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## Abstract


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This dissertation studies the role of urban policies in shaping local housing markets, local labor markets, and transportation decisions. Chapter 1 uses tax records to document the first available evidence on the short-run response of financial capital to the Opportunity Zone (OZ) program, a federal place-based policy that provides tax incentives for capital investments in low-income neighborhoods. My coauthor and I document how OZ investments are spatially concentrated, flow towards areas with improving trends, and are primarily claimed by individuals at the 99th percentile of the national income distribution. In Chapter 2, I explore how real estate investment and home values respond to OZ tax credits. I find that targeted neighborhoods see a significant increase in new development and that local home values appreciate. Nearby areas see an increase in investment as well. Through a spatial-equilibrium model, I find that while the program as implemented had net benefits in terms of home value appreciation, alternative designations of neighborhoods for the tax credit could have substantially improved the program's equity and efficiency. In Chapter 3, my coauthor and I investigate the long-run effects of converting buildings to high-end condominiums on local home prices, demographics, and new business entry. We find that owners who are able to change their building's legal status to condominium tend to see their property values increase and are more likely to invest in renovations and rent out their units before selling them. Following these conversions, the values of adjacent buildings increase and the surrounding neighborhood gentrifies. Finally, in Chapter 4, my coauthors and I study whether ride-hailing complements or substitutes public transportation by examining the response of Uber ridership to rail expansions. We find that a new station significantly increases Uber ridership nearby, and that this effect decays to zero farther away. In studying the economic effects of urban policies, the chapters of my dissertation point to the value of a layered approach: using granular spatial data; adjusting analyses for neighborhood particularities to estimate the causal impacts of a policy and to account for its indirect effects on nearby areas; and delineating the manner in which public policy effects vary with neighborhood characteristics.

*For my grandparents Bebe and GD,*
*who first introduced me to California,*
*and then made it feel like home*

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# Dissertation Introduction

This dissertation studies the role of urban policies in local housing markets, local labor markets, and transportation decisions. Chapters 1 and 2 study a federal policy meant to spur investment in distressed areas. Chapter 3 examines a legal property designation, known as condominiums, that allows individuals to own apartments within multi-unit buildings. Chapter 4 considers new light and heavy rail station openings and their impact on Uber ridership. Throughout the dissertation, these urban policies are used to study questions central to policymakers. Can targeted public policies improve neighborhood outcomes? What is the role of housing policy in gentrification? How do newly available public transportation options alter individual choices of modes of transit?

In Chapter 1, coauthored with Patrick Kennedy, I use de-identified federal tax records from tax years 2019 and 2020 to document the first available evidence on the short-run response of financial capital to the Opportunity Zone (OZ) program, a federal place-based policy that provides tax incentives for capital investments in more than 8,000 low-income neighborhoods across the United States. We observe \$41.5 billion of aggregate cumulative OZ investments by tax year 2020. Using a subsample of electronically filed returns covering 78% of total observed investment, we document three emerging patterns in the data. First, OZ capital is highly spatially concentrated. Second, among OZ-designated neighborhoods, investors report greater equity and property investments in neighborhoods with relatively higher incomes, home values, educational attainment, and pre-existing income and population growth. Third, OZ investors have extremely high incomes relative to the US population, implying that the direct distributional incidence of the tax subsidy benefits households in the 99th percentile of the national income distribution.

The extent to which public policy can encourage new investment into areas that need it, and how those policies should be targeted, remain open questions. In Chapter 2, I evaluate the impact of Opportunity Zones on new residential and commercial development, and quantify how policymakers could have achieved a more efficient and equitable response through alternative designations of the investment tax credit. Using a novel dataset on the location and timing of new development projects in large U.S. cities, I find that receiving the tax credit increases new development in census tracts by 2.9pp (20.5%). I also find positive spillovers on nearby development. Both effects are larger in neighborhoods with three characteristics: more available land to develop, more elastic housing supply, and lower home values. Through a model of new development that accounts for location- heterogeneities, dynamics, localized spillovers, and the equilibrium behavior of developers, I find that the policy as implemented had city-wide impacts on new development on the order of 2.7%. However, optimally chosen Opportunity Zones would have substantially increased the investment response. The results suggest that there is substantial scope for equity and efficiency improvements in how the program was implemented.

In major cities throughout the United States and the world, some of the most contentious debates over housing policy focus on the extent to which high-end developments are a cause of gentrification. In Chapter 3, coauthored with Patrick Kennedy, I shed new light on this question empirically by studying a unique lottery in the city of San Francisco that allowed a limited number of property owners to convert their buildings into high-end condominiums. Relying solely on exogenous variation from the lottery, we study the long-run effects of these developments on local home prices, demographics, and new business entry. Compared to losing lottery applicants, winners are substantially more likely to invest in alterations and renovations in their properties, to see their property values increase, to rent their properties to new tenants, and to sell them to new owners. After such conversions, the home values of adjacent buildings also increase. At the neighborhood level, conversions lead to higher resident incomes, home values, rental prices, and shares of the population that are White and college-educated. New establishments specializing in education and professional services enter the neighborhood. We discuss economic interpretations of these results in the broader context of a local housing market where demand is high and supply is sharply constrained.

In urban policy discussions, there is also heated debate on whether ride-hailing complements or substitutes public transportation. In Chapter 4, coauthored with Marco Gonzalez-Navarro, Jonathan Hall, and Rik Williams, I address this question using novel data and an innovative identification strategy. Our identification strategy relies on exogenous variation in local transit availability caused by rail expansions. Using proprietary, anonymized trip data from Uber for 35 countries, we use a dynamic difference-in-differences strategy to estimate how transit expansions affect local Uber ridership in 100 m distance bands centered on the new train station. Our estimates compare Uber ridership within a distance band before and after a train station opens relative to the next-further-out distance band. Total effects are obtained by aggregating relative effects at all further distance bands. We find that the opening of a new rail station increases Uber ridership within 100 meters of the station by 60%, and that this effect decays to zero for distances beyond 300 meters. This sharp test implies Uber and rail transit are complements.

Taken together, the chapters of my dissertation suggest that several important factors are critical when considering the economic impact of urban policies: the need for granular spatial data to understand local econonomic effects; the importance of analyses that account for the particular reasons neighborhoods are affected by public policy and for the indirect effects on nearby areas; and the value of studying how these effects vary in accordance with neighborhood characteristics. This dissertation provides evidence as to why certain neighborhoods lag behind others when urban policies are implemented and why some others experience rapid change. The findings, I hope, will be of use to future researchers and policymakers as they decide how best to improve neighborhood outcomes.

# Chapter 1

# Neighborhood-Level Investment from the U.S. Opportunity Zone Program: Early Evidence

## 1.1 Introduction

Socioeconomic disparities across regions and neighborhoods are pervasive in the United States (Gaubert, Kline, Vergara, and Yagan 2021a; Reardon and Bischoff 2011), and recent research documents that these disparities are likely to have causal effects on individuals' productivity (Moretti 2012), health (Chandra and Skinner 2003), intergenerational economic mobility (Chetty, Hendren, and Katz 2016b; Chetty, Hendren, Kline, and Saez 2014), and propensity for innovation (Bell, Chetty, Jaravel, Petkova, and Van Reenen 2019). However, researchers and the public disagree about which policies, if any, are effective means to improving these outcomes (Glaeser and Gottlieb 2009; Busso, Gregory, and Kline 2013; Kline and Moretti 2014a; Neumark and Simpson 2015; Gaubert, Kline, and Yagan 2021b).

In this paper, we use de-identified federal business tax records to study the short-run response of financial capital to the U.S. Opportunity Zone (OZ) program, a federal place-based policy enacted in 2017 as part of the Tax Cuts and Jobs Act. As a result of this recent legislation, equity and property investments in more than 8,000 designated census tracts across the United States are eligible for highly favorable tax treatment of income accrued from capital gains.

The scale of the Opportunity Zone (OZ) program is unique in the modern landscape of place-based federal policies, both in terms of its expansive geographic scope and significant federal cost. OZs are located in urban, suburban, and rural areas across all 50 states, covering approximately 12% of all U.S. census tracts. The breadth of the program offers a natural setting to consider how place-based policies impact heterogeneous neighborhoods. The Congressional Joint Committee on Taxation has estimated that the OZ program will cost the government \$1.6 billion annually in foregone tax revenue, more than any other existing federal place-based policy.[1]

A nascent literature on Opportunity Zones studies short-run impacts of the program on real estate prices (Chen, Glaeser, and Wessel 2023), job postings (Atkins, Hernandez-Lagos, Jara-Figueroa, and Seamans 2020), and employment (Freedman, Khanna, and Neumark 2021), finding null or modest effects. An exception is Arefeva, Davis, Ghent, and Park (2020a), who estimate substantial increases in employment from establishment-level data. However, a key missing link in the early evidence is data on the response of financial investors to the tax subsidy. Existing studies estimate intent-to-treat (ITT) effects based on tract-level binary indicators for OZ status, but without information on program take-up are unable to estimate average treatment effects (ATE). While both of these parameters are of clear and natural interest to policymakers and researchers, a richer and more complete understanding of the evidence requires data on how investors have responded to the capital tax subsidy.

In particular, if investor behavior is only weakly responsive to the OZ tax subsidies, then small or null ITT effects are perhaps unsurprising, and policymakers may wish to consider if or how alternative policy mechanisms might attract investment to low-income neighborhoods. On the other hand, if investors are highly responsive to the subsidy and yet over time we do not observe desirable downstream effects on labor market outcomes, then policymakers may wish to shift budget priorities away from capital tax subsidies and consider alternative policy levers that may be more effective.

This paper fills a gap in the existing research by documenting the first available evidence on tract-level financial investment associated with the OZ program. Our data is based on de-identified electronically-filed federal business tax records from tax years 2019 and 2020, the first two years in which OZ investors were required to report detailed information on the location and recipients of their investments to the IRS. We emphasize that these data are preliminary, and do not yet incorporate data from an estimated $9.0% billion (approximately 22%) of cumulative OZ investments filed via paper tax returns. Throughout the paper we explicitly discuss limitations of these early data, and we will continue to update this working paper as more up-to-date information becomes available.

We highlight three main findings from the early evidence.

First, OZ investment is highly spatially concentrated. The vast majority of designated Opportunity Zone tracts in our sample, 63%, receive zero OZ capital. However, among tracts where investing firms report positive investment, the average value is substantial, at approximately $3,313 per resident. The distribution is strongly skewed even among these tracts with positive investment, such that the median value is $386, approximately one-ninth of the average.

Second, we correlate reported OZ investment with demographic and firm characteristics, and show that OZ capital gravitates toward eligible neighborhoods with relatively higher educational attainment, incomes, home values, population density, and concentrations of professional and amenity services. These patterns are strongest for neighborhoods with pre-existing upward trends in population, income, and home values, and declining shares of

[1]See The Joint Committee on Taxation (2020) estimates of federal tax expenditures from 2020-2024.

elderly and non-white residents. On the firm side, we show that reported OZ investment is overwhelmingly concentrated in equity investments in businesses that specialize in real estate, construction, and finance.

Third, OZ investors have extremely high incomes relative to the US population. We identify a large sample of OZ investors and estimate their median and average 2019 household income to be greater than $741,000 and $4.9 million, respectively.[2] These estimates imply that the direct distributional incidence of the tax subsidy is likely to benefit households in the 99th percentile of the US income distribution.

In the final section, we geocode the universe of individual and business tax records to construct novel measures of tract-level household and family income, employment, commuting, firm growth, and real investment. We demonstrate that these estimates closely match corresponding measures from publicly available data and describe advantages of our new measures relative to existing data. As more comprehensive data on OZ investment become available, we plan to use these data to evaluate the causal effect of the OZ tax subsidies on local labor market and real investment outcomes.

The rest of the paper proceeds as follows. Section 1.2 highlights the OZ program's goals and objectives as described by its authors in Congress, describes the process by which neighborhoods were nominated and selected, and provides details on the program's capital tax subsidies. Section 1.3 presents the first available descriptive evidence on the spatial distribution of OZ investment across the United States, based on electronic business tax filings in tax years 2019 and 2020. Section 1.4 presents new tract-level estimates of wages, family income, firm growth, and real investment based on IRS microdata, and relates these measures with the available data on OZ investment. Section 1.5 concludes with a discussion of this new evidence in relation to other recent studies, and provides roadmap for future research.

## 1.2 Opportunity Zones: Brief Background

### Historical Overview

In February of 2017, a bipartisan group of U.S. Senators and Representatives introduced the Investing in Opportunity Act, which was later incorporated into the Tax Cuts and Job Act enacted by Congress in December of that year. The Congressional authors of the legislation described the goals of the OZ program in a joint public statement:

> *"Too many American communities have been left behind by widening geographic disparities and increasingly uneven economic growth.* [...] *Americans should have access to economic opportunity regardless of their zip code. The Investing in Opportunity Act will unlock new private investment for communities where millions*

[2]Throughout the paper, all centile statistics are computed as centile averages to protect taxpayer privacy. For example, medians are computed as the average of all taxpayers in the 49th to 51st percentiles.

> *of Americans face the crisis of closing business, lack of access to capital, and declining entrepreneurship.* [...] *With this bill, we will dramatically expand the resources to restore economic opportunity, job growth, and prosperity for those who need it most."*[3]

The legislative focus on capital subsidies, rather than wage or employment subsidies, distinguishes the OZ program from the federal Empowerment Zone initiative launched in 1995, and is more ambitious in scope but similar in spirit to the federal New Markets Tax Credit Program enacted in 2000. In accordance with Congressional goals, an important aim of this research is to estimate how responsive investment has been to the OZ tax subsidy, how investment has affected local workers and businesses, and how these impacts may be heterogeneous across individuals who live, work, and invest in Opportunity Zones.

### Tract Eligibility and Nomination Process

The primary geographic units of the OZ program are *census tracts*, which we interchangeably refer to as *neighborhoods* or just *tracts*. Census tracts are small spatial units of approximately 4,000 residents, with coverage spanning the entirety of the United States.

Congress determined that tracts would be eligible for OZ designation if they could be classified as a *low income community* (LIC), defined as a tract with a poverty rate above 20% or median family income (MFI) less than 80% of the area median.[4] In practice, policymakers used estimates of tract poverty rates and median family income from the 2015 5-Year American Community Survey (ACS) to assess eligibility.

Congress also allowed for a small number of tracts to be eligible for OZ designation even if they did not meet the poverty or income thresholds. Tracts classified as high-migration rural communities or low-population communities were deemed eligible, as were tracts with median family income of less than 125% of an adjacent eligible low income community.[5] However, the vast majority of designated OZ tracts (97%) were deemed eligible on the basis of their poverty rate or median family income in the 2015 American Community Survey rather than these alternate criteria.

After Treasury and IRS determined which tracts were policy-eligible, state governors were given three months to nominate tracts for OZ designation. States could nominate up to 25% of their eligible tracts, and less populated states were granted a minimum of 25 OZs. Treasury

[3] Statement by Senators Cory Booker and Tim Scott and Representatives Ron Kind and Pat Tiberi, February 2, 2017.

[4] For rural tracts, the area MFI is taken to be the statewide median family income. For urban tracts, the area MFI is the larger of the statewide MFI and the metropolitan area MFI.

[5] A high-migration rural community is defined as a census tract located within a high-migration rural county whose median family income was 85% of the statewide median family income. High migration rural counties are those that have had net outmigration of greater than 10% over the period 1990-2010. A low-popuation tract is a tract within an empowerment zone, contiguous to at least one LIC, with a population of less than 2,000. No more than 5% of a state's tracts could be nominated on the basis of meeting the adjacency criteria.

accepted all state nominations from April to June of 2018, and ultimately designated 8,764 tracts (≈12% of all tracts) as Opportunity Zones. We explore the characteristics of eligible and chosen OZ tracts in greater detail in Section 1.2.

### OZ Tax Subsidies

The central policy instruments of the OZ program are capital subsidies — specifically, highly favorable tax treatment of income accrued from capital gains. Investors intending to claim the tax benefit must (a) register their business as a Qualifying Opportunity Fund (QOF) with the IRS, (b) liquidate an existing asset, and (c) re-invest the capital gains into qualifying OZ assets. There are three main tax advantages conferred on these investments, which we summarize below.

First, taxes owed on capital gains from liquidating the initial asset are deferred until the fund sells its subsequent OZ investments or until the end of 2026, whichever is sooner. Since investors may redeploy this taxable income into income-bearing assets until the tax is due, the deferral is potentially lucrative. Second, investors who hold qualifying OZ assets are eligible for a step-up in basis on their initial capital gains after 5 years (10%) and 7 years (15%), directly reducing tax liability. Finally, investors who hold qualifying OZ assets for at least 10 years may claim a 100% reduction in capital gains tax on appreciation of those OZ assets. The capital gains tax rate typically ranges from 15-20%, and so full elimination of the tax represents a large and significant subsidy.[6]

Broadly, QOF funds may invest in two categories of assets: (1) stock and partnership interests in qualifying operating businesses (QOB), and (2) qualifiying property (QOP), which can be leased or owned. Qualifying OZ businesses (that is, firms receiving investment from QOFs) must meet regulatory criteria requiring that their core economic activities occur within the boundaries of a designated OZ tract, and property investors are generally legally required to demonstrate "substantial" capital improvements in real estate assets.[7] These regulations were introduced by the Treasury Department to curb tax evasion, and to increase the likelihood that OZ investments spur real economic activity and opportunity for OZ workers and residents.

Private-sector investors estimate that, under a range of plausible assumptions about discount rates and rates of return on OZ capital, investors who maximally leverage the OZ policy incentives may ultimately increase their after-tax return by approximately 40%.[8] The OZ program thus introduces a large spatial capital tax wedge that varies sharply even across neighborhoods within the same city.

### Summary Statistics

[6] IRS provides further details on capital gains tax rates here.

[7] The IRS provides further details on these regulatory requirements here .

[8] See e.g. Weinstein and Glickman (2020).

Table 1.1: Tract Summary Statistics

| | (1) OZ Tracts | (2) Eligible, Not Chosen | (3) All Tracts | (4) Diff (1-2) | (5) p-val |
|---|---|---|---|---|---|
| Population | 3,999 | 4,041 | 4,326 | -42 | 0.07 |
| | (1,908) | (1,860) | (2,129) | | |
| Rural | 0.21 | 0.18 | 0.16 | 0.03 | 0.00 |
| | (0.41) | (0.39) | (0.37) | | |
| Median Age | 35.6 | 35.9 | 38.9 | -0.3 | 0.00 |
| | (7.3) | (7.5) | (7.7) | | |
| % White | 0.58 | 0.63 | 0.73 | -0.05 | 0.00 |
| | (0.29) | (0.28) | (0.25) | | |
| % Black | 0.26 | 0.21 | 0.14 | 0.05 | 0.00 |
| | (0.30) | (0.27) | (0.22) | | |
| % Foreign Born | 0.15 | 0.17 | 0.14 | -0.02 | 0.00 |
| | (0.16) | (0.17) | (0.14) | | |
| % High School | 0.49 | 0.51 | 0.58 | -0.02 | 0.00 |
| | (0.12) | (0.12) | (0.12) | | |
| % College | 0.11 | 0.12 | 0.20 | -0.01 | 0.00 |
| | (0.08) | (0.09) | (0.14) | | |
| Median Family Income | 38,978 | 46,000 | 68,357 | -7022 | 0.00 |
| | (15,401) | (16,317) | (33,997) | | |
| % Poverty Rate | 0.29 | 0.25 | 0.16 | 0.04 | 0.00 |
| | (0.13) | (0.11) | (0.12) | | |
| Median Home Values (1000s) | 696 | 748 | 1,021 | -52 | 0.00 |
| | (495) | (465) | (632) | | |
| Household Gini | 0.46 | 0.44 | 0.42 | 0.02 | 0.00 |
| | (0.06) | (0.06) | (0.06) | | |
| N | 8,638 | 23,699 | 74,001 | | |

*Notes*: Unit of analysis is 74,001 census tracts. Demographic data are from the 2015 5-Year American Community Survey (ACS). Table excludes tracts with missing ACS data. The table shows means and standard deviations in parentheses. OZ tracts (Column 1) are socioeconomically disadvantaged relative to eligible-but-not-chosen tracts (Column 2), which are in turn disadvantaged relative to the country as a whole (Column 3). Column 4 computes the difference in means between Columns 1 and 2, and Column 5 presents p-values testing the null hypothesis that these means are equal. These data are consistent with the view that policymakers intended to target populations most in need.

Table 1.1 shows tract-level demographic summary statistics to illustrate differences between OZ-eligible tracts (Column 1), OZ-designated tracts (Column 2), and the country as a whole (Column 3). The data are from the 5-Year 2015 American Community Survey, and corresponds to the data used by the IRS and Treasury to determine which tracts were eligibile to be nominated by states as Opportunity Zones. Column 4 shows differences between OZ-designated tracts and OZ-eligible tracts, and Column 5 calculates the relevant p-values. The table shows that designated OZ tracts (Column 1) tend to have lower incomes, home values, and education attainment – and higher poverty rates and non-white population shares – relative to tracts that were eligible for the OZ tax subsidy but were not nominated by the states (Column 2). Eligible tracts are, in turn, socioeconomally disadvantaged relative to the country as a whole (Column 3).

This evidence, corroborated by other researchers, is consistent with the view that both federal and state lawmakers generally intended to target OZ investment toward populations most in need. In the following section, we present new evidence on take-up and the spatial distribution of OZ investment across tracts.

## 1.3 Descriptive Evidence on OZ Investment

### OZ Data in Federal Tax Records

We measure OZ program investment for all businesses that filed an electronic copy of IRS Form 8996 in tax years 2019 and 2020. This form requires QOF funds to identify the firms and census tracts in which they are investing, as well the corresponding dollar values. These data do not yet cover OZ investments from businesses that submitted paper copies of their tax returns, nor do they cover data from subsequent tax years. We provide details about how line items in Form 8996 correspond to definitions in this paper in Appendix A.1.

The first two columns of Table 1.2 show that QOF businesses reported approximately \$26.7 billion in OZ-subsidized capital investment flows in 2019 and an additional \$14.8 billion in flows in 2020, for a cumulative total of \$41.5 billion by the end of 2020.

Table 1.2: Investment in Opportunity Zones Over Time

| Tax Year | Total Annual Flows (mil) | Cumulative Flows (mil) | Cumulative E-Filed (mil) | E-Filed Share | Tracts (#) | QOF (#) | QOB (#) |
|---|---|---|---|---|---|---|---|
| 2019 | 26,670 | 26,670 | 18,779 | 0.70 | 1,347 | 2,526 | 2,224 |
| 2020 | 14,789 | 41,459 | 32,504 | 0.78 | 3,242 | 3,514 | 3,281 |

*Notes*: Data in the first two columns are from the universe of 8996 QOF returns. Data in all remaining columns cover only electronically-filed 8996 tax returns. The final four columns show cumulative values. We provide additional details about these data in Appendix A.1.

We calculate that the electronic Form 8996 returns in our analysis sample cover approximately 78% of the cumulative value of QOF investments in tax year 2020. Among the sub-sample of e-filers for which we have detailed data, the number of OZ tracts receiving any investment more than doubled from 2019 and 2020 and the cumulative number of QOFs (investors) and QOBs (investees) increased substantially as well.

That our sample is limited to eletronic filers naturally invites the question: how representative are electronic filers of all 8996 filers? Since the OZ program and its associated tax forms are new, historical patterns provide limited guidance in assessing possible differences between electronic and paper filers. Even if electronic filers on average make similar investment decisions to paper filers, the descriptive estimates presented below should nevertheless be interpreted as providing a lower bound on aggregate OZ investment by tax year 2020.

Caveats aside, the existing data from electronic filers provide an emerging picture of OZ investment to date. In what follows, we describe the data sources, present aggregate summary statistics, break out investment by industry and geography, and correlate investment flows with demographic, industry, and firm characteristics. Overall, the data show that OZ investment is highly spatially concentrated, is directed toward the real estate and construction sectors, and gravitates toward tracts with relatively higher educational attainment, income, density, and pre-existing upward income and population growth trends.

### OZ Investment is Spatially Concentrated

We begin with a broad overview of the Form 8996 data. Table 1.3 shows that businesses filing electronic 8996 returns reported approximately $32.5 billion in cumulative OZ-subsidized capital investments by 2020. In total, we observe 3,953 QOF funds investing in 3,677 QOB businesses across 3,242 OZ census tracts. Panel A reveals that this investment is highly concentrated in a small share of tracts: in fact, 5,522 of 8,764 OZ tracts in our sample (63%) appear to receive zero investment. We also find that approximately $3.2 billion of this investment (10%) is not associated with a designated Opportunity Zone tract; this may reflect regulatory guidance allowing QOF funds to invest a fraction (10%) of their assets in non-OZ tracts, as well as taxpayer or administrative error.

Table 1.3: Investment in Opportunity Zones by Type

| | (mil) | Share | # OZ Tracts | # QOF | # QOB |
|---|---|---|---|---|---|
| **Panel A: By Tract Type** | | | | | |
| OZ tract, >0 investment | 29,267 | 0.90 | 3,242 | 3,514 | 3,281 |
| OZ tract, no investment | 0 | 0 | 5,522 | 0 | 0 |
| Unmatched tract | 3,236 | 0.10 | n.a. | 511 | 501 |
| **Panel B: By Investment Type** | | | | | |
| Stock or Partnership Interest | 32,478 | 1.00 | 3,573 | 3,953 | 3,677 |
| Owned or Leased Property | 25 | 0.00 | 6 | 0 | 0 |
| **Panel C: By QOB Entity Type** | | | | | |
| Partnership | 23,217 | 0.71 | 3,102 | 2,623 | 2,149 |
| Other | 9,286 | 0.29 | 1,121 | 1,558 | 1,528 |
| **Total** | **32,504** | **1.00** | **3,242** | **3,953** | **3,677** |

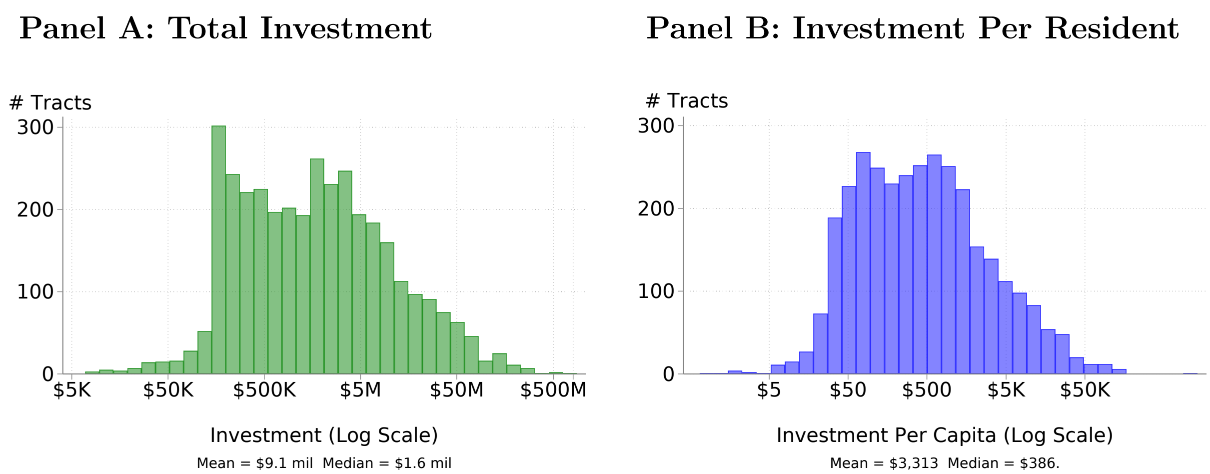
*Notes*: Data based on IRS records of Form 8996 from electronic filers in tax year 2019. Columns need not always sum to totals. The table shows that OZ investment is highly concentrated in a small number of Opportunity Zones and in partnership interests.

Panel B of Table 1.3 shows that OZ investment is virtually entirely concentrated in equity and partnership interests (100%) rather than property (0%). In Panel C we identify the legal entity type of the QOB businesses receiving investment from OZ funds, and confirm that this investment is overwhelminly concentrated in partnerships (71%). Structuring a business as a partnership offers owners several legal and economic advantages over alternative entity types, but in our setting perhaps the most important is that partnerships allow taxable depreciation deductions (such as those resulting from real estate depreciation) to flow through to the investors.

Although Table 1.3 shows that the vast majority of OZ tracts do not attract any investment, the tracts that do receive investment report large and economically significant amounts. Panel A of Figure 1.1 shows the distribution of investment for tracts that received at least $5,000 by 2020, and Panel B shows these values normalized on a per-resident basis. Among these tracts, median OZ investment is $1.6 million, or $386 per resident. Overall, the distribution of investment across all OZ tracts is highly skewed, such that the top 5% of tracts receive 78% of total investment, and the top 1% of tracts receive 42% of total investment.

In summary, many neighborhoods have received no OZ investment, but for those that do, the amount of investment can be quite large. Low tract-level take-up rates may help to explain estimates of modest or null intent-to-treat effects in existing research (e.g., Chen, Glaeser, and Wessel 2023; Atkins, Hernandez-Lagos, Jara-Figueroa, and Seamans 2020; Freedman, Khanna, and Neumark 2021). Among tracts where QOFs do report investment, the extent to which these financial investments translate into physical capital expenditures

Figure 1.1: Tract-Level Distribution of OZ Investment




*Notes*: N=3,230 census tracts with at least $5,000 of OZ investment. Data based on electronic filers of IRS form 8996 in tax year 2020. Panel A shows the distribution of total investment, and Panel B shows the distribution of investment per capita. We use log scales on the x-axes and exclude tracts with less than $5,000 of investment to improve the data visualization. The figures underscore that OZ investment is highly spatially concentrated: the top 5% of OZ tracts receive 78% of total investment, and the top 1% of tracts receive 42% of total investment. As shown in Table 1.3, the bottom 63% of tracts receive zero investment. Among tracts that receive >0 investment, the median investment of $386 per resident is economically large relative to existing federal place-based programs.

that would not have occured in the absence of the OZ tax subsidy is a question we are investigating in ongoing research.

### Industry Composition of OZ Investment

We next examine how OZ investment varies across industires. Panels A and B of Table 1.4 show the NAICS-2 composition of QOF funds and QOB businesses, respectively. Both QOF investor funds and recipient QOB firms are mainly in the business of real estate, with smaller but significant shares in related industries such as construction, finance, and management. Panel B shows that approximately 52% of OZ dollars are invested in real estate firms, while 11% is invested in construction firms, and 9% in finance. In Appendix Table A.2, we further decompose industry composition of funds and recipient firms using finer 6-digit industry codes, and show that both residential and non-residential real estate businesses attract considerable OZ investment.

Table 1.4: Industry Composition of Funds and Recipient Firms

**Panel A: QOF Investor Funds**

| NAICS | Industry | # Funds | (mil) | Share |
|---|---|---|---|---|
| 53 | Real Estate, Renting, and Leasing | 1,943 | 13,738 | 0.42 |
| 52 | Finance and Insurance | 1,238 | 9,376 | 0.29 |
| 23 | Construction | 384 | 1,805 | 0.06 |
| 55 | Management of Companies | 200 | 1,724 | 0.05 |
| – | Other | 188 | 5,860 | 0.18 |
| | **Total** | **3,953** | **32,504** | **1.00** |

**Panel B: QOB Firms Receiving Investment**

| NAICS | Industry | # Targets | (mil) | Share |
|---|---|---|---|---|
| 53 | Real Estate, Renting, and Leasing | 2,066 | 16,778 | 0.52 |
| 23 | Construction | 448 | 3,733 | 0.11 |
| 52 | Finance and Insurance | 326 | 2,985 | 0.09 |
| 55 | Management of Companies | 81 | 888 | 0.03 |
| 72 | Lodging and Restaurants | 111 | 842 | 0.03 |
| 54 | Professional Services | 73 | 823 | 0.03 |
| 31 | Manufacturing | 54 | 325 | 0.01 |
| – | Other | 214 | 4,220 | 0.13 |
| – | Unknown | 304 | 1,909 | 0.06 |
| | **Total** | **3,677** | **32,504** | **1.00** |

*Notes*: Data based on electronic filers of IRS form 8996 in tax years 2019 and 2020. Panel A shows the number of QOF investor funds, dollar values, and dollar share of OZ investment, and Panel B shows analogous measures for the QOB firms receiving investment. QOF investment is highly concentrated in real estate, with smaller but significant shares in related industries such as construction, finance, and management..

Several factors help to explain why OZ funds exhibit a preference for real estate investments. First, real estate is a highly capital-intensive sector. The Bureau of Economic Analysis estimates that residential and non-residential structures account for approximately 39% of annual private fixed asset investment.[9] Second, investment in real estate is geographically versatile and thus well suited to benefit from a tax subsidy that applies broadly to hetergeneous neighborhoods. Virtually any area of the country with population growth is likely to need new housing and commercial structures.[10] By contrast, other capital-intensive sectors such as oil refineries or manufacturing plants are unlikely to sprout up, for example, in dense urban areas. Third, specialists in the real estate sector may be uniquely situated to facilitiate financing and reduce transaction costs associated with investment. This specialization is reflected in part by the large number of real estate funds in Panel A of Table 1.4. Similarly, local real estate developers may have portfolios of potential projects that can be prioritized or de-prioritized depending on the price and availability of capital financing.

[9]See BEA Table 5.10: Changes in Net Stock of Produced Assets (Fixed Assets and Inventories).

[10]In Section 1.3 we show that population growth is indeed a strong predictor of OZ investment.

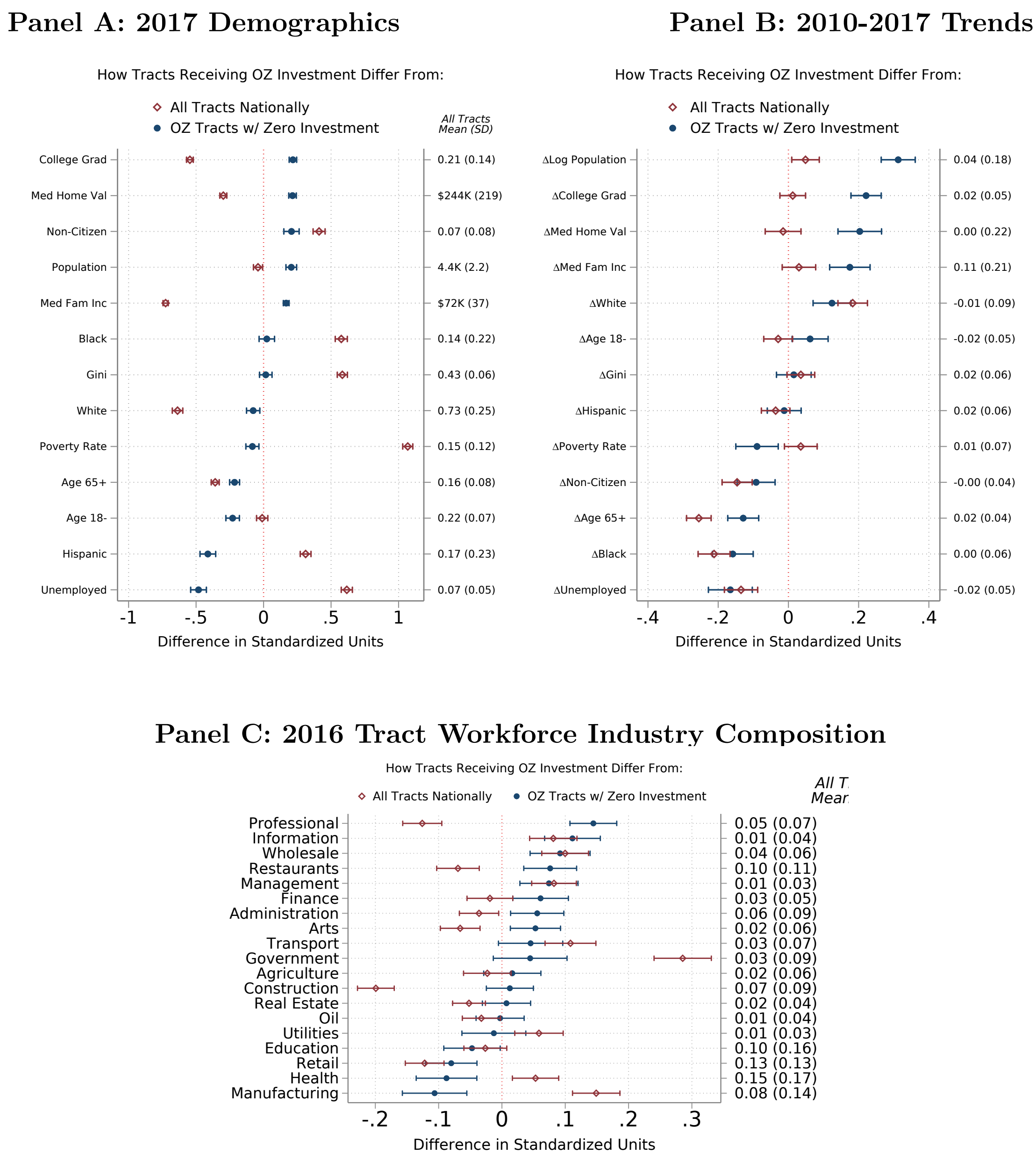
Fourth, the widespread availability of data on real estate price trends may help investors to identify investments likely to have higher returns and lower risk. Finally, legal and regulatory considerations also favor investments in real estate over other sectors; see Hadjilogiou, Lutz, and Bruno (2021) for a review.

### Demographic Correlates of OZ Investment

In this section we explore how OZ investment is correlated with tract demographics. Figure 1.2 compares demographic characteristics for three groups of census tracts: (1) OZ tracts receiving positive investment from QOFs; (2) OZ tracts receiving zero investment from QOFs; and (3) all tracts nationally. In Panels A and B, these demographic characteristics are computed from the 2017 American Community Survey, while in Panel C we use data from the 2016 Census Longitudinal Employer-Household Dynamics LODES data. We standardize the variables to have mean zero and standard deviation one, and report how OZ tracts that receive QOF investment differ in standardized units from all tracts and from OZ tracts that do not receive investment. The confidence bars report 95% confidence intervals computed using robust standard errors.

Panel A of Figure 1.2 shows that, relative to the general population, OZ tracts that receive investment have on average fewer residents with a college degree, lower incomes, and higher poverty rates. Conversely, when compared to other OZ tracts with zero investment, tracts that receive investment have relatively high educational attainment, home values, and incomes, as well as lower unemployment and higher shares of prime-age workers. The interpretation of the coefficients is, for example, that the share of college graduates in tracts that received OZ investment is on average 0.55 standard units lower than the national average, and 0.22 standard units higher than in OZ tracts that did not receive any investment. For reference, we report the raw mean and standard deviation of these variables for all tracts on the right-hand side of the figure, and also report the raw means for each of these outcomes and groups of tracts in tables in Appendix A.1.

Figure 1.2: Demographic Correlates of OZ Investment




*Notes*: N=74,288 census tracts. The figure shows average differences in demographic characteristcs for three groups of census tracts: (1) OZ tracts receiving positive investment; (2) OZ tracts receiving zero investment; and (3) all tracts nationally. The data in Panels A and B are from the 2017 and 2010 5-Year ACS, and the data in Panel C are from 2016 Census Longitudinal Employer-Household Dynamics LODES data. All variables are standardized to have mean zero and standard deviation one. Error bands show 95% confidence intervals with robust standard errors. The coefficients imply, for example from Panel A, that the share of college graduates in tracts that received OZ investment is on average 0.55 standard units lower than the national average, and 0.22 standard units higher than in OZ tracts that did not receive any investment. Among OZ tracts eligible for the tax subsidy, QOFs typically invested in neighborhoods with higher educational attainment, income, demographic change, and concentrations of professional and amenity services. We also present the raw means of these variables for each group of census tracts in Appendix Table A.1.

Panel B of Figure 1.2 correlates OZ investment with 2010-2017 demographic trends. Among OZ tracts, QOF funds invested in neighborhoods where incomes, population, and the share of college educated residents have increased sharply over the past decade, and where the non-white and elderly share of the population have declined. However, the figure also shows that trends in these neighborhoods are similar to trends in the rest of the US. As in Panel A,these results overall point towards investment in tracts with relatively greater pre-existing economic opportunity.

Lastly, Panel C of Table 1.2 compares the 2016 industry composition of the workforce across these three groups of tracts. On average, tracts with higher 2016 shares of workers in professional and amenity services – such as finance, management, restaurants, and the arts – attracted more OZ capital by 2020 relative to other OZ tracts. By contrast, QOF funds were less likely to invest in OZ tracts with higher workforce shares in healthcare, manufacturing, education, or retail. Relative to all tracts, tracts receiving OZ investment have a significantly larger share of government workers and a smaller share of construction workers.

Taken together, the three panels in Figure 1.2 paint a consistent picture. Although all OZ tracts are relatively disadvantaged in comparison to the rest of country, the tracts that received investment were the least disadvantaged of those granted OZ status. Moreover, the preliminary descriptive evidence suggests that OZ capital may disproportionately benefit a narrow subset of tracts in which economic conditions were already improving prior to implementation of the tax subsidy.

In Appendix Figure A.2, we show variations on Panels A and B to illustrate how the characteristics of tracts receiving OZ investment have changed from 2019 to 2020, relative to OZ tracts that did not receive investment in either year. While QOF investment in 2020 continued to favor relatively well-off OZ neighborhoods, the figure shows that this pattern was attenuated relative to 2019.

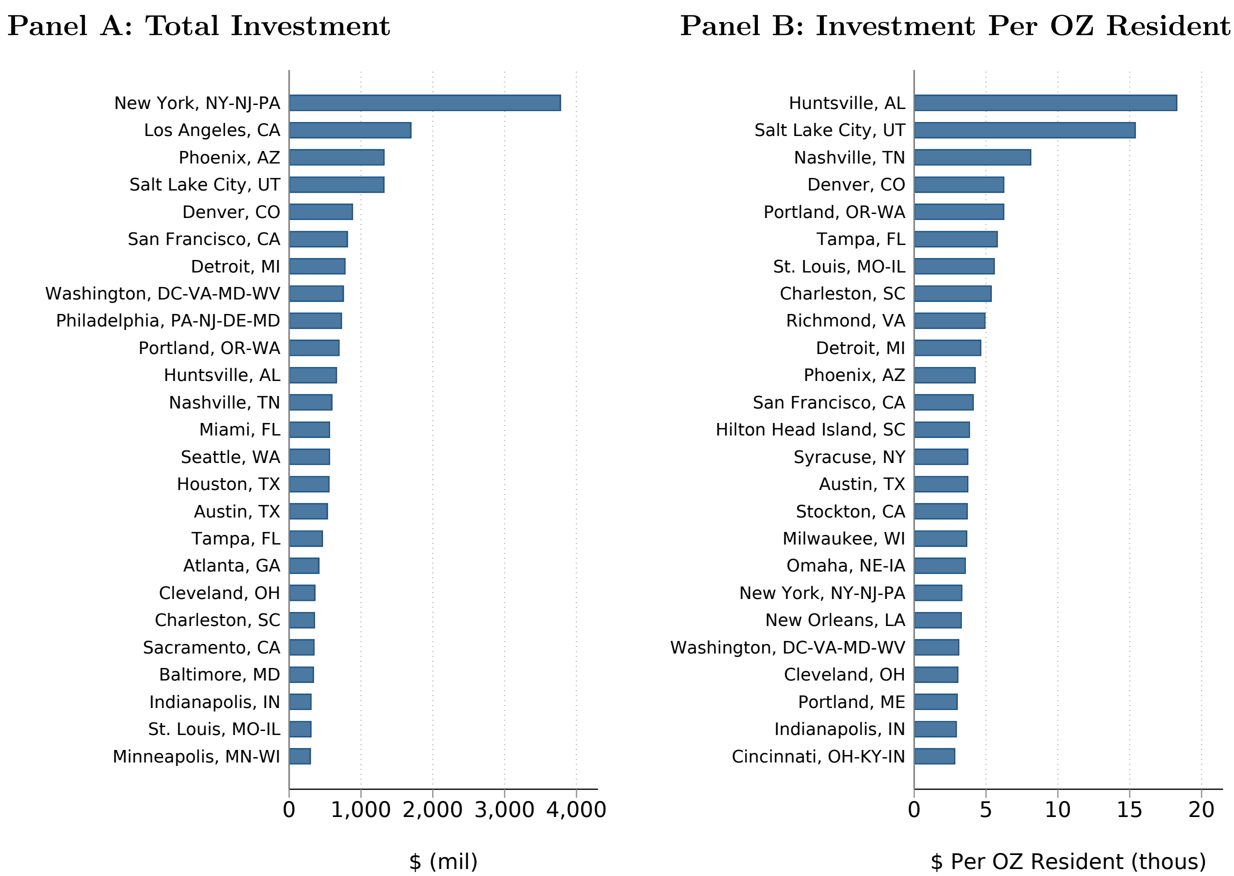
### Geographic Patterns in OZ Investment

We now explore geographic patterns in OZ Investment. Panels A and B of Figure 1.3 show total investment and investment per OZ resident, respectively, for the top 25 commuting zones.[11] The diverse list of commuting zones in Panel A reflects that QOF funds reported investment in virtually every region of the country and, not surprisingly, that the most populous commuting zones such as New York and Los Angeles generally received the most investment. Panel B shows that, on a per capita basis, mid-size commuting zones like Salt Lake City, Nashville, and Tampa received the most investment, although QOF investors did not neglect larger commuting zones like Denver, San Francisco, and Phoenix. OZ investment in Hunstville, Alabama is especially large and appears to be an outlier relative to other eligible OZ labor markets.

In Figure 1.4 we zoom in at a finer level of detail and map the spatial distribution of OZ investment in six illustrative cities: Brooklyn, Philadelphia, Detroit, Nashville, Hunstville,

[11] Appendix Table A.3 shows these statistics for the top 50 commuting zones.

Figure 1.3: OZ Investment in 25 Top Commuting Zones




*Notes*: Panel A shows total OZ investment by commuting zone, and Panel B shows investment per OZ-resident, normalizing by the population of tracts with >0 investment. We compute investment from electronically-filed business tax records of Form 8996 in tax years 2019 and 2020. The panels present data for the top 25 commuting zones, exluding those with few QOF funds and/or QOB businesses to protect taxpayer privacy. The figure shows that QOF's invested in diverse labor markets in nearly every region of the country. Appendix Table A.3 shows these statistics for the top 50 commuting zones.

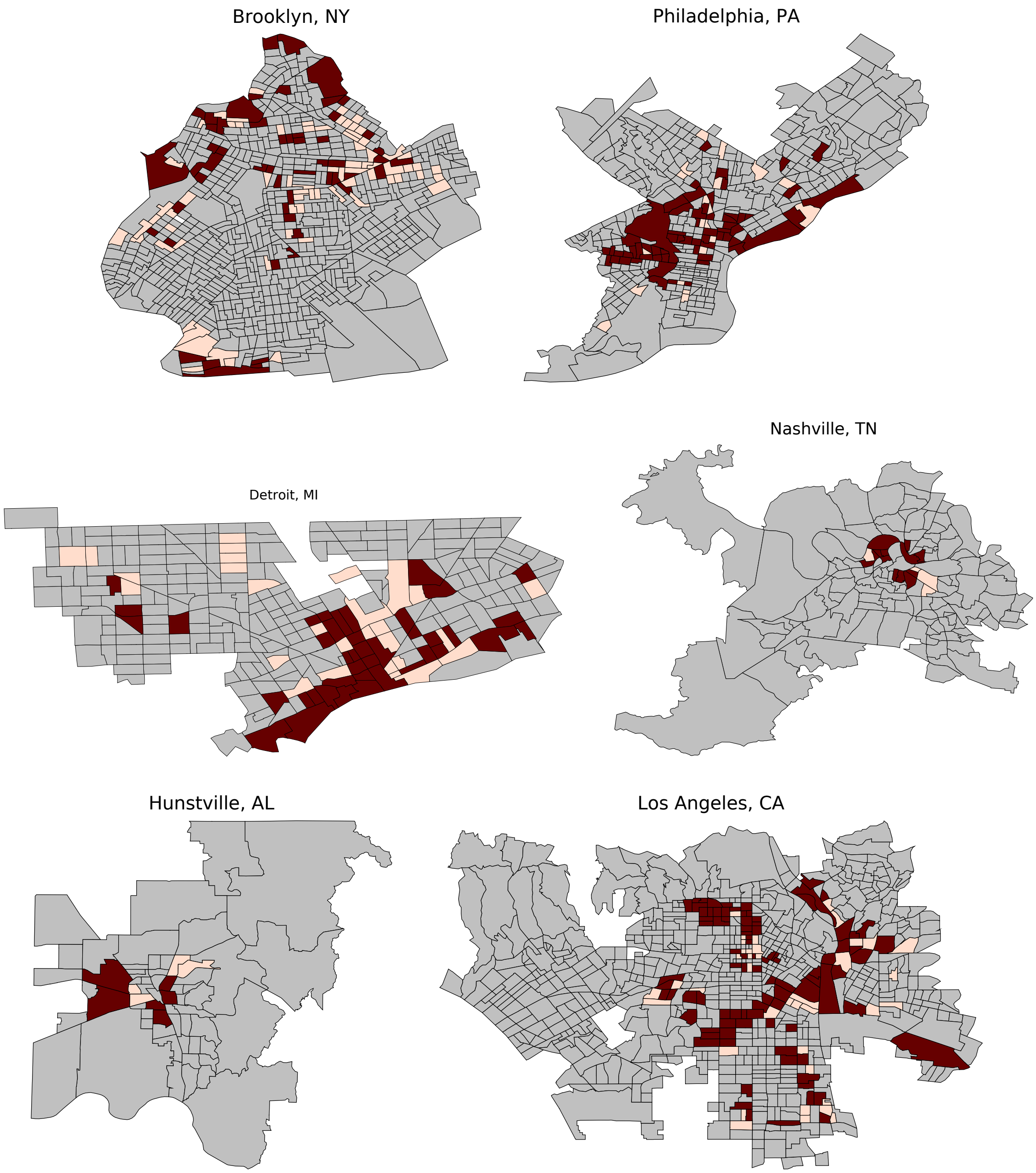
and Los Angeles. Dark red areas on the maps indicate OZ tracts with >0 investment, and pink areas indicate OZ tracts that receive zero investment. Grey areas indicate tracts that are not Opportunity Zones. These illustrative examples suggest that OZ investment gravitated toward dense city centers and central business districts (or, in Brooklyn, the neighborhoods most proximate to Manhattan).

We confirm generalizable relationships between investment, population density, and distance from the city or commuting zone center in Panel A of Figure 1.5, which shows how tracts receiving positive OZ investment differ in economic geography from all tracts and from

OZ tracts that did not receive investment. Tracts receiving OZ investment are on average more densely populated and urban relative to other OZ tracts and relative to non-OZ tracts. These tracts are also closer to the centers of commuting zones relative to other tracts.[12] OZ investment is also decreasing in the distance between investor funds and OZ tracts. Panel B of Figure 1.5 shows the distance distribution between OZ funds and the census tracts in which they invest, and Panel C plots fund-by-tract-level investment against the log distance between funds and OZ tract. Consistent with empirically and theoretically documented linkages between spatial proximity and economic activity, investment between QOFs and QOBs is declining in distance. In the next section we further explore how the locations of not only QOB businesses, but also QOF investors, may have implications for understanding the geographic incidence of the OZ program.

[12]We define the commuting zone center as the census tract with the largest number of jobs in the municipality (commuting zone) in which the tract located.

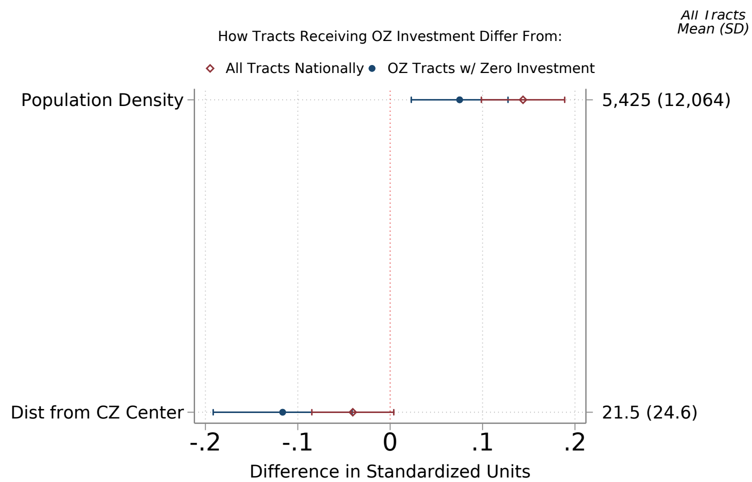
Figure 1.4: Mapping OZ Investment in Six Illustrative Cities



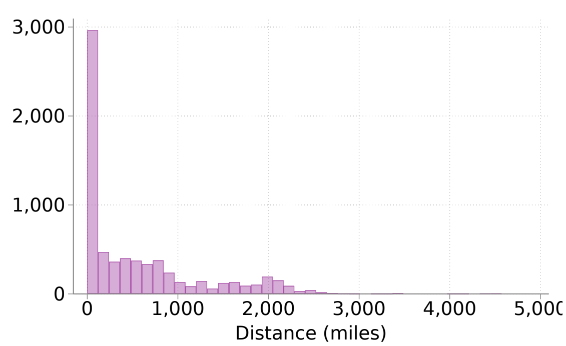

*Notes*: Red areas on the maps indicate OZ tracts with >0 QOF investment, and pink areas indicate OZ tracts that receive zero QOF investment. Grey areas indicate tracts that are not Opportunity Zones. We compute investment from electronically-filed business tax records of Form 8996 in tax years 2019 and 2020. These illustrative examples suggest that OZ investment gravitated toward dense city centers and central business districts (in the case of Brooklyn, investment appears concentrated in the neighborhoods most proximate to Manhattan). We confirm this generalizable relationship in Figure 1.5.

Figure 1.5: OZ Investment, Population Density, and Distance

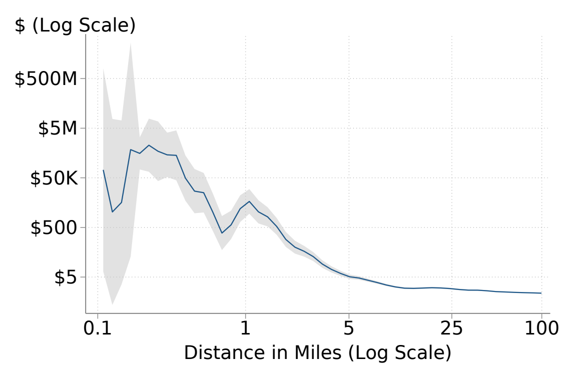
**Panel A: Population Density, Distance from City and CZ Center**




**Panel B: Distance Between Fund-Tract Pairs**




**Panel C: Investment vs. Fund-Tract Distance**




*Notes*: The sample is N=74,288 census tracts. Panel A shows differences in economic geography for three groups of tracts: (1) OZ tracts receiving positive investment; (2) OZ tracts receiving zero investment; and (3) all tracts nationally. On average, QOF funds invest more heavily in densely populated, urban neighborhoods closer to city and commuting zone centers. Panel B reports the distribution of distances between fund-tract pairs, and Panel C plots fund-level investment against distance from OZ tracts using a smooth polynomial fit. The plots highlight that OZ investor funds tend to be located (or, set up ex-post) in locations very close to OZ tracts.

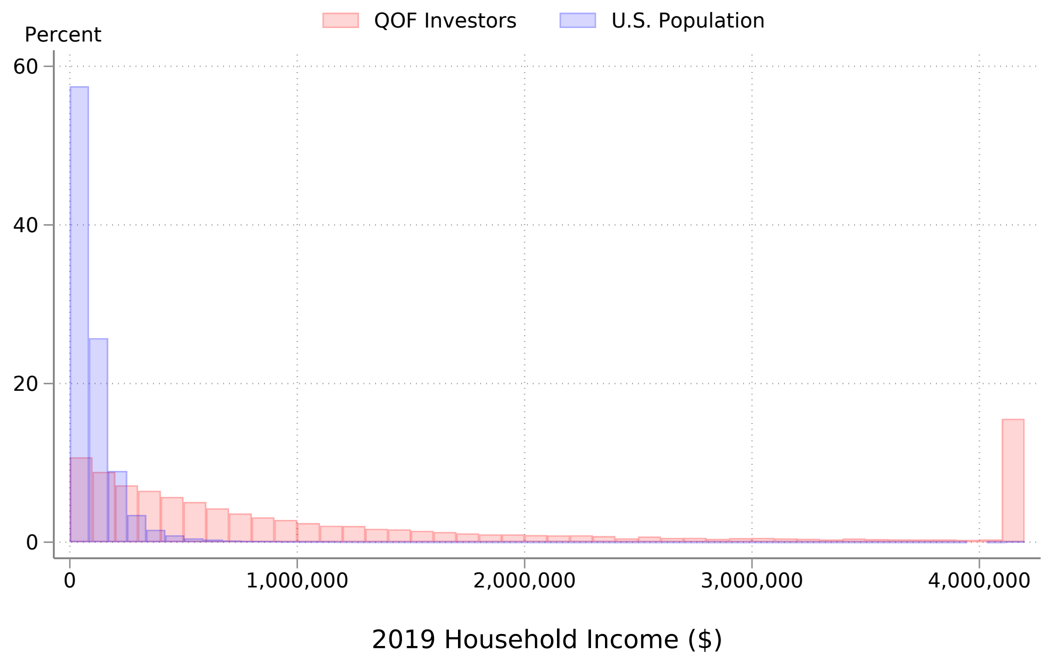
### Income and Geography of QOF Investors

We have focused so far on describing the economic and sectoral characteristics of QOF investments, as well as the demographic and geographic characteristics of neighborhoods that receive those investments. Apart from residents of OZ neighborhoods, the incidence of the OZ program will naturally also fall in part on QOF investors, who are likely to most directly benefit from the tax incentives described in Section 1.2. In this section we briefly describe the income and geographic profiles of QOF investors in the available data.

To estimate the household income of QOF investors, we link QOF partnerships to their partners using the universe of 1065-K1 information return filings, which must be reported to IRS annually for all partners. Partnership ownership structures can be complex — for example, higher-tier partnerships may include both individuals and/or lower-tier partnerships as partners — rendering a complete match of these data to be difficult. Nevertheless, we are able to match approximately 89% of the partners of higher-tier QOF partnerships to individuals, who we then link to our household income database. In Figure 1.6, we show the distribution of household income for these QOF investors relative to the general US population.

The plot shows that, on average, QOF investors have substantially higher household income relative to the general US population. We estimate 2019 median and average household income for QOF investors to be \$741,000 and \$4,852,000 , respectively — an order of magnitude higher than the national median and average household incomes of\$69,000and \$117,000, respectively. While tax benefits to QOF investors will ultimately depend on the extent to which their investments appreciate in value over time, these results suggest that the direct tax incidence of the OZ program is likely to benefit households in the 99th percentile of the national household income distribution.

Figure 1.6: Income Distribution of QOF Investors and the US Population



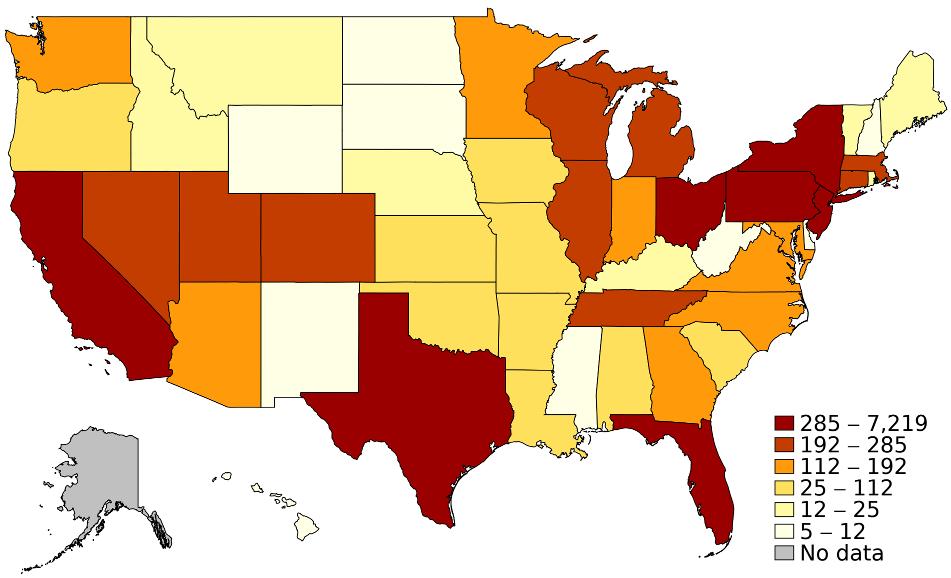

*Notes*: The plot shows the distribution of 2019 household income for QOF investors relative to the general US population. We identify QOF investors by linking QOZ partnerships to their partners using IRS Form 1065-K1, an information return that must be filed annually for all partners. Household income computations are described in Appendix A.1. We winsorize the top 1% of the QOF income distribution to improve the data visualization, and exclude households with negative income. Median and average household income for QOF investors is approximately $741,000 and $4,852,000 , respectively, relative to the national median and average of $69,000 and $117,000 , respectively.

Finally, in Figure 1.7, we link QOF investors to their state of residence, and estimate total the value of QOF investments coming from each state. To perform this computation, we again focus on QOF partnerships, and further make the simplifying assumption that all partners of a fund are equally invested in it. Panel A shows the resulting aggregate QOF investment that we assign to each state, scaled in million of dollars, and shows that the bulk of QOF dollars flow from populous and relatively wealthy states such as California, Texas, Florida, New York, and New Jersey.

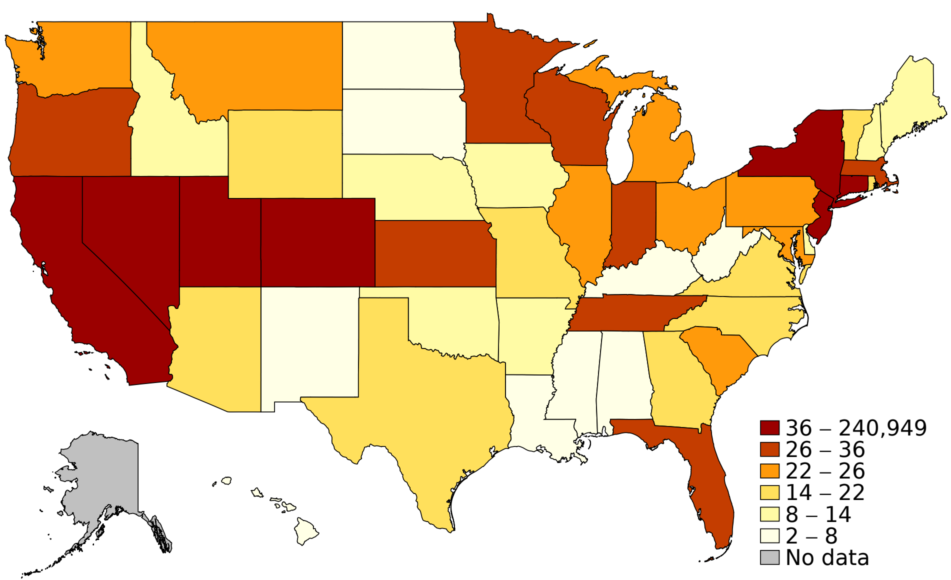
Panel B shows these aggregate totals scaled by state population, and shows that investors disproportionately reside in the Northeast and Pacific Coast, as well as a few states in the Mountain West such as Nevada, Utah, and Colorado. The maps highlight that the geographic incidence of the OZ program depends not only on which OZ tracts receive QOF investments, but also on the residential locations of QOF investors.

Figure 1.7: Geography of QOF Investors

**Panel A: Total QOF Investment (mil $), by Investors' State of Residence**




**Panel B: QOF Investment Per Capita, by Investors' State of Residence**




*Notes*: We link QOF investors to their state of residence by linking QOZ partnerships to their partners using IRS Form 1065-K1, an information return that must be filed annually for all partners. Panel A shows the resulting aggregate QOF investment that we assign to each state, scaled in million of dollars, and shows that the bulk of QOF dollars flow from populous and relatively wealthy states such as California, Texas, Florida, New York, and Illinois. Panel B shows these aggregate totals scaled by state population, and shows that investors disproportionately reside in the Northeast and Pacific Coast, as well as a few states in the Mountain West and Great Plains such as Nevada, Utah, and Colorado. The maps highlight that the geographic incidence of the OZ program depends not only on which OZ tracts receive QOF investments, but also on the residential locations of QOF investors.

## 1.4 New Panels from IRS Microdata

We construct new annual panels of individual tract-level outcomes using rich data from federal tax records. We provide an overview of our data sources below, and provide more detailed discussion of our data processing in Appendix A.1. We then show how these measures correlate with the available data on OZ investment.

### Individual- and Business-Level Federal Tax Records

We leverage the universe of de-identified federal individual- and business-level tax records from the Internal Revenue Service (IRS) to construct novel tract- and block-level measures of economic activity. On the individual side, our work builds on Larrimore, Mortenson, and Splinter (2019), who map virtually all individuals residing in the United States to household identifiers using address data from 1040's and information returns. While taking stringent precautions to protect taxpayer privacy, we use open-source and commercial geocoding services to match household addresses with latitude and longitude coordinates, and to locate households within 2010 census tract boundaries.

We use the individual-level tax data to construct measures of household and family income, poverty, employment, wages, migration, and commuting that closely correspond to analogous measures from publicly available data. The new measures incorporate data from both income tax returns and information returns (such as W2s and 1099s), and thus allow us to observe income even for individuals and households that do not file income tax returns.

On the business side, our sample of firms includes the universe of corporations and partnerships, and excludes self-proprietorships. We link all businesses to their parent companies using the crosswalks constructed by Dobridge, Landefeld, and Mortenson (2019), and geocode them based on the address information provided on the cover form of their annual tax returns. We further link firms to their employees using W2s, and construct firm-level measures of real investment from Form 4562 following Yagan (2015). These measures of real investment capture firm spending on tax-deductible depreciable assets such as buildings, machinery, computer, vehicles, and office furniture.

A limitation of the business tax data is that we are unable to observe the establishment locations of multi-establishment firms. This means, for example, that if a large national retail chain were to purchase new buildings in multiple states, we would be unable to observe the location of such investments. Thus, the firm-level measures must be interpreted with caution. When aggregating the firm-level data, we differentiate firms based on firm size: since smaller firms are less likely to have multiple establisments, they may provide a more geographically accurate picture of local economic conditions even if they are not represenative of all firms.

