Locally Optimal Place-Based Policies: Evidence from Opportunity Zones

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Abstract

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. This paper evaluates the impact of Opportunity Zones on new residential and commercial development, and quantifies how policymakers could have achieved a more efficient 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 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.

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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., 2016; 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]

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, 2021). The program's scope and magnitude offer an ideal context

[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.

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, 2021). 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 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 de-

[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."

velopment. 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., 2019). 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 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

[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, 2019), large market-rate apartment buildings (Asquith et al., 2019), rent control (Autor et al., 2014), urban revitalization programs (Rossi-Hansberg et al., 2010), and foreclosures (Campbell et al., 2011) have all been studied.

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 (2001a)'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, 2014). 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, 1981b; 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., 2019). 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. (2020), Atkins et al. (2020), and Freedman et al. (2021) have focused on wages and employment.[9] Casey (2019) and Chen et al. (2019) have focused on local housing prices. In particular, Chen et al. (2019) find no effect on local housing price *growth* in OZs. They focus on the entire U.S., while I focus on a

[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. (2020) 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.

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] 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 provides context for the Opportunity Zone program. Section 3 describes the data sources used. Section 4 presents reduced-form evidence of the new development response to the investment tax credit. Section 5 documents positive spillovers on development in nearby neighborhoods. Section 6 describes the model and approach to estimation. Section 7 presents the model estimates. Section 8 describes the optimal policy framework and presents policy counterfactuals. Section 9 concludes.

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, 2021).

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

[10] They rely on Federal Housing Finance Agency data. They also find mixed evidence on residential permitting at the census place-level. Further discussion of these differences is included in Section 4.6.

[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.

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 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, 2021).

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 author-

[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.

ities.[16] From April until June of 2018, the IRS released lists of approved census tracts; virtually all of the nominated tracts were approved. Figure A.1 includes maps for examples of eligible neighborhoods and their chosen OZs in four cities.

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.

3.1 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

[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).

program. Figure 1 maps the cities in my sample, with geographic coverage ranging across the U.S. Additional information about the data construction is in Appendix C.

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 Appendix C. Moreover, Section 4.4 finds that address counts have increased in OZs.

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 (2019), 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.

3.2 Preliminary Facts

The distribution of months with new developments is included in Figure A.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 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 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 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 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 A.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 A.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 A.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 A.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.

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.

4.1 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 OZ_i denotes whether tract i is designated an OZ, α_i captures unrestricted tract-level heterogeneity, η_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 4.3.

By including $\theta_{g(i)t}$, estimates of the key parameters β_k come from comparisons between OZs and eligible non-OZs. Identification of the β_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 β_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 4.4, I present evidence of localized, positive spillovers. The downward bias on 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”

[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 4.3 I use propensity-score methods to balance OZs and the comparison group on observable characteristics.

tracts from being near to other OZs. A primary motivation for the model presented in Section 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 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.

4.2 Opportunity Zone Effects

Estimates of the linear probability model are depicted graphically in Figure 3. The coefficients β_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 3a documents the baseline estimates of β_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

[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 Appendix D.

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 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 3b show little evidence for this story from 2016 Q1 to 2018 Q2. Monthly dynamics in Figure 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.

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

The indicator 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 β 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 β are in Table 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 β 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.

4.3 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 con-

[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 4.2, 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.

cern 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 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 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 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 4. This model is doubly-robust;

[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 A.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.

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 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 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.

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

4.4 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.

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

[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.

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 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 A.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 A.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 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 Data. I use the same difference-in-differences specification with a Poisson Pseudo-Maximum Likelihood estimator. These results are contained in Figure A.16. I find no evidence of pre-trends and an increase of

[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.

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.

4.5 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, 2019), 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 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 5.

[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 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.

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 6 and the optimal policy design of Section 8.

4.6 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 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. (2019) 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 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.

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

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

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., 2019), 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 (2016) and Almagro and Domınguez-Iino (2019)). 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 (Appendix D). 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 (2020) 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 4.1 to estimate the spillover effects.

I use the propensity score from Section 4.3 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 β_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 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.

[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 A.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.

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. 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 A.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 A.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 4.1: 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

[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.

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 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 6, and they play a key role in the optimal policy design of Section 8.

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 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.

6.1 Framework

The main outcome of interest is whether new development occurs in a location at a given time, as in Section 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 (2001a)'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 ω_{it}, are hit with a building cost shock ε_{it}, and choose y_{it} to maximize expected profits π^*_{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.

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

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 δ. The latent, net profits for new development take the following form.

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

The location-heterogeneity term α_i capture time-invariant differences in the returns to developing at a location. The α_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 period from 2014 to 2022, the α_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 α_i directly. Moreover, the α_i will govern whether neighborhoods are more or less inframarginal to the policy, aligning with the reduced-form evidence in Section 4.4.

The parameter γ 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 4.1. I set $\bar{K}$ to be twelve months of prior development decisions.[34]

The λ captures how strong spatial complementarities in S_i are across space. Theoretically, λ could be negative (due to "crowd-out") or positive (due to demand externalities). While I do not restrict possible values of λ, consistent with the reduced-form evidence, estimates of λ will be positive. Because of the non-linearity in the function Λ, there will diminishing returns in $S_i(\mathbf{y}_t)$ for neighborhoods near the average in new development behavior (consistent with Section 5).

The indicator T_{it} equals 1 if the location i is an OZ in month t. The β captures the average policy effect. The $\zeta_{g(i)t}$ are secular time trends in city by eligibility status that make investment

[33]In my estimation, distance will correspond to distances between census tract centroids. Figure A.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.

[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.

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 $\omega_{it} = \{\theta, y_{j,t-k}, T_{jt}, \mathbf{x}_j\}_{j=1,\ldots,n}^{k=1,\ldots,\bar{K}}$ 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-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, 2001a). If they are weak, then multiple equilibria can exist with large variation in equilibria behavior.

[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 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, β_0 and λ_0 can be interpretted as the average direct effect and strength of spillovers.

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

6.2 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 α_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 α_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 $\mathbb{P}_t^\star$ denote the set of equilibrium probabilities at time t. Let $\mathbb{P}_t^\star[m_t]$ denote the vector of probabilities associated with the m_tth equilibrium. We can define the likelihood of a given equilibrium as follows.

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

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.

[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 $\lambda(\mathbf{x}_i)$, γ, 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, 1981a). 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.

[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).

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

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

In practice, each θ produces several equilibrium, of which I take the highest likelihood equilibrium as the corresponding likelihood for θ.[40] Comparisons across θ can then be readily made. Only observations with 12 months of prior development data are used in estimation. See Appendix E for further estimation details. Identification of the structural parameter θ 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, 2001a,b).

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 4.4) and indirect (Section 5) effects suggest that this is not only reasonable, but important for understanding the development response to the tax credit.

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.

[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.

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.

7.1 Estimates

The parameter estimates from my model are summarized in Table 7. The first row contains the main parameters: the main spillover effect λ_0, the main program effect β_0, the spillovers decay parameter δ, the state-dependence parameter γ, and the average and standard deviation of the location heterogeneity terms α_i. The second row contains the spillovers heterogeneity parameters. The third row contains the program effect heterogeneity parameters.

Spillovers λ_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 λ_{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 β_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. Interestingly, β_{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 δ is estimated to be 0.63. While the exact weights depend on the particular geography of the city, this δ 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.

7.2 Model Fit

Figure A.21 assesses the model fit for location heterogeneity and dynamics. Figure A.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 A.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 (2019) 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 A.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 $\widehat{\mathbb{P}_{it}^{\star}}$.[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 (β_{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 $\widehat{\mathbb{P}_{it}^{\star}}$, rather than y_{it}. These figures are contained in Figure 6a and Figure 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

[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.

same. This finding suggests that policy heterogeneity in home values operates through spillover heterogeneity and the location heterogeneity terms α_i. Moreover, the model is able to replicate important sources of effect heterogeneity – through local rents – not targeted in estimation.[42]

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.

8.1 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 4.

[42] Figure A.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.

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

Section 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 β_k coefficients are plotted across time in Figure 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 4.4.

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

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

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

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 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.

