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Craig Sylvera

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Place-Based Federal Procurement and Resident Jobs:

Lessons from the Historically Underutilized Business Zone Program

Craig Sylvera*

October 2026

Abstract

Unlike tax-based place policies that subsidize employment or investment, procurement-based approaches direct federal purchasing to businesses in distressed areas. I examine the HUBZone program, which targets federal contracts to firms in economically disadvantaged census tracts. I find that receiving an award increases jobs for residents of the census tract by 3.5 percent over five to seven years, with no corresponding increase in jobs within the census tract. Establishment-level analysis reveals no employment gains at recipient firms themselves, suggesting any employment effects occur outside the receiving firm. Housing markets respond positively with increased mortgage origination activity. Examining the variation in the size of awards does not reveal a relationship between award size and jobs gained. The findings demonstrate that procurement set-asides can improve local outcomes in distressed areas, though whether they create new jobs or redistribute existing opportunities remains an open question.

Keywords: place-based policies, procurement contracts, local employment, distressed communities

JEL Codes: R58, H57, J38, R23

*Federal Reserve Bank of Cleveland. Email: Craig.Sylvera@clev.frb.org.

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1 Introduction

Place-based policies aim to revitalize economically disadvantaged communities and create pathways to jobs for residents (Neumark and Simpson 2015; Kline and Moretti 2014b). However, their effectiveness in improving outcomes for residents has been substantially debated in the academic literature (Glaeser and Gottlieb 2008; Bartik 2020b, 2020a; Neumark 2020). At the federal level, the most common approach involves offering tax credits to subsidize employment or profits in designated areas (Busso, Gregory, and Kline 2013; Freedman 2012; Corinth and Feldman 2024). Yet questions remain about whether these tax-based incentives effectively reach intended communities and their residents (Reynolds and Rohlin 2015; Freedman, Khanna, and Neumark 2023; Neumark and Young 2019), and whether they provide stable benefits during economic downturns when disadvantaged areas need support most.

An alternative approach to place-based economic development involves directing federal procurement funds to businesses in underserved areas. This strategy offers a distinct advantage for community stability, because federal procurement spending remains relatively stable during economic downturns (Nakamura and Steinsson 2014; Chodorow-Reich 2019), potentially sustaining demand for local businesses and translating to more consistent employment opportunities (Ferraz, Finan, and Szerman 2015; Hoekman et al. 2026) precisely when disadvantaged communities are disproportionately affected by broader economic contractions.

This paper explores the Historically Underutilized Business Zone (HUBZone) program, which targets federal contracts to businesses operating in high-poverty, low-income census tracts. Specifically, I examine whether federal procurement dollars directed to businesses in distressed communities translate into jobs for residents, a core goal of the program, and whether housing markets respond to any observed job gains.

The theoretical case for tract-level job effects from HUBZone is ambiguous. Job gains could emerge through two competing channels. Firms might increase hiring of HUBZone residents in anticipation of contract receipt to meet program requirements, generating job gains before the award is received. Alternatively, firms already satisfying program requirements might receive awards and expand operations, with effects concentrated in the post-award period and scaling with contract obligation amounts. However, several program features may limit job impacts. HUBZone requires that 35 percent of a firm's employees reside in eligible areas, but firms have no incentive to employ HUBZone residents beyond this threshold, potentially limiting job gains for tract residents if firms expand by hiring commuters.

Individual contract values tend to be small relative to other place-based interventions, suggesting awards may support existing operations rather than drive substantial expansion. Job effects might also operate indirectly through local demand spillovers if contract dollars flow from the receiving firm into the tract.

Estimating the causal effects of HUBZone awards presents several identification challenges. I must distinguish between the effects of eligibility for HUBZone designation (obtaining Qualified Census Tract status) and the effects when firms in those tracts actually receive an award. Awards occur throughout the year with staggered timing, creating potential for bias in traditional difference-in-differences estimators when treatment effects are heterogeneous (Goodman-Bacon 2021; Sun and Abraham 2021; de Chaisemartin and D'Haultfœuille 2020; Roth et al. 2023). Program success can be defined relative to multiple counterfactuals: pre-eligibility trends, tracts narrowly above or below the eligibility threshold, or eligible tracts where no firms received awards.

I address these challenges through three complementary difference-in-differences designs that exploit different sources of variation, all using a two-stage imputation estimation strategy designed for staggered treatment adoption settings (Gardner et al. 2026; Borusyak, Jaravel, and Spiess 2024; Butts and Gardner 2022). This approach uses only never-treated and not-yet-treated observations to construct counterfactual outcomes, avoiding contamination from forbidden comparisons (Goodman-Bacon 2021; Callaway and Sant'Anna 2021). First, I estimate whether obtaining QCT status itself affects local outcomes by comparing tracts that became eligible following the 2003 release of Census 2000 data to tracts that did not qualify. Second, I estimate the effects of award receipt for tracts near the eligibility thresholds, effectively combining difference-in-differences with regression discontinuity (RD) to leverage quasi-experimental variation (Grembi, Nannicini, and Troiano 2016; Calonico, Cattaneo, and Titiunik 2014; Cattaneo, Titiunik, and Vazquez-Bare 2020). Third, I estimate the effects of award receipt using the full sample of tracts that obtained QCT status in 2003, providing estimates representative of the broader population of eligible tracts.

I combine data on HUBZone contract awards from the Federal Procurement Data System with census-tract-level job data from the Longitudinal Employer-Household Dynamics Origin-Destination Employment Statistics (LODES) (Urban Institute 2022), covering the period 2002-2019. The LODES data distinguish between jobs held by tract residents and jobs located in the tract, allowing me to separately analyze residence-based and workplace-based job effects. I supplement these with housing market data from the Home Mortgage Disclosure Act (HMDA) (Edlebi et al. 2024) and control for Low-Income Housing Tax Credit

developments (US Department of Housing and Urban Development (HUD), Office of Policy Development and Research (PD&R) 2000-2019), which also target QCT-eligible areas and may confound HUBZone effects.

I find that HUBZone award receipt generates modest positive job effects for census tract residents, comparable in magnitude to recent evaluations of Opportunity Zones and Empowerment Zones (Busso, Gregory, and Kline 2013; Freedman, Khanna, and Neumark 2023; Arefeva et al. 2025; Atkins et al. 2023). Award receipt increases resident-area jobs by 3.5 percent, with effects building gradually over five to seven years following initial receipt. Critically, establishment-level analysis reveals no detectable job changes at award-winning firms themselves, indicating that job creation operates primarily through indirect channels such as local demand spillovers or improved job retention rather than direct expansion at recipient establishments (Moretti 2010; Greenstone, Hornbeck, and Moretti 2010). This mechanism differs fundamentally from the direct job creation pathway typically assumed in small business contracting programs. The program also generates positive housing market effects, with increases in both mortgage origination volume and loan amounts, suggesting that job gains translate into increased housing demand (Baum-Snow and Marion 2009; Chen, Glaeser, and Wessel 2023).

Examining how effects vary with award size reveals that larger awards are not associated with proportionally larger job effects per dollar spent. In other words, areas that received a larger amount of HUBZone funds did not experience a larger increase in jobs. This finding implies that the operative margin of the program is receiving a HUBZone award itself, which may be related to incorporating smaller firms into the procurement pipeline or alternatively, that larger firms may have supply relationships that extend beyond HUBZone boundaries.

The findings demonstrate that procurement-based place policies can generate meaningful local improvements but work differently than commonly assumed. Rather than operating primarily through direct job creation at subsidized firms, HUBZone awards function more like demand-side interventions that stimulate local economic activity through spillovers. This aligns with recent theoretical work emphasizing local multipliers in place-based policies (Moretti 2010; Fajgelbaum and Gaubert 2025; Gaubert et al. 2025) and suggests that procurement set-asides may complement rather than substitute for other development tools. However, the modest effect sizes relative to program costs raise important questions about the efficiency of this approach compared to alternative mechanisms for supporting distressed communities (Kline and Moretti 2014b; Slattery and Zidar 2020).

This paper makes three contributions to the literature on place-based policies. First, I provide the first causal evidence on job and housing effects from a procurement-based place policy. While tax-based place policies such as Enterprise Zones and Opportunity Zones have been extensively studied, federal procurement as a place-based development tool remains largely unexamined; the existing procurement literature has focused on auction design, contractor selection, and firm-level outcomes rather than on the places where contractors operate (Marion 2009; Decarolis 2014; Krasnokutskaya and Seim 2011; Carril, Gonzalez-Lira, and Walker 2026; Ferraz, Finan, and Szerman 2015; Hoekman et al. 2026).[1] Understanding whether alternative policy mechanisms can achieve similar goals is essential for designing effective interventions and expanding the toolkit available to policymakers targeting distressed communities.

Second, I estimate the job return per dollar of procurement spending by relating tract-specific treatment effects to contract obligation amounts. In evaluating other place-based programs, cost-effectiveness is typically calculated by dividing total subsidies or tax credits allocated by aggregate job gains, providing an average cost per job across the entire program (Busso, Gregory, and Kline 2013; Kline and Moretti 2014a; Bartik 2020b; Slattery and Zidar 2020). By estimating tract-specific treatment effects and relating them to individual contract obligations, I can examine how job gains vary with contract size, providing more granular evidence on the returns to procurement spending at different scales (Criscuolo et al. 2019).

Third, I separately identify the causal effects of being designated an eligible area versus actual firm participation in the program. While many place-based policies share similar geographic targeting criteria, they differ fundamentally in how eligibility translates to benefits (Neumark and Simpson 2015; Deshpande and Li 2019). In programs such as Opportunity Zones, designation and benefit receipt may be endogenous to local economic and political conditions, complicating causal inference (Frank, Hoopes, and Lester 2022; Corinth et al. 2025; Neumark and Young 2019). In contrast, HUBZone designation follows mechanical income and poverty thresholds, and subsequent contract awards depend on federal agencies' procurement needs, factors largely unrelated to local economic conditions in competing tracts. This administrative structure provides plausibly exogenous variation in program participation. By estimating both the effect of obtaining Qualified Census Tract status and the

[1] On state and federal enterprise and empowerment zones, see Busso, Gregory, and Kline (2013); Ham et al. (2011); Neumark and Kolko (2010); Kolko and Neumark (2010); Freedman (2013); Hanson and Rohlin (2010); Hanson and Rohlin (2013); Reynolds and Rohlin (2015); Bondonio and Greenbaum (2007); and Neumark and Young (2019). On Opportunity Zones, see Chen, Glaeser, and Wessel (2023); Freedman, Khanna, and Neumark (2023); Arefeva et al. (2025); Atkins et al. (2023); Corinth and Feldman (2024); Freedman, Kouchekinia, and Neumark (2026); and Frank, Hoopes, and Lester (2022). On other federal geographically targeted credits, see Freedman (2012) and Corinth et al. (2025).

incremental effect of firms receiving contracts, I clarify whether program impacts flow from the designation itself or from resource transfers to participating firms. This distinction has implications for program design and the interpretation of cost-effectiveness.

The paper proceeds as follows. Section 2 describes the HUBZone program, contrasts it with familiar place-based policies, and offers some hypotheses on potential effects. Section 3 outlines the estimation strategy and discusses the identification and validity of the assumptions. Section 4 discusses the data and provides descriptive statistics. I present the empirical results in Section 5. In Section 6, I describe and estimate the program's effect by the size of the obligation, and I conclude in Section 7.

2 Background

2.1 HUBZone History

The Historically Underutilized Business Zone program was enacted in 1997 with the goal of stimulating economic development in distressed communities through federal procurement preferences (Dilger and Blackford 2022; Blackford 2023, 2025). The program aims to award at least 3 percent of federal contract dollars to HUBZone-certified companies each year through set-aside contracts, sole-source contracts, or price-evaluation preferences; however, program utilization has been variable and generally low, so the goal has never been met in aggregate. Figure 1 illustrates total obligated contracting dollars through HUBZone from 1999, the year in which the first contract was awarded, to 2020 based on when the contract was first signed.[2] Aggregate obligations peak around and in the aftermath of the Great Recession, a pattern that distinguishes HUBZone from other place-based programs as government procurement acted as a countercyclical source of demand during this period.

Award receipt is two-pronged. First, a location must qualify as a HUBZone-eligible area. Second, firms in those areas must satisfy business- and industry-specific requirements. Geographic eligibility can be satisfied through several pathways, including qualification as a metropolitan-area census tract, non-metropolitan county, or location on Indian lands.

For metropolitan census tracts, which are the focus of this study, qualification is determined by income and poverty thresholds. A census tract is eligible for qualified status if at least 50 percent of its households have incomes below 60 percent of area median gross income

[2]Some contracts are multiyear and other contracts receive modifications. In these cases, the total value of the award has been assigned to the initial date of signing.

(AMGI), or if its poverty rate is at least 25 percent. However, no more than 20 percent of a metropolitan area's population can reside in qualified tracts. When more than 20 percent of the population would otherwise qualify, the Department of Housing and Urban Development (HUD) uses a ranking procedure that prioritizes tracts meeting both income and poverty criteria, then tracts meeting only one criterion, with ties broken by tract population in favor of the more populous. These designations, known as Qualified Census Tracts (QCTs), are maintained by HUD and updated periodically (US Department of Housing and Urban Development (HUD), Office of Policy Development and Research (PD&R) 2025). When a census tract loses its QCT designation, it receives a three-year grace period during which it is classified as redesignated and maintains its eligibility. This study focuses on the 2003-2015 period. During this window, QCT designations were based on data from the 2000 Decennial Census through 2012, after which HUD transitioned to using rolling averages of American Community Survey estimates. The relative stability of these designations during this period meant firms faced minimal unexpected changes in eligibility.

Second, a business in a qualified area must meet several requirements to participate in the program. The firm must qualify as a small business according to Small Business Administration size standards and at least 51 percent must be owned by US citizens, a Community Development Corporation, an agricultural cooperative, an Alaska Native Corporation, a Native Hawaiian organization, or an Indian tribe. Additionally, the business must maintain its principal office within a HUBZone, which is determined by where the greatest number of the firm's employees perform their work. In addition, 35 percent of the firm's employees must reside in HUBZones. The 35 percent threshold may limit the program's residential employment impact, as firms have no incentive to increase HUBZone resident employment beyond this minimum requirement.

QCTs also serve as essential infrastructure for the Low Income Housing Tax Credit (LIHTC) program, in which developments located in QCTs receive larger tax benefits than those in non-QCT areas. Unlike HUBZone, LIHTC is not a federally administered program; states exercise considerable discretion in allocating credits through a more complex allocation process. Approximately half of all LIHTC units and allocated credits are currently located in QCTs (see Appendix Figure A.2).[3] This overlap between QCT-eligible areas and LIHTC placement raises potential concerns about confounding effects. Prior research has examined the LIHTC's neighborhood impacts via the QCT threshold; for instance, Baum-Snow and Marion (2009) find that LIHTC developments raise nearby property values in

[3]HUD reports that 34.2 percent of LIHTC projects and 40.5 percent of LIHTC units placed in service in 2004 were located in QCTs (Hollar and Usowski 2007)

declining neighborhoods, depress household income in gentrifying ones, and increase homeowner turnover, with effects concentrated around the 30th percentile of the neighborhood income distribution. Given these potential overlapping impacts, it is important to account for LIHTC activity when estimating HUBZone effects. This study primarily focuses on how receiving a HUBZone contract affects tract-level job outcomes rather than the effect of obtaining QCT status itself.

2.2 How Does HUBZone Compare to Other Place-Based Policies?

Most federal place-based policies operate through tax incentives, credits, or direct subsidies to encourage economic development in distressed areas. The HUBZone program differs in that it functions through federal procurement preferences rather than tax mechanisms. Enterprise Zones and Opportunity Zones represent some of the most extensively studied place-based policies. The federal Empowerment Zone and Enterprise Community program was created by Title XIII of the Omnibus Budget Reconciliation Act of 1993, and combined wage tax credits for hiring zone residents with large block grants; it should be distinguished from the state enterprise zone programs that preceded it, which vary widely in design and generosity. Opportunity Zones, established through the 2017 Tax Cuts and Jobs Act, offer capital gains tax benefits to investors in designated low-income communities. The New Markets Tax Credit provides tax credits to investors who make equity investments in low-income communities through Community Development Entities.

Evidence on employment effects is mixed. Busso, Gregory, and Kline (2013) find that federal Empowerment Zones substantially increased employment in designated neighborhoods, with job growth estimates of roughly 12 to 21 percent in the Longitudinal Business Database and 12 to 19 percent in census data, alongside wage gains for zone workers and little movement in population or local cost of living. State enterprise zone programs generally show weaker effects. For instance, Neumark and Kolko (2010) find no employment gains in California, and Neumark and Young (2019) show that once selection into zone designation is accounted for, much of the apparent reduction in poverty from state programs disappears. Evidence from comparable programs abroad is similarly modest (Gobillon and Magnac 2016). Freedman (2015) finds that New Markets Tax Credit investments create jobs in targeted areas but that many positions go to non-residents, suggesting place-based subsidies may generate limited benefits for local workers when firms can hire from broader labor markets; HUBZone attempts to mitigate this through the 35 percent resident employment requirement.

Program cost-effectiveness is difficult to compare because published figures differ in what costs and employment outcomes they measure, and few account for spatial displacement. For federal Empowerment Zones, Busso, Gregory, and Kline (2013) estimate wage gains of approximately $320 million per year with deadweight costs at most 7 percent of employment credit flows; third-party cost-per-job derivations range from $70,000 to $84,000 depending on assumptions. Bartik (2020b) estimates present-value costs of roughly $196,000 per job induced by typical state or local business tax incentives, $34,000 per job for public services such as customized training, and about $77,000 per job (2019 dollars) for the Tennessee Valley Authority based on Kline and Moretti (2014a). Opportunity Zones show tax expenditures of $8.2 billion over 2020–2024, but Freedman, Kouchekinia, and Neumark (2026) find workplace gains of only 1.4 percent, with fewer than one in eight jobs accruing to zone residents and over three-quarters going to higher-income tract residents. Glaeser and Gottlieb (2008) argue that place-based policies face fundamental challenges when subsidizing distressed locations works against spatial equilibrium forces.

These benchmarks are not directly applicable to HUBZone. Procurement obligations represent payments for goods and services the government would typically purchase regardless; the relevant fiscal cost is the incremental expense of directing contracts to HUBZone firms rather than next-best bidders, not the face value of obligations. Total obligations therefore overstate program costs by a potentially large but unknown factor, and cost-per-job statistics constructed from them should be interpreted as upper bounds. Section 6 reports job responses per dollar obligated and discusses the appropriate interpretation of cost-effectiveness from this measure.

Housing market effects have emerged as an important program consequence. Similar concerns about gentrification and displacement have been raised regarding Opportunity Zones, where increased investment may benefit property owners more than existing low-income residents, though the evidence is mixed (Chen, Glaeser, and Wessel 2023; Corinth et al. 2025).

Other Small Business Administration programs, such as the 8(a) Business Development program and the Service-Disabled Veteran-Owned Small Business program, also provide federal contracting preferences but without the geographic targeting central to HUBZone. Both programs substantially exceed HUBZone in scale, though evidence on their local economic effects remains limited. This study contributes to understanding whether procurement-based place policies can generate tract-level job effects comparable to more extensively studied

tax-based interventions.[4]

2.3 Should HUBZone Have an Effect?

The theoretical case for tract-level job effects from HUBZone is ambiguous. While the program provides contracting preferences to firms in distressed areas, several features may limit its impact. First, the scale of contracting activity is relatively modest—the program has never met its 3 percent federal contracting goal, and individual contract values tend to be small. Second, the 35 percent resident employment requirement creates a ceiling rather than a floor: firms have no incentive to employ HUBZone residents beyond this threshold, potentially limiting local labor market spillovers. Third, most program-eligible firms already qualify as small businesses, suggesting contracts may support existing operations rather than drive substantial expansion.

Two competing mechanisms could generate job effects. In one case, firms increase HUBZone resident employment in anticipation of receiving a contract to meet the 35 percent threshold, generating job gains before an award is received. This would manifest as pre-trends at the firm level, though the tract-level implications are less clear if multiple firms are simultaneously adjusting. Alternatively, firms already in compliance receive awards and expand operations. This mechanism suggests that effects should scale with obligation amounts and be concentrated in the post-award period.

A third possibility is that measured tract-level gains reflect reallocation rather than creation. Firms may relocate or shift hiring across tract boundaries in response to the preference, and the residency requirement gives firms an incentive to recruit workers who already live in eligible tracts rather than to expand employment overall. Hanson and Rohlin (2013) document exactly this pattern for Empowerment Zones, and Freedman, Kouchekinia, and Neumark (2026) for Opportunity Zones.

Job effects might also operate through local demand spillovers rather than direct job creation at recipient firms. If contract dollars flow to local suppliers or HUBZone resident employees spend earnings locally, downstream employment could emerge in sectors that do

[4]Other place-based interventions provide useful points of comparison. Greenstone, Hornbeck, and Moretti (2010) examine "million dollar plants" and find that incumbent plants in counties that won a large new manufacturing facility had total factor productivity roughly 12 percent higher five years later than incumbents in the runner-up counties, with larger spillovers for plants sharing labor and technology pools with the entrant. Jump and Scavette (2025) find that anchor institutions such as flagship universities reduce unemployment during some recessions, while Akee, Jones, and Simeonova (2025) document employment effects from tribal casinos concentrated in casino-related sectors.

not receive HUBZone awards. This would generate broader job gains than direct effects alone, though the magnitude depends on local spending patterns and local supply chain integration.