In total, we geocode more than one billion individual- and business-level tax returns from 2010 to 2019. We aggregate our resulting measures to the census-tract level and, in the following section, evaluate their validity in relation to publicly available datasets. We then correlate these measures with the available evidence on OZ investment.

### Evaluating the New Tract-Level Measures Against Public Data

We probe the validity of our new measures by comparing them with analogous measures from publicly available data. Figure 1.8 compares our tract-level 2017 estimates of income, poverty, and population based on IRS data with survey-based estimates of these measures from the 2017 5-year Census American Community Survey (ACS). The 2017 5-year ACS pools together and averages survey responses from five consecutive years of 1% national population surveys from 2013-2017, which allows the Census to estimate population demographics at the tract-level using larger sample sizes. By contrast, our IRS-based measures are based on the universe of federal tax returns from a single tax-filing year.

In Panels A and B, we use binscatter plots to compare our tract-level IRS measures of median household income (MHI) and median family income (MFI), respectively, with the ACS data. Each point in these plots represents a simple average of an approximately equal number of census tracts. The plots also report the regresssion coefficient, standard error, and R-squared obtained from regressing the IRS measure on the ACS measure using OLS. The slopes of the lines are close to one, implying that a 1% increase in the ACS income is on average associated with an approximately 1% increase in the IRS income. Our IRS-based estimates are systematically higher than the ACS estimates, due primarily to the fact that our IRS measures are based only on data from tax-year 2017, whereas the ACS is based on five-year averages from 2013-2017. The upward level-shift thus represents income growth and inflation relative to the ACS measure.

Figure 1.8: IRS Measures vs. ACS Measures


*Notes*: N=72,349 census tracts with non-missing IRS and ACS measures. The figures compare 2017 5-year ACS tract-level outcomes (horizontal axis) with their corresponding 2017 IRS measure (vertical axis). The blue line shows the line of best fit, and the red line shows the 45-degree line. In Panels A and B, our IRS measures of median household and family income are systematically higher than the ACS measures, since the latter represent five-year 2013-2017 pooled averages whereas the former are based on data only from 2018. We find higher income at the bottom of the tract-level income distribution, due to underreporting of wage and private-retirement income among low-income households in ACS relative to what we observe from information returns in IRS data. This pattern is also reflected in systematically lower poverty rates in our IRS-based measures relative to ACS in Panel C. In Panel D, we find modestly lower population counts in our sample relative to the ACS, driven by households that we are unable to locate from our geocoding procedure.

Panels A and B also reveal that the IRS data yield higher estimates of income at the lower end of the tract-level distribution relative to ACS. This difference reflects underreporting of wage and private-retirement income among low-income households in ACS relative to what we observe from information returns in IRS data (Bee and Rothbaum, 2017; Larrimore, Mortenson, and Splinter, 2020). Consistent with this result, in Panel C we estimate systematically lower poverty rates in the IRS data relative to the survey-based ACS measures.

Finally, Panel D compares our IRS population sample with the ACS estimate of tract population. The gap between the IRS and ACS population estimates is driven by individuals for whom we are unable to assign a census tract using the geocoding procedure discussed in Appendix A.1. Overall, our geocoding procedure captures approximately 81% of the total US population, and approximately 85% of the population that does not report a PO Box address on their tax returns. The close alignment of the IRS- and ACS-based measures of tract-level income in Panels A and B suggest that any biases resulting from non-random biases in the geocoding procedure are likely to be small.

As a final validity exercise, we calculate employment totals by census tracts of residence and workplace location. We compare these counts from our IRS measures with those available in the census LODES data, shown as binscatter plots in Figure 1.9. In Panel A, employment counts by tract of residence align well with the corresponding counts from the LODES data. We underpredict employment by residence for areas with little employment, reflecting poorer geocoding coverage among individuals in sparsely populated areas. In Panel B, employment counts by tract of workplace are highly correlated with those seen in the LODES data, although a sizeable gap exists between the two estimates. This gap reflects that we only tabulate workplace employment counts for small businesses with 1-49 employees – that is, businessesthat are more likely to have only a single establishment and whose employees are thus more likely to work at the same physical location as the address reported on the firm's tax filings. While differences between administrative and survey sources are natural, we find the high R-squared in both plots to be a reassuring signal of the quality of geocoding.

Figure 1.9: Employment in IRS and LODES Data

**Panel A: Employment by Residence** **Panel B: Employment by Workplace**


*Notes*: N=71,809 census tracts with non-missing IRS and LODES data. Panels A and B compare tract employment counts by residential and workplace tract locations, respectively, using our IRS measures and measures from the Census LODES data. The red line is a 45-degree line, and the blue line is the line of best fit. Panel A plots employment counts based on employees' tract of residence. Panel B plots employment counts based on employees' tract of workplace. Our IRS-based measure of workplace employment only covers small businesses with 1-49 employees, since the workplace location data for these businesses is likely to be more reliable; this definitional difference leads to a consistent gap between the IRS and LODES-based measures of workplace employment. Neverthless, the high correlations between the IRS and LODES measures in both panels lend credence to the validty of the geocoding procedure.

### IRS Correlates of OZ Investment

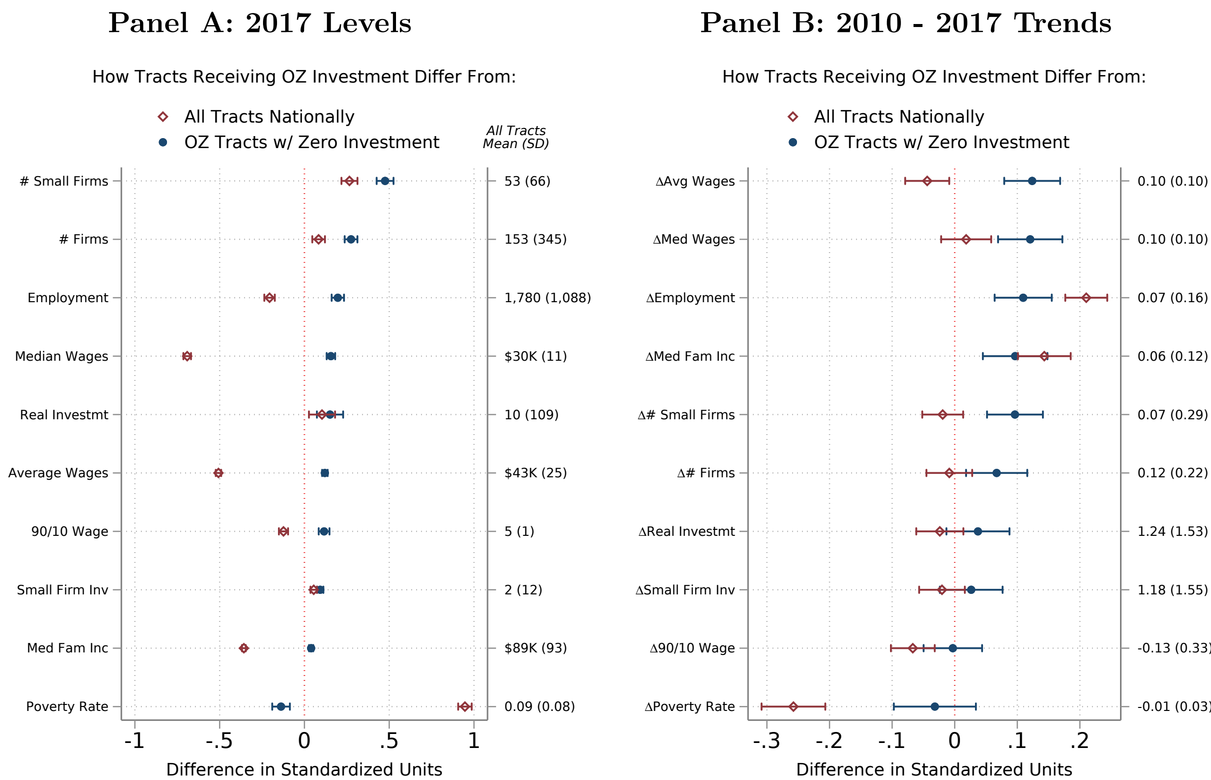
Panels A and B of Figure 1.10 perform the same analysis as in Section 1.3 using our IRS measures. Panel A uses IRS-based measures from 2017, while Panel B uses changes in those measures from 2010 to 2017.[13] As before, we standardize the variables to have mean zero and standard deviation one, and report how OZ tracts that receive QOF investment differ in standardized units from all tracts (in red) and from OZ tracts that do not receive investment (in blue). The confidence bars report 95% confidence intervals computed using robust standard errors.

The evidence presented in Figure 1.10 is broadly consistent with the evidence from Section 1.3. Among OZ-designated tracts, QOF funds invested in neighborhoods with higher wages, lower poverty rates, more employment, more firms, and higher levels of real investment. Still, these tracts receiving investment are economically disadvantaged relative to tracts nationally. Panel B uses 2010 to 2017 changes in the IRS measures to assess the extent to which OZ investment is correlated with recent neighborhood-level trends. While the magnitudes are smaller than those seen in Section 1.3, we find that QOF investment favored neighborhoods with higher income and firm growth. These patterns are most pronounced when the comparison group is OZs with no investment, but OZs also had higher rates of employment and median family income growth relative to all tracts nationally. The raw means for these figures can be found in Appendix A.1.

This evidence suggests that OZ tracts receiving investment from QOF funds were experiencing substantially different trends in economic activity relative to all tracts nationally and relative to OZ tracts that did not receive investment. A natural implication is that research designs that compare trend growth in OZ and non-OZ tracts to assess the causal impacts of the policy must be interpreted with care and caution. Comparable tracts should be balanced on a broad set of demographic and economic characteristics and trends to avoid spuriously conflating pre-existing trends with the causal effects of the OZ tax subsidy.

[13] For median family income and poverty rates, we use a shorter difference of 2015-2017, since we have not yet extended our IRS sample sample back to 2010.

Figure 1.10: IRS Correlates of OZ Investment




*Notes*: N=74,001 census tracts. The figure shows differences in IRS measures for three mutally exclusive groups of census tracts: (1) OZ tracts receiving positive investment; (2) OZ tracts receiving zero investment; and (3) all other tracts. The data in Panels A and B are constructed from IRS microdata as described in Section 1.4 and Appendix A.1. All variables are standardized to have mean zero and standard deviation one. Error bands show 95% confidence intervals with robust standard errors. Among OZ tracts eligible for the tax subsidy, QOFs typically invested in neighborhoods with more firms, more employment, higher wages and income, and lower poverty rates.

## 1.5 Conclusion

We provide the first available evidence on the response of QOF investors to the OZ tax subsidy. We emphasize that this evidence is preliminary and does not yet incorporate data from paper tax filers, who we estimate account for approximately 78% of QOF investment dollars. The OZ investment data are based on business tax returns from tax years 2019 and 2020, the first two years that detailed OZ reporting requirements have made this analysis possible. We also emphasize that the patterns of investment described in this paper may evolve over time, perhaps particularly in response to the coronavirus pandemic beginning in 2020.

Caveats aside, the early evidence shows several striking patterns. We find that OZ investments are highly spatially concentrated in a relatively small number of census tracts, and are heavily concentrated in the real estate sector. Among tracts designated as OZs, investors favored neighborhoods with higher income, educational attainment, home values, and pre-existing population and income growth. These neighborhoods have also experienced significant changes in their demographic composition over the past decade, with increasing shares of college educated adults and declining shares of non-white residents. However, tracts that receive OZ investment are nevertheless considerably economically disadvantaged relative to all tracts nationally. We presented evidence consistent with these findings using a broad range of demographic measures from publicly available ACS data, and corroborated the results using a new panel of IRS-based tract-level measures. Finally, we find that the direct incidence of the OZ tax subsidy is likely to benefit taxpayers in the 99th percentile of national income distribution.

Our results help to contextualize findings from other recent studies on Opportunity Zones. As we have noted, a nascent research literature generally finds modest or null intent-to-treat (ITT) effects of the OZ program on neighborhood-level economic outcomes such as real estate prices, employment, job growth (Chen, Glaeser, and Wessel 2023; Atkins, Hernandez-Lagos, Jara-Figueroa, and Seamans 2020; Freedman, Khanna, and Neumark 2021). Our research raises the possibility that these null intent-to-treat effects may be explained by the fact that a majority of OZ tracts have not received any investment from QOF investors. However, existing research does not yet answer the question whether the OZ program induced positive economic changes in the set of neighborhoods that did receive investment from QOF investors.

An important goal for future research, then, is to estimate not only intent-to-treat effects, but also average treatment effects (ATE). Conditional on receiving OZ investment, what are the causal effects of the OZ program on real investment and local labor markets? In other words, to what extent has financial investment from QOF investors translated into business growth, employment, wage growth, and physical capital expenditures that would not have otherwise occured in the absence of the OZ tax subsidy? The answers to these questions will be of central importance public and scholarly understanding of the Opportunity Zone program and of place-based policies more broadly.

# Chapter 2

# Locally Optimal Place-Based Policies: Evidence from Opportunity Zones

## 2.1 Introduction

An individual's outcomes and opportunities vary greatly with where they reside. Neighborhoods that struggle to attract new businesses and infrastructure investment continue to decline (Glaeser and Gyourko, 2005). High-poverty neighborhoods are linked to worsening health in adult residents (Ludwig et al., 2012) and can, in turn, have deleterious effects on the education, job prospects, and criminal behaviour of children who grow up there (Kling et al., 2005; Chetty et al., 2016a; Chetty and Hendren, 2018a,b). Consequently, policymakers have shown new interest in designing programs that boost investment and employment in distressed areas.

Place-based policies have used various instruments to spur economic activity. State-level Enterprise Zones provided tax credits and incentives to businesses operating in high-poverty locations (Papke, 1993, 1994; Neumark and Kolko, 2010). Empowerment Zones subsidized employment for residents that work in designated areas, as well as give block grants for investments and social programs (Busso et al., 2013). On the capital side, the Low-Income Housing Tax Credit was offered to affordable housing developers operating in certain neighborhoods (Baum-Snow and Marion, 2009); the New Markets Tax Credit provides tax benefits for investments in designated low-income communities (Freedman, 2012). However, the evidence on whether place-based policies can actually increase local investment, employment, and wages is mixed (Neumark and Simpson, 2015), and surprisingly little attention has been paid to linking the spatial implementation of such programs (i.e. which neighborhoods receive hiring credits, tax incentives, etc.) with their particular impacts.[1]

[1]Briant et al. (2015) find an important role for urban geography in the economic impacts of the French enterprise zone program. But to my knowledge, no research has empirically modelled the effectiveness of a specific place-based policy under alternative designs.

This paper studies the effectiveness and design of the recently implemented Opportunity Zone (OZ) program. Passed in 2017 as part of the Tax Cuts and Jobs Act, the goal was to subsidize investment in distressed areas. Specifically, the OZ program provides a capital gains tax credit for investments made in more than 8,000 high-poverty neighborhoods across the U.S. Two types of investments qualified: investment in new or existing businesses that largely operate in OZs, or – the focus of this work – investment in the development of properties located in OZs. The Congressional Joint Committee on Taxation estimates that this incentive will reduce tax revenue by an average $3.4 billion per year from 2019 to 2023 (JCT, 2019), a cost significantly larger than that of prior and current national place-based policies. Total investments claiming OZ tax credits are an order of magnitude larger than the predicted federal costs, with $41.5 billion through 2020 alone (Kennedy and Wheeler, 2022). The program's scope and magnitude offer an ideal context for studying whether such policies can drive investment into neighborhoods that have historically lacked it, with attendant benefits to the community.

This paper contributes to the place-based policy discussion in several ways. First, I collect new data on the timing and location of development projects for 47 large U.S. cities. Second, I present novel evidence that the OZ program has had a significant effect on new development in designated neighborhoods. Third, I document the existence of positive spillovers - that is, increases in new development in nearby areas. I show that these two impacts are larger in neighborhoods with more available land to develop, more elastic housing supply, and lower home values. Fourth, I build a spatial-equilibrium model of new construction projects at different locations within a city. The model rationalizes my reduced-form evidence and provides a rich characterization of counterfactual behavior under alternative neighborhood selections for the tax credit. I use the model to describe the city policymaker's optimal approach to choose neighborhoods for OZ designation. I delineate how these optimal choices differ from and improve upon the locations that were actually designated for the tax credit.

To study new real estate development, granular data on new construction in census tracts is necessary.[2] Through a combination of publicly available data and FOIA requests, I construct a novel dataset of monthly counts of new residential and commercial construction projects for nearly 12,000 census tracts. My main outcome throughout the paper is an indicator for whether a census tract has new construction for a residential or commercial building in a given month. The main estimation sample covers a window of roughly four years prior to and three and a half years after the program was announced. I focus on new residential and commercial construction because new development constitutes a real form of investment explicitly targeted by the program and accounts for the vast majority of OZ investment so far (Kennedy and Wheeler, 2022). The tax credit could help mitigate market failures that may arise in new developments through coordination failures (Owens III et al., 2020) and externalities (Fu and Gregory, 2019; Pennington, 2020).

First, I document the direct effect of the tax credit on new development. I employ a

[2]Census tracts are the geographic level at which OZs were designated. Tracts that were approved for the tax-incentive are referred to as "Opportunity Zones."

difference-in-differences design, comparing OZ tracts to other high-poverty neighborhoods that were eligible for the tax credit, but not designated. The program was a surprise, and governors had little time and guidance for designating neighborhoods. I find no evidence of systematic differences in new construction between OZs and comparable areas in the four years leading up to the program.

After OZs were approved, I find a large and immediate effect of the tax credit on new development. My main estimate is a 2.9 percentage point (pp), or 20.5%, increase in the monthly probability of new development. The effects increase over time. I also find that despite the increased supply of housing, median home values also increase 3.4% in OZs by 2020, relative to 2017. The main findings are robust across a battery of alternative designs: adjusting for baseline neighborhood differences through propensity score-reweighting and regression-adjustment; relying on policy variation at the arbitrary cutoffs for program eligibility; and accounting for selection on time-varying unobservables through synthetic control methods.

If the impact of the investment tax credit on new development were constant across geography and time, then there would be little benefit to alternative designations of the tax credit. The empirical evidence suggests that this is not the case. The policy effect is larger in areas with more developable land, with higher local housing supply elasticities, and with lower property values. Furthermore, the policy effect exhibits an inverse U-shaped relationship in the amount of development happening prior to program implementation; that is, neighborhoods with intermediate levels of prior development had the largest response to the tax credit. These sources of heterogeneity will be important factors in modelling counterfactual investment behavior.

Equipped with estimates of the program's direct effect on new development, I consider its indirect effects. The sign of the indirect effect is a priori unclear. New commercial developments improve local services and employment opportunities, which in turn may increase demand for adjacent residential and commercial space. On the other hand, through encouraging new development in targeted neighborhooods, the OZ program might crowd-out nearby development through increasing supply and lowering prices for residential and commercial space (Baum-Snow and Marion, 2009; Asquith et al., 2019b). Having any nearby OZ within 2 kilometers of the OZ centroid is associated with a 1pp (6%) increase in new development; this effect decays towards zero after 3 kilometers. The evidence suggests that in this context, demand externalities far outweigh supply effects.[3] The spillovers are diminishing in the number of nearby OZs, and like the direct effects of the program, are larger in areas with more developable land, higher supply elasticities, and lower home values.

To consider the efficacy of alternative designations for the tax credit, estimates of the

[3]This has been found in other contexts as well (Pennington, 2020). Restaurants are highly spatially correlated despite the price competition (Handbury and Couture, 2020), reflecting strong demand externalities (Leonardi and Moretti, 2022). These findings are also consistent with a large literature finding localized spillovers in housing markets. The roles of public housing (Diamond and McQuade, 2019b), large market-rate apartment buildings (Asquith et al., 2019b), rent control (Autor et al., 2014), urban revitalization programs (Rossi-Hansberg et al., 2010), and foreclosures (Campbell et al., 2011) have all been studied.

program's direct and indirect effects are not enough. First, we need to be able to aggregate effects up to the city-level. This requires a better understanding of how the direct and indirect effects change in equilibrium. Second, heterogeneity in the investment response to the tax credit may reflect two factors. More housing supply-elastic areas may have a greater response to the tax credit due to the ease of building. They may also see a greater investment response because surrounding areas are also more housing supply-elastic, inducing larger spillovers. Investment will respond differently to designations of the tax credit depending on the relative importance of each mechanism. A model is needed to jointly summarize these reduced-form facts, understand how they change in equilibrium, and be able to consider counterfactual policies.

I model new construction as arising from strategic decisions made by developers at locations within a city. For developers, profits from building depend on prior new development, location fundamentals, the tax credit, and the behavior of other developers in the city.[4] The value of the tax credit, and the responsiveness of a developer to nearby development, are allowed to vary with neighborhood characteristics as the reduced-form evidence suggests. I restrict developer expectations over surrounding behavior to follow a full-information, rational-expectations framework. The model provides a rich set of equilibrium interactions, including the possibility of multiple equilibria.[5] The model follows Brock and Durlauf (2001b)'s work on peer effects, adapting it to an urban setting with spatial complementarities, location fixed effects, and state-dependence. The model is flexible in its characterization of neighborhood responses to the tax credit, but tractable.[6]

The model does well to rationalize several features of the data. First, it can replicate the difference-in-differences estimate of how the OZ program increased new development, as well as observed neighborhood heterogeneity in new development. Second, the parameter estimates indicate that while spillovers are larger in low home value areas, the direct value of the tax credit does not vary with local home values. However, the model is still able to replicate this reduced-form effect heterogeneity. Moreover, the model is able to replicate effect heterogeneity in local rents and the share of the population that is black, features not explicitly targeted in estimation. Through the lens of the model, I find that the OZ program increased city-wide, equilibrium new development by 2.7% and median home values by 0.6 %.

[4]The model follows a small literature in treating developers as strategic agents interacting within the city (Henderson and Mitra, 1996). The roles of heterogeneity, dynamics, and spillovers have long been discussed in explaining urban phenomen (Davis and Weinstein, 2002; Bleakley and Lin, 2012; Allen and Donaldson, 2018).

[5]Multiple equilibria arise naturally from the coordination problem of developers (Owens III et al., 2020). The existence of multiple equilibria is a major efficiency justification for place-based policies, more generally (Kline and Moretti, 2014b). The possibility of coordinating investment, and shifting firm expectations, was at the fore for early proponents of the OZ program (Bernstein and Hassett, 2015).

[6]Addressing the roles of heterogeneity and state-dependence in program evaluations has long been of interest to economists (Heckman, 1981a; Card and Sullivan, 1988; Card and Hyslop, 2005). Including a role for spillovers is a natural extension to the setting of place-based policies. The estimation and identification of strategic games has been discussed in Bajari et al. (2010a), Bajari et al. (2010b), and Bajari et al. (2015).

In the final section of the paper, I turn to the city planner's optimal policy problem. The policymaker must select neighborhoods for the tax credit, given a fixed number of neighborhoods to choose from a pool of program-eligible ones (i.e. sufficiently low-income and high-poverty), to maximize investment. After all, congress's stated goals were to "drive private investment into our nation's most distressed zip codes."[7] Given the strong link between equilibrium developer profits in my model and observed home value appreciation, I relate the optimal policy problem to increasing local property values as well. This is a question that has largely been overlooked in the literature on place-based policy design, in favor of whether a program is efficient altogether (Fajgelbaum and Gaubert, 2020) or redistributive motivations (Gaubert et al., 2022). The perspective in this section is that of the municipality, and hence, is "locally" optimal. The problem defines a mixed-integer, non-linear programming problem which I solve numerically.

I find that under the optimal policy, city-wide new development increases 4.5% and median home values increase 0.8%. This constitutes a 70% increase in investment relative to the actual designations for the tax credit. The optimal policy increases the investment response at all levels of neighborhood poverty rates, offering an equity and efficiency improvement over the existing design. While there are diminishing spillovers in the number of nearby OZs, spatially-correlated heterogeneity in spillovers pushes the optimal policy to cluster the tax credits in low to middle home value areas near a city's downtown. Policymakers chose significantly more college-educated and lower-income neighborhoods than were indicated by the optimal program. A simple cost-benefit analysis finds that aggregate property value appreciation is greater than the expected program costs under both the actual and optimal OZs. As an additional counterfactual, I find that the worst policy increases new development in cities by only 0.8%. These findings show how critical the spatial design of place-based policies is to their impact, and can rationalize the mixed evidence on place-based policy effectiveness in other contexts (Neumark and Simpson, 2015).[8]

A growing literature has explored the effects of the OZ program. Arefeva et al. (2020b), Atkins et al. (2020), and Freedman et al. (2021) have focused on wages and employment.[9] Casey (2019) and Chen et al. (2023) have focused on local housing prices. In particular, Chen et al. (2023) find no effect on local housing price *growth* in OZs. They focus on the entire U.S., while I focus on a sample of large, urban areas. These cities are likely to be where the effect is strongest. Moreover, my measure of home prices (the log level of median home values) and data source (the American Community Survey) differ from their setting.[10]

[7]Taken from Senator Tim Scott's website, one of the authors of the OZ program. `https://www.scott.senate.gov/opportunityzones`. The optimal policy problem and context is similar to Fu and Gregory (2019)'s study of rebuilding subsidies in the wake of Hurricane Katrina.

[8]See for example, Freedman et al. (2021); Busso et al. (2013); Neumark and Kolko (2010); Briant et al. (2015).

[9]Arefeva et al. (2020b) find employment growth in OZs. Atkins et al. (2020) find fewer job postings, but with higher average salaries. Freedman et al. (2021) find small increases in employment in OZs. The authors argue in both Atkins et al. (2020) and Freedman et al. (2021) that the effects are sensitive to the design, and are insignificant under alternative specifications.

[10]They rely on Federal Housing Finance Agency data. They also find mixed evidence on residential

Sage et al. (2019) find that while commercial property prices generally did not increase, they increased some 10-20% for redevelopment sites and vacant plots.[11] The focus of this paper, on alternative program designs, is novel.

The rest of the paper is organized as follows. Section 2.2 provides context for the Opportunity Zone program. Section 2.3 describes the data sources used. Section 2.4 presents reduced-form evidence of the new development response to the investment tax credit. Section 2.5 documents positive spillovers on development in nearby neighborhoods. Section 2.6 describes the model and approach to estimation. Section 2.7 presents the model estimates. Section 2.8 describes the optimal policy framework and presents policy counterfactuals. Section 2.9 concludes.

## 2.2 Opportunity Zones

The idea of Opportunity Zones was initially conceived by the Economics Innovation Group (Bernstein and Hassett, 2015). Under their proposal, OZ funds would reinvest the capital gains of individual investors through projects primarily located in OZs. Senators Tim Scott and Cory Booker and Representatives Pat Tiberi and Ron Kind led a bipartisan group of lawmakers in sponsoring the bill,[12] which was enacted on December 22nd, 2017 as part of the Trump administration's Tax Cuts and Jobs Act. The program designated tax credits for investments made in approximately 10% of all U.S. census tracts, and disproportionately among low-income, high-poverty areas. The Joint Committee on Taxation estimates the program will cost $3.4 billion per year on average from 2019-2023 (JCT, 2019), with $41.5 billion in aggregate cumulative OZ investments through 2020 alone (Kennedy and Wheeler, 2022).

The goal of the program is to provide tax incentives for reinvesting capital gains in distressed neighborhoods. The program provides three incentives: 1) a tax deferral on capital gains, 2) a step-up in basis on reinvested capital gains, and 3) the elimination of capital gains taxes on the new investment if held for at least 10 years. The maximum tax benefits could be achieved for investments made in 2018 through 2021. To receive the credit, capital gains can either be invested directly in the equity of firms operating in OZs (Qualfied Opportunity Zone Businesses) or in real estate (Qualified Opportunity Zone Properties). Under the current capital gains tax rate and an annual appreciation of 7%, the Economic Innovation Group calculates that OZ investments can expect an excess, 10-year return of 44 percentage points over a traditional stock portfolio (EIG, 2018).

Early news coverage of OZs has found residential and commercial real estate developments

permitting at the census place-level. Further discussion of these differences is included in Section 2.4.

[11]Two benefits of primarily focusing on new development projects are that 1) while prices should be forward-looking, they may be slow to adjust, and 2) new development constitutes physical investment rather than market expectations of investment behavior. However, I do find effects on home values as well.

[12]Their statement can be found here.

to be the first form of investment to take advantage of the program.[13] Novogradac (2020) provides a self-reported list of OZ funds; while this list is by no means representative, the OZ funds documented here are largely operating in real estate development. This finding has been confirmed in the 2019 and 2020 waves of tax forms filed by all OZ funds (Kennedy and Wheeler, 2022).

A particular concern of the program is that real estate investment may largely be *financial* (i.e. the purchase of land) rather than *real* (i.e. the construction of buildings). However, OZ real estate investments are required either to make "substantial improvements" to the property or to begin the "original use" of the property with the project. The first condition requires that improvements to the property made within the first 30 months of acquisition exceed the value of structures on the property.[14] The second condition allows for vacant properties (that have been vacant for at least five years) to be purchased and not be subject to the "substantial improvements" requirement. The IRS later noted in their April 2019 guidance that relying on the "original use" qualification still requires that the land be improved by more than an "insubstantial amount" within 30 month of acquisition (Internal Revenue Service, 2019). Moreover, the elimination of capital gains taxes on the new OZ investment incentivizes development of properties, beyond just aquiring land.

The program was designed to encourage investment in low-income, high-poverty neighborhoods. Eligibility for OZ designation was based on the 5-year 2011-2015 American Community Survey, and required tract-level poverty rates above 20%, or median family incomes below 80% of the area median income.[15] Altogether, around 40% of U.S. census tracts were eligible for OZ designation.

State governors were given until March 21st, 2018 to nominate a quarter of their eligible tracts for OZ designation. This nomination process varied among states. Some governors chose directly, some deferred to lower administrative units, while others required applications from local authorities.[16] From April until June of 2018, the IRS released lists of approved census tracts; virtually all of the nominated tracts were approved. Figure B.1 includes maps for examples of eligible neighborhoods and their chosen OZs in four cities.

[13]New York Times coverage can be found here.

[14]While the OZ property acquisition will include both land and structures, only the value of structures are used for determining whether a substantial improvement was made.

[15]For rural tracts, the area median income is defined as the statewide median family income. For urban tracts, the area median income is the smaller of the statewide and metropolitan area median family incomes. This definition of "low-income communities" (LICs) is the same as that used by the NMTC program. A small number of low-population tracts, high-migration rural tracts, and LIC-contiguous tracts were also deemed eligible. The LIC-contiguous tracts could not exceed 125% of the median family income of their adjacent LIC, and 5% of nominated tracts from a state.

[16]Frank et al. (2020) find that political affiliation of governors and representatives affected OZ selection. On the other hand, Duarte et al. (2021) find that governors mainly rubber-stamped OZ recommendations from city mayors. Practices on nominating LIC-contiguous tracts varied across states as well (Wallwork and Schakel, 2018).

## 2.3 Data

To study the new development response to the investment tax credit requires high-frequency and granular data on new construction projects. To that end, I have geo-coded and concorded building permit data across large U.S. cities. This novel dataset tracks new developments across time in 12,000 neighborhoods. To this dataset, I merge census tract and OZ program characteristics.

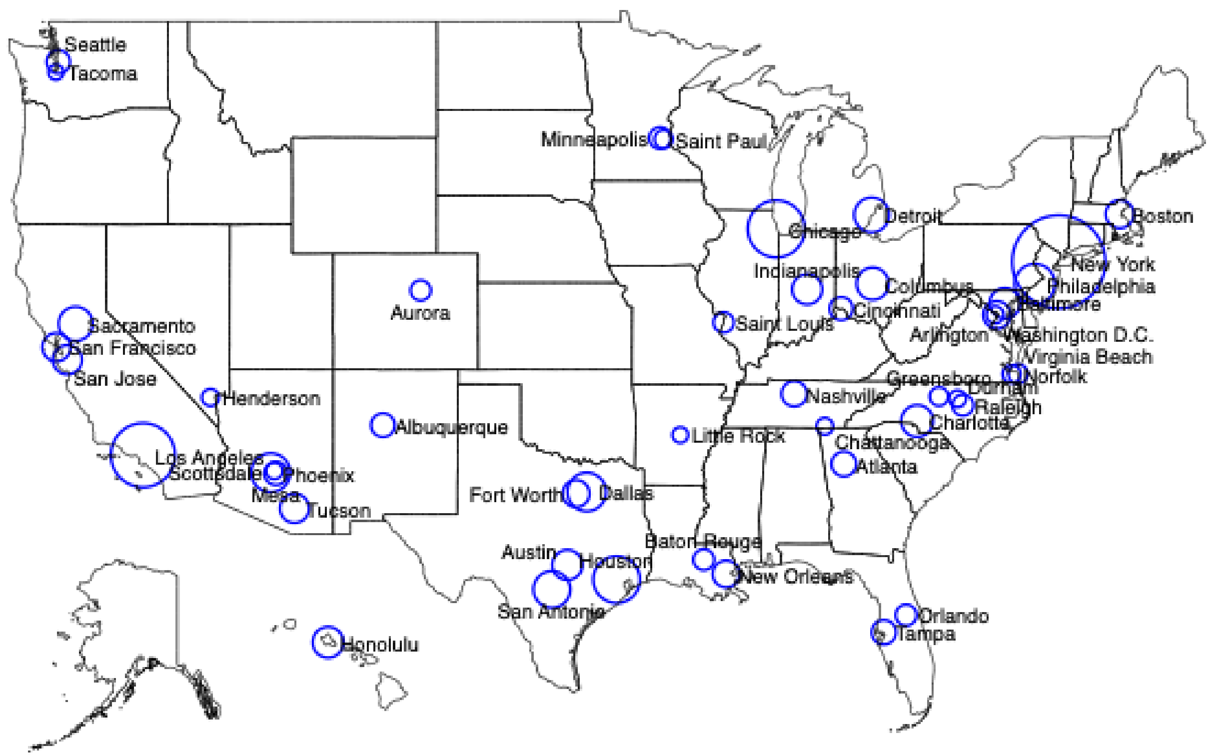
#### Sources

**Building Permits:** The main outcome in this paper is whether a permit for the construction of a new building is issued in a census tract in a given month. Towards that end, data were compiled on millions of building and trade permits for 47 large cities covering more than 15% of the U.S. population. Construction data at the municipality level have been used before to study local housing markets (Glaeser and Gyourko, 2003). However, compiling data to track neighborhood development across a large number of U.S. cities is to the best of my knowledge a contribution of this paper. The data come from municipal planning offices through a mixture of publicly available sources and FOIA requests. The data sources can be found in Table B.4.

Permits that were cancelled or voided are excluded from the sample. Geolocating the buildings was performed by a mix of directly provided coordinates, census tracts, or the assessor parcel number that could be mapped to auxilary shapefiles containing parcel locations. The data contain information about the type of new construction (residential or commercial), and often information on estimated construction costs, the square footage, the number of units, and demolitions. To be included in my sample, the permit data must include information on residential and commercial buildings, and I must be able to readily identify whether the building permit is for a new building, when it was issued, and where the building is located. I also require that cities have at least 50 different census tracts appear in their building permit data. Though the samples vary by city, almost all cover time periods up until June 2022. This is more recent than prior studies of the OZ program. Figure 2.1 maps the cities in my sample, with geographic coverage ranging across the U.S. Additional information about the data construction is in Section B.3.

Applying for a permit is the last step in the building process, after financing, development plans, and contractor selection have been completed. If permits lead to new buildings, we should see lags of permitting activity positively correlated with changes in the number of addresses in a neighborhood. Evidence along these lines is presented in Section B.3. Moreover, Section 2.4 finds that address counts have increased in OZs.

Figure 2.1: Cities in main sample




Note: This map shows all cities included in the main sample. The size of the circle is proportional to the number of tracts in the city.

**Opportunity Zone Details:** Eligible and chosen census tracts come from the CDFI fund. For each state, the month that OZs were approved by the IRS was ascertained from IRS news releases.

**Census Tract Demographics:** Census tract demographics come from the 5-year 2011-2015 American Community Survey (ACS). These demographics were also used to determine a census tract's eligibility via its median family income and poverty rate. Census outcomes are used in some parts of the paper and follow the 2015 through 2020 waves of the ACS. 2020 ACS outcomes are population weighted to 2010 tract boundaries. 2010 census tract locations and shapes come from the TIGER 2019 shapefiles, also available through the Census.

**Additional Data Sources:** Municipality-level zoning measures come from the 2006 Wharton Residential Land Use Regulatory Index (WRLURI) (Gyourko et al., 2008). Tract-level housing supply elasticities for 2011 are provided by Baum-Snow and Han (2022), and have been population-weighted to 2010 census tract boundaries. Tract-level land cover data for 2016 comes from Clarke and Melendez (2019), which relies on the U.S. Geological Survey's National Land Cover Database.

### Preliminary Facts

The distribution of months with new developments is included in Figure B.2. In my sample, 86% of neighborhoods have no new development in a given month, and 17% have no new buildings since 2014. While some of the building permit histories date back to the 1990s, I limit my sample to observations between January 2014 and June 2022. Not all cities have a building permit history beginning in 2014, however. The average city in my sample has 95 months of observations between January 2014 and June 2022, 254 census tracts, 34 OZs, and 18.1% tract-months with a permit issued for the construction of a new building. This information is summarized in Table B.1.

The process by which states chose OZs varied. From the pool of eligible neighborhoods, governors and local policymakers tended to designate the tax credit to areas that were considerably more distressed. Differences between OZs and other eligible tracts are summarized in Table B.2 for the entire U.S. and in Table 2.1 for my sample of cities. While neighborhoods in my sample have an average median family income of nearly $70k, OZs have an average median family income of $38k. The poverty rate for all neighborhoods is 19%, but 33% for OZs. OZs also have lower home values, and are more diverse, less educated, and less populated. These patterns hold both for OZs nationally, and to my restricted sample of cities.

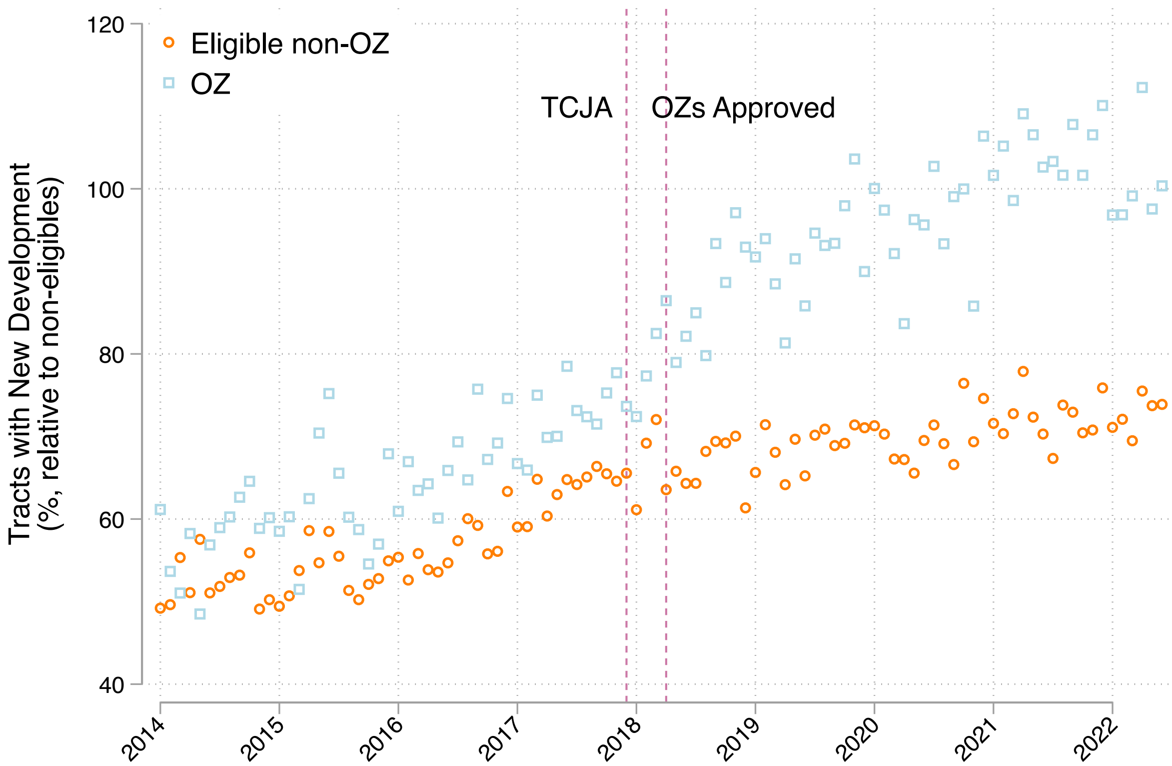
The OZ program was enacted to subsidize investment in distressed neighborhoods. To see how investment was trending in affected neighborhoods, Figure 2.2 plots the fraction of neighborhoods with new development since 2014 separately for OZs and eligible tracts that were not designated for the credit. I detrend the series by normalizing it relative to the fraction of neighborhoods with new development among ineligible tracts. These

Table 2.1: OZ descriptives for the main sample

| | (1) All Tracts | (2) Eligible, Not Chosen | (3) OZ Tracts | (4) Diff (2-3) | (5) p-val |
|---|---|---|---|---|---|
| Population | 4,194 | 4,102 | 3,815 | -287 | 0.00 |
| | (2,029) | (1,855) | (1,933) | | |
| Median Age | 36.2 | 33.7 | 33.0 | -0.7 | 0.00 |
| | (6.7) | (5.8) | (5.8) | | |
| % White | 0.55 | 0.46 | 0.35 | -0.11 | 0.00 |
| | (0.29) | (0.27) | (0.26) | | |
| % Black | 0.23 | 0.30 | 0.43 | 0.13 | 0.00 |
| | (0.29) | (0.32) | (0.34) | | |
| % Foreign | 0.12 | 0.15 | 0.13 | -0.02 | 0.00 |
| | (0.10) | (0.11) | (0.12) | | |
| % High School | 0.57 | 0.49 | 0.47 | -0.02 | 0.00 |
| | (0.14) | (0.13) | (0.12) | | |
| % College | 0.24 | 0.15 | 0.12 | -0.03 | 0.00 |
| | (0.17) | (0.12) | (0.10) | | |
| Median Family Income | 69,984 | 45,813 | 38,461 | -7352 | 0.00 |
| | (41,362) | (19,787) | (17,636) | | |
| % Poverty Rate | 0.19 | 0.27 | 0.33 | 0.06 | 0.00 |
| | (0.14) | (0.12) | (0.13) | | |
| Median Home Value (1000s) | 319 | 240 | 224 | -16 | 0.01 |
| | (265) | (199) | (192) | | |
| Household Gini | 0.44 | 0.45 | 0.46 | 0.01 | 0.00 |
| | (0.07) | (0.06) | (0.06) | | |
| N | 11,060 | 4,668 | 1,410 | | |

Note: This table provides a comparison of demographics for all census tracts (Column 1), tracts that were eligible for OZ designation but were not chosen (Column 2), and those that were designated for the tax credit (Column 3). Column (4) contains the difference between Columns 2 and 3, and Column 5 reports the $p$-value for a test of whether that difference is zero. The sample is restricted to those census tracts that appear in my building permit data, and have non-missing values for all demographic covariates. Variables are from the 2011-2015 5-year ACS.

Figure 2.2: Time series for OZs and eligible non-OZs




Note: This figure plots time series in new development projects for tracts that were eligible to be designated as OZs, but were not (blue), with those that were designated OZs (orange). The time series is the fraction of tracts in each tract type that have new development projects in a given month as a fraction of that for tracts that were ineligible for OZ designation. The first dotted vertical line represents when the TCJA bill was passed (December 2017). The second dotted vertical line represents when OZs began to be approved (April 2018).

neighborhoods are higher-income, higher-educated, and as the chart demonstrates, have had higher levels of new development relative to eligible tracts. The comovement between new development in eligible non-OZs and OZs prior to the policy motivates the difference-in-differences approach in Section 2.4. After OZs were approved, new development in OZs rapidly converged on investment in ineligible areas. New development in eligible non-OZs, however, hovers around 70% of that in ineligible areas. The large gap that emerges between the two groups after the program is implemented suggests that the policy has had a significant impact on investment thus far. The next section will formalize this finding.

New housing investment is closely tied to economic growth in cities (Glaeser et al., 2006; Hsieh and Moretti, 2019). This fact is especially pronounced within cities. Figure B.5 depicts a bin scatterplot of the average number of new buildings in a neighborhood from

2014 through 2017, prior to the OZ program, against its log median family income in 2015, after residualizing on city fixed effects. The relationship is positive and significant, indicating that new development tends to happen within cities where incomes are highest. Figure B.6 performs the same analysis, with changes in median family income from 2015 to 2019; new development is a leading indicator for neighborhood income growth.

We might expect new development projects to appear in areas that have lacked such investment in the past. These neighborhoods have available land not found in a city's more developed areas. Figure B.7 provides a salient example, Brooklyn, to study this possibility. The figure plots the total number of new buildings over two-year horizons in census tracts before the OZ program. The map looks remarkably similar across time, with much of development happening in the northern Brooklyn neighborhoods of Greenpoint and Williamsburg. New construction in Bushwick picks up in 2014 and remains high through 2017. In contrast, stretches of East Flatbush and Carnarsie see little development over the entire time period.

To study the extent to which new development persists across time, Figure B.8 ranks neighborhoods within their cities by the number of new buildings permitted for over 24 months, and plots this rank against its 24-month lag. This chart only relies on data from before the OZ program. A 45-degree line reflect perfect persistence (since ranks are perfectly preserved over time), whereas a horizontal line reflects no persistence. The steeper the gradient, the more past investment begets future investment. The figure shows that a neighborhood at the 80th percentile in new development projects within its city is (on average) at the 70th percentile 24 months later; at the 100th percentile, those neighborhoods were (on average) at the 90th percentile 24 months later. New development is highly correlated with neighborhood income and income growth, but it tends towards areas that have experienced development in the past. The evidence suggests that it may be difficult to encourage development in low-income neighborhoods.

## 2.4 The New Development Response to OZs

In this section, I show that the OZ program had strong effects on new residential and commercial development in designated neighborhoods relative to those that were eligible for the tax credit, but ultimately were not selected. These results are robust across a battery of tests, controls, and alternative specifications.

### Empirical Design

To estimate the impact of the tax credit on new development projects, I compare new development between OZs and eligible non-OZs using a difference-in-differences design. In a first set of regression results, I estimate the following linear probability model.

$$y_{it} = \sum_{k \neq k_0} \beta_k \cdot \big(\mathrm{OZ}_i \times \tau_t(k)\big) + \alpha_i + \eta_t + \theta_{g(i)t} + x'_{it}\zeta + \varepsilon_{it}$$

The outcome $y_{it}$ is an indicator for a new development in census tract $i$ in month $t$ with eligibility status $g(i) \in \{0, 1\}$, where 1 refers to a tract eligible for OZ designation, and 0 an ineligible tract. The indicator $\tau_t(k)$ denotes that the time period is $k$. The indicator $\text{OZ}_i$ denotes whether tract $i$ is designated an OZ, $\alpha_i$ captures unrestricted tract-level heterogeneity, $\eta_t$ are month fixed effects, and $\theta_{g(i)t}$ are eligibility status by month fixed effects. The $x_{it}$ are city-specific linear time trends and season fixed effects. In the robustness exercises, I include additional controls in $x_{it}$.

At the granularity of tract-month observations, the vast majority of neighborhoods have no new developments, and among those with new development, the majority are one new project. Consequently, whether any new development occurs is a natural outcome to focus on. Additional measures of development, like the square footage, construction costs, number of units, and number of addresses are considered in Section 2.4.

By including $\theta_{g(i)t}$, estimates of the key parameters $\beta_k$ come from comparisons between OZs and eligible non-OZs. Identification of the $\beta_k$ requires that OZs and neighborhoods in the comparison group would have had similar trends in new development absent the OZ program. This is a plausible assumption for several reasons. First, eligible neighborhoods are similarly low-income and high-poverty. Second, states were only given four months to nominate tracts and the full extent of the OZ policy was not yet known at the time of nomination.[17] Third, geographic boundaries for census tracts do not naturally correspond to local housing markets, limiting the ability of policymakers to specifically target certain areas. Fourth, the eligibility status by month fixed effects as well as city trends control flexibly for construction behavior across time, while the tract fixed effects paired with the short-time time period allow for unrestricted heterogeneity over short-run development behavior. An implication of the parallel-trends assumption is that trends in new development should be similar prior to the introduction of the tax credit. I formally test this by considering the significance of $\beta_k$ for years, quarters, and months prior to when the OZ program was enacted.

**Reallocation effects:** A concern in this framework is that the OZ status of one location may affect potential outcomes in another. One possibility is that it could increase investment in surrounding neighborhoods through spillover effects. Another possibility is that it could reduce investment elsewhere through developers reallocating projects towards tax-advantaged OZs. The existence and strength of these behaviors could bias up or down my estimates of the policy impact (Rubin, 1990).

In Section 2.4, I present evidence of localized, positive spillovers. The downward bias on

[17] While states chose more disadvantaged neighborhoods for the tax credit, it is unclear how much information they had on the likelihood of encouraging investment in their selections. Consistent with this view, Duarte et al. (2021) find that many state governors simply approved tracts nominated by city mayors, rather than based on predictors of investment, like past investment. I retain ineligible tracts in the main estimation sample, which contribute to estimating the city trends. The results are unchanged by their inclusion. Additionally, OZs tend to be more distressed than other eligible areas. To assess the sensitivity of the main difference-in-differences results, in Section 2.4 I use propensity-score methods to balance OZs and the comparison group on observable characteristics.

the reduced-form effect from these spillovers is mitigated by: (i) a large pool of "control" tracts, many of which will be too far from OZs to have any spillovers, and (ii) positive spillovers on "treated" tracts from being near to other OZs. A primary motivation for the model presented in Section 2.6 is to jointly estimate the direct effect of the program with spillovers on nearby development.

Reallocation of investment to distant neighborhoods is harder to measure. A developer choosing between new projects in a neighborhood without the tax credit and a neighborhood with the tax credit may move investment from the former to the latter. This substitution away from the comparison group will tend to bias upwards my estimate of the tax credit. However, the program structure makes it difficult to do so. OZ funds are seeded by capital gains from individual investors, so a developer could not have lined up financing for a project and then fund an alternative project to claim the credit. Moreover, while the comparison group is where we would expect to see the largest reallocation effects (similarly low-income, near to OZs), Figure 2.2 demonstrates that development also picks up here relative to neighborhoods ineligible for the tax credit.

To formally test this possibility, I ask whether *developers* increased investment in eligible non-OZ neighborhoods relative to ineligible neighborhoods. I construct a panel of developer decisions across the majority of cities in my dataset. The panel consists of developer identifiers, and whether they start projects in any of the three types of neighborhoods: (i) OZs, (ii) eligible non-OZs, and (iii) ineligible areas. Column (1) of Table B.5 shows estimates from a difference-in-differences design using investment in eligible tracts as the control group.[18] I find a significant and positive effect of the policy on OZ investment. Using investment in ineligible tracts as the control group, I find an effect on new development in OZs (Column 2), but no such effect on eligible tracts (Column 3). These results are inconsistent with important reallocation effects.

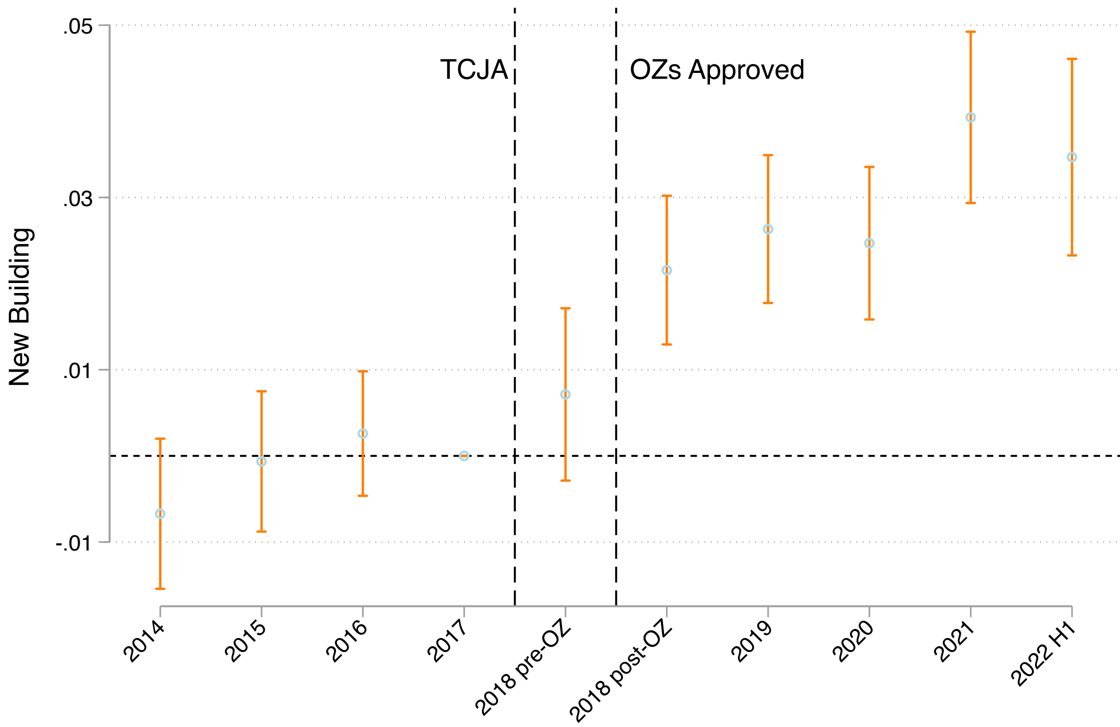
### Opportunity Zone Effects

Estimates of the linear probability model are depicted graphically in Figure 2.3. The coefficients $\beta_k$ capture conditional differences in the monthly probability of new development between OZs and eligible non-OZs in a given calendar time period. The regression is estimated separately at the annual, quarterly, and monthly levels to examine pre-trends at different frequencies. All standard errors are clustered at the level of treatment – the census tract (Bertrand et al., 2004).

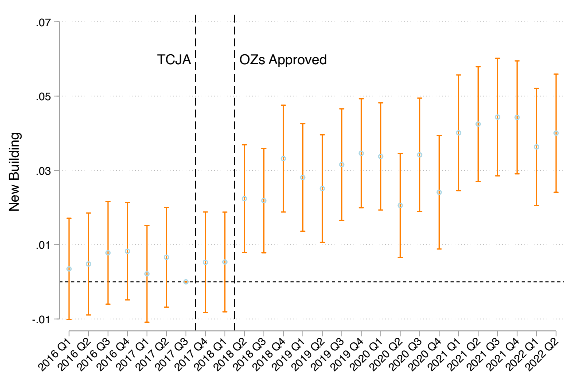
Figure 2.3a documents the baseline estimates of $\beta_k$ at the annual level. Reassuringly, I cannot reject $\beta_k = 0$ for years before OZs were enacted. Moreover, new development in OZs and non-OZs is statistically indistinguishable prior to the program for longer than the program has been in existence for. New development increases 2.2pp immediately after OZs are passed. The effect increases to 3.9pp by 2021, before declining slightly in the first half of 2022. Interacting OZ status with quarters and months offers a more

[18] Specifically, I include developer by tract type fixed effects, and developer by time fixed effects. The dataset construction and specification are discussed in more detail in Section B.4.

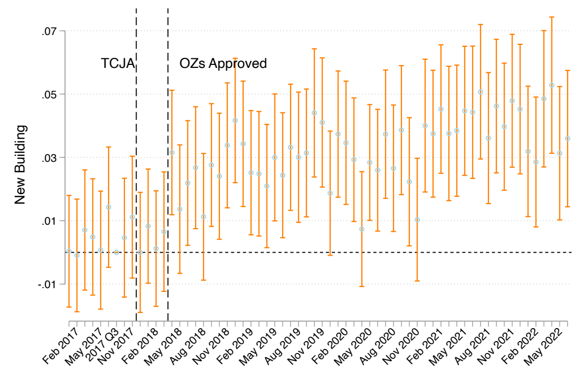
Figure 2.3: Difference-in-differences estimates




(a) Annual




(b) Quarterly




(c) Monthly

Note: This chart contains estimates from a linear probability model including tract, month, and eligibility by month fixed effects, as well as city linear and seasonal trends. The outcome variable is an indicator for whether a tract had a permit issued for the construction of a new building in a month. The coefficients correspond to OZ status interacted with various time periods. Panel (a) depicts annual interactions with OZ status. Panel (b) depicts quarterly and panel (c) depicts monthly interactions. All specifications are estimated on monthly data from January 2014 to June 2022. All errors are clustered at tract-level.

granular look at the program dynamics. For example, we might be concerned that state lawmakers chose tracts with new construction in progress during the months leading up to OZ nominations. Quarterly dynamics in Figure 2.3b show little evidence for this story from 2016 Q1 to 2018 Q2. Monthly dynamics in Figure 2.3c demonstrate no differences before the program was implemented as well, suggesting that new development in OZs was similar to eligible non-OZs leading up to the IRS approval of the tax credits.

**Overall effect**: The average effect of the program is given by the following specification.

$$y_{it} = \beta \cdot \left(\mathrm{OZ}_i \times \mathrm{Post}_{it}\right) + \alpha_i + \eta_t + \theta_{g(i)t} + x_{it}'\zeta + \varepsilon_{it}$$

The indicator $\mathrm{Post}_{it}$ denotes whether $t$ is past the date when OZs were announced for tract $i$'s state by the IRS, and its associated parameter $\beta$ captures the average policy effect. The IRS announced OZs between April and June 2018, with the announcement date for each state included in Table B.3.[19]

Estimates of $\beta$ are in Table 2.2. A concern is that cities that were already developing received more OZs than other cities. To address these concerns, I add increasing controls for city trends in Column (2) through Column (4). Column (2) parsimoniously controls for city trends and is my baseline specification, including a city linear trend in years and seasonal effects. This approximates secular trends in new development well over the 2014 - 2022 period. Column (3) controls for city by month fixed effects, while Column (4) allows for differential trends between eligible tracts and non-eligible tracts within cities. The latter estimates $\beta$ by comparing eligible non-OZs with OZs within the same city. The baseline model finds a sizeable and significant policy impact of 2.9pp (20.5%) on the monthly probability of new development. Controlling for city trends does not noticeably impact the magnitude or precision of the estimate.

### Robustness

The evidence supports comparable levels of new development in OZs and non-OZs before the OZ program was implemented, and a large, significant increase in OZ new developments after. A concern of the research design is that OZs are lower-income, more-impoverished, less-educated, and more-diverse than non-OZs; subsequently, the positive effect of the tax credit may reflect trends in baseline differences. Worse yet, OZs could have been chosen for unobservable reasons that effect new development behavior. I assess these possibilities through a battery of robustness tests.

**Eligibility discontinuity:** Eligibility for OZ designation was determined based on a tract's median family income and poverty rate. Comparing tracts near these cutoffs provides

[19] There are technically three dates in which OZs became active for different states: April, May, and June of 2018. In the interacted difference-in-differences specifications of Section 2.4, I simply use calendar time to assess pre-trends and dynamics. However, coefficients on April, May, and June 2018 should be interpretted as "partially" treated months.

Table 2.2: Overall effect of OZ designation on new development

| | (1)<br>New Building | (2)<br>New Building | (3)<br>New Building | (4)<br>New Building |
|---|---|---|---|---|
| **OZ and Post-Period** | 0.0284*** | 0.0294*** | 0.0296*** | 0.0300*** |
| | (0.00346) | (0.00333) | (0.00335) | (0.00329) |
| **Observations** | 1,175,040 | 1,175,040 | 1,175,040 | 1,175,040 |
| $R^2$ | 0.303 | 0.305 | 0.311 | 0.306 |
| **Dep. Var. Mean** | .1441 | .1441 | .1441 | .1441 |
| **Tract FE** | ✓ | ✓ | ✓ | ✓ |
| **Elig. x Month FE** | ✓ | ✓ | ✓ | ✓ |
| **Semi-Elasticity** | .1972 | .2045 | .2055 | .2083 |
| **City x Season FE** | | ✓ | | ✓ |
| **City Linear Trend** | | ✓ | | ✓ |
| **City x Month FE** | | | ✓ | |
| **Trends by Elig.** | | | | ✓ |

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: This table contains linear regression models including tract and eligibility by month fixed effects. The outcome variable is an indicator for whether a tract had a permit issued for the construction of a new building in a given month. The reported coefficient is the interaction of whether the time period is after when OZs were announced for the census tract's state, and whether a tract was designated as an OZ. Specifications vary in which additional time trends are included. Column (2), the baseline specification, includes city by quarter fixed effects and a linear annual trend. Column (3) includes city by month fixed effects. Column (4) includes city by month by eligibility status fixed effects. All specifications are estimated on monthly data from January 2014 to June 2022. The sample include 11,936 total tracts, of which 7,801 were eligible for OZ designation and 1,602 were chosen as OZs. All errors are clustered at tract-level.

believably exogenous variation in OZ assignment. While a full regression discontinuity is underpowered in this setting,[20] I make use of this variation in two ways. First, I augment my baseline regression with eligibility status by year fixed effects, interacted with polynomials in the eligibility assignment variables. This regression compares OZs with other tracts after fully controlling for how development behavior may depend on income and poverty, across time, away from the threshold. These results are contained in Table 2.3, where each column corresponds to increasingly higher order polynomials in the eligibility assignment variables. Across specifications, there are no pre-trends as well as comparable effects of the OZ program on new development. Second, I simply use ineligible tracts near either the income or the poverty cutoffs as the comparison group. These results are contained in Table B.7. At the bandwidths from Calonico and Titiunik (2014) in Column (3), there are no pre-trends and the policy effects look similar.

**Propensity score and regression-adjustment:** I run an inverse propensity score-reweighted (IPW) version of the annual interacted differences-in-differences specification in Column (2) of Table 2.4. This allows me to econometrically balance covariates between OZs and non-OZs that are predictive of OZ status. Propensity scores are estimated via a logistic regression of OZ status on the sample of eligible tracts using the following covariates: total housing units, total vacant units, median home values, median family income, poverty rate, as well as population percentage for various ethnicities and educational attainment.[21] In a second specification, I also augment the inverse propensity score-reweighting with regression-adjustment (IPWRA) using the same set of ACS covariates. These results are contained in Column (3) of Table 2.4. This model is doubly-robust; consistent estimation of the OZ policy effect is guaranteed if either the propensity score specification is correct, or the outcomes model for new development is correct (Sant'Anna and Zhao, 2018).[22] Again, in both the IPW and IPWRA models, the pre-trends and estimated effects are consistent with the baseline specification.

**Synthetic control:** The synthetic control method forms weighted averages of non-OZ tracts to closely match baseline covariates and pre-treatment outcomes of OZ tracts. If the procedure can match these moments, it is robust to differences between OZs and non-OZs in observable and unobservable characteristics with time-varying effects (Abadie, 2021). To make this procedure tractable, I collapse the data to fractions of neighborhoods with new

[20]In my sample, crossing the poverty and income thresholds increases the probability of being selected by 5% and 8%, respectively. The first stage is marginally significant. Moreover, the heterogeneity analysis later in this section suggests that the largest effects on new development are away from the eligibility cutoffs.

[21]Overlap in the propensity scores is shown in Figure B.10. I trim the sample of tracts with extreme propensity scores, consistent with Crump et al. (2009). Econometrically, I implement this by defining a new set of "eligible" tracts that had propensity scores within $[0.05, 0.95]$. I include this "eligible" status by month fixed effects, while reweighting the entire regression by the inverse propensity score. This allows me to maintain non-eligible and eligible tracts with propensity scores outside of $[0.05, 0.95]$ within the regression sample.

[22]See Acemoglu et al. (2019) or Suárez Serrato and Wingender (2016) for examples of this procedure.

Table 2.3: Policy variation at the eligibility cutoffs (I)

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | New Building | New Building | New Building | New Building |
| **OZ and 2014** | -0.00275 | -0.00317 | -0.00278 | -0.00251 |
| | (0.00460) | (0.00456) | (0.00457) | (0.00456) |
| **OZ and 2015** | 0.000439 | 0.000397 | 0.000787 | 0.00104 |
| | (0.00427) | (0.00427) | (0.00427) | (0.00427) |
| **OZ and 2016** | 0.00336 | 0.00302 | 0.00340 | 0.00346 |
| | (0.00382) | (0.00381) | (0.00381) | (0.00381) |
| **OZ and 2018 pre-OZ** | 0.00544 | 0.00554 | 0.00572 | 0.00549 |
| | (0.00518) | (0.00519) | (0.00519) | (0.00519) |
| **OZ and 2018 post-OZ** | 0.0191*** | 0.0191*** | 0.0192*** | 0.0190*** |
| | (0.00452) | (0.00453) | (0.00452) | (0.00452) |
| **OZ and 2019** | 0.0200*** | 0.0197*** | 0.0198*** | 0.0196*** |
| | (0.00455) | (0.00455) | (0.00454) | (0.00454) |
| **OZ and 2020** | 0.0168*** | 0.0156*** | 0.0159*** | 0.0158*** |
| | (0.00464) | (0.00465) | (0.00464) | (0.00464) |
| **OZ and 2021** | 0.0290*** | 0.0276*** | 0.0279*** | 0.0276*** |
| | (0.00519) | (0.00520) | (0.00520) | (0.00520) |
| **OZ and 2022 H1** | 0.0248*** | 0.0245*** | 0.0247*** | 0.0245*** |
| | (0.00601) | (0.00602) | (0.00601) | (0.00601) |
| **Observations** | 1,175,040 | 1,175,040 | 1,175,040 | 1,175,040 |
| $R^2$ | 0.305 | 0.306 | 0.306 | 0.306 |
| **Dep. Var. Mean** | .1441 | .1441 | .1441 | .1441 |
| **Tract FE** | ✓ | ✓ | ✓ | ✓ |
| **Month FE** | ✓ | ✓ | ✓ | ✓ |
| **City x Season FE** | ✓ | ✓ | ✓ | ✓ |
| **City Linear Trend** | ✓ | ✓ | ✓ | ✓ |
| **Order of Z Controls** | Linear | Quadratic | Cubic | Quartic |

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: This table contains linear regression models including tract and month fixed effects, as well as city seasonal effects and city linear trends. The outcome variable is an indicator for whether a tract had a permit issued for the construction of a new building in a given month. The reported coefficients interact a time period with whether a tract was designated as an OZ. Column (1) through Column (4) add increasingly higher-order polynomials of the variables used to determine eligibility (based on tract-level median family income and poverty rates) interacted with eligibility status by year fixed effects. All specifications are estimated on monthly data from January 2014 to June 2022. The sample include 11,936 total tracts, of which 7,801 were eligible for OZ designation and 1,602 were chosen as OZs. All errors are clustered at tract-level.