8.2 Framework

City planner's have a set of Pareto weights ω_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:

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

$$\text{s.t.} \ \sum_i T(i) = K \qquad (1 - g(i))T(i) = 0, \forall i \tag{2}$$

$$\mathbf{y}_t \sim \text{Bernoulli}\big(\mathbb{P}^{\star}_t(T, \theta, \mathbf{y}_0^{t-1}, \mathbf{x}_i)\big), \ \forall t \tag{3}$$

$$\mathbb{P}^{\star}_t(T, \theta, \mathbf{y}_0^{t-1}, \mathbf{x}_i) = \mathbf{G}_t(\mathbb{P}^{\star}_t(T, \theta, \mathbf{y}_0^{t-1}, \mathbf{x}_i)), \ \forall t \tag{4}$$

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 ρ. 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 ω_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 4.4 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

[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 Appendix E.

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.

8.3 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 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 8a and Figure 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 8c against the optimal designated tax credits in Figure 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 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,

[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.

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 A.24 and Dallas, Texas in Figure A.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 α_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 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 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 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. (2019), 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. (2019). Due to a reasonable amount of

[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).

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 8.1. 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 (2021) suggests that most of the investment so far has gone to larger cities. Conservatively, I use the 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.

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

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

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., 2019).

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.

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Tables

Table 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.

Table 2: Overall effect of OZ designation on new development

	(1) New Building	(2) New Building	(3) New Building	(4) 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.

Table 3: Policy variation at the eligibility cutoffs (I)

	(1) New Building	(2) New Building	(3) New Building	(4) 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 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.

Table 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.

Table 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, 2020)). 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.

Table 7: Model estimates

Panel A: Main parameters						
	$\boldsymbol{\lambda_0}$	$\boldsymbol{\beta_0}$	$\boldsymbol{\delta}$	$\boldsymbol{\gamma}$	$\bar{\alpha_i}$	sd(α_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 6. λ denote the spillover and spillover heterogeneity parameters and β denote the policy and policy heterogeneity parameters. δ captures how quickly spillovers decay across space and γ the strength of state-dependence. The average and standard deviation of the location heterogeneity terms α_i are also included. A description of the estimation procedure and standard errors calculation is included in Appendix E.

Table 8: Characterizing optimal and actual OZs

	(1) OZs (optimal)	(2) OZ	(3) OZ	(4) 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