Firm characteristics may moderate program effects. Firm age presents competing predictions. Younger firms may be more likely to apply and benefit from capital infusion for growth, while older firms may possess the administrative capacity and relationships necessary to capture federal contracts. Similarly, while the small business size threshold suggests that most firms qualify, job effects may be stronger among the smallest firms if contracts represent a larger share of their revenue.

Housing market effects depend on whether employment gains materialize and money flows locally. Absent job effects, housing demand should remain unchanged. However, if the program generates local job or income gains, increased housing demand could raise property values or rents, which may scale proportionally by cumulative obligations. Given these considerations, we test whether receiving a HUBZone contract generates detectable tract-level job effects, whether effects vary by contract intensity and firm characteristics, and whether housing market responses emerge consistent with local demand increases.

3 Empirical Strategy

I aim to estimate the causal impact of receiving a HUBZone award on census-tract jobs, housing prices, and housing demand. The empirical approach must address several identification challenges to recover credible causal estimates.

First, I must distinguish between the effects of eligibility for HUBZone designation (obtaining QCT status) and the effects when firms located in those tracts actually receive an award. If QCT eligibility itself affects local economic outcomes, for example, by signaling opportunity to firms, then a simple comparison of QCTs with awards versus QCTs without awards would confound these two mechanisms. To isolate these distinct channels, I first estimate the causal effect of obtaining QCT status on local outcomes, then separately estimate the effect of receiving a HUBZone award conditional on QCT eligibility.

Second, HUBZone awards occur throughout the year and eligibility status can change over time, creating a staggered treatment adoption setting. In such settings, traditional two-way fixed effects (TWFE) estimators use already-treated or concurrently treated units as controls for newly treated units. When treatment effects are heterogeneous across cohorts

or over time, this creates "forbidden comparisons" that can bias estimates of the average treatment effect (Goodman-Bacon 2021; Sun and Abraham 2021). To avoid this contamination, I use a two-stage difference-in-differences estimator with imputation methods designed for staggered adoption settings (Borusyak, Jaravel, and Spiess 2024; Gardner et al. 2026; Liu, Wang, and Xu 2024). This approach uses only never-treated and not-yet-treated observations to construct counterfactual outcomes, ensuring clean comparisons.

Third, program success can be defined in multiple ways, each addressing a distinct policy question. The program aims to increase employment in economically distressed areas, but success could be evaluated by comparing outcomes to: (a) pre-eligibility trends within treated tracts (does obtaining QCT status improve local conditions?), (b) tracts narrowly above the eligibility threshold (does the program generate benefits for marginally qualifying tracts?), or (c) eligible tracts where no firms received awards (do actual contract awards matter beyond eligibility designation alone?). I examine multiple comparison groups to provide a comprehensive assessment, with particular emphasis on comparisons (b) and (c), which leverage quasi-experimental variation from the eligibility threshold and isolate the incremental effect of firms receiving, and by extension the census tract, HUBZone contracts, respectively.

I address these challenges through three complementary difference-in-differences designs that exploit different sources of variation. All three use the same two-stage imputation estimation strategy, which I describe alongside the first estimation strategy.

3.1 Pre-/Post-Eligibility Effects

To estimate whether QCT eligibility itself affects local outcomes, I implement a difference-in-differences design comparing census tracts that became QCT-eligible following the release of Census 2000 data in 2003 to tracts that did not qualify. Tracts qualify as QCTs if their median household income is below 60 percent of area median income (AMI) or their poverty rate exceeds 25 percent. The analysis uses job data from 2002 to 2019 and housing data from 2000 to 2019.

The estimation proceeds in two stages. In the first stage, I estimate a fixed effects regression using only not-yet-treated observations (including never-treated observations):

$$y_{ijt}^0 = \alpha_i^0 + \delta_t^0 + \lambda_j^0 t + \beta_1 QCT_{it}^0 + \beta_2 LIHTC_{it}^0 + \varepsilon_{ijt}^0 \tag{1}$$

where y^0_{ijt} is the outcome of interest in census tract i, metropolitan statistical area j, in year t. The specification includes tract fixed effects (α^0_i), which absorb time-invariant tract characteristics, time fixed effects (δ^0_t), which capture common shocks affecting all tracts in a given year, and metro-area-specific linear time trends ($\lambda^0_j t$). I control for baseline QCT status (QCT^0_{it}) and Low-Income Housing Tax Credit (LIHTC) project presence ($LIHTC^0_{it}$), which may be correlated with both QCT status and local economic outcomes.

By restricting the sample to not-yet-treated observations, the estimated fixed effects $\hat{\alpha}^0_i$, $\hat{\delta}^0_t$, and $\hat{\lambda}^0_j$ are not contaminated by treatment. In the second stage, I use these estimates to construct counterfactual outcomes for all tract-year observations:

$$\hat{y}^0_{ijt} = \hat{\alpha}^0_i + \hat{\delta}^0_t + \hat{\lambda}^0_j t + \hat{\beta}_1 QCT^0_{it} + \hat{\beta}_2 LIHTC^0_{it} \tag{2}$$

The outcome variable for the second stage is the difference between observed and counterfactual outcomes: $\tilde{y}_{ijt} = y_{ijt} - \hat{y}^0_{ijt}$. I then regress this residualized outcome on treatment indicators:

$$\tilde{y}_{ijt} = \tau \mathbf{1}\{2003 - t \geq 0\} \times HZ_i + \mu \mathbf{1}\{2003 - t = k\} \times HZ_i \times f(e^Y_{ij}, e^p_{ij}; \theta) + \eta_{ijt} \tag{3}$$

The coefficient τ captures the average treatment effect of QCT eligibility starting in 2003, evaluated at the eligibility thresholds. The term $f(e^Y_{ij}, e^p_{ij}; \theta)$ controls for the distance from both eligibility thresholds: e^Y_{ij} is the distance of tract i's median household income from 60 percent of AMI normalized by AMI, and e^p_{ij} is the distance of its poverty rate from 25 percent normalized by 25. I model $f(\cdot)$ as a linear function that allows different slopes on either side of each threshold. This controls for the direct relationship between the running variables and outcomes, ensuring τ isolates the discontinuous change at the eligibility threshold.

For the regression discontinuity component, I restrict the sample to tracts within 1.5 bandwidths of either eligibility threshold. Using the optimal bandwidth calculation of Calonico, Cattaneo, and Titiunik (2014), 1.5 bandwidths corresponds to 0.285 for the income running variable and 0.317 for the poverty running variable. This bandwidth provides a sufficient treatment sample while focusing on tracts close to the eligibility thresholds where comparability is strongest.

The coefficient τ identifies the causal effect of eligibility under the parallel trends assumption. For job outcomes, I have only one pre-treatment year (2002), which prevents me from estimating a full event study to assess pre-trends for this outcome. The 2003 eligibility analysis therefore relies on the baseline difference-in-differences specification in Equation 3 with the maintained assumption of parallel trends. I implement this estimator using the `did2s` package from Butts and Gardner (2022) and cluster standard errors at the census-tract level to account for serial correlation within tracts over time.

3.2 HUBZone Award Receipt Effects: Tracts Near the Eligibility Threshold

Having estimated the effect of eligibility, I now turn to estimating the effect of firms receiving HUBZone contract awards. Specifically, I estimate the impact of the first HUBZone award received by a firm located in a QCT-eligible census tract. This isolates the incremental effect of actual program participation.

The design leverages two key sources of variation. First, because awards occur at different times across tracts (staggered adoption), the two-stage imputation estimator uses not-yet-treated tracts as controls, avoiding bias from forbidden comparisons. Second, I interact treatment effects with distance from the QCT eligibility thresholds, effectively combining difference-in-differences with regression discontinuity. The estimation proceeds as:

$$
\begin{aligned}
y^0_{ijt} &= \alpha^0_i + \delta^0_t + \lambda^0_j t + \beta_1 QCT^0_{it} + LIHTC^0_{it} + \varepsilon^0_{ijt} \\
\tilde{y}_{ijt} &= \sum_{k=-8}^{-2} \tau_k \mathbf{1}\{HZ_{it} - t = k\} + \sum_{l=0}^{12} \tau_l \mathbf{1}\{HZ_{it} - t = l\} \\
&\quad + \left[\sum_{k=-8}^{-2} \mu_k \mathbf{1}\{HZ_{it} - t = k\} + \sum_{l=0}^{12} \mu_l \mathbf{1}\{HZ_{it} - t = l\} \right] \times f(e^Y_{2003}, e^p_{2003}; \theta) + \eta_{ijt}
\end{aligned} \tag{4}
$$

where HZ_{it} is the year tract i first receives a HUBZone award, and relative time $t - HZ_{it}$ indexes years before/after the first award. The event study coefficients $\tau_{k\ \{k<0, k\neq -1\}}$ capture pre-treatment dynamics and provide evidence on whether treated and control tracts followed similar trends before treatment. The coefficients $\tau_{l\ \{l\geq 0\}}$ trace out post-treatment effects over time. Control units are set to relative year -1, the year before award receipt, and are normalized to zero. To avoid imprecise estimates from small sample sizes at long leads and

lags, I bin event-time endpoints, grouping all periods $k \leq -8$ into a single indicator and all periods $l \geq 12$ into a single indicator (Schmidheiny and Siegloch 2023).

The interaction terms with $f(e^Y_{2003}, e^p_{2003}; \theta)$ control for distance from the eligibility thresholds. At the threshold where both running variables equal zero, the conditional expectation of these interaction terms is zero, so the coefficients τ_k, τ_l identify the treatment effects at the discontinuity. This focuses identification on tracts at the eligibility margin where treatment assignment is plausibly as-good-as-random. The design is valid if, conditional on distance from thresholds and fixed effects, tracts receiving awards at different times would have evolved similarly absent treatment.

3.3 HUBZone Award Receipt Effects: QCT Tracts in 2003

Finally, I estimate the effects of receiving an award using the full sample of tracts that obtained QCT status in 2003, without restricting the sample to those near eligibility thresholds. This provides estimates that are more representative of the broader population of QCT-eligible tracts, though potentially with weaker identification than the threshold-based design. Because all tracts in this sample face the same eligibility rules, this approach mitigates concerns that gains in QCT tracts might simply reflect displacement from similar but ineligible tracts near the threshold.

I restrict the sample to census-tracts that: (1) had QCT status in 2003, and (2) had not received a HUBZone award as of the 2003 designation. This restriction focuses the comparison on: among tracts that were QCT-eligible in 2003, what is the effect of firms in some tracts subsequently receiving awards? The estimation proceeds as:

$$
\begin{aligned}
y^0_{ijt} &= \alpha^0_i + \delta^0_t + LIHTC^0_{it} + \varepsilon^0_{ijt} \\
\tilde{y}_{ijt} &= \sum_{k=-8}^{-2} \tau_k \mathbf{1}\{HZ_{it} - t = k\} + \sum_{l=0}^{12} \tau_l \mathbf{1}\{HZ_{it} - t = l\} + \eta_{ijt}
\end{aligned} \tag{5}
$$

The first stage uses only tract and time fixed effects plus LIHTC controls. The event-study coefficients $\tau_{k\{k\leq -2\}}$ test for differential pre-trends between tracts that eventually receive awards and those that do not. The post-treatment coefficients $\tau_{l\{l\geq 0\}}$ capture the dynamic treatment effects after receipt of the first award.

3.4 Testing for Manipulation of the QCT Eligibility Threshold

To support the RD design, I test for manipulation of the running variables using the density test of Cattaneo, Jansson, and Ma (2020). Manipulation occurs when units strategically sort around the eligibility threshold, violating the continuity assumption underlying the RD design. In this context, manipulation is somewhat implausible. Strategic sorting would require coordination between firms anticipating future HUBZone contract applications and the residents whose income and poverty rates determine tract eligibility. Firms would need to predict their program participation years in advance, identify tracts near eligibility thresholds, and influence residential composition before QCT designation occurs.

Figure 2 presents the results of these tests. The test examines whether the density of observations changes discontinuously at the eligibility threshold. A significant discontinuity would suggest that tracts bunch just below the income threshold or just above the poverty threshold to gain QCT eligibility. The tests fail to reject the null hypothesis of continuity in both running variables ($p = 0.861$ for income and $p = 0.624$ for poverty), providing no evidence of systematic manipulation. These high p-values indicate that the distribution of tracts is smooth through the eligibility thresholds, consistent with the assumption that tracts near the cutoff are comparable in both observed and unobserved characteristics.

4 Data

HUBZone Awards - Federal Procurement Data System: The main explanatory variable is receipt of a HUBZone award. To construct receipt at the tract level, I begin by accessing the Federal Procurement Data System (FPDS), a real-time database tracking US government procurement spending. I access FPDS through its publicly available ATOM feed API (version 1.5.3) beginning in 1999, filtering for contracts with a HUBZone award designation. This yields the universe of contract transactions for the HUBZone program during this period.

The FPDS data contain detailed information for each award, including the recipient vendor's street address, the dollar value of obligations, and the contract signing date. Additional fields capture vendor and owner characteristics (such as business size, ownership demographics, and SBA certifications), contract characteristics (such as NAICS codes, product/service codes, and contract pricing type), competition details (such as extent competed and type of set-aside), and transaction identifiers (such as agency ID, contract ID, and modification numbers). FPDS records data at the transaction level rather than the contract level, so each

modification to a contract, including non-monetary administrative changes, generates a new record. As an intermediate processing step, I collapse all transactions associated with the same contract to a single observation.[5] For each unique contract, I retain the initial signing date and the final recorded total obligation as of the query date. This produces a dataset of unique contracts with their associated values and characteristics that can subsequently be allocated to geographic units.

The FPDS data contain vendor street addresses but not census tract identifiers, so I geocode each unique address to assign contracts to census tracts. For each vendor, I retain the address recorded at the time of initial contract signing. I use the tidygeocoder package in R (Cambon et al. 2021), which queries the US Census Bureau's geocoding service. This service matches addresses against the Master Address File and returns geographic coordinates along with corresponding census geographic identifiers. I use the Public Address Ranges benchmark with the Census 2010 vintage to ensure consistency with the 2010 census tract definitions used throughout the analysis. Overall, approximately 90 percent of addresses successfully match to a census tract. Unmatched addresses include records with incomplete address fields that cannot be geocoded, addresses outside the United States (such as contractors in US territories), and locations outside metropolitan statistical areas, which fall outside my analytical sample. Consequently, the effective match rate for the relevant sample is higher than the raw match rate suggests.

QCT Status and MSA Boundaries: To receive a HUBZone award, businesses must be located in Qualified Census Tracts (QCTs). I map census tracts to QCT status using the Department of Housing and Urban Development's annual QCT designations (US Department of Housing and Urban Development (HUD), Office of Policy Development and Research (PD&R) 2025). My analysis uses 2003 as the anchor year because this is when income and poverty measures from the 2000 Census first became applicable for determining program eligibility. This timing defines my treatment group: tracts that gained QCT status based on 2000 Census data. In addition, I construct a time-varying QCT eligibility indicator that spans all years in my sample. This approach accounts for tracts that were already eligible prior to 2003 under earlier criteria as well as tracts that subsequently gain or lose eligibility in later years. I restrict the analytical sample to census tracts located within metropolitan statistical areas (MSAs) as defined in 2003. Additional tract-level characteristics, including demographic and economic variables, are obtained from IPUMS NHGIS (Schroeder et al. 2026). I also incorporate data on Low-Income Housing Tax Credit (LIHTC) allocations

[5] Contracts are identified and ordered by the combination of agency ID, procurement instrument identifier, referenced indefinite delivery vehicle ID, modification number, and transaction number.

from HUD (US Department of Housing and Urban Development (HUD), Office of Policy Development and Research (PD&R) 2000-2019) to account for potential correlated place-based policies.

Job Outcomes: The main outcome variables are census-tract job counts. I use data from the Longitudinal Employer-Household Dynamics Origin-Destination Employment Statistics (LODES), specifically the Urban Institute's summary files aggregated to the census tract level (Urban Institute 2022). LODES data are derived from state unemployment insurance wage records linked with other administrative data sources.[6] I use two categories of files. The Residence Area Characteristics (RAC) file counts jobs by the census tract where workers live, which I refer to as resident-area jobs throughout. The Workplace Area Characteristics (WAC) file counts jobs by the census tract where workers are employed, which I refer to as workplace-area jobs throughout. The analysis uses only the private-sector job files, covering 2002 to 2019. Within these files, job counts are available with breakdowns by worker characteristics (age, earnings, race and ethnicity, educational attainment, and sex) and, in the WAC files, by establishment characteristics, including industry sector (NAICS code), firm age, and firm size. I exclude states missing data in 2002, because I cannot observe jobs in the year before the 2003 eligibility determination based on the 2000 Census.[7]

HMDA Longtiudinal Data: To examine whether HUBZone awards are capitalized into local housing prices, I use mortgage origination data from the Home Mortgage Disclosure Act Longitudinal Dataset (HLD) compiled by the National Neighborhood Data Archive (Edlebi et al. (2024)). From this dataset, which spans 1981-2021, I use data from 2000 to 2020 to align with my analysis period. The HLD aggregates individual loan records for owner-occupied, single-family dwellings (1-4 units) to the census-tract level, providing annual counts and dollar amounts of mortgage originations by loan type (conventional, FHA, VA, RHS/FSA), as well as breakdowns by borrower race, ethnicity, and gender where available. The data are normalized to 2010 census-tract boundaries using the Longitudinal Tract Database crosswalk, ensuring geographic consistency across the sample period. While HMDA provides the most comprehensive source of mortgage lending data, it does not capture cash transactions, which constitute a minority of residential real estate purchases. If HUBZone contracts primarily affect financed purchases, the dominant form of home buying, HMDA data should capture the main housing market effects.

[6]The data exclude military personnel, self-employed workers, and other workers not covered by unemployment insurance. For individuals with multiple jobs, LODES records all jobs separately.

[7]Excluded states include Arizona, Arkansas, Massachusetts, Mississippi, New Hampshire, and the District of Columbia.

YourEconomy Time Series data: To examine employment effects at HUBZone-recipient establishments, I use the YourEconomy Time Series (YTS), a longitudinal establishment-level database compiled by the Universities of Wisconsin Office of Business and Entrepreneurship (Universities of Wisconsin Office of Business and Entrepreneurship 2026). YTS is sourced from Data Axle, a commercial data provider that maintains a comprehensive database of US business establishments through annual telephone surveys and web-based verification. The data span 1997 to 2020 and include employment counts at the establishment level for businesses, government entities, and non-profits.

YTS contains several key establishment characteristics, including employment, location coordinates, industry classifications (NAICS), and corporate family relationships.[8] Employment figures include all persons working at an establishment—full-time, part-time, temporary workers, and proprietors, making no distinction between payroll and non-payroll workers. More than half of establishments have employment confirmed through telephone interviews or verified online sources, while the remainder are estimated via a proprietary predictive model using the verified counts.[9]

4.1 Descriptive Stats

Table 1 presents summary statistics for the main analysis sample of census tracts from 2000 to 2019. The sample includes 586,288 tract-year observations, though the number of observations varies by variable due to differences in data availability across sources. On average, more people work in a tract than residents of that tract who are employed.

Housing market activity averages 139 mortgage originations per tract-year with a total value of $23.1 million (in nominal dollars), corresponding to an average loan size of $148,000. Fourteen percent of tract-year observations have QCT status as of 2003, the key eligibility threshold for the analysis. Two percent of observations have Low-Income Housing Tax Credit properties, which may generate correlated place-based investment. The average tract has a median household income of $44,866 with a minimum of $2,499 and maximum of $250,001. The average poverty rate is 4.4 percent, ranging from 0 to 100 percent. QCT designation depends on whether tracts fall below the income threshold of 80 percent of area median income or exceed the poverty rate threshold of 25 percent.

[8] In contrast to the LODES data in which I refer to "job effects," I use "employment effects" with YTS data. This distinction reflects differences in measurement. YTS reports employees that are person-level counts, whereas LODES reports jobs that can include multiple jobs held by the same person.

[9] YTS excludes "paper companies," which are entities created solely for financial, real estate, or tax reporting purposes that do not conduct commercial activities, ensuring the database reflects active business operations.

In Appendix Table A.6, I present additional descriptive statistics for the two analytic samples used in the main analysis, separated by treatment status. The RD sample includes 3,201 tracts near the QCT designation threshold, of which 315 received HUBZone awards. The QCT 2003 sample includes 3,873 tracts with QCT status as of 2003, of which 474 received HUBZone awards.

Table 2 describes establishment-level characteristics in the YTS data for firms with Unique Entity Identifiers, comparing establishments that never receive HUBZone awards to those that do. A Unique Entity Identifier is required for entities seeking to do business with the federal government. The treatment group includes 18,825 establishment-year observations across firms that receive at least one HUBZone contract during the sample period, while the control group includes 4,021,485 establishment-year observations. Mean employment levels are similar across groups, though the median treated establishment is larger.[10]

The industry composition differs substantially between groups. Construction accounts for 31 percent of treated establishments compared to 10 percent of control establishments. Professional, Scientific, and Technical Services represent 21 percent of treated establishments and 17 percent of controls. Manufacturing comprises 19 percent of treated establishments and 12 percent of controls. Health Care and Social Assistance, which represents 9 percent of control establishments, accounts for less than 1 percent of treated establishments. This distribution aligns with the contract-level industry composition in Table A.3. Given these industry differences between treated and control establishments, the establishment-level analysis focuses on the subset of establishments that receive HUBZone awards.