Table 2.4: Alternative specifications

| | (1) New Building | (2) New Building | (3) New Building | (4) New Building |
|---|---|---|---|---|
| **OZ and 2014** | -0.00672 | 0.00301 | -0.00299 | -0.0308 |
| | (0.00445) | (0.00504) | (0.00496) | (0.0376) |
| **OZ and 2015** | -0.000642 | 0.00361 | -0.000485 | 0.0156 |
| | (0.00415) | (0.00477) | (0.00482) | (0.0335) |
| **OZ and 2016** | 0.00260 | 0.00785* | 0.00450 | 0.0349 |
| | (0.00369) | (0.00458) | (0.00448) | (0.0285) |
| **OZ and 2018 pre-OZ** | 0.00714 | 0.0134** | 0.0108* | 0.0478 |
| | (0.00510) | (0.00670) | (0.00646) | (0.0352) |
| **OZ and 2018 post-OZ** | 0.0216*** | 0.0187*** | 0.0189*** | 0.133*** |
| | (0.00440) | (0.00510) | (0.00543) | (0.0290) |
| **OZ and 2019** | 0.0263*** | 0.0190*** | 0.0208*** | 0.163*** |
| | (0.00438) | (0.00526) | (0.00522) | (0.0291) |
| **OZ and 2020** | 0.0247*** | 0.0190*** | 0.0165*** | 0.186*** |
| | (0.00452) | (0.00525) | (0.00563) | (0.0321) |
| **OZ and 2021** | 0.0393*** | 0.0264*** | 0.0259*** | 0.242*** |
| | (0.00507) | (0.00568) | (0.00568) | (0.0335) |
| **OZ and 2022 H1** | 0.0347*** | 0.0184*** | 0.0231*** | 0.203*** |
| | (0.00582) | (0.00695) | (0.00672) | (0.0375) |
| **Observations** | 1,175,040 | 1,105,842 | 738,903 | 977,011 |
| $R^2$ | 0.305 | 0.311 | 0.282 | |
| **Number of Tracts** | 11936 | 11936 | - | 9949 |
| **Number of Eligibles** | 7801 | 7095 | 7486 | 6527 |
| **Number of QOZs** | 1602 | 1579 | 1586 | 1407 |
| **Dep. Var. Mean** | .1441 | .1441 | .1212 | .1733 |
| **Tract FE** | ✓ | ✓ | ✓ | ✓ |
| **Elig. x Month FE** | ✓ | ✓ | ✓ | ✓ |
| **City x Season FE** | ✓ | ✓ | ✓ | ✓ |
| **City Linear Trend** | ✓ | ✓ | ✓ | ✓ |
| **Model** | Baseline | IPW | IPWRA | PPML |

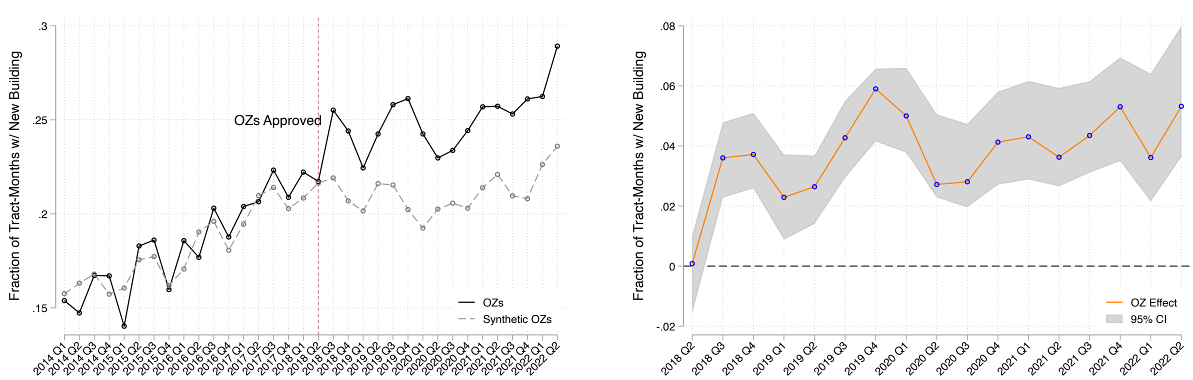
Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: This table contains alternative specifications to the baseline model in Column (1). Column (2) inverse propensity-score reweights the baseline specification, where the propensity score is estimated via a logit model of OZ status on 2011-2015 ACS tract-level demographics for the sample of eligible tracts. Tracts with propensity scores of less than 5% or greater than 95% are dropped. Column (3) adds in regression adjustment for the outcome specification. This procedure is implemented via the Stata package `rifhdreg` on the sample of eligible tracts (Rios Avila, 2019). Column (4) estimates the model via poisson pseudo-maximum likelihood estimation. For Column (4), the coefficients should be interpretted as semi-elasticities. Observations that are separated by a fixed effect are dropped in Column (4). All specifications are estimated on monthly data from January 2014 to June 2022. All errors are clustered at tract-level.

development within eligibility status by OZ status by city-quarter cells. I then match OZs in a given city to the donor pool of eligible and non-eligible tracts in various cities on median family income, poverty rate, population, percentage black, percentage college educated, median home values, as well as the average of every pair of quarters up until treatment. Inference is performed as in the setting of Cavallo et al. (2013).[23] Figure 2.4 contains a depiction of the model fit and the treatment effects with confidence intervals. The method does well to match OZ development behavior prior to the policy implementation. The estimator finds large and significant effects of the policy, similar in size and significance to other results presented above.

Figure 2.4: Synthetic control design




(a) Model fit (b) Treatment effect estimates

Note: This figure presents model fit and treatment effect estimates using a synthetic control method. The data is first collapsed to average number of tract-months with new development in a quarter in a city by tract type (where tract type can be OZ, eligible but not OZ, or ineligible). A synthetic control for OZs in a city are constructed from the pool of non-OZs in all cities, matching on the average outcome in every pair of quarters before treatment and tract demographics. These treatment effects are averaged across cities and inference is performed via Cavallo et al. (2013). Panel (a) presents the average outcome for OZs and for the synthetic control in every quarter from 2014 Q1 to 2022 Q2. Panel (b) shows treatment effect estimates for quarters after OZs were announced, with the corresponding 95% confidence interval. This analysis is performed for cities with data from 2014 Q1 through 2022 Q2.

In additional robustness, I see how sensitive the results are to trends in baseline demographics and alternative specifications of the linear probability model. The impact of the

[23]For each set of city OZs, I construct placebo synthetic controls from the remaining pool of city eligible non-OZs and city ineligibles. Bootstrap samples are drawn from these placebo treatment effects to generate a distribution of average placebo treatment effects. The two-sided $p$-value for the average treatment effect (across city OZs) is the fraction of average placebos with a larger magnitude, which can then be inverted to form the confidence intervals presented in the chart. While confidence intervals do not have a natural intepretation in the synthetic control framework, they are a convenient way to graphically represent the significance of the estimated treatment effects.

tax credit also passes several placebo tests in the timing of the policy and the selection of OZs. These results are discussed in Section B.4.

### Additional Measures of the New Development Response

I now explore other possible margins affected by the program: new residential versus commercial buildings, demolitions, as well as the square footage, construction costs, and number of new units of projects.

**New developments and demolitions:** In addition to an indicator for whether a permit is issued for the construction of a new building, I have also compiled information on the total number of such permits, whether they are for residential or commercial buildings, and demolitions for most cities.[24] Figure 2.5 contains average effects of OZ status on the number of new buildings, indicators for whether the new construction is for a residential or a commercial building, and this same information for demolitions. The OZ tax credit increases the number of new buildings by 24 % - similar to that for the extensive margin. The new construction is for both commercial and residential buildings. Residential buildings make up a larger share of the new construction, though commercial buildings have a larger semi-elasticity with respect to the program - on the order of 28 % compared with 20 % for residential. Total demolitions and residential demolitons do not increase in OZs, but commercial demolitions increase slightly. In net, most of the housing supply and commercial construction response seems to be "filling-in" vacant or unused areas, with existing structures removed for a small fraction of the new construction. This is consistent with stronger demand for vacant plots, as documented in Sage et al. (2019).

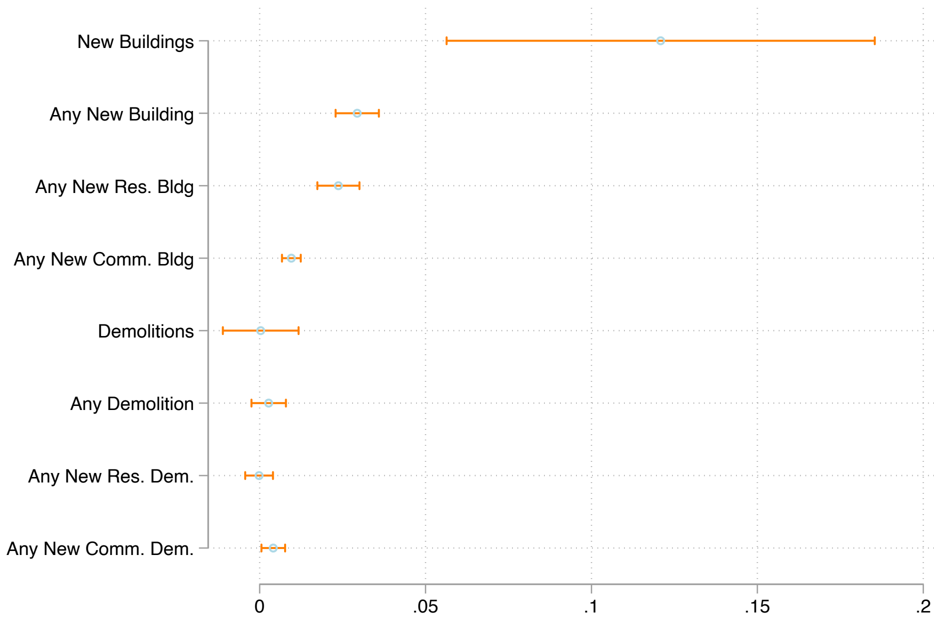
**Extensive vs. intensive margins:** The similar response between whether new development is occuring, versus the number of such projects, suggests the extensive margin $y_{it}$ is reasonable to focus on. To further explore the intensive response, I have collected data on the square footage, estimated construction costs, and number of units associated with new development. This information is available for most, but not all, cities in my sample. Difference-in-differences estimates in Figure B.14 show large and significant increases along all margins.[25] Dropping observations with no new developments however, as in the right-side panel of Figure B.14, shows a muted intensive response on several margins - particularly, the estimated construction value and square footage. These results provide further evidence that the primary investment response has been along the extensive margin, and so, motivates focusing on $y_{it}$ in Section 2.6.

**Address counts**: The increase in permitting should lead to new residential and commercial buildings in OZs. To test this, I use quarterly address counts from the USPS Vacancy

[24] Where possible, I classify mixed-use buildings as commercial.

[25] I also include the fully-interacted difference-in-differences model in Table B.8. The lack of pre-trends across various margins is reassurring for the empirical design.

Figure 2.5: Other development responses




Note: This figure contains estimates of the OZ effect using the baseline difference-in-differences model on various outcomes. The top row uses as an outcome the number of new buildings, rows 2 through 4 use as outcomes an indicator for a new building, and whether its residential or commercial. Rows 5 through 8 look at the same outcomes, except for demolitions instead of new development projects.

Data. I use the same difference-in-differences specification with a Poisson Pseudo-Maximum Likelihood estimator. These results are contained in Figure B.16. I find no evidence of pre-trends and an increase of 2% by 2021 Q4 relative to 2017 Q3. This suggests that the tax credits have lead to a substantial change in the stock of residential and commercial housing - and this effect is likely to increase as more construction is finished.

### Heterogeneity by Neighborhood Characteristics

Several mechanisms could drive how strong the investment response is to the OZ program. The availability of developable land, the availability of cheaper land, and laxer land use regulations could make it easier for developers to build using the OZ tax credit. Neighborhood demographics may affect the strength of demand for new residential and commercial space, and consequently, the profitability of investing in certain locations. I explore neighborhood heterogeneities in the response to the tax credit by interacting OZ status with the following neighborhood characteristics: the 2016 share of land that is open space or has low development, a measure of the local supply elasticity from 2011 (Baum-Snow and Han, 2022), and covariates from the 2011-2015 ACS including the log of median home values, the log of median family income, the share of the population that has a college degree, and the poverty rate.

The interaction of the policy effect with neighborhood characteristics is contained in Table B.9. The first two rows confirm that the tax credit is more effective in areas with more developable land and higher supply elasticities. A bigger response can also be found in lower home value neighborhoods, where land is also likely to be less expensive. Neighborhoods with a lower college-educated share also see a larger response. Including all interactions in Column (7) reveals that local home values remain one of the strongest predictors of the tax credit response; neighborhoods with greater supply elasticities and lower poverty rates also see larger development effects (significant at the 10%-level).[26]

The descriptive evidence in Section 2.3 shows that the same high-income neighborhoods with new development in the past continue to be developed in the present. This suggests that new development in many neighborhoods will be inframarginal: neighborhoods with either a little or a large amount of new development will be less likely to respond to the OZ tax credit. I test this possibility through interacting OZ status with the share of pre-program months with new development - a measure of the amount of prior investment. These results are contained in Table 2.5. The linear specification in Column (1) is insignificant. But a quadratic specification finds a strong, inverse U-shaped relationship. In particular, the OZ policy impacts were significantly stronger for neighborhoods that previously had intermediate levels of new development.[27] Neighborhoods that are very desirable or not desirable at all for new construction will respond little to policies meant to spur such investment. Effect heterogeneities of this form will be an essential ingredient in the model of Section 2.6 and the optimal policy design of Section 2.8.

### Home Values and Rents

Finally, I consider how prices have responded to the tax credit. Absent data on neighborhood land values, I focus on home values. If the tax credit improves expectations over neighborhood outcomes, then demand for homes, and consequently home values, will increase. The increase in residential supply could also suppress prices. I rely on the ACS log of home value quartiles to test how prices have changed. I estimate the same difference-in-differences regression on the 25th, 50th, and 75th quartiles of local home values, as well as the log of local rents. I balance the sample for each price measure, reducing my neighborhood coverage by $13-18\%$ depending on the outcome. These results are contained in Table B.11. I find that rents and home values trend comparably in OZs and eligible non-OZs prior to the

[26] I also interact the OZ effect with municipality zoning and land use restriction data from Gyourko et al. (2008). These results are presented in Table B.10. The OZ effect is declining in indices for the restrictiveness of local zoning approval and the length of approval delays, but increasing in density restrictions and the restrictiveness of local project approval. While suggestive, Section 2.8 focuses on the problem of the city planner, so across city variation in land use regulation will not be relevant. Moreover, the local supply elasticities also reflect the stringency of local land use regulation.

[27] These effects are depicted in Figure B.15.

Table 2.5: Heterogeneity by share of pre-OZ months with new development

| | (1) New Building | (2) New Building |
|---|---|---|
| **QOZ x Post x Dev. Shr.** | -0.0478* | 0.241*** |
| | (0.0255) | (0.0588) |
| **QOZ x Post x Dev. Shr. Sq.** | | -0.424*** |
| | | (0.0790) |
| **Observations** | 1,175,040 | 1,175,040 |
| $R^2$ | 0.305 | 0.305 |
| **Dep. Var. Mean** | .1441 | .1441 |
| **Tract FE** | ✓ | ✓ |
| **Elig. x Month FE** | ✓ | ✓ |
| **City x Season** | ✓ | ✓ |
| **City Linear Trend** | ✓ | ✓ |

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: This table shows estimates of the effect of OZ designation interacted with the fraction of months before OZs were announced in which a tract had new development projects. Column (1) contains a linear interaction and Column (2) contains a quadratic interaction. All specifications are estimated on monthly data from January 2014 to June 2022. The sample include 11,936 total tracts, of which 7,801 were eligible for OZ designation and 1,602 were chosen as OZs. All errors are clustered at tract-level.

program. Home values increase for all quartiles beginning in 2018, after the program was announced; median home values increase 3.4% by 2020. Rents remain stable from 2018 to 2020.[28]

In other work on the OZ program, Chen et al. (2023) find no change in housing price *growth* for a sample of neighborhoods with a repeat-sales price index. The findings in this paper are different for two reasons: 1) my sample contains all neighborhoods within the largest U.S. cities, for which I have already documented a strong new development response, and 2) I focus on changes in the log level of home values rather than changes in the annual rate of housing price growth. I perform two replication exercises along these lines. Table B.12 contains the first set of results. I use the log level of their home price measure and find a

[28]Home value increases and no effect on rents were also found in Busso et al. (2013)'s study of Empowerment Zones.

nearly identical difference-in-differences estimate for OZ home value appreciation from 2017 to 2020. Table B.13 contains the second set of results. Consistent with their findings, neither annual growth in the FHFA home price index or the ACS median home values significantly increased.

## 2.5 Spillovers

The tax credit's effect on new development in surrounding neighborhoods is theoretically ambiguous. One possibility is that it reduces nearby development. New construction will increase supply and could lower local rents and home values (Asquith et al., 2019b), deterring new development. "Crowd-out" of this form has been documented, for example, in the Low-Income Housing Tax Credit program (Baum-Snow and Marion, 2009). On the other hand, new residential space and new commercial space can create strong demand externalities. A new commercial building offers new employment opportunities, or local services, which in turn increase demand for residential space (an "endogenous amenities" channel, à la Diamond (2016b) and Almagro and Domınguez-Iino (2022)). For example, a new OZ project in Bronx, New York was a charter school, which surely increases residential demand from parents seeking to locate near schools (Section B.4). New construction induced by the tax credit is likely of higher quality than the existing stock, which can be internalized in other property owners investment decisions (Fu and Gregory, 2019; Hornbeck and Keniston, 2017). As evidence of this mechanism, Pennington (2020) finds that new construction resulting from house fires increases new construction nearby.

**Design:** To measure the strength and sign of the spillover effect, comparisons must be made between neighborhoods with nearby OZs to those without. However, while the tax credit appears to be exogenous conditional on the baseline set of covariates, proximity to OZs is unlikely to be. Neighborhoods located in the city center will be closer to OZs, and a neighborhood's location is plausibly correlated with other unobservable trends that determine new development. In such settings, Borusyak and Hull (2022) argue that one needs to control for the *expected* treatment under repeated realizations of the treatment assignment. Comparing two neighborhoods with a similar expected number of nearby OZs, but a different *realized* number of nearby OZs, leverages the same quasi-experimental policy variation in Section 2.4 to estimate the spillover effects.

I use the propensity score from Section 2.4 to model how likely a neighborhood was to be designated for the tax credit. To calculate an expected exposure to nearby OZs, I permute OZ status among eligible neighborhoods with probabilities proportional to their propensity score. Let $N_i^m$ be the number of OZs within distance band $m$ of neighborhood $i$. My estimate of the expected number of nearby OZs is given by $\widehat{\mathbb{E}}[N_i^m]$, the average number of OZs within distance band $m$ across simulations.

If $N_i^m - \hat{\mu}_i^m$ captures random variation in a nearby neighborhood's policy status (conditional on the baseline set of covariates), then we would expect it to be uncorrelated with demographic trends. Reassuringly, a balance test in Table B.14 shows that 2015 to 2017 changes in tract-level demographics are uncorrelated with the difference between realized and expected nearby OZs.[29]

I first aggregate the spillover effect to an indicator for having any nearby OZ, before moving to how the spillover varies with the number of nearby OZs. I estimate the following regression.

$$y_{it} = \sum_m \mathbb{1}\{N_i^m > 0\} \times \text{Post}_{it} \times \beta_m$$
$$+ \sum_k \sum_m \widehat{\mathbb{E}}[\mathbb{1}\{N_i^m > 0\}] \times \tau_t(k) \times \gamma_{mk} + \alpha_i + \theta_{g(i)t} + x'_{it}\zeta + \varepsilon_{it}$$

The index $g(i) \in \{0, 1, 2\}$ denotes whether a neighborhood is ineligible, eligible and without the tax credit, or an OZ. The $\theta_{g(i)t}$ denote OZ by eligibility status by month fixed effects.[30] I control for $\widehat{\mathbb{E}}[\mathbb{1}\{N_i^m > 0\}]$, the fraction of simulations with any nearby OZ at distance $m$, interacted with year fixed effects. I create distance bins based on census tract centroids, of 0-2 km, 2-3 km, and so on, through 6-7 km.[31] The spillovers are estimated by comparisons between neighborhoods of a similar type controlling for differences in expected proximity to OZs. The $x_{it}$ contain granular within-city location trends, depending on the specification. The $\beta_m$ are the parameters of interest, and capture the causal effect on new development of having any OZ $m$ kilometers away.

**Results:** Estimates of the spillovers on nearby new development are in Table 2.6. Column (1) includes a baseline set of city trends. Columns (2) through (4) add linear, quadratic, and cubic polynomials in neighborhood locations by city by year fixed effects. These fixed effects offer granular local controls for new development trends. I find evidence of positive spillover effects from 0-2 km, across specifications, on the order of 1pp (6%). The effects are still significant at 2-3 km. Both the "crowd-out" and demand externality mechanisms suggest that the effects should be localized and decay towards zero. Reassuringly, the effects are insignificant after 2-3 km, and decline towards zero across the specifications. I conclude that in the context of the OZ program, demand externalities dominate crowd-out, increasing nearby development in neighborhoods near OZs.

Dynamics are likely to play an important role in spillovers. First, the direct effect of the tax credit increases over time. Second, it is probable that the presence of new construction and new buildings is important for changing expectations over how a neighborhood will grow.

[29]Moreover, the magnitudes of the coefficients are economically small. Another concern of this econometric design is that there may be too little variation in $N_i^m$ once we residualize on $\widehat{\mathbb{E}}[N_i^m]$. Figure B.17 plots distribution of $N_i^m - \widehat{\mathbb{E}}[N_i^m]$, demonstrating a reasonable amount of variation for estimating spillovers.

[30]Note that while OZs are included in the regression, I only compare them with other OZs - netting out the direct effect of the tax credit and focusing on variation in nearby OZs.

[31]The 0-1 distance band, when distances are measured by tract centroids, ends up with a large number of "treated" tracts being in the downtown areas of New York and Los Angeles. The 0-2 distance band ensures a more representative treatment group across cities.

Table 2.6: Spillovers

| | (1) New Building | (2) New Building | (3) New Building | (4) New Building |
|---|---|---|---|---|
| **Has QOZ (0-2 km) x Post** | 0.00982*** | 0.0116*** | 0.0125*** | 0.0123*** |
| | (0.00285) | (0.00284) | (0.00287) | (0.00287) |
| **Has QOZ (2-3 km) x Post** | 0.00901*** | 0.00941*** | 0.00926*** | 0.00909*** |
| | (0.00305) | (0.00304) | (0.00303) | (0.00303) |
| **Has QOZ (3-4 km) x Post** | 0.00543 | 0.00588* | 0.00517 | 0.00493 |
| | (0.00349) | (0.00350) | (0.00350) | (0.00350) |
| **Has QOZ (4-5 km) x Post** | 0.00491 | 0.00539 | 0.00396 | 0.00377 |
| | (0.00357) | (0.00360) | (0.00361) | (0.00361) |
| **Has QOZ (5-6 km) x Post** | 0.000241 | 0.000672 | -0.00177 | -0.00218 |
| | (0.00357) | (0.00356) | (0.00358) | (0.00357) |
| **Has QOZ (6-7 km) x Post** | 0.00142 | 0.00241 | -0.000550 | -0.000825 |
| | (0.00359) | (0.00358) | (0.00358) | (0.00357) |
| **Observations** | 1,174,782 | 1,174,782 | 1,174,782 | 1,174,782 |
| $R^2$ | 0.306 | 0.309 | 0.310 | 0.311 |
| **Dep. Var. Mean** | .1441 | .1441 | .1441 | .1441 |
| **Tract FE** | ✓ | ✓ | ✓ | ✓ |
| **E[Nearby QOZ] x Year FE** | ✓ | ✓ | ✓ | ✓ |
| **QOZ x Elig. x Month FE** | ✓ | ✓ | ✓ | ✓ |
| **City x Season FE** | ✓ | ✓ | ✓ | ✓ |
| **City Linear Trend** | ✓ | ✓ | ✓ | ✓ |
| **City x Location Trends** | None | Linear | Quadratic | Cubic |

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: This table shows estimates of the effect of OZ designation on nearby new development. I first calculate the number of OZs that are within various distances from the centroid of a given tract. I then interact whether a tract has an OZ within a certain distance of it for various distance bands with whether the time period is after OZs have been announced. I control for trends in a tract's endogenous exposure to nearby OZs due to their location (a la (Borusyak and Hull, 2022)). I take the fraction of 100 simulations with at least one nearby OZ within a certain distance of the tract; the simulations permute OZs among eligible tracts within a city, with probabilities proportional to their propensity score. I then interact this continous measure with year fixed effects. I include OZ by eligibility status by year fixed effects. Columns (2) through (4) include increasingly higher order polynomials in a tract's location interacted with year fixed effects. Column (2) includes a first-order polynomial in a tract's centroid. Column (3) includes a second-order polynomial. Column (4) includes a third-order polynomial. All specifications are estimated on monthly data from January 2014 to June 2022. All errors are clustered at tract-level.

To study these dynamics, I interact having nearby OZs by year. I average the 0-2 and 2-3 km effects, normalizing each by the average number of nearby OZs in their respective distance bands, to increase power. These coefficients are plotted in Figure B.18. The coefficients can be interpretted as the increase in new development from one additional OZ within 0-3 km in a certain year. As further support for the econometric design, exposure to nearby OZs does not predict new development prior to the OZ program. Spillovers increase from 2018 until 2020 before flattening out. These results suggest that dynamics play an important role for spillovers in this context.

Crowd-out of investment is more likely to occur in neighborhoods with a large number of nearby OZs. I test how spillovers vary with the number of nearby OZs through the following regression.

$$y_{it} = \sum_m N_i^m \times \text{Post}_{it} \times \beta_{m,1} + \left(N_i^m\right)^2 \times \text{Post}_{it} \times \beta_{m,2}$$

$$+ \sum_k \sum_m \left( \widehat{\mathbb{E}}[N_i^m] \times \tau_t(k) \times \gamma_{mk,1} + \widehat{\mathbb{E}}[N_i^m]^2 \times \tau_t(k) \times \gamma_{mk,2} \right) + \alpha_i + \theta_{g(i)t} + x_{it}'\zeta + \varepsilon_{it}$$

A quadratic effect in the number of nearby tax credits is allowed. I control for trends in a quadratic function of the expected number of nearby tax credits. These effects are then plotted graphically in Figure B.19 with 95% confidence intervals.[32] The left hand figure plots these effects for 0-2 km, depicting diminishing spillovers in the number of nearby OZs; the effects are larger for a smaller number of nearby OZs before flattening out. While crowd-out may be present for neighborhoods with many nearby OZs, the net effect on nearby investment is still positive.

I finally consider how these spillovers vary with respect to neighborhood characteristics. In the main spillovers specification, I interact having any nearby OZ with the same set of covariates as in Section 2.4: the share of developable land, local supply elasticities, the log of median home values, the log of median family income, the share of the population with a college degree, and the poverty rate. I also include OZ status to test whether OZs experience larger spillovers than other neighborhoods. These interactions are contained in Table B.15. In Row (1), OZ status does not predict higher spillovers, suggesting contamination in the main policy effect estimates may be limited. As in the direct effect, developable land, supply elasticities, and low home values predict larger spillovers. However, including all interactions in Column (7), only home values remain significant. A higher college-share of the population, and lower poverty rates also induce larger spillovers.

These heterogeneities, in combination with the diminishing spillovers in nearby tax credits, will offer an important trade-off for the city planner deciding on whether to geographically

[32]The effect at 0 can be interpretted as the average spillover effect from having no nearby OZs at distance $m$, but having the average number of nearby OZs at other $m$. The right hand figure plots these effects for spillovers 6-7 km away, a distance at which it is reasonable to think that demand externalities should be limited. Reassuringly, at this distance, the quadratic effects are flat and insignificant at all exposures to nearby OZs.

cluster tax credits or not. Each additional nearby OZ will have diminishing indirect effects on nearby development. However, spatially-correlated home values will encourage clustering in low-home value areas. I formally model the spillovers magnitude, dynamics, diminishing effects, and heterogeneity in Section 2.6, and they play a key role in the optimal policy design of Section 2.8.

## 2.6 A Model of New Development

The previous section demonstrated several facts of the new development response to the OZ tax credit. First, the tax credit has had a significant, causal impact on new development. Neighborhoods with intermediate levels of prior investment of this type are driving the response. Second, the tax credit has induced localized, positive spillovers on new development in nearby locations. These spillovers are diminishing in the number of nearby OZs. Heterogeneities and dynamics play an important role in both the direct and indirect investment reponses. I now present a model that parsimoniously captures these features.

Beyond synthesizing the reduced-form evidence, the model is useful for several reasons. First, it will simultaneously estimate the direct and indirect effects of the program - alleviating concerns that the positive spillovers attenuate the direct response estimates, and how that response varies with neighborhood characteristics. The model allows me to aggregate the effects of the program as implemented, as well be able to conduct policy counterfactuals for how new development would have responded to alternative designations for the tax credit.

Second, while the reduced-form evidence on the direct and indirect effects point to substantial heterogeneity across neighborhoods, these results may reflect the same underlying fact. Low home value areas may have a bigger response to the program because they have cheaper, under-utilized land. They could also have a greater investment response because they are surrounded by other low home value areas, for which the indirect effects are larger. The importance of either mechanism will be essential to how the city planner should choose neighborhoods for designation in Section 2.8, and the model is able to discern which mechanism matters.

Finally, the reduced-form evidence on spillovers made use of variation in the number of nearby OZs. However, spillovers through demand externalities would operate by inducing new development, or at least, changing expectations over nearby development. The model relies on spatial complementarities in new development in this way, offering a richer characterization of the indirect effects of the program.

### Framework

The main outcome of interest is whether new development occurs in a location at a given time, as in Section 2.4. Developer profits depend on the tax credit and strategic complementarities across space. This follows other work that have formalized developers as strategic agents (Henderson and Mitra, 1996), and have considered coordination problems in local development (Owens III et al., 2020). Developers have exclusive rights to develop a location. At the level of a parcel of land, this assumption is self-evident. However, for estimation purposes and because my main outcomes of interest are neighborhood quantities, I abstract to the level of census tracts. I adapt Brock and Durlauf (2001b)'s model of peer effects to an urban setting, with spatial complementarities, state-dependence, and location heterogeneities.

In each period, a developer in neighborhood $i$ at time $t$ decides whether to build $y_{it}$. Profits depend on simultaneous decisions by other developers in the city, given by the vector $\mathbf{y}_t$. Developers form expectations over those decisions with information $\omega_{it}$, are hit with a building cost shock $\varepsilon_{it}$, and choose $y_{it}$ to maximize expected profits $\pi^*_{it}$.

$$\max_y \pi^*_{it} = \begin{cases} \mathbb{E}_{it}[\pi_{it}(\mathbf{y}_t)|\omega_{it}] - \varepsilon_{it}, & y = 1 \\ 0 & y = 0 \end{cases}$$

$$y_{it} = \mathbb{1}\{\mathbb{E}_{it}[\pi_{it}(\mathbf{y}_t)|\omega_{it}] > \varepsilon_{it}\}$$

I assume the costs are idiosyncratic and logistically distributed. This gives the probability of new development as follows.

$$\mathbb{P}[y_{it} = 1|\omega_{it}] = \Lambda\bigg(\mathbb{E}_{it}[\pi_{it}(\mathbf{y}_t)|\omega_{it}]\bigg), \qquad \Lambda(z) = \frac{\exp(z)}{1 + \exp(z)}$$

Profits depend on a function $S_i$ of nearby development $\mathbf{y}_t$.

$$S_i(\mathbf{y}_t) = \sum_{j \neq i} w_{ij} y_{jt}, \qquad w_{ij} = \frac{\exp(-\delta \cdot \text{distance}_{ij})}{\sum_{j \neq i} \exp(-\delta \cdot \text{distance}_{ij})}, \forall i \neq j \text{ and } w_{ii} = 0$$

The $S_i$ function is a weighted average of nearby development, with weights that sum to one and decay towards zero in the distance between location $i$ and $j$.[33] The speed of the decay is governed by parameter $\delta$. The latent, net profits for new development take the following form.

$$\pi_{it}(\mathbf{y}_t) - \varepsilon_{it} = \underbrace{\alpha_i}_{\text{heterogeneity}} + \underbrace{\sum_{k=1}^{\bar{K}} \gamma^k y_{i,t-k}}_{\text{state-dependence}} + \underbrace{\lambda(\mathbf{x}_i) S_i(\mathbf{y}_t)}_{\text{spillovers}} + \underbrace{T_{it}\beta(\mathbf{x}_i)}_{\text{direct policy effect}} + \underbrace{\zeta_{c(i)g(i)t}}_{\text{eligibility by city trends}} - \varepsilon_{it}$$

The location-heterogeneity term $\alpha_i$ capture time-invariant differences in the returns to developing at a location. The $\alpha_i$ contain fundamental physical aspects of the neighborhood, like its climate and access to bodies of water and parks. By focusing on the eight-year time

[33] In my estimation, distance will correspond to distances between census tract centroids. Figure B.20 plots the distribution of these distances across my sample. The median tract-to-tract distance is approximately 14 kilometers, the distribution is highly skewed towards zero.

period from 2014 to 2022, the $\alpha_i$ also contain information on slow-moving public policy and infrastructure, like zoning and public transit. A key strength of the approach outlined below is to remain agnostic on its sources and structure, and estimate the $\alpha_i$ directly. Moreover, the $\alpha_i$ will govern whether neighborhoods are more or less inframarginal to the policy, aligning with the reduced-form evidence in Section 2.4.

The parameter $\gamma$ captures state-dependence through a decaying function of prior development decisions. These dynamics capture increased demand for residential and commercial space from improvements to the quantity and quality of buildings in a neighborhood. Since infrastructure investment is irreversible, these dynamics are likely to play an important role. Moreover, this will be important to match the observed dynamics in the direct and indirect effects of the policy in Section 2.4. I set $\bar{K}$ to be twelve months of prior development decisions.[34]

The $\lambda$ captures how strong spatial complementarities in $S_i$ are across space. Theoretically, $\lambda$ could be negative (due to "crowd-out") or positive (due to demand externalities). While I do not restrict possible values of $\lambda$, consistent with the reduced-form evidence, estimates of $\lambda$ will be positive. Because of the non-linearity in the function $\Lambda$, there will diminishing returns in $S_i(\mathbf{y}_t)$ for neighborhoods near the average in new development behavior (consistent with Section 2.5).

The indicator $T_{it}$ equals 1 if the location $i$ is an OZ in month $t$. The $\beta$ captures the average policy effect. The $\zeta_{g(i)t}$ are secular time trends in city by eligibility status that make investment more or less profitable in OZ-eligible neighborhoods. Both $\lambda(x_i)$ and $\beta(x_i)$ are allowed to vary with neighborhood observables as in the reduced-form evidence: the share of land with low development, the local supply elasticity, the log of local home values, the log of median family income, the college share, and the poverty rate.[35]

$$\beta(\mathbf{x}_i) = \beta_0 + \mathbf{x}_i'\beta_x, \qquad \lambda(\mathbf{x}_i) = \lambda_0 + \mathbf{x}_i'\lambda_x$$

To complete the model, we need to specify the information set available to developers and how expectations are formed. In my main specification, I take $\omega_{it} = \{\theta, y_{j,t-k}, T_{jt}, \mathbf{x}_j\}_{j=1,\dots,n}^{k=1,\dots,\bar{K}}$ i.e. the information set contains all previous time period choices, location heterogeneities, policy status, and neighborhood characteristics. In equilibrium, I require that a developer's expectations over nearby development correspond to true expectations - that is, the actual probability that development occurs at nearby locations. This full-information rational-

[34] The finite-order state-dependence does not necessarily imply myopia on the part of developers. See for example Card and Hyslop (2005). Another way to motivate the set up is developers in a neighborhood are selected at random in each period to decide whether to develop or not. They do not, then, have control over prior investment decisions made by other developers.

[35] I use quarter and year fixed effects and interact city and eligibility status with year fixed effects. These fixed effects replicate the fixed effect structure in Section 2.4. For heterogeneity in the spillovers and direct policy effect, I normalize the covariates as follows. For the policy effect, I substract off the mean within OZs within a city. For the spillovers, I subtract off the mean within a city. I divide both by the standard deviation of the characteristic across the city. Thus, $\beta_0$ and $\lambda_0$ can be interpretted as the average direct effect and strength of spillovers.

expectations (FIRE) equilibrium at time $t$ occurs if $\mathbb{E}_{it}[y_{jt}|\omega_t] = \mathbb{E}[y_{jt}|\omega_t] = \mathbb{P}[y_{jt} = 1|\omega_t]$, $\forall i, j$. This ensures that expectations in the model are self-consistent.

Linearity and rational expectations imply that expectations can pass through the profit function.

$$\mathbb{E}[\pi_{it}(\mathbf{y}_t)|\omega_t] = \pi_{it}(\mathbb{E}[\mathbf{y}_t|\omega_t]) = \pi_{it}(\mathbb{P}[\mathbf{y}_t = 1|\omega_t])$$

Under a FIRE equilibrium, we have the following restriction on equilibrium probabilities $\mathbb{P}^\star$.

$$\mathbb{P}^\star[y_{it}|\omega_t] = \Lambda\big(\pi_{it}(\mathbb{P}^\star[\mathbf{y}_t|\omega_t])\big) = G_{it}\big(\mathbb{P}^\star[\mathbf{y}_t|\omega_t]\big), \quad \forall i$$

Let $\mathbf{G}_t$ be a vector-valued function produced by stacking each individual function $G_{it}$. The FIRE equilibrium condition describes a system of $n$ equations in $n$ unknowns governed by the equation $\mathbb{P}^\star[\mathbf{y}_t|\boldsymbol{\omega}_t] = \mathbf{G}_t\big(\mathbb{P}^\star[\mathbf{y}_t|\boldsymbol{\omega}_t]\big)$.[36] The role of dynamics and heterogeneity is particularly important in this equilibrium concept. If dynamics or heterogeneity are strong, then expectations are anchored and the presence of multiple equilibria is limited (Brock and Durlauf, 2001b). If they are weak, then multiple equilibria can exist with large variation in equilibria behavior.

### Identification and Estimation

Equipped with probabilities for new development in every time period, I estimate the model through a maximum likelihood approach.[37] In particular, I treat the location heterogeneity terms $\alpha_i$ as unrestricted fixed effects to be estimated directly. One concern with this approach is the incidental parameter bias. In my setting, this is mitigated by (i) high frequency data, so $T$ is large, and (ii) the externalities add cross-sectional variation to the estimation of each $\alpha_i$, since a location's own heterogeneity term impacts the activity of its neighbors.[38]

A second concern is that multiple FIRE equilibria may exist. Let $\theta = \{\alpha_i, \lambda(\mathbf{x}_i), \delta, \gamma, \beta(\mathbf{x}_i), \zeta, \eta\}$. Let $\mathbb{P}^\star_t$ denote the set of equilibrium probabilities at time $t$. Let $\mathbb{P}^\star_t[m_t]$ denote the vector of probabilities associated with the $m_t$th equilibrium. We can define the likelihood of a given equilibrium as follows.

$$\ln \mathcal{L}(y_{it}|\theta, \boldsymbol{\omega}_t)[m_t] = y_{it} \ln \mathbb{P}^\star_{it}(\theta, \boldsymbol{\omega}_t)[m_t] + (1 - y_{it}) \ln\big(1 - \mathbb{P}^\star_{it}(\theta, \boldsymbol{\omega}_t)[m_t]\big)$$

[36] As shown in Brock and Durlauf (2001b), since $\mathbf{G}_t : [0, 1]^n \to [0, 1]^n$ is continuous in $\mathbb{P}(\mathbf{y}_t|\boldsymbol{\omega}_t)$, a solution $\mathbb{P}^\star_t(\boldsymbol{\omega}_t) = (\mathbb{P}^\star_{it}(\boldsymbol{\omega}_t))_{i=1}^n$ exists by Brouwer's fixed point theorem.

[37] It is possible that stratifying the sample on new development combined with non-parametric estimates of development response functions could identify the main parameters of the model without imposing the equilibrium constraint. However, "conditioning" to obtain an estimate of $\lambda(\mathbf{x}_i)$, $\gamma$, and $\beta(\mathbf{x}_i)$ is not enough to conduct meaningful policy counterfactuals. The location heterogeneity terms will be critical too.

[38] Moreover, even when $T$ is small, the incidental parameters bias of the related probit model appears to be small (Heckman, 1981b). In my setting and sample, $T$ is on average 95. Additionally, the latter point has the upside that the mass of neighborhoods with no new development are maintained in the sample, whereas those locations would be dropped under a standard conditional likelihood approach.

Each equilibrium is associated with a different likelihood, so we need only choose the one that fits the data best. This is an appealing feature of multiple equilibria in this model, relative to others - there is a data-driven, equilibrium selection rule.[39] The joint probability of development decisions over time can be partitioned into the product of development probabilities in each time period conditional on the relevant information set $\boldsymbol{\omega}_t$.

$$\mathbb{P}(\mathbf{y}_i) = \mathbb{P}(y_{i0}) \times \prod_{t=1}^{T} \mathbb{P}(y_{it}|\boldsymbol{\omega}_t)$$

This motivates the following constrained maximum likelihood estimator.

$$\hat{\theta}, \{\hat{m_t}\}_{t=1}^{T} = \underset{\theta, \{m_t\}_{t=1}^{T}}{\arg\max} \sum_{t=1}^{T} \sum_{i=1}^{n} y_{it} \ln \mathbb{P}^{\star}_{it}(\theta, \boldsymbol{\omega}_t)[m_t] + (1 - y_{it}) \ln \Big(1 - \mathbb{P}^{\star}_{it}(\theta, \boldsymbol{\omega}_t)[m_t]\Big)$$

$$\text{s.t.} \quad \mathbb{P}^{\star}[y_{it}|\omega_t] = \Lambda\Big(\pi_{it}(\mathbb{P}^{\star}[\mathbf{y}_t|\omega_t])\Big), \; \forall i, t \tag{FIRE eq.}$$

In practice, each $\theta$ produces several equilibrium, of which I take the highest likelihood equilibrium as the corresponding likelihood for $\theta$.[40] Comparisons across $\theta$ can then be readily made. Only observations with 12 months of prior development data are used in estimation. See Section B.5 for further estimation details. Identification of the structural parameter $\theta$ requires mild assumptions on the joint distribution of outcomes and covariates, and a stronger assumption that the model is correctly specified - that is, the errors are logistic and independent of the covariates (Brock and Durlauf, 2001b,a).

Identification of the externality parameters relies on non-linearities in the model. In particular, this estimation procedure does not suffer from the well-known reflection problem of Manski (1993), since the effect of one neighborhood on another will depend on *each* neighborhood's level of the latent developer profits. For example, if neighborhood A has high latent developer profits and neighborhood B has latent developer profits close to zero, then the effect of development in A on B is greater than the reverse. The non-linearities in the direct (Section 2.4) and indirect (Section 2.5) effects suggest that this is not only reasonable, but important for understanding the development response to the tax credit.

[39] Fu and Gregory (2019), for example, use the ad-hoc criterion of the equilibrium that maximizes joint welfare for their estimation procedure. It is also worth comparing this approach to Bajari et al. (2010a). In their setting, the econometrician has $T$ observations from the *same* game, so a two-step procedure can be used where estimates of the equilibrium strategic behavior of agents is first generated, and used as inputs into a a second procedure to back out agent's utilities. In my paper, a different equilibrium may appear in each time period. The approach taken here gives a direct link between the parameters and the likelihood, avoiding issues that can arise from multiple equilibria in i.e. Ahlfeldt et al. (2015).

[40] For example, it could be the case that if all developers expect little investment in their city, than a low equilibrium arises. But if all expect high investment, a high equilibrium arises. However, given the development decisions that actually happened in the city, one equilibrium will better describe the data. The enumeration of equilibria is discussed in the appendix.

The moment conditions for each parameter will depend on equilibrium probabilities. The $\beta(\mathbf{x}_i)$ will require variation in neighborhood development due to the policy, and $\lambda(\mathbf{x}_i)$ will require variation in the probability of nearby development. This is useful, since the OZ tax credit has produced large, quasi-exogenous changes in development behavior that will be central to identifying these parameters. I include the same set of controls that were required for a causal interpretation of the direct effects so that the model relies on similar variation to identify the parameters. More importantly though, I show in the next section that the model is able to replicate the reduced-form evidence well. In particular, the model can replicate direct effect heterogeneity not explicitly targeted by the model.

## 2.7 Model Estimates

The parameter estimates in the model of new development show a strong role for location heterogeneity, dynamics, and spillovers. The model also estimates a significant impact of the OZ tax credit on new developments. The model fits the reduced-form evidence well, even along margins not explicitly targeted by the estimation.

### Estimates

The parameter estimates from my model are summarized in Table 2.7. The first row contains the main parameters: the main spillover effect $\lambda_0$, the main program effect $\beta_0$, the spillovers decay parameter $\delta$, the state-dependence parameter $\gamma$, and the average and standard deviation of the location heterogeneity terms $\alpha_i$. The second row contains the spillovers heterogeneity parameters. The third row contains the program effect heterogeneity parameters.

Spillovers $\lambda_0$ for the average neighborhood are significant. Consider neighborhood A with average latent profits from new development. If all nearby neighborhoods had their probability of new development increase 5pp, then development in A would increase 1.5pp. The model confirms that spillovers are stronger in low home value areas, with $\lambda_{hval}$ significant. A 1 standard deviation increase in home values lowers spillovers locally by 20%. This is consistent with lower home value neighborhoods having cheaper, under-utilized land or less political power to prevent new development projects, and consequently responding more to surrounding investment. Areas with more developable land respond more to nearby investment. A 1 standard deviation increase in the share of developable land increases spillovers by 20%. Median family incomes, poverty rates, and the college share are not found to effect spillovers significantly.

The direct effect of the tax credit $\beta_0$ for an average neighborhood is significant ($0.19^{***}$). The effect is declining in median family incomes. A 1 standard deviation increase in median family incomes halves the policy effect. Increases in the share of developable land, local supply elasticities, poverty rates, or college shares do not lead to larger program effects.

Table 2.7: Model estimates

| | | | | | | |
|---|---|---|---|---|---|---|
| *Panel A: Main parameters* | | | | | | |
| | $\boldsymbol{\lambda_0}$ | $\boldsymbol{\beta_0}$ | $\boldsymbol{\delta}$ | $\boldsymbol{\gamma}$ | $\bar{\alpha_i}$ | $\text{sd}(\alpha_i)$ |
| | 1.14*** | 0.19*** | 0.63*** | 0.33*** | −1.92 | 2.20 |
| | (0.07) | (0.02) | (0.00) | (0.00) | | |
| *Panel B: Spillovers* | | | | | | |
| | $\boldsymbol{\lambda}_{\text{dev}}$ | $\boldsymbol{\lambda}_{\text{supply}}$ | $\boldsymbol{\lambda}_{\text{hval}}$ | $\boldsymbol{\lambda}_{\text{col}}$ | $\boldsymbol{\lambda}_{\text{pov}}$ | $\boldsymbol{\lambda}_{\text{mfi}}$ |
| | 0.23*** | 0.08 | −0.23*** | −0.10 | 0.01 | 0.05 |
| | (0.06) | (0.06) | (0.07) | (0.07) | (0.06) | (0.09) |
| *Panel C: Program effects* | | | | | | |
| | $\boldsymbol{\beta}_{\text{dev}}$ | $\boldsymbol{\beta}_{\text{supply}}$ | $\boldsymbol{\beta}_{\text{hval}}$ | $\boldsymbol{\beta}_{\text{col}}$ | $\boldsymbol{\beta}_{\text{pov}}$ | $\boldsymbol{\beta}_{\text{mfi}}$ |
| | 0.01 | 0.01 | −0.04 | −0.06 | 0.04 | −0.13*** |
| | (0.04) | (0.03) | (0.04) | (0.04) | (0.03) | (0.05) |

Note: This table contains parameter estimates from my baseline model in Section 2.6. $\lambda$ denote the spillover and spillover heterogeneity parameters and $\beta$ denote the policy and policy heterogeneity parameters. $\delta$ captures how quickly spillovers decay across space and $\gamma$ the strength of state-dependence. The average and standard deviation of the location heterogeneity terms $\alpha_i$ are also included. A description of the estimation procedure and standard errors calculation is included in Section B.5.

Interestingly, $\beta_{hval}$ is small and insignificant. This suggests that effect heterogeneity in local home values were primarily driven by spillovers and location fundamentals.

The spillovers decay parameter $\delta$ is estimated to be 0.63. While the exact weights depend on the particular geography of the city, this $\delta$ corresponds to halving $w_{ij}$ with every additional kilometer from the centroid of tract $i$ to $j$. The state-dependence parameter is 0.33 and significant at conventional levels.

### Model Fit

Figure B.21 assesses the model fit for location heterogeneity and dynamics. Figure B.21a plots the probability of new development in the data and the model against the number of prior months in which a tract has new development. While these are not the dynamics targeted by the model, it appears to match the data well - especially for 0 to 3 months, where nearly 83% of all tract-month observations lie. Figure B.21b plots the equilibrium probabilities against the fraction of months that a neighborhood has new development. While there is still a large amount of variation in the model probabilities, on average, the model captures the time-invariant component of new development. As a further exercise, I plot the neighborhood-level housing supply elasticity from Baum-Snow and Han (2022) against the baseline probability of new development in a neighborhood, as estimated by $\Lambda(\hat{\alpha}_i)$. It is reasonable to expect these two objects to be closely related. Figure B.22 shows that there is a strong positive relationship.

To relate the regression evidence with the model, I run the main difference-in-differences regression on the equilibrium probability estimates $\widehat{\mathbb{P}^{\star}_{it}}$.[41] I test whether this estimate is different than the reduced-form estimates using new development data $y_{it}$. These results are captured in Table B.16. The test in Column (3) shows that the two estimates are statistically indistinguishable. The model is able to replicate the causal estimates of the OZ tax credit.

As a final exercise, I consider the model's ability to reproduce heterogeneity in the direct effect of the tax credit. In the first exercise, I consider non-parametric effect heterogeneity in home values. While home value heterogeneity is included in the model, 1) I see how restrictive the linear functional form is, and 2) the model estimates suggest that home values do not directly increase the value of the tax credit for developers ($\beta_{hval}$ is small and insignificant). In a second exercise, I consider how well the model can replicate effect heterogeneity in local rents, a characteristic excluded from the model.

To implement these tests, I interact the OZ effect with twenty 5-percentile bins based on the neighborhood characteristic (i.e. home values, rents). I then plot these effects against those from a regression using model-based $\widehat{\mathbb{P}^{\star}_{it}}$, rather than $y_{it}$. These figures are contained in Figure 2.6a and Figure 2.6b, respectively. The 45-degree line and associated $p$-value tests whether the estimates are different up to sampling error. I cannot reject the hypothesis that the two sets of estimates are the same. This finding suggests that policy heterogeneity in home values operates through spillover heterogeneity and the location heterogeneity terms $\alpha_i$. Moreover, the model is able to replicate important sources of effect heterogeneity – through local rents – not targeted in estimation.[42]

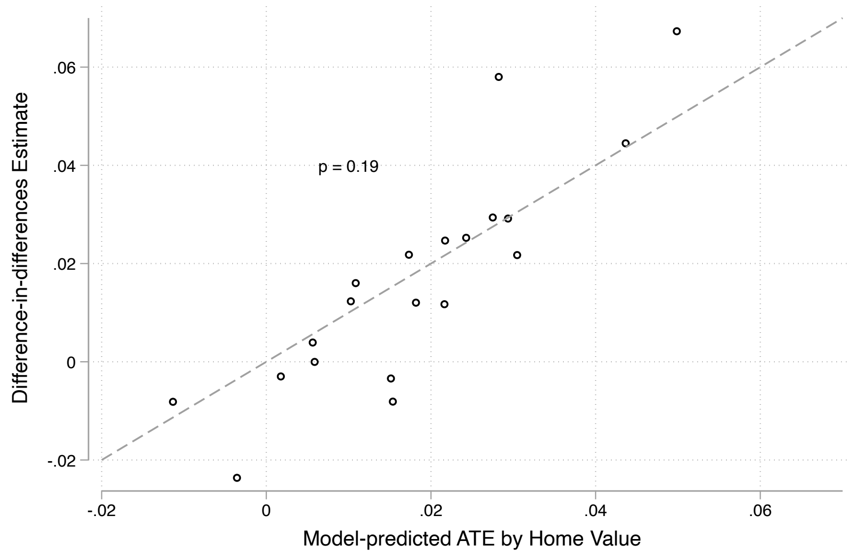
## 2.8 Optimal Policy

Equipped with a model describing new development, I now turn to the policymaker's problem. They understand the strategic behavior of developers, and have at their disposal a number of locations that they can designate for special tax-treatment under the OZ program. The following section develops a framework for how they can optimize the investment response to the tax program. I find that alternative neighborhood selections in this optimal framework lead to substantial gains over the OZ program as implemented.

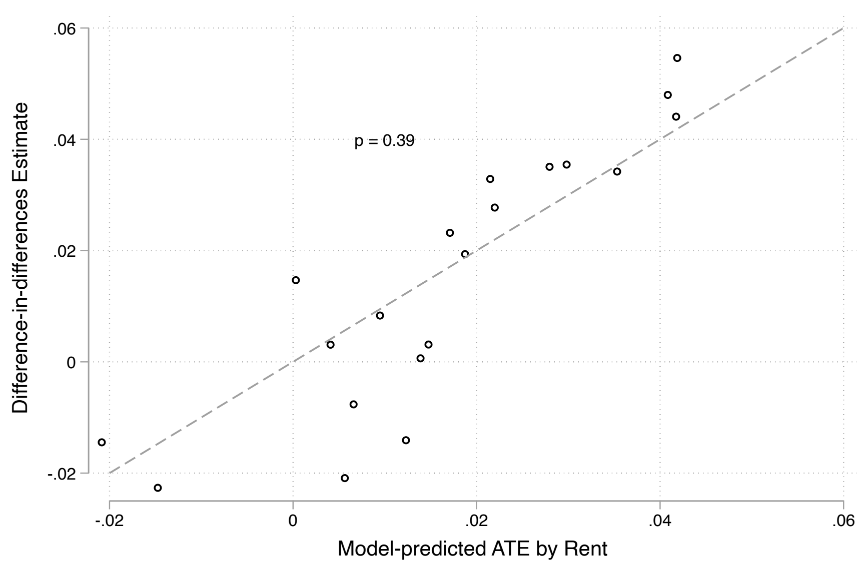
[41] The state-dependence term requires at least 12 months of prior development decision. Beccause this results in an increasingly unbalanced sample in 2014, I run these tests for the main sample restricted to 2015-2022.

[42] Figure B.23 performs the same analysis for the share of a neighborhood's population that is black. This demographic information is not targeted in the model estimation. However, like for rents, I am able to replicate effect heterogeneity by this neighborhood characteristic.

Figure 2.6: Model-predicted effects versus design-based effects




(a) Median home values




(b) Rents

Note: This figure compares model-based estimates of the OZ effect by (a) home value vingtile or (b) rent vingtile with those from an interacted difference-in-differences model. The dashed line corresponds to the 45 degree line. The p-value comes from a test of the hypothesis that the difference-in-differences estimates are equal to the model-based estimates up to sampling error. Tracts with missing home value or rent data are omitted. The sample covers 2015 through 2022.

### Metric for Designating OZs

The perspective in this section is local – that of the city planner. The federal or state government has decided that the policy will happen and how many resources are to be allocated to a city. New investment resulting from the capital gains tax cut is being driven into low-income neighborhoods in cities across the U.S. The question is - how should this complicated tax instrument be implemented? This problem has been understudied to date, but is especially important in light of the heterogeneities and indirect effects documented in this paper. However, the approach here is a partial equilibrium one, studying the short-run investment response to the tax credit. This stands in contrast with the general equilibrium framework of Fajgelbaum and Gaubert (2020), for example. However, Fajgelbaum and Gaubert (2020) ignore the strategic interactions of developers, which are central to the present analysis.

Moving from the model to welfare implications is not immediately clear. Arnott and Stiglitz (1979) show that in a broad set of economies changes in social welfare are fully captured by land values. Thus, land values are a natural metric to maximize.[43] Since I focus on the extensive margin response to the program, changes in equilibrium latent profits to development induced by the OZ tax credit should reflect changes in land values. In fact, I now show that this model object mediates all of the home value increases in OZs observed in Section 2.4.

Section 2.4 demonstrated that median home values had increased 3.4% in OZs relative to other eligible neighborhoods by 2020, relative to 2017. If my model is able to capture changes in the underlying land value, we would expect that $\pi^\star_{it}(\text{OZs}) - \pi^\star_{it}(\text{no OZs})$ is predictive of home value increases. I construct this object, and average it for 2018 through 2020: $\overline{\Delta\pi^\star_i}$. I then run the following regression.

$$\log(\text{median home values}_{it}) = \sum_{k\neq 2017} \beta_k \cdot \left(\overline{\Delta\pi^\star_i} \times \tau_t(k)\right) + \alpha_i + \theta_{g(i)c(i)t} + \varepsilon_{it}$$

The $\theta_{g(i)c(i)t}$ are city by eligibility status by year fixed effects. The $\beta_k$ coefficients are plotted across time in Figure 2.7a. Reassuringly, the measure of average latent profits is not predictive of different trends in median home values prior to the OZ's announcement. However, by 2019, neighborhoods with a bigger change in latent developer profits experience greater median home value growth. By 2020, the effects are very significant, mirroring the difference-in-differences results in Section 2.4.

To test whether $\overline{\Delta\pi^\star_i}$ mediates the home value increases in OZs, I run the following regression.

$$\log(\text{median home values}_{it}) = \sum_{k\neq 2017} \tilde{\beta}_k \cdot \left(\text{OZ}_i \times \tau_t(k)\right) +$$

$$\sum_{k} \left(\overline{\Delta\pi^\star_i} \times \tau_t(k) \cdot \eta_{1,k} + \overline{\Delta\pi^\star_i}^2 \times \tau_t(k) \cdot \eta_{2,k}\right) + \alpha_i + \theta_{g(i)c(i)t} + \varepsilon_{it}$$

[43] A recent example of such an approach is taken in Smith (2020).

The interpretation of $\tilde{\beta}_k$ is the change in log median home values relative to 2017 in OZs with no change in average latent profits. These coefficients are plotted in Figure 2.7b. They are insignificant at all values, suggesting that all of the OZ home value appreciation can be explained through the lens of the model. These results provide important evidence for using $\pi^{\star}_{it}$ as the welfare metric to maximize. Moreover, the OZ policy's justification was to bring revitalization and investment into distressed neighborhoods. Investment will be an increasing function of latent developer profits.

### Framework

City planner's have a set of Pareto weights $\omega_i$ capturing how much they value outcomes in neighborhood $i$ relative to others. Let $T(i) \in \{0, 1\}$ be a policy function assigning the tax credit to location $i$, where $K$ overall units of policy are available to assign to eligible neighborhoods. In practice, I take $K$ to be the actual number of OZs in a city. The policymaker's problem is to choose the policy to maximize a weighted sum of latent developer profits as follows:

$$\max_{T} \mathbb{E}_0 \sum_t \sum_i \rho^t \cdot \omega_i \cdot \pi^{\star}_{it}(T, \theta, \mathbf{y}_0^{t-1}, \mathbf{x}_i) \tag{2.1}$$

$$\text{s.t. } \sum_i T(i) = K \qquad (1 - g(i))T(i) = 0, \forall i \tag{2.2}$$

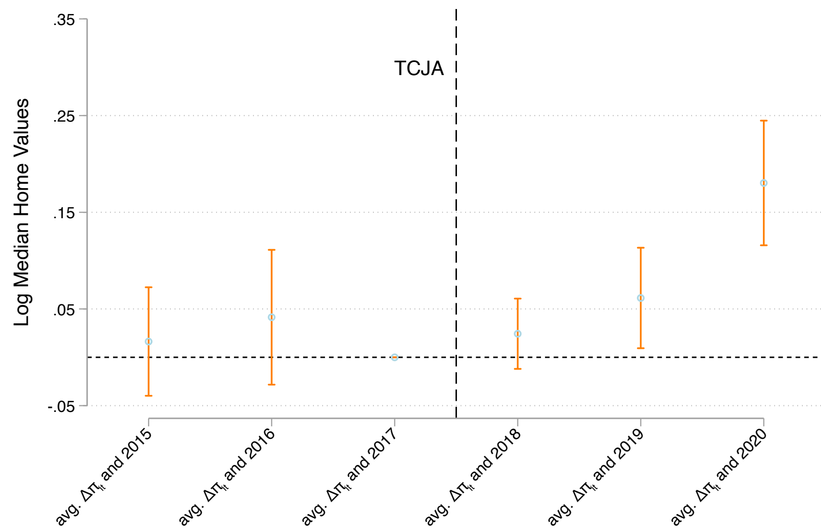
$$\mathbf{y}_t \sim \text{Bernoulli}\big(\mathbb{P}^{\star}_t(T, \theta, \mathbf{y}_0^{t-1}, \mathbf{x}_i)\big), \ \forall t \tag{2.3}$$

$$\mathbb{P}^{\star}_t(T, \theta, \mathbf{y}_0^{t-1}, \mathbf{x}_i) = \mathbf{G}_t(\mathbb{P}^{\star}_t(T, \theta, \mathbf{y}_0^{t-1}, \mathbf{x}_i)), \ \forall t \tag{2.4}$$

Equation 1 is the expected discounted sum of the weighted sum of neighborhood-specific latent profits (and by extension, median home values and an increasing function of investment), with discount factor $\rho$. Equation 2 is the policy resource constraint. There are $K$ neighborhoods that can be designated for the tax credit, and they must be eligible according to the program constraints i.e. sufficiently low-income or high-poverty. Equation 3 is the law of motion, governing how new development evolves in the city. Equation 4 is the full-information rational-expectations equilibrium constraint that governs how $\mathbb{P}^{\star}_{it}$ are interrelated across space. Ignoring the Pareto weights, the planner's problem is equivalent to which neighborhoods a developer would select for the tax credit if offered exclusive rights to develop the city. In other words, the optimized criterion is the value that a single developer should be willing to bid for the tax credits in an auction.

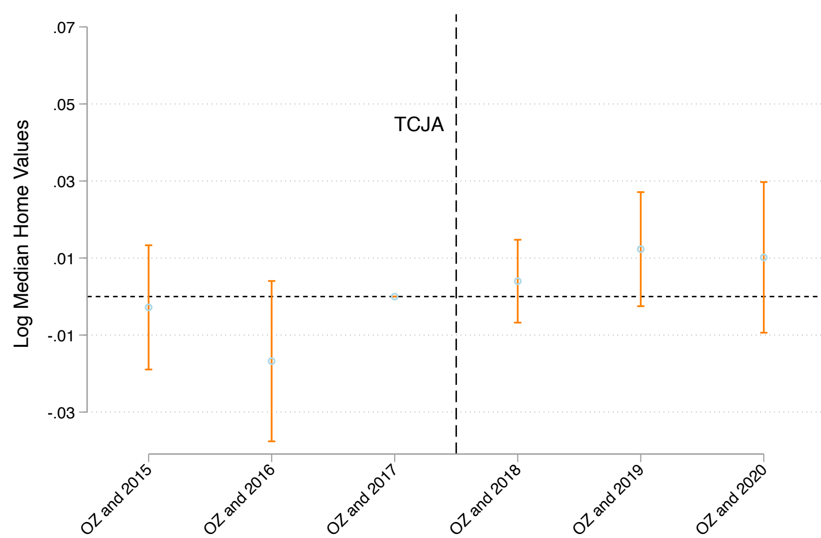
In practice, solving for the optimal policy requires simulating all conditional distributions $\mathbf{y}_t|\mathbf{y}_{t-1}, ..., \mathbf{y}_{t+1}|\mathbf{y}_{t-1}, ...,$ and beyond. This is computationally difficult. Moreover, if dynamics are strong and the discount rate is high, optimal policy may be unduly responsive to initial conditions, which are in part due to randomness. Thus, I take a simpler approach and focus on the stationary distribution of investment. While there is flexibility in choosing the

Figure 2.7: Log median home value changes




(a) Treatment: $\overline{\Delta\pi_i^\star}$

Note: This figure contains estimates from a difference-in-differences model where treatment is $\overline{\Delta\pi_i^\star}$. The sample only includes census tracts with median home value data for all years. The sample covers years 2015 through 2020. Errors are clustered at tract-level.




(b) Treatment: OZ status, after controlling for quadratic in $\overline{\Delta\pi_i^\star}$

Note: This figure contains estimates from a difference-in-differences model where treatment is OZ status. I control for a quadratic in $\overline{\Delta\pi_i^\star}$ interacted with year. The sample only includes census tracts with median home value data for all years. The sample covers years 2015 through 2020. Errors are clustered at tract-level.

$\omega_i$, I take $\omega_i = 1$ in my baseline calculation. This is motivated by the equity considerations already included in the eligibility constraints. Moreover, while we may be concerned about inducing home value appreciation in areas with a large number of renters, Section 2.4 found no evidence for local rent increases by 2020. I solve this mixed-integer, linear programming problem numerically.[44]

The city planner faces several trade-offs in this problem. Should they target neighborhoods that look like particularly good opportunites to induce investment and home value appreciation? Or areas, that through spillovers, can have a large response to the tax credit? Clustering the tax credit results in diminishing spillovers. However, many of the neighborhoods with larger spillover responses have nearby areas that also respond more to the tax credit. Central to the optimal policy problem will be the number of tax credits available, as well as the choice set and locations of neighborhoods that can be designated.