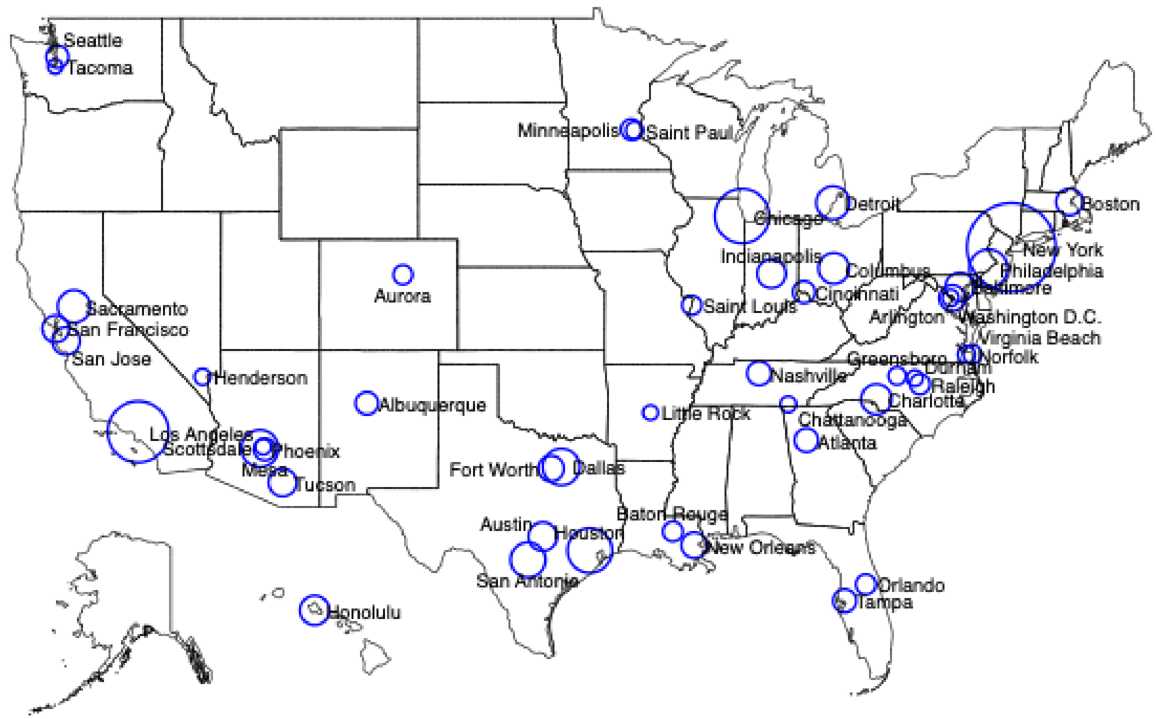
Robust standard errors in parentheses

*** 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.

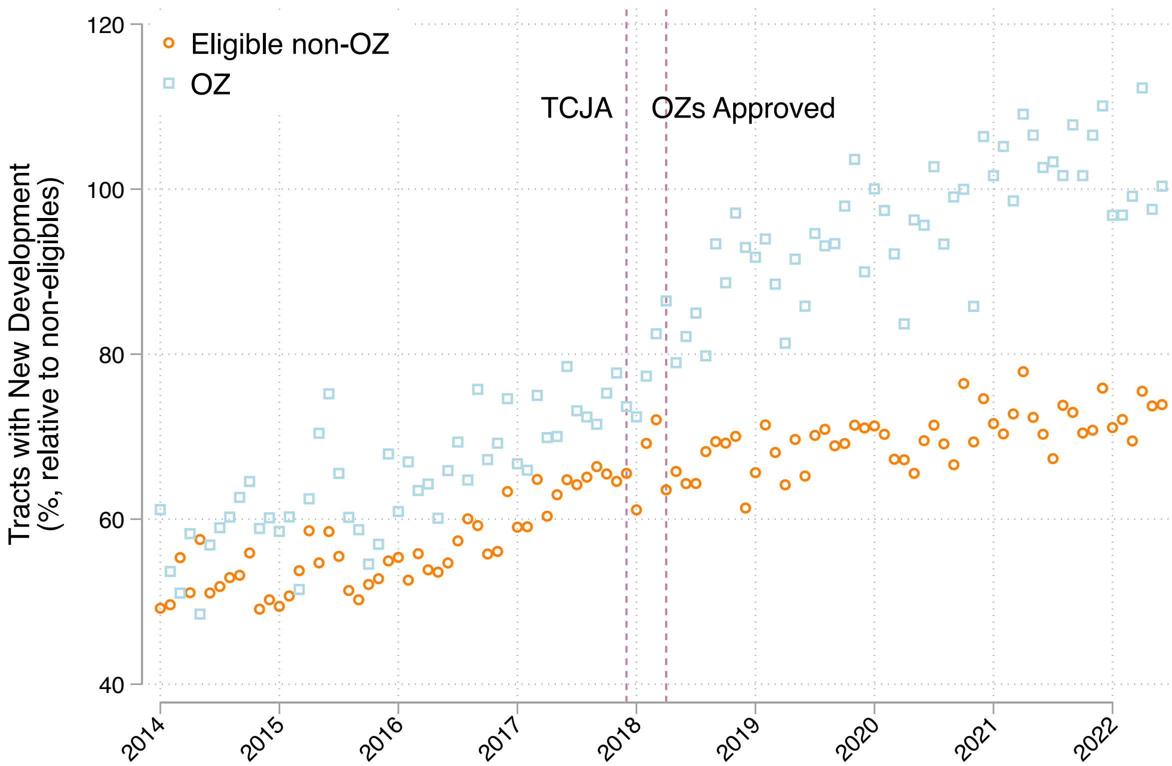
Figures

Figure 1: Cities in main sample



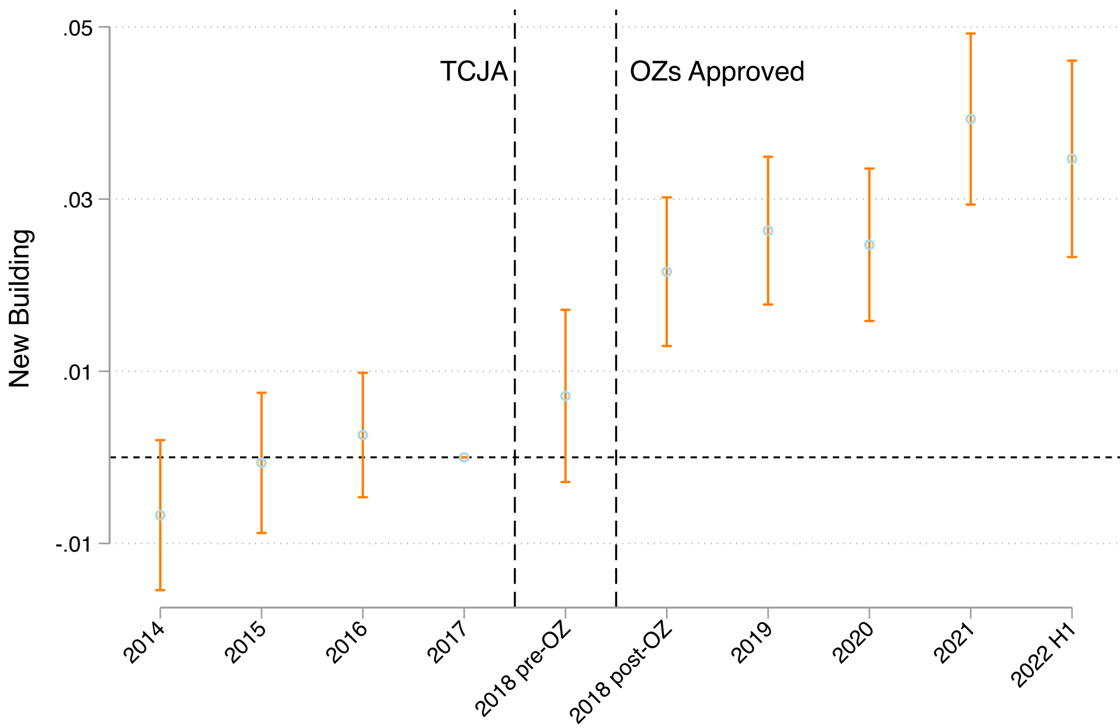
Note: This map shows all cities included in the main sample. The size of the circle is proportional to the number of tracts in the city.

Figure 2: Time series for OZs and eligible non-OZs

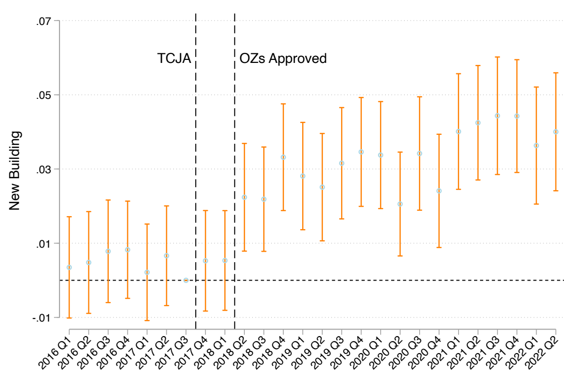


Note: This figure plots time series in new development projects for tracts that were eligible to be designated as OZs, but were not (blue), with those that were designated OZs (orange). The time series is the fraction of tracts in each tract type that have new development projects in a given month as 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).

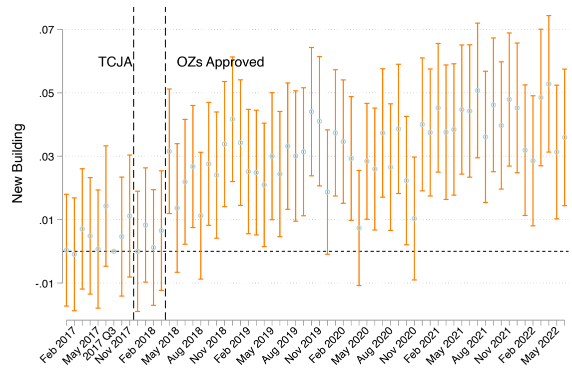
Figure 3: Difference-in-differences estimates



(a) Annual



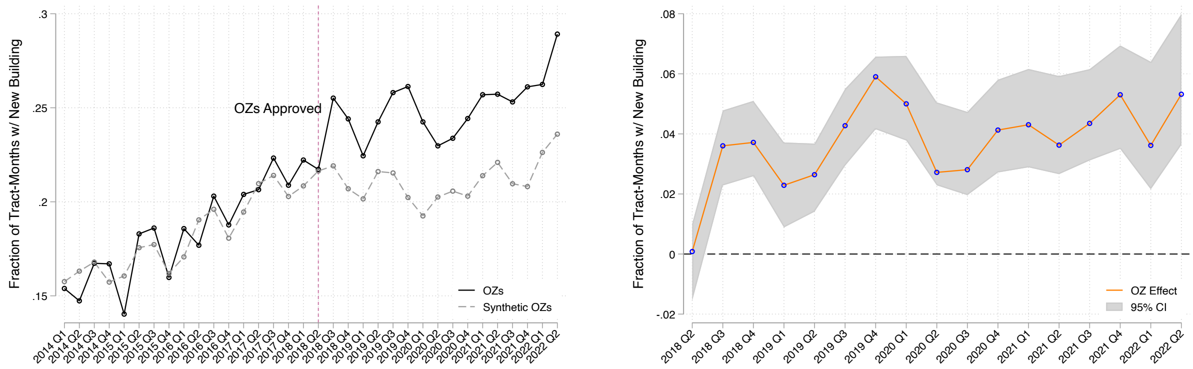
(b) Quarterly



(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.

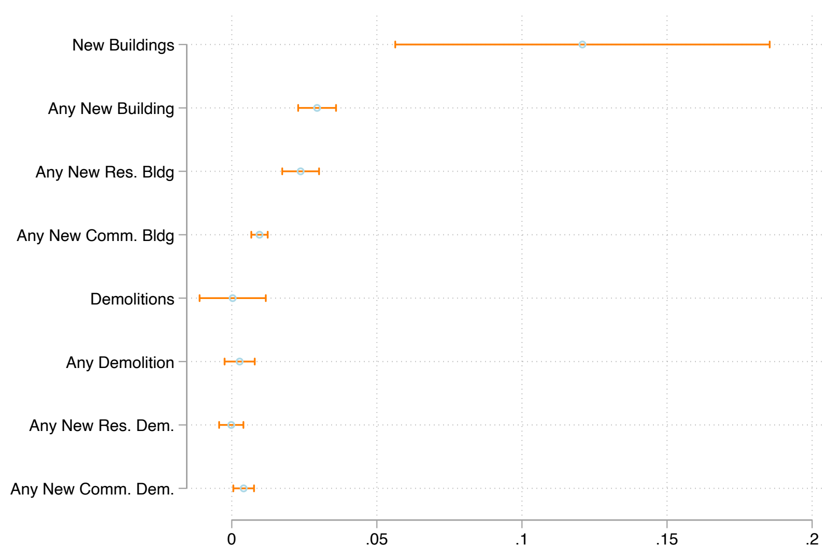
Figure 4: Synthetic control design



(a) Model fit (b) Treatment effect estimates

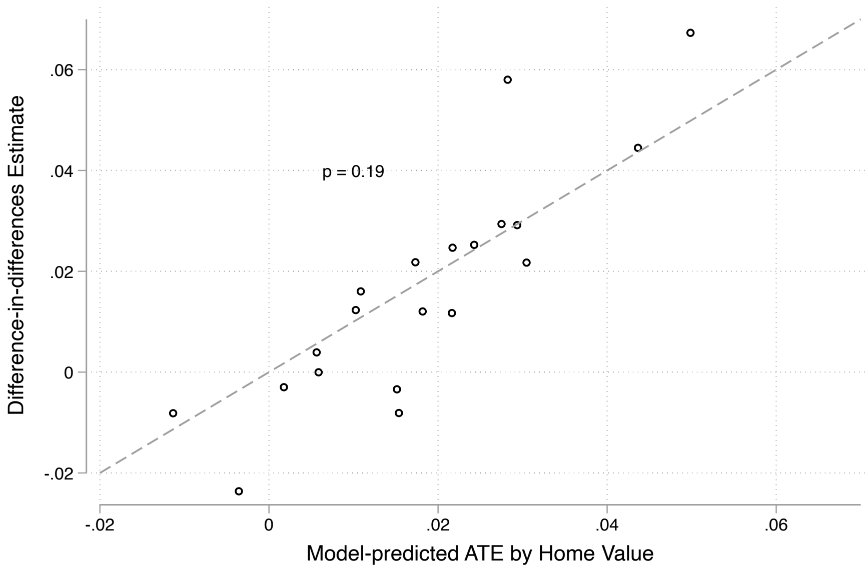
Note: This figure presents model fit and treatment effect estimates using a synthetic control method. The data is first collapsed to average number of tract-months with new development in a quarter in a city by tract type (where tract type can be OZ, eligible but not OZ, or ineligible). A synthetic control 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.