5 Results

5.1 Eligibility Takeaway

In Table 3, I examine the impact of QCT status on job and housing finance outcomes for tracts near the eligibility threshold. Panel A presents results from a basic two-way fixed effects specification, while Panel B, the preferred specification, adds MSA-specific linear trends to account for differential metropolitan growth trajectories.

Panel A reveals consistently negative associations between QCT designation and both job and housing outcomes. Resident-area jobs decline by 4.2 log points relative to a baseline

[10] Additional establishment-level statistics are available in the Appendix, including match rates between contracts and establishment identifiers in Table A.7, owner characteristics in Table A.8, and statistical comparisons between recipient and non-recipient firms in Table A.9.

mean of 6.92, corresponding to approximately 42 fewer jobs in a typical tract. Workplace-area jobs fall by 1.8 log points, or roughly 18 jobs. The housing finance effects are more pronounced. Mortgage originations decline by 8.7 log points from a baseline of 3.37, representing approximately 2-3 fewer originations per tract. However, the amount per origination shows essentially no change, indicating that conditional on obtaining credit, loan sizes remain unaffected. This pattern points to effects operating primarily on the extensive margin of credit access rather than loan terms.

Panel B, which controls for MSA-specific trends, shows smaller estimated effects while preserving the overall pattern. Resident-area jobs now decline by 3.7 log points, corresponding to approximately 37 fewer jobs, while workplace jobs fall by 2.1 log points, or about 21 jobs. The mortgage origination effect is 7.7 log points, and the amount per origination remains statistically and economically zero. The consistency of results across specifications strengthens the evidence that QCT designation is associated with reductions in both job and housing finance activity near the eligibility threshold.

The income and poverty interaction terms show mixed signs and lack statistical precision across both panels, which is expected in this regression discontinuity-style difference-in-differences design. Since tracts near the eligibility threshold should be comparable in baseline characteristics, these running variable interactions should, in expectation, equal zero, serving primarily as specification checks rather than substantive heterogeneity tests. Their imprecision and values centered near zero support the validity of the identifying assumption that treatment and control tracts are balanced on pre-treatment characteristics.

Overall, the results indicate no evidence of positive effects from obtaining QCT status. Job levels are lower and mortgage originations decline, with no offsetting effects on loan sizes. Given the absence of positive outcomes from designation itself, I turn next to examining the effects of actual program participation through HUBZone procurement award receipt.

5.2 Jobs

Figure 3 presents event-study estimates of the effect of HUBZone contract receipt on resident-area jobs. Panel A shows results from the estimation of equation 4 for the RD sample of tracts near the eligibility thresholds, while Panel B presents results from the estimation of equation 5 for the full sample of tracts that obtained QCT status in 2003. Both panels plot point estimates and 95 percent confidence intervals for job changes relative to the year before receipt of the first contract normalized to zero. In addition, I print pre-HUBZone

award means, Wald test p-values for the joint nullity of pre-award coefficients, and post-award average treatment effect on the treated (ATT), denoted τ_{POST}, in each plot.

The pre-treatment coefficients in both panels are statistically indistinguishable from zero and exhibit no systematic trends, supporting the parallel trends assumption. The Wald test fails to reject the null hypothesis of joint insignificance for pre-treatment periods, providing formal evidence of no differential pre-trends between treated and control tracts.

Following contract receipt, resident-area jobs increases in both samples. In the RD sample (Panel A), the average post-treatment effect is 0.9 log points, though the confidence intervals widen considerably in later periods. Relative to the baseline mean of 7.05, this represents approximately 11 additional jobs for tract residents. In the 2003 QCT sample (Panel B), job effects are larger and more persistent, with an average post-treatment effect of 3.5 log points and a baseline mean of 6.86, translating to approximately 35 additional jobs. The effects grow over time, peaking around years 8-10 before moderating slightly. The confidence intervals remain relatively stable throughout the post-period. The difference in magnitudes between the two samples reflects the choice of counterfactual: the RD sample compares tracts near the eligibility threshold to one another, while the QCT sample compares tracts that received awards to eligible tracts that did not.

Figure 4 presents event-study estimates for workplace-area jobs. In Panel A, the RD sample shows an average post-treatment effect of approximately zero. While point estimates fluctuate around zero throughout the post-period, the wide confidence intervals cannot rule out modest positive or negative effects. In Panel B, the 2003 QCT sample shows an average post-treatment effect of -1.1 log points with a baseline mean of 7.26 log points, translating to approximately 15 fewer jobs located in the tract, though the confidence intervals similarly cannot rule out modest positive job gains.

The divergence between workplace and resident jobs suggests that HUBZone contract receipt may not generate net job growth in recipient tracts, but rather reallocates existing job opportunities toward tract residents. This is consistent with firms adjusting their hiring to meet the 35 percent resident employment requirement without expanding total operations.

Table 4 examines whether receiving a HUBZone contract generates industry-specific job effects in workplace-area jobs. If contracts lead recipient firms to expand operations, job gains should concentrate in the industries of receiving firms. The table presents results for industries accounting for the largest share of HUBZone contracts: Construction, Manufacturing, Professional Services, Administrative Services, Agriculture, and Wholesale Trade.

Column 1 in each panel reproduces the aggregate workplace-area job effect for comparison. Panel A presents RD estimates for tracts near the eligibility thresholds, while Panel B presents estimates for the full 2003 QCT sample.

In Panel A, the RD sample shows a statistically significant negative effect in manufacturing of -12.3 log points. construction shows a positive, marginally significant point estimate of 7.2 log points. Other industries show mixed signs with no significant effects. In Panel B, the 2003 QCT sample shows a statistically significant positive effect in construction of 0.113 log points. This is notable given that construction accounts for the largest share of HUBZone contracts. Other industries show mixed signs but no significant effects at conventional levels.

Table 5 examines whether the resident job gains documented in Figure 3 concentrate in specific industries. If residents are being hired by HUBZone recipient firms, job gains should appear in the industries of those firms. Alternatively, if gains reflect broader labor market improvements or spillovers, effects may be distributed across industries. Column 1 reproduces the aggregate resident-area job effect for comparison.

In Panel A, the RD sample shows a statistically significant negative effect in construction of -5.9 log points, directly opposite to the positive effect of workplace construction in Table 4. Other industries show negative but insignificant point estimates. In Panel B, the 2003 QCT sample shows a statistically significant positive effect in professional services of 3.6 log points. Construction shows a small negative but insignificant effect of -1.0 log points. Other industries show mixed signs but no significant effects.

The industry-level results present a complex pattern. In workplace jobs, construction shows a significant positive effect in the QCT sample, while manufacturing shows a significant negative effect in the RD sample . In resident jobs, construction shows a significant negative effect in the RD sample, while professional services shows a significant positive effect in the QCT sample. The lack of consistent patterns across samples and job measures, combined with null aggregate workplace effects, suggests that any industry-specific impacts are modest and sample-dependent rather than reflecting systematic job creation or reallocation within specific sectors.

Table 6 examines whether receiving a HUBZone contract affects workplace jobs in establishments of different ages. This analysis tests whether effects concentrate in younger establishments, which is related to early discussions about the program boosting capital formation and younger firms being more likely to apply for HUBZone contracts. Data on establishment age are available only from 2011 onward, limiting the analysis to contracts

received between 2012 and 2015. This restriction means we observe only a subset of contract recipients, potentially limiting statistical power.

Panel A presents RD estimates for tracts near the eligibility thresholds. Point estimates are positive across all age categories but imprecise, ranging from 0.011 log points for establishments less than 2 years old to 0.477 log points for establishments 4-5 years old. None of the estimates approach statistical significance, with standard errors ranging from 0.068 to 0.213. Panel B presents estimates for the 2003 QCT sample, showing a similar pattern. Point estimates range from -0.069 log points for establishments 2-3 years old to 0.205 log points for establishments 6-10 years old, with none statistically significant.

The absence of detectable job effects across establishment age categories provides no evidence that HUBZone contracts generate workplace job gains in either young or mature establishments. While point estimates tend to be larger for middle-aged establishments (4-10 years), the confidence intervals are too wide to draw meaningful conclusions. The limited time window (2012-2015) and smaller sample sizes relative to the main job analysis likely contribute to imprecision, though the consistently null results across age bins align with the aggregate workplace job findings in Figure 4.

Figure 5 presents event-study estimates of the effects of HUBZone awards on establishment-level employment using YourEconomy Time Series data matched to contracts identified in the FPDS. Across all three specifications, the estimated effects are economically negligible. Model (1), which includes MSA-specific linear time trends, yields an ATT of zero. Models (2) and (3), which progressively add four-digit NAICS-by-year and firm size-by-year fixed effects, produce similarly small estimates. The most saturated specification (Model 3) produces an ATT of 0.0001 log points, corresponding to a 0.01 percent change relative to a baseline mean of 2.57 log jobs. Pre-trend tests show some evidence of differential trends in Model (1), but this concern dissipates when controlling for industry-year and firm size-year shocks in Models (2) and (3). Despite improved pre-trend balance, all specifications point to the same conclusion that HUBZone awards have no detectable effect on establishment-level jobs.

These null findings reflect the nature of employment reporting in the YourEconomy database. Establishments report employment changes infrequently and in lumpy adjustments rather than continuously. When YourEconomy does not receive updated information and models employment based on prior reports, the reported employment levels may not capture gradual changes occurring at the establishment. The combination of null effects in

establishment-level data and modest positive effects in tract-level residential jobs suggests that any job creation occurs primarily through indirect channels, such as spillovers to nearby non-recipient establishments or improved job retention among existing workers, rather than through direct expansion at award-winning establishments themselves.

5.3 Housing

Place-based policies often generate housing market consequences as residents and investors internalize program benefits into property values and housing demand. To examine whether HUBZone awards affect local housing markets, I analyze mortgage origination activity using data from the Home Mortgage Disclosure Act (HMDA). These data capture both purchase and refinance mortgages, providing a measure of housing market activity that reflects both new homebuying and existing homeowners' access to credit.

Figure 6 presents event-study estimates of the effects pf HUBZone awards on mortgage originations within census tracts. Panel (a) shows results for the RD sample of tracts near the eligibility threshold, while panel (b) examines all tracts that obtained QCT status in 2003. The two specifications reveal divergent patterns in housing market responses. The RD estimate yields an average treatment effect of 2.4 log points, which is not statistically significant at conventional levels. In contrast, the 2003 QCT sample produces a larger and statistically significant effect of 13.3 log points, corresponding to approximately a 14 percent increase in mortgage activity. Pre-trend tests fail to reject parallel trends in both samples, supporting the identification strategy and suggesting that differential pre-treatment dynamics do not drive the results.

The divergence in the magnitudes of the effects across specifications reflects differences in sample composition and tract characteristics. The stronger results in the QCT sample may reflect the fact that these persistently disadvantaged tracts experience larger changes in mortgage activity, though the RD sample's null result suggests that effects are not uniform across all eligible areas. The post-treatment trajectory in panel (b) shows a sustained upward trend through year 10, with effects building gradually over time. The pattern of gradual growth is consistent with the prediction that housing market effects should emerge when the program generates local jobs or income gains that increase housing demand and also aligns with the results in Figure 3.

Beyond the volume of mortgage activity, loan amounts provide additional insight into housing market dynamics by reflecting property values, borrower characteristics, and lender

assessments of neighborhood quality. Figure 7 examines whether HUBZone awards affect the average amount per mortgage origination, relative to a baseline mean of approximately $86,000 per origination (11.36 log amount per origination). The RD sample yields an average treatment effect of 4.9 log points, corresponding to approximately a 5 percent increase in the average loan amount. The 2003 QCT sample produces a similar effect of 7.2 log points, or approximately a 7.5 percent increase. Pre-trend tests support parallel trends in the RD sample but show some evidence of differential pre-treatment trends in the QCT sample, though evidence of a systematic trend is less visible.

The increases in loan amounts complement the growth in origination volume documented in Figure 6, indicating that HUBZone awards affect housing markets along both extensive and intensive margins. As discussed in the theoretical framework, housing market effects should emerge when the program generates local jobs or income gains that increase housing demand. The positive effects on both origination volume and loan amounts are consistent with this prediction and align with the modest job gains documented in Figure 3. The similarity in effect sizes between the RD and QCT samples contrasts with the origination results, where the QCT sample showed substantially larger volume effects. This pattern suggests that while persistently disadvantaged tracts experience greater increases in mortgage activity, the property values and borrower characteristics that determine loan amounts respond more uniformly across different tract contexts.

6 Are Job Gains Related to the Size of the Award?

The preceding analysis establishes that receiving a HUBZone award generates job gains in recipient census tracts. A natural follow-up question is whether the program's job returns scale with the volume of procurement directed to a tract. The answer bears directly on how the program should allocate a fixed budget. If returns scale with award size, concentration is efficient; if they do not, the operative policy margin is the number of tracts reached rather than the dollars each receives.

To examine this, I estimate tract-specific treatment effects and relate them to the total dollar value of awards received in each tract. For brevity, I focus on the QCT 2003 sample, which includes all census tracts that obtained QCT status in 2003, because evidence of receipt effects is stronger in that sample. For the establishment-level analysis, I use YourEconomy Time Series data for the set of matched firms that received awards from 2003 to 2015.

6.0.1 Estimating Tract-Level Treatment Effects

For each tract in which a firm received a HUBZone award, I construct a separate dataset containing that tract and a comparison group of not-yet-treated tracts. I then estimate the following specification:

$$y_{ijts} = \alpha_{is} + \delta_{ts} + \lambda_{js}t + \tau_{1s}HZ_{its} + \varepsilon_{ijts} \tag{6}$$

where y_{ijts} represents jobs per capita (resident-area or workplace-area) for tract i in metro area j in year t in dataset s. The subscript s indexes the dataset constructed for each treated tract. Each dataset includes dataset-specific tract fixed effects α_{is}, year fixed effects δ_{ts}, and metro-area-specific linear time trends $\lambda_{js}t$. Within each dataset s, only one tract receives a HUBZone award, indicated by HZ_{its}.

The coefficient τ_{1s} captures the causal effect of award receipt for tract s. This event-specific approach, where I create a separate dataset for each treated unit, ensures that each tract's treatment effect is estimated relative to a clean set of not-yet-treated controls, avoiding contamination from other treated units (Cengiz et al. 2019; Deshpande and Li 2019). Importantly, this yields a distribution of tract-specific treatment effects $\hat{\tau}_{1s}$, which I can then relate to tract-specific award size.

6.0.2 Relating Treatment Effects to Award Size

In the second step, I estimate the relationship between tract-level jobs effects and the total dollar value of HUBZone obligations received:

$$\hat{\tau}_{1s} \times W_s = \alpha_0 + \beta \cdot Obligation_s + \eta_s \tag{7}$$

where $\hat{\tau}_{1s}$ is the estimated jobs effect for tract s from the first stage, and $Obligation_s$ is the cumulative dollar value of HUBZone contract obligations received by firms in tract s measured in hundreds of thousands of dollars. The dependent variable is scaled by W_s, the length of the post-treatment window (in years) for stack s. This scaling converts the annual jobs effect into total job-years per capita, making the coefficient β interpretable as job-years per capita created per \$100,000 of HUBZone spending. If β is positive and economically meaningful, it suggests the program efficiently converts procurement dollars

into local jobs. If β is near zero or imprecisely estimated, it would indicate that award size does not systematically predict jobs gains, potentially pointing to fixed costs, diminishing returns, or heterogeneity in how firms respond to awards. I weight each observation by the inverse of the variance of $\hat{\tau}_{1s}$. This weighting ensures that tract-level estimates with greater statistical certainty receive more influence in the second-stage regression, improving efficiency.

A couple of features of equation 7 shape what β can be asked to do, and stating them here clarifies the results that follow. First, β describes variation *within* the treated sample. Every tract in the second stage received an award; the coefficient traces how realized effects covary with award intensity across recipients, holding receipt fixed. It is therefore a dose-response relationship conditional on treatment, and it complements rather than substitutes for the extensive-margin estimates in Section 5, which identify the effect of receipt itself.

Second, obligation amounts are not randomly assigned. Award size reflects firm capacity, industry, contract vehicle, and the depth of a firm's existing relationships with contracting agencies. A tract whose firms win large awards may differ from a tract whose firms win small ones in ways equation 6 do not absorb. β should accordingly be read as a descriptive gradient that is informative about where in the award distribution job gains concentrate, not as the causal return to an exogenously larger contract.

6.1 Relationship Between Obligations and Jobs Results

Figure 8 examines whether larger HUBZone awards generate proportionally larger jobs effects at the tract level. The analysis includes 455 treated tracts, grouped into 15 bins based on obligation amounts. Panel (a) presents results for resident-area jobs. The precision-weighted linear fit yields a negative slope of -0.16, indicating that each additional \$100,000 of HUBZone obligations is associated with roughly 1.6 fewer job-years gained per 100 tract residents. Panel (b) shows that the corresponding slope for workplace-area jobs is approximately zero and imprecisely estimated. Taken together, the two panels say that the job gains documented in Section 5 do not scale with the dollar value of the awards that produced them. The pattern suggests that what matters for tract jobs is that an establishment wins a HUBZone award but not necessarily the size of that award.

The binned scatter plots reveal considerable variation in job outcomes across the obligation distribution, with substantial dispersion around the fitted line. To assess whether the relationship is non-linear, I test for higher-order polynomial terms in the second-stage

regression, but find no evidence of statistically significant curvature. The linear specification appears to adequately characterize the relationship between award size and job effects. The tract-level estimates capture the combined impact of direct jobs at recipient firms and indirect effects such as spillovers to nearby establishments.

Figure 9 examines the relationship between award size and employment effects at the establishment level using YourEconomy Time Series data. Panel (a) presents a binned scatter plot showing job-years gained as a function of total HUBZone obligations received. The precision-weighted linear fit yields a slope of 0.19, with a standard error of 1.64. This coefficient suggests that an additional $100,000 of HUBZone contracts generates approximately 0.19 job-years of jobs at the receiving establishment, though the standard error admits a wide range of values.

Panel (b) splits the sample at the median obligation amount to examine whether the relationship differs by awards above and below the median award. For establishments receiving below-median awards (yellow points), the estimated slope is -11.85. Comparatively, establishments receiving above-median awards (purple points) show a negative slope of -8.41. The similarity between the slopes suggests no differences by award size at the establishment level, roughly echoing the finding in panel (a) and consistent with job gains occuring outside of the receiving establishment.

The establishment-level results are, in short, uninformative about scaling on their own. This is at least partly because the YTS sample is small relative to the dispersion in establishment-level job responses, and the data's infrequent and lumpy reporting introduces measurement error that attenuates estimated effects toward zero. This is a limitation of the establishment data rather than a finding about the program. The establishment results do not give any indication that award size drives large direct hiring responses at recipient firms, which is the pattern one would need to observe to attribute the tract-level gains primarily to expansion at the award-winning establishment.

That divergence between levels of aggregation is itself informative. The tract-level estimates show positive jobs effects from receipt with no or a negative gradient in award size; the establishment-level estimates show no detectable relationship between obligations and hiring at the recipient firm. Read together, an interpretation is that HUBZone awards operate through channels broader than direct expansion at the winning establishment. This is the multi-channel account discussed in Section 2, and the tract-level estimates, which capture all of these channels jointly, are the appropriate summary measure of the program's

local jobs impact.

Figure 10, panel (a), examines whether larger HUBZone awards generate proportionally larger effects on mortgage origination activity using a binned scatter plot. The precision-weighted linear fit reveals a statistically significant negative relationship of 0.8 fewer mortgage originations per 100 residents. However, visual inspection of the binned points reveals substantial variation around this fitted line. The bins at lower obligation levels show effects clustered near zero, while higher obligation bins display considerable dispersion in both positive and negative directions. This pattern suggests the precision weighting, which down-weights less precisely estimated effects, plays an important role in generating the negative slope. Effects at higher obligation levels appear to have been estimated with greater first-stage uncertainty, reducing their influence on the fitted relationship.

Panel (b) examines the relationship between award size and the average amount per mortgage origination. The binned scatter plot shows a mostly flat relationship between obligation amounts and changes in loan sizes. This pattern contrasts with the origination volume results in panel (a), suggesting that HUBZone awards affect housing markets along primarily the extensive margin.

The negative relationship between award size and mortgage originations mirrors the pattern observed for resident-area job outcomes. This parallel across labor and housing markets suggests that larger awards generate weaker local economic spillovers per dollar spent, which may be driven by linkages between winning firms and their suppliers. In other words, the supply relationships of larger firms may extend farther than HUBZone boundaries, so the procurement funds may never flow into the intended area. The substantial dispersion across bins indicates that local conditions and tract characteristics play an important role in mediating program effects beyond award size alone.

7 Conclusion

This paper examines the effects of the HUBZone program on local jobs and housing market outcomes using regression discontinuity and difference-in-differences designs. I find that receiving a HUBZone award generates modest positive employment effects for census tract residents, with average gains of approximately 3.5 percent in resident-area employment relative to census tracts where firms were eligible for HUBZone contracts but received none. Establishment-level data from YourEconomy Time Series show no detectable employment changes at award-winning firms, suggesting that job creation operates through spillovers to

nearby businesses or improved job retention among existing workers rather than through hiring at recipient firms themselves. The program also generates positive effects on housing market activity, with increases in both mortgage origination volume and loan amounts, indicating that the employment gains translate into gains in neighborhood housing demand.