### Results

**Case Study - Philadelphia:** To illustrate this framework in practice, I focus on Philadelphia. Philadelphia offers an interesting case study. It is a large city, with a large number of eligible neighborhooods. Of its more than 400 census tracts, nearly 20% were designated for the tax credit. This is substantially more than the 14% for the average city in my sample. Consequently, the program effects are larger for Philadelphia than for other cities. The solutions to Philadelphia's optimal policy problem are mapped in Figure 2.8.

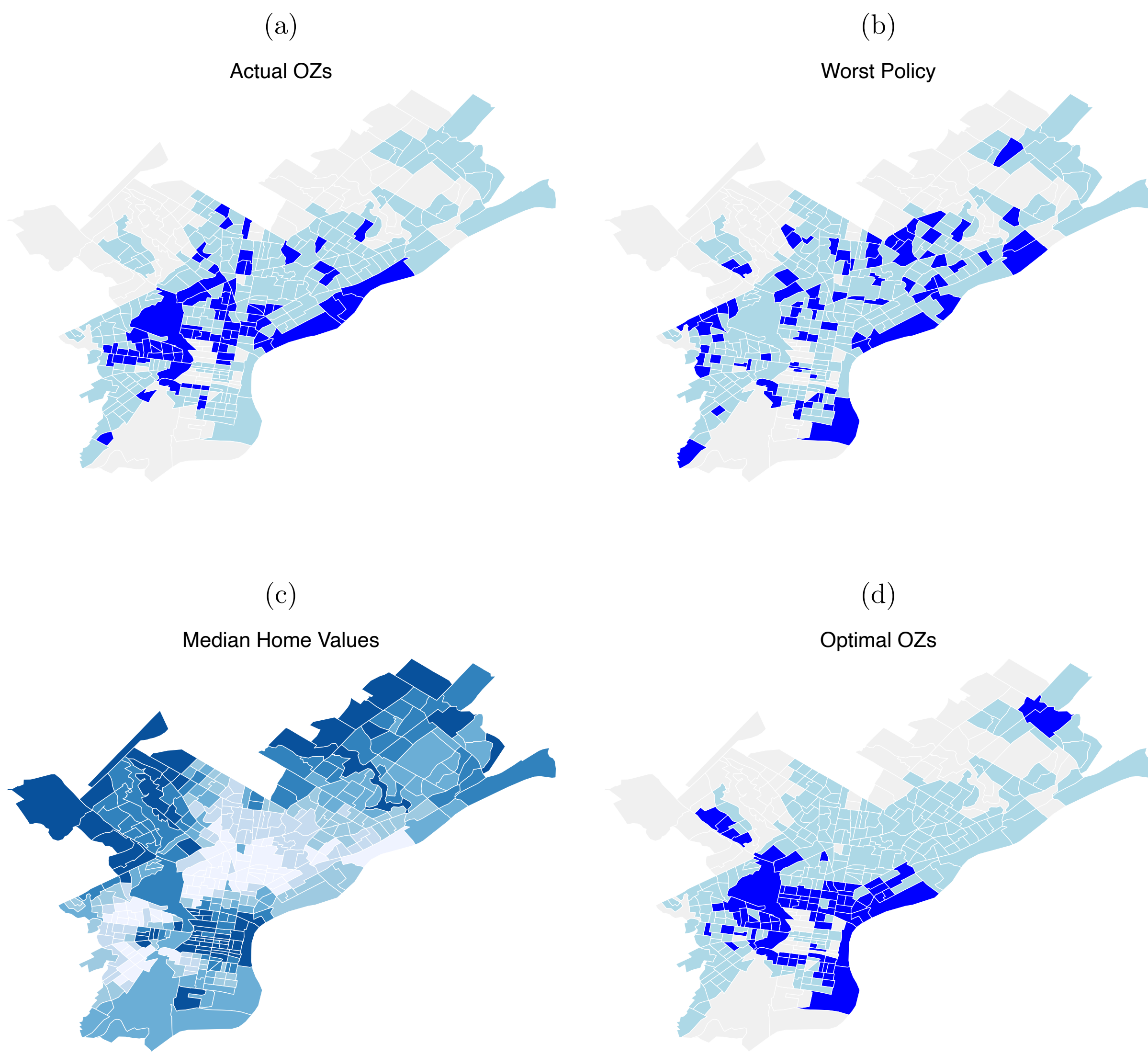
Before moving to the optimal policy, I first solve the "disoptimal" problem - the designation of neighborhoods to minimize aggregate latent profits. The actual choice of OZs and the worst choices are depicted in Figure 2.8a and Figure 2.8b, respectively. Ineligible neighborhoods are colored gray, eligible neighborhoods are in light blue, and OZs are in dark blue. The actual designations are clustered, particularly in higher home value areas near Center City and across the Schuylkill River into University City. A number of isolated tracts are chosen north of the downtown area. Some of these neighborhoods are also designated under the worst policy. In general, the worst policy tends to pick isolated neighborhoods in areas on the periphery of the city. These higher home value areas lead into more affluent suburbs. In all, the actual OZs increased investment by 5.8% and home values by 1.1% in the city.[45] The worst OZs increase investment by 1.9% and home values by 0.4%.

Given the critical role of home values in interpreting these findings, I map median home values in Figure 2.8c against the optimal designated tax credits in Figure 2.8d. Philadelphia, like many cities, has a central downtown area with high home values. Home values decline away from the city center before increasing again into the suburbs. The optimal policy

[44] This already difficult problem is made worse by the fact that the objective function does not have a closed-form representation, and must be simulated. I limit $\beta(\mathbf{x}_i)$ to be positive, setting a policy effect floor for the few neighborhoods whose covariates predict negative effects of the policy. Additional estimation details are included in Section B.5.

[45] These home value calculations are based on the regression results from earlier in this section. The coefficient for the average change in profits on 2020 log median home values was 0.18.

Figure 2.8: Philadelphia: actual, worst, and optimal OZs




Note: these maps different OZ policies for census tracts in Philadelphia. In the top left are the actual OZs. In the top right are the worst OZs. The bottom left shows 2015 median home values by neighborhood. The bottom right depicts the optimal OZs. For the policy maps, ineligible neighborhood are in light gray, eligible neighborhoods are in light blue, and OZs are in dark blue.

depends on this gradient in two ways. First, despite diminishing spillovers in the number of nearby OZs, the optimal designations are clustered, relying on larger direct and indirect effects of the policy to compensate. Second, the optimal policy prefers clustering in areas where the gradient moves from higher home values to lower home values. Here, neighborhoods are less inframarginal with respect to the program. There are no optimally chosen OZs in the center of Philadelphia's downtown, despite the fact that these areas have received much new residential and commercial development in the past. In all, the optimal policy increases investment by 8.8% and city-wide home values by 1.6%, substantially greater than those under the actual policy. The optimal policy also does so by targeting many low-income neighborhoods.

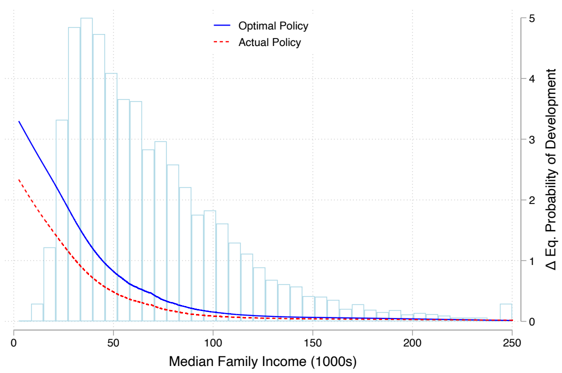
The same policy maps are depicted for Columbus, Ohio in Figure B.24 and Dallas, Texas in Figure B.25. The optimal policy for Columbus shares many similarities with that for Philadelphia. It also concentrates the tax credits in low to middle home value areas near the city center. The optimal policy clusters substantially more than the actual selections for the tax credit. Ultimately, the optimal choices depend on the underlying economic geography, the set of eligible neighborhoods, and the number of neighborhoods to be designated. For example, Dallas had a large share of eligible neighborhoods, but many fewer OZs allocated to the city. The optimal policy clusters the tax credits to a lesser degree. It selects the most promising neighborhoods because indirect effects are limited with so few OZs available. Cities with less spatially-correlated home values, developable land, and location heterogeneity terms $\alpha_i$ also exhibit this pattern. I now describe optimal OZs and generalize the above evidence for all neighborhoods in my sample.

**All cities:** I now aggregate the predicted investment and home value increases across all neighborhoods. Under the actual OZ program, new development increased by 2.7 % and home values increased 0.6 %. Under the worst policy, new development increases 0.8 % and home values increase 0.3 %. Under the optimal program, new development increases 4.5 % and home values increase 0.8 %. The actual OZs performed significantly better than the worst policy in terms of attracting investment and home value appreciation. However, the optimal program is a substantial improvement over the neighborhoods that were designated.

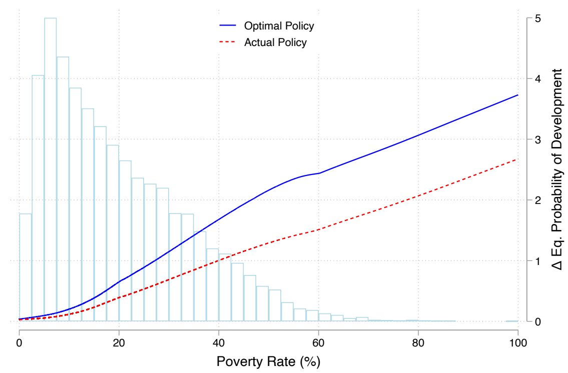
Given the eligibility constraints, the neighborhoods that benefit most from this program will largely be low-income and high-poverty. The neighborhoods near them, which also tend to be low-income, will benefit indirectly through spillovers. To see this point directly, I plot changes in investment due to both the actual and optimal programs in Figure 2.9. These investment changes are plotted against a neighborhood's median family income, poverty rate, and home values. There is a strong positive relationship between a neighborhood's poverty rate and its equilibrium response to the OZ program. The investment response is also stronger among lower income and lower home value neighborhoods. These facts hold true for the optimal program as well. Moreover, while OZs will be made worse off if they are not selected under the optimal policy, the optimal policy increases investment across the entire distribution of neighborhood poverty rates, median family incomes, and home values.

Taken together, these results suggest the crucial role that a place-based policy's spatial

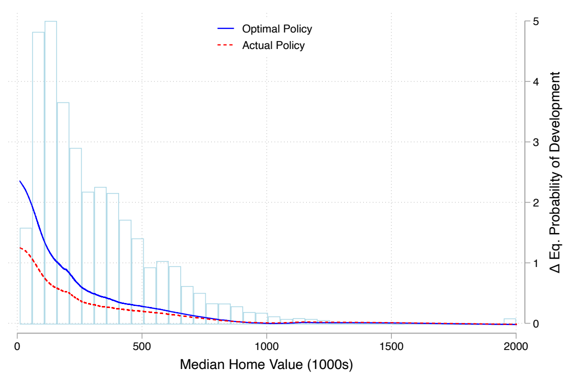
Figure 2.9: Actual versus optimal policy




(a) Median Family Income




(b) Poverty Rate




(c) Median Home Value

Note: This figure shows estimates of the change in the equilibrium probability of new development $\bar{\mathbb{P}}^\star(T) - \bar{\mathbb{P}}^\star(0)$ across various implementations $T$ of the investment tax credit. The actual OZ program is in red and the optimal one is in blue. This change is plotted against several tract-level covariates: median family income (top left), poverty rate (top right), and median home value (bottom). All tract-level covariates are from the 2011-2015 ACS. The lines depict predictions from a locally-weighted regression via lowess smoothing. A histogram of the covariate is included in the background in light blue.

design plays in the response of economic activity. Not only does it offer scope for reconciling the mixed evidence on place-based policies to date (Neumark and Simpson, 2015), but it suggests that there are large efficiency and equity gains that can be had under alternative implementations.

**Characterizing optimal OZs:** Table 2.8 correlates optimal OZs and actual OZs with 2011-2015 5-year ACS demographics among eligible tracts. The regressions include city fixed effects. Column (1) shows that optimal OZs tend to be less populated, lower-income, and have higher poverty rates. These results are largely true for actual designated OZs as well. However, the share of the population with a college degree is significantly predictive of being selected as an OZ, whereas it is not for optimal OZs. Column (3) shows that within cities, being an optimal OZ is associated with a 30% increase in the probability of being selected for the tax credit. In the entire sample, 44% of actual OZs are chosen by the optimal program.[46] After controlling for whether a neighborhood is selected under the optimal program, I find that the college-educated population share still remains an important predictor of actual OZ designation. Actual OZs were lower-income and less-dense as well. These results suggest that even though designations for the tax credit were lower-income, they did not result in a greater investment response in lower-income areas.

**Cost-benefit analysis:** The above findings offer scope for a simple cost-benefit analysis. I add up all property value increases and subtract off the federal cost of the program (an approach taken in Chen et al. (2023), for example). In 2017, the $11,936$ census tracts in my sample had an average of 747 owner-occupied units with a median home value of \$360k. These numbers, combined with the model estimates, imply an aggregate increase in property values of \$19.3 billion. This is close to the consensus point estimate of \$20 billion in Chen et al. (2023). Due to a reasonable amount of skepticism in self-reported home values during the pandemic, I also perform the same calculation with median home value increases equal to the lower limit of their confidence intervals in Section 2.8. This generates an aggregate increase in property values on the order of \$11.8 billion.[47]

The JCT estimates that the OZ program will cost \$3.4 billion per year. Not all of this will flow into my sample of neighborhoods, but the evidence in Kennedy and Wheeler (2022) suggests that most of the investment so far has gone to larger cities. Conservatively, I use the

[46] The optimal program is unable to improve upon the actual designations for eight cities / boroughs in my sample. These cities contain 13% of all OZs. These cities have very little new development in general, and a smaller number of eligible neighborhoods to choose from. Both OZs and optimal OZs are in areas that were "redlined" - an institutional practice begun in the 1930s that restricted lending to these areas. Neighborhoods were graded on their riskiness, and areas that were grade C ("declining") or D ("hazardous") experienced long-run, persistently worse economic outcomes (Aaronson and Mazumder, 2020; Hynsjö and Perdoni, 2022). Among OZs in cities with redlining map data, 26% were grade C and 35% were grade D neighborhoods. These fractions are similar for optimal OZs as well, at 28% and 32% respectively. Data for redlined 2010 census tracts comes from Meier and Mitchell (2020).

[47] This follows the conservative approach taken in Busso et al. (2013).

Table 2.8: Characterizing optimal and actual OZs

| | (1)<br>OZs (optimal) | (2)<br>OZ | (3)<br>OZ | (4)<br>OZ |
|---|---|---|---|---|
| **OZs (optimal)** | | | 0.301*** | 0.273*** |
| | | | (0.0152) | (0.0155) |
| **Log Median Family Income** | -0.0553** | -0.112*** | | -0.0971*** |
| | (0.0236) | (0.0253) | | (0.0241) |
| **% Poverty, 2015** | 0.00480*** | 0.00165** | | 0.000337 |
| | (0.000704) | (0.000752) | | (0.000725) |
| **Log Population, 2015** | 0.00214 | -0.0522*** | | -0.0528*** |
| | (0.0121) | (0.0121) | | (0.0116) |
| **% Female, 2015** | -0.00147 | -0.00526*** | | -0.00485*** |
| | (0.00123) | (0.00130) | | (0.00123) |
| **% White, 2015** | -0.000453 | -0.000550 | | -0.000426 |
| | (0.000429) | (0.000447) | | (0.000435) |
| **% Black, 2015** | 0.00102*** | 0.00139*** | | 0.00111*** |
| | (0.000393) | (0.000408) | | (0.000392) |
| **% High School, 2015** | -0.00238*** | -0.00324*** | | -0.00259*** |
| | (0.000743) | (0.000805) | | (0.000793) |
| **% College, 2015** | 0.00138 | 0.00379*** | | 0.00342*** |
| | (0.000879) | (0.000931) | | (0.000912) |
| **Log Median Home Value, 2015** | -0.0193 | -0.00411 | | 0.00117 |
| | (0.0164) | (0.0169) | | (0.0162) |
| **Observations** | 6,073 | 6,073 | 6,073 | 6,073 |
| $R^2$ | 0.092 | 0.082 | 0.127 | 0.149 |
| **Dep. Var. Mean** | .2062 | .2137 | .2137 | .2137 |
| **Fixed Effects** | City | City | City | City |
| **Sample** | Eligibles | Eligibles | Eligibles | Eligibles |

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: This table contains regression results of optimal OZ and actual OZ status on 2011-2015 5-year ACS demographics. All regressions use only eligible tracts in my sample that contain all relevant ACS covariates. All regressions include city fixed effects.

JCT's total estimated costs. For the three years from 2018 through 2020, costs in foregone tax revenues equal \$10.2 billion.

Taken together, these suggest a point estimate of net benefits at \$9.1 billion, and \$1.6 billion in the worst-case scenario. These estimates do not include benefits to cities outside my sample, or property value increases from non-homeowner occupied units (like many multi-unit residential and commercial buildings). The baseline estimate for the OZ policy's marginal value of public funds is 2.9 if policymakers care about the welfare of developers and each dollar of foregone tax revenue adds a dollar in profits for developers. If policymakers do not care about developer profits, then the marginal value of public funds drops to 1.9 (Hendren and Sprung-Keyser, 2020). If we assume that the costs of the program scales with total investment in OZs, the point estimate of net benefits would decline to \$7.4 billion under the optimal program. This is driven by increasing costs to funding the OZ investment.

## 2.9 Conclusion

The design of public policies meant to improve neighborhood outcomes is not well understood. This paper addresses these questions in the context of a spatial investment tax-credit: the Opportunity Zone program. Data on new developments, a form of investment targeted by the program, was collected for 12,000 neighborhoods. The empirical evidence indicates that new development has significantly increased in designated areas. The policy also increases development in nearby areas. Both the direct and indirect effects are larger in neighborhoods with more available land to develop, more elastic housing supply, and lower home values. Despite the increased supply of residential and commercial space, local home values appreciate as well.

A model is needed to capture these effects in equilibrium as well as counterfactual behavior under alternative designations for the tax credit. I build a spatial-equilibrium model of new construction projects at different locations within a city. The model matches the reduced-form facts and can explain the observed home value appreciation in OZs. Through the lens of the model, I find that the actual program increased new development by 2.7% and home values by 0.6% in aggregate. I then use the model to describe the city planner's optimal approach to choose neighborhoods for OZ designation. Under these alternative selections, new development would have increased 4.5% and home values 0.8% in aggregate.

The optimal program offers justification for clustering these tax credits. While there are diminishing spillovers in the number of nearby OZs, spatial correlation in the magnitude of direct and indirect effects dominates. The optimal program favors clustering tax credits in neighborhoods just outside the central downtown area. The optimal program in this paper suggests large opportunities for efficiency and spatial equity gains in how this place-based policy was implemented. Mixed evidence on the efficacy of prior place-based policies may, in part, reflect differences in how they were spatially designed. My work contributes to a literature documenting how the effects of place-based policies vary with their design (Briant et al., 2015), and considerations of what their optimal implementation looks like (Fajgelbaum and Gaubert, 2020; Gaubert et al., 2022).

The cost-benefit analysis suggests that property value gains from the program outweigh the federal costs through 2020. However, the approach in this paper is short-run and partial-equilibrium, and the measured benefits will accrue to developers and property owners. Much of the value of this program will hinge on whether the new investment translates into wage gains for workers, and neighborhood revitalization more generally. Moreover, my sample of neighborhoods contains those most likely to attract investment through the OZ program. Along those lines, more work is necessary to link this investment response with their effect on wages and employment, for incumbents and for new residents, and for all neighborhoods in the U.S.

# Chapter 3

# High-End Housing and Gentrification: Evidence from a San Francisco Lottery

## 3.1 Introduction

Policymakers in cities across the United States and around the world are grappling with how best to address surging rents and home prices. Since the 1990s, a steady influx of high-income workers to major cities has led to rapid increases in home prices, changes in demographic characteristics, and shifts in the composition of local businesses --- a process commonly referred to as gentrification.

In response to these trends, many economists and policymakers have advocated relaxing regulatory barriers that restrict market-rate housing developments in expensive cities (e.g., Furman, 2015; Glaeser, 2017; Hsieh and Moretti, 2017, 2019). Yet despite its popularity among economists, this policy prescription has proven highly controversial among the broader public. From the perspective of many observers, market-rate housing development seems to make problems worse: luxury condominiums sprout up in low-income neighborhoods, high-income residents continue to stream in, and local price growth continues unabated. Rather than taming the excesses of gentrification, opponents argue that market-rate housing developments cause and exacerbate it.

In this paper, we provide new evidence about the extent to which high-end, market-rate housing developments are a cause of gentrification. Empirical strategies to answer this question must address a fundamental econometric concern: Precisely because high-end developments are most likely to occur in neighborhoods with strong housing demand, comparisons between locations with and without such developments will overstate their role in driving neighborhood change. The econometric challenge is thus to isolate the causal effect of high-end developments independently from local shocks that may simultaneously induce neighborhood change.

To overcome this challenge, we study a unique administrative lottery in the city of San Francisco that permitted a limited number of property owners to legally convert buildings into high-end condominiums. In 1979, San Francisco banned condominium conversions due to widespread public concern about their effects on local home prices and the supply of rentable units. However, in response to growing cross-pressure from opponents of the ban, city officials in 1981 struck a compromise: Each year the city would run a lottery allowing a maximum of 200 winning units to convert their properties into condominiums.

Beyond the econometric appeal of the lottery, our focus on San Francisco and on condominiums is motivated by their central place in national debates about gentrification and housing policy. As we will discuss in greater detail, San Francisco has been a poster city of skyrocketing home prices and demographic change in recent decades, providing an ideal empirical setting for this research. At the same time, condominiums are front-and-center in controversies about housing policy not only in San Francisco, but in cities across the United States and around the globe. As a legal structure designed to facilitate ownership of units within multi-family buildings, condominiums are often attractive to high-income workers with preferences for living in dense urban areas. Condominiums are also generally exempt from rent control and tenant eviction protections that apply to other multi-family buildings, further fueling concerns about displacement of low-income residents.

To shed new light on these controversies, we study the long-run effects of lottery-induced condominium conversions in San Francisco on local home prices, demographics, and new business entry. To do so, we use annual lottery data from 2001 to 2013, including applicant information from both lottery winners and losers. We supplement the lottery panel with a rich and detailed suite of data sources and outcomes: address-level data on evictions, building permits, building characteristics, assessed home values, property sales, and homeowner-occupied status from the San Francisco Assessor's Office; block-level data on new business and establishment entry from SF Open Data; and tract- and block-level demographic data from the U.S. Census Bureau.

Our empirical strategy combines exogenous variation from the lottery with a stacked difference-in-differences design to estimate the causal effects of condominium conversions on key outcomes. This framework allows us to assess the validity of the research design using balance tests; to combine information across lotteries; to consider treatment dynamics over a 15 year period; and to move seamlessly from estimating the intention-to-treat (ITT) effect of winning the lottery to the local average treatment effect (LATE) of converting to a condominium.

Lottery winners and losers are statistically indistinguishable on key outcomes and characteristics as far back as 12 years before the lottery, implying the lottery was successfully randomized. After the lottery, we find that winners invest in costly new alterations and renovations to their properties, and on average see their home values increase by 45% 15 years later. Condominium converters see their home values increase by 53% over the same horizon. Lottery winners shift towards renting their units, and hold their properties in the near-term before selling at higher rates in the long-run.

A central controversy in policy debates focuses on the extent to which new condominium supply affects nearby home and rental prices. To address this question, we turn towards estimating price spillovers on nearby properties. Our empirical design offers a convenient setting for estimating these effects, by comparing nearby properties of lottery winners to nearby properties of lottery losers. We augment our main empirical specification with controls for the expected number of nearby lottery winners at various distances. This procedure addresses endogeneity concerns stemming from the fact that being close to a lottery winner is, in part, a function of location; the location is, in turn, potentially correlated with unobservable characteristics or shocks determining home values (Borusyak and Hull, 2023). Following a condominium conversion, we find that home values for parcels nearby lottery winners increase by 11%.

The finding that nearby home values increase is suggestive that condominium conversions may play a larger role in neighborhood change. To further explore this possibility, we adapt our lottery design to exploit exogenous variation in neighborhood-level exposure to condominium conversions. Over a horizon spanning approximately 15 to 20 years, we find that an additional lottery winner increases home values, rents, the population of high-income residents, and shares of the population that are White and college-educated. Using data on new business formation, we also find that lotteries lead to increases in establishments specializing in education, real estate, and professional services.

Heterogeneity analyses suggest that the effects of condominium conversions on neighborhood-level home price appreciation are smaller in neighborhoods with initially higher poverty rates and Hispanic population shares, and larger in neighborhoods with initially higher shares of college graduates. These results are consistent with a wealth of qualitative research in sociology arguing that demographic characteristics such as income, race, and education are key mediators of gentrification in American cities (Zukin, 1987; Lees et al., 2013; Freeman, 2005). Overall, the results imply that supply-side housing policies play a significantly larger role in gentrification than has been previously documented in the existing literature.

Our study contributes to a growing body of research on the determinants of housing prices and gentrification in cities. Existing studies have emphasized that demand for low-income neighborhoods reflects changes in employment and amenity opportunities in the city core (Diamond, 2016a; Almagro and Dominguez-Iino, 2021; Couture et al., 2019). At the same time, a budding and complementary literature has examined the role of supply-side drivers of local home price appreciation, like new construction (Asquith et al., 2019a; Pennington, 2021). Policy debate further focuses on how the quality and price of housing responds to urban development policies such as zoning, rent control, and eviction protections (Autor et al., 2015; Diamond et al., 2019). Our paper contributes to frontier research that uses highly credible empirical variation, detailed data, and detailed analysis of counterfactuals to study the impacts of key urban policies on local home prices and demographics.

To our knowledge, Boustan et al. (2019) is the only other paper to consider the role of condominiums in urban change. Boustan et al. (2019) instrument for city-level variation in condominium density with regulatory changes governing conversions. They find no relationship between condominiums and resident income, education, or race. Their work leaves open the possibility that condominiums could affect the distribution of individuals and incomes within the city. In our study, we focus on the experience of one city — San Francisco —

but provide credible estimates of the within-city housing and neighborhood effects of condominium conversions. Our work is closely related to Diamond et al. (2019) and Pennington (2021), both of which consider the setting of San Francisco, and study the local effects of rent-control and new construction, respectively. Our paper is also similar in spirit to Greenstone et al. (2010), who study the spillover effects of plausibly exogenous commercial plant openings on local labor markets. By contrast, we study the spillover effects of high-end residential buildings on local home prices and demographics.

The rest of the paper is organized as follows. Section 3.2 discusses the history of condominium conversions and our institutional setting. Section 3.3 discusses the data, and section 3.4 discusses how we implement the lottery design in a regression framework. Section 3.5 presents our estimates of the effect of winning the lottery on winning and nearby parcels. Section 3.6 studies the impacts of condominium conversions on neighborhood outcomes, finding a significant effect on demographic outcomes normally associated with gentrification. Section 3.7 concludes.

## 3.2 Background and Setting

### Changes in Demographics and Housing Markets in San Francisco and Other Major U.S. Cities

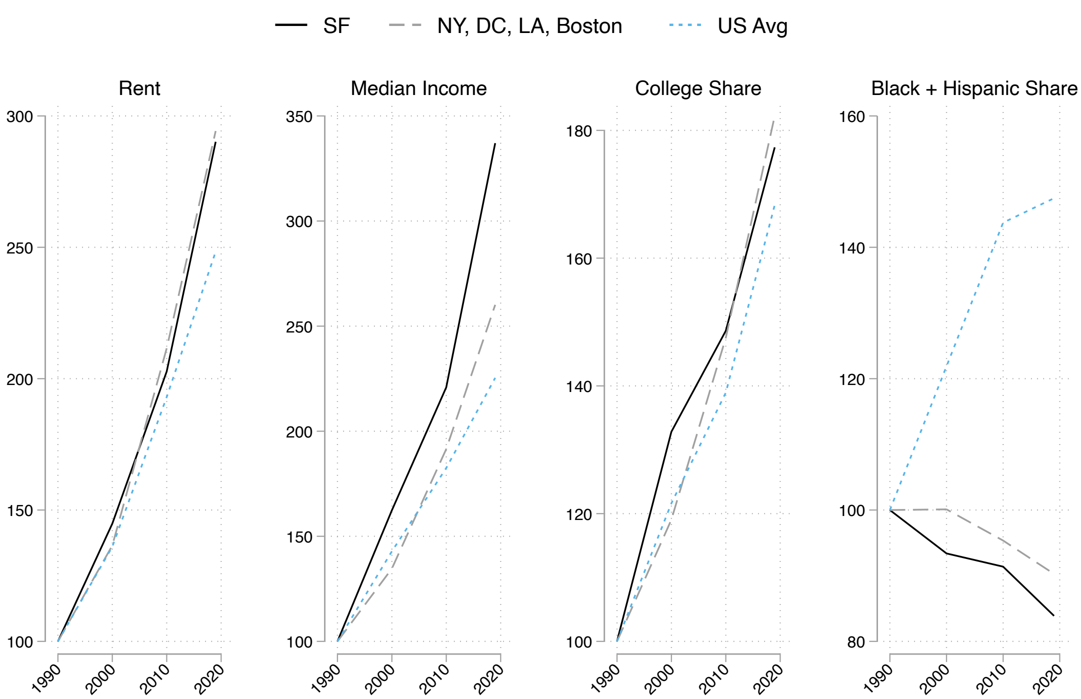
San Francisco provides an ideal setting for an empirical study of gentrification and housing supply, for two reasons.

First, like many major American cities, San Francisco has undergone dramatic demographic changes since the 1990s (Couture and Handbury, 2023). Panel A of Figure 3.1 plots time series for San Francisco, selected major American cities, and the U.S. national average in rental prices, median family incomes, the share of the population that is college-educated, and the share of the population that is Black or Hispanic. All values are indexed to 100 in 1990. Relative to the national average, San Francisco, New York, Washington DC, Los Angeles, and Boston have seen meteoric growth in rents, median family income, and the share of the population that is college-educated over the last three decades. Moreover, while the country as a whole became increasingly diverse over this period, the share of the population that is Black or Hispanic declined in San Francisco and other major U.S. cities.

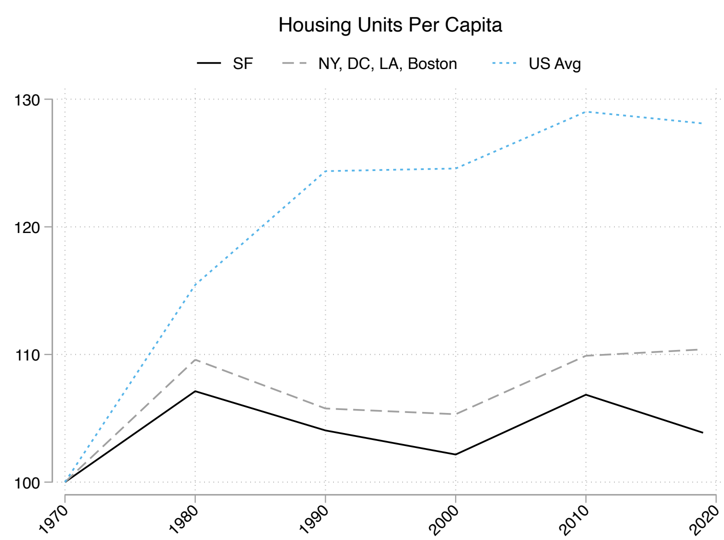
Second, Panel B shows that San Francisco, like other major cities, has failed to increase the per capita supply of housing at a pace consistent with the national average. In a world of scarce housing, changes to the existing stock of units, like condominium conversions, could play an outsized role in shaping broader demographic trends.

Figure 3.1: Gentrification in San Francisco and Other U.S. Cities

Panel A: Rents and Demographics




Panel B: Housing Units Per Capita




*Notes*: The unit of analysis is a census tract, and data are from the Neighborhood Change Database. In Panel A, data values are indexed to 100 in 1990, the earliest year consistently available for these outcomes. In Panel B, data values are indexed to 100 in 1970.

### The Rise of Condominiums

A condominium is a legal form of ownership for housing units, often thought of as apartments, within a multi-unit building. An individual owns the unit itself, while common spaces (such as elevators, hallways, stairwells, and yards), building infrastructure (such as heating and water pipes), and the land under the building are jointly owned by residents. In the U.S., laws governing this ownership structure were first passed in Puerto Rico in 1958. The Federal Housing Administration began to insure mortages on condominiums as part of the National Housing Act of 1961 (Kerr, 1963). Over the next few years, states passed their own laws authorizing condominiums en masse. By 1969, all U.S. states had passed such statutes (Boustan et al., 2019).

Condominiums can be added to the local housing supply either through new construction or by converting existing units. In the 1970s, city lawmakers became concerned that conversions were drastically reducing supply in the rental housing market, leading to higher prices and displacing renters. In response, several cities passed ordinances to limit or prohibit this behavior (Boustan et al., 2019).[1] Public debates over condominium developments and conversions have recently reemerged. For example, in response to growing public concern, New York state legislators passed a law in 2019 requiring approval from current tenants to convert a building into condominiums.[2] Following passage of the law, conversions in New York City declined sharply by 80%.[3]

### San Francisco Lottery

By 1979, condominium conversions were commonplace in the Bay Area, doubling each year since 1975. For approximately 15% of Bay Area municipalities, conversions comprised 10% or more of the existing rental stock (Ichino, 1979). Following several high-profile conversions that lead to the eviction of long-term tenants, San Francisco city officials moved to regulate the practice.[4] New regulations were designed to "prevent the displacement of existing tenants," "reduce the impact of conversions on nonpurchasing tenants who may be required to relocate," and "prevent the effective loss of the City's low or moderate income housing stock" (San Francisco Board of Supervisors, 2004). Starting in 1981, the City prohibited conversions for buildings with more than six units. Buildings with fewer than six units could still convert, but owners were required to apply and win the right to do so through a lottery process.[5] These buildings had to be Tenancy-in-Commons (TICs), a cooperative legal form in which the property is jointly owned. Because of the unusual owner arrangement,

[1]The timing of these laws serve as an instrumental variable for city condominium density in Boustan et al. 2019.

[2]Wall Street Journal. "New York Condo Conversions Near the End, a Casualty of Rent Reform." 2019.

[3]The Real Deal. "Rental-to-condo Conversions Drop 80% After 2019 Rent Law: Report." 2021.

[4]Discussions over condominium conversions were regularly in the newspaper: "Condos Put Squeeze on Rentals" (San Francisco Chronicle, December 1977), "S.F. Problem That's Hard to Live With" (San Francisco Chronicle, March 1979).

[5]Sirkin-Law. "Summary of San Francisco Condominium Conversion Rules." 2022.

difficulty in mortgage financing, and tenant protections, TICs are often an intermediate step as their owners pursue converting to a condominium.[6]

City officials in San Francisco limited the total number of units eligible to convert by lottery each year to 200 units. A lottery applicant is an entire building. The lottery consists of two separate applicant pools vying for 100 units of eligible conversions: Pool A and Pool B. Pool A contains applicants who applied to (and lost) three or more prior lotteries, and imposes some restrictions on tenant eviction history, ownership, and occupancy. Prior to 2006, a simple lottery was run among Pool A applicants if the the number of units in pool A exceeded 100 units. Any unallocated units were added to Pool B. From 2006 until 2013, applicants were grouped and ranked by the number of times they had previously lost the lottery. If the first group included fewer than 100 units, all lottery applicants were allowed to convert. Any remaining units were allocated to the second group, and so on, until the final group's total number of units was larger than the remaining number of units available in Pool A. At that point, those units were randomly allocated among that group's applicants. In Pool B, each applicant receives additional tickets equal in number to the times they have previously lost the lottery. Tickets were then drawn randomly until 100 units were deemed eligible for conversion.[7] Prior to 2006, the number of tickets was limited to be at most five (San Francisco Board of Supervisors, 2005).

Lottery tickets were priced at $250 each, but upon winning, the conversion application required additional fees. Inspection plus application fees, on average, totaled approximately $13,000. For some 5-6 unit buildings, an additional charge of $1,700 was levied by the state of California. A mandatory engineering survey of the building costed at least $8,000.[8]

After a growing backlog of lottery applicants, San Francisco halted the lottery program in 2013. City officials replaced it with the Expedited Conversion Program (ECP) beginning in 2015. Under this new program, TICs satisfying certain ownership and occupancy requirements would be eligible to convert. Buildings that had been owned continuously for longer would be eligible first. A new Expedited Conversion Fee of $22,500 per unit would also be charged.[9] Buildings with renters are required to offer a lifetime lease upon conversion; due to legal challenges, the city stopped accepting conversion applications from buildings with renters in 2017.

### Application Behavior

We now summarize descriptive patterns in lottery applications. Figure C.1 describes how the probability of winning the lottery and reapplying to the lottery varies with the number of tickets recieved for the lottery. By design, the probability of winning the lottery increases with the number of tickets. Given high demand for the lottery, the probability of winning is still low (<30%) even after applying five times before. If a building had applied seven times

[6]KQED. "San Francisco Inches Toward Deal on 'Tenants in Common' Condo Conversions." 2013.
[7]Sirkin-Law. "San Francisco's Condo Conversion Lottery System." 2022.
[8]Cost estimates from: Sirkin-Law. "San Francisco's Condo Conversion Lottery System." 2022.
[9]GMH - Real Estate Law. "Condominium Conversion in San Francisco." 2019.

previously, and consequently was awarded eight lottery tickets, the probability of winning was close to 90%. Reapplication rates increase in the number of tickets a building recieves.

Figure C.2 plots how the probability of applying and winning varies with whether a building had applied a certain number of years ago. The light blue coefficient above an x-axis value of one captures how much more likely an individual is to apply if they had applied one year ago. The dark blue coefficients show the same coefficients for whether an individual won the lottery. The plot shows that most individuals that apply also reapply. About 12% of applicants win in a given lottery year, but 82% of lottery applicants reapply the following year. This suggest that over 90% of lottery losers reapply. Applicants dynamically selecting into lotteries is thus a minor concern in our setting, given that reapplication rates are so high. Second, over 60% of applicants had won a lottery within seven years.

This latter point is confirmed in Figure C.3, which plots the probability of being a lottery winner given the building lost the lottery a certain number of years prior. Many lottery losers reapply and become lottery winners soon after. A coefficient of 45% for an x-axis vaue of seven means that 45% of losers had eventually won seven years later. Simple comparisons in outcomes between lottery winners and losers are likely to bias down the effect of a condominium conversion, since many losers ultimately convert. This fact motivates the regression design we discuss in Section 3.4.

## 3.3 Data

Our primary outcome of interest is home values, for which we use the assessed value of land and structures for a parcel as given in the San Francisco Assessor's Office annual files. We further merge information about parcel-level building permits and evictions, as well as information about lottery applicants and winners. For neighborhood outcomes, we rely on data from the ACS and business registrations.

### Sources

Our main data come from four main sources, most available through `data.sfgov.org`.

**Property Tax Rolls (1999 - 2019)** : The San Francisco Office of the Assessor-Recorder makes the years 2007 through 2019 publicly available through their website. To this, we merge in years 1999 through 2006 provided to us directly by the assessor's office. This dataset contains information about the property location, type and construction type, number of bathrooms, bedrooms, rooms, stories, and units, local zoning, property area, whether the property is homeowner-occupied, most recent sale, and assessed value of land and improvements (structures).

**Building Permits (1983 - 2019)**: This dataset contains information on the parcel number, date, estimated cost, and type of building permits.

**Evictions (1997 - 2019)**: This dataset contains each eviction in the city of San Francisco, with its location, file date, and the reason for the eviction.

**Lottery Information (2001 - 2013)**: Lottery information on the applicants, the number of tickets they were assigned, and the winners was provided as part of a public records request (#17-1329 accessible through `https://sanfrancisco.nextrequest.com/`). Importantly, the number of tickets allows us to infer whether an applicant was in Pool A or Pool B of the lottery.

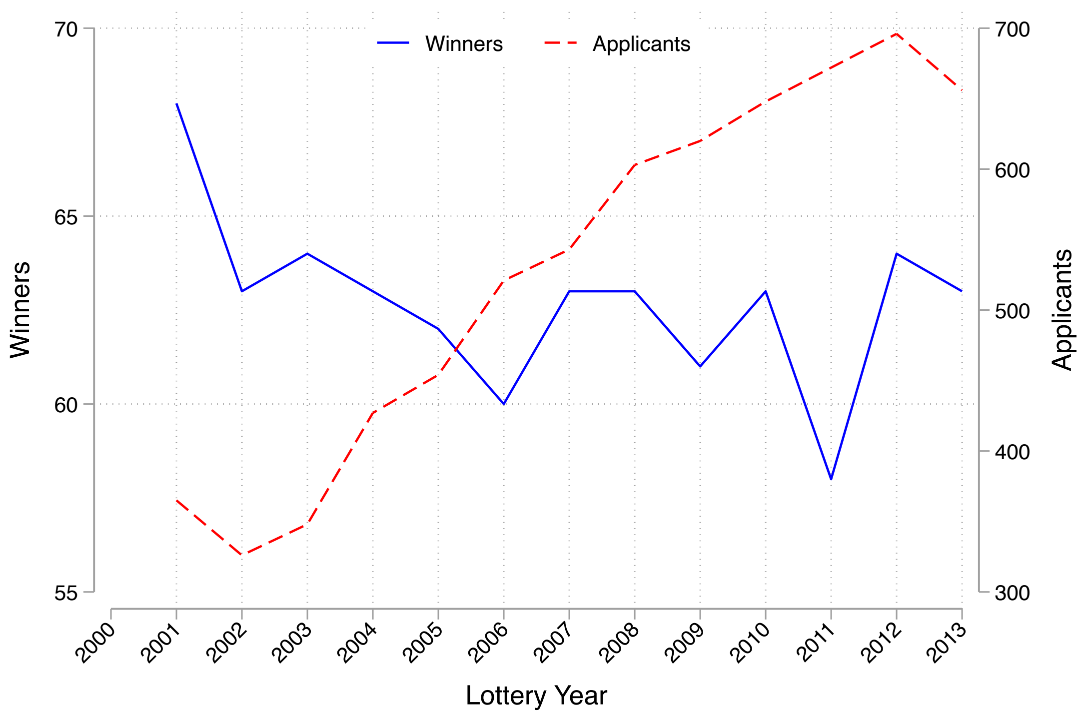
**Census & ACS Data (2000, 2013-2019)**: We collect ACS census block group-level data for years 2013 to 2019 for the city of San Francisco. This dataset contains information about demographics, income, home values, rents, and education. We also use tract-level outcomes from the 2000 census, concorded to 2010 census tracts in the Neighborhood Change Database.

**Business Registrations (2000 - 2019)**: The Office of the Treasurer and Tax Collector contains information on business registrations, including their location, sector, date of registration, and whether the business is still active or not. We aggregate this data to the census block group level, and tabulate counts of establishments in different sectors. We tally new establishments as well as the stock of active ones.

### Summary Statistics

Our lottery data begins in 2001 and continues until the lottery ended in 2013. Figure 3.2 presents time series for the number of applicants (right axis) and the number of winners (left axis) for each lottery in our sample. The number of winners remains flat at 60 to 65 per year. Each winner on average is a 3-4 unit building, leading to cumulative totals of approximately 200 units per year. Over the study period, the number of applicants nearly doubled. This dramatic increase ensured that winners were randomized even amongst pool A applicants for most years.

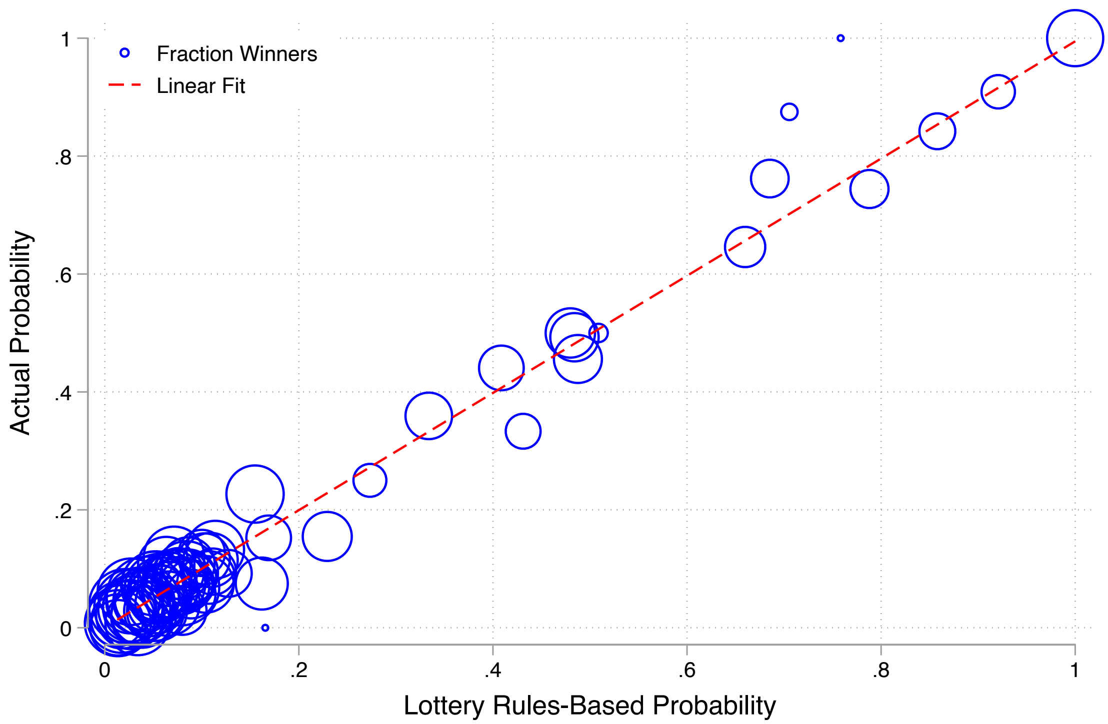
Figure 3.2: Lottery Applicants and Winners




*Notes*: The figure plots the number of lottery applicants (right axis) and winners (left axis) for each year of the sample. Data are from the City of San Francisco Assessor's Office.

Figure 3.3 plots our estimate of an applicant's probability of winning, based on rules stipulated in city ordinances, and the empirical probability that the applicants actually won. The plot shows that we are able to replicate the lottery's randomization procedure, with the line of fit precisely on the 45 degree line. The years 2001, 2006, 2010, and 2012 saw a sizeable fraction of applicants guaranteed winning in pool A — that is, these applicants had probability "1" of winning. These buildings accounted for 93 of the total 812 winners we observe. Our results on lottery winners, which fully match on the lottery propensity score, will effectively ignore variation from these applicants.

Figure 3.3: Lottery Probability of Winning

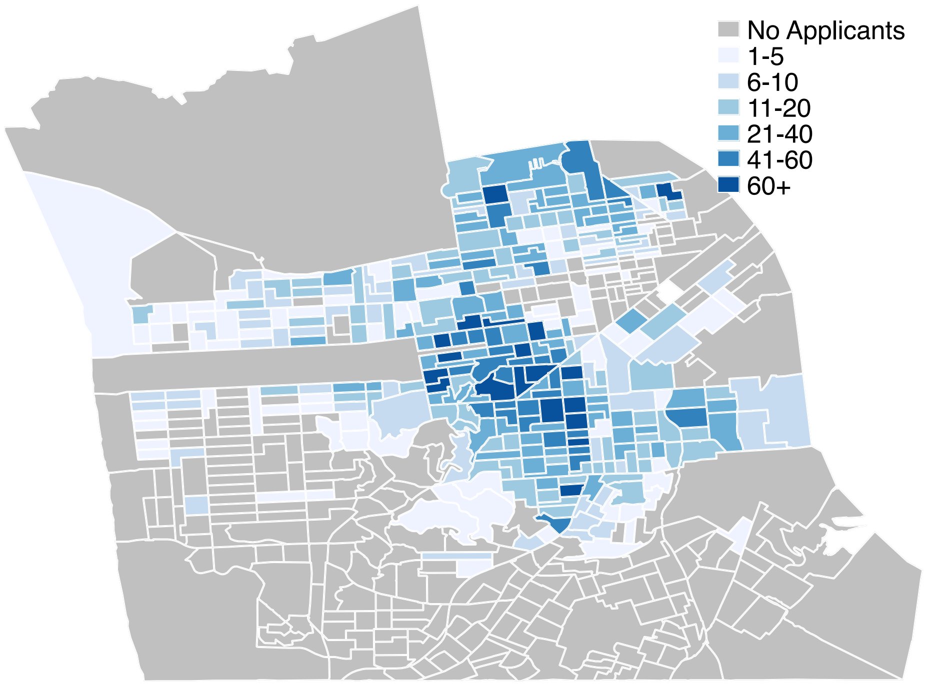



*Notes*: The unit of analysis is a lottery applicant, and marker sizes are proportional to the number of applicants. Data are from the City of San Francisco Assessor's Office. The x-axis reports each applicant's predicted probability of winning the lottery, based on the lottery rules and regulations described in section 3.2, and the y-axis reports the corresponding share of actual lottery winners. The dashed line shows the linear best-fit, which lies precisely on the 45-degree line.

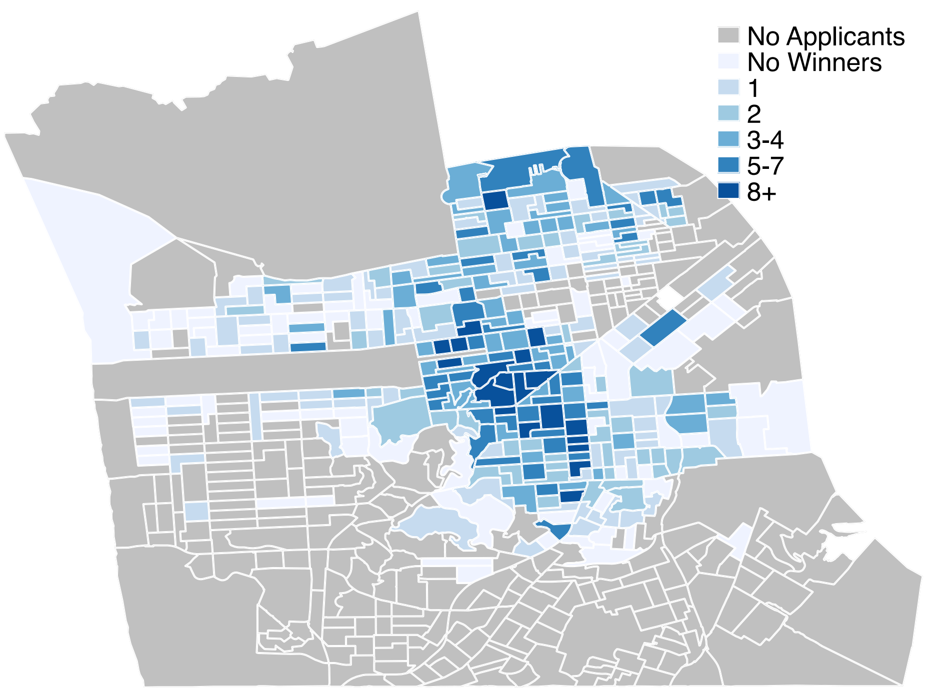
Figure 3.4 maps the geographic distribution of lottery applicants and winners. Neighborhoods like North Panhandle, Haight Ashbury, Duboce Triangle, and the Mission saw high demand for conversions as well as many lottery winners. Russian Hill had many applicants but few winners. Inner Sunset had few applicants but a surprising number of winners. This random geographic variation will be central to estimating the neighborhood effects of condominium conversion in Section 3.6. Our main set of findings will rely on locations with or near to lottery applicants. Consequently, neighborhoods like Outer Parkside and South of Market will be largely excluded from the analysis.

Figure 3.4: Map of Lottery Applicants and Winners

Panel A: Applicants




Panel B: Winners




*Notes*: The figure llustrates geographic variation in the cumulative number of lottery applicants (Panel A) and lottery winners (Panel B) across Census block groups over the sample period.

Summary statistics for winning and losing applications are presented in Table 3.1. The calculations are for two years prior to an application. Applicant buildings have around 15 rooms and 1300 sq. ft. per unit. The average assessed home value was nearly 1 million dollars for winners. Column (5) calculates the difference between building characteristics for winners and losers after controlling for lottery p-score and year fixed effects, and column (7) has the corresponding p-value. Reassuringly, no building characteristics are significantly different between lottery winners and losers prior to their application.

Table 3.1: Balance Table

| Summary Statistics | | | | | | | |
|---|---|---|---|---|---|---|---|
| | Winners | | Losers | | Difference | | |
| Parcel Characteristic | mean | s.d. | mean | s.d. | diff | s.e. | pval |
| Value (1000s USD) | 999 | 696 | 1,231 | 1,191 | 21 | 35 | 0.55 |
| Year Built | 1915 | 20 | 1915 | 20 | -0 | 1 | 0.60 |
| Homeowner | 0.67 | 0.47 | 0.58 | 0.49 | 0.00 | 0.02 | 0.95 |
| Evictions | 0.03 | 0.26 | 0.02 | 0.24 | 0.01 | 0.01 | 0.25 |
| Units | 3.23 | 1.09 | 3.24 | 1.21 | 0.02 | 0.06 | 0.68 |
| Rooms | 14.99 | 4.52 | 14.82 | 4.60 | -0.00 | 0.23 | 0.98 |
| Sq. Ft. Per Unit | 1,321 | 473 | 1,284 | 475 | 17 | 25 | 0.48 |
| Beds | 1.21 | 2.69 | 1.16 | 2.72 | 0.22 | 0.14 | 0.13 |
| Baths | 3.60 | 1.35 | 3.58 | 1.39 | 0.07 | 0.07 | 0.35 |
| Permits | 0.47 | 1.16 | 0.63 | 1.58 | -0.06 | 0.08 | 0.43 |
| Permit Costs (1000s USD) | 5.27 | 26.53 | 8.25 | 45.30 | -1.43 | 2.06 | 0.49 |
| N | 812 | | 6023 | | 6835 | | |

*Notes*: The table reports the means and standard deviations of building characteristics for winning and losing applicants two years prior to the lottery. The difference in means reported in Column (5) controls for the probability of winning the lottery and year fixed effects. Sample includes lotteries from 2001 to 2013.

## 3.4 Design

Our setting provides a unique randomized experiment for studying the effects of condominium conversions on building home values, investment, sales, and renting behavior. Framing this design within an econometric framework provides some complications however, driven largely by repeated applications of losing properties and multiple event times. We now discuss our data structure and framework for estimating the relevant treatment effects.

### Simple Lottery Design

We begin by discussing a regression implementation of the simple lottery design before generalizing to our setting. Let $y_{it}$ denote assessed home value, one of our key outcomes, for parcel $i$ in year $t$. Let $\tau_t(k)$ indicate whether year $t$ is $k$ years from the lottery and $\nu_i$ indicate whether parcel $i$ wins the lottery.

Applicants have unequal probabilities of winning the lottery according to how many tickets they purchase and the ticket composition of other applicants. The number of tickets that can be purchased depends on how many previous times an applicant has lost. Comparisons between lottery winners and losers may give misleading estimates of the effect of condominium conversions if, for example, applicants with the greatest expected housing price appreciation apply more frequently. Controlling for the probability of winning the lottery ensures that we rely on random variation generated by the lottery, rather than endogenous selection into the lottery. As such, we include $\chi_i(p)$ fixed effects, which indicate whether parcel $i$ had probability $p$ (from support set $\mathcal{P}$) of winning the lottery.

We model home values using the following equation:

$$y_{it} = \sum_{k\neq -1} \beta_k^{ITT} \tau_t(k)\nu_i + \sum_k \sum_{p\in\mathcal{P}} \gamma_{pk}\tau_t(k)\chi_i(p) + \varepsilon_{it} \tag{3.1}$$

The second summation term ensures that comparisons are made between parcels with the same probability of winning the lottery.[10] Following Abdulkadiroğlu et al. (2017), this type of full propensity-score matching ensures that the coefficients $\beta_k^{ITT}$ capture a convex-weighted average of the causal effects of winning in a building's specific lottery-strata. The coefficients $\beta_k^{ITT}$ map out the full set of intention-to-treat (ITT) effects — that is, the effect of winning on home values $k$ years before or after the lottery. Years prior to lottery implementation serve as balance tests, allowing to evaluate whether, within a lottery-strata, outcomes are similar prior to the lottery. Throughout, we cluster standard errors at the level of lottery applicant.

The ITT effects provide a transparent assessment of the lottery design. However, we are primarily interested in the effect on home values from parcel $i$ converting to a condominium at time $t$. We therefore augment the above design to an instrumental-variables setting, where we instrument for whether a parcel ever converts to a condominium after the lottery (given by $\kappa_i$) with whether a building won the conversion lottery.[11] The second-stage equation that relates home values to condominium status is given by the following equation:

$$y_{it} = \sum_{k\geq 0} \beta_k^{LATE} \tau_t(k)\kappa_i + \sum_k \sum_{p\in\mathcal{P}} \gamma_{pk}\tau_t(k)\chi_i(p) + \varepsilon_{it} \tag{3.2}$$

[10]The probability of winning a lottery is the same for almost all applicants who purchase the same number of tickets in a given lottery; consequently, specifications with lottery by ticket fixed effects produce nearly identical regression results.

[11]We instrument for this variable, rather than if a parcel is currently a condominium, so that we do not need to keep track of two sets of event times: one for lottery application and one for conversion.

We instrument for condominium conversion with winning the lottery in the following first-stage regressions:

$$\tau_t(k)\kappa_i = \sum_{k\geq 0}\beta_k^{FS}\tau_t(k)\nu_i + \sum_k\sum_{p\in\mathcal{P}}\gamma_{pk}^{'}\tau_t(k)\chi_i(p) + \varepsilon_{it}^{'} \tag{3.3}$$

We use this instrumental variables (IV) approach to assess how the magnitude and significance of the effects change when treatment is defined as a condominium conversion, rather than winning the lottery. Thus, we estimate and report $\beta_k^{LATE}$ for years after the lottery is implemented.

Intuitively, this empirical strategy is an instrumented difference-in-differences design comparing the outcomes of applicant lottery winners and losers with the same ex-ante probability of winning. The lottery design ensures independence of the instrument, but in this setting, only mean-independence of the potential outcomes and treatment assignment with respect to the lottery is required (Hudson et al., 2017). Testing for parallel-trends in the fully-interacted ITT specification offers a diagnostic to assess this assumption.

The exclusion restriction requires that winning or losing the lottery does not directly affect home values — that is, it requires that the lottery can only affect home values through the condominium conversion itself. This is a natural assumption since the lottery's sole purpose is to allow or prohibit condominium conversions. Monotonicity is guaranteed in this setting, as is one-sided non-compliance — regulation for these buildings prohibited condominium conversions except through the lottery while it was active. In Section 3.5, we show that the first-stage is strong, with more than 80% of winning properties converting. Consequently, the coefficient $\beta_k^{LATE}$ can be interpretted as the causal effect of converting to a condominium on home values at time $k$ after the lottery (Imbens and Angrist, 1994).

### Dynamic Lottery Design

Parcels that lost the lottery but had the same probability of winning are a natural control group for lottery winners. We now extend the above framework to a setting with repeated lotteries and where losing parcels continue to apply. We also rely on recent research on difference-in-differences designs with heterogeneity in treatment timing in order to implement our econometric analysis.

We first create thirteen (one for each lottery) simple lottery designs, composed of each applicant for each lottery year from 2001 to 2013. We then stack these observations according to each lottery $m$. The $\tau_{mt}(k)$ denotes whether year $t$ for lottery $m$ is $k$ years since the lottery was run. The $\nu_{im}$ is an indicator for whether the parcel won that lottery. The $\chi_{im}(p)$ are indicators that the parcel in a given lottery had probability $p$ of winning. Our new outcome $y_{imt}$ denotes assessed home value for parcel $i$ in lottery $m$ in year $t$.

Parcels have histories duplicated according to how many times they have applied. Consistent with Cengiz et al. (2019) and Baker et al. (2021), we construct a clean set of controls by only including observations for control parcels that are yet to convert to a condiminium. This ensures that we do not compare outcomes of previous winners and later winners, which would naturally bias down our results. If we were interested in the effect of winning the lottery, it would be reasonable to include observations from later winners in the control group, as those are downstream effects from losing the lottery. A dynamic approach like Cellini et al. (2010) could then be used to estimate the desired treatment effects. Given that our object of interest is the effect of condominium conversion rather than winning the lottery, our approach is more natural.

The ITT version of our main specification is as follows.

$$y_{imt} = \sum_{k \neq -1} \beta_k^{ITT} \tau_{mt}(k) \nu_{im} + \sum_k \sum_m \sum_{p \in \mathcal{P}} \gamma_{pmk} \tau_{mt}(k) \chi_{im}(p) + x'_{imt}\zeta + \alpha_{im} + \eta_t + \varepsilon_{imt} \quad (3.4)$$

While not necessary for a causal interpretation of the coefficients $\beta_k^{ITT}$, we include parcel by lottery fixed effects $\alpha_{im}$ in our main specification, for two reasons. First, the fixed effects reduce residual variation in the errors, which increases precision. Second, we drop observations for lottery losers that ultimately convert to a condominium. This occurs if lottery losers convert through the Expedited Conversion Program (implemented in 2013 and discussed in Section 3.2) or win in a subsequent lottery. The parcel fixed effects help address (i) possible selection bias among losing parcels that later convert, and (ii) an unbalanced sample stemming from different event-time coverage for each lottery. The fact that most losing applicants reapply, as shown in Section 3.2, lessens the first concern. Nevertheless, we consider several robustness exercises to explore these issues in Section 3.5. With parcel-lottery fixed effects, the coefficients $\beta_k^{ITT}$can be interpretted as a convex-weighted average of the underlying treatment effects for each lottery (Sun and Abraham, 2020).

While we do not include them in our main specification, we allow for additional controls $x_{imt}$, like neighborhood trends. These controls may adjust for random imbalances between lottery winners and losers, and allow us to assess the importance of sample attrition in the control group later in event time. We consider how robust our results are to their inclusion in Section 3.5. Additionally, an attractive feature of the dynamic setting is that it allows us to disentangle event-time effects from calendar-time effects, given by $\eta_t$, and to control for them separately.

This design corresponds to the "stacked" difference-in-differences design of Cengiz et al. (2019) and Baker et al. (2021). While other approaches to multiple event timings have been suggested (Borusyak et al., 2021; Callaway and SantAnna, 2020), the stacked difference-in-differences design offers greater transparency and most naturally accomodates our IV and spillovers analyses.

The LATE implementation estimates the following second-stage regression.

$$y_{imt} = \sum_{k \geq 0} \beta_k^{LATE} \tau_{mt}(k) \kappa_{im} + \sum_k \sum_m \sum_{p \in \mathcal{P}} \gamma_{pmk} \tau_{mt}(k) \chi_{im}(p) + x'_{imt}\zeta + \alpha_{im} + \eta_t + \varepsilon_{it} \quad (3.5)$$

We instrument for winning the lottery and eventually converting to a condominium $\kappa_{im}$ with winning the lottery through the following first-stage.

$$\tau_{mt}(k)\kappa_{im} = \sum_{k\geq 0} \beta_k^{FS} \tau_{mt}(k) \upsilon_{im} + \sum_k \sum_m \sum_{p\in\mathcal{P}} \gamma'_{pmk} \tau_{mt}(k)\chi_{im}(p) + x'_{imt}\zeta' + \alpha'_{im} + \eta'_t + \varepsilon'_{it} \quad (3.6)$$

As in the ITT specification, attrition in our control group due to the Expedited Conversion Program and later lottery winners might raise concerns over the independence of the instrument. However, the IV difference-in-differences relaxes the necessary assumptions to maintain a causal interpretation. We report coefficients $\beta_k^{LATE}$ for event times after the lottery was conducted. We cluster errors at the applicant level. This is particularly important in this context since applicant histories appear multiple times according to how many times they have entered lotteries.

### Spillovers Design

Evaluating the effects of condominium conversions on nearby home values is complicated by two features of our setting. First, treatment will depend on the number of winners at various distances. Second, while winning the lottery may be random, being located near a winner is likely not. For example, parcels in the city center are more likely to be close to winners than parcels on the periphery, and being closer to the city center is likely correlated with unobservable shocks that determine home values. We extend our approach in the previous section to account for these facts.

We consider all residential parcels $j$ within a fixed distance of any lottery applicant. For lottery $m$, we calculate the number of winning applicants within distance band $d$ given by $\tilde{\nu}_{jm}(d)$. Through repeated simulations of the lottery, we also calculate the probabilities for each distance band that any nearby lottery applicant wins and stack them into the vector $\tilde{\chi}_{jm}$. The ITT version of our main spillovers specification is as follows.

$$\begin{aligned} y_{jmt} = &\sum_d \sum_{k\neq -1} \beta_{dk}^{ITT} \tau_{mt}(k) 1(\tilde{\nu}_{jm}(d) > 0) \quad (3.7) \\ &+ \sum_d \sum_k \sum_m \tau_{mt}(k) f(\tilde{\chi}_{jm}, \gamma_{dmk}) + x'_{jmt}\zeta + \alpha_{jm} + \eta_t + \varepsilon_{jmt} \end{aligned}$$

In our main specification, $f$ captures all linear terms and first order interactions of the coordinates of $\tilde{\chi}_{jm}$. The vector $\gamma_{dmk}$ contains the coefficients on the terms in the function $f$. These parametric controls adjust for the fact that buildings near lottery applicants and winners are unlikely to be comparable with buildings that were not near lottery applicants and winners. This is the same insight as in Borusyak and Hull (2023), ensuring that we still rely on lottery variation to estimate the spillover effects while controlling for endogeneity due to a parcel's location. We focus on having any lottery winner a certain distance away as the treatment.[12] The parameters of interest $\beta_{dk}^{ITT}$ map the full set of spillover dynamics for

[12] The vast majority of parcels are at most near one winner.

each distance band $d$. Parcels are included as controls until their nearby applicants convert to a condominium, if ever. We map parcels to their closest applicant, and cluster errors at that location.

The IV model is estimated in the same way as before. We instrument whether the nearby winner ever converts to a condominium after the lottery with whether the nearby parcel wins the lottery. We estimate these coefficients for all event times greater than zero.