Figure 5: Other development responses



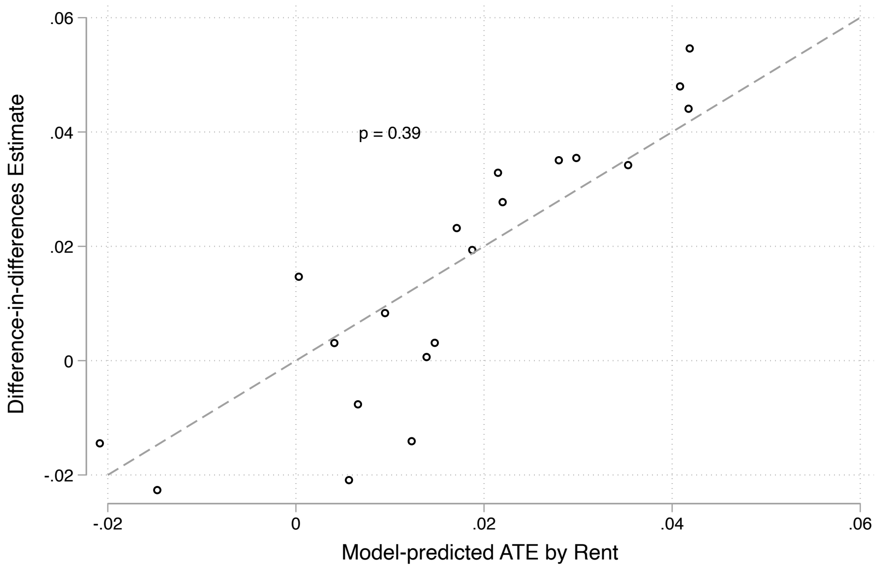
Note: This figure contains estimates of the OZ effect using the baseline difference-in-differences model on various outcomes. The top row uses as an outcome the number of new buildings, rows 2 through 4 use as outcomes an indicator for a new building, and whether its residential or commercial. Rows 5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

Figure 6: Model-predicted effects versus design-based effects



(a) Median home values

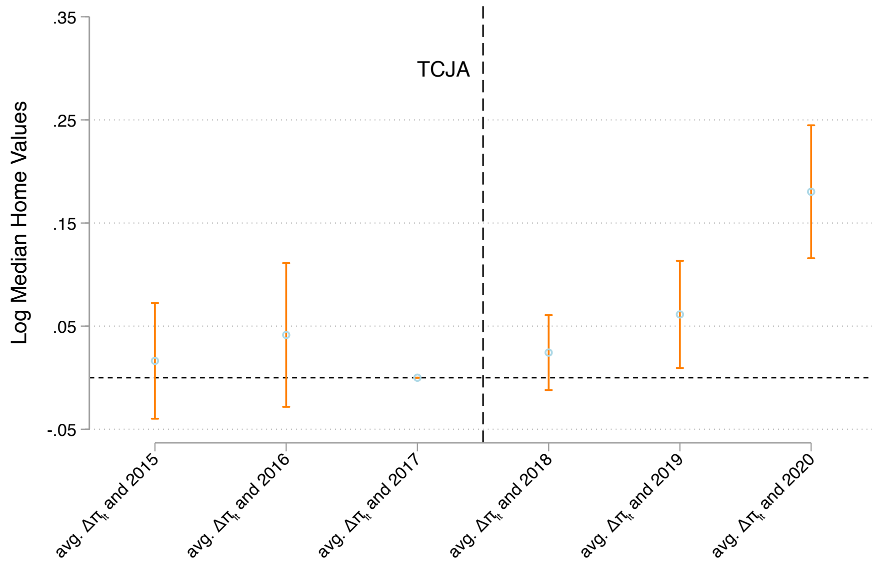
Note: This figure compares model-based estimates of the OZ effect by median home value vingtile with those from an interacted difference-in-differences model. The dashed line corresponds to the 45 degree line. The p-value comes from a test of the hypothesis that the difference-in-differences estimates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



(b) Rents

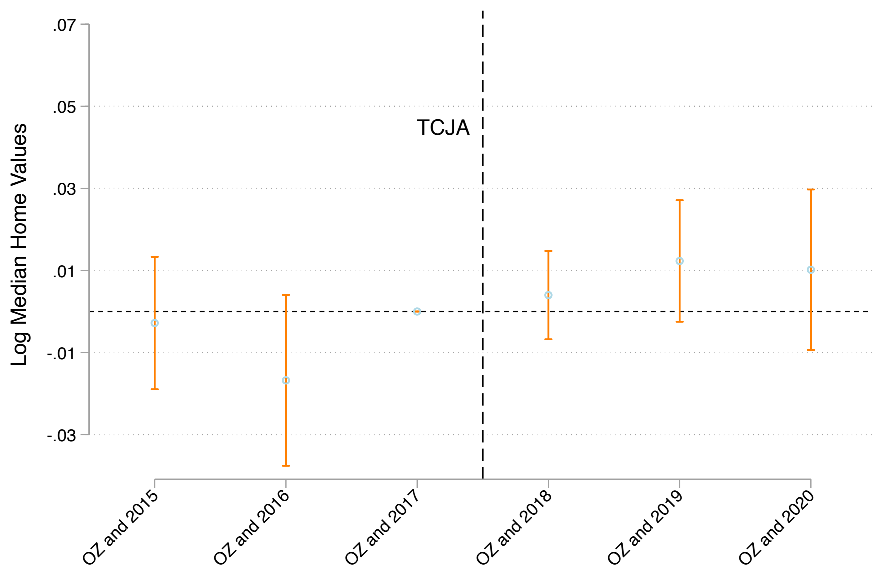
Note: This figure compares model-based estimates of the OZ effect by rent vingtile with those from an interacted difference-in-differences model. The dashed line corresponds to the 45 degree line. The p-value comes from a test of the hypothesis that the difference-in-differences estimates are equal to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

Figure 7: Log median home value changes



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

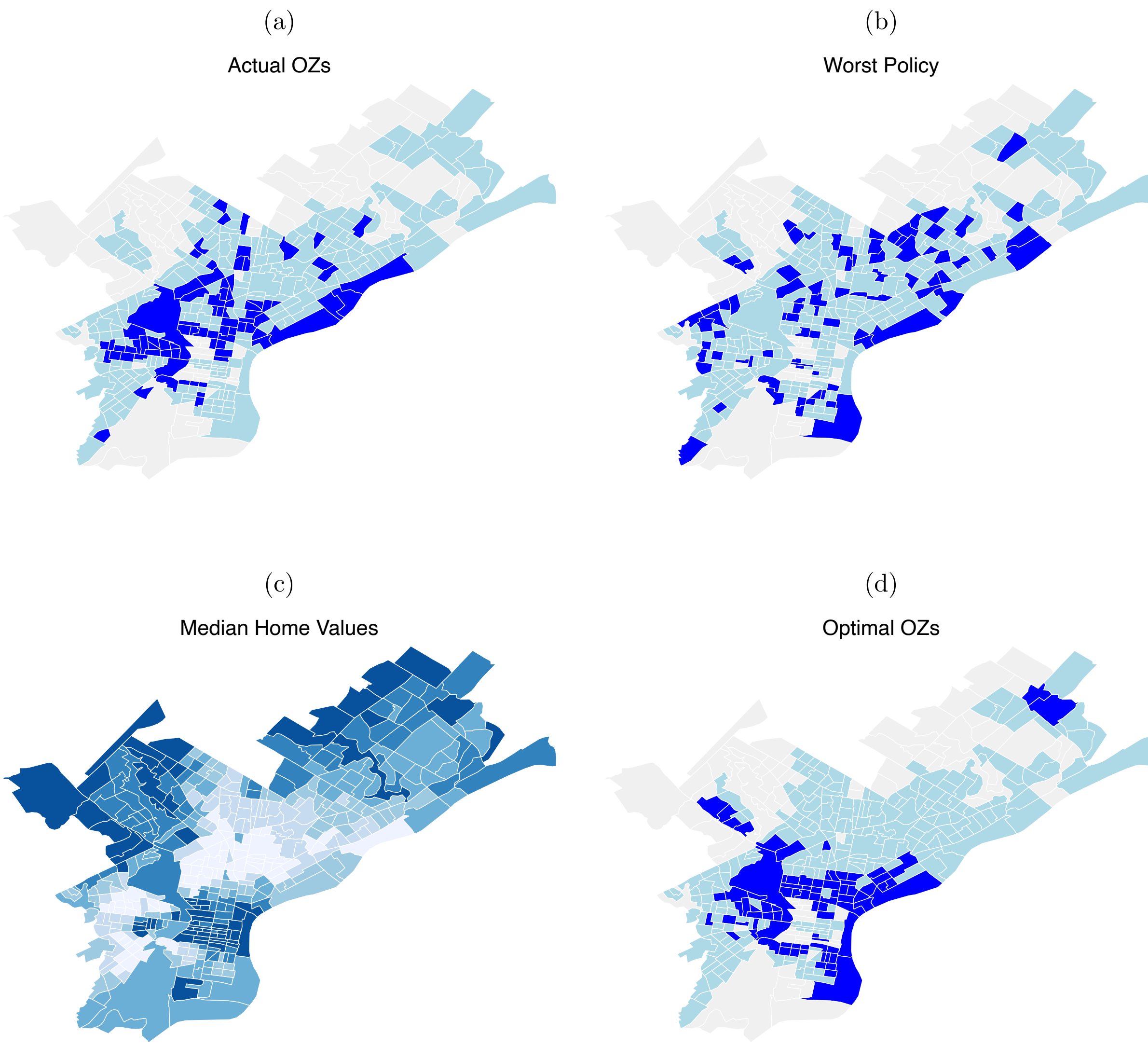
Note: This figure contains estimates from a difference-in-differences model where treatment is $\overline{\Delta\pi_i^\star}$. The sample only includes census tracts with median home value data for all years. The sample covers years 2015 through 2020. Errors are clustered at tract-level.



