0.97	386
Infrastructure	379,879	467	316	58,797	0.10	66
Micro-Enterprise	47,958	5,827	1,980	45,962	0.57	132
Technical Assistance	105,178	2,233	714	20,795	0.18	57
Non-Profits and Other	82,729	5,142	1,245	39,441	0.32	104
All Economic Development	79,609	42,943	8,812	575,163	3.66	779

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 25 percent of all similarly defined treatments. Treated tracts are also restricted to tracts where the treatment date allows for sufficiently many pre- and post-treatment observations. Column (4) describes the average treatment size for tracts in this sample. Column (5) provides the total number of projects represented by treatments defined in this way. Columm (6) shows the number of treated tracts with at least one project in each category.

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

Census Tract Characteristics	Treated (1)	Untreated (2)	Difference (T-U) (3)	Difference (T-SCM) (4)
Socioeconomic Index	-0.49	0.02	-0.50	-0.07
EPOP Ratio	0.42	0.46	-0.05	-0.02
HH Income	43,923	64,333	-20,410	-2,248
% in Poverty	0.22	0.12	0.09	0.03
% Professional Occupation	0.27	0.33	-0.06	0.00
Demographic Index	-0.55	0.02	-0.56	-0.06
% White	0.57	0.69	-0.12	-0.03
% Married	0.43	0.54	-0.11	-0.03
% College Educated	0.17	0.24	-0.07	0.00
% HS Grad or Less	0.58	0.48	0.10	0.02
% Single Mother	0.21	0.13	0.08	0.02
Neighborhood Index	-0.39	0.01	-0.40	0.01
Median Rent	603	787	-184	-16
Median Home Value	135,366	190,738	-55,372	3,568
% Vacant Housing	0.10	0.08	0.02	0.00
Density Index	0.14	-0.01	0.15	0.13
Jobs per Sq. Mile	5,168	2,076	3,092	2,388
Population per Sq. Mile	5,210	5,442	-233	-75
% Working Age (18-59)	0.58	0.58	0.00	0.00
Jobs	4,343	1,617	2,726	913
From Moderate-Poor Tracts	2,092	710	1,383	395
From Poor Tracts	1,052	321	731	221
Low-Wage Jobs	1,193	502	691	177
From Tracts within 5 Miles	1,334	498	836	205
Mean Jobs in Bordering Tracts	2,616	1,617	3,266	-72
N (Number of Tracts)	779	61,868	62,647	779

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
Jobs										
Estimate	14.3	43.1***	59.5*	41.1	27.4	67.2**	71.8**	54.1*	129.7***	142.1**
	(13.3)	(16.2)	(32.8)	(24.9)	(31.4)	(31.1)	(32.9)	(31.6)	(43.4)	(61.0)
N	461	461	461	461	461	414	349	277	219	159
Workers from LMI Tracts										
Estimate	8.8*	42.8***	52.9***	43.8***	48.1***	68.0***	74.4***	79.9***	104.2***	110.1***
	(5.0)	(8.9)	(14.2)	(12.1)	(11.6)	(10.9)	(11.9)	(15.6)	(21.1)	(29.5)
N	453	453	453	453	453	409	344	275	222	157
Workers from Poor Tracts										
Estimate	8.2***	21.1***	24.9***	23.2***	19.7***	32.5***	34.1***	23.9**	44.1***	32.2***
	(2.8)	(4.6)	(8.1)	(5.9)	(5.9)	(7.3)	(7.9)	(9.7)	(9.4)	(11.1)
N	430	430	430	430	430	383	321	255	205	150
Low-Wage Jobs										
Estimate	19.5***	23.7***	40.4***	46.5***	47.1***	50.0***	54.8***	60.9***	67.0***	65.5***
	(3.9)	(4.8)	(7.4)	(6.8)	(7.0)	(7.5)	(8.9)	(10.6)	(13.8)	(15.8)
N	444	444	444	444	444	403	341	281	226	158
Workers from Nearby Tracts										
Estimate	5.3	24.1***	29.9***	30.0***	31.4***	48.7***	53.3***	52.7***	62.1***	74.7***
	(4.9)	(5.7)	(7.7)	(8.2)	(8.2)	(9.7)	(11.4)	(16.3)	(17.9)	(25.5)
N	435	435	435	435	435	392	332	264	211	146
Jobs in Adjacent Tracts										
Estimate	5.8	18.5*	33.5**	39.9**	54.9***	69.4***	88.7***	109.8***	144.4***	142.0**
	(6.3)	(10.6)	(14.4)	(16.3)	(18.9)	(20.2)	(26.4)	(29.8)	(41.9)	(55.3)
N	633	633	633	633	633	566	473	365	285	199