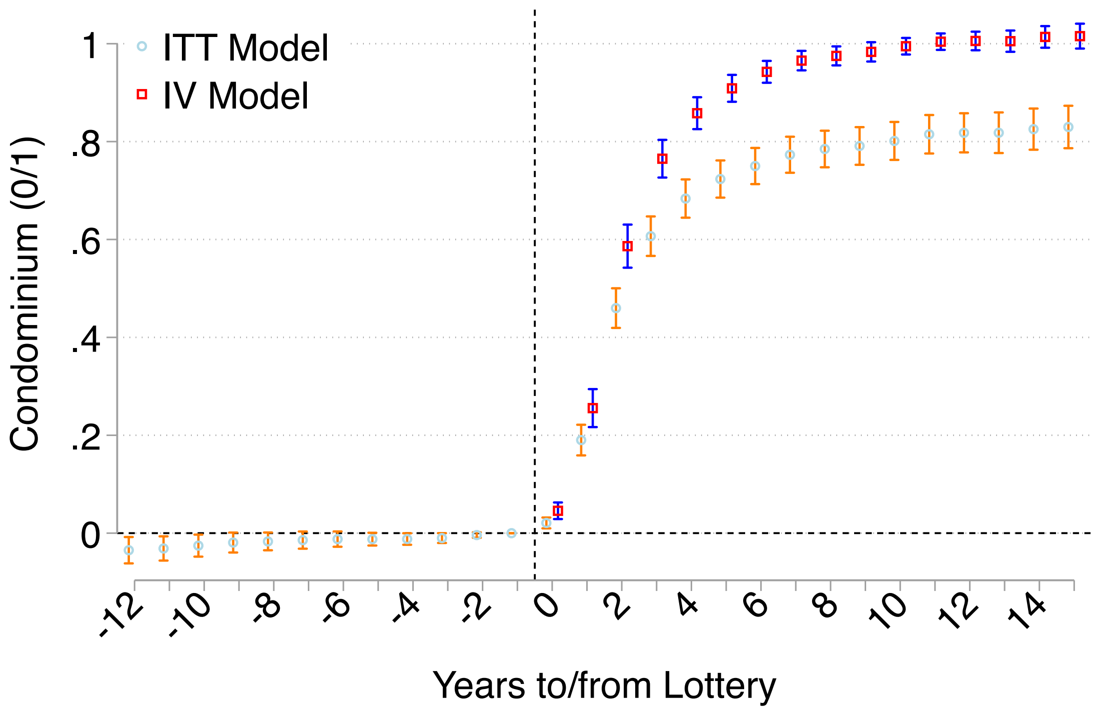
## 3.5 Results

In this section, we leverage the lottery design to estimate the causal impact of condominum conversions on winning and nearby properties. We find that winning property owners see large increases in assessed home values. Owners renovate their properties, rent them out, and eventually sell them 7 years on from the lottery. We also find large and highly localized price spillovers on nearby properties. Parcels within 25 meters of a winner see their home values increase 11% after 15 years, with this effect becoming insignificant farther away.

### First-Stage: Effects of Winning the Lottery on Condominium Conversions

We first document that lottery winners overwhelmingly convert their properties into condominiums. Figure 3.5 plots the $\beta_k^{ITT}$and $\beta_k^{IV}$ coefficients from equations 3.4 and 3.5, respectively, along with their associated 95% confidence intervals. In both specificiations, the outcome is an indicator equal to one if the property is legally registered as a condominium, and zero otherwise.

Figure 3.5: First-Stage: Effect of Winning Lottery on Condominium Conversions




*Notes*: The unit of analysis is a property-year, and the sample includes properties whose owners apply to the lottery. Data are from the City of San Francisco Assessor's Office. The outcome is an indicator for converting to a condominium. The figure reports the $\beta_k^{ITT}$ and $\beta_k^{IV}$ coefficients from equations 3.4 and 3.5, respectively. These specifications compare trends in conversions of lottery winners versus losers. In the IV model, the endogenous variable is a time-invariant indicator for properties that ever convert to a condominium, interacted with years to/from the lottery. Standard errors are clustered by lottery applicant, and error bands show 95% confidence intervals.

The $\beta_k^{ITT}$ coefficients in Figure 3.5 trace the dynamic treatment effects of winning the lottery over time. In years prior to the lottery, winning and losing applicants are equally (un)likely to convert to condominiums. This is unsurprising, since conversions are legally prohibited unless property owners win the lottery. After the lottery, winning property owners are generally unable to immediately convert their properties into condominiums, since the process for preparing and approving applications is costly and takes time. However, over time, the share of conversions steadily increases. Approximately 20% of winners convert their properties within the first full year, and 60% convert within 3 years. At longer time horizons, the conversion rate surpasses 80% within 10 years, and stabilizes at around 83% within 15 years, which is the end of our sample horizon. That the vast majority of lottery winners eventually convert is again unsurprising, since the sole purpose of the lottery is to obtain legal permission to do so.[13] In Appendix Figure C.4, we show that the propensity for lottery winners to convert their properties is statistically similar irrespective of initial neighborhood characteristics.

In Figure 3.5, the endogenous variables in the IV model are time-invariant indicators equal to one if the property ever converts to a condominium in our sample period interacted with indicators for each year since the lottery. In the figure, the $\beta_k^{IV}$ coefficients are informative of the share of lottery-induced compliers who have converted within $k$ years from the lottery. For example, three years after the lottery, the $\beta_{k=3}^{IV}$coefficient of $0.74 = (0.61/0.83)$ implies that 74% of lottery winners that will ever convert have already done so. By construction, this share converges to 1 by the end of our sample period.

Overall, the high condominium conversion rates in Figure 3.5 provide compelling evidence of an economically and statistically strong first-stage in our instrumental variables design.

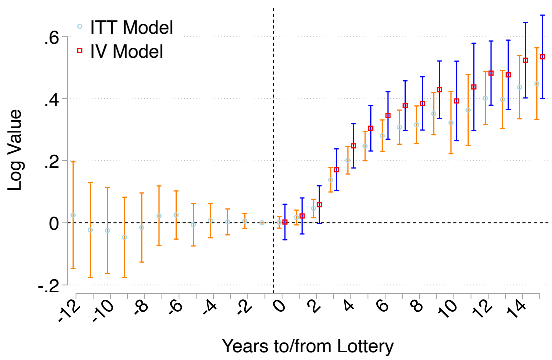
### Effects on Winning Properties

We now study the effects of the lottery on winning properties. The panels in Figure 3.6 plot the $\beta_k^{ITT}$and $\beta_k^{IV}$ coefficients from equations 3.4 and 3.5 for a suite of key outcomes: property values and building and renovation permits. Across all the outcomes, the panels show that winning and losing properties were on common trends prior to the lottery, consistent with effective random assignment from the lottery and with the balance tests presented in Table 3.1.

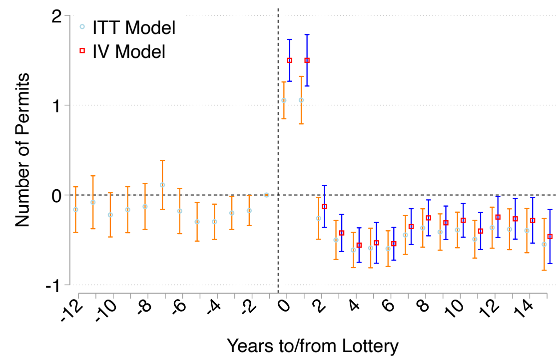
[13]For a neglible share of observations (less than 2%), we observe properties classifed as condominiums prior to implementation of the lottery. One possible explanation for this fact is that, several years before the lottery, property owners may have converted in the opposite direction (that is, they changed from condominiums to another legal form), and then applied to the lottery in order to change back. Another perhaps more likely possibility is measurement error in the administrative data.

Figure 3.6: Effect of Winning the Lottery on Property Values and Permits

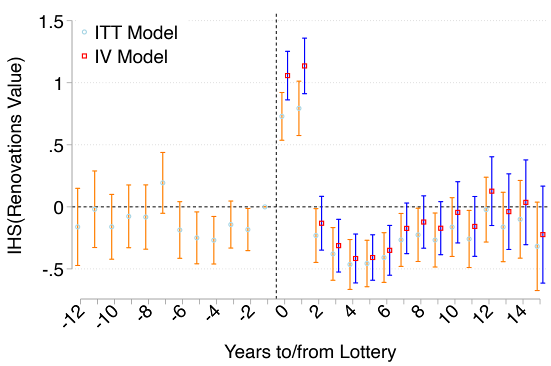
(a) Panel A: Log Property Value



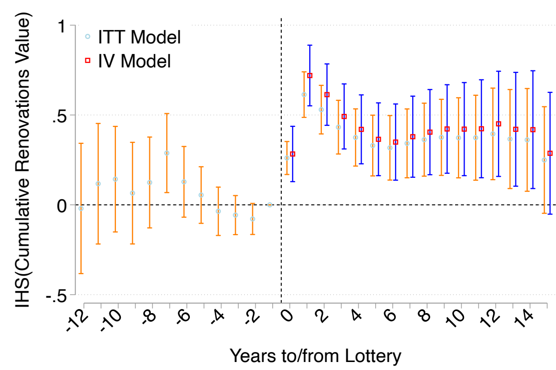

(b) Panel B: Total Building Permits




(c) Panel C: IHS Value of Renovations




(d) Panel D: IHS Cumulative Value of Renovations




*Notes*: The unit of analysis is a property-year, and the sample includes properties whose owners apply to the lottery. Data are from the City of San Francisco Assessor's Office. The figure reports the $\beta_k^{ITT}$ and $\beta_k^{IV}$ coefficients from equations 3.4 and 3.5, respectively. These specifications compare trends in outcomes of lottery winners versus losers. In the IV model, the endogenous variable is a time-invariant indicator for properties that ever convert to a condominium, interacted with years since the lottery. Standard errors are clustered by lottery applicant, and error bands show 95% confidence intervals.

Panel A of Figure 3.6 shows that, over time, winning properties increase dramatically in value relative to losing properties. Within 15 years, the IV estimates indicate that condominium conversions on average cause property values to appreciate by 53%. Since the average property in the sample is worth approximately $1 million, this implies that condominium conversion was on average worth more than $500,000 during our sample period. Table 3.2 shows that this result is robust to alternative specifications that control for local time trends by neighborhood, census tract, or block-group.

Table 3.2: Effect of Winning the Lottery on Property Values

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | $\Delta^{15}_{t=-1}$Log Value | $\Delta^{15}_{t=-1}$Log Value | $\Delta^{15}_{t=-1}$Log Value | $\Delta^{15}_{t=-1}$Log Value |
| **Condo Conversion** | 0.534*** | 0.536*** | 0.503*** | 0.517*** |
| | (0.0684) | (0.0692) | (0.0757) | (0.0813) |
| **Observations** | 116,086 | 116,016 | 115,922 | 115,547 |
| **Model** | IV | IV | IV | IV |
| **Geography x Year FE** | None | Nbhd | Tract | Block Group |

Robust standard errors in parentheses

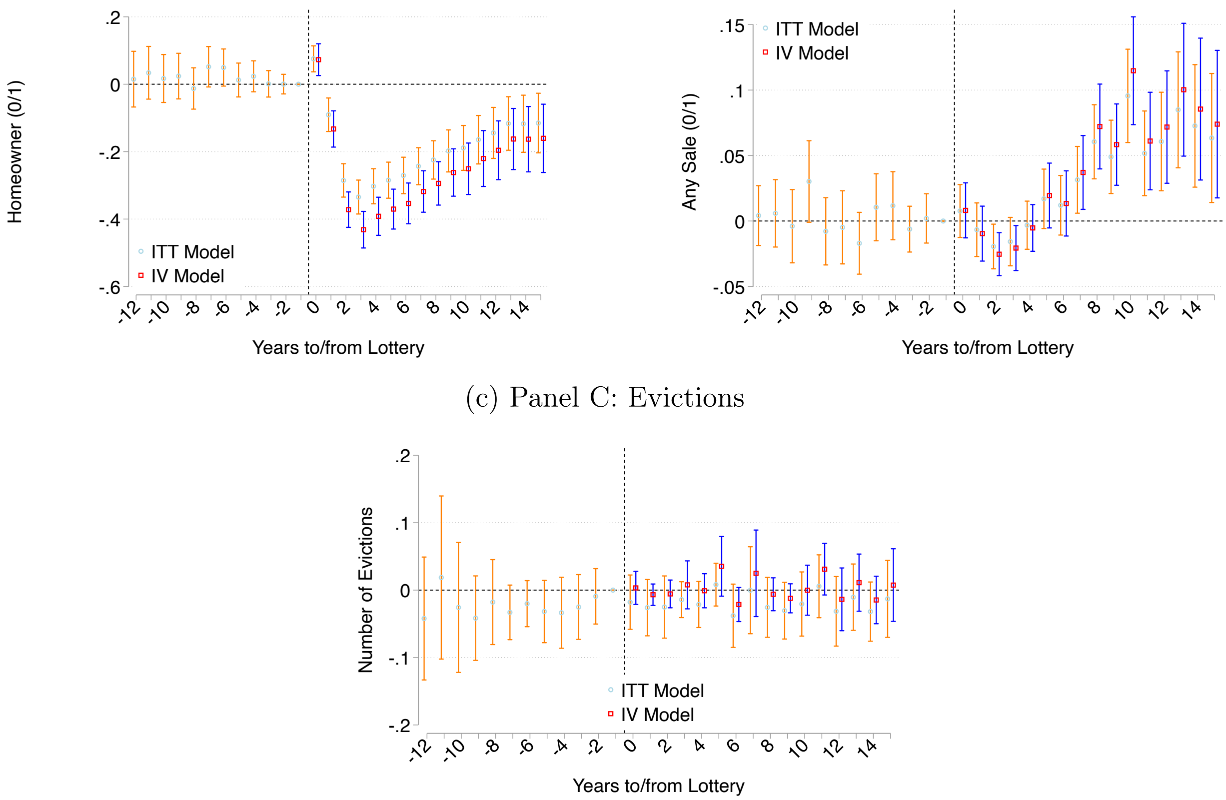
*** p<0.01, ** p<0.05, * p<0.1

*Notes*: The unit of analysis is a property-year, and the sample includes properties whose owners apply to the lottery. Data are from the City of San Francisco Assessor's Office, and the outcome is log property value. The table reports the $\beta_{k=15}$ coefficient from equation 3.5, comparing the values of properties that win the lottery versus those that lose. Column 1 reports the benchmark specification. Columns 2-4 include controls for local trends by neighborhood (Column 2), Census tract (Column 3), and Census block group (Column 4). Standard errors are clustered by lottery applicant.

Panels B, C, and D of Figure 3.6 document the effects of winning the lottery on permits for alterations and renovations (Panel B), the inverse hyperbolic sine transformation of the estimated cost of these renovations (Panel C), and their cumulative value, summed from 1999 forwards (Panel D). The number of permits and their value increase sharply in the two years immediately after the lottery, as winning owners make new investments and improvements in their properties. These improvements likely explain part of the increase in property values documented in Panel A. Permit effects are entirely concentrated within the first two years, after which they decline to levels below the level for lottery losers. Panel D demonstrates that the overall value of alterations and renovations for winning owners remain 29% above losing owners through the study period.

The panels in Figure 3.7 plot the $\beta_k^{ITT}$ and $\beta_k^{IV}$ coefficients from equations 3.4 and 3.5 for a second set of outcomes: tenant evictions, homeownership, and property sales. Panel A shows that winning properties are more likely to be occupied by a homeowner in the year following the lottery. This implies that the winning property owners are more likely to live in their units when they are making renovations and alterations. However, these owners then quickly move out of the units after the first full year, and instead rent the properties to new tenants. The owners are likely to benefit from higher rental prices, both because condominiums are not subject to rent control and because renters are willing to pay more for the recently renovated (and presumably higher quality) units. Three years after the lottery, the IV estimates imply that lottery-induced converted units are 43 percentage points more likely to be occupied by renters compared to losing lottery units. The magnitude of this effect steadily attenuates as units are sold to new homeowners over time, such that winning units are 16 percentage points more likely to be occupied by renters 15 years after the lottery.

Figure 3.7: Effect of Winning the Lottery on Homeownership, Sales, and Evictions




(a) Panel A: Homeowner-Occupied (0/1)

(b) Panel B: Property Sales (0/1)

(c) Panel C: Evictions

*Notes*: The unit of analysis is a property-year, and the sample includes properties whose owners apply to the lottery. Data are from the City of San Francisco Assessor's Office. The figure reports the $\beta_k^{ITT}$ and $\beta_k^{IV}$ coefficients from equations 3.4 and 3.5, respectively. These specifications compare trends in outcomes of lottery winners versus losers. In the IV model, the endogenous variable is a time-invariant indicator for properties that ever convert to a condominium, interacted with years since the lottery. Standard errors are clustered by lottery applicant, and error bands show 95% confidence intervals.

Panel B of Figure 3.7 traces the effects of winning and converting on property sales, defined as an indicator equal to one if any unit in the parcel is sold.[14] Winning property owners are modestly less likely than losing applicants to sell their units in the years immediately

[14]The comparison of lottery winners vs. losers is apples-to-apples because units of condominiums can be sold (winners) as can units of tenancy-in-commons (losers; see section 3.2 for more details).

following the lottery — this result is consistent with the finding that owners are more likely to be renovating and renting their properties during these years. However, 7 years after the lottery, winning applicants that convert are on average 4 percentage points more likely to sell their properties than losing applicants. Within 15 years, this effect increases modestly to 7 percentage points.

Lastly, contrary to the concerns of many policymakers and voters, Panel C reveals that condominium conversions do not cause a statistically discernable change in eviction rates. However, we caution that this result does not necessarily imply there is no turnover or displacement of tenants, since in some cases owners may induce tenants to move out without resorting to the legal eviction process.

In the Appendix, we explore whether the effects of condominium conversions on property values vary with neighborhood or property characteristics. Appendix Figure C.5 shows that average property appreciation of winning properties does not vary systematically with initial neighborhood characteristics. Appendix Figure C.6 shows that appreciation is modestly larger for older buildings with intially lower values, consistent with significant value-added from alterations and renovations.

### Spillover Effects on Nearby Properties

We now use the econometric design described in Section 3.4 to evaluate the spillover effects of condominium conversions on nearby properties. Panel A of Figure 3.8 plots the $\beta_k^{ITT}$ and $\beta_k^{IV}$ coefficients from estimating equation 3.4, where the outcome is the assessed value of homes within a 25-meter radius from the winning property. The 25-meter bandwidth typically includes 3 to 6 properties that are either immediately adjacent to the winning property, across or behind the street, or two to three doors down. Before the lottery, price trends are similar for homes nearby winning and losing applicants, as expected from random treatment assignment. After the lottery, homes located nearby the winners do not immediately increase in value, but do increase beginning approximately 8 years later. This timing is consistent with the treatment dynamics we observed for property sales in Figure 3.6. Within 15 years of the lottery, these nearby home prices appreicate by 11% (s.e.=4.8%) relative to homes nearby losing lottery applicants. Appendix Table C.1 shows that this result is qualitatively similar when including controls for local time trends by neighborhood, census tract, or census block group.

Figure 3.8: Spillovers on Nearby Property Values

(a) Panel A: Log Value of Properties Within 25 Meters


(b) Panel B: Log Value of Properties Within 25-50 Meters


*Notes*: The unit of analysis is a property-year, and the sample includes "nearby" properties within 25 meters (Panel A) or 25-50 meters (Panel B) of a lottery winner. Data are from the City of San Francisco Assessor's Office. The figure reports the $\beta_k^{ITT}$ and $\beta_k^{IV}$ coefficients from equation 3.4. The specifications compare trends in log home prices of properties located near lottery winners versus properties located near lottery losers. The regressions control for predicted probabilities of treatment, as in Borusyak and Hull (2023); see section 3.4 for details. In the IV model, the endogenous variable is a time-invariant indicator for properties that ever convert to a condominium, interacted with years since the lottery. Standard errors are clustered by lottery applicant, and error bands show 95% confidence intervals.

Panel B of Figure 3.8 shows results from estimating the same equation but using a different bandwidth: the outcome in Panel B is the assessed value of homes within a 25- to 50-meter bandwidth from the winning property. These parcels are typically on the same block as the winning property, but further down the street. In this bandwidth, we do not find compelling evidence of spillovers after the lottery: the estimated treatment effects are small and positive but imprecisely estimated ($\beta^{IV}_{k=15}$=5%, s.e. = 5%).

The evidence from Figure 3.8 thus suggests that the spillover effects of condominium conversions are highly localized. Properties within 25 meters of a condominium conversion appreciate in value, while those farther away see weaker or no appreciation. This empirical pattern is informative of the mechanisms that may be driving the spillovers. First, nearby properties might appreciate due to an aesthetic externality from the improved and renovated condominiums, which fades at further distances. Second, nearby properties might appreciate due to a form of behavioral benchmarking, whereby prospective home buyers face imperfect information in the real estate market and so use information about highly local properties as a signal of underlying value. Third, to the extent that local demographic characteristics affect home values, changes in the composition of residents living in the converted condominiums may also play a role. We explore these issues in greater detail in the following section.

## 3.6 Neighborhood Effects

We now turn to addressing whether conversions lead to changes in demographic or economic outcomes in affected neighborhoods. In the previous section we documented that conversions have a large effect on the prices of winning properties, and induce substantial turnover in resident composition as owners move in, move out, rent, and eventually sell their properties. We also documented that conversions increase the prices of adjacent buildings. However, directly affected properties comprise only a small share of the neighborhood housing stock – approximately 2.4% of local residential units, on average. Thus, if conversions have a broad-based effect on neighborhood-wide home prices and demographics, they must have spillover effects on the surrounding area.

Condominium conversions could play a role in broader neighborhood-level gentrification through a series of self-reinforcing mechanisms. First, the new, higher-income condominium residents may increase demand for local goods and services, putting upward pressure on local prices and changing the composition of local businesses. Second, the changing quality and prices of local goods and services -– as well as the changing composition of the residents themselves — may in turn affect which residents find the neighborhood attractive. For example, low-income residents may not value the new neighborhood amenities given the prices, whereas high-income residents may find the neighborhood increasingly desirable. Demographic homophily, racism, and network effects may also play an important role in residential sorting patterns, as has been documented extensively in existing research.[15] As more

[15]For example, immigrants are more likely to move to neighborhoods with other immigrants that share

high-income residents enter the neighborhood, the pattern reinforces itself, fueling broader neighborhood change.

Below we explore to what extent condominium conversions in San Francisco caused neighborhood-wide gentrification, and evaluate whether the evidence is consistent with these mechanisms.

### Neighborhood Design

To study neighborhood outcomes, we first adapt our lottery design to a setting in which a cross-section of census block groups are impacted by the cumulative number of lottery winners from 2001 to 2013. We focus on the long-run, aggregate effect of condominium conversions on census block group outcomes. In addition to being a natural time horizon for studying a slow-moving process such as gentrification, census block group data is largely only available after 2013. Consequently, we focus on neighborhood outcomes from 2013 to 2019 and combine information across all lotteries. Census block groups are the smallest geographic unit for which the Census and the American Community Survey (ACS) release information.[16] Being a small, contiguous set of city blocks, they are an intuitive definition of a neighborhood.

To estimate the effect of a continuous treatment (conversions) on neighborhood outcomes, we rely on generalized propensity score methods (Imbens, 2000; Hirano and Imbens, 2004). In particular, let $y_{gt}$ be an outcome for census block group $g$ in year $t$. The variable $\nu_g$ denotes the number of lottery winners from all of the thirteen lotteries in our sample and $p_g(\nu_g)$ denotes the probability that the census block group had $\nu_g$ winners, given the lottery design.[17] The $p_g(\nu_g)$ captures a neighborhood's demand for the lottery, and by extension, demand for condominium conversions. Once adequately controlled for, we can leverage random variation in condominium conversions through the number of actual winners in the lottery.

We follow the Hirano and Imbens (2004) multi-step procedure in two parts. First, we model neighborhood outcomes as arising from a quadratic in the actual number of conversion lottery winners and the probability of having that number of winners, as follows:

$$y_{gt} = \gamma_0 \nu_g + \gamma_1 \nu_g^2 + \gamma_2 p_g(\nu_g) + \gamma_3 p_g(\nu_g)^2 + \gamma_4 p_g(\nu_g)\nu_g + x_g'\zeta + \varepsilon_{gt} \tag{3.8}$$

their ethnicity (Abramitzky and Boustan, 2017). In general, to the extent that social and information networks are segregated by education, class, and/or race, this is also likely to affect sorting (Jackson, 2021; DiMaggio and Garip, 2012). For a treatment of the history of racism and segregation in housing markets, see Rothstein (2017).

[16] While their size varies, they correspond to a median of nine city blocks (and most between six and ten blocks) in San Francisco. In our sample, census block groups have a median of 1400 individuals and 600 residential units in 2019.

[17] These probabilities are calculated as follows. For each lottery, we permute winners according to the lottery probabilities. We then repeat this process across a large number of simulations. We then calculate $p_g(\nu)$ as the fraction of the simulations in which census block group $g$ recieved $\nu$ winners.

The $x_g$ denote additional controls. In our main specification, we include all relevant outcomes in the year 2000 as controls i.e. $x_g = \mathbf{y}_{g,2000}$. For demographic outcomes available from the ACS, this information is only available at the tract level.[18] For outcomes of establishment counts from business registrations data, this information is available at the census block group level. These controls serve dual purposes. First, our estimate of the generalized propensity score does not fully capture dynamic behavior among lottery applicants. We have argued elsewhere that this is not a substantial concern in our context, and including the baseline controls further mitigates any potential for imbalance. Second, the controls improve the precision of our estimates.

In the second step, we use equation 3.8 to map out the entire dose-response function $\hat{\mathbb{E}}[y_{gt}|\nu, p_g(\nu)]$. The dose-response function can be used to estimate the marginal change in neighborhood outcomes from one additional lottery winner at every level of the treatment and for every block group. These differences can be averaged over all $G$ block groups to get an overall effect as follows:

$$\hat{\beta} = \frac{1}{G} \sum_{g} \sum_{\nu>0} p_g(\nu) \cdot \left( \hat{\mathbb{E}}[y_{gt}|\nu, p_g(\nu)] - \hat{\mathbb{E}}[y_{gt}|\nu - 1, p_g(\nu - 1)] \right) \tag{3.9}$$

Standard errors are calculated by bootstrapping the entire procedure, clustering on neighborhoods. We run this regression only for census block groups that had at least one lottery application over the study period. There are six census block groups (out of 326 that had at least one lottery applicant) with 10 or more lottery winners. The linear specification in lottery winners is sensitive to their inclusion; the quadratic specifcation in the main specification above is far more stable.

### Results

Table 3.3 presents our main results for how the number of lottery winners impacts demographic change in neighborhoods. All outcomes are scaled so that the coefficients can be interpretted as a percentage point (pp) change.

[18] Census tract outcomes in the year 2000 using 2010 boundaries come from the Neighborhood Change Database. We have all outcomes for census tracts in 2000 with the exception of the lower and upper quartiles of rent and home values. We simply control for the median home value and the median rent in 2000. The percentage of the population that is hispanic also does not appear in our census tract data, so we calculate it as 100 minus the percent of the population that is White, Asian, or Black. For consistency, we also use this definition in our census block group data.

Table 3.3: Effects of Lottery Winners on Neighborhood Demographics

| | | | | |
|---|---|---|---|---|
| | *Panel A: Population and Income* | | | |
| | $100 \times \log$ Pop. | Poverty (pp) | College Deg. (pp) | $100 \times \log$ MHI |
| Lottery Winners | 0.28 | −0.30 | 0.87* | 2.47* |
| | (2.68) | (0.31) | (0.49) | (1.46) |
| | *Panel B: Demographics* | | | |
| | White (pp) | Asian (pp) | Black (pp) | Hispanic (pp) |
| Lottery Winners | 1.57*** | −1.46*** | −0.09 | −0.03 |
| | (0.51) | (0.44) | (0.25) | (0.31) |
| | *Panel C: Home Values* | | | |
| | $100 \times \log$(Home Val. Q25) | $100 \times \log$(Home Val. Q50) | $100 \times \log$(Home Val. Q75) | |
| Lottery Winners | 2.34* | 2.22** | 2.19*** | |
| | (1.33) | (1.01) | (0.81) | |
| | *Panel D: Rent* | | | |
| | $100 \times \log$(Rent Q25) | $100 \times \log$(Rent Q50) | $100 \times \log$(Rent Q75) | |
| Lottery Winners | 0.70 | 2.57* | 2.18** | |
| | (1.60) | (1.44) | (0.92) | |
| N | 2200 | 2200 | 2200 | 2200 |
| Year FE | ✓ | ✓ | ✓ | ✓ |
| Block Group Pairs | ✓ | ✓ | ✓ | ✓ |
| Tract Value in 2000 | ✓ | ✓ | ✓ | ✓ |

Notes: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

*Notes*: The unit of analysis is a census block group, and the sample includes all block groups with lottery applicants over the sample period. Data are from the City of San Francisco Assessor's Office and from the 2000 and 2019 American Community Surveys. The table reports the $\beta$ coefficients from equation 3.9. The specifications estimate the effect of an additional lottery winner on the long-run change in neighborhood demographics. See section 3.6 for details.

We find that a conversion lottery winner increases median household income by 2.47 pp and the share of the population with a college degree by 0.87 pp (both significant at the 10% level). Lower quartile rents are unchanged, but median and upper quartile rents increase by 2.57 pp and 2.18 pp respectively. Aligning with our results on assessed property value effects and spillovers, home values increase by more than 2 pp across quartiles. The population undergoes a significant demographic change as well. The White population increases by 1.57 pp, while the Asian population declines by 1.46 pp.

We now consider whether the count and sectoral composition of businesses in these neighborhoods changed as a result of condominium conversions. Our main outcomes will be the total number of new establishments and the total stock of active establishments, broken down by sector. We consider eight large sector groups: Food, Retail, Education, Arts and Entertainment, Professional Services, Manufacturing, Real Estate, and Construction. Many of the neighborhoods in our sample are residential, so our variables contain a large number of zeros. For this reason, we take the inverse hyperbolic sine transformation, and multiply the outcome by 100 so that coefficients can be interpretted as percentage point changes. However, we stress that the extensive margin response is important for interpretting the magnitude of the effects.

These results are contained in Table 3.4. Panel A uses counts of new establishments for the first four sector groups as outcomes. Panel B uses total counts of active establishments for those same sector groups as outcomes. Panel C and Panel D are structured similarly for the other four sector groups. We find that an additional lottery winner induces a 6.41 pp decline in total food establishments (significant at the 10% level) and a 9.93 pp decline in total construction establishments. Education, real estate, and professional services establishments increase by 7.70 pp, 2.20 pp, and 5.10 pp respectively. We find no effect for the retail, arts, and manufacturing sectors.

Table 3.4: Neighborhood Effects (Business Counts)

| | | | | |
|---|---|---|---|---|
| | *Panel A: Local Sectors (New)* | | | |
| | $100 \times IHS$(New Food) | $100 \times IHS$(New Retail) | $100 \times IHS$(New Educ.) | $100 \times IHS$(New Arts & Entertainment) |
| Lottery Winners | −1.75 | −1.31 | 2.96 | −0.82 |
| | (2.65) | (1.84) | (1.94) | (1.76) |
| | *Panel B: Local Sectors (Total)* | | | |
| | $100 \times IHS$(Tot. Food) | $100 \times IHS$(Tot. Retail) | $100 \times IHS$(Tot. Educ.) | $100 \times IHS$(Tot. Arts & Entertainment) |
| Lottery Winners | −6.41* | −3.24 | 7.70*** | 3.40 |
| | (3.59) | (2.74) | (2.98) | (2.98) |
| | *Panel C: Other Sectors (New)* | | | |
| | $100 \times IHS$(New Prof. Services) | $100 \times IHS$(New Manufacturing) | $100 \times IHS$(New Real Estate) | $100 \times IHS$(New Construction) |
| Lottery Winners | 5.07** | −1.13 | 2.34 | −4.10*** |
| | (2.31) | (1.09) | (1.75) | (1.54) |
| | *Panel D: Other Sectors (Total)* | | | |
| | $100 \times IHS$(Tot. Prof. Services) | $100 \times IHS$(Tot. Manufacturing) | $100 \times IHS$(Tot. Real Estate) | $100 \times IHS$(Tot. Construction) |
| Lottery Winners | 5.10* | −1.34 | 2.20* | −9.93*** |
| | (2.65) | (3.02) | (1.25) | (3.30) |
| N | 2200 | 2200 | 2200 | 2200 |
| Year FE | ✓ | ✓ | ✓ | ✓ |
| Gen. P-score | ✓ | ✓ | ✓ | ✓ |
| Block Value in 2000 | ✓ | ✓ | ✓ | ✓ |

Notes: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

*Notes*: The unit of analysis is a census block group, and the sample includes all block groups with lottery applicants over the sample period. Data are from the City of San Francisco Assessor's Office and SF Open Data. The table reports the $\beta$ coefficients from equation 3.9. The specifications estimate the effect of an additional lottery winner on new business entry. See section 3.6 for details.

The story that emerges is consistent with condominium conversions inducing gentrifcation in neighborhoods. White, college-educated, high-income individuals move in. Asian individuals move out. Consistent with our findings in Section 3.5, home values increase. The fact that rents increase is consistent with condominiums being rent de-controlled, and also consistent with pass-through from increased home values to rents. New establishments in the education sector enter to meet increasing local demand. We also find that some professional service and real estate businesses move in as the neighborhood gentrifies. All of these neighborhood changes likely magnify and reinforce one another.

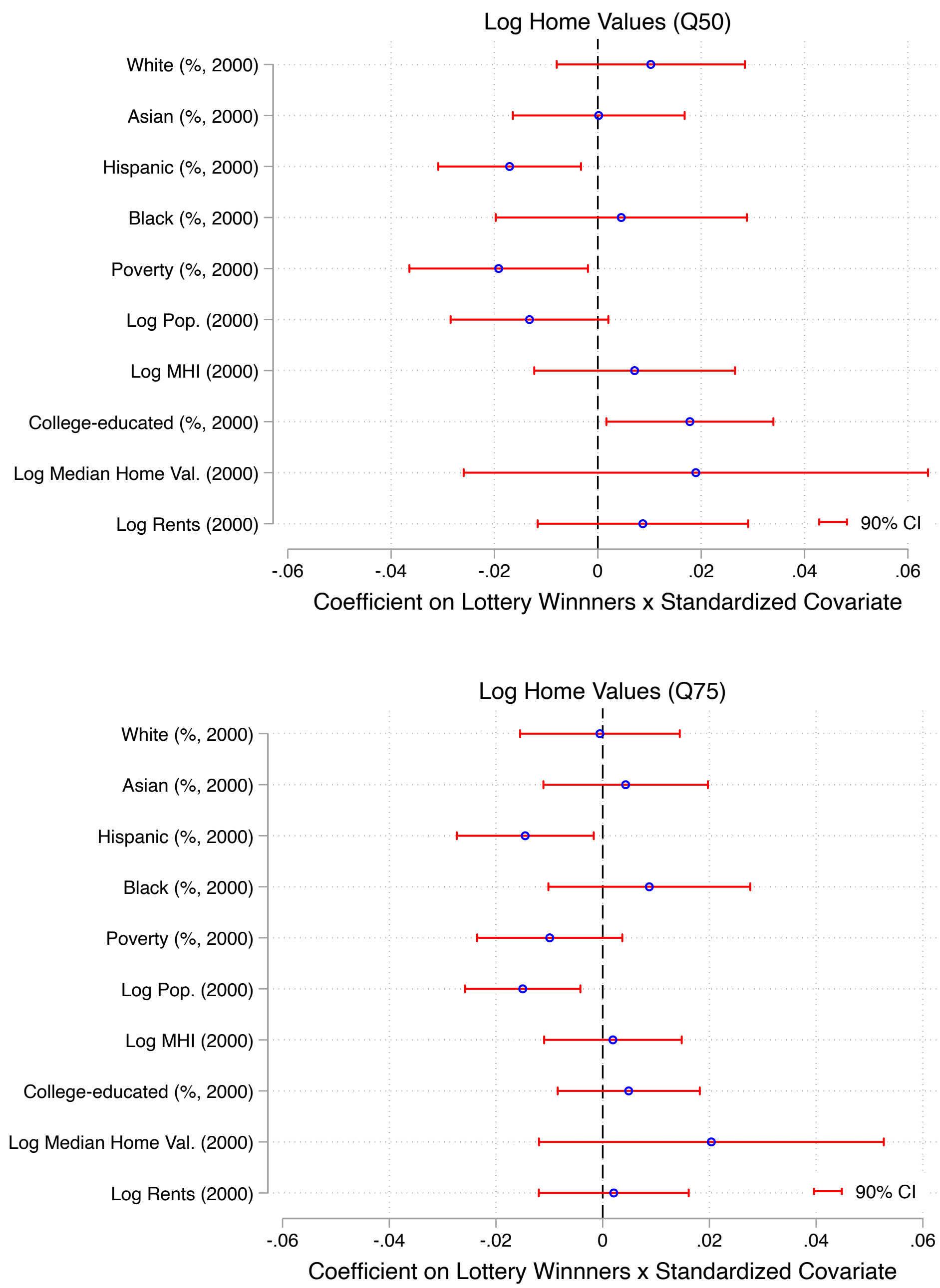
### Heterogeneity

The effects of condominium conversions on demographic outcomes may vary with prelottery neighborhood characteristics. To assess this possibility, we estimate a fully-interacted version of equation 3.8 with census tract covariates in 2000. All covariates are normalized to have mean zero and standard deviation one. We then augment the estimand in equation 3.9 to estimate the additional effect of a lottery winner on a neighborhod one standard deviation from the mean of covariate $x$. The new estimand is given below. As before, standard errors are calculated by bootstrapping the entire procedure.

$$\hat{\beta}_x = \frac{1}{G}\sum_g \sum_{\nu>0} p_g(\nu) \cdot \Bigg( \big(\hat{\mathbb{E}}[y_{gt}|\nu, p_g(\nu), x=1] - \hat{\mathbb{E}}[y_{gt}|\nu-1, p_g(\nu-1), x=0]\big) - \big(\hat{\mathbb{E}}[y_{gt}|\nu, p_g(\nu), x=1] - \hat{\mathbb{E}}[y_{gt}|\nu-1, p_g(\nu-1), x=0]\big) \Bigg) \tag{3.10}$$

These estimates are plotted in Figure 3.9 for each of ten baseline covariates: percentages of the population that are White, Asian, Hispanic, Black, college-educted, living in poverty, and the logs of the population, median household income, median home values, and rents. We focus on the critical outcomes of home value appreciation, and perform the same analysis separately for home values at the 50th and 75th percentile of the neighborhood distribution. Given the modest number of census tracts in our sample (n=123), confidence intervals are plotted at the 90% level.

Figure 3.9: Neighborhood Heterogeneity




*Notes*: The unit of analysis is a block-group-year, and the sample includes block groups with lottery applicants over the sample period. Data are from the City of San Francisco Assessor's Office and the 2000 and the American Community Survey, and the outcome is log property value. The figure reports the $\beta_x$ coefficients from equation 3.10. These specifications estimate how the effects of lottery winners on property appreciation vary with initial neighborhood characteristics. Standard errors are clustered by census block group, and error bands show 90% confidence intervals.

Panels A and B of Figure 3.9 provide suggestive evidence that the neighborhood-level effects of condominum conversions on home price appreciation are smaller in areas with higher poverty rates and Hispanic population shares, and larger in areas with a higher share of college graduates. The results are consistent with the hypothesis that demographic characteristics such as income, race, and education are key mediators of gentrification in US cities.

## 3.7 Conclusion

Condominium conversions, at the core, entail a change in the legal form of how building units are owned. But conversions bundle several other changes that are important for understanding their effects on neighborhoods: the units are attractive to homeowners in part because they are no longer subject to rent control, face less stringent evictions protections, and are more liquid on the real estate market. We find that conversions also induce property owners to renovate and upgrade units, further causing home values to appreciate. Consequently, nearby parcels become more attractive. In the long-run, the neighborhood gentrifies: rents and home values increase; more educated, higher-income, and white individuals move in; the sectoral composition of local businesses changes; and demographic minorities move out.

Housing supply in many major U.S. cities, and particularly so for San Francisco, has failed to keep pace with housing demand (Figure 3.1). In housing markets without new construction, how the existing housing stock is renovated, used, sold, and rented is especially important. In this paper, we study the effect of one such supply behavior: condominium conversions. By analyzing the direct and indirect effects of conversions, our findings contribute to the debate across the U.S. on how best to regulate them. While our results are difficult to extrapolate to housing markets where supply is more elastic, even here, condominiums are a legal form commonly chosen by developers. Our paper sheds light on the role that condominiums play in cities and neighborhoods more generally.

# Chapter 4

# Uber versus Trains? Worldwide Evidence from Transit Expansions

## 4.1 Introduction

The explosive growth of ride-hailing around the world has sparked an important debate about the repercussions this new mobility option is having on cities. Urban planners and policymakers around the world have grappled with how to regulate ride-hailing companies in their jurisdictions. Indeed, several countries have banned ride-hailing while others have heavily regulated it.[1] Of particular interest to urban planners and policy makers is whether ride-hailing technologies increase or decrease public transit ridership. Understanding the degree of complementarity or substitutability between ride-hailing and public transit is important for at least four reasons. First, reductions in transit ridership can potentially generate major budgetary shortfalls for transit authorities. Second, reductions in transit ridership likely have social welfare costs because transit ridership is inefficiently too low.[2] Third, reductions in transit ridership likely increase congestion and pollution.[3] Fourth, changes in transportation technologies, such as steam railways, the automobile, and limited-access highways, have repeatedly reshaped urban spatial structure.[4]

Because of its importance, determining the impact of ride-hailing on public transportation has attracted significant attention from researchers. In spite of this, there remains great

[1] Countries banning ride-hailing include Denmark, Hungary, and Bulgaria.

[2] Transit fares are typically above social marginal cost (though below average cost) due to economies of scale and density. Existing research shows that, given the existing set of transportation policies, increasing transit subsidies, and so increasing transit ridership, increases social welfare (e.g., Parry and Small, 2009; Basso and Silva, 2014).

[3] See, for example Anderson (2014) and Gendron-Carrier et al. (forthcoming).

[4] Heblich et al. (2020) shows how steam railways allowed London to double in population and Baum-Snow (2007) estimates that limited-access highways reduced central city population by 8%. Gorback (2019) finds ride-hailing is already affecting urban spatial structure, with UberX doubling restaurant net creation in previously inaccessible locations.

uncertainty as existing estimates vary in sign and magnitude. Estimates range from as high as +5% after two years (Hall et al., 2018) to as low as -16% after four years (Diao et al., 2021).

Our contribution is to address the question of whether ride-hailing and public transportation are complements or substitutes using novel data and an innovative identification strategy. Using proprietary trip data from Uber, we use a dynamic difference-in-differences strategy to estimate how rail transit expansions from around the world affect local Uber ridership. We focus on rail transit due to the difficulty in documenting bus expansions. Our approach has several advantages. First, the timing of rail expansions are plausibly exogenous to underlying trends in Uber ridership. Rail expansions are planned years in advance, and indeed, many of those in our sample were initially planned before the existence of Uber. Second, our detailed and geographically precise data allows us to provide tests for the mechanisms by which transit and ride-hailing impact each other. Third, our research design allows us to flexibly control for hyper-local and highly variable time trends in Uber ridership. Fourth, we are able to use data for 35 countries.

We use a dynamic difference-in-differences strategy to control for hyper-local trends in Uber ridership. Our novel approach exploits the high frequency and extremely granular Uber trip data as well as the sharp opening date for new transit stations. We compare the number of Uber trips in two adjacent distance bands around a new train station (for example 0–100 m and 100–200 m from a station) before and after a train station opens for service. While the further distance band plays the role of a local "control group," we expect that it is also affected by the new transit station opening. Thus our estimates at, say, 100—200 m are the effect of a new transit opening on Uber ridership at 100-–200 m relative to the effect of a new transit station opening at 200—300 m. We repeat this estimation strategy for adjacent distance bands up until 1200 m from transit stations. We find that relative treatment effects are indistinguishable from zero beyond 300 m. We obtain the total effect of a new train station on Uber ridership by summing up the relative effects at all distance bands.

Our test shows clear evidence that Uber and train service are highly complementary, as we observe large increases in ride hailing trips after a train station opens. Effects are concentrated within 300 meters of a station, and show no signs of decay after 6 months of train service. We also find that the average length of an Uber trip decreases after train service begins, consistent with the idea that ride-hailing in the presence of a rail station starts being used for last mile trips where both modes of transport are used.

This paper builds on a quickly growing literature seeking to determine whether ride-hailing and public transportation are complements or substitutes. Most papers in this literature use variation across US metropolitan areas in the timing of Uber entry to estimate the effect of ride-hailing on public transit. Hall et al. (2018) finds that ride-hailing complements the average transit agency, while Graehler et al. (2019), Erhardt et al. (2021), and Diao et al. (2021) find ride-hailing is a substitute. Nelson and Sadowsky (2018), Babar and Burtch (2020), and Cairncross et al. (2021) find mixed or statistically insignificant results. This paper takes a completely different approach by exploiting exogenous variation from the timing of train service starts to assess what happens to Uber trips in the face of a new train transit option. The paper also adds to the literature by including data from other countries, allowing for a rich heterogeneity analysis.

There is also a broader literature working to understand the effect of ride-hailing on cities. This includes understanding the impact of ride-hail on traffic safety (Greenwood et al., 2017; Burgdorf et al., 2019; Barrios et al., 2020; Barreto et al., 2020; Anderson and Davis, 2021), and the impacts of surge pricing (Castillo, 2020; Castillo et al., 2021).

## 4.2 Data

To investigate the effect of new transit stations on Uber ridership we require data describing new transit station locations and dates of opening as well as panel data on Uber ridership near these stations. We use data on Uber ridership constructed from Uber's trip database. Our data on new transit stations are the result of primary data collection. We describe these datasets and their construction below.

#### Uber ridership

We use Uber's trip database to calculate the number of trips starting or stopping within a given distance band from the transit station in a month, for example, all trips within 100–200 meters (m). We do this for 12 different 100 m bands from 0–100 m up to 1,100–1,200 m. To avoid double-counting, each Uber pickup or drop-off is allocated to the station to which it is closest. This means the distance bands are not always perfect circles. Our Uber trip data spans January 2012 to December 2018, but note that Uber was expanding in this time period so that not every city has trip data from 2012. This provides us with between 15 and 79 months of Uber trip data for each transit expansion, with a mean and median of 61 months. We exclude cities where Uber availability began, ended, or paused within six months of the rail expansion. We also excluded New York City and China.

#### Transit stations

To collect data on new transit stations, we start with a list of cities Uber operates in worldwide, and for each city, find all rail transit stations that opened after Uber entered the city. We obtain data on subway openings through 2017 from Gendron-Carrier et al. (forthcoming), and extend this dataset to include light rail and commuter rail, and update it through 2018 using online sources such as `www.urbanrail.net`, `www.wikipedia.org`, and news sites. We limit attention to rail stations as these are better documented than bus stops. For each station, we record opening date, latitude and longitude, station name, whether it is the terminal station, city, and country. We define the exact latitude and longitude of each station using Google Maps. We also find the locations of the pre-existing stations that had been the terminal stations before the transit expansion. Table 4.1 reports the 78 cities that expanded their rail transit between the date Uber entered and 2018. The table lists the date Uber entered, the number of expansions, number of new stations, type of service (subway,

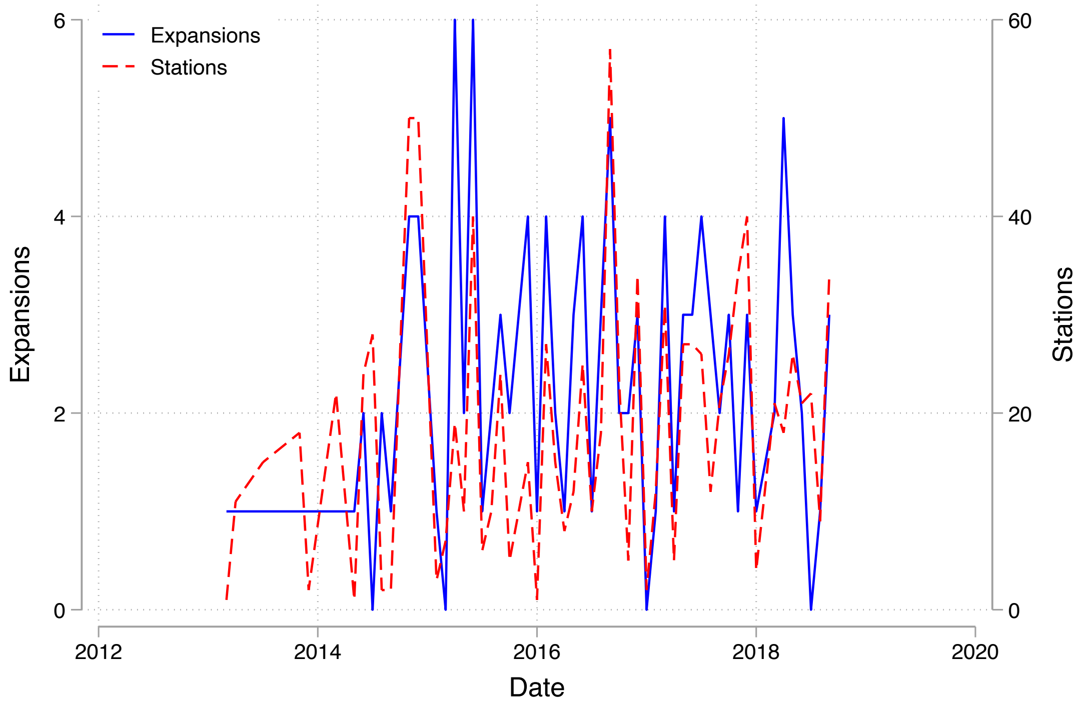



Figure 4.1: Transit expansions in our data over time

light rail, etc.), and the first and last date of the expansions. Figure 4.1 shows the time series of rail expansion events and number of stations opened, showing there is a notable drop in openings in January and February, but they are otherwise fairly uniform over time of year. Figure 4.2 plots the geographic distribution of cities used in our analysis, showing they are heavily concentrated in India and Southeast Asia, Europe, and North America.

Our analysis requires us to observe Uber ridership at a transit station for sometime before and after the transit station opens. Thus, we face a trade-off between sample size and the length of time we observe each transit station. We focus on a 13-month window, six months before and after the station opening; excluding any transit stations for which we do not have this data.

## 4.3 Methodology

Our main specification estimates the impact of a new transit station opening on Uber ridership using a dynamic difference-in-differences design that exploits our high-frequency data on Uber trips as well as the sharp opening date of new transit stations. The first difference compares Uber ridership in a given distance band before and after the transit station opens. The second difference adjusts for time trends in Uber ridership. Adjusting for

Table 4.1: City-level descriptive statistics

| City | # Expansions | # Stations | # Monorail | # Light Rail | # Commuter Rail | # Subway | # Interacts w/ Traffic | Uber Entry | First Exp. Year | Last Exp. Yr |
|---|---|---|---|---|---|---|---|---|---|---|
| Amsterdam | 0 | 6 | 0 | 0 | 0 | 6 | 0 | 2012 | 2018 | 2018 |
| Atlanta | 0 | 12 | 0 | 12 | 0 | 0 | 12 | 2012 | 2014 | 2014 |
| Baku | 0 | 1 | 0 | 0 | 0 | 1 | 0 | 2015 | 2016 | 2016 |
| Bangalore | 4 | 34 | 0 | 0 | 0 | 34 | 0 | 2013 | 2014 | 2017 |
| Bangkok | 3 | 18 | 0 | 0 | 0 | 18 | 0 | 2014 | 2016 | 2017 |
| Birmingham, UK | 1 | 4 | 0 | 4 | 0 | 0 | 4 | 2015 | 2015 | 2016 |
| Bordeaux | 1 | 9 | 0 | 9 | 0 | 0 | 9 | 2014 | 2015 | 2016 |
| Boston | 0 | 1 | 0 | 0 | 0 | 1 | 0 | 2012 | 2014 | 2014 |
| Bratislava | 0 | 3 | 0 | 3 | 0 | 0 | 2 | 2015 | 2016 | 2016 |
| Brussels | 1 | 11 | 0 | 11 | 0 | 0 | 11 | 2014 | 2018 | 2018 |
| Bucharest | 0 | 2 | 0 | 0 | 0 | 2 | 0 | 2015 | 2017 | 2017 |
| Buenos Aires | 0 | 1 | 0 | 0 | 0 | 1 | 0 | 2016 | 2018 | 2018 |
| Charlotte | 1 | 16 | 0 | 16 | 0 | 0 | 5 | 2013 | 2015 | 2018 |
| Chennai | 4 | 29 | 0 | 0 | 0 | 29 | 0 | 2014 | 2015 | 2018 |
| Chicago | 1 | 2 | 0 | 0 | 0 | 2 | 0 | 2012 | 2015 | 2017 |
| Cincinnati | 0 | 18 | 0 | 18 | 0 | 0 | 18 | 2014 | 2016 | 2016 |
| Cleveland | 0 | 1 | 0 | 0 | 0 | 1 | 0 | 2014 | 2015 | 2015 |
| Dallas | 4 | 15 | 0 | 15 | 0 | 0 | 9 | 2012 | 2014 | 2016 |
| Delhi NCR | 12 | 65 | 0 | 0 | 0 | 65 | 0 | 2013 | 2014 | 2018 |
| Denver | 1 | 19 | 0 | 19 | 0 | 0 | 4 | 2012 | 2013 | 2017 |
| Detroit | 0 | 12 | 0 | 12 | 0 | 0 | 12 | 2013 | 2017 | 2017 |
| Dubai | 4 | 18 | 0 | 10 | 0 | 8 | 0 | 2013 | 2014 | 2017 |
| Dublin | 0 | 13 | 0 | 13 | 0 | 0 | 9 | 2014 | 2017 | 2017 |
| Dusseldorf | 1 | 9 | 0 | 9 | 0 | 0 | 3 | 2014 | 2016 | 2018 |
| Edinburgh | 0 | 1 | 0 | 1 | 0 | 0 | 1 | 2015 | 2016 | 2016 |
| Edmonton | 0 | 3 | 0 | 3 | 0 | 0 | 0 | 2014 | 2015 | 2015 |
| Florence | 0 | 12 | 0 | 12 | 0 | 0 | 12 | 2015 | 2018 | 2018 |
| Fortaleza | 2 | 8 | 0 | 7 | 0 | 1 | 0 | 2016 | 2017 | 2018 |
| Frankfurt | 0 | 9 | 0 | 0 | 0 | 9 | 0 | 2014 | 2016 | 2016 |
| Gold Coast | 0 | 3 | 0 | 3 | 0 | 0 | 3 | 2014 | 2017 | 2017 |
| Gothenburg | 0 | 1 | 0 | 1 | 0 | 0 | 1 | 2014 | 2015 | 2015 |
| Hong Kong | 3 | 10 | 0 | 0 | 0 | 10 | 0 | 2014 | 2014 | 2016 |
| Houston | 2 | 9 | 0 | 9 | 0 | 0 | 9 | 2014 | 2015 | 2017 |
| Hyderabad | 1 | 39 | 0 | 0 | 0 | 39 | 0 | 2014 | 2017 | 2018 |
| Istanbul | 4 | 17 | 0 | 0 | 0 | 17 | 0 | 2014 | 2015 | 2017 |
| Jaipur | 0 | 9 | 0 | 0 | 0 | 9 | 0 | 2014 | 2015 | 2015 |
| Kochi | 1 | 16 | 0 | 0 | 0 | 16 | 0 | 2014 | 2017 | 2017 |
| Kuala Lumpur | 4 | 50 | 0 | 22 | 0 | 28 | 0 | 2013 | 2015 | 2017 |
| Lisbon | 1 | 11 | 0 | 10 | 0 | 1 | 10 | 2014 | 2016 | 2018 |
| Los Angeles | 1 | 13 | 0 | 13 | 0 | 0 | 3 | 2012 | 2016 | 2016 |
| Lucknow | 0 | 7 | 0 | 0 | 0 | 7 | 0 | 2016 | 2017 | 2017 |
| Lyon | 0 | 2 | 0 | 2 | 0 | 0 | 2 | 2013 | 2014 | 2014 |
| Manchester | 1 | 16 | 0 | 16 | 0 | 0 | 16 | 2014 | 2014 | 2015 |
| Milan | 7 | 16 | 0 | 6 | 0 | 10 | 6 | 2013 | 2014 | 2018 |
| Minneapolis - St. Paul | 0 | 18 | 0 | 18 | 0 | 0 | 18 | 2012 | 2014 | 2014 |
| Moscow | 12 | 42 | 0 | 0 | 0 | 42 | 0 | 2013 | 2014 | 2017 |
| Munich | 0 | 6 | 0 | 6 | 0 | 0 | 6 | 2013 | 2016 | 2016 |
| New Jersey | 0 | 1 | 0 | 0 | 1 | 0 | 0 | 2013 | 2016 | 2016 |
| New Orleans | 0 | 6 | 0 | 6 | 0 | 0 | 6 | 2014 | 2016 | 2016 |
| Nice | 0 | 12 | 0 | 12 | 0 | 0 | 12 | 2014 | 2018 | 2018 |
| Novosibirsk | 0 | 2 | 0 | 2 | 0 | 0 | 0 | 2015 | 2016 | 2016 |
| Oslo | 0 | 1 | 0 | 0 | 0 | 1 | 0 | 2014 | 2016 | 2016 |
| Panama City, PA | 1 | 2 | 0 | 0 | 0 | 2 | 0 | 2014 | 2015 | 2015 |
| Paris | 6 | 76 | 0 | 75 | 0 | 1 | 75 | 2012 | 2013 | 2017 |
| Phoenix | 1 | 7 | 0 | 7 | 0 | 0 | 7 | 2012 | 2015 | 2016 |
| Portland | 0 | 10 | 0 | 10 | 0 | 0 | 7 | 2014 | 2015 | 2015 |
| Prague | 0 | 4 | 0 | 0 | 0 | 4 | 0 | 2014 | 2015 | 2015 |
| Rio de Janeiro | 7 | 31 | 0 | 26 | 0 | 5 | 26 | 2014 | 2016 | 2017 |
| Rome | 2 | 22 | 0 | 0 | 0 | 22 | 0 | 2013 | 2014 | 2015 |
| Sacramento | 0 | 3 | 0 | 3 | 0 | 0 | 0 | 2013 | 2015 | 2015 |
| Salvador | 3 | 11 | 0 | 0 | 0 | 11 | 0 | 2016 | 2016 | 2018 |
| San Diego | 0 | 1 | 0 | 1 | 0 | 0 | 1 | 2012 | 2018 | 2018 |
| San Francisco | 4 | 15 | 0 | 0 | 11 | 4 | 0 | 2012 | 2014 | 2018 |
| Santiago | 0 | 10 | 0 | 0 | 0 | 10 | 0 | 2013 | 2017 | 2017 |
| Santo Domingo | 0 | 4 | 0 | 0 | 0 | 4 | 0 | 2015 | 2018 | 2018 |
| Sao Paulo | 8 | 15 | 4 | 0 | 0 | 11 | 0 | 2014 | 2017 | 2018 |
| Seattle | 2 | 14 | 0 | 14 | 0 | 0 | 11 | 2012 | 2016 | 2016 |
| Singapore | 8 | 39 | 0 | 6 | 0 | 33 | 0 | 2013 | 2013 | 2017 |
| Stockholm | 1 | 2 | 0 | 1 | 1 | 0 | 1 | 2013 | 2017 | 2017 |
| Strasbourg | 0 | 2 | 0 | 2 | 0 | 0 | 2 | 2015 | 2017 | 2017 |
| Sydney | 1 | 10 | 0 | 10 | 0 | 0 | 10 | 2012 | 2014 | 2015 |
| Taipei | 2 | 29 | 0 | 0 | 0 | 29 | 0 | 2013 | 2014 | 2017 |
| Tallinn | 0 | 2 | 0 | 2 | 0 | 0 | 2 | 2015 | 2017 | 2017 |
| Toronto | 2 | 12 | 0 | 2 | 4 | 6 | 2 | 2012 | 2015 | 2017 |
| Tucson | 0 | 23 | 0 | 23 | 0 | 0 | 23 | 2013 | 2014 | 2014 |
| Vienna | 0 | 5 | 0 | 0 | 0 | 5 | 0 | 2014 | 2017 | 2017 |
| Warsaw | 0 | 7 | 0 | 0 | 0 | 7 | 0 | 2014 | 2015 | 2015 |
| Washington D.C. | 1 | 13 | 0 | 8 | 0 | 5 | 8 | 2012 | 2014 | 2016 |

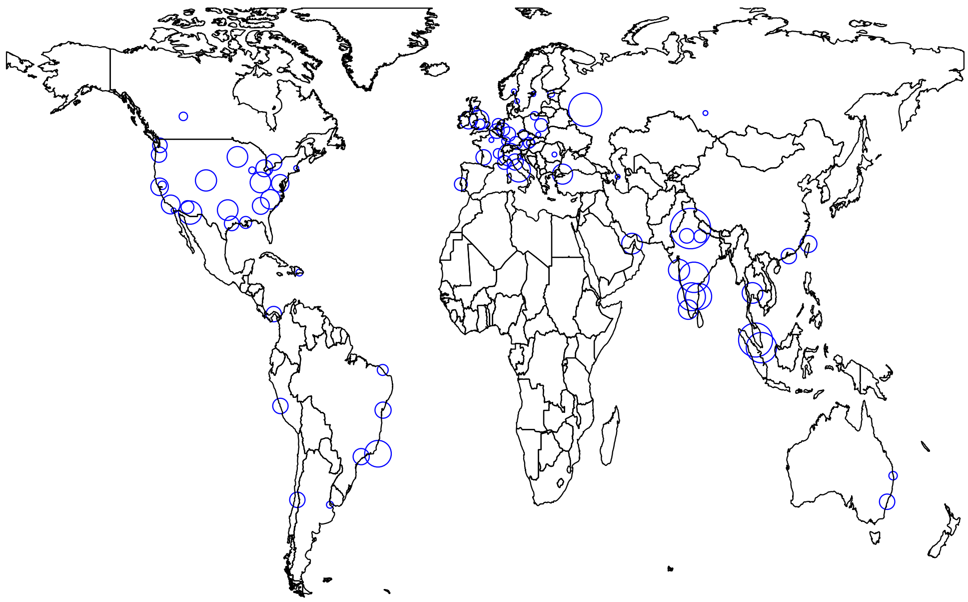


Figure 4.2: Locations of transit expansions in our data

*Notes:* This map plots the locations of new transit openings in our data. The size of each circle is proportional to the number of new stations that opened between Uber's entry date and 2018.

time trends is vital because, over our study period, Uber is growing quickly and is not in a steady state. To address this challenge, we use Uber ridership in the next furthest distance band to adjust for hyper-local time trends in Uber ridership. We expect that Uber ridership in the next furthest distance band is also affected by the new transit station opening, and thus our estimates at, say, 100–200 m are of the effect of a new transit opening on Uber ridership at 100–200 m *relative* to the effect of a new transit station opening at 200–300 m. Should we find a distance where the treatment effect is zero, we can then find the total effect at 100–200 m by summing the relative effects at all further out distances.[5]

Let $y_{dit}$ denote the inverse hyperbolic sine number of Uber trips starting or ending in distance band $d$ around transit station $i$ during month $t$.[6] Denote the month that station

[5]This procedure can be viewed as a discrete approximation to the difference-in-differences style estimator of Diamond and McQuade (2019a). Our regression approach estimates the gradient of the treatment effect with respect to distance from the train expansion using finite differences across 100 m bands. Difference-in-differences estimates at farther distances provide tests of whether the treatment effect eventually converges to zero with distance.

[6]We focus on this outcome for two reasons. First, the inverse hyperbolic sine transformation allows us to

$i$ opens as $t'_i$ and define the time since the station opened ("relative time") as $\tau_{it} = t - t'_i$, noting that $\tau_{it}$ is negative before the station opens. In the month a station opens, $\tau_{it} = 0$; for stations that open at the start of the month, nearly the entire month is treated, while for stations that open at the end of the month, nearly the entire month is untreated.

To estimate the relative effect of a new transit station opening on Uber ridership in distance band $\tilde{d}$, we use data on distance band $\tilde{d}$ and the next distance band further out, $\tilde{d}+1$, and estimate the following dynamic difference-in-differences specification:

$$\begin{aligned} y_{dit} = \gamma_{it} + \delta_{di} + &\sum_{j \in \{-6,-5,\ldots,6\} \setminus \{-2\}} \alpha_{\tilde{d}j} \times \mathbb{1}_{\tau_{it}=j} \times \mathbb{1}_{d=\tilde{d}} \\ &+ \left(\beta_{1,\tilde{d}} \times \mathbb{1}_{\tau_{it}<-6} + \beta_{2,\tilde{d}} \times \mathbb{1}_{\tau_{it}>6}\right) \mathbb{1}_{d=\tilde{d}} + \epsilon_{dit}, \\ &\forall\ d \in \{\tilde{d}, \tilde{d}+1\}, \end{aligned} \tag{4.1}$$

where $\mathbb{1}_{\tau_{it}=j}$ is an indicator function for whether the given observation occurs $j$ months after the station opens, $\mathbb{1}_{d=\tilde{d}}$ is an indicator function for whether the given observation is at distance $\tilde{d}$, $\gamma_{it}$ is a station-time fixed effect, and $\delta_{di}$ is a station-distance fixed effect. The coefficients of interest are $\alpha_{\tilde{d}j}$, which are the percentage changes in Uber trips $j$ months after a new transit station opens, relative to distance band $\tilde{d}+1$. The index $j$ runs from -6 to 6, excluding -2, so the second month prior to the transit station openings is the reference category.[7] For $j \geq 0$, $\alpha_j$ is the treatment effect we seek to measure. For $j < 0$, $\alpha_j$ allows us to test whether there are pre-trends in Uber trips before stations open. The coefficients $\beta_{1,d}$ and $\beta_{2,d}$ are event study coefficients for before and after our treatment window. This specification allows us to use all the data for each station (anywhere between 15–79 months) to precisely estimate station-by-distance fixed effects, while only using variation in Uber trips around the station opening date to estimate the effect of a station opening on Uber trips. We cluster our standard errors at the station level.