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

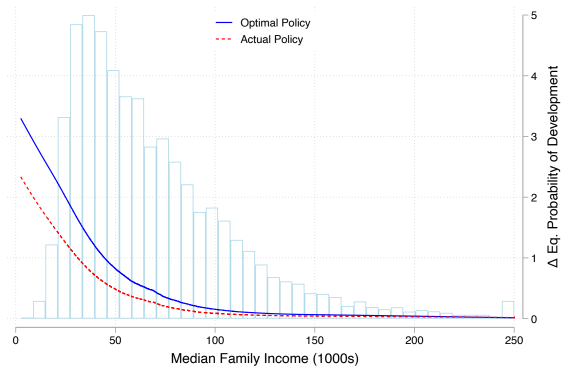
Note: This figure contains estimates from a difference-in-differences model where treatment is OZ status. I control for a quadratic in $\overline{\Delta\pi_i^\star}$ interacted with year. The sample only includes census tracts with median home value data for all years. The sample covers years 2015 through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

Figure 8: Philadelphia: actual, worst, and optimal OZs

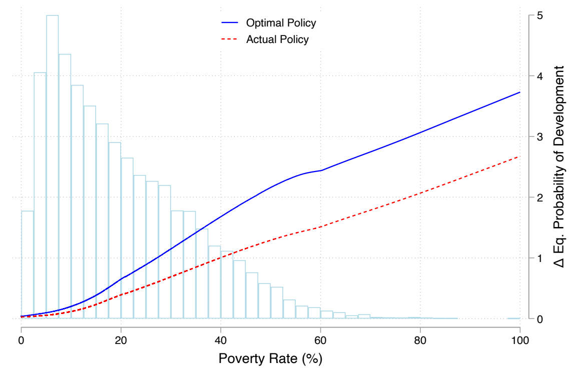


Note: these maps different OZ policies for census tracts in Philadelphia. In the top left are the actual OZs. In the top right are the worst OZs. The bottom left shows 2015 median home values by neighborhood. The bottom right depicts the optimal OZs. For the policy maps, ineligible neighborhood are in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

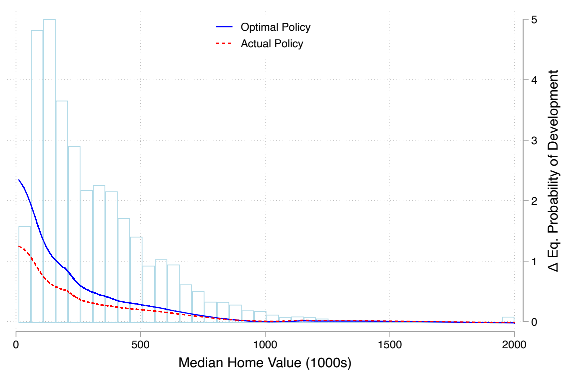
Figure 9: Actual versus optimal policy



(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.

Locally Optimal Place-Based Policies:
Evidence from Opportunity Zones

Online Appendix

Harrison Wheeler - UC Berkeley[1]