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. Baseline medians: jobs (1,135), workers from LMI tracts (530), workers from low-income tracts (227), low-wage jobs (306), workers from nearby tracts (332), mean jobs in adjacent tracts (1,475). 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
Clearance								
Estimate	6.3	22.9*	27.4	-8.9	4.6	58.8**	51.6*	68.4*
	(8.8)	(12.2)	(16.8)	(26.5)	(21.6)	(22.0)	(30.4)	(40.0)
N	88	88	88	88	88	77	63	47
Comm./Ind. Construction								
Estimate	-9.6	88.2***	78.2**	64.0	71.7**	97.1**	102.5**	148.2**
	(30.1)	(21.9)	(31.6)	(38.3)	(30.5)	(34.8)	(43.8)	(56.0)
N	34	34	34	34	34	31	27	25
Financial Aid								
Estimate	21.9***	59.6***	49.5***	66.0***	68.1***	92.0***	104.2***	97.0***
	(6.9)	(12.7)	(13.3)	(17.6)	(20.9)	(21.5)	(19.2)	(25.3)
N	219	219	219	219	219	198	171	142
Infrastructure								
Estimate	-18.2	-41.4	7.5	3.7	20.0	50.1	25.0	28.6
	(18.1)	(31.7)	(36.3)	(34.4)	(38.5)	(51.6)	(54.3)	(73.7)
N	30	30	30	30	30	28	23	18
Micro-Enterprise								
Estimate	-8.1	16.9	95.2**	69.6***	51.2	45.3**	39.1	33.9
	(10.6)	(17.8)	(42.6)	(23.7)	(31.9)	(21.6)	(27.8)	(28.8)
N	72	72	72	72	72	64	52	40
Exterior Improvements								
Estimate	-8.9	20.3	-4.3	-5.7	-8.5	19.5	50.9	52.8
	(25.3)	(30.2)	(29.8)	(39.3)	(42.4)	(40.4)	(43.3)	(57.5)
N	52	52	52	52	52	46	32	23
Technical Aid								
Estimate	0.3	69.3*	158.3	51.4	90.2	108.8	108.0	129.0
	(10.2)	(37.4)	(108.6)	(47.8)	(57.6)	(81.7)	(65.8)	(88.9)
N	67	67	67	67	67	57	53	46
Non-Profits and Other								
Estimate	4.4	33.3*	43.2	11.8	4.6	40.8	65.4	93.5
	(11.4)	(16.7)	(36.4)	(40.5)	(31.0)	(30.3)	(52.9)	(134.0)
N	28	28	28	28	28	23	19	12