Analysis by size of the HUBZone award suggests larger awards do not generate proportionally larger employment effects per dollar spent. The lack of relationship between obligation amounts and job-years gained per capita suggests that receiving an award has a greater jobs effect than the amount received. Mortgage origination activity follows a similar pattern, with larger awards also not associated with increases in mortgage origination. This pattern across both labor and housing markets suggests larger contracts may involve non-local supplier linkages or other arrangements reducing economic spillovers, while smaller awards may be more closely tied to community-level economic activity.

These findings contribute to the broader literature on place-based policies in several ways. First, the modest job effects align with recent evidence from Opportunity Zones and Empowerment Zones, which similarly find limited job creation from place-based tax incentives (Busso, Gregory, and Kline 2013; Arefeva et al. 2025). The HUBZone program's focus on procurement contracts rather than tax subsidies does not appear to generate substantially different labor market outcomes. Second, the null establishment-level effects combined with positive tract-level effects suggest that indirect mechanisms play a more important role than direct job expansion, consistent with theories emphasizing agglomeration economies and local multipliers (Moretti 2010). Third, the positive housing market effects indicate that even modest employment gains translate into measurable housing market responses, though the diminishing returns across both labor and housing markets suggest constraints on local economic spillovers from larger interventions.

The findings also inform the policy debate about complementarities and substitutability among place-based programs. HUBZone designations overlap substantially with Opportunity Zones, Empowerment Zones, and Low-Income Housing Tax Credit allocations. My analysis controls for LIHTC spending and finds that HUBZone effects persist, suggesting that procurement-based policies may complement rather than substitute for housing subsidies. However, the modest effect sizes raise questions about whether procurement set-asides represent the most efficient mechanism for revitalizing distressed areas. The diminishing returns to award size indicate that concentrating large contracts in a few areas may be less effective than distributing smaller awards more broadly across eligible tracts.

Several important questions remain for future research. First, the divergence between establishment-level and tract-level job effects warrants further investigation into specific spillover channels. Do awards increase demand for local suppliers, or do they primarily retain workers who would otherwise leave the area? Second, the heterogeneity in how tracts respond to awards suggests that local conditions, firm characteristics, and industry composition mediate program effectiveness. Understanding which contexts generate the largest returns would inform more targeted program design. Third, the long-run persistence of job and housing effects remains unclear, as my analysis captures effects within twelve years of initial award receipt. Finally, the welfare implications depend critically on program costs, including administrative burden and potential efficiency losses from restricting contract competition. A comprehensive cost-benefit analysis would help policymakers assess whether the HUBZone program represents an effective tool for economic development relative to alternative policies.

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Table 1: Descriptive Statistics of Census Tracts 2000-2019

Statistic	Mean	St. Dev.	Median	Min	Max	N
Resident Area Jobs	1,589.47	820.00	1,474.00	1.00	24,030.00	528,924
Worker Area Jobs	1,636.81	2,929.36	801.00	1.00	118,961.00	528,924
Originations	138.54	138.05	102	0	4,091	583,365
Originations Amount (Millions)	23.11	31.41	13.40	0.00	1,363.08	583,365
Amount Per Origination (Thousands)	148.44	101.99	128.20	0.00	15,517.82	586,288
Obligation (Millions)	0.31	7.48	0.00	0.00	723.59	586,288
Median Income	44,865.52	20,525.18	41,297.17	2,499.00	250,001.00	565,600
Poverty Rate	0.14	0.12	0.10	−0.00	1.00	565,600
LIHTC	0.02	0.13	0	0	1	586,288
QCT 2003	0.14	0.34	0	0	1	586,288
HZ Receipt	0.04	0.20	0	0	1	586,288

Notes: Table reports summary statistics for census tracts from 2000-2019. Resident Area Jobs and Worker Area Jobs are from LODES via the Urban Institute (available from 2002 onward). Housing finance variables are from the HMDA Longitudinal Dataset via the National Neighborhood Data Archive. Tract characteristics are from IPUMS.

Table 2: General Business Characteristics by Treatment Group

Characteristic	Treated - Receipt N = 577[1]
Number of Jobs	36.1 (256.4) | 7.0
Last Year Jobs	38.4 (190.4) | 11.0
NAICS 2-Digit	
Construction	183 (31.7%)
Health Care and Social Assistance	3 (0.5%)
Manufacturing	109 (18.9%)
Other	148 (25.6%)
Other Services (except Public Administration)	12 (2.1%)
Professional, Scientific, and Technical Services	122 (21.1%)

[1] Mean (SD) | Median; n (%)

Table 3: Impact of Obtaining QCT Status in 2003 on Jobs and Housing Near Eligibility Threshold

(a) Unit and Time Fixed Effects

	Resident-Area Jobs	Workplace-Area Jobs	Originations	Amount per Origination
	Jobs		**Housing**	
	(1)	(2)	(3)	(4)
QCT 2003	-0.042	-0.018	-0.087	0.0001
	(0.005)	(0.012)	(0.018)	(0.008)
QCT 2003 $\times$ e^{income}	-0.067	0.021	0.015	-0.021
	(0.043)	(0.082)	(0.113)	(0.048)
QCT 2003 $\times$ $e^{poverty}$	0.125	0.191	0.431	0.084
	(0.034)	(0.073)	(0.083)	(0.044)
Baseline Mean	6.92	6.56	3.37	11.3
Observations	82,876	82,876	91,272	91,272

(b) MSA-Specific Linear Trends

	Resident-Area Jobs	Workplace-Area Jobs	Originations	Amount per Origination
	Jobs		**Housing**	
	(1)	(2)	(3)	(4)
QCT 2003	-0.037	-0.021	-0.077	-0.005
	(0.006)	(0.014)	(0.016)	(0.007)
QCT 2003 $\times$ e^{income}	-0.028	0.042	0.155	-0.068
	(0.042)	(0.084)	(0.108)	(0.043)
QCT 2003 $\times$ $e^{poverty}$	0.042	0.130	0.222	0.038
	(0.035)	(0.075)	(0.075)	(0.037)
Baseline Mean	6.92	6.56	3.37	11.3
Observations	82,876	82,876	91,272	91,272

Notes: Table reports estimates of the effect of obtaining Qualified Census Tract (QCT) status in 2003 on job and housing finance outcomes for tracts near the eligibility threshold. Panel A presents results from a two-way fixed effects specification with tract and year fixed effects. Panel B adds MSA-specific linear time trends to account for differential metropolitan growth trajectories. Columns (1) and (2) report effects on resident-area jobs (jobs held by census-tract residents) and workplace-area jobs (jobs located in the census tract), respectively. Columns (3) and (4) report effects on mortgage originations and the average amount per origination. All regressions include interactions between QCT status and baseline tract income and poverty rates. Baseline mean refers to the mean of the dependent variable in 2002 (the year before QCT designation). All specifications include controls for LIHTC allocations. Standard errors in parentheses are clustered at the tract level. Job estimates include only one year of pre-treatment data (2002) and should be interpreted with caution.

Table 4: Effect of HUBZone Receipt on Workplace-Area Jobs by Industry

Panel A: RD Estimates

	Workplace-Area Jobs (1)	Construction (2)	Manufacturing (3)	Professional (4)	Administrative (5)	Agriculture (6)	Wholesale (7)
HZ Receipt	-0.002	0.072	-0.123	0.088	0.057	-0.075	-0.026
	(0.023)	(0.053)	(0.056)	(0.052)	(0.058)	(0.119)	(0.044)
HZ Receipt $\times$ e^{income}	0.108	0.242	0.493	-0.140	0.458	-0.363	0.217
	(0.139)	(0.300)	(0.339)	(0.269)	(0.371)	(0.640)	(0.295)
HZ Receipt $\times$ $e^{poverty}$	-0.024	-0.312	-0.032	-0.079	-0.295	-0.061	0.188
	(0.096)	(0.211)	(0.214)	(0.192)	(0.211)	(0.430)	(0.164)
Baseline Mean	7.25	4.09	4.53	4.04	4.1	1.84	3.97
Observations	83,352	73,917	62,593	73,485	74,552	14,205	70,476

Panel B: Receipt Estimates

	Workplace-Area Jobs (1)	Construction (2)	Manufacturing (3)	Professional (4)	Administrative (5)	Agriculture (6)	Wholesale (7)
HZ Receipt	-0.008	0.113	-0.089	0.059	0.037	0.054	-0.059
	(0.019)	(0.037)	(0.046)	(0.037)	(0.051)	(0.098)	(0.036)
Baseline Mean	7.26	4.09	4.52	3.97	4.12	1.89	4.03
Observations	70,188	57,235	50,987	57,144	59,101	11,632	55,923

Note: Table reports estimates of the effect of HUBZone award receipt on jobs by industry in the workplace area (the census tract where jobs are located). Panel A presents RD estimates for tracts near the eligibility threshold, including interactions with baseline tract income and poverty rates. Panel B presents estimates using all tracts that obtained Qualified Census Tract status in 2003. Column (1) reports the effect on total workplace-area jobs; columns (2)-(7) report results for specific industries: Construction; Manufacturing; Professional, Scientific, and Technical Services; Administrative and Support Services; Agriculture, Forestry, Fishing, and Hunting; and Wholesale Trade. Baseline mean refers to the mean of the dependent variable in the years before receipt. All specifications include controls for LIHTC allocations, tract fixed effects, year fixed effects, and MSA-specific linear time trends. Panel A additionally includes controls for QCT status. Standard errors in parentheses are clustered at the tract level.

Table 5: Effect of HUBZone Receipt on Resident-Area Jobs by Industry

Panel A: RD Estimates

	Resident-Area Jobs (1)	Construction (2)	Manufacturing (3)	Professional (4)	Administrative (5)	Agriculture (6)	Wholesale (7)
HZ Receipt	0.008	-0.059	-0.042	-0.025	-0.012	-0.045	-0.017
	(0.012)	(0.019)	(0.016)	(0.021)	(0.014)	(0.048)	(0.017)
HZ Receipt × e^{income}	-0.098	-0.157	0.008	-0.123	0.036	0.116	-0.108
	(0.099)	(0.154)	(0.121)	(0.151)	(0.103)	(0.215)	(0.133)
HZ Receipt × $e^{poverty}$	0.120	0.171	0.010	0.278	0.068	0.227	0.150
	(0.051)	(0.074)	(0.068)	(0.089)	(0.060)	(0.193)	(0.066)
Baseline Mean	7.05	4.12	4.56	4.18	4.62	1.36	3.9
Observations	83,352	83,333	83,324	83,323	83,336	70,105	83,315

Panel B: Receipt Estimates

	Resident-Area Jobs (1)	Construction (2)	Manufacturing (3)	Professional (4)	Administrative (5)	Agriculture (6)	Wholesale (7)
HZ Receipt	0.036	-0.010	-0.019	0.036	-0.024	-0.042	0.023
	(0.010)	(0.014)	(0.012)	(0.016)	(0.013)	(0.032)	(0.014)
Baseline Mean	6.86	3.85	4.36	3.91	4.45	1.44	3.67
Observations	70,188	70,170	70,172	70,172	70,181	54,604	70,165

Notes: Table reports estimates of the effect of HUBZone award receipt on jobs by industry in the resident area (jobs held by census-tract residents, regardless of work location). Panel A presents RD estimates for tracts near the eligibility threshold, including interactions with baseline tract income and poverty rates. Panel B presents estimates using all tracts that obtained Qualified Census Tract status in 2003. Column (1) reports the effect on total resident-area jobs; columns (2)-(7) report results for specific industries: Construction; Manufacturing; Professional, Scientific, and Technical Services; Administrative and Support Services; Agriculture, Forestry, Fishing, and Hunting; and Wholesale Trade. Baseline mean refers to the mean of the dependent variable in the years before receipt. All specifications include controls for LIHTC allocations, tract fixed effects, year fixed effects, and MSA-specific linear time trends. Panel A additionally includes controls for QCT status. Standard errors in parentheses are clustered at the tract level.

Table 6: Effect of HUBZone Receipt on Workplace-Area Jobs by Age of the Establishment

Panel A: RD Sample

	<2 yrs (1)	2-3 yrs (2)	4-5 yrs (3)	6-10 yrs (4)	>10 yrs (5)
HZ Receipt	0.011	0.181	0.477	0.068	0.123
	(0.171)	(0.155)	(0.204)	(0.213)	(0.068)
HZ Receipt $\times$ e^{income}	0.706	-1.16	-0.499	-0.623	-1.22
	(0.940)	(1.02)	(1.27)	(1.76)	(0.338)
HZ Receipt $\times$ $e^{poverty}$	-0.293	-0.090	-0.917	0.970	-0.220
	(0.603)	(0.438)	(0.634)	(0.602)	(0.345)
Baseline Mean	3.5	3.73	3.69	4.72	6.97
Observations	36,810	36,507	35,754	37,780	38,795

Panel B: 2003 QCT Sample

	<2 yrs (1)	2-3 yrs (2)	4-5 yrs (3)	6-10 yrs (4)	>10 yrs (5)
HZ Receipt	0.063	-0.069	0.110	0.205	0.083
	(0.111)	(0.115)	(0.150)	(0.104)	(0.043)
Baseline Mean	3.43	3.73	3.66	4.62	6.97
Observations	28,418	28,174	27,450	29,564	30,980

Notes: Table reports estimates of the effect of HUBZone award receipt on workplace-area jobs by establishment age. Panel A presents RD estimates for tracts near the eligibility threshold; Panel B presents estimates using all tracts that obtained Qualified Census Tract status in 2003. The dependent variable is the natural log of workplace-area jobs for establishments of different ages within the tract. Baseline mean refers to the mean of the dependent variable in event year -1. All specifications include controls for LIHTC allocations, tract fixed effects, year fixed effects, and MSA-specific linear time trends. Panel A additionally includes controls for QCT status. Standard errors in parentheses are clustered at the tract level.

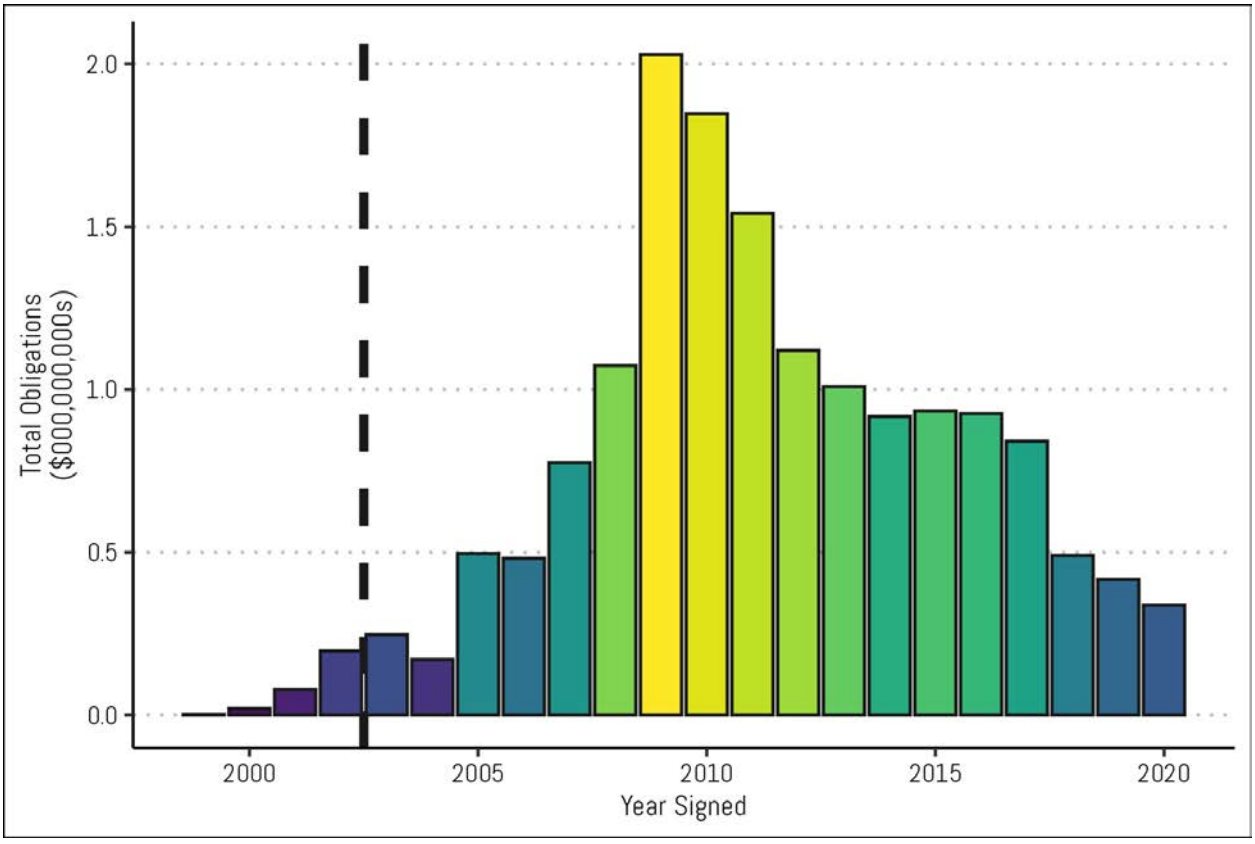
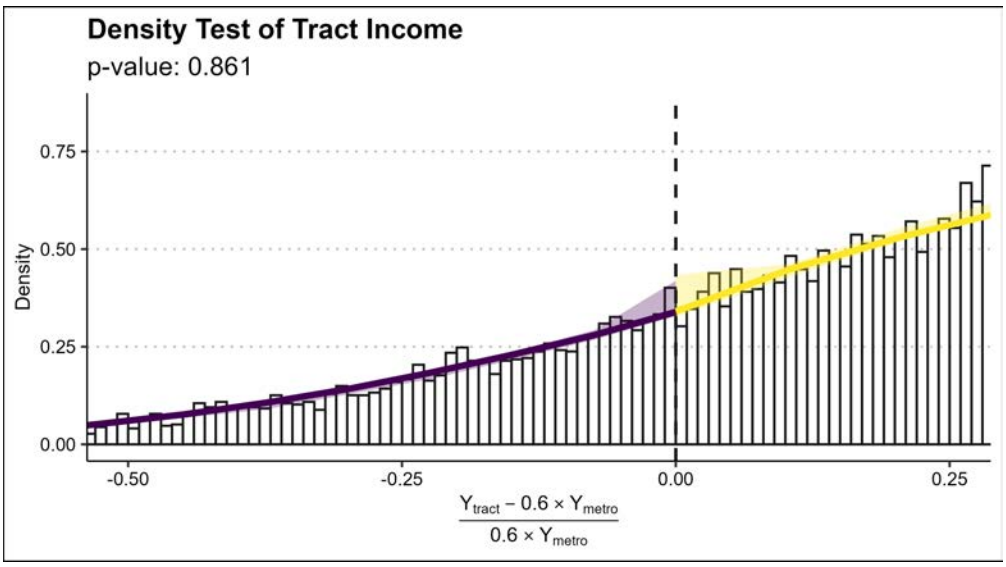
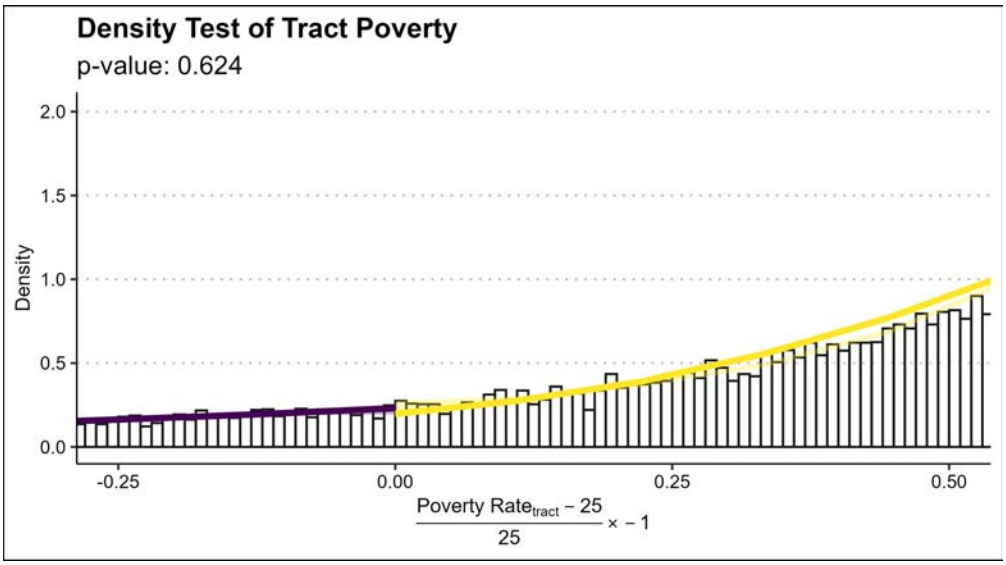


Figure 1: HUBZone Award Obligations by Year Contract Signed

Notes: Figure shows annual HUBZone contract obligations in nominal dollars, aggregated to the year each contract was signed. Obligations include HUBZone set-asides, sole-source contracts, and preference-based awards. Source: Federal Procurement Data System. Author's calculations.