We often wish to aggregate the treatment effect to a single coefficient, which is helpful when reporting regression results across multiple specifications or distances in a single table or figure. We do so using the following difference-in-difference specification:

$$\begin{aligned} y_{dit} = \gamma_{it} + \delta_{di} + &\alpha_{\tilde{d}} \times \mathbb{1}_{\tau_{it} \in \{1,\ldots,6\}} \times \mathbb{1}_{d=\tilde{d}} \\ &+ \left(\beta_{1,\tilde{d}} \times \mathbb{1}_{\tau_{it}<-6} + \beta_{2,\tilde{d}} \times \mathbb{1}_{\tau_{it}>6} + \beta_{3,\tilde{d}} \times \mathbb{1}_{\tau_{it} \in \{-2,-1,0\}}\right) \mathbb{1}_{d=\tilde{d}} + \epsilon_{dit}, \\ &\forall\ d \in \{\tilde{d}, \tilde{d}+1\}, \end{aligned} \tag{4.2}$$

In this regression, the omitted category is the fourth through sixth months before the station opened and the coefficient of interest is $\alpha_{\tilde{d}}$. $\alpha_{\tilde{d}}$ captures the average percentage effect of a station opening on Uber trips at distance $d$ relative to $d+1$, for months $\tau_{it} \in \{1, ..., 5\}$

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interpret the regression coefficients as approximately the percentage change in Uber trips. Second, farther rings mechanically have more Uber usage given their larger area. Differences in percents will thus be more relevant than level differences.

[7]We use the second month prior to the opening as the reference category to test for anticipation effects.

relative to months $\tau_{it} \in \{-6, -5, -4\}$. We separately control for the effect two months prior and the month of the station opening. This specification mitigates concerns over the partial treatment of month $\tau_{it} = 0$ and soft openings which may have had an impact on Uber usage prior to the station's official open date.

We have two core identifying assumptions. First, we assume that local governments do not time the opening of new transit stations to coincide with a sharp break in Uber ridership. Indeed, Gendron-Carrier et al. (forthcoming) find that it typically takes 11 years between when the plan is approved for a new subway and the opening date. This means that the vast majority, and maybe even all, of the transit openings in our data were approved before Uber existed. Second, we make the standard difference-in-differences assumptions that Uber ridership in adjacent distance bands would have moved in parallel in the absence of a new transit station opening. A strength of our approach is the granularity of our data, which uses adjacent distances to provide a more plausible counterfactual. We will present evidence that Uber ridership in adjacent bands moved in parallel prior to the station opening.

## 4.4 Results

We start by presenting our estimates of the marginal effect of transit on Uber ridership by distance. We then estimate the total effect, which city or expansion characteristics predict the magnitude of the effect of transit on Uber, and finally, present evidence on the mechanisms by which transit affects Uber.

### Average relative effect

We start with estimating the average effect of a new transit station opening on Uber trips by the distance from a transit station. Figure 4.3 plots our estimates of the effect on trips within 0–100 m of a station, relative to the effect on trips within 100–200 m. It shows there is a large and statistically significant increase in ridership of 53% within a month of opening (0.43 log points). This effect is persistent for at least six months and does not show signs of decay. There is a smaller increase in the month the station opens (month 0) which could be occurring for two reasons: first, part of this is mechanical, as transit stations do not always open on the first day of the month, and so the month of opening is only partially treated, and second, it likely takes time for travelers to re-optimize their decisions of how to travel.

The figure also shows evidence in favor of our identification assumption. There are no pre-trends in Uber trips before the train station opens, and the increase in Uber ridership we observe only occurs after the train opens and not before.

The sharp increase in Uber ridership when the transit station opens is strong evidence in favor of the transit opening itself increasing Uber ridership, rather than an indirect effect mediated through restaurant openings and other commercial activities. The high frequency of our data allow us to rule out this alternative explanation given that neighborhood or commercial environment changes take years or even decades to occur. While a new restaurant or store may want to time their opening with the transit station opening, this is difficult to achieve.

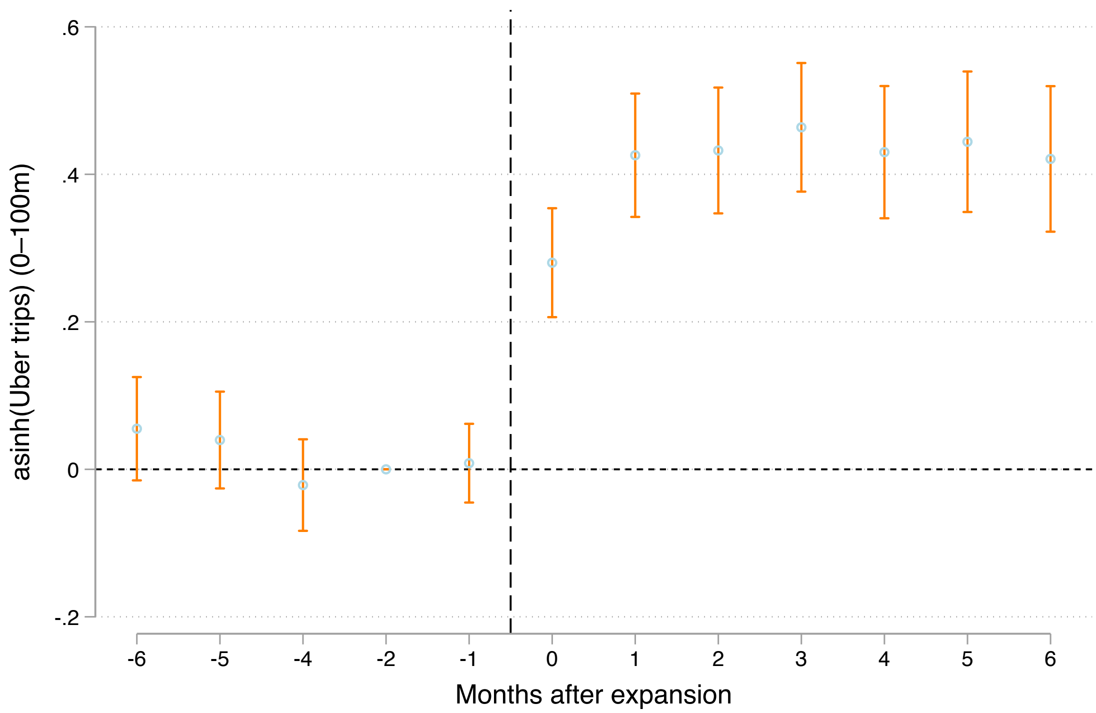


Figure 4.3: Dynamic difference-in-difference estimates, 0–100 m relative to 100–200 m

*Notes:* This figure plots the coefficients (circles) and 95% confidence intervals (bars) for the percentage effect of a new train station opening on Uber trips 0–100 m away, relative to 100–200 m away. The coefficients are plotted for each month between six months prior and six months after an expansion. The regression is a dynamic difference-in-differences model of station openings on the inverse hyperbolic sine of Uber trips that either originated or terminated at a given distance band from the opening. The model contains station by distance band and station by time fixed effects; additionally, fixed effects for distance band by more than 6 months before or after an expansion are included. All errors are clustered at the station-level.

Figure 4.4 plots how the average effect during months 1–6 of a new transit station on Uber ridership changes with distance. It shows that the a new transit station opening increases Uber ridership at 100–200 m and 200–300 m (relative to the next further distance band). However, beyond these distances there is no detectable effect. Appendix Figure D.1 plots the coefficients for the full dynamic difference-in-differences specification for each distance.

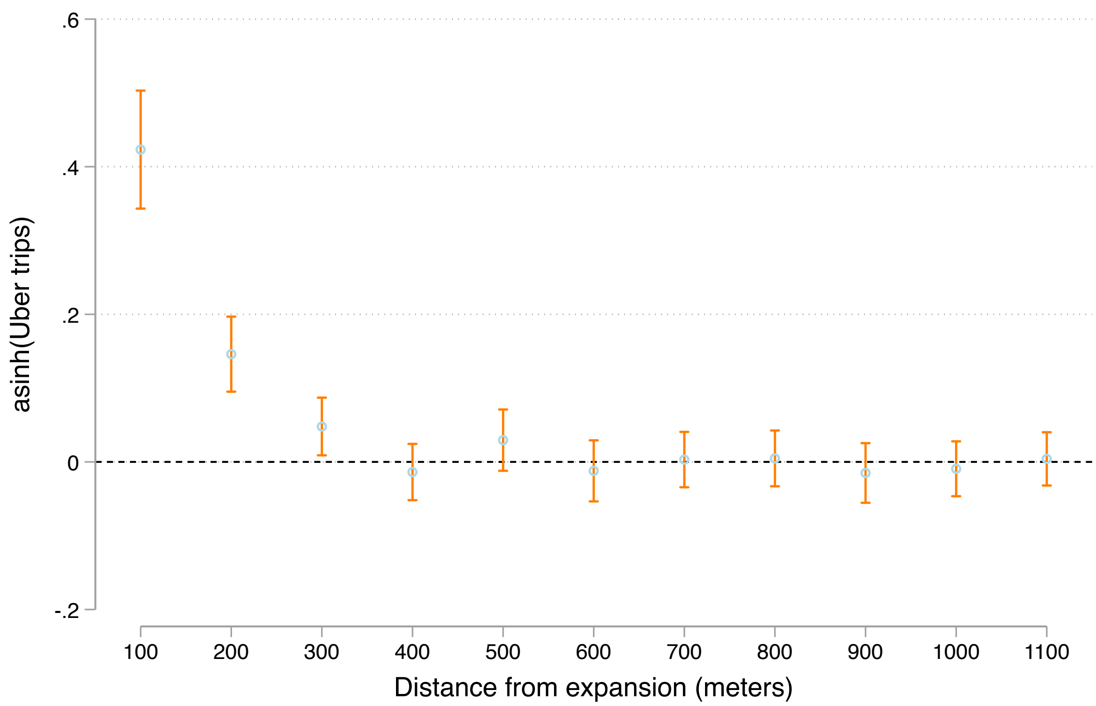


Figure 4.4: Relative effect by distance of new train stations on Uber trips

*Notes:* This figure plots the coefficients (circles) and 95% confidence intervals (bars) from estimating equation (4.2). These coefficients estimate the effect of a new train station opening on Uber trips at varying distances from the train station during months 1 through 6, relative to the next further distance band, and relative to months -6 through -3. Each distance is measured over a 100 m band ending at the given distance; for example, the coefficient at 400 m reports the change in trips between 300 and 400 m away from a train station.

Our finding of no marginal effect, relative to the next further out distance band, past 300 m from a transit station, implies that the interaction between transit and Uber past 300 m is too small to detect. An alternative hypothesis is that the effect from 300–1100 m is constant, and so our difference-in-differences design nets it out. To test this, we estimate an event study of the effect of a new transit station opening on Uber ridership at 1100–1200 m. This specification does not use the next further out distance band to control for time trends, instead relying on city by month fixed effects. Figure 4.5 shows that we find no detectable treatment effect.[8]

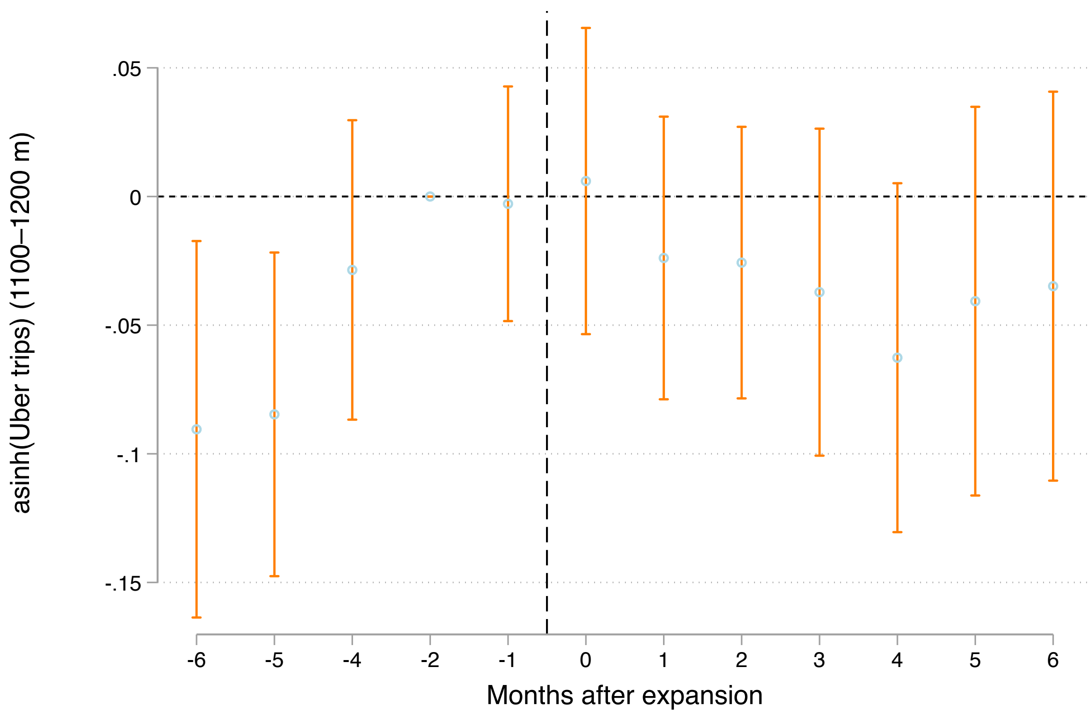


Figure 4.5: Event study estimates for 1100–1200 m

*Notes:* This figure plots the coefficients (circles) and 95% confidence intervals (bars) for the percentage effect of a new train station opening on Uber trips 1100–1200 m away. The coefficients are plotted for each month between six months prior and six months after an expansion. The regression is an event study model of station openings on the inverse hyperbolic sine of Uber trips that either originated or terminated at a given distance band from the opening. The model contains station by distance band and city by time fixed effects; additionally, fixed effects for distance band by more than 6 months before or after an expansion are included. All errors are clustered at the station-level.

[8]While this specification controls for city trends, it still does not control for the hyper-local trends captured in our difference-in-difference analysis. Moreover, the event study specification leads to cities without multiple, differently-timed expansions contributing no variation to the estimation of the main coefficients.

If we assume the true effect of new transit stations on Uber ridership at 1100–1200 m is zero, then we can sum up the relative treatment effects beyond a given distance to estimate the total treatment effect at that distance band. Figure 4.6 reports the results from doing so. As expected, the total effects at 0–100 and 100–200 m are larger, the total effect at 200–300 m is relatively unchanged, and the total effect beyond 300 m is indistiguishable from zero. Details of this calculation can be found in Section D.2.

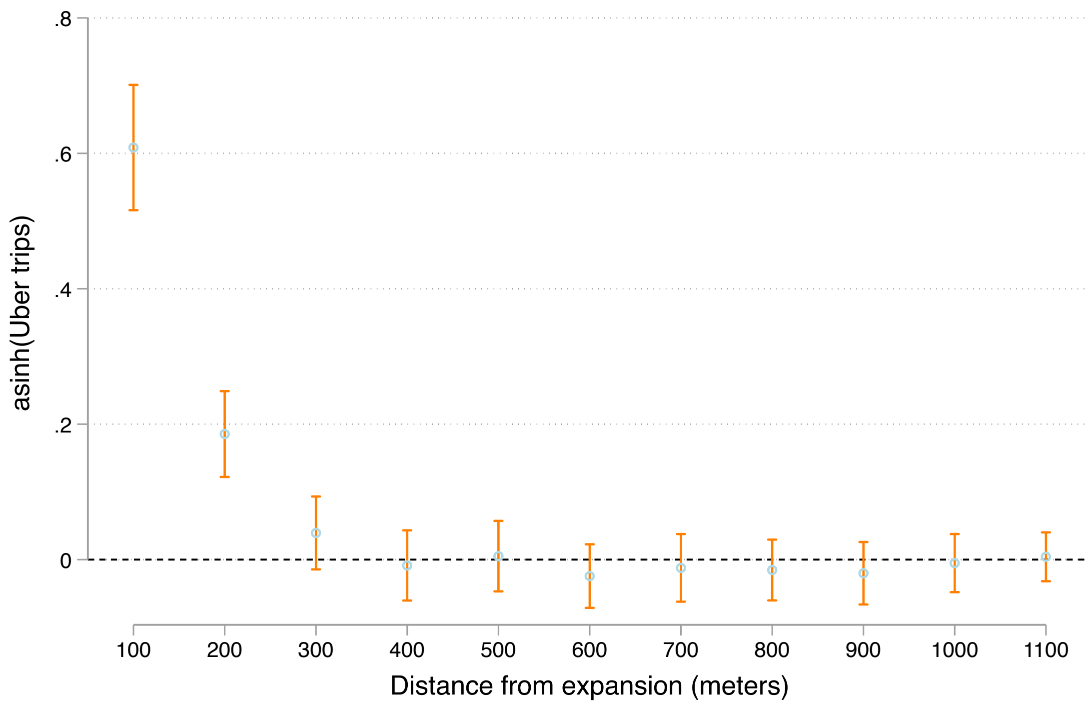


Figure 4.6: Effect by distance of new train stations on Uber trips

*Notes:* This figure plots the coefficients (circles) and 95% confidence intervals (bars) for the effect of a new train station opening on Uber trips at varying distances from the train station. Each distance is measured over a 100 m band ending at the given distance; for example, the coefficient at 400 m reports the change in trips between 300 and 400 m away from a train station.

### Heterogeneity

In this section we explore sources of heterogeneity in the effect of a new transit station opening on Uber ridership. We start by allowing the treatment effect to differ by city. We use a version of equation (4.2) that allows the coefficient $\alpha$ to be city specific. We plot the results of doing so for Uber trips within 0–100 m of a transit station using a funnel graph in Figure 4.7. In this figure, the x-axis shows estimated coefficients and the y-axis shows standard errors. The region in white contains estimates that are not statistically different from zero. The light, medium, and dark gray regions contain estimates that are statistically different from zero at 10%, 5%, and 1%, respectively. The figure shows that the estimated coefficients are clustered around our average estimate (marked with the large triangle), showing that the treatment effect at 0–100 m is consistent across cities.

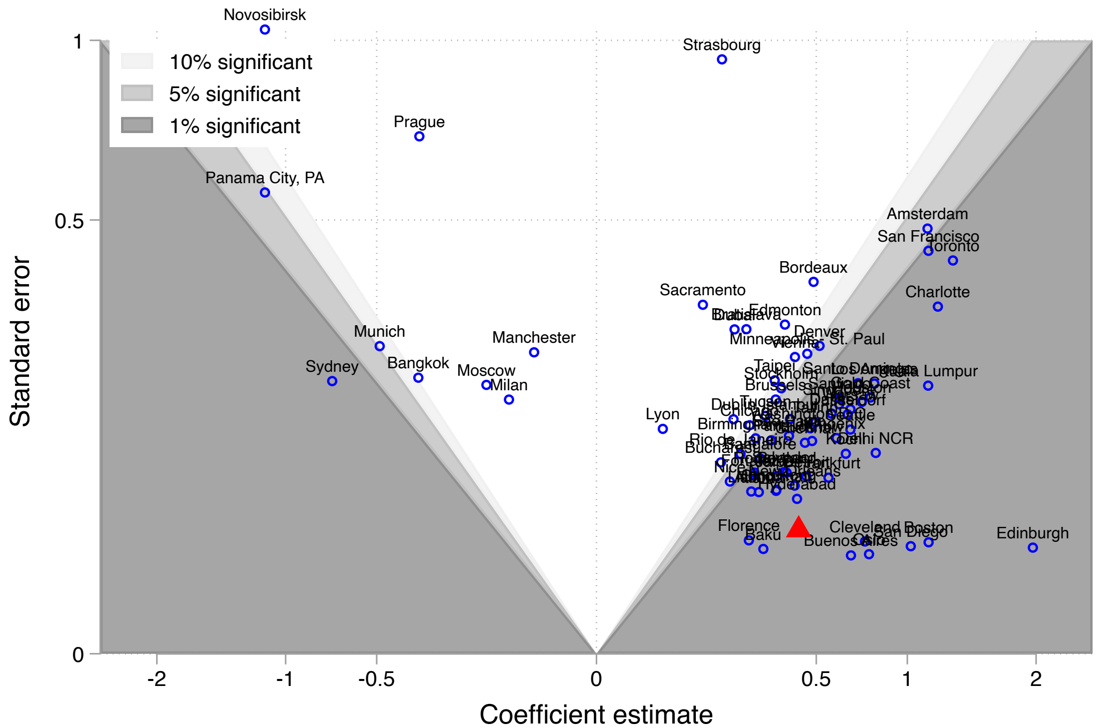


Figure 4.7: Heterogeneous effect by city, 0–100 m

*Notes:* This funnel graph shows city-specific treatment effects based on equation (4.2) where the coefficient $\alpha$ is allowed to vary by city. The x-axis shows coefficient estimates, the y-axis shows standard errors. The region in white contains estimates that are not statistically different from zero. The light, medium, and dark gray regions contain estimates that are statistically different from zero at 10%, 5%, and 1%, respectively. The large red triangle indicates the average effect reported in Figure 4.4.

There are a variety of mechanisms by which Uber and public transportation affect each other, and there exist trips for which they are substitutes and other trips for which they are complements. Thus, it is reasonable to expect that in some cities the net effect would be that of substitutes while in other cities the net effect would be that of complements. While we do not see this at 0–100 m, to give ourselves the best chance of detecting this, we also investigate heterogeneity at 300–400 m. This is where the average effect is first zero, and so seems the best place to look for cities with a negative effect. We plot the city-level heterogeneity in the relative treatment effect at 300–400 m in Figure 4.8. We find that most of the city-level effects are clustered around zero, with few of them statistically significant.

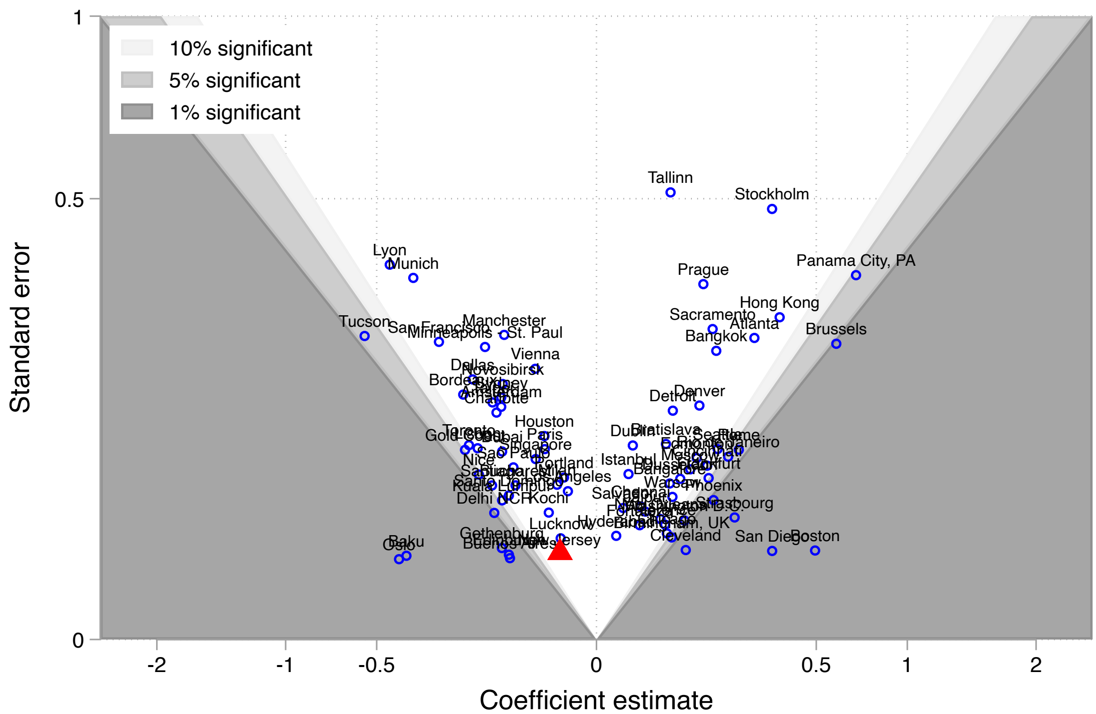


Figure 4.8: Heterogeneous effect by city, 300–400 m

*Notes:* This funnel graph shows city-specific treatment effects based on equation (4.2) where the coefficient $\alpha$ is allowed to vary by city. The x-axis shows coefficient estimates, the y-axis shows standard errors. The region in white contains estimates that are not statistically different from zero. The light, medium, and dark gray regions contain estimates that are statistically different from zero at 10%, 5%, and 1%, respectively. The large red triangle indicates the average effect reported in Figure 4.4.

### Mechanisms

We now turn to identifying the mechanisms by which public transit and Uber affect each other. If Uber is being used to help with the first- and last-mile of transit trips, then we expect to see that, close to transit stations, the average length of an Uber trip that starts or ends decreases after a transit station opens. To test this, we use the same specification as in (4.2), except that we use the average trip length as our outcome. The results from doing so are plotted in Figure 4.9. We find that the a new transit station opening reduces the average trip length by 1.26 km within 0–100 m and 0.43 km within 100–200 m of a transit station.

We find no change at 200–300 m, which suggests that the increase in trips we observe at that distance is not due to first- and last-mile usage.

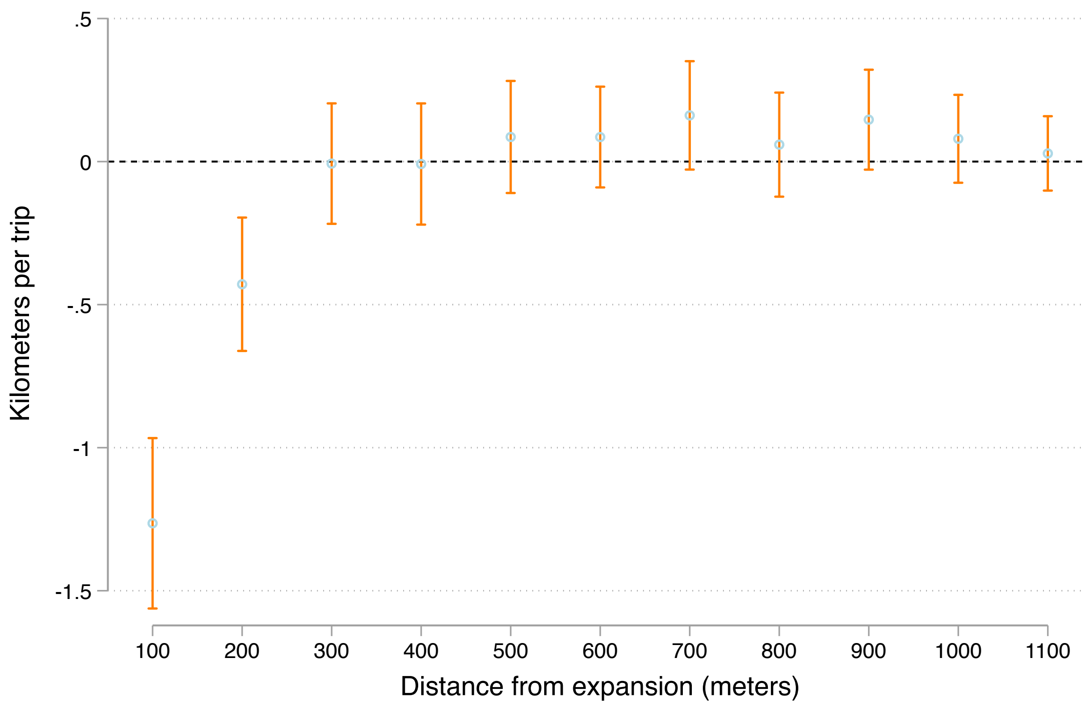


Figure 4.9: Effect by distance of new train stations on kilometers per Uber trip

*Notes:* This figure plots the coefficients (circles) and 95% confidence intervals (bars) for the effect of a new train station opening on kilometers travelled per Uber trip at varying distances from the train station. Each distance is measured over a 100 m band ending at the given distance; for example, the coefficient at 400 m reports the change in trips between 300 and 400 m away from a train station.

### Robustness

This section contains four robustness tests. We start by conducting placebo tests and showing our results are robust to alternative specifications. We start with placebo tests of our estimates of the effect of a transit expansion on Uber trips at different distance bands. For each placebo test, we assign each transit station a fake opening date randomly drawn from the list of dates for which that station has sufficient data to run our specification.[9] We repeat this process 100 times. Figure 4.10 uses a box and whisker plot to compare the 5$^{\text{th}}$, 25$^{\text{th}}$, 50$^{\text{th}}$, 75$^{\text{th}}$, and 95$^{\text{th}}$ percentiles of distribution of placebo results to our actual

[9]We require six months of data before and after the station opens.

estimates, denoted by blue circles. It confirms the results from Figure D.1 that a transit expansion increases Uber ridership close to the station while reducing it further way.

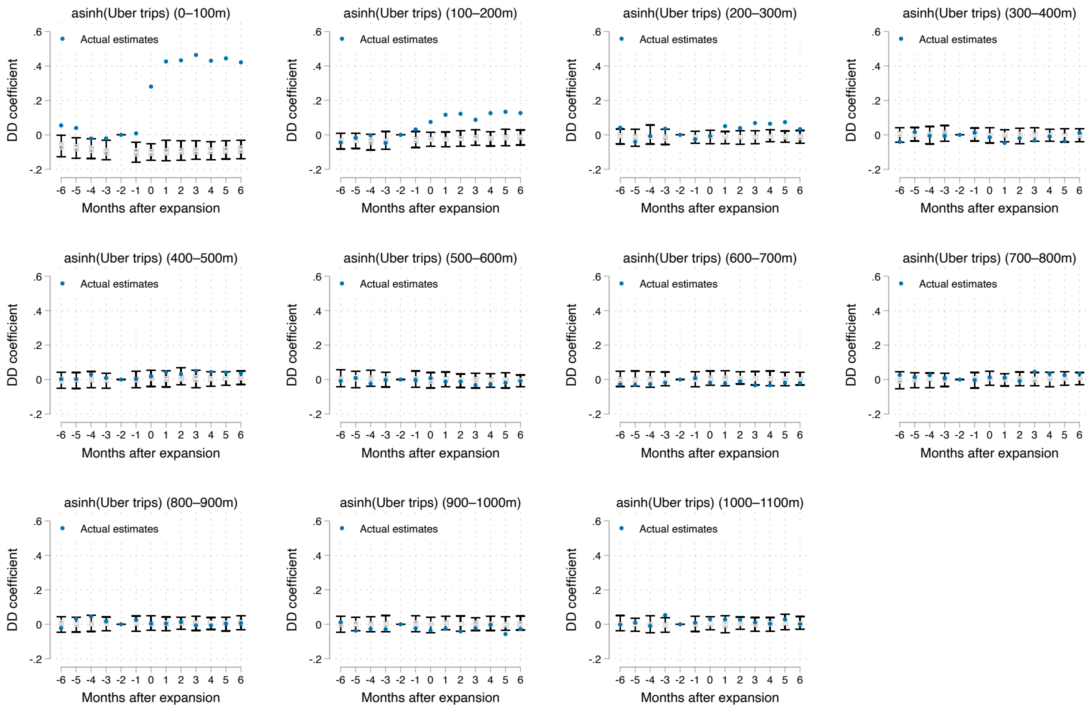


Figure 4.10: Placebo tests for all 100 m distance bands, up through 1000–1100 m

*Notes:* This figure plots the results of a placebo test where we randomly assign placebo opening dates to each transit station.

Appendix Figure D.2 reports results using pseudo-Poisson maximum likelihood. Appendix Figure D.3 reports results from a simpler "donut" design, using 1100–1200 m as a comparison group for all closer distances. Appendix Figure D.4 reports results from the first-difference specification, which uses city by month fixed effects to control for time trends.

## 4.5 Conclusion

There is an on-going debate on whether ride-hailing complements or substitutes public transportation. The answer to this question has important public policy implications regarding how ride-hailing is regulated and taxed, for transit service and infrastructure planning, and for transit-ride-hailing partnerships. However, there remains great uncertainty as existing estimates vary in sign and magnitude.

We address this question using novel data and an innovative identification strategy. Our identification strategy relies on exogenous variation in transit availability caused by rail expansions. Using proprietary trip data from Uber, we use a dynamic difference-in-differences strategy to estimate how transit expansions affect local Uber ridership. Our method controls for hyper-local trends in Uber ridership. We find that a new rail station opening increases Uber ridership within 300 m, and has no impact between 300–1200 m. This implies that Uber and rail transit complement each other.

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# Appendix A

# Appendix for Neighborhood-Level Investment from the U.S. Opportunity Zone Program: Early Evidence

## A.1 Data Appendix

**Qualified Opportunity Fund Investment from Form 8996**

Our main analysis of OZ investment is based on electronically filed tax records of IRS Form 8996. In this appendix we provide additional details about how the definitions in our analysis correspond to line items from this tax form, available online here.

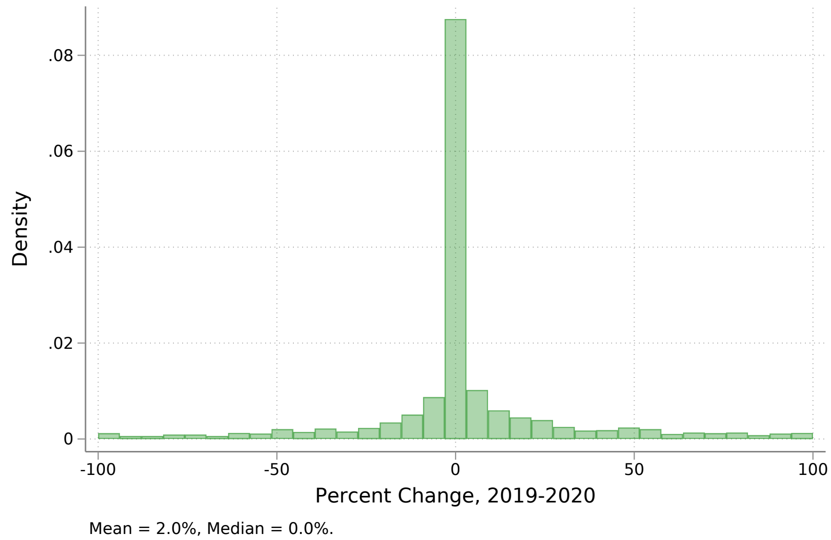
Form 8996 allows us to separately observe QOF property and business investment (in sections V and VI of the form, respectively), as well as the associated dollar value and OZ census tract receiving the investment. For business investment, QOFs also report the Employee Identification Number (EIN) of the QOBs in which they invest. We use tax records associated with these EINs to compute statistics on QOBs, such as the industry breakdown in Table 1.4.

We use end-of-year values for all QOF investment computations. For electronic filers, we define property investment as the sum of columns (d) and (e) in Section V line 1; business investment as the sum of column (f) in Section VI line 1; and total investment as the sum of property and business investment. For paper filers (for whom we do not observe detailed tract-level reporting from Sections V and VI) we measure total investment from Section II, line 11.

These end-of-year values represent stocks, not flows. When analyzing trends in OZ investment over time, the data do not allow us to distinguish between financial inflows/outflows versus appreciation/depreciation of assets. In Table 1.2 of the main text, we estimate aggregate flows as the change in stock from one year to the next, assuming that net appreciation is equal to zero. In Figure A.1, shown below, we provide evidence that this assumption appears reasonable: the median and average percent change in reported assets from 2019-2020 at the QOF-QOB-tract level is approximately equal to zero. The data thus suggest that the 2019-2020 net difference in reported assets is likely to closely approximate new investment in 2020.

For approximately $3 billion of reported QOF investment, we are unable to match census tracts reported by QOFs on Form 8996 to a legally designated Opportunity Zone tract. We consider two possible reasons for this mismatch. First, regulatory guidance from the Treasury Department allows that QOFs may hold a fraction of their assets (10%) in non-qualifying OZ property. Second, the mismatches may simply reflect taxpayer or administrative error. In either case, we do not attempt to assign these unmatched tracts to proper OZ census tracts. This choice implies that, beyond our exclusion of paper filers, we may further underestimate the share of OZ tracts receiving QOF investment. As we have emphasized, these data are preliminary and will be subject to revision when more comprehensive data becomes available.

Figure A.1: Percent Change in Reported OZ Assets at the QOF-QOB-Tract Level, 2019-2020




*Notes*: Plot shows the distribution of 2019-2020 changes in reported end-of-year assets at the QOF-QOB-tract level from electronic filers of IRS form 8996. N = 2,344 QOF-QOB-tract pairs. We exclude changes greater than 100%, as these observations are likely to capture capital inflows rather than appreciation or depreciation.

## Individual, Household, and Family Income Definitions

Individual wage income

We measure wage income at the individual-level using the universe of IRS Form W2. For each individual, we sum up wage and salary income from all employers, and count an individual as employed if they receive at least one W-2 from an employer. These measures currently do not capture self-employed individuals, although we intend to measure them in future revisions of this working paper.

Household income estimates for income tax filers

Our estimates of household income start from the household identifiers and income definitions from Larrimore, Mortenson, and Splinter (2019) and Larrimore et al. (2020). We describe these computations below, and indicate where we make alternate definitional choices to make our estimates more comparable with tract-level estimates from the Census American Community Surveys. These measures use information returns to compute income for non-filers, thus allowing us to construct income estimates for 98-99% of the U.S. population.

1. Start with total income from line 22 of IRS Form 1040, which is the sum of wage income, salary income, business income, dividends, alimony, taxable interest, rents and

royalties, unemployment compensation, taxable Social Security income, and taxable private retirement income.

2. Add non-taxable interest from IRS Form 1040.

3. Subtract taxable social security income and add total social security benefits from IRS Form SSA-1099.

4. Subtract taxable private retirement income and add gross private retirement income, defined as savings distributions minus rollovers reported on IRS Forms 5498 and 1099-R.

5. Bottom-code incomes at zero to mitigate the effects of business losses.

As Larrimore, Mortenson, and Splinter (2019) note, federal tax records do not allow us to observe non-taxable cash transfer income such as public assistance and supplemental security income, which comprise approximately 2.5% of income tabulated by the Census Bureau. We differ from these authors in that we do not subtract capital gains reported on IRS Form 1040 Schedule D, so as to make our measures more comparable with the income definitions used in the Census American Community Surveys.

Household income estimates for non-filers

To estimate income for households that do not file income tax returns, we again follow Larrimore, Mortenson, and Splinter (2020) and sum up income from the following information tax returns:

- Wage and salary income from IRS Form W-2
- Unemployment compensation reported on IRS Form 1099-G
- Social Security and disability income reported on IRS Form SSA-1099
- Interest income from IRS Form 1099-INT
- Dividends from IRS Form 1099-DIV
- Retirement savings distributions minus rollovers reported on IRS Forms 5498 and 1099-R
- Self-employment income from IRS Forms 1099-K and 1099-MISC, scaled by a factor of 0.7 to correct for the fact that these values reflect gross income and do not subtract business expenses. The resulting value is an estimate of *net* self-employment income.
- Business income from partnerships and S-corporations from Schedules K-1 attached to IRS Forms 1065 and 1120S.

We include these income sources since individuals would be required to report them on IRS Form 1040 if they had positive income tax liability.

Family income estimates

The Census Bureau defines a family as two or more individuals related by blood or marriage. To estimate family income, we link individuals living in the same household who we observe to be married or claimed as dependents and assign them a unique family ID variable. We always assign the same family ID to all individuals who appear on the same tax form, and link married couples together even if they file their tax returns separately. To better capture intergenerational families living within the same household, we also link individuals over age 65 to the family ID of a prime-age filer over 30 years old if there is only one such prime-age filer in the household. These measure nevertheless modestly understate family size relative to Census measures, since tax data do not allow us to observe whether individuals in the same household are related by blood. For example, our family definition is likely to exclude adult children who live with their parents and are not claimed as dependents. If these adult children earn income, our estimates will understate family income relative to ACS estimates. However, our comparisons of IRS and ACS-based measures presented in Section 1.4 suggest that any such differences are likely to be small.

**Firm Employment, Location, and Real Investment Definitions**

With the exception of self-proprietors, all US businesses are legally required to file annual tax returns with the IRS. Our firm sample excludes self-proprietors and is based on the universe of C corporations, S corporations, and partnerships. We begin by linking all firms and EINs to their parent company EIN using the crosswalks constructed by Dobridge, Landefeld, and Mortenson (2019). Similarly, we link employers on all IRS Forms W-2 to their parents. We define firm employment as the total number of individuals receiving a W-2 from the parent company. Since individuals may change jobs or leave the labor force throughout the course of a calendar year, and firms may or may not replace those employees throughout the year, our annual estimates of firm employment are higher than point-in-time snapshots of firm employment.

We assign firms the address that they report on the cover page of their annual tax return (IRS Forms 1120, 1120S, or 1065). As we discussed in Section 1.4, a limitation of these data is that business tax records typically provide only headquarter addresses and do not allow us to identify the establishment locations of multi-establishment firms. To assess the sensitivity of our measures to this data limitation, when aggregating our measures we differentiate by firm size, defined as the number of employes. Since smaller firms are less likely to have multiple establisments, they may provide a more geographically accurate picture of local economic conditions, with the caveat they are not represenative of all firms.

We follow Yagan (2015) in defining firm-level real investment as the sum of the following line items reported on IRS Form 4562:

- Section 179 property reported on line 8
- Tenative deductions reported on line 9

- Basis of assets placed in service during the current tax year using the General Depreciation System, reported on lines 19a-19i
- Basis of assets placed in service during the current tax year using the Alternative Depreciation System, reported on lines 20a-20c
- Listed property repoted on line 21.

**Geocoding Procedure**

For individuals, our starting point is address information reported on IRS Form 1040. For non-filers, we use address information from information returns in the following order of prioritization: IRS Form SSA-1099 (reporting social security income), IRS Form W-2 (reporting wage and salary income), and IRS Form 1099-G (reporting unemployment compensation). For businesses, we use the address that firms report on the cover form of IRS Form 1120, 1120S, or 1065. If multiple addresses are available from different forms, we prioritize PO boxes last. We do not attempt to geocode PO boxes, which account for approximately 3-4% of the general population and are disproportionately prevelant in rural areas.

We clean the addresses to remove non-alphanumeric characters, and shorten street suffixes using standardized abbreviations (for example, "STREET" becomes "ST"). We strip out text preceding the numeric house number or following the street suffix, such as apartment or unit identifiers. To correct minor mispellings, we fuzzy match street names to a file of street names compiled by the US Postal Service, and require that zip codes match exactly. We do not use city or state information, finding that street addresses and zipcodes are less prone to textual error.

To protect taxpayer privacy, we do not share taxpayer address information with any commercial geocoding firms. Rather, we import publicly available address databases from Open Street Maps, the National Address Database, and Nominatum into secure federal government servers and geocode all addresses in-house. We also externally geocode a limited number of publicly available addresses from the US Postal Service using the commerical service OpenCageGeo.

When matching to these databases, we always require that zipcodes match exactly to reduce the prevalence of false positive matches. We obtain the latitude and longitude coordinates from these matches and then link them to 2010 block and tract identifiers using shapefiles provided by the US Census. If we are unable to match an address directly to its geo-coordinates, but observe a house number that is between two higher and lower addresses on the same zip-street for which we do have geo-coordinates and observe the same tract of block ID, we then infer and impute the missing tract and block ID. For example, if we were to observe that 10 Main St. 10001 and 14 Main St. 10001 are both located in census tract A, we would also infer that 12 Main St. 10001 is located in census tract A.

Overall, we match approximately 81% of the US population to a census tract, which corresponds to approximately 85% of non-PO Box addresses. This match rate is approximately constant with respect to tract population (recall that Census tracts are delineated to be of approximately even populations), but is lower in rural areas than in urban areas. Nevertheless, we obtain significantly larger sample sizes relative to those available in the Census American Community Surveys (ACS), which are based on 1% random stratified samples of the population. Our comparisons of IRS- and ACS-based measures of tract-level income in Figure 1.8 suggest that any biases resulting from non-random biases in the geocoding procedure are likely to be small.

## A.2 Appendix to Section 1.3: Descriptive Statistics

### Demographic and Economic Indicators from ACS and IRS

Table A.1: Characteristics of Neighborhoods Receiving OZ Investment

**Panel A: Correlates with Census ACS**

| | 2017 Demographics | | | 2010-2017 Trends | | |
|---|---|---|---|---|---|---|
| | OZ Inv>0 | OZ Inv=0 | All | OZ Inv>0 | OZ Inv=0 | All |
| Population | 4,297 | 3,840 | 4,385 | 0.05 | -0.01 | 0.04 |
| Median Family Income | 46,386 | 40,174 | 72,109 | 0.12 | 0.08 | 0.11 |
| Poverty Rate | 0.27 | 0.28 | 0.15 | 0.01 | 0.02 | 0.01 |
| Median Home Value | 181,806 | 134,859 | 244,328 | -0.00 | -0.05 | 0.00 |
| Gini | 0.46 | 0.46 | 0.43 | 0.02 | 0.02 | 0.02 |
| White | 0.57 | 0.59 | 0.73 | 0.01 | -0.00 | -0.01 |
| Black | 0.26 | 0.25 | 0.14 | -0.01 | -0.00 | 0.00 |
| Hispanic | 0.24 | 0.34 | 0.17 | 0.01 | 0.01 | 0.02 |
| Non-Citizen | 0.10 | 0.08 | 0.07 | -0.01 | -0.01 | -0.00 |
| College Graduate | 0.13 | 0.10 | 0.21 | 0.02 | 0.01 | 0.02 |
| Age 65+ | 0.13 | 0.15 | 0.16 | 0.01 | 0.02 | 0.02 |
| Age 18- | 0.22 | 0.24 | 0.22 | -0.02 | -0.02 | -0.02 |
| Unemployed | 0.10 | 0.13 | 0.07 | -0.02 | -0.02 | -0.02 |
| **Number of Tracts** | **3,242** | **5,446** | **74,288** | **3,242** | **5,446** | **74,288** |

**Panel B: Correlates with New IRS Measures**

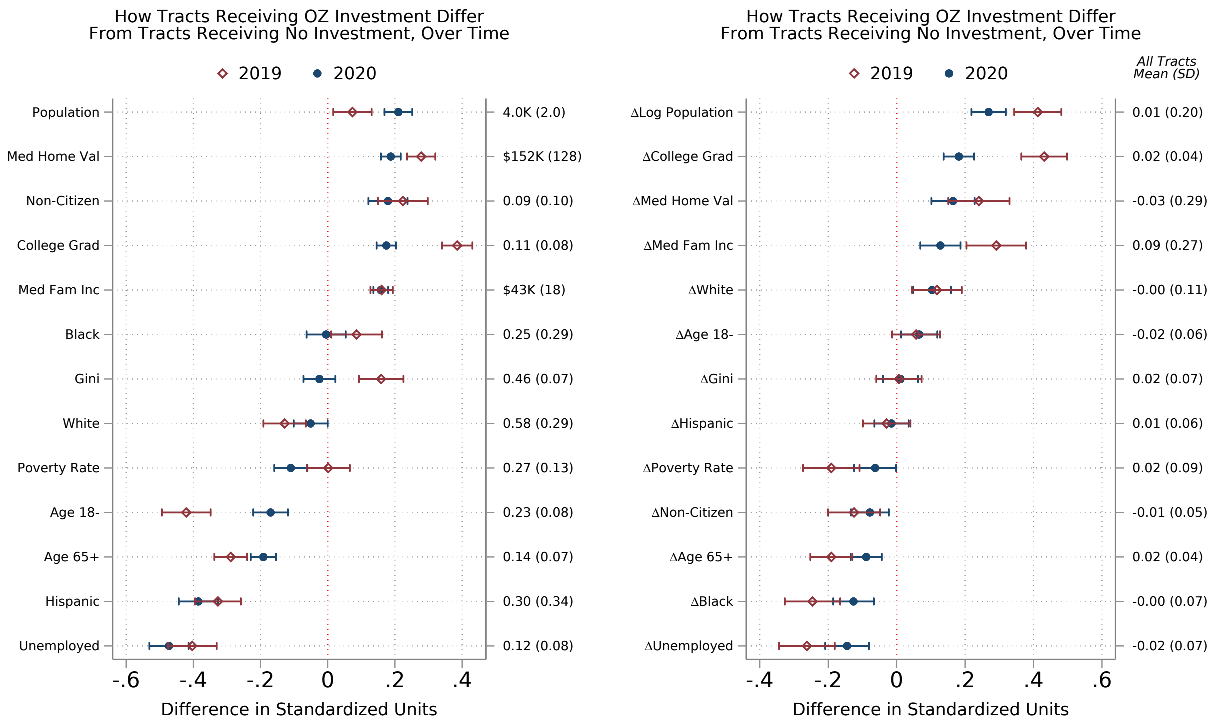
| | 2017 Demographics | | | 2010-2017 Trends* | | |
|---|---|---|---|---|---|---|
| | OZ Inv>0 | OZ Inv=0 | All | OZ Inv>0 | OZ Inv=0 | All |
| Median Family Income | 57,531 | 53,860 | 89,209 | 0.08 | 0.07 | 0.06 |
| Poverty Rate | 0.16 | 0.17 | 0.09 | -0.01 | -0.01 | -0.01 |
| Employment | 1,566 | 1,352 | 1,780 | 0.11 | 0.09 | 0.07 |
| Average Wages | 30,758 | 27,713 | 42,998 | 0.09 | 0.08 | 0.10 |
| Median Wages | 22,214 | 20,425 | 29,784 | 0.10 | 0.09 | 0.10 |
| 90/10 Wage Ratio | 4.43 | 4.36 | 4.50 | -0.15 | -0.15 | -0.13 |
| Real Investment (mil) | 20.92 | 4.54 | 10.18 | 1.21 | 1.15 | 1.24 |
| Small Firm Real Investment (mil) | 2.12 | 1.04 | 1.52 | 1.15 | 1.11 | 1.18 |
| # Firms | 181 | 86 | 153 | 0.11 | 0.10 | 0.12 |
| # Small Firms | 70 | 38 | 53 | 0.07 | 0.04 | 0.07 |
| **Number of Tracts** | **3,242** | **5,446** | **74,288** | **3,242** | **5,446** | **74,288** |

*Notes*: This table provides summary statistics comparable with the estimates provided in Figures 1.2 and 1.10. The table compares average demographic and economic characteristics for three groups of census tracts: (1) OZ tracts receiving positive investment from QOFs; (2) OZ tracts receiving zero investment from QOFs; and (3) all tracts nationally. *Trends for IRS measures of median family income and poverty are constructed from 2015-2017, as we have not yet extended the IRS sample back to 2010.

## Demographic Correlates of OZ Investment Over Time

Figure A.2: Characteristics of Neighborhoods Receiving OZ Investment Over Time

**Panel A: 2017 Demographics** **Panel B: 2010-2017 Trends**




*Notes*: N=8,764 census tracts. The figure shows average differences in demographic characteristcs for three groups of census tracts: (1) OZ tracts receiving positive investment in 2019; (2) OZ tracts receiving positive investment in 2020 (but not in 2019); and (3) OZ tracts receiving zero investment in both 2019 and 2020. The data are from the 2017 and 2010 5-Year ACS. All variables are standardized to have mean zero and standard deviation one. Error bands show 95% confidence intervals with robust standard errors.

## Investment by Industry: 6-digit NAICS Codes

Table A.2: Industry Composition of Funds and Recipient Firms

**Panel A: QOF Investor Funds**

| NAICS | Industry | # QOF | $ (mil) | $ Share |
|---|---|---|---|---|
| 531390 | Activities Related to Real Estate | 554 | 3,742 | 0.20 |
| 520000 | Finance and Insurance | 445 | 3,403 | 0.18 |
| 531120 | Lessors of Nonresidential Buildings | 307 | 1,998 | 0.11 |
| 531110 | Lessors of Residential Buildings and Dwellings | 311 | 1,591 | 0.08 |
| 551112 | Offices of Holding Companies | 112 | 1,355 | 0.07 |
| 523900 | Financial Investment Activities | 98 | 718 | 0.04 |
| 236000 | Construction of Buildings | 93 | 651 | 0.03 |
| 531100 | Lessors of Buildings | 123 | 650 | 0.03 |
| 525110 | Pension Funds | 126 | 552 | 0.03 |
| 531000 | Real Estate | 53 | 514 | 0.03 |
| 531310 | Nonresidential Property Managers | 70 | 494 | 0.03 |
| 236110 | Residential Building Construction | 71 | 365 | 0.02 |
| 525990 | Financial Vehicles | 58 | 253 | 0.01 |
| 531190 | Lessors of Real Estate Property | 32 | 235 | 0.01 |
| 236220 | Non-Residential Building Construction | 24 | 215 | 0.01 |
| 525000 | Funds and Trusts | 52 | 184 | 0.01 |
| 721110 | Hotels and Motels | 19 | 173 | 0.01 |
| 523920 | Portfolio Management | 23 | 101 | 0.01 |
| – | Other | 158 | 630 | 0.03 |
| – | Unknown | 20 | 565 | 0.03 |
| | **Total** | **2,756** | **18,906** | 1.00 |

**Panel B: QOB Firms Receiving Investment**

| NAICS | Industry | # QOB | $ (mil) | $ Share |
|---|---|---|---|---|
| 531390 | Activities Related to Real Estate | 431 | 3,742 | 0.20 |
| 520000 | Finance and Insurance | 422 | 3,403 | 0.18 |
| 531120 | Lessors of Nonresidential Buildings | 265 | 1,998 | 0.11 |
| 531110 | Lessors of Residential Buildings and Dwellings | 280 | 1,591 | 0.08 |
| 551112 | Offices of Holding Companies | 84 | 1,355 | 0.07 |
| 523900 | Financial Investment Activities | 100 | 718 | 0.04 |
| 236000 | Construction of Buildings | 93 | 651 | 0.03 |
| 531100 | Lessors of Buildings | 103 | 650 | 0.03 |
| 525110 | Pension Funds | 125 | 552 | 0.03 |
| 531000 | Real Estate | 45 | 514 | 0.03 |
| 531310 | Nonresidential Property Managers | 55 | 494 | 0.03 |
| 236110 | Residential Building Construction | 82 | 365 | 0.02 |
| 525990 | Financial Vehicles | 45 | 253 | 0.01 |
| 531190 | Lessors of Real Estate Property | 24 | 235 | 0.01 |
| 236220 | Non-Residential Building Construction | 30 | 215 | 0.01 |
| 525000 | Funds and Trusts | 35 | 184 | 0.01 |
| 721110 | Hotels and Motels | 12 | 173 | 0.01 |
| 523920 | Portfolio Management | 24 | 101 | 0.01 |
| – | Other | 152 | 630 | 0.03 |
| – | Unknown | 74 | 565 | 0.03 |
| | **Total** | **2,490** | **18,906** | 1.00 |

*Notes*: This table shows the industry composition of investing QOF funds and recipient QOB businesses by 6-digit NAICS code. We exclude industries with few QOF investing funds and/or QOB businesses to protect taxpayer privacy.

## Investment by Commuting Zone: Top 50 Commuting Zones

Table A.3: OZ Investment in 50 Top Commuting Zones

| CZ | Total $ (mil) | $ Per OZ Resident | $ Per CZ Resident |
|---|---|---|---|
| New York, NY-NJ-PA | 3,782 | 3,358 | 181 |
| Los Angeles, CA | 1,701 | 1,916 | 92 |
| Phoenix, AZ | 1,328 | 4,274 | 275 |
| Salt Lake City, UT | 1,325 | 15,416 | 542 |
| Denver, CO | 889 | 6,277 | 238 |
| San Francisco, CA | 816 | 4,140 | 143 |
| Detroit, MI | 786 | 4,666 | 158 |
| Washington, DC-VA-MD-WV | 759 | 3,146 | 110 |
| Philadelphia, PA-NJ-DE-MD | 734 | 2,142 | 111 |
| Portland, OR-WA | 703 | 6,271 | 295 |
| Huntsville, AL | 666 | 18,305 | 822 |
| Nashville, TN | 600 | 8,141 | 302 |
| Miami, FL | 571 | 1,748 | 86 |
| Seattle, WA | 570 | 2,593 | 118 |
| Houston, TX | 563 | 1,587 | 82 |
| Austin, TX | 537 | 3,772 | 257 |
| Tampa, FL | 469 | 5,815 | 158 |
| Atlanta, GA | 419 | 2,817 | 73 |
| Cleveland, OH | 365 | 3,083 | 127 |
| Charleston, SC | 360 | 5,390 | 460 |
| Sacramento, CA | 355 | 2,501 | 149 |
| Baltimore, MD | 343 | 2,569 | 118 |
| Indianapolis, IN | 313 | 2,979 | 155 |
| St. Louis, MO-IL | 311 | 5,611 | 108 |
| Minneapolis, MN-WI | 300 | 2,512 | 86 |
| Stockton, CA | 261 | 3,729 | 164 |
| Boston, MA-NH | 256 | 1,466 | 50 |
| Cincinnati, OH-KY-IN | 251 | 2,864 | 110 |
| Dallas, TX | 230 | 1,129 | 30 |
| Richmond, VA | 229 | 4,967 | 183 |
| San Jose, CA | 218 | 2,512 | 82 |
| Charlotte, NC-SC | 211 | 1,969 | 82 |
| Columbus, OH | 194 | 2,100 | 90 |
| Bakersfield, CA | 187 | 1,063 | 126 |
| Fresno, CA | 183 | 906 | 92 |
| Las Vegas, NV | 181 | 2,045 | 84 |
| Chicago, IL-IN-WI | 180 | 1,611 | 18 |
| Orlando, FL | 172 | 1,253 | 58 |
| Bridgeport, CT | 157 | 1,291 | 44 |
| Omaha, NE-IA | 155 | 3,599 | 161 |
| Raleigh, NC | 143 | 1,143 | 67 |
| San Antonio, TX | 138 | 2,054 | 58 |
| Providence, RI-MA | 136 | 2,292 | 84 |
| New Orleans, LA | 131 | 3,321 | 92 |
| Memphis, TN-MS-AR | 121 | 1,389 | 83 |
| Greenville, SC | 121 | 1,802 | 115 |
| Louisville, KY-IN | 114 | 2,490 | 87 |
| Tucson, AZ | 111 | 1,077 | 97 |
| Kansas City, MO-KS | 107 | 1,152 | 50 |
| Birmingham, AL | 101 | 2,388 | 94 |

*Notes*: Table shows OZ investment for the top 50 commuting zones. Investment data based on electronically-filed business tax records in tax years 2019 and 2020. Column 1 shows total OZ investment by commuting zone. Column 2 shows investment per OZ-resident, normalizing by the population of tracts with >0 investment. Column 3 shows investment per CZ-resident, normalizing by the total commuting zone population. We exlude commuting zones with few QOF investing funds and/or QOB businesses to protect taxpayer privacy.

# Appendix B

# Appendix for Locally Optimal Place-Based Policies: Evidence from Opportunity Zones

## B.1 Additional Figures

Figure B.1: Eligible and OZ census tracts within cities

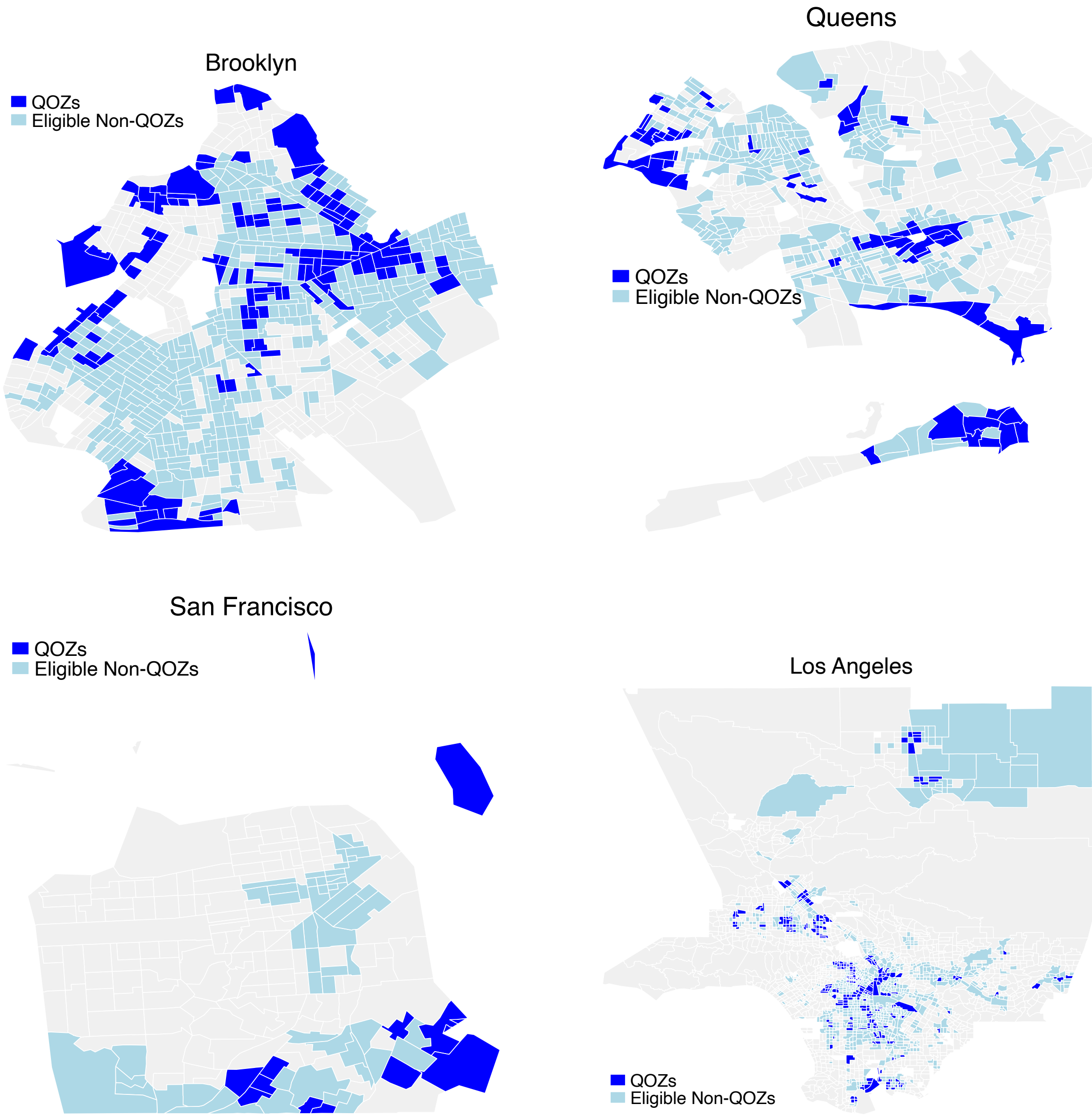
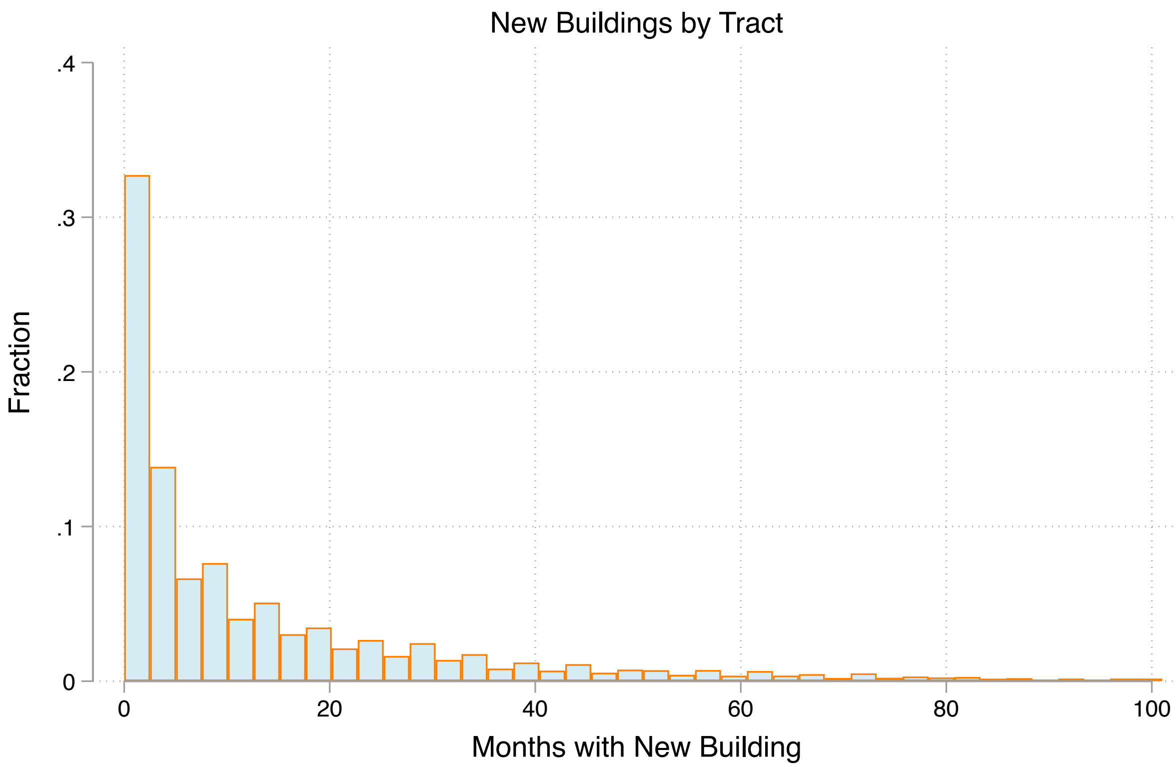
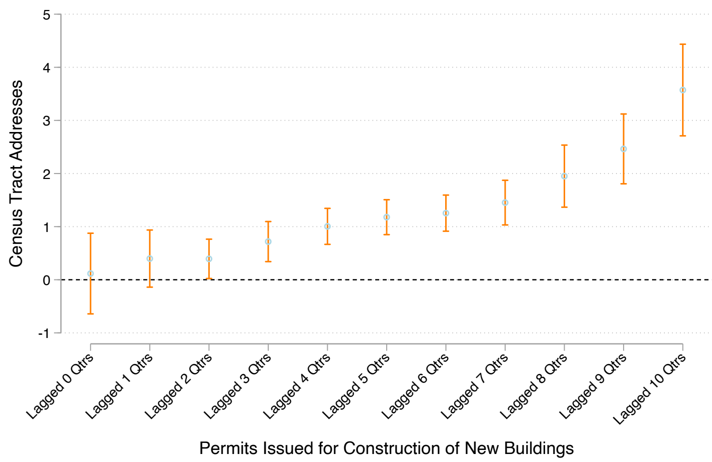


Figure B.2: Distribution of new development




Note: This histogram plots the distribution of number of months with new developments for each census tract in the sample. The time coverage is January 2014 to June 2022. The sample includes 11,936 total tracts.