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A Additional Figures

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

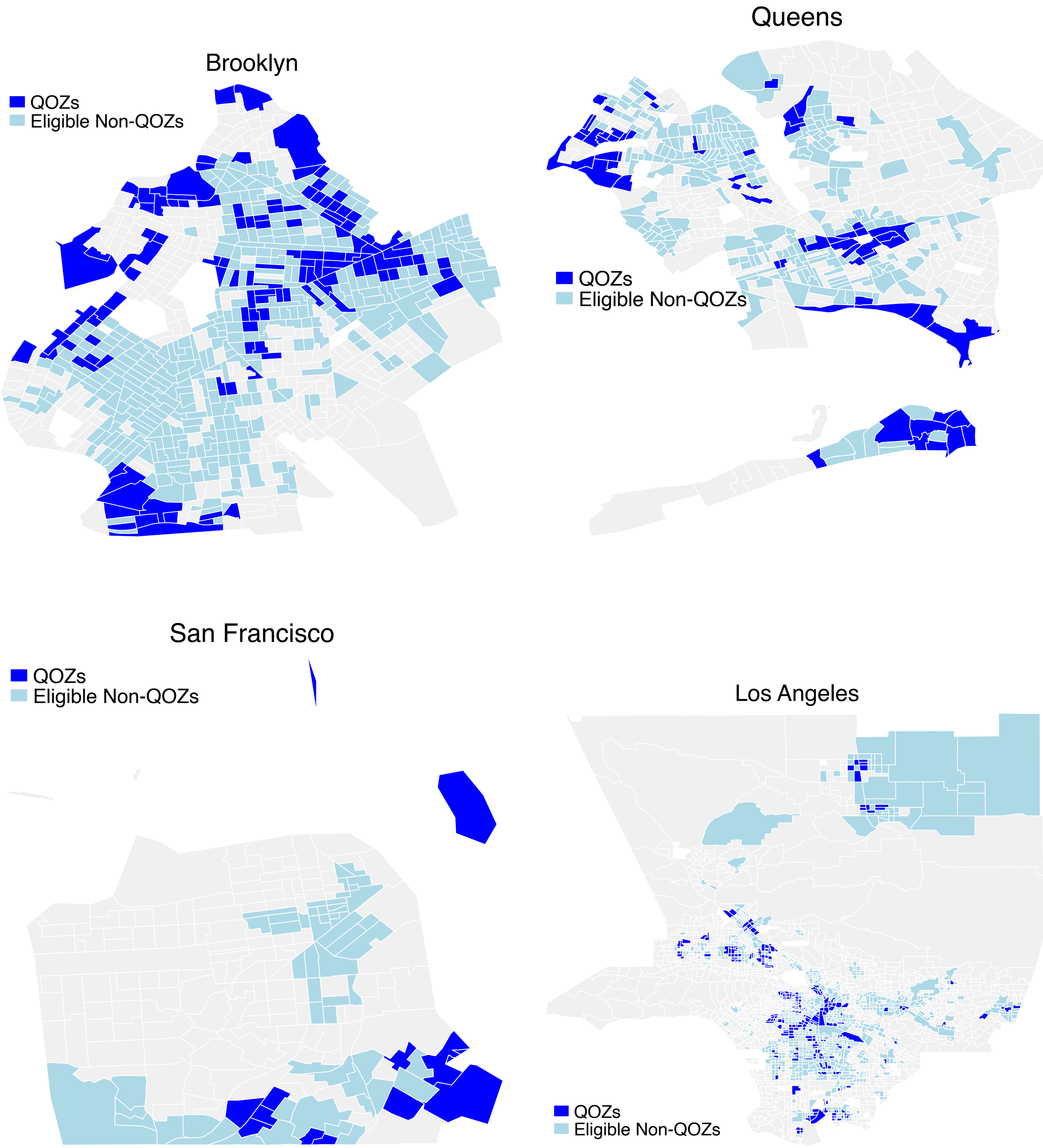
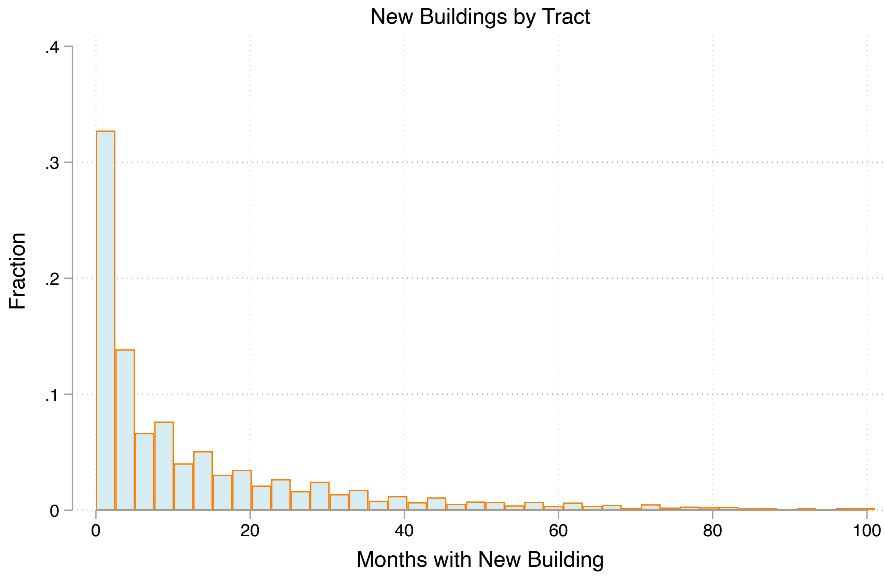
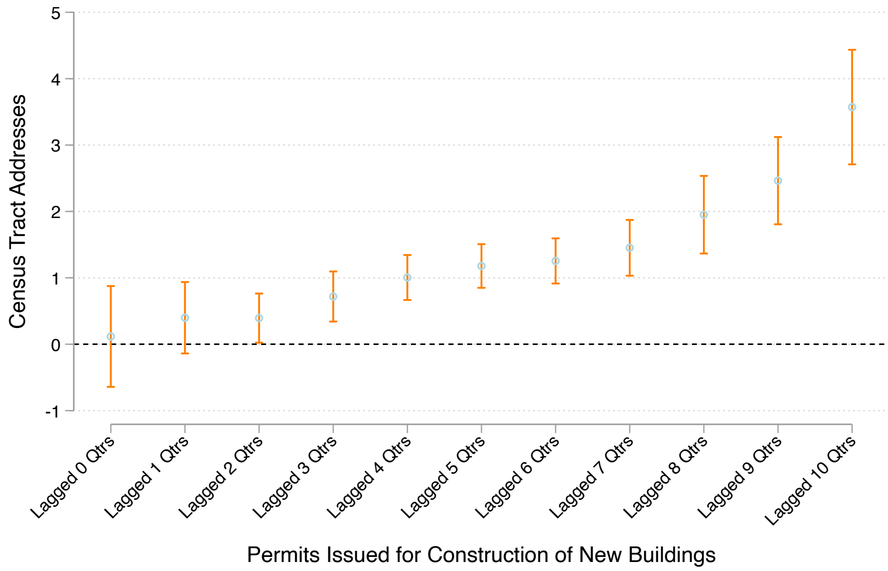


Figure A.2: Distribution of new development



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 A.3: Correlation of new construction measure and tract addresses

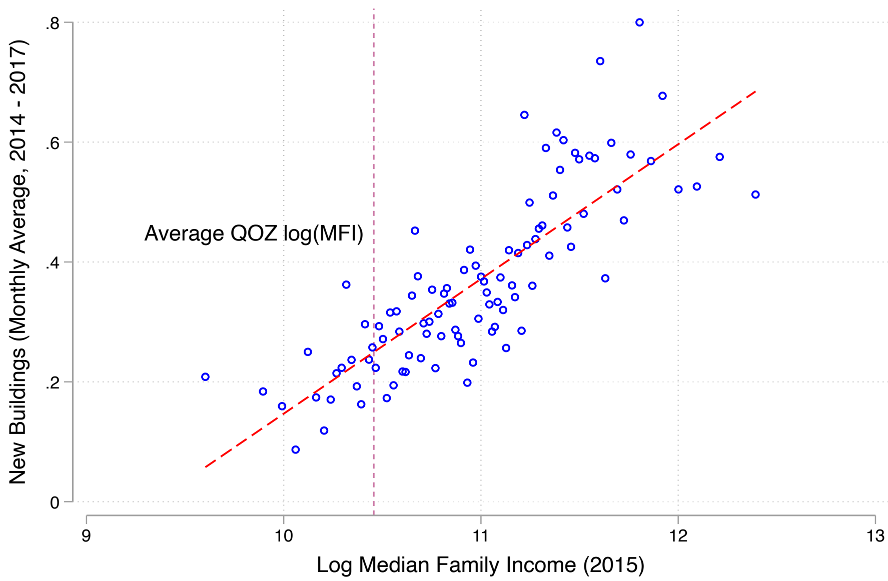


Note: This chart shows coefficients from a regression of total addresses in a census tract quarter on lags of number of permits issued for the construction of new buildings. The address data comes from HUD's USPS vacant addresses data. The regression includes tract and date fixed effects. Errors are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

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

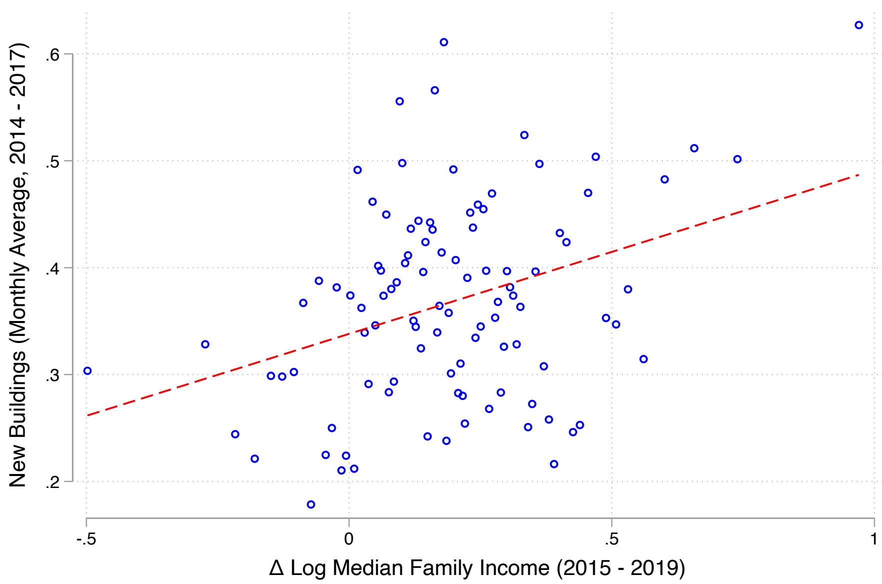
Note: This chart shows coefficients from a regression of "no-status" addresses in a census tract quarter on lags of number of permits issued for the construction of new buildings. The address data comes from HUD's USPS vacant addresses data. The regression includes tract and date fixed effects. Errors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