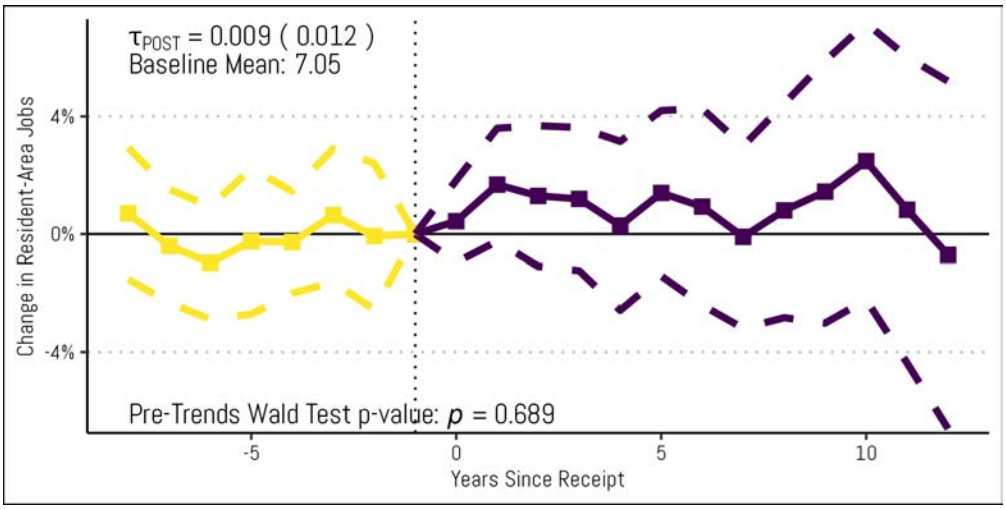
(a) Income



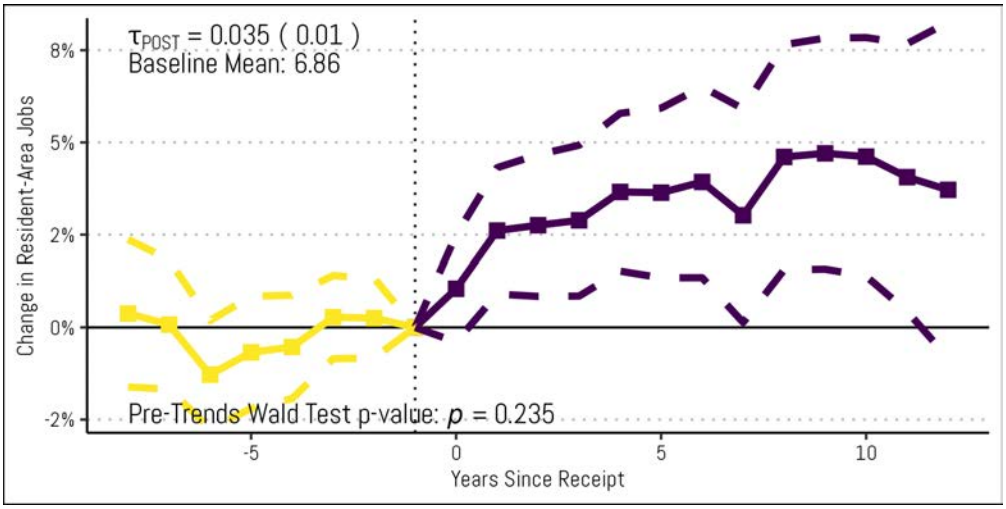
(b) Poverty

Figure 2: Running Variable Density Tests

Notes: Figure displays density tests for discontinuities in the distribution of census tracts at QCT eligibility thresholds. Panel (a) tests for manipulation of the income running variable, defined as the difference between tract median household income and 60 percent of area median income, normalized by area median income. Panel (b) tests for manipulation of the poverty running variable, defined as the difference between the tract poverty rate and 25 percent, normalized by 25. The vertical dashed line indicates the eligibility threshold (zero). Histograms show the empirical distribution of tracts; solid lines show kernel density estimates on either side of the threshold with shaded 95 percent confidence intervals. P-values report tests of the null hypothesis that the density is continuous at the threshold. Failure to reject the null supports the validity of the RD design.



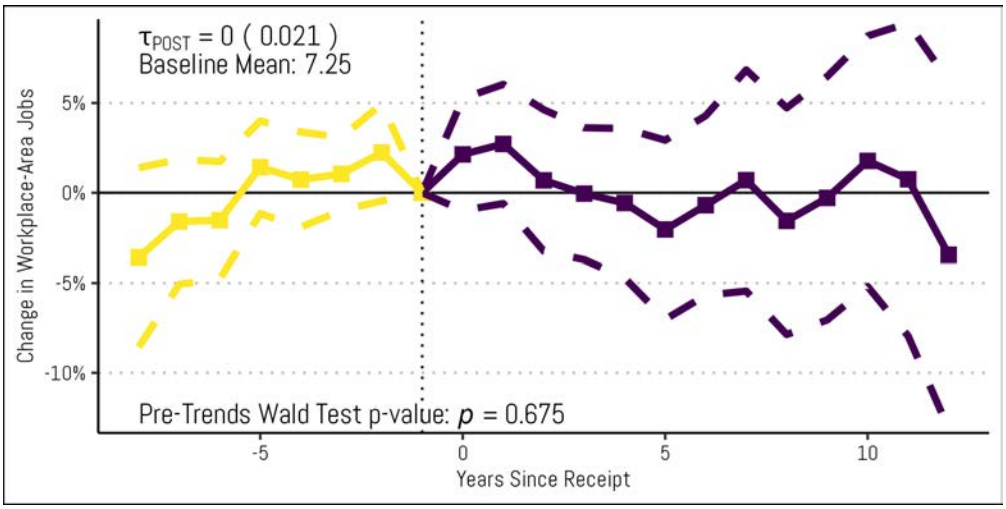
(a) RD Sample



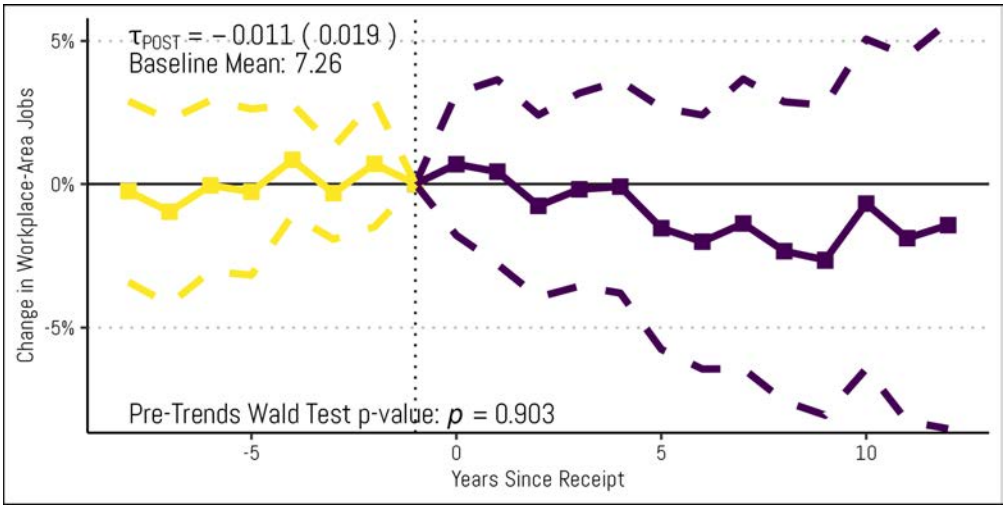
(b) 2003 QCT Sample

Figure 3: Effect of HUBZone Receipt on Resident-Area Jobs

Notes: Figure reports event-study estimates of the effect of HUBZone award receipt on jobs among census-tract residents. Panel (a) restricts the sample to tracts near the eligibility threshold (RD sample); panel (b) uses all tracts that obtained Qualified Census Tract status in 2003. Estimates are relative to event year -1 (the year before first award receipt), which is normalized to zero. Event years -8 and earlier are binned into a single coefficient; event years 12 and later are binned into a single coefficient. All specifications include controls for LIHTC allocations, tract fixed effects, year fixed effects, and MSA-specific linear time trends. The RD sample additionally includes controls for QCT status. Standard errors are clustered at the tract level. Dashed lines indicate 95 percent confidence intervals. Pre-trend p-values report Wald tests of the joint null hypothesis that all pre-event coefficients equal zero. τ_{POST} reports the average treatment effect on the treated group for all post-event periods, with standard error in parentheses. Baseline mean is the average of the treated group's pre-treatment outcomes.



(a) RD Sample



(b) 2003 QCT Sample

Figure 4: Effect of HUBZone Receipt on Workplace-Area Jobs

Notes: Figure reports event-study estimates of the effect of HUBZone award receipt on jobs in the workplace area (the census tract where jobs are located). Panel (a) restricts the sample to tracts near the eligibility threshold (RD sample); panel (b) uses all tracts that obtained Qualified Census Tract status in 2003. Estimates are relative to event year -1 (the year before first award receipt), which is normalized to zero. Event years -8 and earlier are binned into a single coefficient; event years 12 and later are binned into a single coefficient. All specifications include controls for LIHTC allocations, tract fixed effects, year fixed effects, and MSA-specific linear time trends. The RD sample additionally includes controls for QCT status. Standard errors are clustered at the tract level. Dashed lines indicate 95 percent confidence intervals. Pre-trend p-values report Wald tests of the joint null hypothesis that all pre-event coefficients equal zero. τ_{POST} reports the average treatment effect on the treated group for all post-event periods, with standard error in parentheses. Baseline mean is the average of the treated group's pre-treatment outcomes.

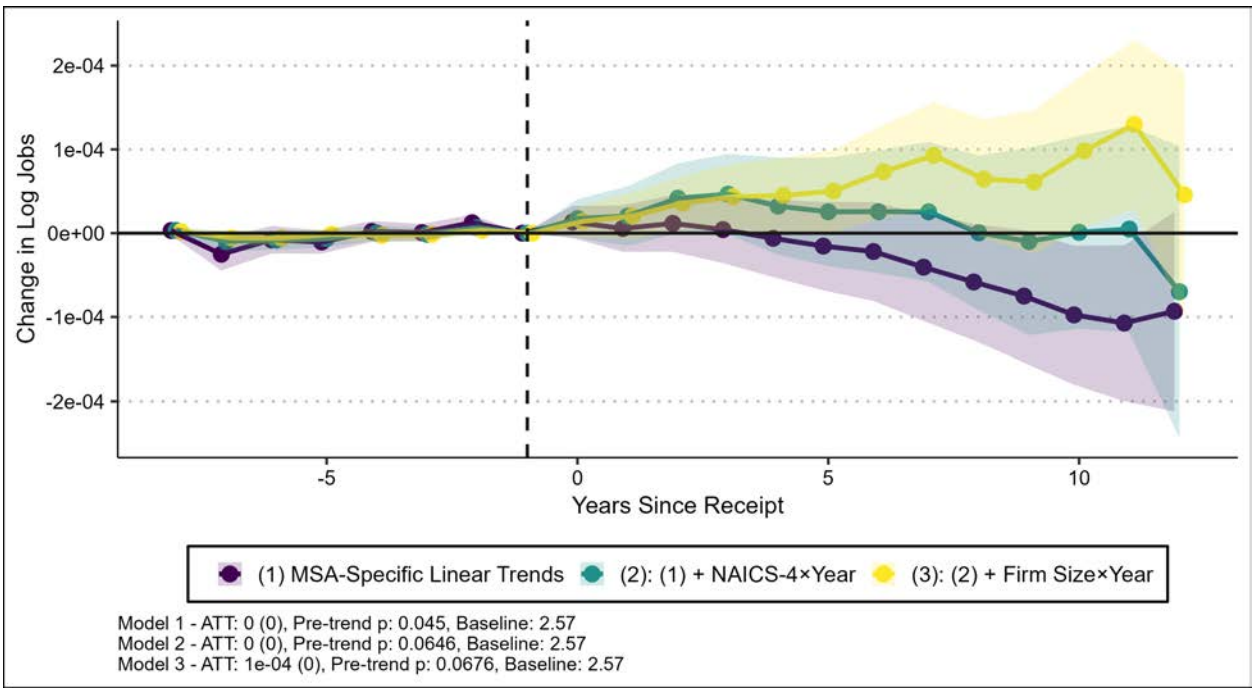
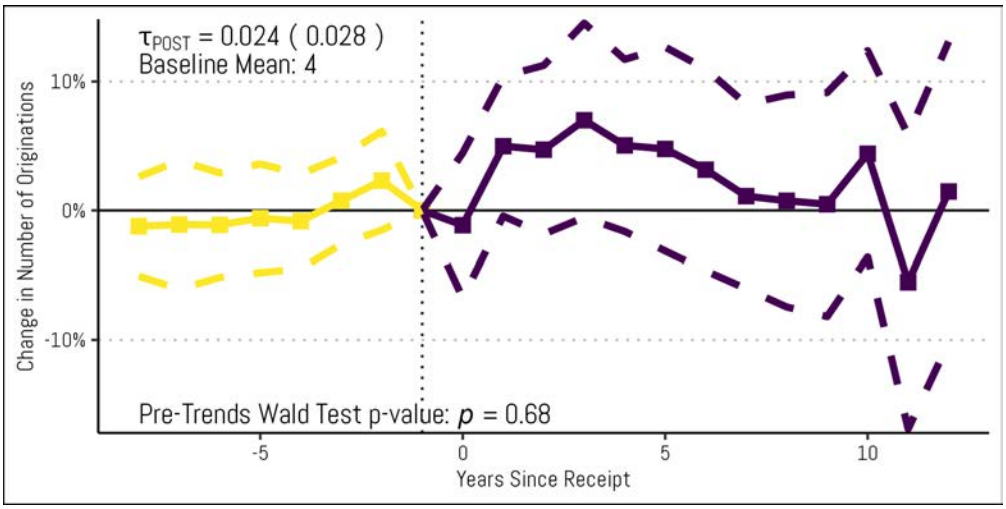
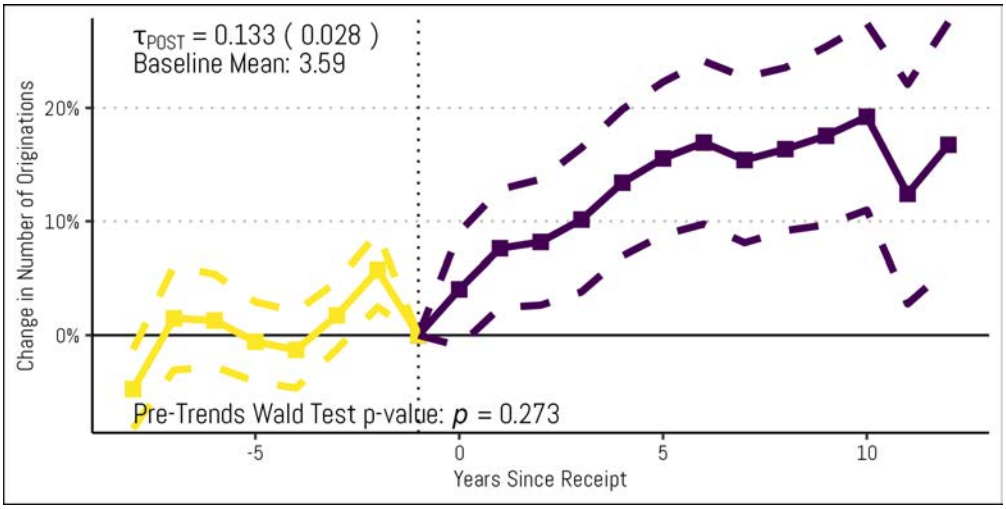


Figure 5: Firm-Specific HUBZone Award Impact on Jobs

Note: Figure reports event-study estimates of the effect of HUBZone award receipt on log jobs using establishment-level data from YourEconomy Time Series. The sample consists of firms that received their first HUBZone award between 2003 and 2015 and were successfully matched to the YourEconomy database using the vendor's unique entity ID. Model (1) includes MSA-specific linear time trends; Model (2) adds four-digit NAICS code-by-year fixed effects; Model (3) additionally includes firm size-by-year fixed effects. Estimates are relative to event year -1 (the year before first award receipt), which is normalized to zero. All specifications include firm fixed effects and year fixed effects. Standard errors are clustered at the establishment level, and shaded regions indicate 95 percent confidence intervals. Pre-trend p-values report tests of the joint null hypothesis that all pre-event coefficients equal zero. ATT reports the average post-event treatment effect with standard error in parentheses. Baseline refers to the mean of the dependent variable in event year -1.



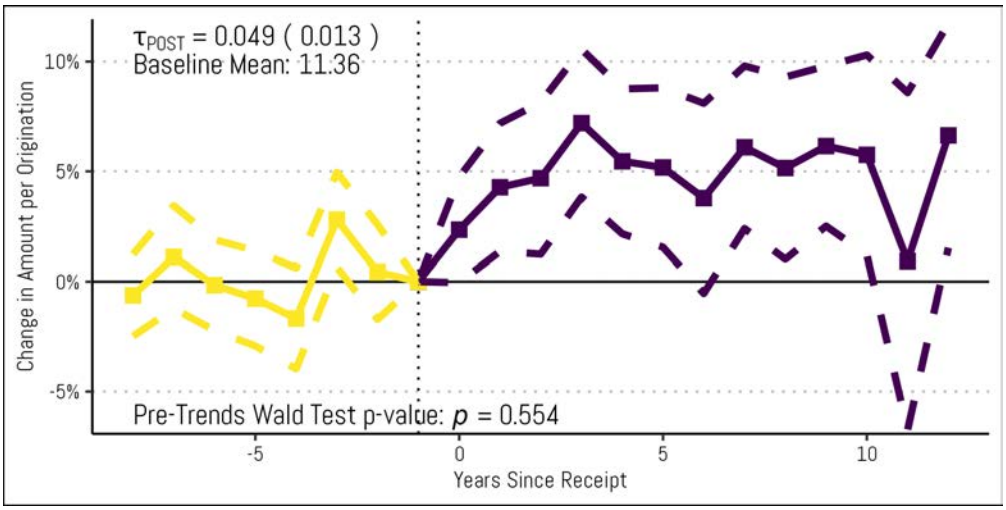
(a) RD Sample



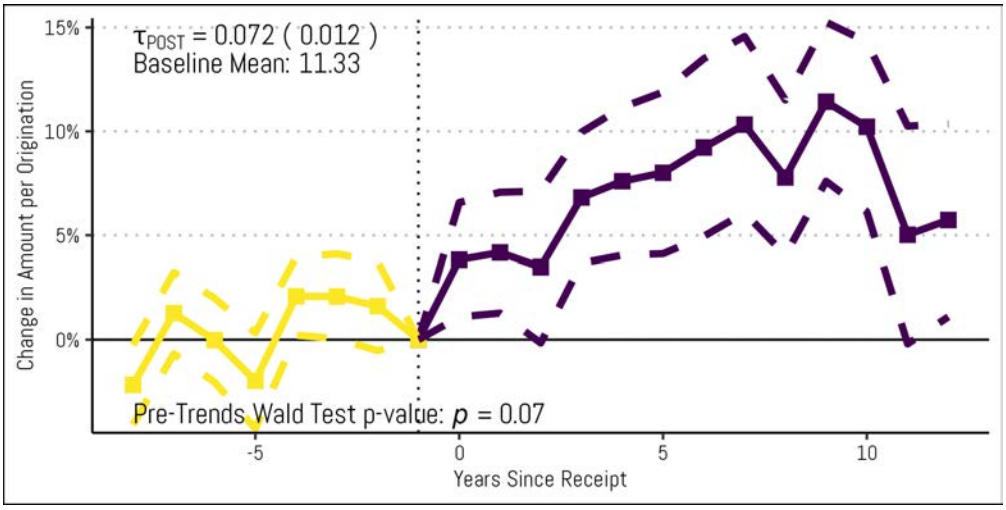
(b) QCT 2003 Sample

Figure 6: Effect of HUBZone Receipt on Number of Mortgage Originations

Notes: Figure reports event-study estimates of the effect of HUBZone award receipt on the number of mortgage originations in census tracts. Panel (a) restricts the sample to tracts near the eligibility threshold (RD sample); panel (b) uses all tracts that obtained Qualified Census Tract status in 2003. Estimates are relative to event year -1 (the year before first award receipt), which is normalized to zero. Event years -8 and earlier are binned into a single coefficient; event years 12 and later are binned into a single coefficient. All specifications include controls for LIHTC allocations, tract fixed effects, year fixed effects, and MSA-specific linear time trends. The RD sample additionally includes controls for QCT status. Standard errors are clustered at the tract level. Dashed lines indicate 95 percent confidence intervals. Pre-trend p-values report Wald tests of the joint null hypothesis that all pre-event coefficients equal zero. τ_{POST} reports the average treatment effect on the treated group for all post-event periods, with standard error in parentheses. Baseline mean is the average of the treated group's pre-treatment outcomes.