Figure B.3: Correlation of new construction measure and tract addresses

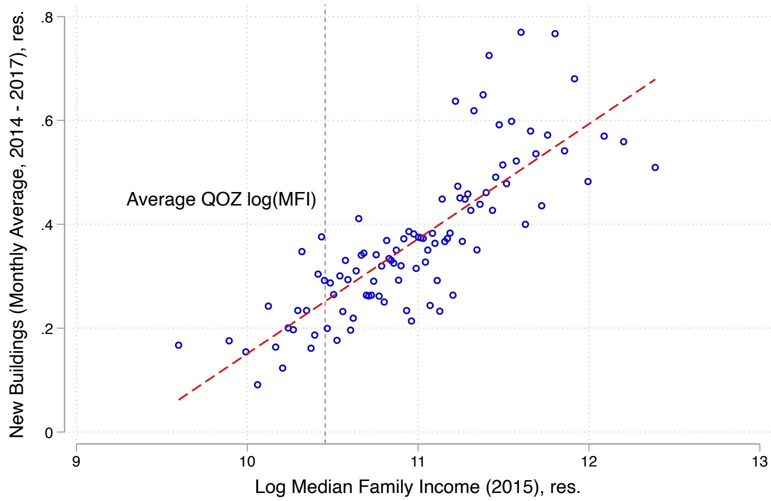



Note: This chart shows coefficients from a regression of total addresses in a census tract quarter on lags of number of permits issued for the construction of new buildings. The address data comes from HUD's USPS vacant addresses data. The regression includes tract and date fixed effects. Errors are clustered at tract-level.

Figure B.4: Correlation of new construction measure and tract "no-status" addresses


Note: This chart shows coefficients from a regression of "no-status" addresses in a census tract quarter on lags of number of permits issued for the construction of new buildings. The address data comes from HUD's USPS vacant addresses data. The regression includes tract and date fixed effects. Errors are clustered at tract-level.

Figure B.5: Median family income vs. new development projects

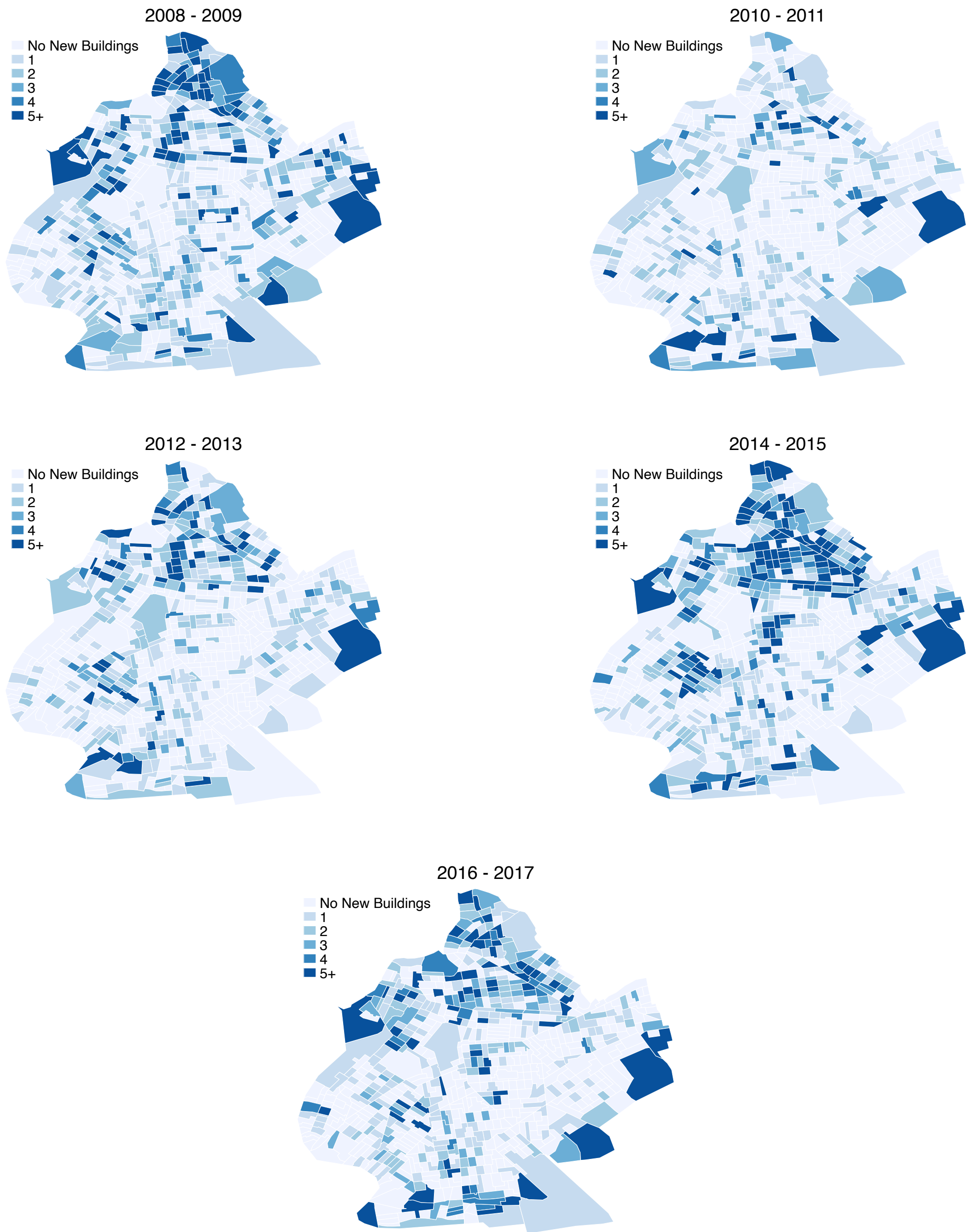



Note: This bin scatterplot shows ACS 2011-2015 tract-level log median family income against the monthly average of new buildings permitted for from 2014 to 2017. The dotted line denotes the average log median family income of Opportunity Zones. A line of best fit is depicted in red.

Figure B.6: Change in median family income vs. new development projects

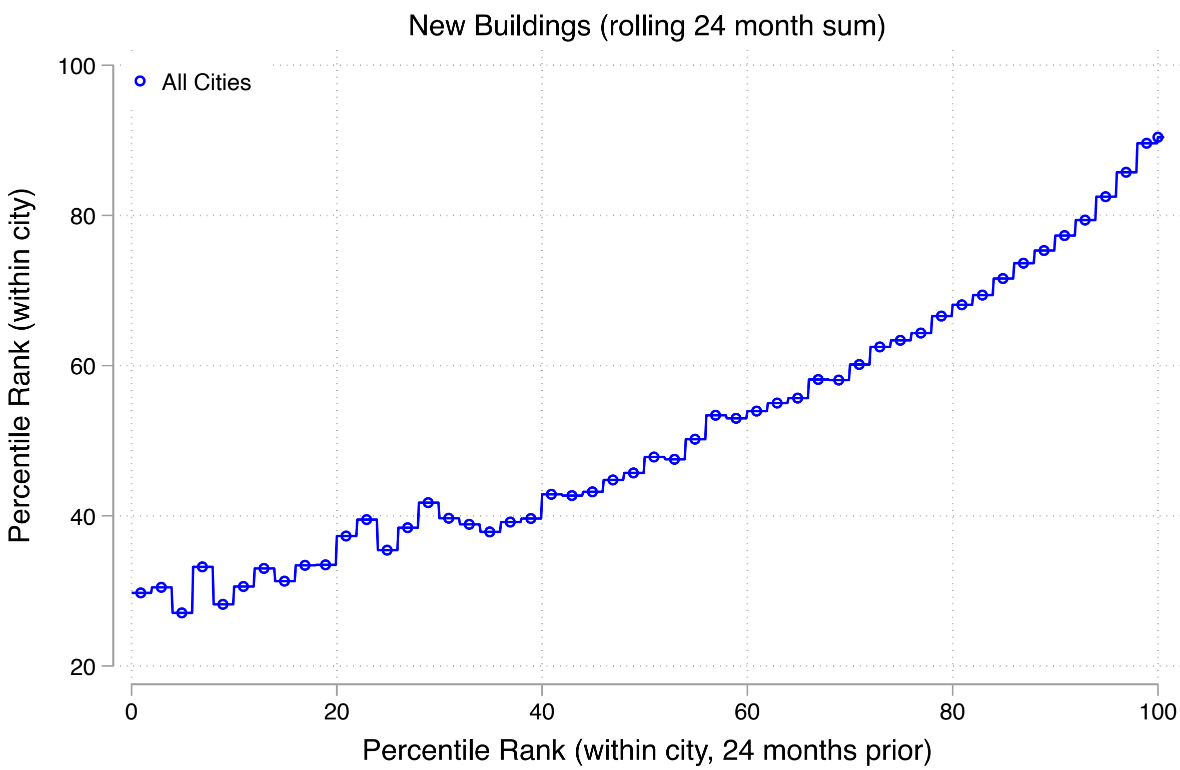

Note: This bin scatterplot shows the change in the log median family income from the 2015 to 2019 ACS against the monthly average of new buildings permitted for from 2014 to 2017. A line of best fit is depicted in red.

Figure B.7: New developments case study: Brooklyn



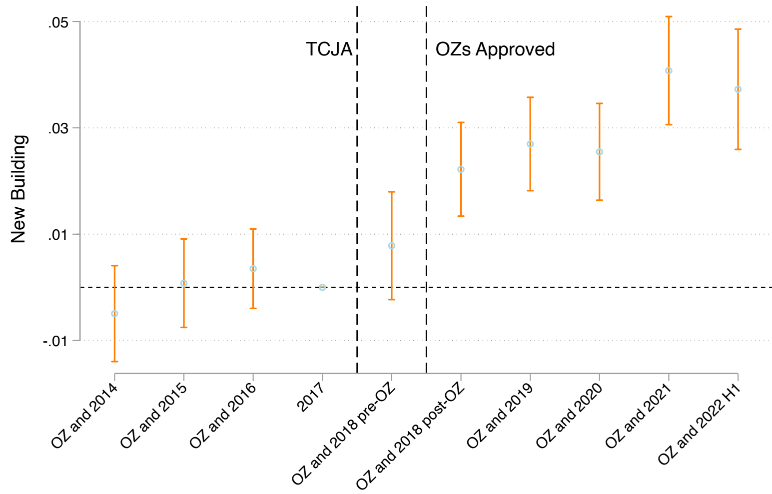

Note: These maps shows the number of new buildings over 2 year horizons for census tracts in Brooklyn.

Figure B.8: Persistence in new development



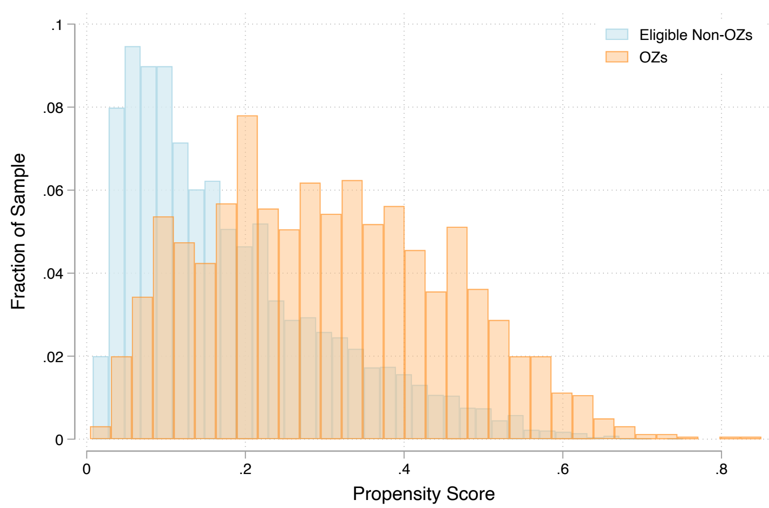

Note: These figures are produced by ranking tracts within cities in terms of the number of new buildings with permits issued in the previous 24 months. I then plot this percentile rank on its 24 month lag, and aggregate within 2-percentile bins across months.

Figure B.9: Difference-in-difference estimates balancing sample



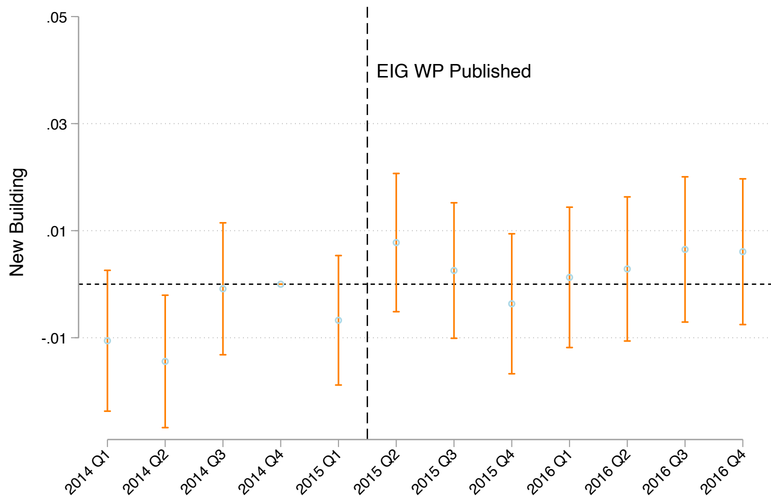

Note: This figure plots the annual version of the main difference-in-differences coefficients. However, a logistic model is run between any time period and right before the policy is implemented to estimate how ACS covariates affect whether the tract is in the sample or not. Observations are then reweighted according to the inverse propensity score. All errors are clustered at tract-level.

Figure B.10: Overlap of propensity scores between OZs and eligible non-OZs



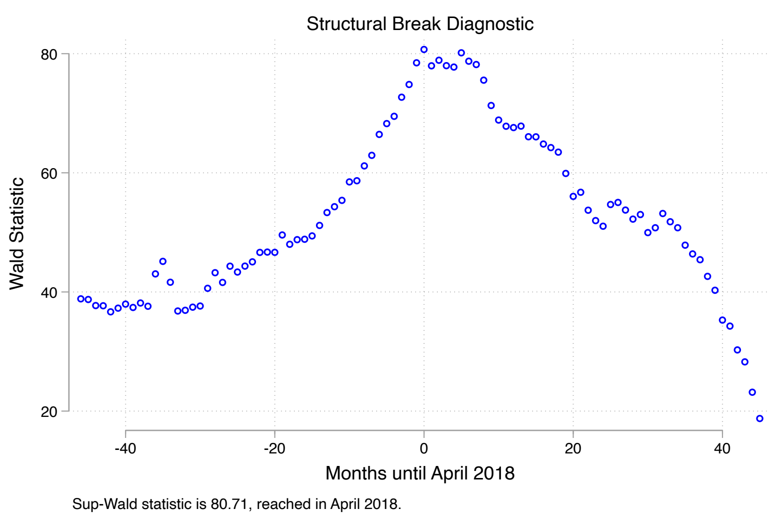

Note: This chart plots propensity scores for OZs against eligible non-OZs. Propensity scores were estimated via a logit model with 2015 5-year ACS tract-level demographics and local housing market covariates as predictors.

Figure B.11: Placebo using EIG white paper release date



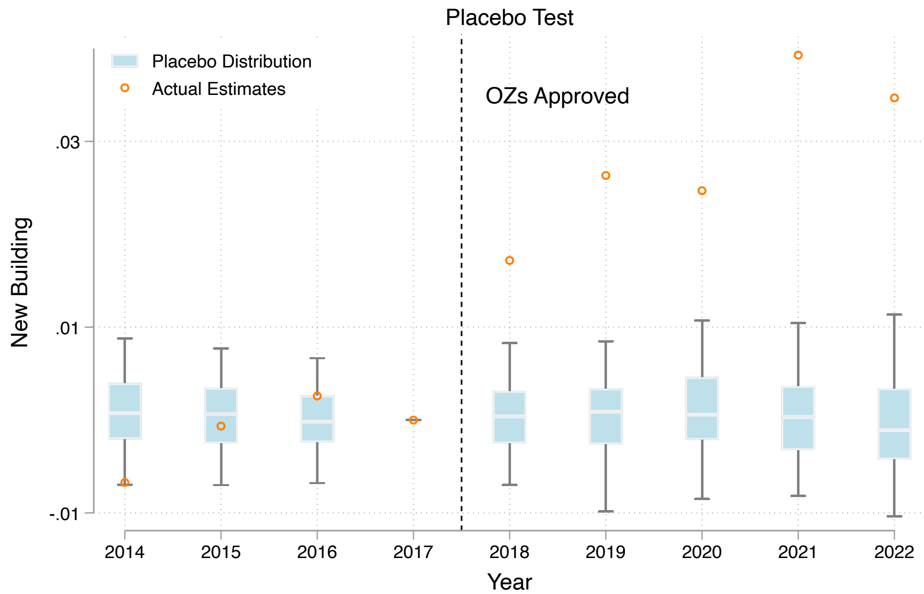

Note: This figure plots difference-in-differences coefficients from a version in which May 2015 (the publication date of the EIG white paper proposing the OZ tax credit) is the program implementation date. The model uses the same controls as the baselin specification: city linear trends, city seasonal effects, and date and tract fixed effects. All errors are clustered at tract-level.

Figure B.12: Andrews (1993, 2003) test for a structural break



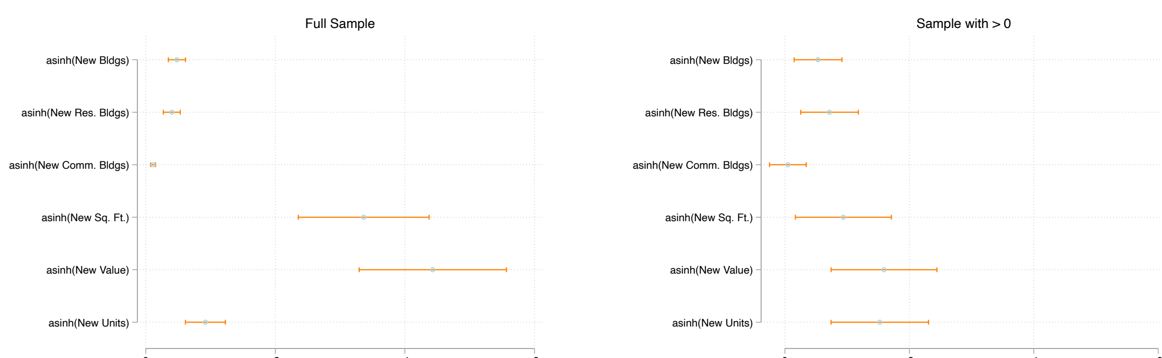

Note: This chart plots the Wald statistic for testing the null hypotheses that the coefficient on $\mathbb{1}\{i \text{ is a OZ}\} \cdot \mathbb{1}\{t \text{ is after } j\}$ in the baseline specification is zero, for each $j$ from Jun 2014 to October 2019. All errors are clustered at tract-level.

Figure B.13: Placebo tests



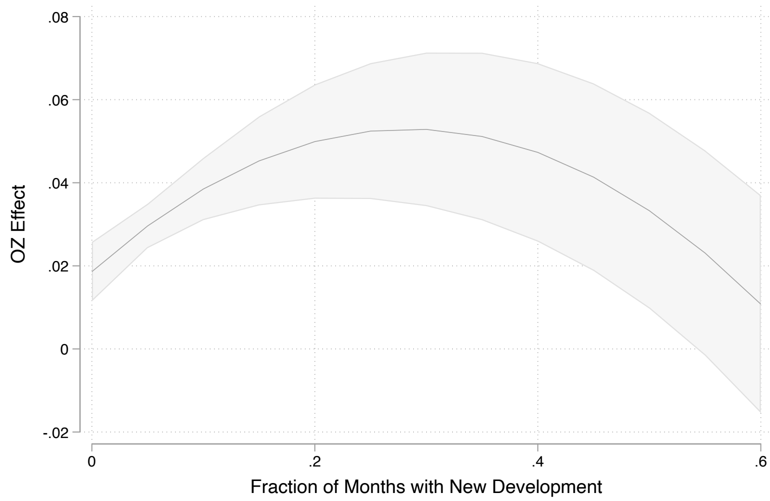

Note: This chart shows the distribution of point estimates from a series of placebo OZ programs. To implement, I simulate 100 different OZ programs by randomly drawing OZs from the population of eligible tracts (with probability equal to the fraction of eligible tracts that were actually chosen as OZs). The main difference-in-differences specification is then run on these "placebo" OZs. Box-whisker plots are plotted for the distribution of regression coefficients. Boxes are bounded by the lower and upper quartile. Whiskers are set so that 95% of the point estimates lie within them.

Figure B.14: Intensive margins of response to OZ program



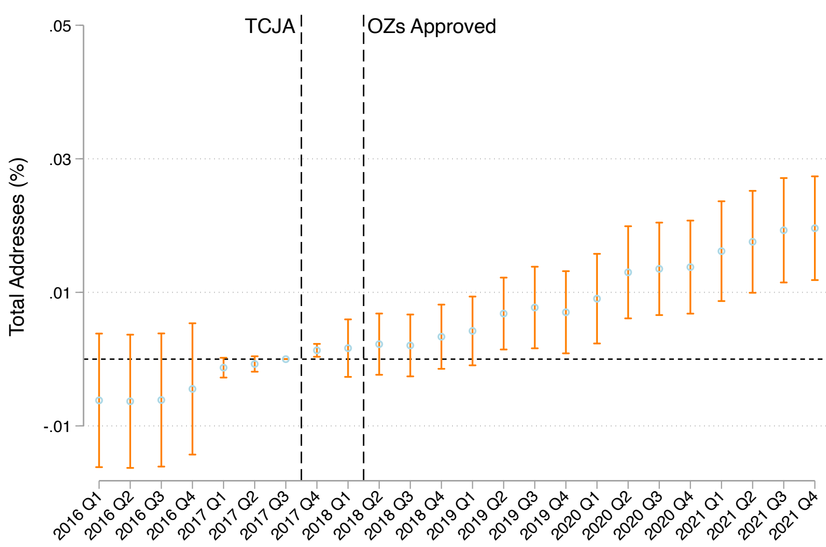

Note: These figures contain difference-in-differences estimates on various outcomes. The left hand chart runs these regressions on the full sample. The right hand chart conditions the sample to observations in which the outcome is greater than zero. All outcomes are transformed using the inverse hyperbolic sine function.

Figure B.15: Heterogeneous policy response in prior development



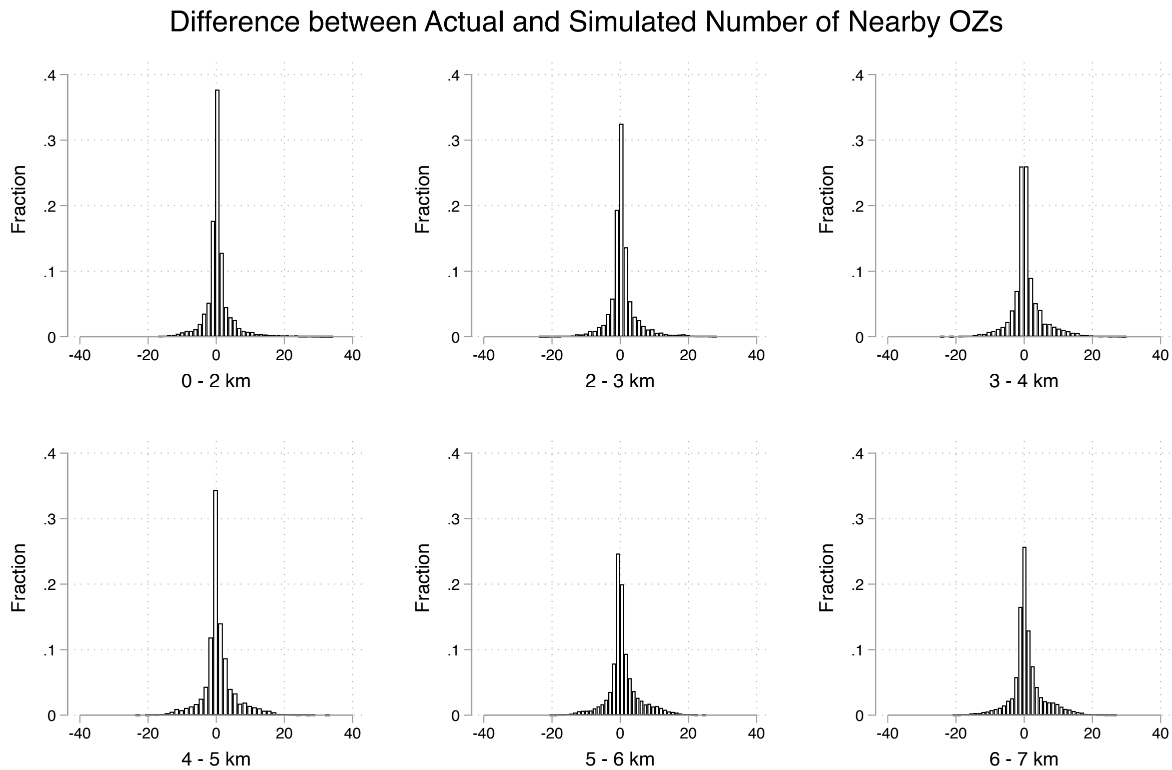

Note: This chart plots treatment effects from the regression model of Table 2.5. Errors are clustered at tract-level.

Figure B.16: Difference-in-differences with addresses



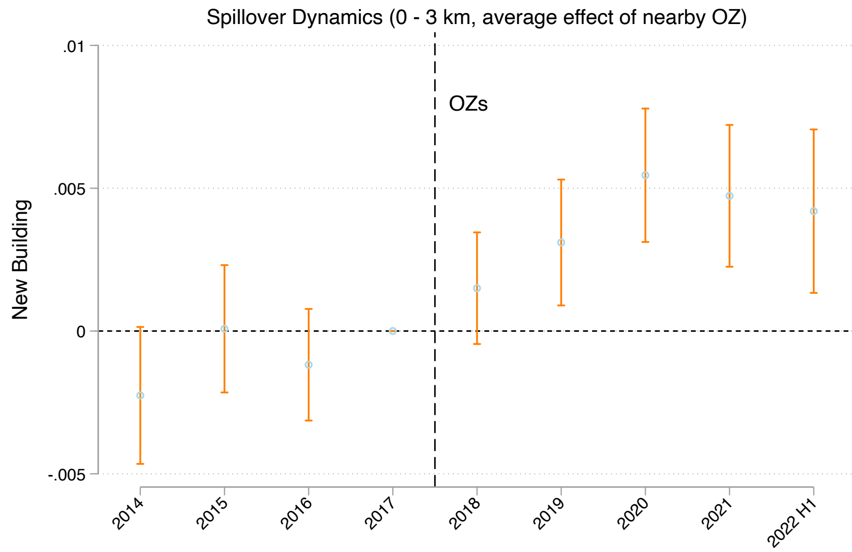

Note: This chart shows difference-in-differences coefficients from a poisson pseudo-maximum likelihood estimator. The outcome is total addresses that appear in a tract in a given quarter. Tract fixed effects, eligibility by month fixed effects, and city trends are included. All errors are clustered at tract-level.

Figure B.17: Distribution of $N_i^k - \hat{\mu}_i^k$ for distance bands $k$



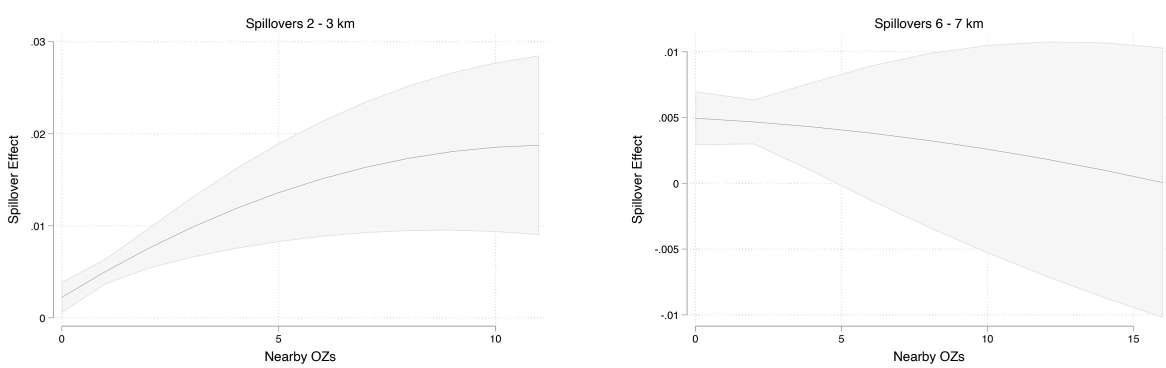

Note: This chart plots the distribution of differences between the actual number of OZs and the expected number of OZs across tracts and for different distance bands. The expected number of OZs is calculated by simulating OZ status among eligible tracts in a city according to the city-specific empirical fraction of OZs. Each plot corresponds to a different one kilometer distance band.

Figure B.18: Spillover dynamics



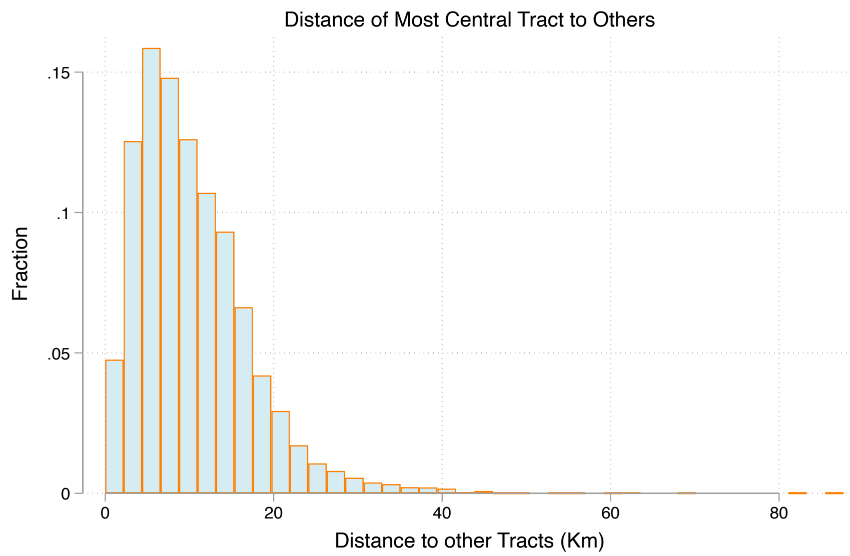

Note: This chart plots difference-in-difference coefficients from the main spillovers specification. The exposure to nearby OZs at various distances is interacted with year. I then scale (according to the average number of nearby OZs) and combine the coefficients for the distance bands 0-2 km and 2-3 km. These are the distances where I detect a positive spillover effect. Thus, the coefficient can be interpretted as the average effect of an additional OZ 0-3 km away, relative to 2017. All errors are clustered at the tract-level.

Figure B.19: Non-linearity in spillovers



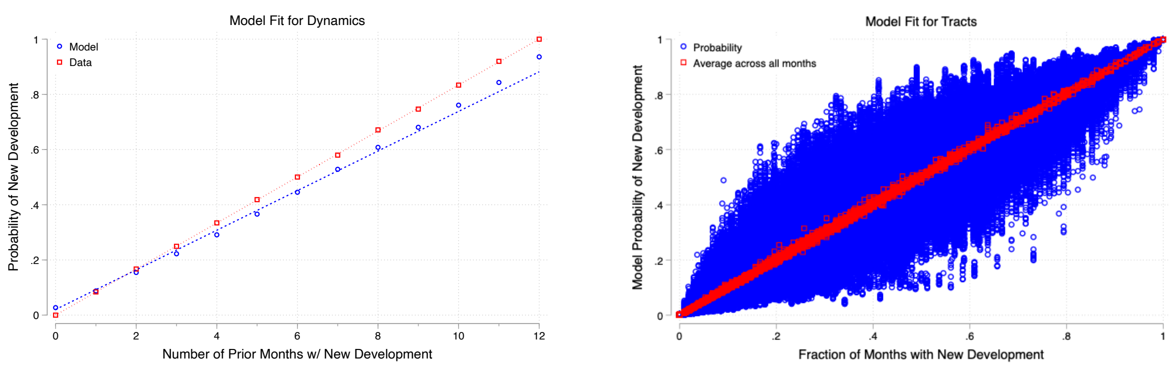

Note: This chart plots quadratic effects of having nearby OZs at 0-2 km (left) and 6-7 km (right). The main spillovers specification is augmented with a linear and quadratic term in the number of nearby OZs at various distances. Following Borusyak and Hull (2022), I control for the expected number of nearby OZs (according to the propensity score model), and its square, interacted with year. The quadratic effects are evaluated at the mean number of OZs at other distances. All errors are clustered at the tract-level.

Figure B.20: Distribution of tract-tract distances




Note: This chart plots the distribution of distances from the centroid of the most central tract to all other tracts within the city.

Figure B.21: Model fit

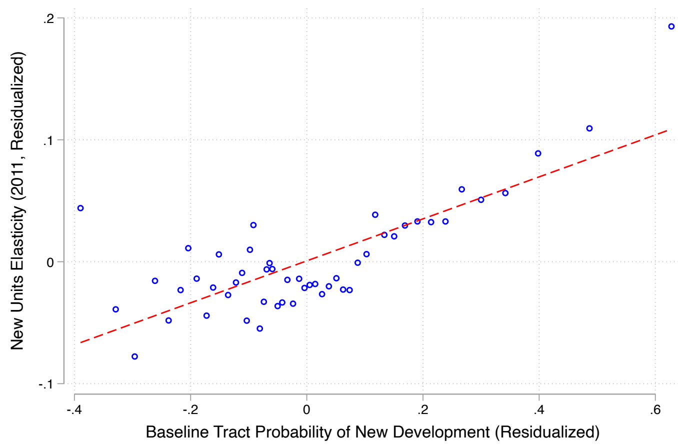



(a) Dynamics

(b) Heterogeneity

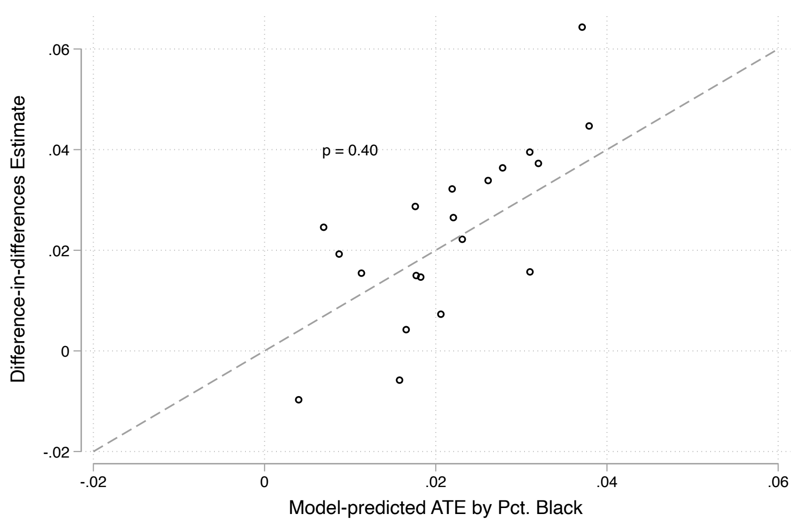
Note: This figure assesses the fit of the model to the data. Panel (a) plots the fraction of all months with new development (data, in red) and the model's estimated equilibrium probability of new development (model, in blue) against the number of prior months with new development. These probabilities are aggregated across months and tracts. Panel (b) plots the fraction of all months with new development against the model's estimated equilibrium probability of new development. Blue points are the actual model probabilities. Red points indicate the average across all months.

Figure B.22: Model comparison with Baum-Snow and Han (2022)




Note: This table compares housing supply elasiticity estimates from Baum-Snow and Han (2022) with baseline estimates of a tract's propensity to develop, calculated as the logit function applied to the model estimates of tract-heterogeneity. I use the elasticity with respect to new units, estimated via their "linear, IV" specification. Both are residualized on city fixed effects. I then plot a line of best fit.

Figure B.23: Model-predicted effects versus design-based effects

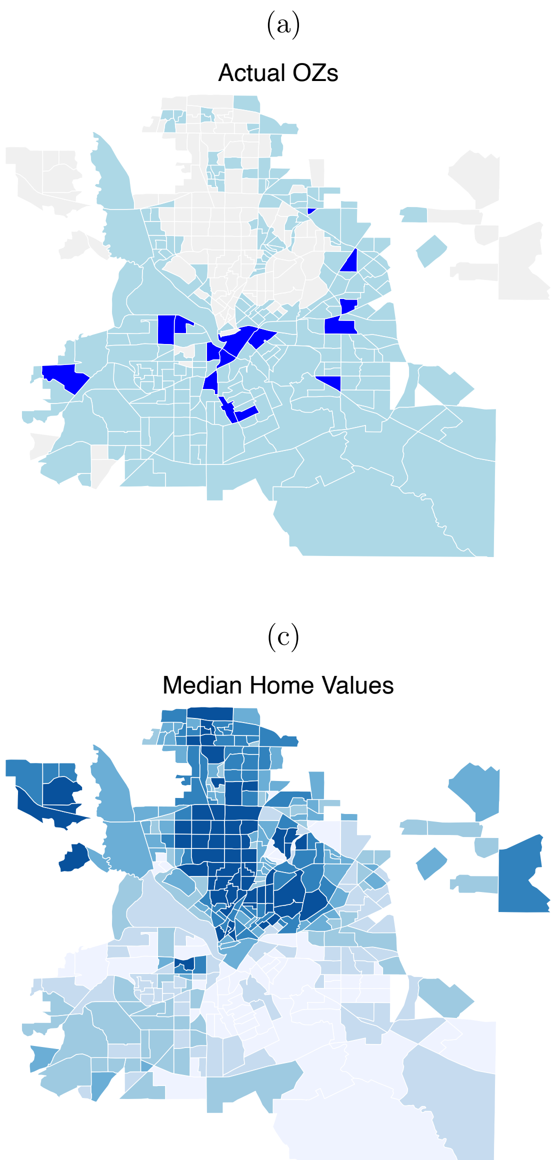



Note: This figure compares model-based estimates of the OZ effect by black population share vingtile with those from an interacted difference-in-differences model. The dashed line corresponds to the 45 degree line. The p-value comes from a test of the hypothesis that the difference-in-differences estimates are equal to the model-based estimates up to sampling error. Tracts with missing data on the black population share are omitted. The sample covers 2015 through 2022.

Figure B.24: Columbus: actual, worst, and optimal OZs


Note: these maps different OZ policies for census tracts in Columbus, Ohio. In the top left are the actual OZs. In the top right are the worst OZs. The bottom left shows 2015 median home values by neighborhood. The bottom right depicts the optimal OZs. For the policy maps, ineligible neighborhood are in light gray, eligible neighborhoods are in light blue, and OZs are in dark blue.

Figure B.25: Dallas: actual, worst, and optimal OZs




Note: these maps different OZ policies for census tracts in Dallas, Texas. In the top left are the actual OZs. In the top right are the worst OZs. The bottom left shows 2015 median home values by neighborhood. The bottom right depicts the optimal OZs. For the policy maps, ineligible neighborhood are in light gray, eligible neighborhoods are in light blue, and OZs are in dark blue.

## B.2 Additional Tables

Table B.1: Summary statistics for sample cities

| City | Time Period | # Months | # Tracts | # OZs | Tract-Months w/ New Construction |
|---|---|---|---|---|---|
| Albuquerque, NM | Jan 2014 - Jun 2022 | 102 | 141 | 14 | 15.78% |
| Arlington, VA | Feb 2015 - Jun 2022 | 89 | 74 | 4 | 17.69% |
| Atlanta, GA | Jan 2014 - Jun 2022 | 102 | 153 | 28 | 24.30% |
| Aurora, CO | Jan 2014 - Jun 2022 | 102 | 101 | 5 | 12.93% |
| Austin, TX | Jan 2014 - Jun 2022 | 102 | 227 | 21 | 28.97% |
| Baltimore, MD | Jan 2014 - Oct 2021 | 94 | 231 | 13 | 14.29% |
| Baton Rouge (East), LA | Jan 2014 - Jun 2022 | 102 | 109 | 25 | 24.41% |
| Boston, MA | Jan 2014 - Jun 2022 | 102 | 196 | 15 | 7.39% |
| Charlotte, NC | Jan 2014 - Jun 2022 | 102 | 255 | 17 | 39.74% |
| Chattanooga, TN | Jan 2014 - May 2020 | 77 | 70 | 8 | 27.38% |
| Chicago, IL | Jan 2014 - Jun 2022 | 102 | 813 | 138 | 7.91% |
| Cincinnati, OH | Jan 2014 - Jun 2022 | 102 | 135 | 26 | 8.71% |
| Columbus, OH | Jan 2014 - Jun 2022 | 102 | 259 | 42 | 11.02% |
| Dallas, TX | Jan 2014 - Jun 2022 | 102 | 374 | 15 | 20.04% |
| Detroit, MI | Jan 2014 - Jun 2022 | 102 | 303 | 71 | 0.99% |
| District of Columbia | Jan 2014 - Jun 2022 | 102 | 187 | 28 | 15.11% |
| Durham, NC | Jan 2014 - Jun 2022 | 102 | 70 | 7 | 37.37% |
| Fort Worth, TX | Jan 2014 - Jun 2022 | 102 | 181 | 6 | 31.38% |
| Greensboro, NC | Jan 2014 - Jun 2022 | 102 | 86 | 10 | 20.31% |
| Henderson, NV | Jan 2016 - Jun 2022 | 78 | 73 | 4 | 17.14% |
| Honolulu, HI | Jan 2014 - Mar 2022 | 99 | 237 | 13 | 12.94% |
| Houston, TX | Jan 2014 - Jun 2022 | 102 | 549 | 98 | 25.43% |
| Indianapolis, IN | Jan 2014 - Nov 2020 | 83 | 226 | 36 | 15.51% |
| Little Rock, AR | Jan 2016 - Jun 2022 | 78 | 61 | 4 | 20.34% |
| Los Angeles, CA | Jan 2014 - Jun 2022 | 102 | 1027 | 193 | 16.67% |
| Mesa, AZ | Jan 2014 - Jun 2022 | 102 | 135 | 11 | 29.80% |
| Minneapolis, MN | Dec 2016 - Jun 2022 | 67 | 118 | 19 | 11.50% |
| Nashville, TN | Dec 2016 - Jun 2022 | 67 | 160 | 18 | 42.38% |
| New Orleans, LA | Jan 2014 - Jun 2022 | 102 | 180 | 25 | 22.88% |
| New York City, NY | Jan 2014 - Jun 2022 | 102 | 2167 | 306 | 4.49% |
| Norfolk, VA | Jul 2016 - Jun 2022 | 72 | 80 | 16 | 20.14% |
| Orlando, FL | Jan 2014 - Jun 2022 | 102 | 111 | 17 | 13.07% |
| Philadelphia, PA | Jan 2014 - Jun 2022 | 102 | 406 | 82 | 11.35% |
| Phoenix, AZ | Jan 2014 - Jun 2022 | 102 | 381 | 46 | 16.29% |
| Raleigh, NC | Jan 2014 - Jun 2022 | 102 | 112 | 11 | 28.93% |
| Sacramento, CA | Jan 2014 - Jun 2022 | 102 | 291 | 37 | 5.45% |
| San Antonio, TX | Jan 2014 - Mar 2020 | 75 | 338 | 23 | 16.32% |
| San Francisco, CA | Jan 2014 - Jun 2022 | 102 | 200 | 12 | 4.98% |
| San Jose, CA | Jan 2014 - Jun 2022 | 102 | 214 | 11 | 3.22% |
| Scottsdale, AZ | Jan 2014 - Jun 2022 | 102 | 68 | 3 | 12.11% |
| Seattle, WA | Jan 2014 - Jun 2022 | 102 | 137 | 10 | 39.90% |
| St. Louis, MO | Jan 2014 - Jun 2022 | 102 | 102 | 26 | 10.99% |
| St. Paul, MN | Jan 2015 - Jun 2022 | 90 | 83 | 18 | 18.57% |
| Tacoma, WA | Jan 2014 - Jun 2022 | 102 | 57 | 6 | 20.09% |
| Tampa, FL | Jan 2014 - Oct 2020 | 82 | 149 | 30 | 15.54% |
| Tucson, AZ | Jan 2014 - Jun 2022 | 102 | 213 | 27 | 11.05% |
| Virginia Beach, VA | Jan 2016 - Jul 2020 | 55 | 93 | 7 | 21.00% |
| Average | | 95.1 | 253.9 | 34.1 | 18.17% |

Note: This table contains summary information for each city in my sample. Column 1 contains the 47 cities in my sample. Column 2 contains the time period for my main sample. Column 3 and 4 contain the number of months and tracts that appear for that city. Column 5 counts the number of OZs in the city and Column 6 contains the fraction of tract-months that have issued permits for new building construction. Data sources for each city are contained in Table B.4.

Table B.2: OZ descriptives for all census tracts

| | (1) All Tracts | (2) Eligible, Not Chosen | (3) OZ Tracts | (4) Diff (2-3) | (5) p-val |
|---|---|---|---|---|---|
| Population | 4,400 | 4,066 | 4,033 | -33 | 0.19 |
| | (2,083) | (1,819) | (1,901) | | |
| Rural | 0.17 | 0.19 | 0.24 | 0.05 | 0.00 |
| | (0.37) | (0.39) | (0.43) | | |
| Median Age | 39.1 | 36.1 | 35.2 | -0.9 | 0.00 |
| | (7.5) | (7.3) | (7.2) | | |
| % White | 0.74 | 0.63 | 0.58 | -0.05 | 0.00 |
| | (0.25) | (0.28) | (0.30) | | |
| % Black | 0.13 | 0.21 | 0.27 | 0.06 | 0.00 |
| | (0.22) | (0.27) | (0.30) | | |
| % Foreign | 0.06 | 0.09 | 0.09 | 0.00 | 0.00 |
| | (0.08) | (0.10) | (0.10) | | |
| % High School | 0.86 | 0.79 | 0.77 | -0.02 | 0.00 |
| | (0.11) | (0.12) | (0.12) | | |
| % College | 0.29 | 0.18 | 0.16 | -0.02 | 0.00 |
| | (0.19) | (0.13) | (0.11) | | |
| Median Family Income | 69,156 | 45,487 | 40,492 | -4995 | 0.00 |
| | (33,613) | (14,502) | (14,084) | | |
| % Poverty Rate | 0.16 | 0.25 | 0.29 | 0.04 | 0.00 |
| | (0.12) | (0.11) | (0.12) | | |
| Median Home Value (1000s) | 225 | 157 | 141 | -16 | 0.00 |
| | (196) | (129) | (117) | | |
| Household Gini | 0.42 | 0.44 | 0.45 | 0.01 | 0.00 |
| | (0.06) | (0.06) | (0.06) | | |
| N | 70,697 | 22,478 | 7,233 | | |

Note: This table provides a comparison of demographics for all U.S. census tracts across tract types relevant for the OZ program. Column (1) contains average demographics for the entire U.S. Column (2) and (3) contain the same information for tracts that were eligible for OZ designation, but not chosen, and OZs, respectively. Column (4) is the difference between Columns (2) and (3), and Column (5) is the *p*-value on a test of whether the difference is zero. Demographics are from the 2011-2015 5-year ACS.

Table B.3: Dates that OZs were officially approved by state

| State | OZ Approval Date |
|---|---|
| Alaska | May 18, 2018 |
| Alabama | April 18, 2018 |
| Arkansas | May 18, 2018 |
| American Samoa | April 9, 2018 |
| Arizona | April 9, 2018 |
| California | April 9, 2018 |
| Colorado | April 9, 2018 |
| Connecticut | May 18, 2018 |
| District Of Columbia | May 18, 2018 |
| Delaware | April 18, 2018 |
| Florida | June 14, 2018 |
| Georgia | April 9, 2018 |
| Guam | May 18, 2018 |
| Hawaii | May 16, 2018 |
| Iowa | May 17, 2018 |
| Idaho | April 9, 2018 |
| Illinois | May 18, 2018 |
| Indiana | May 17, 2018 |
| Kansas | May 17, 2018 |
| Kentucky | April 9, 2018 |
| Louisiana | May 16, 2018 |
| Massachusetts | May 18, 2018 |
| Maryland | May 18, 2018 |
| Maine | May 17, 2018 |
| Michigan | April 9, 2018 |
| Minnesota | May 18, 2018 |
| Missouri | April 18, 2018 |
| Mississippi | April 9, 2018 |
| Montana | May 18, 2018 |
| North Carolina | May 18, 2018 |
| North Dakota | May 18, 2018 |
| Nebraska | April 9, 2018 |
| New Hampshire | May 18, 2018 |
| New Jersey | April 9, 2018 |
| New Mexico | May 18, 2018 |
| Nevada | June 14, 2018 |
| New York | May 18, 2018 |
| Ohio | April 18, 2018 |
| Oklahoma | April 9, 2018 |
| Oregon | May 18, 2018 |
| Pennsylvania | June 14, 2018 |
| Puerto Rico | April 9, 2018 |
| Rhode Island | May 18, 2018 |
| South Carolina | April 9, 2018 |
| South Dakota | April 9, 2018 |
| Tennessee | May 18, 2018 |
| Texas | April 18, 2018 |
| Utah | June 14, 2018 |
| Virginia | May 18, 2018 |
| Virgin Islands | April 9, 2018 |
| Vermont | April 9, 2018 |
| Washington | May 18, 2018 |
| Wisconsin | April 9, 2018 |
| West Virginia | May 18, 2018 |
| Wyoming | May 18, 2018 |

Table B.4: Sources of permit data

| City | Time Coverage | Source |
|---|---|---|
| Albuquerque, NM | Jan 2009 - Jun 2022 | http://data.cabq.gov/business/buildingpermits/ |
| Arlington, VA | Apr 2015 - Jun 2020 | https://data.arlingtonva.us/home |
| Atlanta, GA | Jan 2010 - Jun 2022 | FreedomofInformationRequest |
| Aurora, CO | Jan 1998 - Jun 2022 | https://hub.arcgis.com |
| Austin, TX | Jan 1981 - Jun 2022 | https://data.austintexas.gov/ |
| Baltimore, MD | Jan 1998 - Jun 2022 | https://hub.arcgis.com |
| Baton Rouge (East), LA | Mar 2012 - Jun 2022 | https://data.brla.gov/ |
| Boston, MA | Dec 2009 - Jun 2022 | https://data.boston.gov/ |
| Charlotte, NC | Jan 2010 - Jun 2022 | https://www.mecknc.gov/ |
| Chattanooga, TN | Dec 2008 - May 2020 | https://internal.chattadata.org/Economy/All-Permit-Data/v7br-pci3 |
| Chicago, IL | Jan 2006 - Jun 2022 | https://data.cityofchicago.org/ |
| Cincinnati, OH | Jan 2010 - Jun 2022 | https://data.cincinnati-oh.gov/ |
| Columbus, OH | Jan 2010 - Jun 2022 | http://data-columbus.opendata.arcgis.com/ |
| Dallas, TX | Jan 2000 - Jun 2022 | https://dallascityhall.com/Pages/default.aspx |
| District of Columbia | Jan 2009 - Jun 2022 | https://opendata.dc.gov/ |
| Detroit, MI | May 2010 - Jun 2022 | https://data.detroitmi.gov/ |
| Durham, NC | Nov 2007 - Jun 2022 | https://hub.arcgis.com |
| Fort Worth, TX | Jan 2002 - Jun 2022 | https://mapit.fortworthtexas.gov/#Downloads |
| Greensboro, NC | Mar 1998 - Jun 2022 | https://data.greensboro-nc.gov/ |
| Henderson, NV | Jan 2016 - Jun 2022 | FreedomofInformationRequest |
| Honolulu, HI | Jan 2005 - Jun 2022 | https://data.honolulu.gov/ |
| Houston, TX | Jan 2011 - Jun 2022 | FreedomofInformationRequest |
| Indianapolis, IN | Jan 1997 - Nov 2020 | FreedomofInformationRequest |
| Little Rock, AR | Jan 2016 - Jun 2022 | https://data.littlerock.gov/ |
| Los Angeles, CA | Jan 2013 - Jun 2022 | https://data.lacity.org/ |
| Mesa, AZ | Jan 2004 - Jun 2022 | https://data.mesaaz.gov/ |
| Minneapolis, MN | | https://opendata.minneapolismn.gov/ |
| Nashville, TN | Nov 2016 - Jun 2022 | https://data.nashville.gov/ |
| New Orleans, LA | Jan 2012 - Jun 2022 | https://datadriven.nola.gov/home/ |
| New York, NY | Jan 1990 - Jun 2022 | https://opendata.cityofnewyork.us/ |
| Norfolk, VA | Jul 2016 - Jun 2022 | https://data.norfolk.gov/ |
| Orlando, FL | Jul 1997 - Jun 2022 | https://data.cityoforlando.net/ |
| Philadelphia, PA | Jan 2007 - Jun 2022 | https://data.phila.gov/visualizations/li-building-permits |
| Phoenix, AZ | Jan 1997 - Jun 2022 (no 2002) | https://apps-secure.phoenix.gov/PDD/Search/IssuedPermit |
| Raleigh, NC | Apr 2000 - Jun 2022 | https://data-ral.opendata.arcgis.com/ |
| Sacramento, CA | Jan 2007 - Jun 2022 | https://hub.arcgis.com |
| San Antonio, TX | May 2003 - Mar 2020 | FreedomofInformationRequest |
| San Francisco, CA | Dec 1981 - Jun 2022 | https://datasf.org/opendata/ |
| San Jose, CA | Jan 2005 - Jun 2022 | https://sjpermits.org/permits/general/reportdata.asp |
| Scotsdale, AZ | Oct 2016 - Jun 2022 | https://eservices.scottsdaleaz.gov/bldgresources/BuildingPermit/reports# |
| Seattle, WA | Aug 2005 - Jun 2022 | https://data.seattle.gov/ |
| St. Louis, MO | Sep 1991 - Jun 2022 | https://www.stlouis-mo.gov/ |
| St. Paul, MN | Jan 2015 - Jun 2022 | https://information.stpaul.gov/ |
| Tacoma, WA | Jan 2015 - Jun 2022 | https://wspdsmap.cityoftacoma.org/website/PDS/permits/ |
| Tampa, FL | Jan 2010 - Jun 2022 | http://www.civicdata.com/dataset/tampa_permit_standard_permits_v11_11419 |
| Tucson, AZ | Mar 1997 - Jun 2022 | http://gisdata.tucsonaz.gov/datasets/permits-planning-and-development-services-open-data |
| Virginia Beach, VA | Jan 2016 - Jul 2020 | https://data.vbgov.com/ |

Table B.5: OZ effect using developer-level variation

| | (1)<br>New Projects | (2)<br>New Projects | (3)<br>New Projects |
|---|---|---|---|
| **T x Post** | 0.0125*** | 0.0147*** | 0.00217 |
| | (0.00163) | (0.00165) | (0.00196) |
| **Observations** | 1,494,392 | 1,494,392 | 1,494,392 |
| $R^2$ | 0.537 | 0.533 | 0.538 |
| **Developers / Contractors** | 11550 | 11550 | 11550 |
| **Dep. Var. Mean** | .018 | .019 | .026 |
| **ID x Tract Type** | ✓ | ✓ | ✓ |
| **ID x Date** | ✓ | ✓ | ✓ |
| **Treated Group** | QOZs | QOZs | Eligibles |
| **Control Group** | Eligibles | Ineligibles | Ineligibles |

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: This table contains regression results from a difference-in-differences specification using *within* developer / contractor variation. The dataset contains the number of new development projects in a month by tract type for a developer / contractor. Tract types are tracts that were ineligible or eligible but not chosen for OZ designation, as well as OZs. Details of the dataset construction are contained in Section B.4. The regression includes developer ID by tract type and developer ID by date fixed effects. The coefficient of interest is "treatment" status interacted with the time period being after OZs were announced. Columns (1) and (2) use OZs as the treatment group, and eligible and ineligible tracts respectively as the control group. Column (3) uses eligible tracts as the treatment group, and ineligible tracts as the control group. For better measuring when developers are actually active, I focus on January 2017 to June 2022 and restrict the sample to developers with at least two new development projects since 2014. Some cities without developer / contractor information were excluded. All errors are clustered at the developer-level.

Table B.6: Robustness to trends

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | New Building | New Building | New Building | New Building |
| **OZ and 2014** | -0.00672 | -0.00701 | -0.00442 | -0.00342 |
| | (0.00445) | (0.00447) | (0.00450) | (0.00457) |
| **OZ and 2015** | -0.000642 | -0.000360 | 0.000880 | 0.000721 |
| | (0.00415) | (0.00416) | (0.00420) | (0.00425) |
| **OZ and 2016** | 0.00260 | 0.00275 | 0.00387 | 0.00370 |
| | (0.00369) | (0.00370) | (0.00373) | (0.00380) |
| **OZ and 2018 pre-OZ** | 0.00714 | 0.00721 | 0.00594 | 0.00490 |
| | (0.00510) | (0.00511) | (0.00514) | (0.00521) |
| **OZ and 2018 post-OZ** | 0.0216*** | 0.0216*** | 0.0204*** | 0.0193*** |
| | (0.00440) | (0.00441) | (0.00444) | (0.00452) |
| **OZ and 2019** | 0.0263*** | 0.0260*** | 0.0238*** | 0.0208*** |
| | (0.00438) | (0.00439) | (0.00442) | (0.00454) |
| **OZ and 2020** | 0.0247*** | 0.0234*** | 0.0200*** | 0.0184*** |
| | (0.00452) | (0.00453) | (0.00455) | (0.00464) |
| **OZ and 2021** | 0.0393*** | 0.0380*** | 0.0356*** | 0.0306*** |
| | (0.00507) | (0.00508) | (0.00509) | (0.00520) |
| **OZ and 2022 H1** | 0.0347*** | 0.0342*** | 0.0311*** | 0.0260*** |
| | (0.00582) | (0.00583) | (0.00585) | (0.00600) |
| **Observations** | 1,175,040 | 1,175,040 | 1,175,040 | 1,175,040 |
| $R^2$ | 0.305 | 0.305 | 0.305 | 0.305 |
| **Dep. Var. Mean** | .1441 | .1441 | .1441 | .1441 |
| **Tract FE** | ✓ | ✓ | ✓ | ✓ |
| **Elig. x Month FE** | ✓ | ✓ | ✓ | ✓ |
| **City x Season FE** | ✓ | ✓ | ✓ | ✓ |
| **City Linear Trend** | ✓ | ✓ | ✓ | ✓ |
| **More Trends** | | Home Val. | Median Inc. | Pov. Rate |

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: This table contains linear regression models including tract and eligibility by month fixed effects, as well as city seasonal effects and city linear trends. The outcome variable is an indicator for whether a tract had a permit issued for the construction of a new building in a given month. Column (1) shows the baseline specification, while all others add an additional set of trends. Column (2) include tract-level median home value by year fixed effects, and column (3) and (4) do similarly with median family income and the poverty rate. All tract-level covariates come from the 2011-2015 5-year ACS. Tracts with missing values for home values, median family income, or poverty rates are maintained in the sample; having a missing value by year fixed effects are included to control for differential behaviour of these tracts. All specifications are estimated on monthly data from January 2014 to June 2022. The sample include 11,936 total tracts, of which 7,801 were eligible for OZ designation and 1,602 were chosen as OZs. All errors are clustered at tract-level.

Table B.7: Difference-in-difference at eligibility cutoffs

| | (1) New Building | (2) New Building | (3) New Building | (4) New Building |
|---|---|---|---|---|
| **OZ and 2014** | -0.0251*** | -0.0129* | -0.00763 | 0.00328 |
| | (0.00510) | (0.00744) | (0.00883) | (0.0105) |
| **OZ and 2015** | -0.0182*** | -0.0119* | -0.00979 | 0.000556 |
| | (0.00476) | (0.00699) | (0.00837) | (0.0101) |
| **OZ and 2016** | -0.00857** | -0.00290 | 7.16e-05 | 0.0117 |
| | (0.00409) | (0.00608) | (0.00729) | (0.00903) |
| **OZ and 2018 pre-OZ** | 0.0177*** | 0.0219*** | 0.0297*** | 0.0315** |
| | (0.00556) | (0.00840) | (0.0102) | (0.0126) |
| **OZ and 2018 post-OZ** | 0.0293*** | 0.0352*** | 0.0388*** | 0.0427*** |
| | (0.00485) | (0.00735) | (0.00869) | (0.0106) |
| **OZ and 2019** | 0.0417*** | 0.0349*** | 0.0289*** | 0.0393*** |
| | (0.00490) | (0.00729) | (0.00839) | (0.0104) |
| **OZ and 2020** | 0.0521*** | 0.0365*** | 0.0293*** | 0.0372*** |
| | (0.00520) | (0.00765) | (0.00879) | (0.0107) |
| **OZ and 2021** | 0.0675*** | 0.0457*** | 0.0366*** | 0.0410*** |
| | (0.00572) | (0.00831) | (0.00962) | (0.0117) |
| **OZ and 2022 H1** | 0.0618*** | 0.0453*** | 0.0385*** | 0.0410*** |
| | (0.00659) | (0.00951) | (0.0113) | (0.0137) |
| **Observations** | 563,848 | 244,493 | 161,501 | 106,492 |
| $R^2$ | 0.335 | 0.309 | 0.304 | 0.291 |
| **OZs** | 1,602 | 804 | 601 | 442 |
| **Inelig.** | 4,135 | 1,678 | 1,037 | 636 |
| **Tract FE** | ✓ | ✓ | ✓ | ✓ |
| **Month FE** | ✓ | ✓ | ✓ | ✓ |
| **City x Season FE** | ✓ | ✓ | ✓ | ✓ |
| **City Linear Trend** | ✓ | ✓ | ✓ | ✓ |
| **Pov. Rate BW pct.** | $[-\infty,\infty]$ | [-10,10] | [-7,7] | [-5,5] |
| **MFI BW 1000s** | $[-\infty,\infty]$ | [-20,20] | [-15,15] | [-10,10] |

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: This table contains linear regression models including tract and month fixed effects, as well as city seasonal effects and city linear trends. The sample consists of OZs and tracts that are ineligible for the program, within a certain bandwidth of the eligibility cutoffs for tract poverty rate and median family income. Column (3) is the approximate bandwidth preferred by Calonico and Titiunik (2014). All specifications are estimated on monthly data from January 2014 to June 2022. All errors are clustered at tract-level.

Table B.8: Margins of development

| | (1) asinh(New Bldgs) | (2) asinh(New Res. Bldgs) | (3) asinh(New Comm. Bldgs) | (4) asinh(New Sq. Ft.) | (5) asinh(New Val.) | (6) asinh(New Units) |
|---|---|---|---|---|---|---|
| **OZ and 2014** | -0.00957 | -0.0120 | -0.000410 | -0.101 | -0.0397 | -0.0236 |
| | (0.00826) | (0.00784) | (0.00305) | (0.0684) | (0.0796) | (0.0189) |
| **OZ and 2015** | 0.000693 | -0.00169 | 0.000731 | -0.0144 | 0.00473 | 0.00694 |
| | (0.00737) | (0.00702) | (0.00266) | (0.0623) | (0.0723) | (0.0170) |
| **OZ and 2016** | 0.00463 | 0.00117 | 0.000629 | 0.00435 | 0.00881 | 0.00380 |
| | (0.00634) | (0.00598) | (0.00268) | (0.0544) | (0.0634) | (0.0148) |
| **OZ and 2018 pre-OZ** | 0.00749 | 0.00593 | 0.000508 | 0.0270 | 0.0853 | 0.0146 |
| | (0.00804) | (0.00747) | (0.00347) | (0.0757) | (0.0886) | (0.0195) |
| **OZ and 2018 post-OZ** | 0.0317*** | 0.0234*** | 0.00916*** | 0.289*** | 0.329*** | 0.0500*** |
| | (0.00739) | (0.00713) | (0.00289) | (0.0653) | (0.0764) | (0.0166) |
| **OZ and 2019** | 0.0421*** | 0.0340*** | 0.00853*** | 0.254*** | 0.348*** | 0.0734*** |
| | (0.00782) | (0.00763) | (0.00270) | (0.0658) | (0.0755) | (0.0176) |
| **OZ and 2020** | 0.0457*** | 0.0378*** | 0.00836*** | 0.241*** | 0.389*** | 0.0885*** |
| | (0.00867) | (0.00862) | (0.00271) | (0.0691) | (0.0809) | (0.0188) |
| **OZ and 2021** | 0.0625*** | 0.0500*** | 0.0167*** | 0.402*** | 0.587*** | 0.112*** |
| | (0.0100) | (0.00980) | (0.00306) | (0.0800) | (0.0930) | (0.0228) |
| **OZ and 2022 H1** | 0.0601*** | 0.0454*** | 0.0185*** | 0.472*** | 0.679*** | 0.155*** |
| | (0.0114) | (0.0110) | (0.00398) | (0.0870) | (0.104) | (0.0270) |
| **Observations** | 1,174,851 | 1,174,851 | 1,174,851 | 617,340 | 848,197 | 497,613 |
| $R^2$ | 0.417 | 0.429 | 0.179 | 0.356 | 0.325 | 0.338 |
| **Number of Tracts** | 11936 | 11936 | 11936 | 6411 | 8752 | 5317 |
| **Number of Eligibles** | 7801 | 7801 | 7801 | 4003 | 5605 | 3154 |
| **Number of QOZs** | 1602 | 1602 | 1602 | 790 | 1117 | 667 |
| **Tract FE** | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| **Elig. x Month FE** | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| **City x Season** | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| **City Linear Trend** | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: This table shows estimates of the semi-elasticity of several margins of new development with respect to OZ status. These margins are new buildings (and whether they are for residential or commercial / mixed-use purposes), as well as the square feet, estimated construction costs, and units associated with these projects. Since the transformation used is inverse hyperbolic sine, zeroes are maintained in the sample. The coefficients can be interpretted as a semi-elasticity that mix intensive and extensive responses. All specifications are estimated on monthly data from January 2014 to June 2022. All errors are clustered at tract-level.