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 (baseline median: 530 jobs). 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
EPOP Ratio								
Estimate - Tract	6.6	9.3	11.6	17.0	25.8*	27.7*	53.1***	19.5
	(6.0)	(10.9)	(19.9)	(15.0)	(15.0)	(16.5)	(16.9)	(17.7)
Estimate - CZ	-2.3	0.8	-16.1	-6.1	-8.6	-3.5	2.6	21.0
	(4.2)	(7.1)	(18.5)	(11.1)	(13.1)	(12.5)	(13.6)	(21.1)
N	453	453	453	453	453	409	344	275
Jobs per Square Mile								
Estimate - Tract	-23.2	-20.6	18.1	-21.5	-5.2	-0.5	20.4	45.1
	(17.0)	(18.2)	(19.6)	(22.6)	(23.3)	(21.9)	(31.3)	(71.3)
Estimate - CZ	-5.3	-15.6**	5.3	-26.4	-26.6	-24.3	-24.1	-36.6
	(4.8)	(7.7)	(31.2)	(17.4)	(20.8)	(15.4)	(16.1)	(26.3)
N	453	453	453	453	453	409	344	275
Socioeconomic Index								
Estimate - Tract	1.6	11.7	25.3	23.7	36.2***	28.2*	41.8**	13.7
	(5.1)	(7.8)	(26.2)	(17.3)	(12.3)	(14.6)	(17.4)	(15.0)
Estimate - CZ	-0.3	-0.0	-8.5	-4.3	-7.5**	-4.9	-2.0	5.5
	(1.5)	(2.8)	(8.1)	(4.0)	(3.8)	(3.8)	(3.6)	(7.0)
N	452	452	452	452	452	408	343	274
Demographic Index								
Estimate - Tract	2.6	9.9	13.3	15.1	22.1**	17.2	32.1***	14.1
	(5.0)	(7.3)	(17.1)	(12.0)	(10.9)	(10.9)	(12.1)	(11.9)
Estimate - CZ	0.4	-5.7	-17.1	-11.6**	-10.4	-6.6	-3.7	-8.1
	(2.6)	(5.5)	(12.1)	(5.8)	(6.8)	(6.3)	(7.2)	(7.0)
N	452	452	452	452	452	408	343	274
Neighborhood Index								
Estimate - Tract	3.9	4.5	12.1	7.2	11.8	23.9	46.2***	36.0**
	(7.3)	(7.1)	(16.9)	(13.2)	(14.7)	(15.6)	(16.2)	(16.3)
Estimate - CZ	-0.2	8.2	1.5	3.1	3.1	-2.8	-10.7	-9.9
	(3.4)	(5.8)	(5.9)	(6.7)	(9.1)	(8.6)	(8.0)	(10.0)
N	452	452	452	452	452	408	343	274
Density Index								
Estimate - Tract	-3.0	-3.7	16.6	14.5	16.4	34.5	54.6**	43.5
	(9.0)	(14.4)	(15.7)	(18.0)	(21.0)	(24.2)	(23.7)	(30.7)
Estimate - CZ	-3.1**	-2.7	3.6	-7.9	-5.9	-8.5*	-11.0**	-10.7
	(1.4)	(3.2)	(11.3)	(6.3)	(7.6)	(4.7)	(4.4)	(7.6)
N	453	453	453	453	453	409	344	275

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 (baseline median: 530 jobs). 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
Log Median Home Value									
Estimate	-0.006	-0.002	0.002	0.002	-0.001	-0.009	-0.003	-0.004	0.005
	(0.008)	(0.010)	(0.009)	(0.010)	(0.010)	(0.009)	(0.010)	(0.012)	(0.016)
N	462	462	462	462	462	462	418	370	297
Median Rent									
Estimate	4.1	8.0	13.7**	16.4**	15.5**	14.9**	15.3*	13.4	5.2
	(5.4)	(5.9)	(6.8)	(6.6)	(6.7)	(6.9)	(8.2)	(8.2)	(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. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