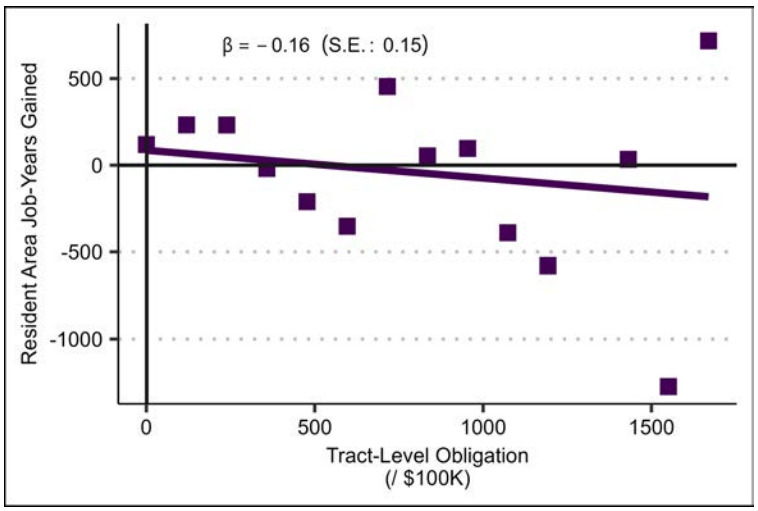
(a) RD Sample



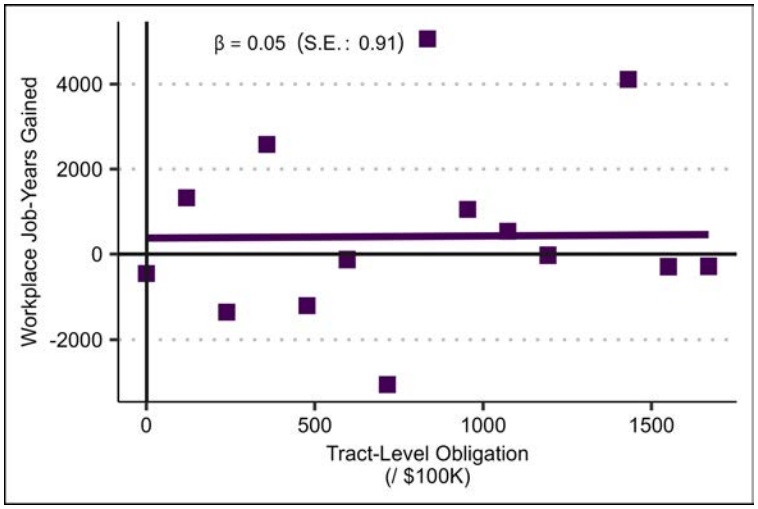
(b) QCT 2003 Sample

Figure 7: Effect of HUBZone Receipt on Amount per Mortgage Origination

Notes: Figure reports event-study estimates of the effect of HUBZone award receipt on the average amount per mortgage origination in census tracts. Panel (a) restricts the sample to tracts near the eligibility threshold (RD sample); panel (b) uses all tracts that obtained Qualified Census Tract status in 2003. Estimates are relative to event year -1 (the year before first award receipt), which is normalized to zero. Event years -8 and earlier are binned into a single coefficient; event years 12 and later are binned into a single coefficient. All specifications include controls for LIHTC allocations, tract fixed effects, year fixed effects, and MSA-specific linear time trends. The RD sample additionally includes controls for QCT status. Standard errors are clustered at the tract level. Dashed lines indicate 95 percent confidence intervals. Pre-trend p-values report Wald tests of the joint null hypothesis that all pre-event coefficients equal zero. τ_{POST} reports the average treatment effect on the treated group for all post-event periods, with standard error in parentheses. Baseline mean is the average of the treated group's pre-treatment outcomes.



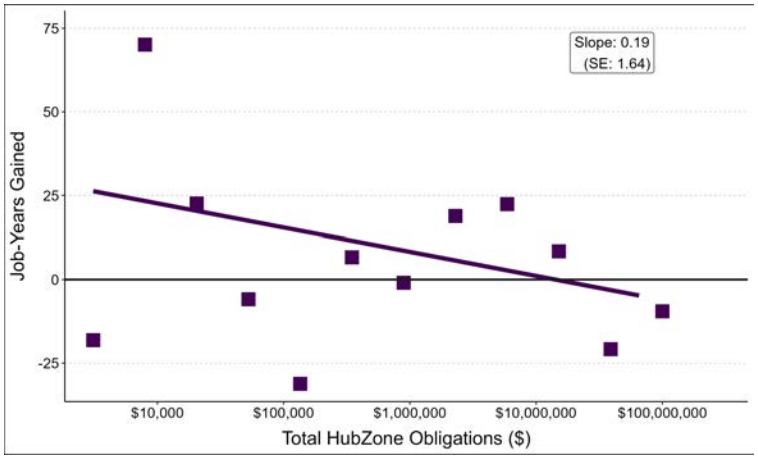
(a) Resident-Area Jobs



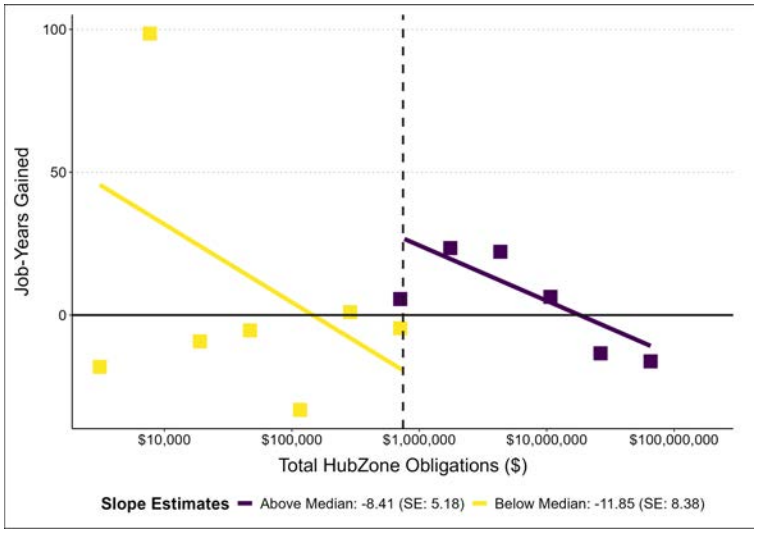
(b) Workplace-Area Jobs

Figure 8: Relationship Between HUBZone Award Amount to Job-Years Gained

Notes: Figure examines the relationship between HUBZone award obligations and tract-level jobs effects using binned scatter plots. Panel (a) shows results for resident-area jobs (jobs held by tract residents); panel (b) shows results for workplace-area jobs (jobs located in the tract). The sample includes 455 treated tracts, binned into 15 groups based on obligation amounts. Each point represents the mean job-years gained and mean tract-level obligations (per capita) within a bin. Tract-specific jobs effects are estimated from the event-specific difference-in-differences specification described in equation 6. The solid line represents a precision-weighted linear fit from equation 7, where weights equal the inverse variance of the tract-specific treatment effect estimates. The y-axis measures job-years gained, calculated as the estimated annual jobs effect multiplied by the length of the post-treatment window for that tract. The x-axis shows tract-level obligations in dollars per capita (tract population), plotted on a logarithmic scale to accommodate the wide range of award sizes. The estimated slope coefficient and standard error are reported in the figure, representing job-years created per dollar of HUBZone spending.



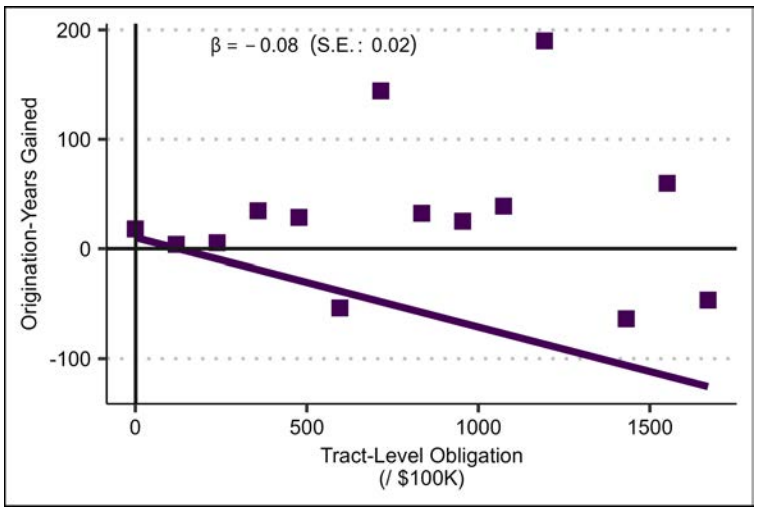
(a) Firm-Level Effects (Binned)



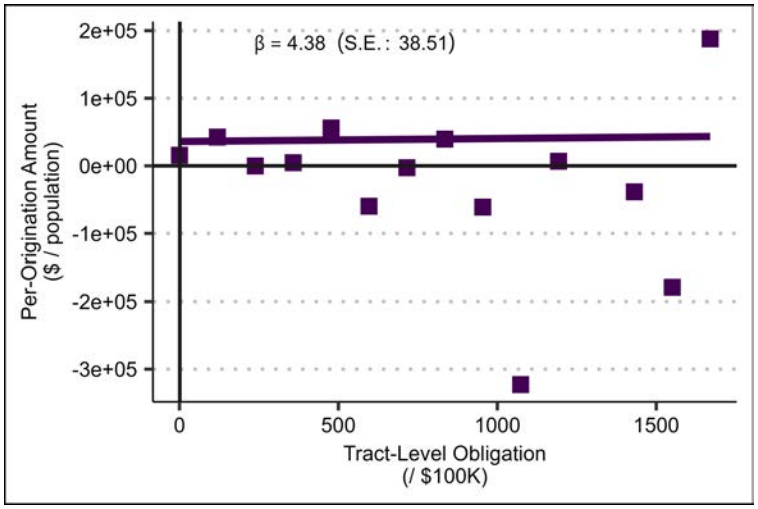
(b) Firm-Level Effects (Split)

Figure 9: Relationship between HUBZone Award Amount to Job-Years Gained at the Establishment

Notes: Figure examines the relationship between HUBZone award obligations and establishment-level jobs effects using YourEconomy Time Series data. Panel (a) presents a binned scatter plot of job-years gained against total HUBZone obligations received, with a precision-weighted linear fit. Establishments are grouped into bins based on obligation amounts, with each point representing the mean job-years gained and mean obligation amount within that bin. The solid line shows the estimated relationship, with slope coefficient and standard error reported in the figure legend. Panel (b) splits the sample at the median obligation amount and estimates separate slopes for below-median (yellow) and above-median (purple) awards. The vertical dashed line indicates the median split point. Within each subsample, establishments are binned and fitted separately. The y-axis measures job-years gained, calculated as the estimated annual jobs effect multiplied by the length of the post-treatment window for that establishment. Obligations represent the cumulative dollar value of all HUBZone contracts received by the establishment. The x-axis uses a logarithmic scale to accommodate the range of award sizes.



(a) Number of Originations



(b) Amount per Origination

Figure 10: Relationship Between HUBZone Award Amount and Housing Market Outcomes

Notes: Figure examines the relationship between HUBZone award obligations and tract-level housing market effects using binned scatter plots. Panel (a) shows results for the number of mortgage originations; panel (b) shows results for the average amount per origination. The sample includes 455 treated tracts, binned into 15 groups based on obligation amounts. Each point represents the mean origination-years gained (panel a) or mean change in per-origination amount (panel b) and mean tract-level obligations (per capita) within a bin. Tract-specific housing effects are estimated from the event-specific difference-in-differences specification described in equation 6. The solid line represents a precision-weighted linear fit from equation 7, where weights equal the inverse variance of the tract-specific treatment effect estimates. The y-axis measures origination-years gained (panel a) or change in per-origination amount (panel b), calculated as the estimated annual effect multiplied by the length of the post-treatment window for that tract. The x-axis shows tract-level obligations in dollars per capita (tract population), plotted on a logarithmic scale to accommodate the wide range of award sizes. The estimated slope coefficient and standard error are reported in the figure.

9 Appendix

Table A.1: Contract Summary Statistics for Numeric and Binary Variables

Variable	Mean (SD) or %
Obligated Amount	312398.54 (1313800.04)
Price Evaluation Percent Difference	0.01 (0.52)
HUBZone Set-Aside	93.4%
HUBZone Sole Source	6.0%
HUBZone Preference 8(a)	0.5%
Multi-Year Contract	12.8%
Has Price Evaluation Difference	0.0%
SBA Certified Small Disadvantaged	22.3%
SBA Certified 8(a) Participant	16.7%
Veteran Owned	13.6%
Woman Owned	29.3%
Minority Owned	38.6%
Black American Owned	9.7%
Hispanic American Owned	13.2%
Native American Owned	7.8%

Note: For continuous variables, Mean (SD) is reported. For binary variables, percentage is reported.

Table A.2: Description of Contract Action, Competition, and Pricing

Variable	Top Categories
Contract Action Type	Delivery/Task Order (66.9%); Purchase Order (20.6%); Definitive Contract (11.3%); Blanket Purchase Agreement (1.2%)
Extent Competed	Full and Open After Exclusion (75.9%); Competed under Simplified Acquisition Procedures (SAP) (14.3%); Unavailable (5.1%); Full and Open (3.5%); Not Competed under SAP (1.1%); Not Subject to Full Competition (0.1%)
Type of Contract Pricing	Firm Fixed Price (96.6%); Fixed Price with Economic Price Adjustment (0.9%); Time and Materials (0.8%); Labor Hours (0.6%); Unavailable (0.5%); Cost No Fee (0.2%); Cost Plus Fixed Fee (0.2%); Fixed Price Redetermination (0.1%); Cost Plus Award Fee (0.1%); Fixed Price Incentive (0.0%); Fixed Price Award Fee (0.0%); Fixed Price Level of Effort (0.0%); Cost Sharing (0.0%); Cost Plus Incentive Fee (0.0%)

Note: Percentages in parentheses represent the proportion of observations in each category.

Table A.3: Contract Distribution by Two-Digit NAICS Industry Sector

Industry Sector	%
Construction	39.1
Manufacturing	18.9
Professional, Scientific, and Technical Services	18.9
Administrative and Support and Waste Management and Remediation Services	12.8
Agriculture, Forestry, Fishing and Hunting	2.7
Wholesale Trade	2.4
Accommodation and Food Services	1.1
Other Services (except Public Administration)	0.9
Transportation and Warehousing	0.7
Retail Trade	0.6
Health Care and Social Assistance	0.5
Information	0.4
Real Estate and Rental and Leasing	0.4
Educational Services	0.2
Mining, Quarrying, and Oil and Gas Extraction	0.2
Other	0.2

Note: "Other" represents 4 industry sectors with less than 0.1 percent of contracts each.

Table A.4: Contract Distribution by Government Agency

Agency	%
Department of Defense	65.0
Department of Homeland Security	11.2
Department of Agriculture	7.2
Department of the Interior	3.1
General Services Administration	1.8
Department of Health and Human Services	1.7
Department of State	1.6
Department of Veterans Affairs	1.6
Department of the Treasury	1.4
Department of Justice	1.2
National Aeronautics and Space Administration	0.9
Department of Commerce	0.7
Department of Transportation	0.6
Other	0.6
Department of Labor	0.4
Social Security Administration	0.3
Department of Energy	0.2
Department of Housing and Urban Development	0.1
Securities and Exchange Commission	0.1
Environmental Protection Agency	0.1
Executive Office of the President	0.1

Note: "Other" represents 23 agencies with less than 0.1 percent of contracts each.

Table A.5: Statistics on Match Bewteen Vendor Addresses and Census Geography

Metric	Value
Observations with address (Overall %)	89.24
Observations without address (Overall %)	10.76
Entities with at least one address (%)	90.53
Observations without coordinates	678.00
Observations without coordinatess - **Metro**	189.00
N observations without coordinates - **Non-Metro**	370.00
N observations without coordinates - **Unknown**	119.00
Pct observations without coordinates - **Metro**	27.88
Pct observations without coordinates - **Non-Metro**	54.57
Number unique entities without coordinates	646.00
Number unique entities without coordinates - **Metro**	186.00
Number unique entities without coordinates - **Non-Metro**	354.00
Number of unique entities without coordinates - **Unknown**	112.00

Notes: This table presents geocoding match rates for establishments that received HUBZone awards, matched to 2010 Census geographies by metropolitan area status. Metro designates establishments whose county could be matched to a metropolitan statistical area. Non-metro designates establishments in counties outside metropolitan statistical areas. Unknown includes establishments with missing location information or principal offices outside the 50 states.

Table A.7: HUBZone Match Rates and Median Contracts (Receipt Sample)

Treatment Group	HUBZone Certified Firms	Total Firms	Match Rate (%)	Median Contracts per Firm
Treated - Receipt	16906	18825	89.8	2

Table A.6: Balance Tables: Pre-Treatment Characteristics

Panel A: RD Sample

Variable	Non-QCT		QCT w/ Receipt		Norm. Diff.	Var. Ratio
	Mean	(SD)	Mean	(SD)		
Resident Area Jobs	1382.21	(581.30)	1262.49	(562.13)	-0.21	0.94
Worker Area Jobs	1788.70	(3372.45)	3345.60	(5567.77)	0.34	2.73
Originations	77.17	(51.69)	84.17	(63.75)	0.12	1.52
Originations Amount (Millions)	9449743.21	(9444670.18)	9058332.65	(10613156.36)	-0.04	1.26
Amount Per Origination (Thousands)	115513.46	(59947.29)	96145.54	(71600.20)	-0.29	1.43
Obligation (Millions)	0.00	(0.00)	5.76	(18.23)	0.45	Inf
Median Income	31522.77	(5991.01)	23778.55	(5741.22)	-1.32	0.92
Poverty Rate	0.18	(0.06)	0.24	(0.07)	0.80	1.61
LIHTC	0.02	(0.04)	0.05	(0.11)	0.33	6.12
Unique Tracts	2886		315			

Panel B: QCT 2003 Sample

Variable	No Receipt		HZ Receipt		Norm. Diff.	Var. Ratio
	Mean	(SD)	Mean	(SD)		
Resident Area Jobs	1025.22	(491.18)	1068.64	(524.83)	0.09	1.14
Worker Area Jobs	1714.23	(3649.85)	3259.52	(4960.87)	0.35	1.85
Originations	39.15	(38.06)	60.76	(56.31)	0.45	2.19
Originations Amount (Millions)	4582615.36	(6921473.92)	6520253.39	(9606833.83)	0.23	1.93
Amount Per Origination (Thousands)	97196.27	(60193.98)	91375.39	(64996.78)	-0.09	1.17
Obligation (Millions)	0.00	(0.00)	6.36	(19.50)	0.46	Inf
Median Income	22574.80	(6458.71)	19530.07	(6733.88)	-0.46	1.09
Poverty Rate	0.35	(0.11)	0.33	(0.11)	-0.13	0.95
LIHTC	0.04	(0.06)	0.06	(0.12)	0.22	3.61
Unique Tracts	3399		474			

Notes: This table presents balance tests for pre-treatment characteristics across treatment and control groups. Panel A compares HUBZone awarded tracts to non-QCT tracts within the RD sample (those near the QCT designation threshold). Panel B compares QCT tracts that received HUBZone (HZ) awards to those that did not. All variables are measured prior to treatment assignment. Resident Area Jobs and Worker Area Jobs are from LODES data. Originations refer to mortgage originations from HMDA data from the NNDA. Originations Amount is measured in millions of dollars. Amount Per Origination is measured in thousands of dollars. Obligation refers to HUBZone spending obligations in millions of dollars. LIHTC indicates presence of Low-Income Housing Tax Credit projects. Standard deviations are shown in parentheses. Norm. Diff. is the normalized difference in means. Var. Ratio is the ratio of variances (treated/control).

Table A.8: Receiver-Specific Information (Treated)

Characteristic	Treated - Receipt, N = 18825
Owner Gender	
Female	3,881 (24.0%)
Male	12,266 (76.0%)
SBA Certified Small Disadvantaged	4,522 (24.0%)
SBA 8(a) Program Participant	4,071 (21.6%)
Veteran Owned	2,888 (15.3%)
Women Owned	4,844 (25.7%)
Minority Owned	6,452 (34.3%)
Black American Owned	3,082 (16.4%)
Hispanic American Owned	1,848 (9.8%)
Asian Pacific American Owned	744 (4.0%)
Small Business	10,011 (53.2%)
Number of Contracts	19.9 (76.1) | 2.0
Total Obligations ($ Millions)	9.7 (33.5) | 0.7

Notes: n (%); Mean (SD) | Median

Table A.9: Statistical Tests: Difference in Means and Tests of Equivalence Comparing Treated vs. Not Treated Groups

Variable	Mean (Treated)	Mean (Not Treated)	Difference	t-stat	p-value	95% CI Lower	95% CI Upper	T
Jobs	38.75	39.17	−0.42	−0.26	0.792	−3.56	2.72	
Last Year Jobs	35.22	40.03	−4.81	−3.65	0.001	−7.39	−2.23	
Sales (millions)	1,955.41	5,815.53	−3,860.12	−8.55	<0.001	−4,749.72	−2,970.53	
Revenue (millions)	2,029.29	6,961.67	−4,932.39	−10.00	<0.001	−5,903.78	−3,961.00	
Contracts	19.94	–	–	–	–	–	–	
Total Obligations (millions)	9.66	–	–	–	–	–	–	
Duration	18.89	17.48	1.41	22.28	<0.001	1.28	1.53	

[1] TOST (Two One-Sided Tests) uses equivalence bounds of 0.5 pooled standard deviations

Table A.10: Impact of HUBZone Award Reciept on Jobs and Housing - Consistent Census Tracts from 2000 to 2010 Only

(a) RD Sample

	Resident-Area Jobs	Workplace-Area Jobs	Originations	Amount per Origination
Jobs		2		
Housing		2		
	(1)	(2)	(3)	(4)
HZ Receipt = 1	0.016	0.008	0.002	0.043
	(0.012)	(0.022)	(0.029)	(0.013)
e^{income} × HZ Receipt = 1	-0.096	0.053	-0.260	-0.133
	(0.029)	(0.047)	(0.080)	(0.037)
$e^{poverty}$ × HZ Receipt = 1	0.175	0.016	0.331	0.040
	(0.016)	(0.034)	(0.044)	(0.022)
Observations	66,338	66,338	73,252	73,252

(b) QCT 2003 Sample

	Resident-Area Jobs	Workplace-Area Jobs	Originations	Amount per Origination
Jobs		2		
Housing		2		
	(1)	(2)	(3)	(4)
HZ Receipt = 1	0.033	0.001	0.136	0.072
	(0.011)	(0.020)	(0.029)	(0.013)
Observations	58,706	58,706	63,762	63,762

Notes: Table reports estimates of the effect of obtaining Qualified Census Tract (QCT) status in 2003 on job and housing finance outcomes for tracts near the eligibility threshold. Tracts are restricted to be those whose boundaries did not change between the 2000 and 2010 Census. Panel A presents results from a two-way fixed effects specification with tract and year fixed effects. Panel B adds MSA-specific linear time trends to account for differential metropolitan growth trajectories. Columns (1) and (2) report effects on resident-area jobs (jobs held by census-tract residents) and workplace-area jobs (jobs located in the census tract), respectively. Columns (3) and (4) report effects on mortgage originations and the average amount per origination. All regressions include interactions between QCT status and baseline tract income and poverty rates. All specifications include controls for LIHTC allocations. Standard errors in parentheses are clustered at the tract level. Job estimates include only one year of pre-treatment data and should be interpreted with caution.