Table B.9: Heterogeneity in OZ effect

| | (1) New Building | (2) New Building | (3) New Building | (4) New Building | (5) New Building | (6) New Building | (7) New Building |
|---|---|---|---|---|---|---|---|
| **T x Developable Land Shr. (Low)** | 0.0750*** | | | | | | 0.000979 |
| | (0.0145) | | | | | | (0.0242) |
| **T x Elasticity (New Units)** | | 0.0758*** | | | | | 0.0393* |
| | | (0.0116) | | | | | (0.0210) |
| **T x Log Home Value** | | | -0.0227*** | | | | -0.0168*** |
| | | | (0.00395) | | | | (0.00486) |
| **T x Log MFI** | | | | -0.0140* | | | -0.0192 |
| | | | | (0.00798) | | | (0.0138) |
| **T x College Shr** | | | | | -0.123*** | | -0.0308 |
| | | | | | (0.0281) | | (0.0337) |
| **T x Poverty Shr** | | | | | | 0.0107 | -0.0747* |
| | | | | | | (0.0254) | (0.0401) |
| **Observations** | 1,175,040 | 1,175,040 | 1,175,040 | 1,175,040 | 1,175,040 | 1,175,040 | 1,175,040 |
| $R^2$ | 0.305 | 0.305 | 0.305 | 0.305 | 0.305 | 0.305 | 0.306 |
| **Dep. Var. Mean** | .1441 | .1441 | .1441 | .1441 | .1441 | .1441 | .1441 |
| **Tract FE** | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| **Elig. x Month FE** | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| **City x Season FE** | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| **City Linear Trend** | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| **Post x Covariate** | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: This table shows estimates of the coefficient on OZ status interacted with the following covariates: the 2016 share of land that is either open space or low development (Clarke and Melendez, 2019), 2011 local supply elasticities (Baum-Snow and Han, 2022), and 2011-2015 5-year ACS covariates. The shown estimates are those from interacting the OZ and after OZs were announced indicator with the relevant covariates. Tracts with missing values for the covariates are maintained in the sample; having a missing value by year fixed effects are included to control for differential behaviour of these tracts. All specifications are estimated on monthly data from January 2014 to June 2022. The sample include 11,936 total tracts, of which 7,801 were eligible for OZ designation and 1,602 were chosen as OZs. All errors are clustered at tract-level.

Table B.10: Heterogeneity in OZ effect by zoning covariates

| | (1) New Building | (2) New Building | (3) New Building | (4) New Building | (5) New Building | (6) New Building |
|---|---|---|---|---|---|---|
| **T x Local Political Pressure** | 0.000292 | | | | | -0.000426 |
| | (0.00232) | | | | | (0.00309) |
| **T x Local Zoning Approval** | | -0.0182*** | | | | -0.0220*** |
| | | (0.00432) | | | | (0.00614) |
| **T x Local Project Approval** | | | -0.00382 | | | 0.00910** |
| | | | (0.00248) | | | (0.00401) |
| **T x Density Restrictions** | | | | 0.0259** | | 0.0289** |
| | | | | (0.0119) | | (0.0122) |
| **T x Approval Delay** | | | | | -0.00433*** | -0.00349*** |
| | | | | | (0.000656) | (0.000778) |
| **Observations** | 1,108,024 | 1,108,024 | 1,108,024 | 1,108,024 | 1,108,024 | 1,108,024 |
| $R^2$ | 0.303 | 0.303 | 0.303 | 0.303 | 0.303 | 0.303 |
| **Dep. Var. Mean** | .1385 | .1385 | .1385 | .1385 | .1385 | .1385 |
| **Tract FE** | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| **Elig. x Month FE** | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| **City x Season FE** | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| **City Linear Trend** | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: This table shows estimates of the effect of OZ status by various measures of land use restrictions from the 2006 Wharton Land Use Regulation Survey (Gyourko et al., 2008). The shown regression coefficients are those from interacting the treatment indicator with relevant covariates. All specifications are estimated on monthly data from January 2014 to June 2022. Chattanooga and Scottsdale do not appear in the Wharton zoning data and are omitted from this regression; the remaining sample includes 11,157 total tracts, of which 7,330 were eligible for OZ designation and 1,500 were chosen as OZs. All errors are clustered at tract-level.

Table B.11: Home value and rents

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Log Home Value (Q25) | Log Home Value (Q50) | Log Home Value (Q75) | Log Rent |
| **OZ and 2015** | -0.000199 | -0.00112 | -0.00942* | -0.00199 |
| | (0.00536) | (0.00484) | (0.00546) | (0.00274) |
| **OZ and 2016** | -0.00469 | -0.00127 | 0.000173 | 0.00246 |
| | (0.00809) | (0.00733) | (0.00738) | (0.00423) |
| **OZ and 2018** | 0.0151*** | 0.00651* | 0.0151*** | -0.00239 |
| | (0.00452) | (0.00356) | (0.00394) | (0.00224) |
| **OZ and 2019** | 0.0231*** | 0.0157*** | 0.0200*** | -0.00184 |
| | (0.00706) | (0.00500) | (0.00524) | (0.00311) |
| **OZ and 2020** | 0.0435*** | 0.0338*** | 0.0336*** | 0.00411 |
| | (0.00833) | (0.00631) | (0.00646) | (0.00411) |
| **Observations** | 59,418 | 62,592 | 62,358 | 58,788 |
| $R^2$ | 0.980 | 0.982 | 0.980 | 0.951 |
| **Dep. Var. Mean** | 12.16 | 12.5 | 12.8 | 7.068 |
| **Tract FE** | ✓ | ✓ | ✓ | ✓ |
| **City x Elig. x Month FE** | ✓ | ✓ | ✓ | ✓ |

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: This table tests the response of home values and rents to the tax credit. The outcomes are ACS measures of log home values at the 25th, 50th, and 75th quartiles. Column (4) contains log rents. All errors are clustered at tract-level.

Table B.12: Chen et al. (2023) comparison using log-levels

| | (1)<br>Log (HPI) | (2)<br>Log (Home Val. Q50) | (3)<br>Log (Home Val. Q50) |
|---|---|---|---|
| **OZ and 2020** | 0.0331*** | 0.0338*** | 0.0333*** |
| | (0.00508) | (0.00631) | (0.00891) |
| **Observations** | 10,546 | 20,864 | 10,546 |
| $R^2$ | 0.995 | 0.991 | 0.991 |
| **Dep. Var. Mean** | 5.668 | 12.5 | 12.5 |
| **Tract FE** | ✓ | ✓ | ✓ |
| **City x Elig. x Month FE** | ✓ | ✓ | ✓ |
| **Source** | FHFA | ACS | ACS |
| **Sample** | | | Has HPI |

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: This table tests the response of log home values to the tax credit. The first column uses the FHFA repeat-sales home price index. The second and third columns use the ACS median home values. Column (3) restricts the ACS sample to only those tracts with FHFA data. I restrict the regression to the years 2017 and 2020. All errors are clustered at tract-level.

Table B.13: Chen et al. (2023) comparison using first-differences

| | (1)<br>Δ Log (HPI) | (2)<br>Δ Log (Home Val. Q50) | (3)<br>Δ Log (Home Val. Q50) |
|---|---|---|---|
| **OZ x Post** | -0.000422 | 0.00198 | 0.00152 |
| | (0.00379) | (0.00353) | (0.00480) |
| **Observations** | 14,342 | 35,023 | 14,256 |
| $R^2$ | 0.232 | 0.119 | 0.085 |
| **Dep. Var. Mean** | .06179 | .05649 | .05649 |
| **Tract FE** | ✓ | ✓ | ✓ |
| **Elig. x Month FE** | ✓ | ✓ | ✓ |
| **ACS Covariates x Yr** | ✓ | ✓ | ✓ |
| **Source** | FHFA | ACS | ACS |
| **Sample** | | | Has HPI |

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: This table tests the response of the first-difference in log home values to the tax credit. The first column uses the FHFA repeat-sales home price index. The second and third columns use the ACS median home values. Column (3) restricts the ACS sample to only those tracts with FHFA data. As in Chen et al. (2023), I restrict the sample to eligible tracts, and I include trends in baseline tract covariates. The sample includes years 2016 through 2020. All errors are clustered at tract-level.

Table B.14: Balance table for spillovers analysis

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Δ Log MFI | Δ Log Pop. | Δ Log Home Value | Δ % Poverty | Δ % College | Δ % High School | Δ % White | Δ % Black |
| $N^0$-$\hat{\mu}_i^0$ | 0.000912 | 0.000210 | -9.29e-05 | -0.0126 | 0.000235 | 0.0188 | 0.0431** | -0.0185 |
| | (0.000642) | (0.000375) | (0.000590) | (0.0202) | (0.0181) | (0.0162) | (0.0202) | (0.0154) |
| $N^2$-$\hat{\mu}_i^2$ | 0.000230 | 1.63e-05 | 0.000840 | 0.00550 | 0.0182 | 0.0150 | -0.00756 | -0.00775 |
| | (0.000683) | (0.000396) | (0.000645) | (0.0205) | (0.0171) | (0.0169) | (0.0219) | (0.0160) |
| $N^3$-$\hat{\mu}_i^3$ | -0.000594 | 0.000171 | -0.000338 | -0.0151 | 0.0172 | 0.00776 | 0.00264 | 0.0169 |
| | (0.000622) | (0.000372) | (0.000587) | (0.0189) | (0.0167) | (0.0156) | (0.0206) | (0.0154) |
| $N^4$-$\hat{\mu}_i^4$ | -0.000453 | -0.000500 | 0.000372 | 0.000379 | -0.00286 | 0.0238 | 0.00748 | -0.0182 |
| | (0.000581) | (0.000369) | (0.000508) | (0.0170) | (0.0154) | (0.0161) | (0.0193) | (0.0144) |
| $N^5$-$\hat{\mu}_i^5$ | 0.000404 | 0.000398 | -0.000288 | 0.00728 | -0.00591 | -0.0198 | 0.0415** | -0.00129 |
| | (0.000549) | (0.000312) | (0.000515) | (0.0165) | (0.0144) | (0.0138) | (0.0184) | (0.0137) |
| $N^6$-$\hat{\mu}_i^6$ | 0.000136 | -0.000188 | -0.000353 | -0.00140 | -0.00488 | -0.00142 | -0.00300 | 0.0167 |
| | (0.000445) | (0.000255) | (0.000391) | (0.0129) | (0.0121) | (0.0114) | (0.0154) | (0.0106) |
| **Observations** | 11,430 | 11,641 | 11,041 | 11,641 | 11,640 | 11,640 | 11,641 | 11,641 |
| $R^2$ | 0.026 | 0.027 | 0.170 | 0.035 | 0.013 | 0.023 | 0.033 | 0.012 |
| **City FE** | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| **OZ x Elig. FE** | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: This table shows how changes in tract demographics correlate with the number of nearby OZs net of an estimate for the expected number of nearby OZs. The outcomes are 2015-2017 differences, where the relevant variables come from the 2011-2015 ACS and 2013-2017 ACS. All outcomes have been scaled to be interpretted as percentage points. The covariates are the number of OZs within distance band $k$ minus the expected number of OZs within distance band $k$. The distances are $0-2$ kilometers, $2-3$ kilometers, up to $6-7$ kilometers. The expected number of OZs is calculated through a simulation discussed in the text. Errors are heteroskedasticity robust.

Table B.15: Spillovers heterogeneity

| | (1) New Building | (2) New Building | (3) New Building | (4) New Building | (5) New Building | (6) New Building | (7) New Building | (8) New Building |
|---|---|---|---|---|---|---|---|---|
| **T x QOZ** | -0.00110 | | | | | | | 0.000413 |
| | (0.00797) | | | | | | | (0.00823) |
| **T x Developable Land Shr. (Low)** | | 0.0305*** | | | | | | 0.00575 |
| | | (0.00817) | | | | | | (0.0123) |
| **T x Elasticity (New Units)** | | | 0.0294*** | | | | | -0.00189 |
| | | | (0.00846) | | | | | (0.0132) |
| **T x Log Home Val.** | | | | -0.0182*** | | | | -0.0173*** |
| | | | | (0.00213) | | | | (0.00267) |
| **T x Log MFI** | | | | | -0.0181*** | | | -0.00814 |
| | | | | | (0.00284) | | | (0.00548) |
| **T x College Shr.** | | | | | | -0.00181 | | 0.0390** |
| | | | | | | (0.0112) | | (0.0152) |
| **T x Pov. Rate** | | | | | | | -0.0177 | -0.0635*** |
| | | | | | | | (0.0154) | (0.0222) |
| **Observations** | 1,174,782 | 1,174,782 | 1,174,782 | 1,174,782 | 1,174,782 | 1,174,782 | 1,174,782 | 1,174,782 |
| $R^2$ | 0.306 | 0.306 | 0.306 | 0.306 | 0.306 | 0.306 | 0.306 | 0.306 |
| **Dep. Var. Mean** | .1441 | .1441 | .1441 | .1441 | .1441 | .1441 | .1441 | .1441 |
| **Tract FE** | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| **E[Nearby QOZ] x Year FE x Covariate** | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| **QOZ x Elig. x Month FE** | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| **City x Season FE** | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| **City Linear Trend** | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: This table augments the main spillovers specification by interacting the exposure to OZs and "post" indicator with the following covariates: OZ status, the 2016 share of land that is either open space or low development (Clarke and Melendez, 2019), 2011 local supply elasticities (Baum-Snow and Han, 2022), and 2011-2015 5-year ACS covariates.. The coefficients in the table are these interactions for the 0-2 km distance band. Controls in the expected exposure to nearby OZs are interacted with the ACS covariates as well. Tracts with missing ACS covariates are maintained in the sample, and controls for whether a tract has missing values are included. Errors are clustered at tract-level.

## B.3 Data Construction

### Sample of cities

I searched for building permit data for all U.S. cities with populations of 200,000 or greater. I used a mix of google and a city's open data website. Additionally, I found permits data through `https://hub.arcgis.com`. I also added a few cities through Freedom of Information Act requests. I added a few smaller cities that I readily found building permit data for, and that had at least 50 unique census tracts appear in their permits. I excluded cities whose data prohibited me from either identifying new developments or their location. These sources are summarized in Table B.4.

### Geocoding

Geocoding was performed via two methods. Many cities directly provided coordinates or census tracts. Others had assessor parcel numbers that could be matched to plot centroids

Table B.16: Model fit to reduced-form effects

| | (1) Data | (2) Model | (3) Diff. |
|---|---|---|---|
| **QOZ x Post** | 0.0273*** | 0.0247*** | 0.00256 |
| | (0.00330) | (0.00142) | (0.00238) |
| **Observations** | 1,029,840 | 1,029,840 | 2,059,680 |
| $R^2$ | 0.310 | 0.937 | 0.465 |
| **Tract FE** | ✓ | ✓ | ✓ |
| **Elig. x Month FE** | ✓ | ✓ | ✓ |
| **City x Season FE** | ✓ | ✓ | ✓ |
| **City Linear Trend** | ✓ | ✓ | ✓ |

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: This table reproduces the reduced-form policy effect through the model. Column (1) shows the overall effect of the OZ policy on new development for the sample of observations from my model, using the baseline specification in Section 2.4. Column (2) uses the model equilibrium probabilities as the dependent variable. I then stack both datasets in Columns (1) and (2), and run a fully interacted version of the difference-in-differences model. The coefficient on the difference-in-difference coefficient interacted with the stack is shown in Column (3). These regressions are run on the main sample from 2015-2022. Errors are clustered at tract-level.

through assessor shapefiles. For some cities with sparser geographic information, I complemented these methods with other indirect means to geolocate a permit. For example, if I knew that "100 Main St." and "150 Main St." were located in the same census tracts, I assumed that "125 Main St." was also in the same census tract. For parcel numbers that could not be linked through assessor shapefiles, I would try to assign them the average centroid for an assessor page.[1] The permit coordinates were then linked to 2010 census tracts.

### Identification of new developments

The building permit data usually contains a text description of the permit, and several variables that categorized the type of work being done. For example, Austin, Texas includes a variable `workclass` which identifies "New" structures. Another variable `permitclass` describes the type of structure being built: a new single-family residential home, an apartment building, a commerical building, etc. For some cities in which I had doubts that these characterizations were identifying new buildings, I included additional restrictions. I removed building permits whose value of construction was too small, or whose text description in-

[1] These pages, were in general, very small geographic areas. They could correspond to a residential street that ended in a cul-de-sac.

volved things like additions or renovations. For many cities, I was also able to identify permits for demolitions as well.

I drop permits that were rejected, cancelled, or voided. I use the date of permit approval for the new development. For a small number of permits, if the approval date was missing, I would use the date of submission.

### Final data build

Equipped with new developments and their location, I added in where possible covariates on the estimated cost of construction, the number of units, the square footage, and whether the development was commercial or residential. Not all cities had these covariates. I drop early time periods for which the number of building permits was an order of magnitude lower than it was in later years.[2] I then aggregate my data by summing or averaging these covariates within census tract-month cells. I include all census tracts for which a building permit appears at some point in the database.

### Addresses

In Figure B.3, I regress total tract-level addresses from the USPS Vacancy Data on lags of permits for new buildings as well as tract and quarter fixed effects.[3] I find that each permit for a new building is associated with one additional address a year later and two addresses two years later. These dynamics are consistent with larger construction projects taking longer to complete.[4]

[2] I suspect this occurred as cities rolled out their online building permit platforms.

[3] I compile USPS Vacancy Data collected by the U.S. Department of Housing and Urban Development, providing a count of addresses within each census tract for each quarter from 2012 Q1 to 2021 Q4.

[4] Additionally, the USPS collects information on "no-status" addresses, which can include those under construction but not occupied. In Figure B.4, I find that new construction permits are associated with 0.2 to 0.3 more "no-status" addresses within the first 5 quarters of issuance, but that this effect declines over time to zero (presumably, as the construction is completed and the address is reclassified).

## B.4 Empirics

### New York OZ projects

While OZ projects take many forms, news reports of large funds offer insights into what type of investments were made and how they have been made so quickly. In November 2018, just six months after OZs were approved for New York state, Youngwoo & Associates broke ground on a 22-story office tower and hotel in a Washington Heights OZ (NYREJ, 2018). The developers aquired the site in 2013, but did not begin construction until 2018. Also in November 2018, Goldman Sach's Urban Investment Group provided construction financing for an apartment complex in a Long Island City OZ (NYDB, 2018). Both are emblematic of how some developers were able to respond to the OZ program so quickly; they either (i) pushed idle projects into development, or (ii) provided construction financing for projects. Later OZ developments also consist of projects that were newly created. In May 2019, Starwood Capital's OZ-specific fund announced a new mixed-use project in the Bronx, housing a charter school and commercial space (CPE, 2019).

### Developers dataset and difference-in-differences design

For most cities in my sample, each building permit is associated with a parcel owner. I refer to the owners that engage in the new construction of a residential or commercial building as developers. Some cities may not record the actual owner, but the contractor on the project. Often this will correspond to the owner, but it may refer to an outside construction company hired to complete the work. I standardize the names of these developers and contractors, and create a unique identifier within the city. I drop developers that have missing names, and those that are associated with the construction of more than 100 buildings in the city since 2014.[5] I can identify developers or contractors for 37 of the 46 cities in my sample.[6]

To create a panel of developers and their investment decisions, I need to know when developers are active. I use the first date that the unique developer ID appears on any permit as the moment a developer becomes active. In all periods after in which no permits are observed, I assume the developer is active but has not developed. I then aggregate this data, summing up the number of new projects associated with a developer for a tract type (ineligible, eligible, or OZ) in a given month. To focus on developers who were active prior to the OZ program, I restrict my sample to include those that developed at least twice since 2014, and have had some permit activity before 2017. I then restrict the sample to time periods from 2017 on.

I run the following difference-in-differences specification. Let $n_{igt}$ denote the number of new projects started by developer $i$ in tract type $g$ in month $t$.

[5]The latter I do to avoid names that cities use when they lack information about the specific developer.

[6]I have no developer information for Aurora, Durham, Detroit, Indianapolis, Honolulu, Henderson, Little Rock, Minneapolis,Norfolk, Seattle, San Francisco, Tacoma, Tampa, Tucson, and Virginia Beach.

$$n_{igt} = \beta \times T_i \times \text{Post}_{it} + \alpha_{ig} + \eta_{it} + \varepsilon_{igt}$$

$\alpha_{ig}$ denote developer-tract type fixed effects and $\eta_{it}$ denote developer-month fixed effects. $T_i$ is an indicator for the "treatment" group, which varies across specifications. $\beta$ is the difference-in-differences estimate of how investment decisions changed towards a tract type after the OZ program was announced. This design controls for time-invariant differences in how developers invested in different tracts, as well as secular trends in developer investment behavior.

These regression results are contained in Table B.5. I run this regression with OZs being the treatment group in Columns (1) and (2) and eligibles being the treatment group in Column (3). Eligibles are the control group in Column (1) and ineligibles are the control group in Columns (2) and (3). If we assume that there is little, or at least less, substitutability between development in ineligible tracts and eligible tracts, as there is between eligible tracts and OZs, and if development was being reallocated from eligible tracts to OZs, we should see a positive effect in all columns. In fact, we see a positive effect of the OZ program on development in OZs, but no effect on development in eligible tracts. This suggests that the effects in Section 2.4 are not driven by reallocation effects.

### Additional robustness

**Trends**: For trends, I include 2011-2015 5-year ACS median family income, poverty rate, and median home values interacted with year fixed effects. These results are presented in Table B.6. Median family income (Column (2)) and poverty rates (Column (3)) were used for eligibility, and OZs and non-OZs differed in their distribution of the two covariates; median home value (Column (3)) is an important measure of the local housing market that could be forward-looking of future investment. Inclusion of these trends does not substantively affect the comparability of OZs and non-OZs in the pre-OZ period, nor the size and significance of new development effects in the post-OZ period. Furthermore, controlling for OZ, OZ by city, and tract-level linear trends in Table B.7 still leaves a significant overall effect of the program. The diminished effect is not surprising, since these controls will also partial out dynamic policy impacts (Wolfers, 2006). In the context of this program, these dynamics seem to be important since the effect increases substantially from 2018 to 2020.

**Alternative specification:** While the linear probability model is misspecified, it offers a convenient way to summarize average program responses while accounting for high-dimensional controls. I also estimate the OZ effect on new development using Poisson Pseudo-Maximum Likelihood (PPML) regression (Silva and Tenreyro, 2006). While also misspecified for the binary outcome case, PPML regression offers computational advantages for including the same set of high-dimensional controls. The results of these models are included in Column (4) of Table 2.4. The coefficients can be interpretted as semi-elasticities with respect to the policy, and show zero pre-trends and similarly sized and significant policy effects on new development.

**Placebos in time & structural break test:** Beyond testing for pre-trends, both the quarterly and monthly results point to a clear, structural break in new development at OZ implementation. Differences between OZs and eligible non-OZs hover around zero in periods prior to the policy, then significantly increase near 2018 Q2 (when OZs were announced). I first test how strong this relationship is by running a placebo test: I use May 2015, the date the original OZ framework was published, as a "fake" date in which OZs were designated; the quarterly difference-in-difference estimates are presented in Figure B.11. Reassuringly, no effects can be detected under this placebo OZ program.

To test this more generally, I implement a structural break test from Andrews (1993) and Andrews (2003). The baseline model in Section 2.4 is estimated as if OZs had been announced at month $m$, for each $m$ between the first and last 5 months of my sample. Figure B.12 plots the Wald test, for each $m$, under the null hypothesis that the "pseudo" average treatment effect is zero. The figure shows that the significance of the break increases monotonically up until April 2018, before monotonically declining. The sup-Wald test yields a statistic of 58, significant at any conventional level using critical values from Andrews (2003). This test demonstrates a surge in new development in OZs relative to non-OZs happening precisely when the OZ program was implemented.

**Placebos in tracts & randomization test:** The Andrews (2003) test can be viewed as asking how powerful the OZ effect is under placebo program adoption dates. We can similarly ask the question: how strong are the observed OZ effects under alternative re-assignments of census tracts to OZ status and non-OZ status? This randomization test accounts for design-based uncertainty rather than sampling-based uncertainty, and is particularly appealing when the units are fixed geographic units, not necessarily sampled from a larger population (Abadie et al., 2020). To implement this test, I draw OZs randomly from the set of eligible tracts (with probability equal to the empirical fraction of OZs among all eligible tracts). Second, I re-estimate the baseline annual specification with the new "placebo" OZs. I then perform this 100 times and plot the distribution of the point estimates relative to the actual estimates, as seen in Figure B.13. Reassuringly, the placebo point estimates all hover near zero. The actual pre-trends are well within the center of the placebo distributions, and the actual OZ effects are well above the placebo distribution in years after the OZ program was implemented.[7]

[7]These placebo tests can alternatively be viewed as demonstrating the treatment effects are significant under exact inference (Hagemann, 2019).

## B.5 Model Details

### Model Estimation

For estimation, I use a global optimization procedure that compares local minima at stochastically chosen initial values. A root-finding algorithm is employed within this procedure to solve for the rational expectations equilibrium. More details are given below.[8] Standard errors are analytically calculated and correspond to the asymptotic variance of the maximum likelihood estimator. Details of this calculation are given in the next subsection.

### Variance Calculation

The FIRE equilibrium is a solution to the following set of equations in each time period.

$$\mathbb{P}_t^\star(\theta, \boldsymbol{\omega}_t) = \mathbf{G}_t\big(\mathbb{P}_t^\star(\theta, \boldsymbol{\omega}_t)\big)$$

$\mathbf{G}_t$ is the function that takes as an input a vector of subjective expectations over all agents, and outputs the vector of implied probabilities that a developer will engage in new development in that period. Equivalently, we have $\mathbb{P}_t^\star(\theta, \boldsymbol{\omega}_t) - \mathbf{G}_t(\mathbb{P}_t^\star(\theta, \boldsymbol{\omega}_t)) = 0$. By the Implicit Function Theorem (where $I_n$ is a $n \times n$ identity matrix)

$$\frac{\partial \mathbb{P}_t^\star(\theta, \boldsymbol{\omega}_t)}{\partial \theta'} = \left[I_n - \frac{\partial \mathbf{G}_t}{\partial \mathbb{P}_t^{\star\prime}}\right]^{-1} \frac{\partial \mathbf{G}_t}{\partial \theta'}$$

The maximum likelihood estimator $\hat{\theta}$ sets

$$s(\hat{\theta}|\mathbb{P}^\star) = \sum_{t=1}^{T} \sum_{i=1}^{n} \left(\frac{y_{it}}{\mathbb{P}_{it}^\star(\hat{\theta})} - \frac{1 - y_{it}}{1 - \mathbb{P}_{it}^\star(\hat{\theta}, \boldsymbol{\omega}_t)}\right) \frac{\partial \mathbb{P}_{it}^\star(\hat{\theta}, \boldsymbol{\omega_t})}{\partial \theta'} = 0$$

A second derivative yields two set of terms. The first contains the residual $y_{it} - \mathbb{P}_{it}^\star(\hat{\theta}, \boldsymbol{\omega}_t)$, which has expectation zero when $\hat{\theta}$ is replaced in the limit with the true $\theta$, and so can be dropped from the estimate for the asymptotic variance. The remaining term gives an estimator for the information matrix as

$$\hat{I} = \frac{1}{nT} \sum_{t=1}^{T} \sum_{i=1}^{n} [\mathbb{P}_{it}^\star(\hat{\theta}, \boldsymbol{\omega_t})\big(1 - \mathbb{P}_{it}^\star(\hat{\theta}, \boldsymbol{\omega_t})\big)]^{-1} \frac{\partial \mathbb{P}_{it}^\star(\hat{\theta}, \boldsymbol{\omega_t})}{\partial \theta} \frac{\partial \mathbb{P}_{it}^\star(\hat{\theta}, \boldsymbol{\omega_t})}{\partial \theta'}$$

Its inverse is an estimate of the asymptotic variance-covariance matrix for $\hat{\theta}$.

[8]Additionally, I treat New York separately by its five boroughs, as well as Los Angeles and Chicago separately by their Northern and Southern regions, for the purposes of calculating equilbria.

### Model Estimation

All calculations were performed using Python version 3.10.7. The optimization toolkit is from SciPy's optimize package. The rational expectations solver uses a modified Powell method from MINPACK (a FORTRAN library, accessed via option "hybr" in function `root`). I search for all solutions to the rational expectations equation from three starting points: the lowest "rationalizable" expectations, with expectations set to be the average for each unit across the entire time sample, and with expectations at the highest "rationalizable" expectations. The lowest and highest rationalizable expectations are calculated as the probability of new development in a census tract if they assume all other census tracts are engaging in new development with probability zero and one respectively.

The estimation can spend large amounts of computational time in regions of the parameter space with $\delta < 0$. To speed up convergence, I estimate the model using the transformed parameter $\tilde{\delta}$ where $\delta = \exp(\tilde{\delta})/(1 + \exp(\tilde{\delta}))$, explicitly restricting $\delta$ to lie within the unit interval. $\hat{\delta}$ across cities tend to be well within this interval, suggesting the transformation is not restrictive. Standard errors are calculated via the Delta method.

The global optimization procedure for maximizing the likelihood uses "basin-hopping" paired with an inner maximization procedure using the exact trust-region algorithm (option "trust-krylov" in function `minimize`). Analytic gradients are calculated and used in the root-finding and optimization procedures. The estimate of the expectation of the information matrix is used in the "trust-exact" routine.

A pseudo-algorithm for the estimation procedure is included below. Here, $\theta_k$ denotes an iterative guess of $\theta$, not the $k$th component of $k$. `root_solver` refers to the inner loop – the rational expectations solver. `local_maximizer` refers to the outer loop – the likelihood maximization procedure. `global_maximizer` refers to the stochastic optimization that wraps the entire estimation procedure, re-estimating at stochastically chosen initial values and stopping when some criterion is achieved for the local maxima.

### Pseudo-code

**Algorithm 1** Calculate $\hat{\theta}$.

$j = 0$
$\theta_0 = \theta_0^* = 0$
$L_{-1} = -10^3$
$tol_1 = tol_2 = 10^3$
**while** $tol_1 > \varepsilon_1$ **do**
  $k = 0$
  **while** $tol_2 > \varepsilon_2$ **do**
    $P = \{\mathbb{P}_t^\star(\theta_k)\} \leftarrow$ `root_solver` $(\theta_k, t)$
    $L_k \leftarrow \max_P \mathcal{L}$
    $tol_2 \leftarrow |L_k - L_{k-1}|$
    **if** $tol_2 \leq \varepsilon_2$ **then**
      **return** $\theta_{j+1}^* \leftarrow \theta_k$
    **end if**
    $\theta_{k+1} \leftarrow$ `local_maximizer` $(\theta_k)$
    $k \leftarrow k + 1$
  **end while**
  $tol_1 \leftarrow ||\theta_{j+1}^* - \theta_j^*||$
  **if** $tol_1 \leq \varepsilon_1$ **then**
    **return** $\hat{\theta} \leftarrow \theta_{j+1}^*$
  **end if**
  $\theta_0 \leftarrow$ `global_maximizer`$(\theta_{j+1}^*)$
  $j \leftarrow j + 1$
**end while**

### OZ Stationary Effect and Optimal Policy Estimation

Throughout the model and optimal policy design, I solve for the equilibrium (stationary) probability of new development for a given implementation of the investment tax credit. To solve for the stationary distribution of new development, I simulate new development from a city for 1000 months. I then take the fraction of months spent in a state of new development over the last 200 months as my estimate of the stationary probability. In addition to the computational details in the main text, I use the modal equilibrium (between "low," "middle," and "high") in the post-period, and the most recent time and eligibility by year effects, for calculating stationary distributions.

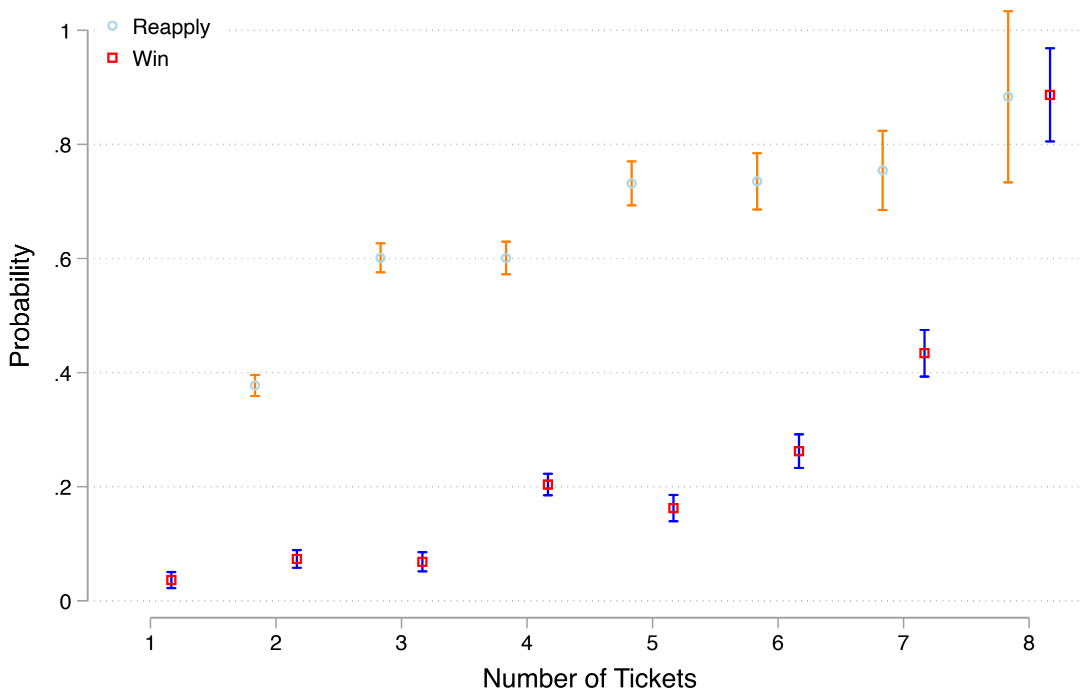
To solve for the optimal OZ design, I use the above procedure to calculate the stationary probability for every potential policy the optimization tries. The global optimization procedure for searching over policies to maximize the stationary level of new development uses "basin-hopping," with constraints on the OZ policy units to be between 0 and 1, to only be allowable for eligible tracts, and such that the total number of OZs cannot exceed the actual observed number for the city. The maximization is then re-run on stochastically chosen initial values. I pair this procedure with an inner root-finding algorithm to solve for the new city equilibrium condition. In practice, the algorithm ends up assigning integer (0 or 1) units of the policy to most tracts. For the few that optimally have fractional policy units, I take those with the highest amount of policy as included in the optimal OZ implementation, up until the constraint on the total number of OZs a city has at their disposal.

# Appendix C

# Appendix for High-End Housing and Gentrification: Evidence from a San Francisco Lottery

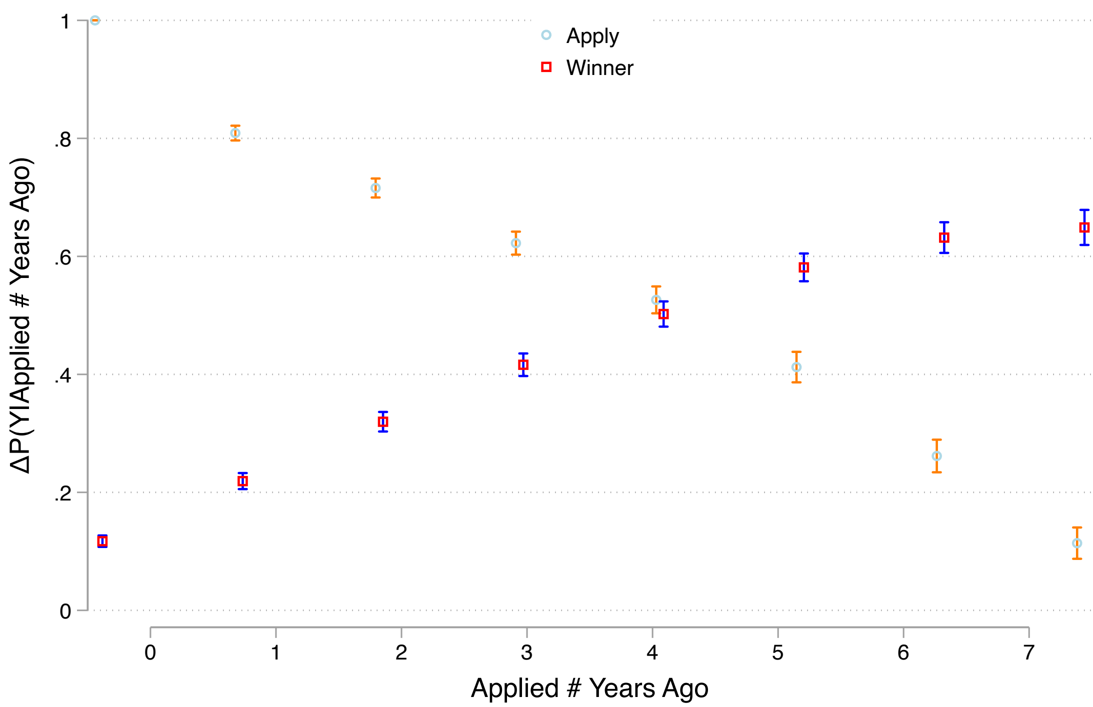
## C.1 Appendix Figures

Figure C.1: Application Behavior by Number of Tickets



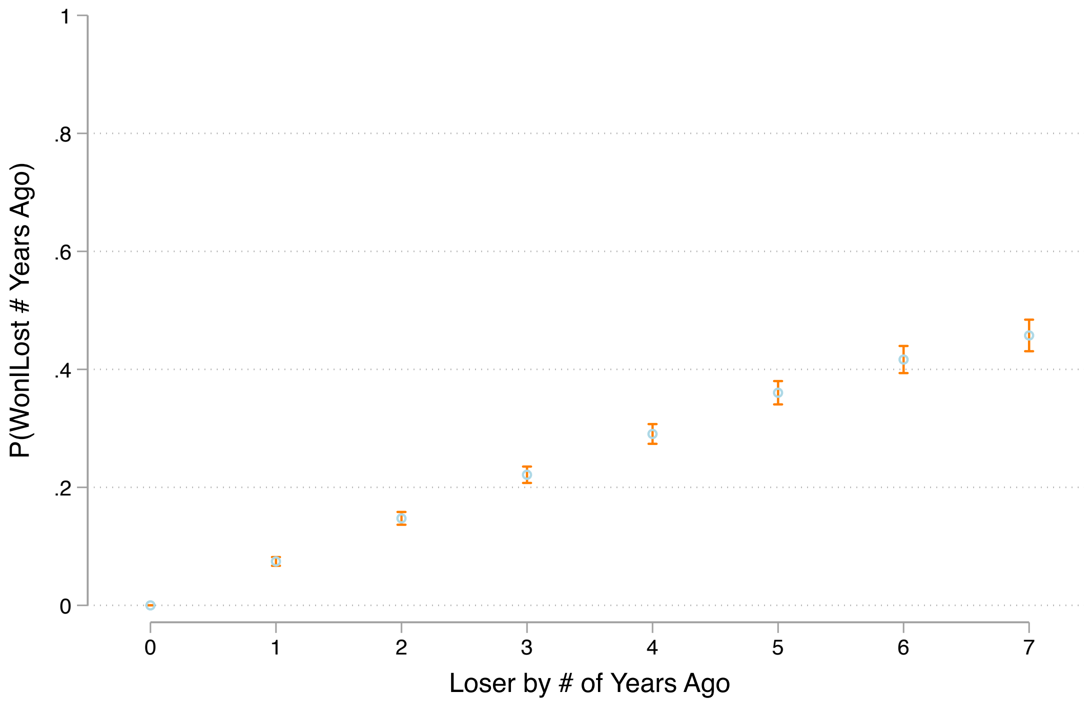

*Notes*: The unit of analysis is a lottery applicant from 2001-2013, and data from the City of San Francisco Assessor's Office. The square markers indicate how an applicant's probability of winning the lottery varies with their number of tickets. The circle markers indicate how an applicant's probability of re-applying to the lottery varies with the number of tickets. See section 3.2 for details. Errors bands show 95% confidence intervals.

Figure C.2: Application Behavior by Previous Application



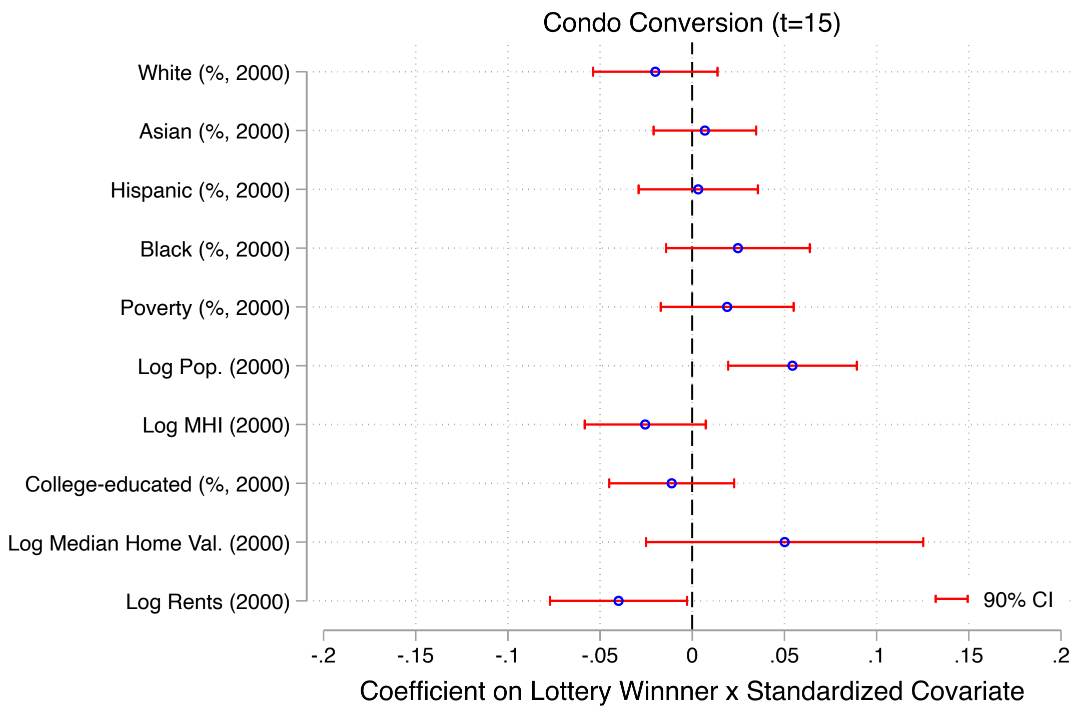

*Notes*: The unit of analysis is a lottery applicant from 2001-2013, and data from the City of San Francisco Assessor's Office. The square markers indicate how an applicant's probability of winning the lottery varies with the number of times they previously applied. The circle markers indicate how an applicant's probability of re-applying to the lottery varies with the number of times the have previously applied.

Figure C.3: Fraction of Losers who Win by Number of Years



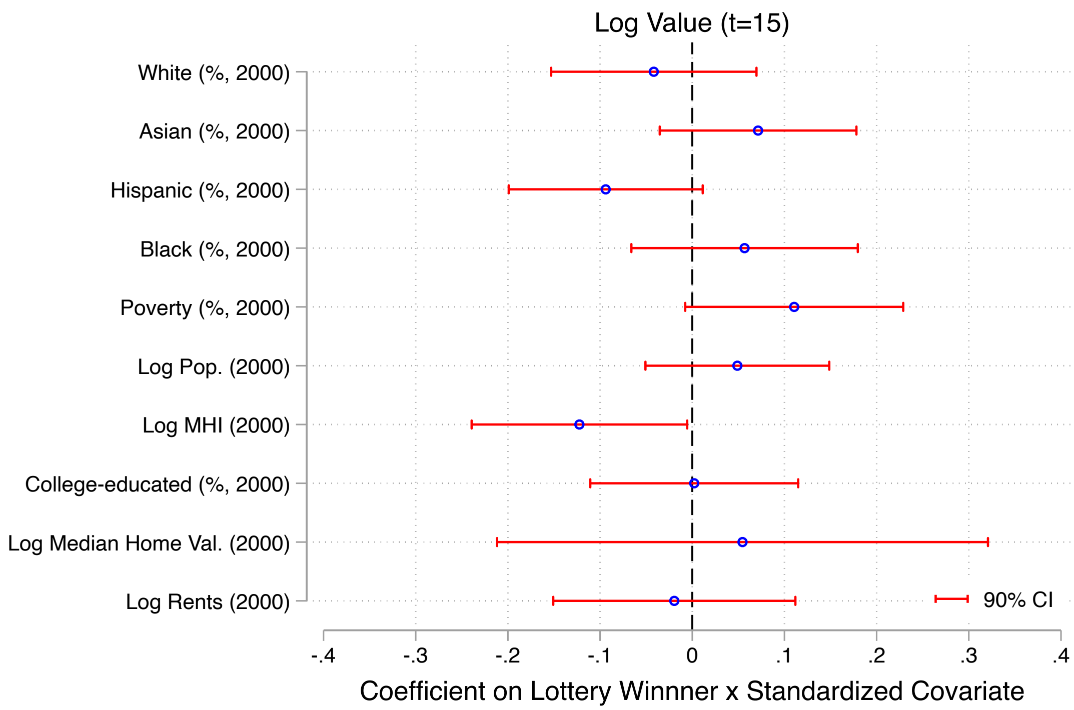

*Notes*: The unit of analysis is a lottery applicant from 2001-2013, and data from the City of San Francisco Assessor's Office. The figure plots the probability that an applicant who loses the lottery in year t=0 wins the lottery in a later year. Standard errors are clustered by applicant, and error bands show 95% confidence intervals.

Figure C.4: Neighborhood Heterogeneity in Condominium Conversions



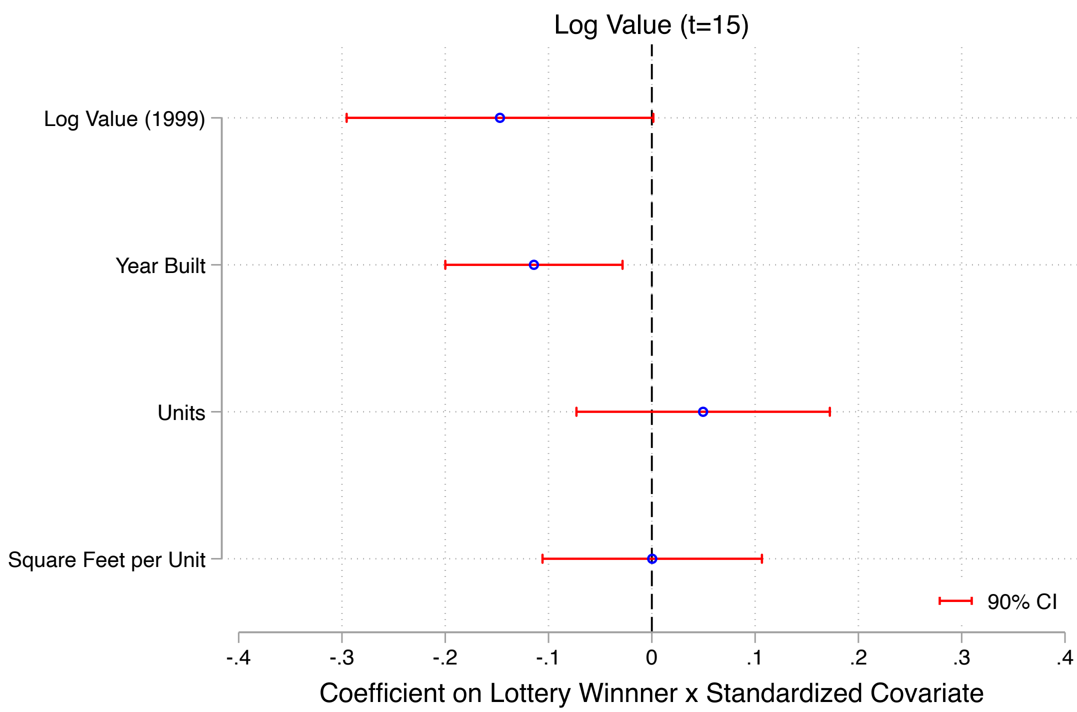

*Notes*: The unit of analysis is a property whose owners apply to the lottery, and the outcome is an indicator for condominium conversions. The figure reports interaction coefficients from alternate specifications of equation 3.5 where the treatment indicators $\beta_k$ are interacted with initial neighborhood characteristics. Standard errors are clustered by applicant, and error bands show 90% confidence intervals.

Figure C.5: Neighborhood Heterogeneity in Condominium Value Appreciation




*Notes*: The unit of analysis is a property- whose owners apply to the lottery, and the outcome is log property value. The figure reports interaction coefficients from alternate specifications of equation 3.5 where the treatment indicators $\beta_k$ are interacted with initial neighborhood characteristics. Standard errors are clustered by applicant, and error bands show 90% confidence intervals.

Figure C.6: Building Heterogeneity in Condominium Value Appreciation




*Notes*: The unit of analysis is a property whose owners apply to the lottery, and the outcome is log property value. The figure reports interactions coefficients from alternate specifications of equation 3.5 where the treatment indicators $\beta_k$ are interacted with initial property characteristics. Standard errors are clustered by applicant, and error bands show 90% confidence intervals.

## C.2 Appendix Tables

Table C.1: Spillover Effects of Condominium Conversions on Nearby Properties

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | $\Delta^{15}_{t=-1}$Log Value | $\Delta^{15}_{t=-1}$Log Value | $\Delta^{15}_{t=-1}$Log Value | $\Delta^{15}_{t=-1}$Log Value |
| **Condo Conversion (0-25m)** | 0.111** | 0.0853* | 0.0828* | 0.0873* |
| | (0.0477) | (0.0463) | (0.0471) | (0.0461) |
| **Observations** | 3,706,112 | 3,706,112 | 3,706,112 | 3,706,103 |
| **Model** | IV | IV | IV | IV |
| **Geography x Year FE** | None | Nbhd | Tract | Block Group |

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

*Notes*: The unit of analysis is a property-year, and the sample includes "nearby" properties within 25 meters of a lottery winner. Data are from the City of San Francisco Assessor's Office. The figure reports the $\beta^{IV}_{k=15}$ coefficients from equation 3.4. The specifications compare trends in log home prices of properties located near lottery winners versus properties located near lottery losers. The regressions control for predicted probabilities of treatment, as in Borusyak and Hull (2023); see section 3.4 for details. Column 1 shows the benchmark specification. Columns 2-4 respectively add controls for local time trends by neighborhood, census tract, and census block group. Standard errors are clustered by lottery applicant.

# Appendix D

# Appendix for Uber versus Trains? Worldwide Evidence from Transit Expansions

## D.1 Additional figures and tables

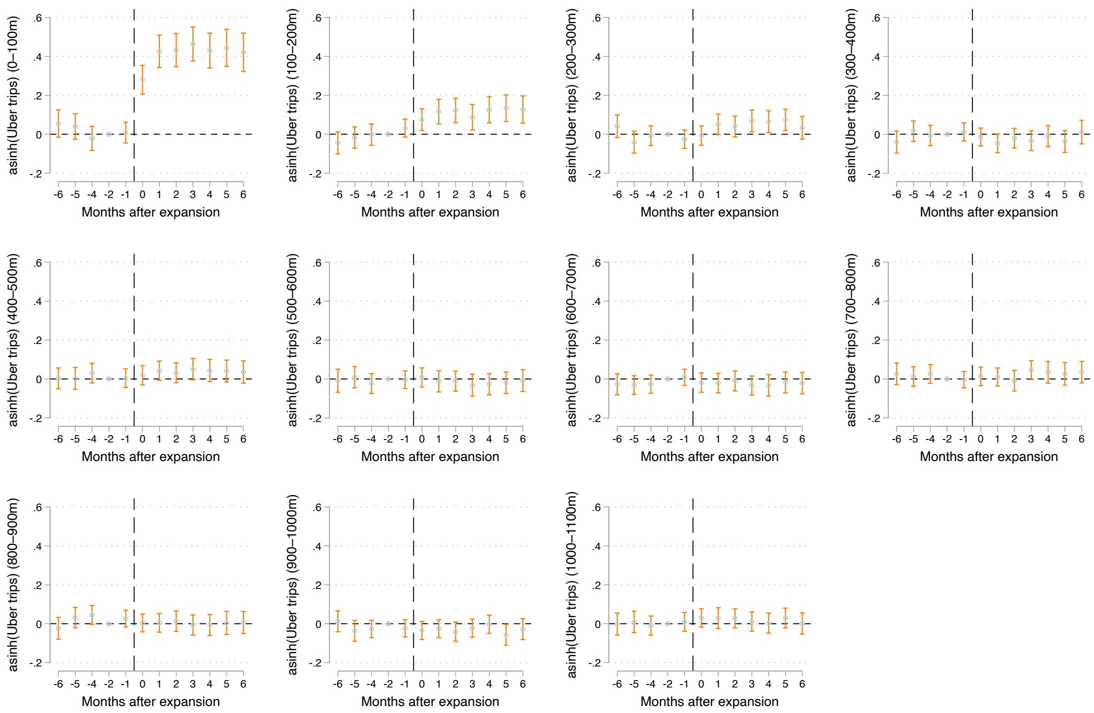


Figure D.1: Difference-in-differences estimates, using the adjacent distance band as the control group, from 0–100 m to 1000–1100 m

*Notes:* These subfigures plot the coefficients (circles) and 95% confidence intervals (bars) for the percentage effect of a new train station opening on Uber trips for the labeled distance bands. The coefficients are plotted for each month between six months prior and six months after an expansion. The regression is a dynamic difference-in-differences model of station openings on the inverse hyperbolic sine of Uber trips that either originated or terminated at a given distance band from the opening, and the regression equation is given by equation (4.1). The model contains station by distance band and station by time fixed effects; additionally, fixed effects for distance band by more than 6 months before or after an expansion are included. All errors are clustered at the station-level.

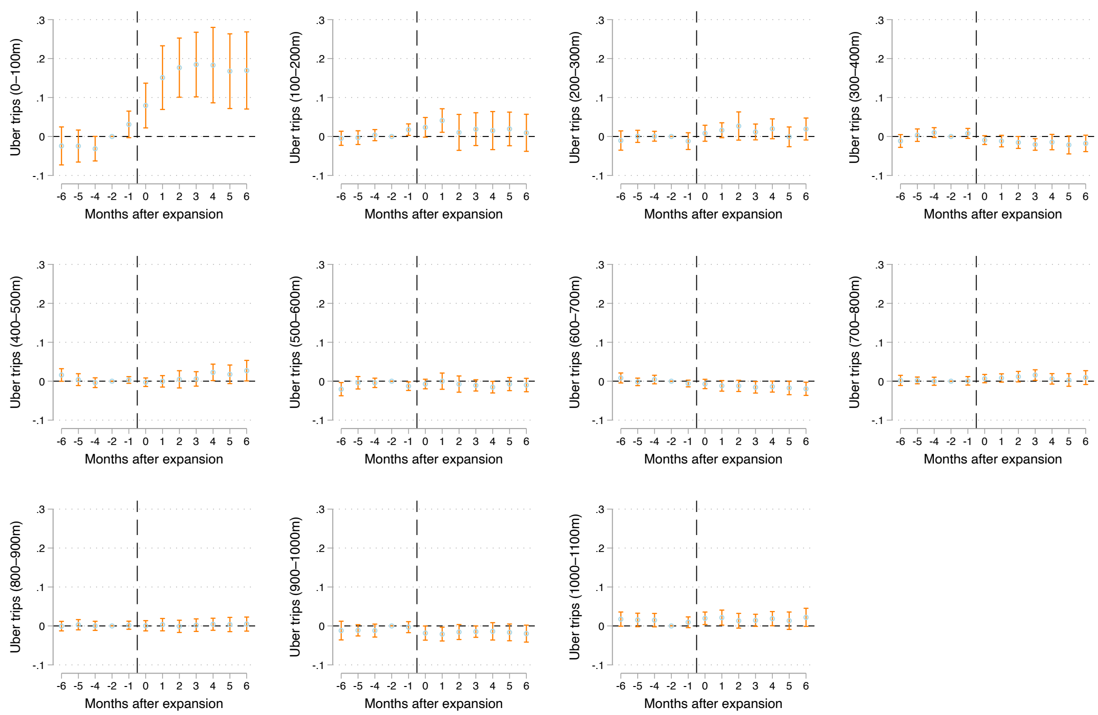



Figure D.2: Difference-in-differences estimates, using adjacent distances as the control group, from 0–100 m to 1000–1100 m, using PPMLE

*Notes:* These subfigures report the results of re-estimating the analysis reported in Figure D.1, except that we now use Poisson pseudo-likelihood regression rather than ordinary least squares.

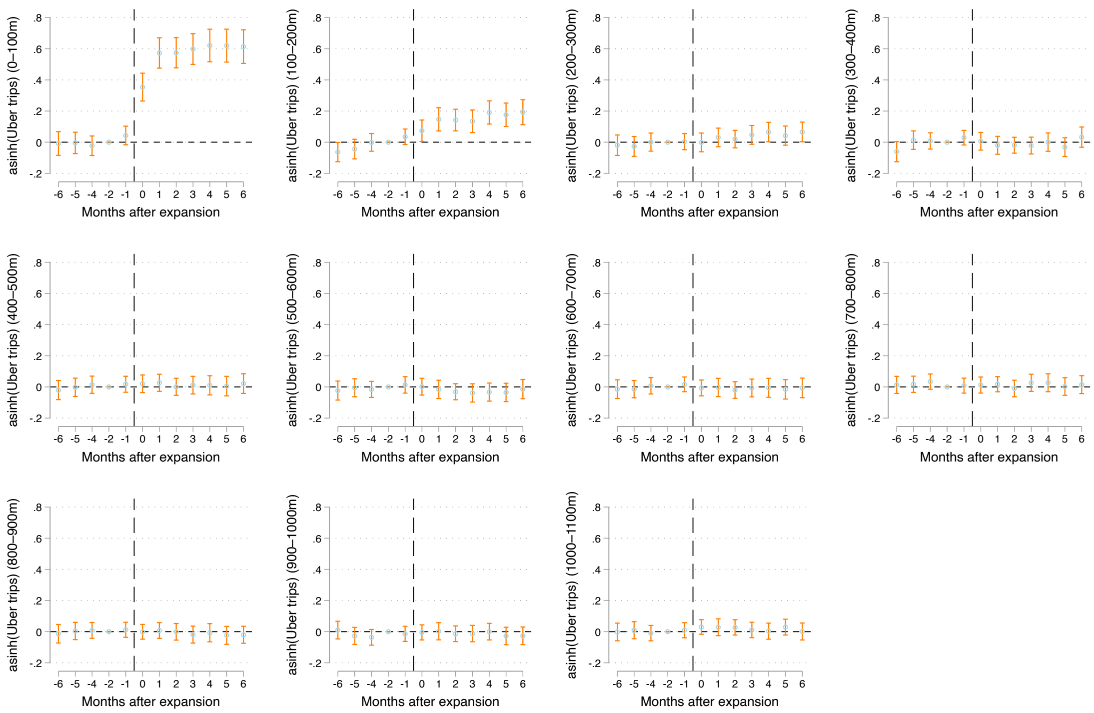



Figure D.3: Difference-in-differences estimates, using 1100–1200 m as the control group, from 0–100 m to 1000–1100 m

*Notes:* These subfigures plot the coefficients (circles) and 95% confidence intervals (bars) for the percentage effect of a new train station opening on Uber trips for the labeled distance bands. The coefficients are plotted for each month between six months prior and six months after an expansion. The regression is a dynamic difference-in-differences model of station openings on the inverse hyperbolic sine of Uber trips that either originated or terminated at a given distance band from the opening, and the regression equation is given by equation (4.1). The model contains station by distance band and station by time fixed effects; additionally, fixed effects for distance band by more than 6 months before or after an expansion are included. All errors are clustered at the station-level.

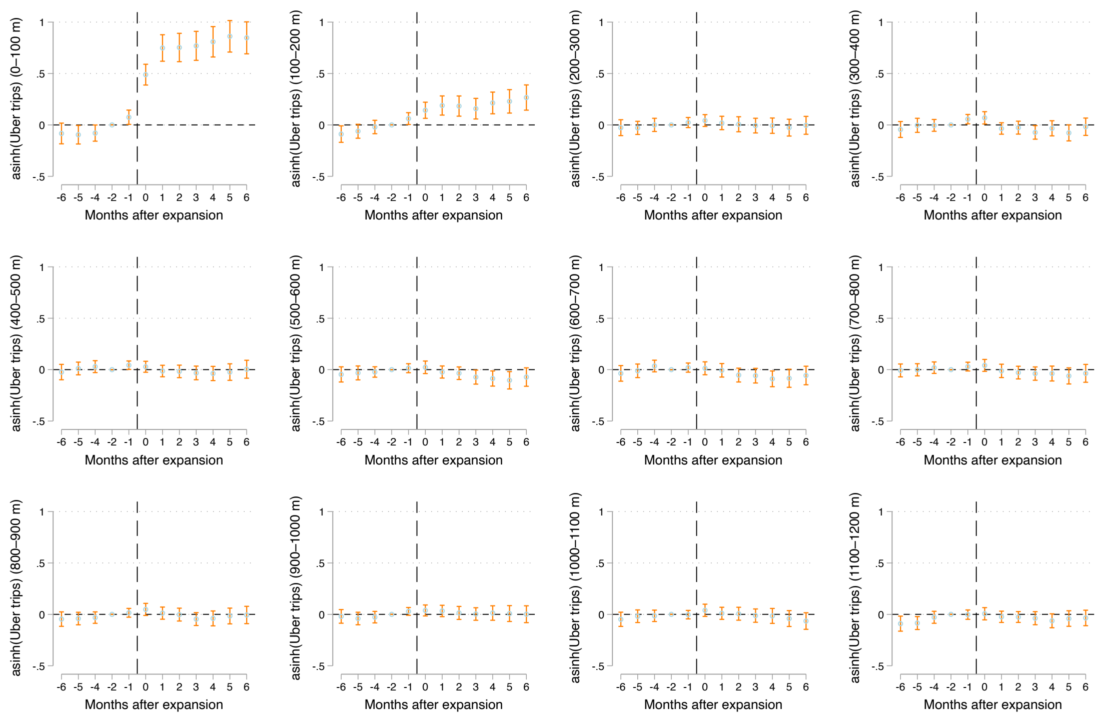



Figure D.4: Event study estimates for 100 m distance bands, using city-month fixed effects

*Notes:* These subfigures report the results of re-estimating the analysis reported in Figure D.1, except that we now use city-month fixed effects to adjust for time trends, rather than differencing out the effect at the next further distance band.

## D.2 Appendix: Methodology

**Total effects by distance calculation**

Our main difference-in-differences estimates come from (4.2), given by

$$\begin{gathered} y_{dit} = \gamma_{it} + \delta_{di} + \alpha_{\tilde{d}} \times \mathbb{1}_{\tau_{it}\in\{1,\ldots,6\}} \times \mathbb{1}_{d=\tilde{d}} \\ + \left(\beta_{1,\tilde{d}} \times \mathbb{1}_{\tau_{it}<-6} + \beta_{2,\tilde{d}} \times \mathbb{1}_{\tau_{it}>6} + \beta_{3,\tilde{d}} \times \mathbb{1}_{\tau_{it}\in\{-2,-1,0\}}\right) \mathbb{1}_{d=\tilde{d}} + \epsilon_{dit}, \\ \forall\ d \in \{\tilde{d}, \tilde{d}+1\}. \end{gathered}$$

Each regression is estimated separately by $\tilde{d}$, producing an array of expansion effects at varying distances relative to the next distance band $\tilde{d}+1$. The $\alpha_d$ can be estimated jointly by stacking all such distance pairs $p$, where $p \in \{100, ...., 1100\}$ refers to the "treated" distance in each pair. In the new data set, distance bands 0–100 m and 1100–1200 m will appear once, while all others will appear twice (once in the control group and once in the treatment group). The specification can then be written as follows.

$$\begin{gathered} y_{dipt} = \gamma_{ipt} + \delta_{dip} + \sum_{\tilde{d}} \alpha_{\tilde{d}} \times \mathbb{1}_{\tau_{it}\in\{1,\ldots,6\}} \times \mathbb{1}_{d=\tilde{d}} \times \mathbb{1}_{p=\tilde{d}} \\ + \sum_{\tilde{d}} \left(\beta_{1,\tilde{d}p} \times \mathbb{1}_{\tau_{it}<-6} + \beta_{2,\tilde{d}p} \times \mathbb{1}_{\tau_{it}>6} + \beta_{3,\tilde{d}p} \times \mathbb{1}_{\tau_{it}\in\{-2,-1,0\}}\right) \mathbb{1}_{d=\tilde{d}} \times \mathbb{1}_{p=\tilde{d}} + \epsilon_{dipt} \end{gathered}$$

The $\alpha_{\tilde{d}}$ will be numerically equivalent to those from (4.2) when errors are clustered at the station-level. As before, the $\alpha$ estimate the effect at one distance relative to another. In order to estimate the total effect of an expansion on Uber trips at a given distance, we sum all estimates from farther distances. To implement this, we simplify the above regression as follows.

$$\begin{gathered} y_{dipt} = \gamma_{ipt} + \delta_{dip} + \sum_{\tilde{d}} \alpha^*_{\tilde{d}} \times \mathbb{1}_{\tau_{it}\in\{1,\ldots,6\}} \times \mathbb{1}_{d=\tilde{d}} \\ + \sum_{\tilde{d}} \left(\beta_{1,\tilde{d}} \times \mathbb{1}_{\tau_{it}<-6} + \beta_{2,\tilde{d}} \times \mathbb{1}_{\tau_{it}>6} + \beta_{3,\tilde{d}} \times \mathbb{1}_{\tau_{it}\in\{-2,-1,0\}}\right) \mathbb{1}_{d=\tilde{d}} + \epsilon_{dipt} \end{gathered}$$

We no longer estimate the $\alpha$'s by comparisons within distance-pairs. By not fully-saturating on pairs $p$, we allow the treatment effect from one distance to pass through into the next, closer distance. Numerically, $\alpha^*_{\tilde{d}} = \sum_{d\geq\tilde{d}} \alpha_d$. Additionally, the regression implementation of summing the coefficients correctly calculates standard errors. These estimates are reported in Figure 4.6.