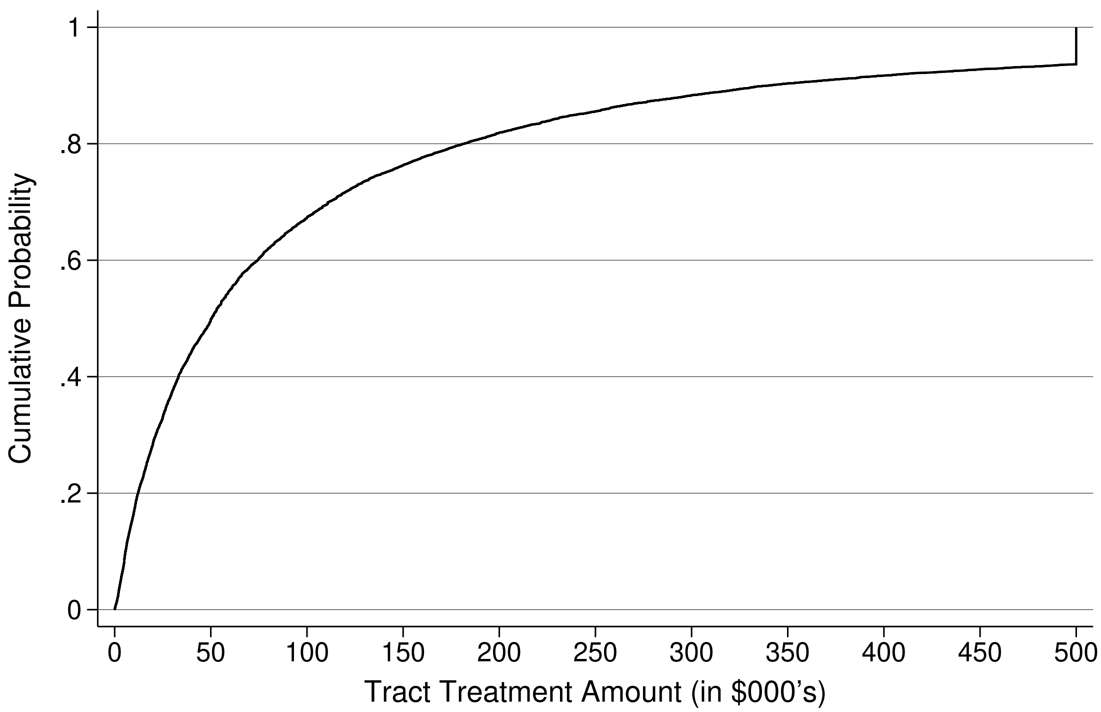
Table 7: Estimates of CDBG Public Spending Multipliers

	OLS	Reduced Form	First Stage	Simulated IV
Outcome (Per Capita):	HCD Spend	HCD Spend	CDBG Allocation	HCD Spend
Per-capita CDBG	2.00**			
	[0.37-3.64]			
Per-capita Simulated CDBG		1.86**	0.59***	3.16**
		[0.29-3.44]	[0.50-0.68]	[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. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