Table A.11: Effect of HUBZone Receipt on Jobs by Income of Tract Residents

(a) RD Sample

	Earns < $1250	Earns $1251-$3333	Earns > $3333
	(1)	(2)	(3)
HZ Receipt	-0.010	0.019	0.047
	(0.010)	(0.012)	(0.018)
HZ Receipt × e^{income}	-0.075	-0.081	-0.210
	(0.085)	(0.095)	(0.135)
HZ Receipt × $e^{poverty}$	0.102	0.074	0.074
	(0.048)	(0.058)	(0.070)
Baseline Mean	6.02	6.25	5.28
Observations	83,347	83,350	83,346

(b) QCT 2003 Sample

	Earns < $1250	Earns $1251-$3333	Earns > $3333
	(1)	(2)	(3)
HZ Receipt	0.006	0.031	0.066
	(0.009)	(0.010)	(0.016)
Baseline Mean	5.93	6.01	4.93
Observations	70,185	70,188	70,187

Notes: Table reports estimates of the effect of HUBZone award receipt on resident-area jobs by weekly income of resident job. Panel A presents RD estimates for tracts near the eligibility threshold; Panel B presents estimates using all tracts that obtained Qualified Census Tract status in 2003. Baseline mean refers to the mean of the dependent variable in event year -1. All specifications include controls for LIHTC allocations, tract fixed effects, year fixed effects, and MSA-specific linear time trends. Panel A additionally includes controls for QCT status. Standard errors in parentheses are clustered at the tract level.

Table A.12: Effect of HUBZone Receipt on Jobs by Income of Tract Workers

(a) RD Sample

	Earns < $1250	Earns $1251-$3333	Earns > $3333
	(1)	(2)	(3)
HZ Receipt	0.008	-0.040	-0.026
	(0.024)	(0.025)	(0.032)
HZ Receipt × e^{income}	-0.037	0.083	0.219
	(0.161)	(0.144)	(0.181)
HZ Receipt × $e^{poverty}$	-0.068	-0.008	0.035
	(0.106)	(0.108)	(0.116)
Baseline Mean	6.04	6.35	5.74
Observations	83,243	83,283	82,873

(b) QCT 2003 Sample

	Earns < $1250	Earns $1251-$3333	Earns > $3333
	(1)	(2)	(3)
HZ Receipt	-0.002	-0.032	-0.030
	(0.020)	(0.019)	(0.026)
Baseline Mean	6.04	6.35	5.74
Observations	70,077	70,066	69,541

Notes: Table reports estimates of the effect of HUBZone award receipt on workplace-area jobs by weekly income of those employed in the census tract. Panel A presents RD estimates for tracts near the eligibility threshold; Panel B presents estimates using all tracts that obtained Qualified Census Tract status in 2003. Baseline mean refers to the mean of the dependent variable in event year -1. All specifications include controls for LIHTC allocations, tract fixed effects, year fixed effects, and MSA-specific linear time trends. Panel A additionally includes controls for QCT status. Standard errors in parentheses are clustered at the tract level.

Table A.13: Effect of HUBZone Receipt on Jobs by Age of Tract Residents

(a) RD Sample

	<30 yrs	30-54 yrs	>55 yrs
	(1)	(2)	(3)
HZ Receipt	0.019	0.012	-0.014
	(0.014)	(0.012)	(0.014)
HZ Receipt $\times$ e^{income}	-0.151	-0.117	0.077
	(0.112)	(0.101)	(0.113)
HZ Receipt $\times$ $e^{poverty}$	0.128	0.128	0.059
	(0.063)	(0.052)	(0.058)
Baseline Mean	5.9	6.44	5.01
Observations	83,348	83,350	83,343

(b) QCT 2003 Sample

	<30 yrs	30-54 yrs	>55 yrs
	(1)	(2)	(3)
HZ Receipt	0.037	0.035	0.026
	(0.012)	(0.010)	(0.011)
Baseline Mean	5.74	6.23	4.77
Observations	70,187	70,188	70,187

Notes: Table reports estimates of the effect of HUBZone award receipt on resident-area jobs by age of employed residents. Panel A presents RD estimates for tracts near the eligibility threshold; Panel B presents estimates using all tracts that obtained Qualified Census Tract status in 2003. The dependent variable is the natural log of workplace-area jobs for establishments of different ages within the tract. Baseline mean refers to the mean of the dependent variable in event year -1. All specifications include controls for LIHTC allocations, tract fixed effects, year fixed effects, and MSA-specific linear time trends. Panel A additionally includes controls for QCT status. Standard errors in parentheses are clustered at the tract level.

Table A.14: Effect of HUBZone Receipt on Jobs by Age of Tract Workers

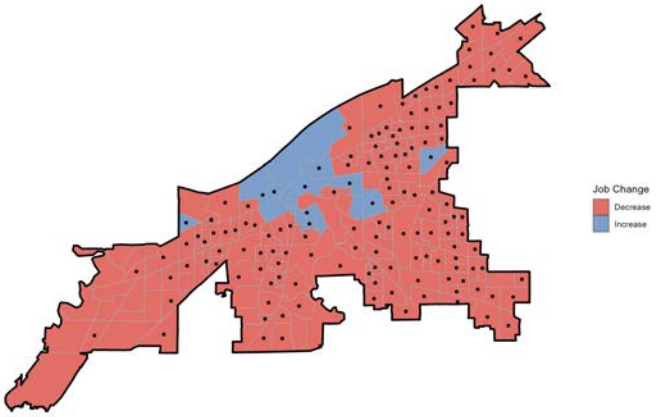
(a) RD Sample

	<30 yrs (1)	30-54 yrs (2)	>55 yrs (3)
HZ Receipt	0.110	0.157	0.116
	(0.070)	(0.067)	(0.074)
HZ Receipt × e^{income}	-0.770	-1.26	-1.12
	(0.363)	(0.354)	(0.410)
HZ Receipt × $e^{poverty}$	-0.032	-0.034	-0.006
	(0.278)	(0.270)	(0.227)
Baseline Mean	5.87	6.69	5.42
Observations	38,793	38,879	38,820

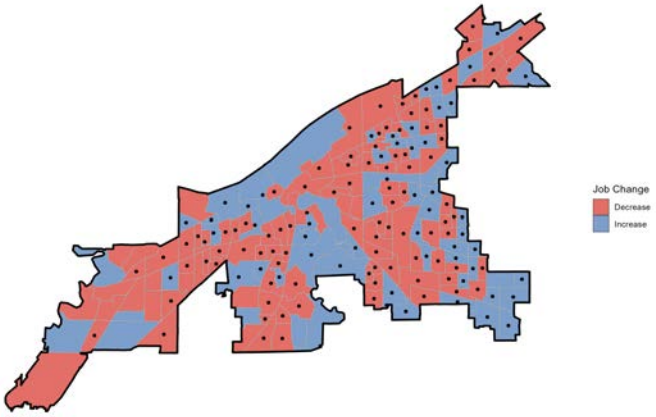
(b) QCT 2003 Sample

	<30 yrs (1)	30-54 yrs (2)	>55 yrs (3)
HZ Receipt	0.066	0.082	0.084
	(0.045)	(0.047)	(0.047)
Baseline Mean	5.87	6.7	5.41
Observations	30,965	31,086	31,014

Notes: Table reports estimates of the effect of HUBZone award receipt on workplace-area jobs by age of those employed within the tract. Panel A presents RD estimates for tracts near the eligibility threshold; Panel B presents estimates using all tracts that obtained Qualified Census Tract status in 2003. The dependent variable is the natural log of workplace-area jobs for establishments of different ages within the tract. Baseline mean refers to the mean of the dependent variable in event year -1. All specifications include controls for LIHTC allocations, tract fixed effects, year fixed effects, and MSA-specific linear time trends. Panel A additionally includes controls for QCT status. Standard errors in parentheses are clustered at the tract level.



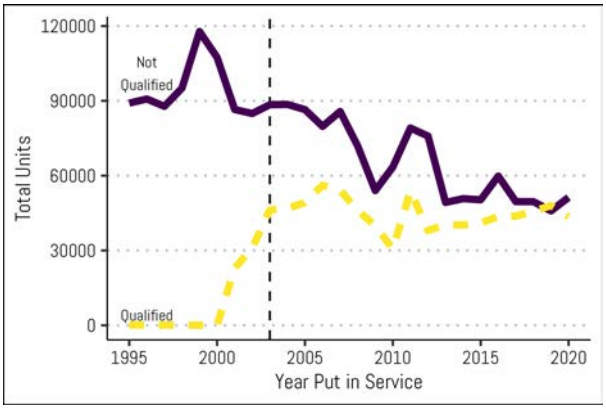
(a) Residents



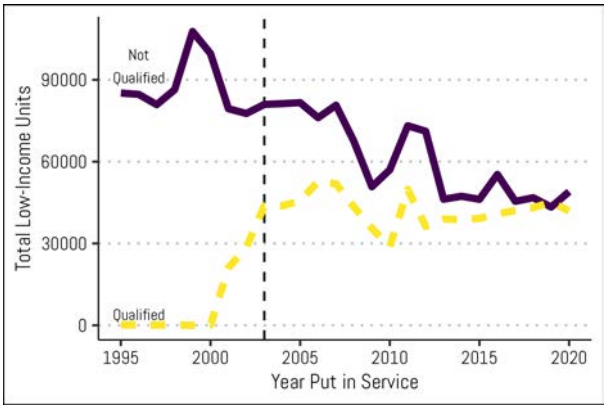
(b) Workers

Figure A.1: Change in Residential and Total Workers After HUBZone Eligibility

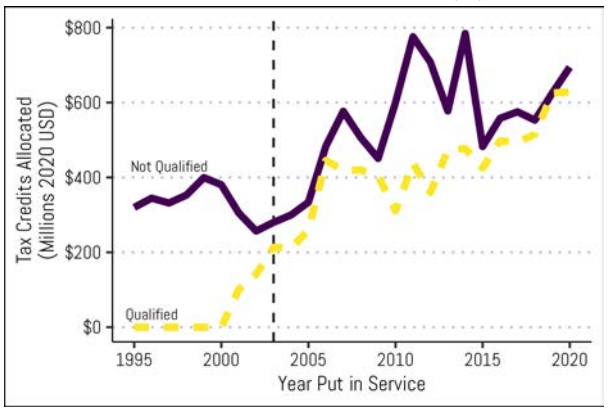
Notes: Sources: LODES. Author's calculation. Black dots indicate QCT status.



(a) Total Units



(b) Total Low-Income Units



(c) Credit Allocated

Figure A.2: LIHTC Program Overview by Qualified Census Tract Status

Notes: Dashed vertical line at 2003 to indicate year of program eligibility was based on 2000 Census income and poverty thresholds.

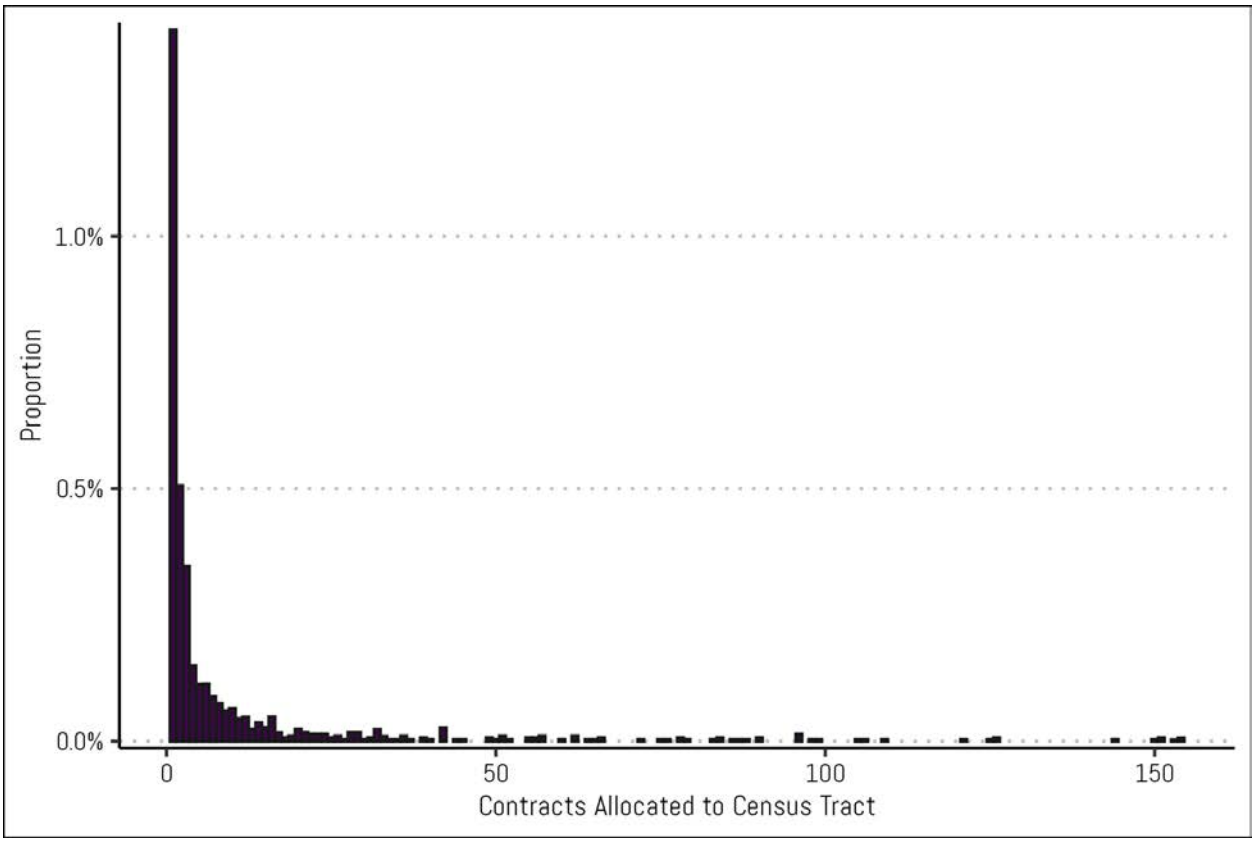


Figure A.3: Number of Unique Contracts per Tract

Notes: Figure shows the distribution of contracts across metropolitan-area census tracts. For readability, the figure excludes tracts awarded zero contracts (96.4 percent of all tracts) and tracts in the top 10th percentile of the distribution (<0.01 percent of all tracts).

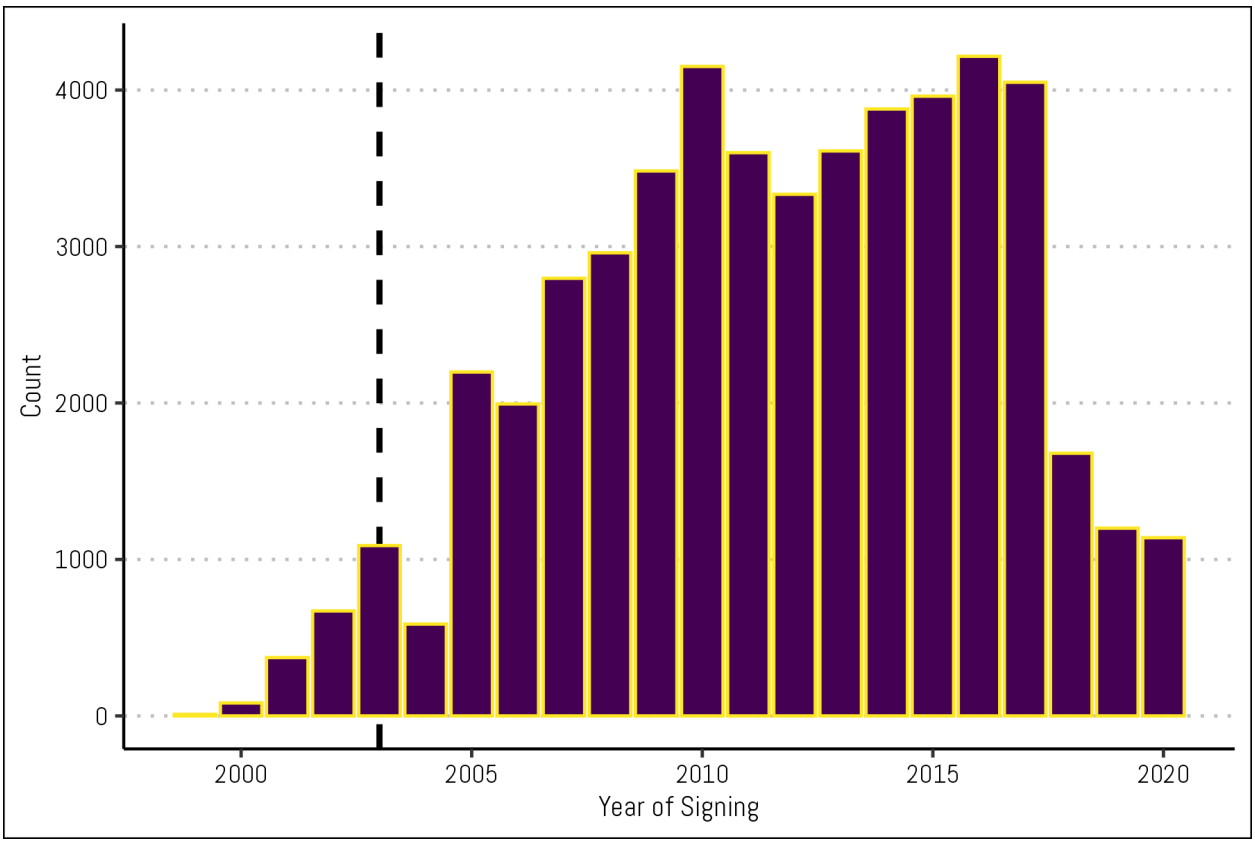
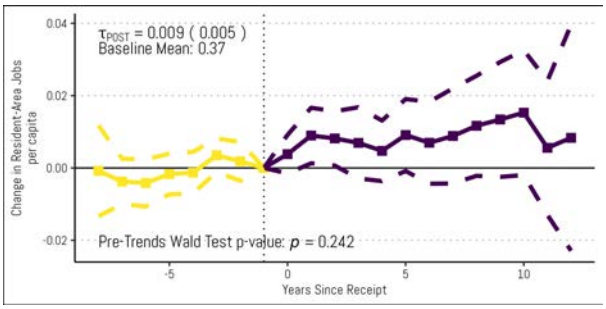
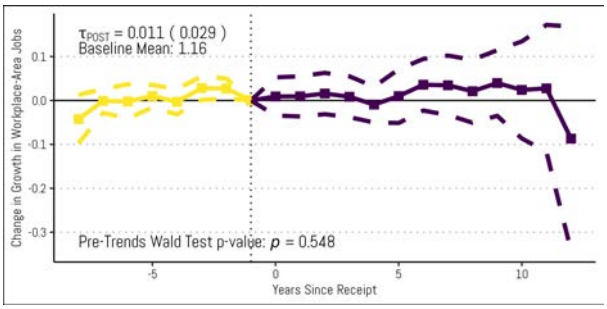


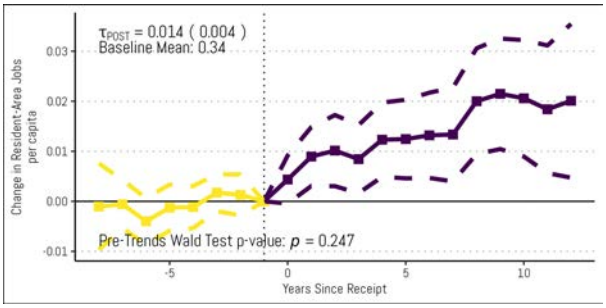
Figure A.4: Number of Contracts Signed by Year of Signing



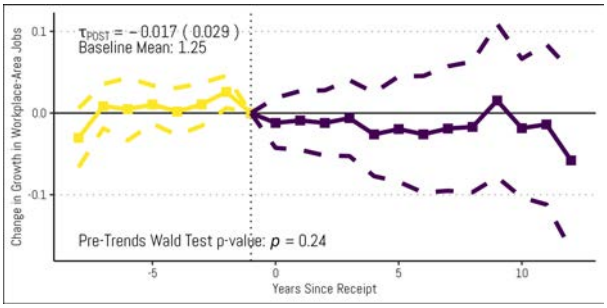
(a) Resident Jobs - RD



(b) Workplace Jobs - RD



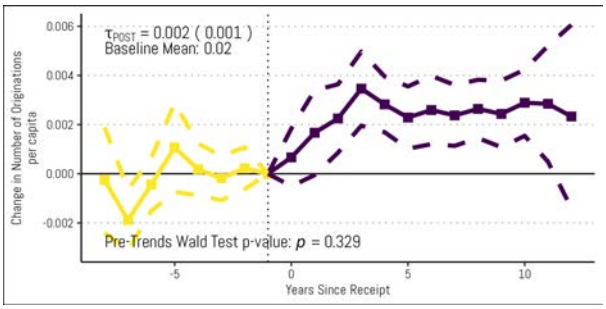
(c) Resident Jobs - QCT 2003



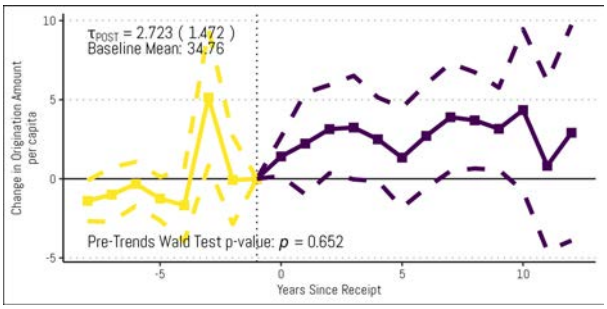
(d) Workplace Jobs - QCT 2003

Figure A.5: Effect of HUBZone Receipt on Resident-Area and Workplace-Area Jobs per Capita

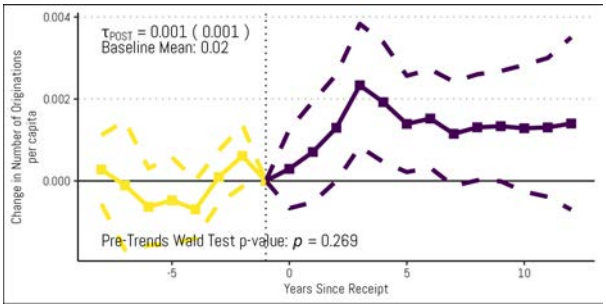
Notes: Figure reports event-study estimates of the effect of HUBZone award receipt on jobs. Outcomes are reported in jobs per census tract residents in 2000. Panels (a) and (b) restrict the sample to tracts near the eligibility threshold (RD sample); panels (c) and (d) use all tracts that obtained Qualified Census Tract status in 2003. Estimates are relative to event year -1 (the year before first award receipt), which is normalized to zero. Event years -8 and earlier are binned into a single coefficient; event years 12 and later are binned into a single coefficient. All specifications include controls for LIHTC allocations, tract fixed effects, year fixed effects, and MSA-specific linear time trends. The RD sample additionally includes controls for QCT status. Standard errors are clustered at the tract level. Dashed lines indicate 95 percent confidence intervals. Pre-trend p-values report Wald tests of the joint null hypothesis that all pre-event coefficients equal zero. τ_{POST} reports the average treatment effect on the treated for all post-event periods, with standard error in parentheses. Baseline mean is the average of the treated group's pre-treatment outcomes.