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



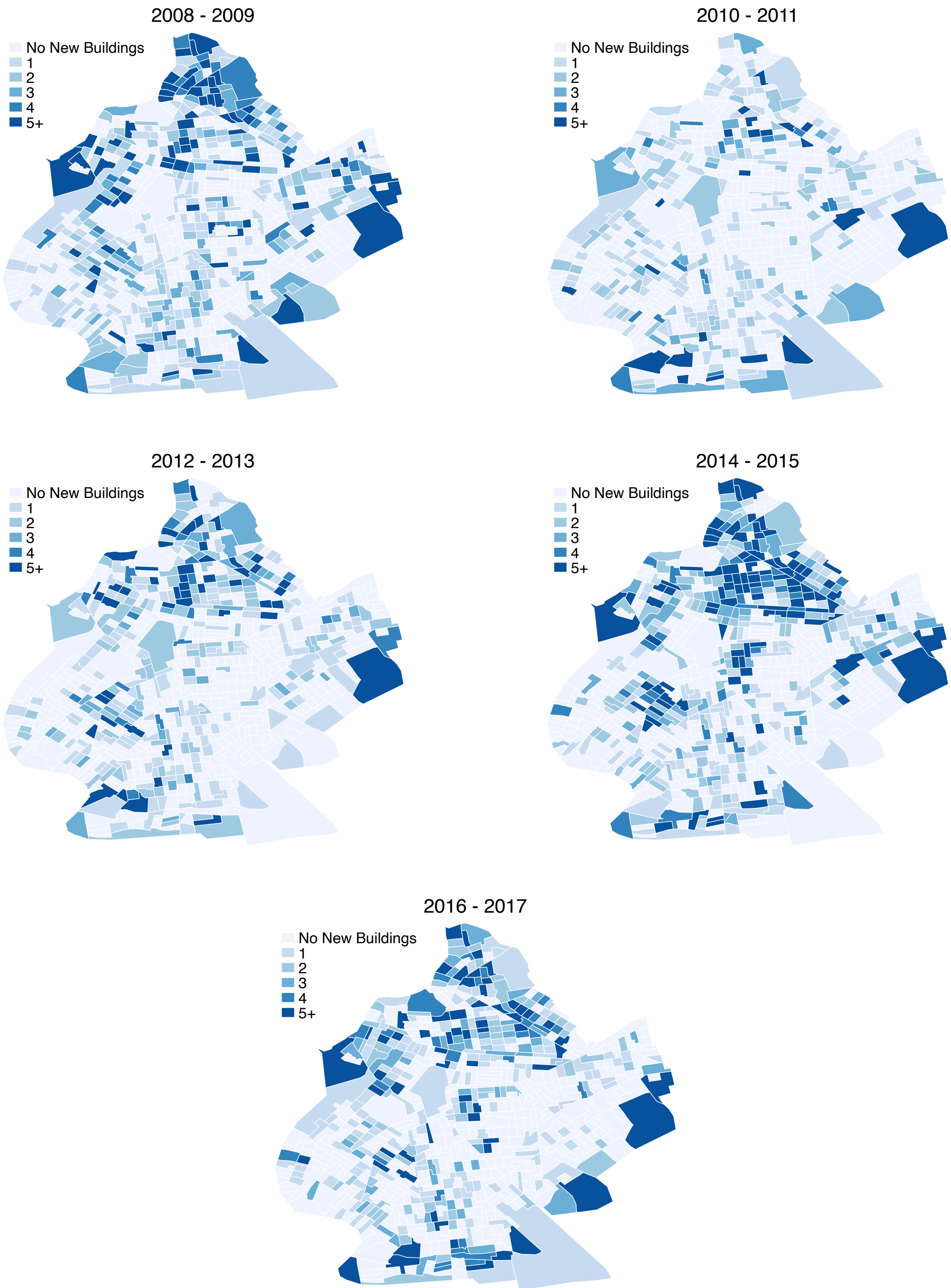
Note: This bin scatterplot shows ACS 2011-2015 tract-level log median family income against the monthly average of new buildings permitted for from 2014 to 2017. The dotted line denotes the average log median family income of Opportunity Zones. A line of best fit is depicted in red.

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



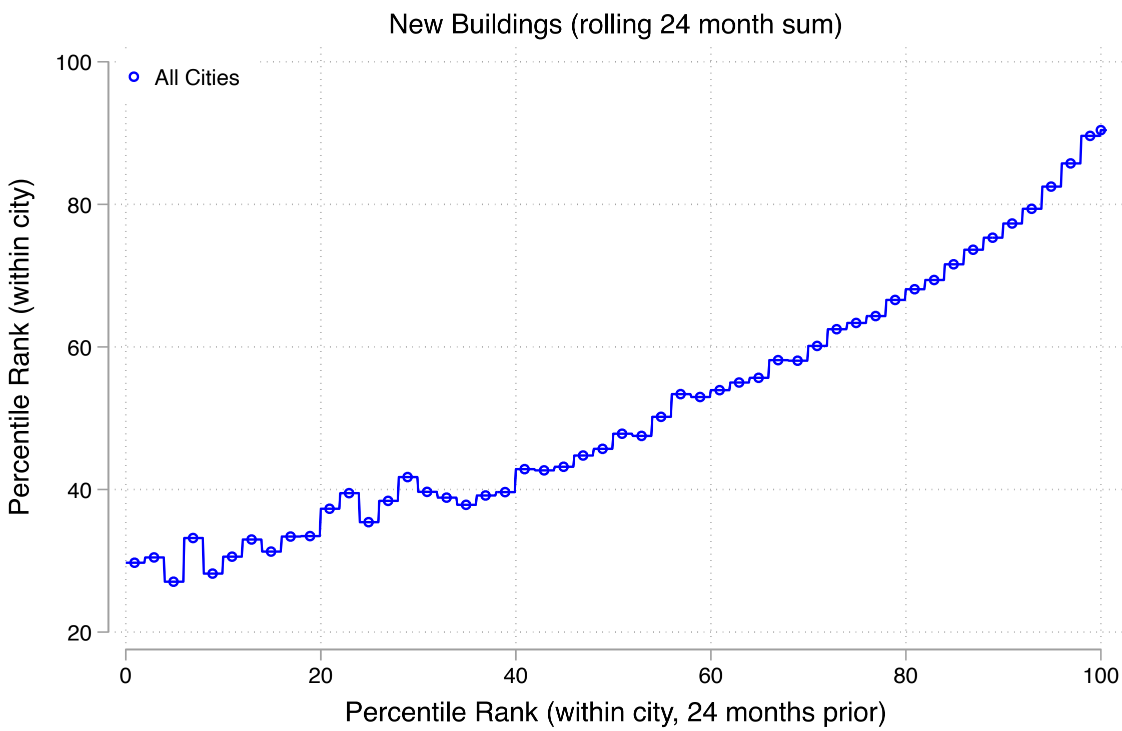
Note: This bin scatterplot shows the change in the log median family income from the 2015 to 2019 ACS against the monthly average of new buildings permitted for from 2014 to 2017. A line of best fit is depicted in red.

Figure A.7: New developments case study: Brooklyn



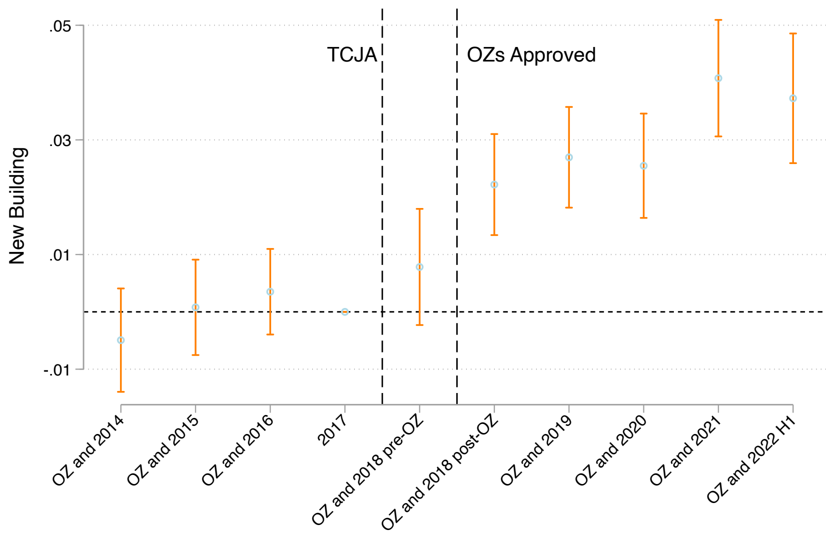
Note: These maps shows the number of new buildings over 2 year horizons for census tracts in Brooklyn.