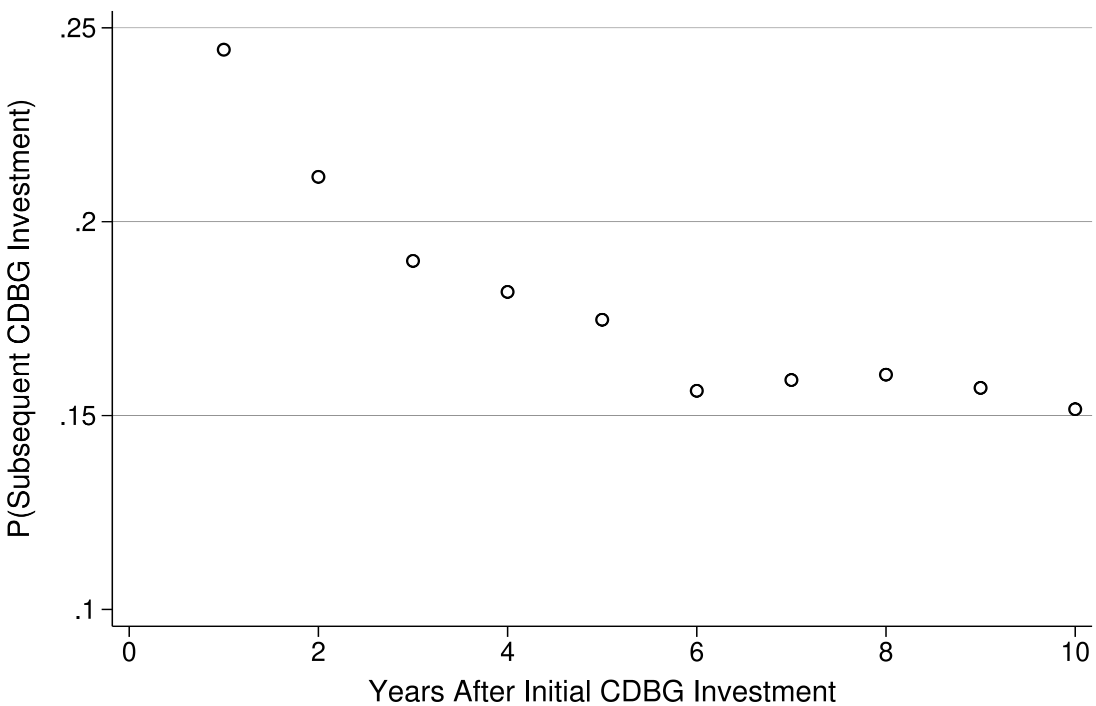
Appendix A: Appendix Tables and Figures

Figure A1: Distribution of Tract-Level Single-Year Investments



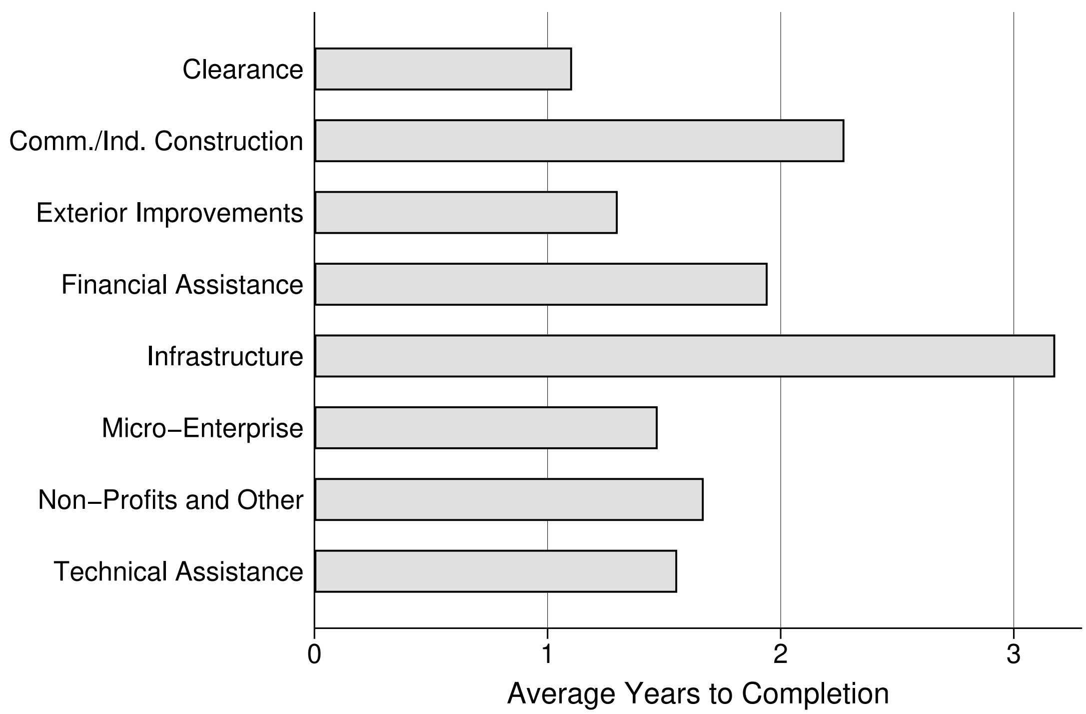
Note: This figure provides a cumulative density plot of single-year tract-level CDBG investments. Investments greater than $500,000 have been binned together.