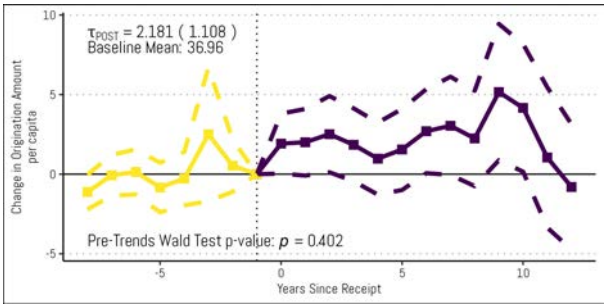
(a) Originations per Capita - RD



(b) Amount per Origination per Capita - RD



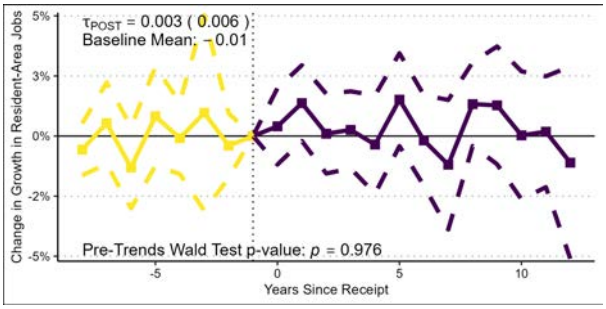
(c) Originations per Capita - QCT 2003



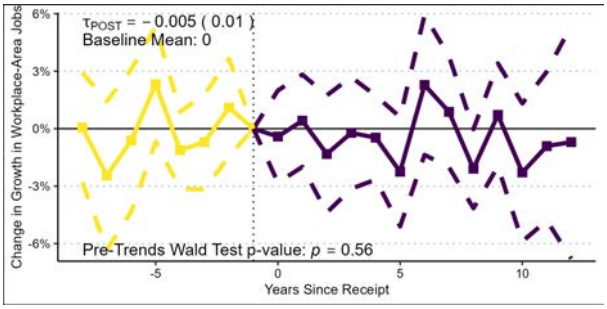
(d) Amount per Origination per Capita - QCT 2003

Figure A.6: Effect of HUBZone Receipt on Housing Outcomes per Capita

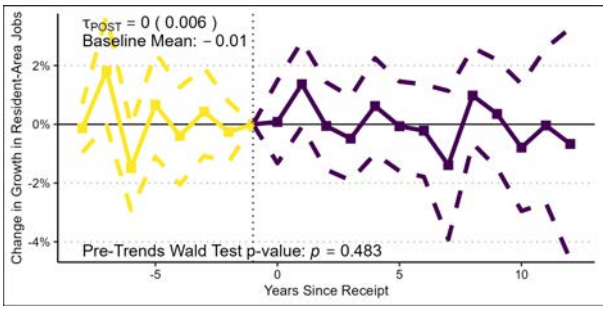
Notes: Figure reports event-study estimates of the effect of HUBZone award receipt on the number of mortgage originations per capita and the amount per origination per capita in census tracts. Panels (a) and (b) restrict the sample to tracts near the eligibility threshold (RD sample); panels (c) and (d) use all tracts that obtained Qualified Census Tract status in 2003. Estimates are relative to event year -1 (the year before first award receipt), which is normalized to zero. Event years -8 and earlier are binned into a single coefficient; event years 12 and later are binned into a single coefficient. All specifications include controls for LIHTC allocations, tract fixed effects, year fixed effects, and MSA-specific linear time trends. The RD sample additionally includes controls for QCT status. Standard errors are clustered at the tract level. Dashed lines indicate 95 percent confidence intervals. Pre-trend p-values report Wald tests of the joint null hypothesis that all pre-event coefficients equal zero. τ_{POST} reports the average treatment effect on the treated group for all post-event periods, with standard error in parentheses. Baseline mean is the average of the treated group's pre-treatment outcomes.



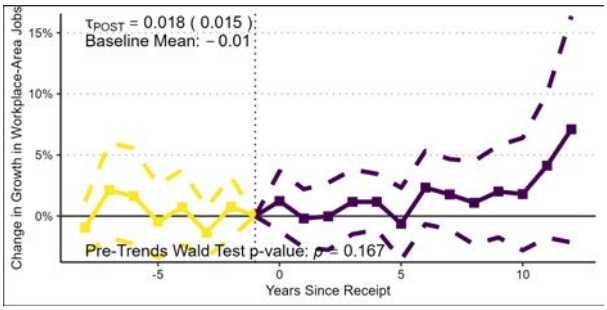
(a) Resident-Area Jobs Growth - RD



(b) Workplace-Area Jobs Growth - RD



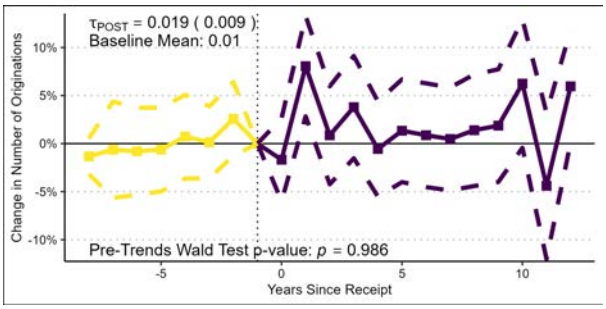
(c) Resident-Area Jobs Growth - QCT 2003



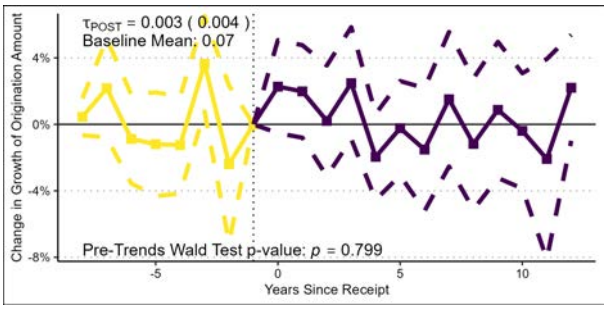
(d) Workplace-Area Jobs Growth - QCT 2003

Figure A.7: Effect of HUBZone Receipt on Job Growth Rates

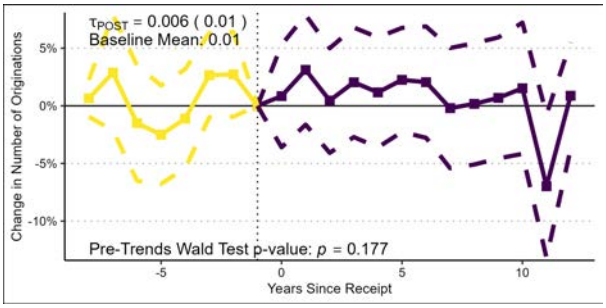
Notes: Figure reports event-study estimates of the effect of HUBZone award receipt on job growth rates. Job growth is calculated as the year-over-year log difference in resident-area/workplace-area jobs. Panels (a) and (b) restrict the sample to tracts near the eligibility threshold (RD sample); panels (c) and (d) use all tracts that obtained Qualified Census Tract status in 2003. Estimates are relative to event year -1 (the year before first award receipt), which is normalized to zero. Event years -8 and earlier are binned into a single coefficient; event years 12 and later are binned into a single coefficient. All specifications include controls for LIHTC allocations, tract fixed effects, year fixed effects, and MSA-specific linear time trends. The RD sample additionally includes controls for QCT status. Standard errors are clustered at the tract level. Dashed lines indicate 95 percent confidence intervals. Pre-trend p-values report Wald tests of the joint null hypothesis that all pre-event coefficients equal zero. τ_{POST} reports the average treatment effect on the treated group for all post-event periods, with standard error in parentheses. Baseline mean is the average of the treated group's pre-treatment outcomes.



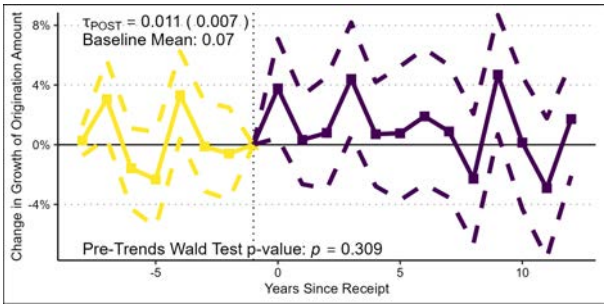
(a) Originations Growth - RD



(b) Amount per Origination Growth - RD



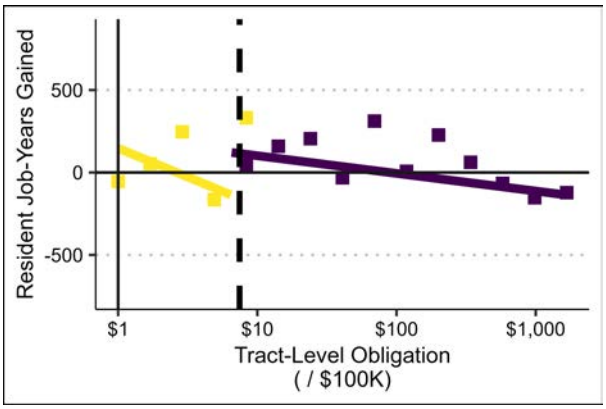
(c) Originations Growth - QCT 2003



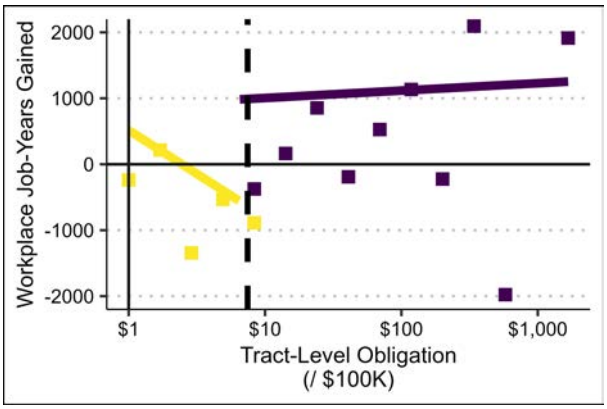
(d) Amount per Origination Growth - QCT 2003

Figure A.8: Effect of HUBZone Receipt on Growth Rates of Housing Outcomes

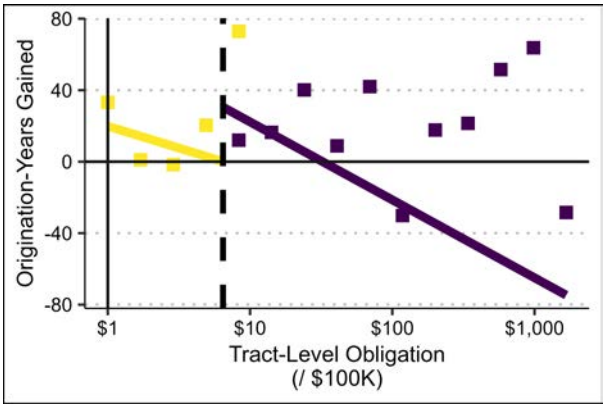
Notes: Figure reports event-study estimates of the effect of HUBZone award receipt on the growth rates of mortgage originations and the amount per origination in census tracts. Growth rates are calculated as the year-over-year log difference in resident-area/workplace-area jobs. Panels (a) and (b) restrict the sample to tracts near the eligibility threshold (RD sample); panels (c) and (d) use all tracts that obtained Qualified Census Tract status in 2003. Estimates are relative to event year -1 (the year before first award receipt), which is normalized to zero. Event years -8 and earlier are binned into a single coefficient; event years 12 and later are binned into a single coefficient. All specifications include controls for LIHTC allocations, tract fixed effects, year fixed effects, and MSA-specific linear time trends. The RD sample additionally includes controls for QCT status. Standard errors are clustered at the tract level. Dashed lines indicate 95 percent confidence intervals. Pre-trend p-values report Wald tests of the joint null hypothesis that all pre-event coefficients equal zero. τ_{POST} reports the average treatment effect on the treated groups for all post-event periods, with standard error in parentheses. Baseline mean is the average of the treated group's pre-treatment outcomes.



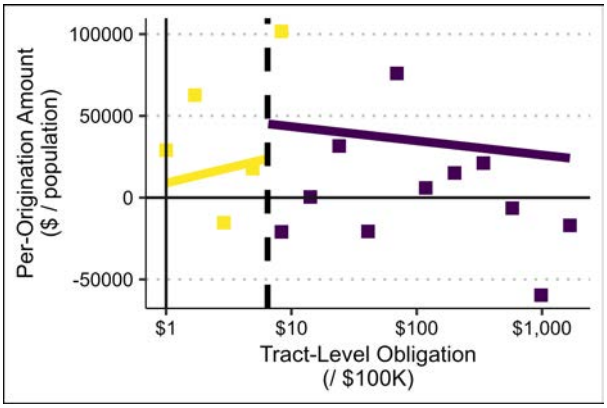
(a) Residential Jobs



(b) Workplace Jobs



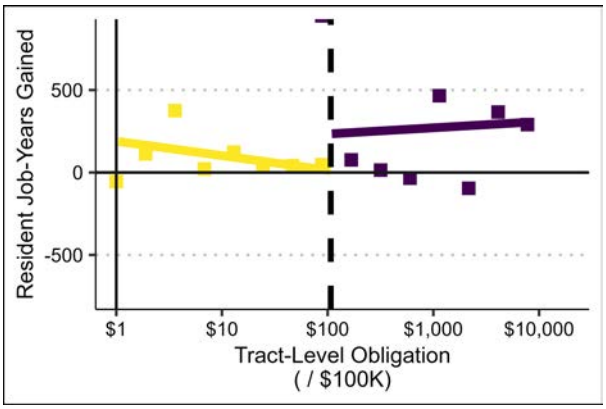
(c) Originations



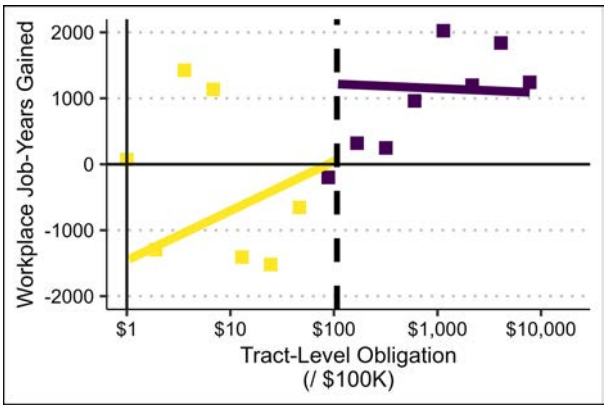
(d) Amount per Origination

Figure A.9: Obligation Effects of HUBZone Awards Split by Above/Below Median Award: **QCT 2003 Sample**

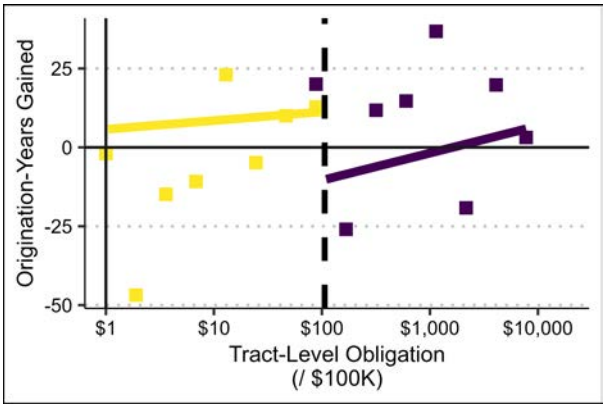
Notes: Figure examines the relationship between HUBZone award obligations and tract-level jobs and housing effects split by awards above or below the median cumulative award per capita using binned scatter plots. Panel (a) shows results for resident-area jobs; panel (b) shows results for workplace-area jobs; the y-axis measures job-years gained, calculated as the estimated annual job effect multiplied by the length of the post-treatment window for that tract. Panel (c) shows results for number of originations; panel (d) shows results for amount per origination. The sample includes 424 treated tracts, binned into 15 groups based on obligation amounts. Each point represents the mean job-years gained and mean tract-level obligations (per capita) within a bin. Tract-specific job effects are estimated from the 2x2 difference-in-differences specification described in equation 6. The solid line represents a precision-weighted linear fit from equation 7, where weights equal the inverse variance of the tract-specific treatment effect estimates. The x-axis shows tract-level obligations in dollars per capita (tract population), plotted on a logarithmic scale to accommodate the wide range of award sizes. The estimated slope coefficient and standard error are reported in the figure.



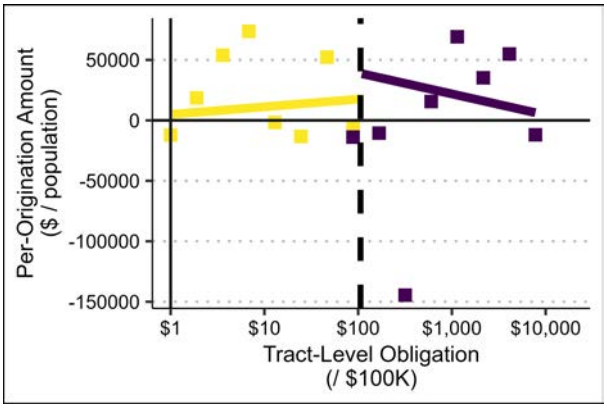
(a) Resident Jobs



(b) Workplace Jobs



(c) Originations



(d) Amount per Origination

Figure A.10: Obligation Effects of HUBZone Awards Split by Above/Below Median Award: **RD Sample**

Notes: Figure examines the relationship between HUBZone award obligations and tract-level jobs and housing effects split by awards above or below the median cumulative award per capita using binned scatter plots. Panel (a) shows results for resident-area jobs; panel (b) shows results for workplace-area jobs; the y-axis measures job-years gained, calculated as the estimated annual job effect multiplied by the length of the post-treatment window for that tract. Panel (c) shows results for number of originations; panel (d) shows results for amount per origination. The sample includes 315 treated tracts, binned into 15 groups based on obligation amounts. Each point represents the mean job-years gained and mean tract-level obligations (per capita) within a bin. Tract-specific job effects are estimated from the 2x2 difference-in-differences specification described in equation 6. The solid line represents a precision-weighted linear fit from equation 7, where weights equal the inverse variance of the tract-specific treatment effect estimates. The x-axis shows tract-level obligations in dollars per capita (tract population), plotted on a logarithmic scale to accommodate the wide range of award sizes.