Figure A2: Temporal Correlation of CDBG Investments



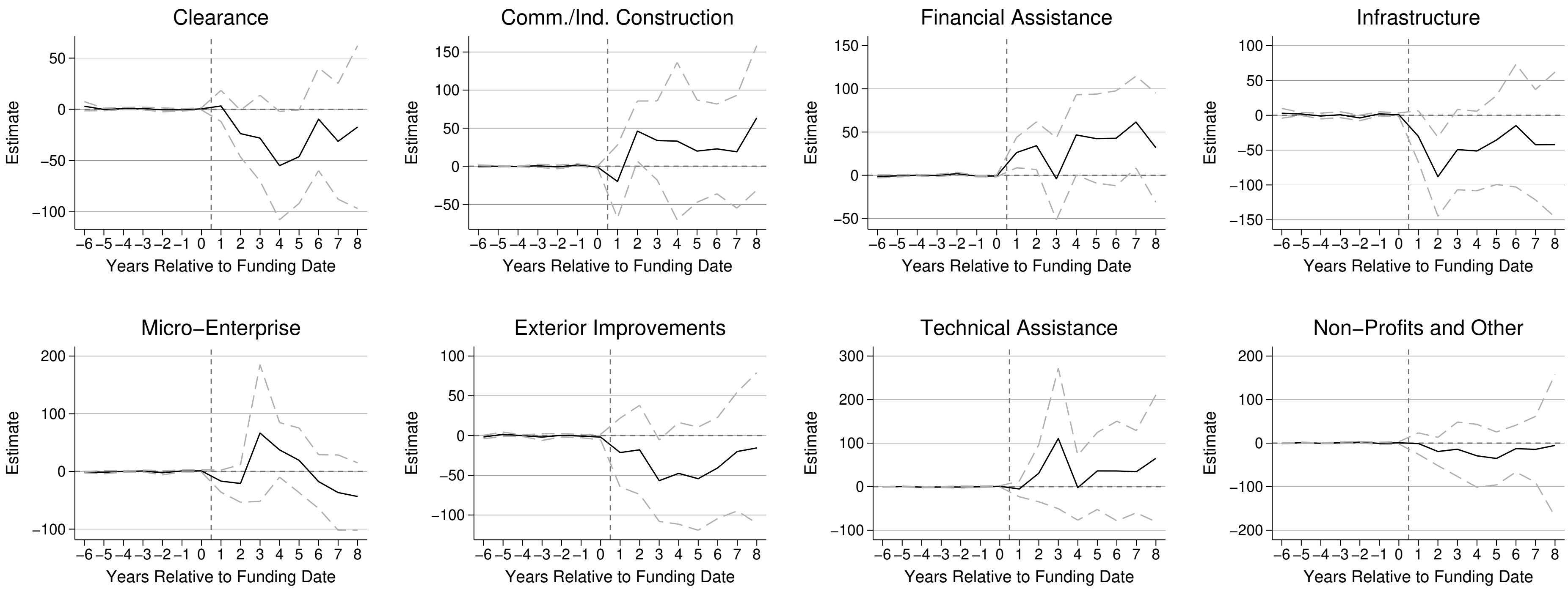
Note: This figure shows the proportion of treated tracts that receive a subsequent investment t years after initial investment.

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



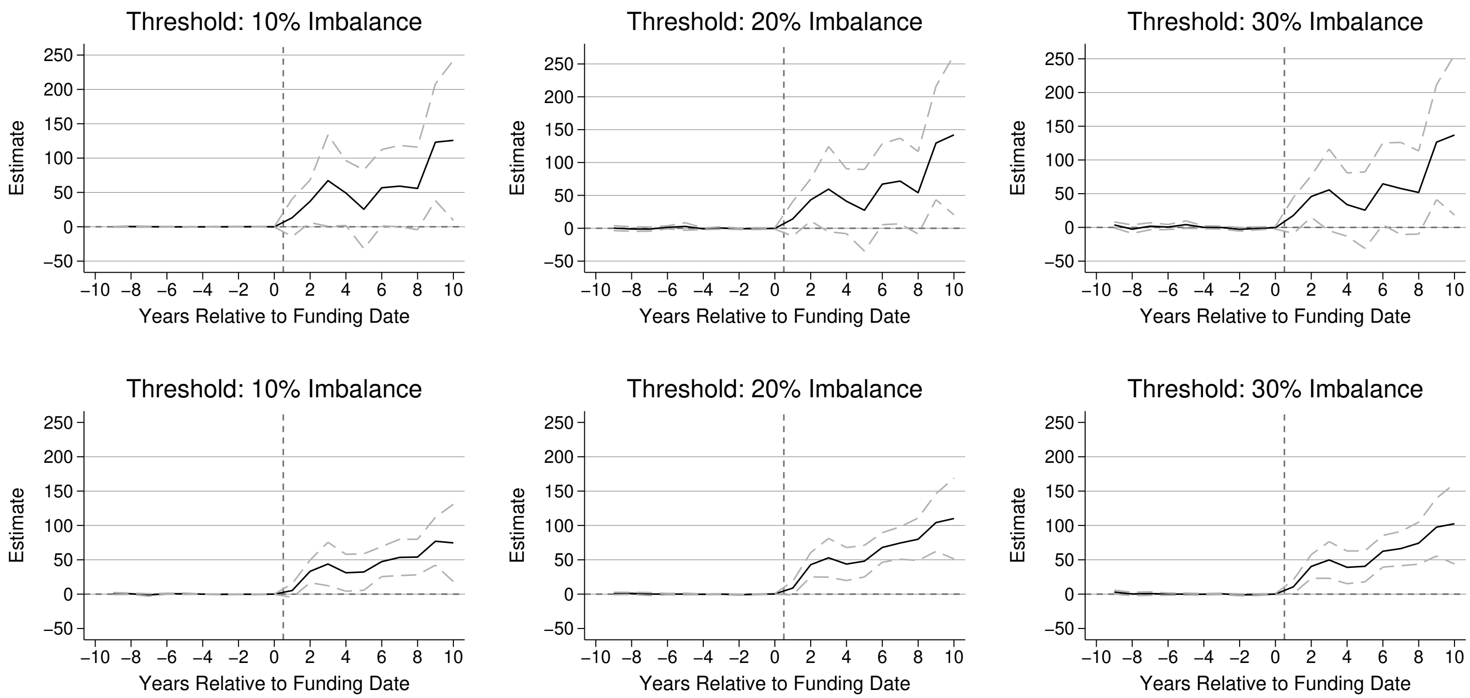
Note: This figure plots the average duration of time elapsed between the date when a project was funded by the CDBG and when the project was ultimately marked as completed within the CDBG's internal records.

Figure A4: Sensitivity to Pre-Treatment Balance Threshold



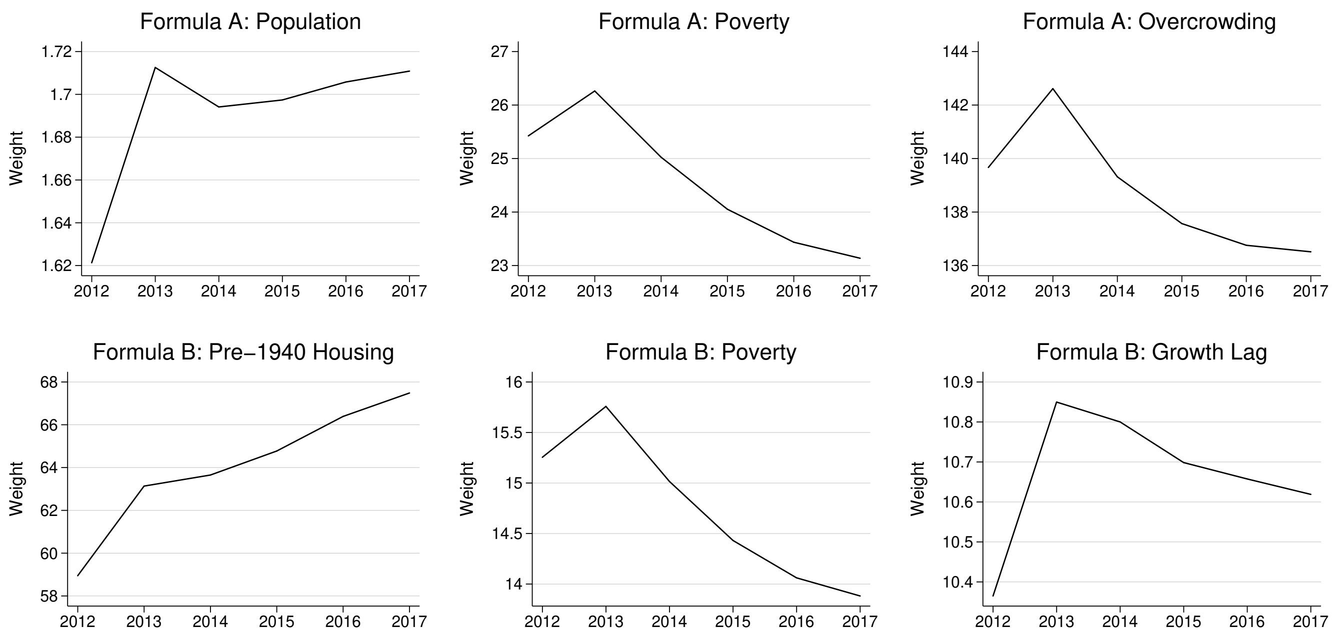
Note: This figure plots the correlation between each activity category and individual estimates of the CDBG on jobs held by workers from LMI tracts. Estimates are obtained via Regression (8). 90 percent confidence intervals are plotted using standard errors adjusted for clustering at the commuting zone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

Figure A5: Comparative CDBG type



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 A6: 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

Census Tract Characteristics	Treated (1)	Treated - Good Fit (3)	Treated - Poor Fit (4)
Socioeconomic Index	-0.49	-0.50	-0.47
EPOP Ratio	0.42	0.42	0.42
HH Income	43,923	45,434	41,730
% in Poverty	0.22	0.21	0.22
% Professional Occupation	0.27	0.25	0.28
Demographic Index	-0.55	-0.59	-0.49
% White	0.57	0.52	0.65
% Married	0.43	0.44	0.42
% College Educated	0.17	0.17	0.19
% HS Grad or Less	0.58	0.59	0.57
% Single Mother	0.21	0.22	0.20
Neighborhood Index	-0.39	-0.36	-0.43
Median Rent	603	622	575
Median Home Value	135,366	136,255	134,074
% Vacant Housing	0.10	0.10	0.11
Density Index	0.14	0.02	0.32
Jobs per Sq. Mile	5,168	2,298	9,329
Population per Sq. Mile	5,210	6,305	3,622
% Working Age (18-59)	0.58	0.57	0.60
Jobs	4,343	1,697	8,178
From Moderate-Poor Tracts	2,092	866	3,870
From Poor Tracts	1,052	414	1,978
Low-Wage Jobs	1,193	540	2,139
From Tracts within 5 Miles	1,334	619	2,370
Mean Jobs in Bordering Tracts	2,616	2,168	3,266
N (Number of Tracts)	779	461	318

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
Clearance								
Estimate	3.3	-23.7*	-28.0	-54.9*	-46.2*	-9.6	-31.2	-17.3
	(9.2)	(13.8)	(25.3)	(31.9)	(27.5)	(30.3)	(34.1)	(47.9)
N	453	453	453	453	453	409	344	275
Comm./Ind. Construction								
Estimate	-19.9	46.2*	33.8	33.0	20.0	22.9	19.1	63.6
	(28.9)	(23.8)	(31.4)	(62.3)	(40.5)	(35.6)	(44.7)	(57.1)
N	453	453	453	453	453	409	344	275
Financial Aid								
Estimate	26.3**	34.2**	-4.1	46.7*	42.4	42.8	61.6*	31.9
	(10.6)	(16.6)	(28.6)	(28.0)	(31.0)	(33.1)	(32.1)	(38.0)
N	453	453	453	453	453	409	344	275
Infrastructure								
Estimate	-30.2	-88.2**	-49.2	-51.2	-35.5	-14.8	-42.2	-42.0
	(22.2)	(34.3)	(34.8)	(34.4)	(38.5)	(53.2)	(47.7)	(62.8)
N	453	453	453	453	453	409	344	275
Micro-Enterprise								
Estimate	-16.7	-20.8	66.7	37.4	19.4	-17.5	-36.5	-43.4
	(11.5)	(19.7)	(71.5)	(28.5)	(33.8)	(28.1)	(39.3)	(35.2)
N	453	453	453	453	453	409	344	275
Exterior Improvements								
Estimate	-21.3	-17.9	-56.7*	-47.5	-54.2	-40.7	-20.0	-15.6
	(26.1)	(33.8)	(30.9)	(38.6)	(39.1)	(38.4)	(45.1)	(57.0)
N	453	453	453	453	453	409	344	275
Technical Aid								
Estimate	-5.2	30.8	110.8	-2.2	36.2	36.1	34.5	65.1
	(10.7)	(39.4)	(97.2)	(45.0)	(53.3)	(69.1)	(57.0)	(87.7)
N	453	453	453	453	453	409	344	275
Non-Profits and Other								
Estimate	-0.9	-19.2	-14.0	-29.0	-35.1	-12.5	-14.3	-5.2
	(14.8)	(19.6)	(37.7)	(43.6)	(36.7)	(32.4)	(45.6)	(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 A4. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

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.