Figure A.8: Persistence in new development



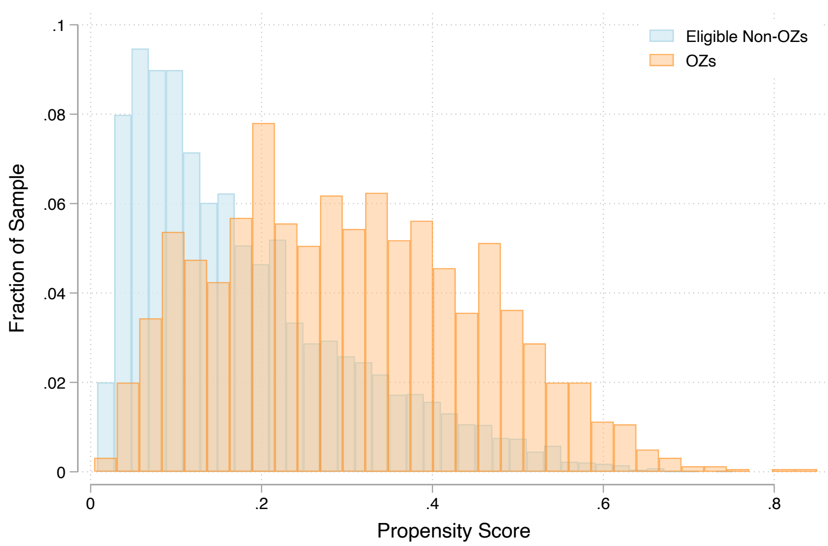
Note: These figures are produced by ranking tracts within cities in terms of the number of new buildings with permits issued in the previous 24 months. I then plot this percentile rank on its 24 month lag, and aggregate within 2-percentile bins across months.

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



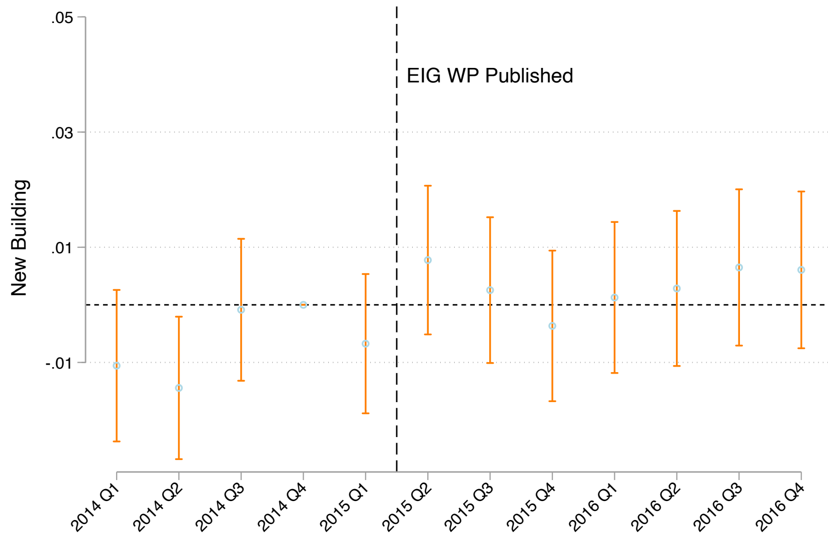
Note: This figure plots the annual version of the main difference-in-differences coefficients. However, a logistic model is run between any time period and right before the policy is implemented to estimate how ACS covariates affect whether the tract is in the sample or not. Observations are then reweighted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

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



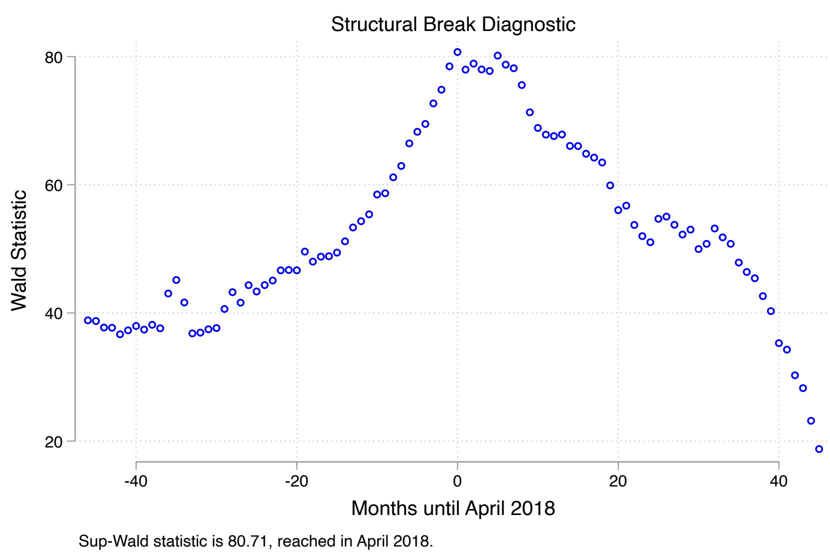
Note: This chart plots propensity scores for OZs against eligible non-OZs. Propensity scores were estimated via a logit model with 2015 5-year ACS tract-level demographics and local housing market covariates as predictors.

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



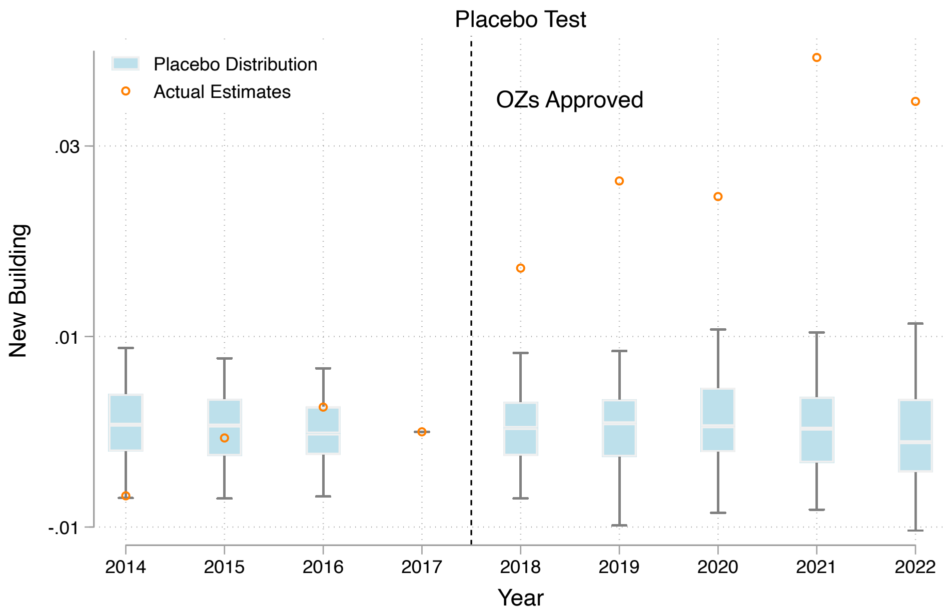
Note: This figure plots difference-in-differences coefficients from a version in which May 2015 (the publication date of the EIG white paper proposing the OZ tax credit) is the program implementation date. The model uses the same controls as the baselin specification: city linear trends, city seasonal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

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



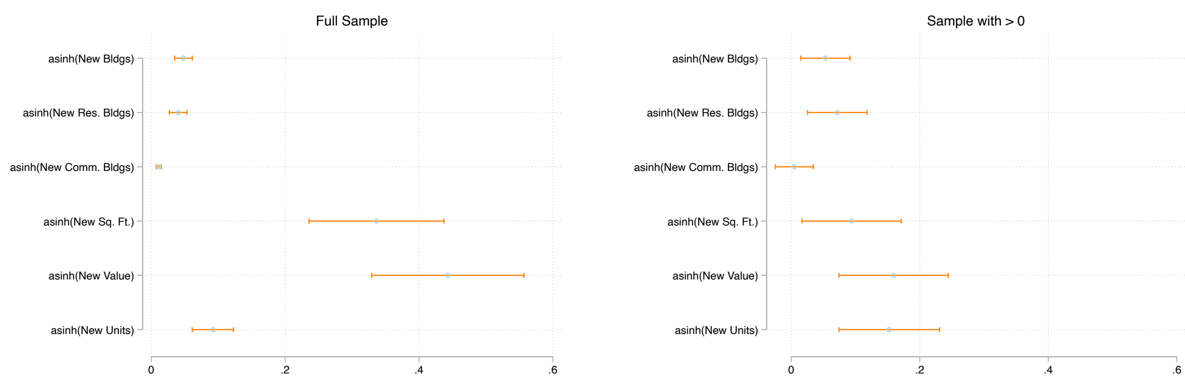
Note: This chart plots the Wald statistic for testing the null hypotheses that the coefficient on $\mathbb{1}\{i \text{ is a OZ}\} \cdot \mathbb{1}\{t \text{ is after } j\}$ in the baseline specification is zero, for each j from Jun 2014 to October 2019. All errors are clustered at tract-level.

Figure A.13: Placebo Tests



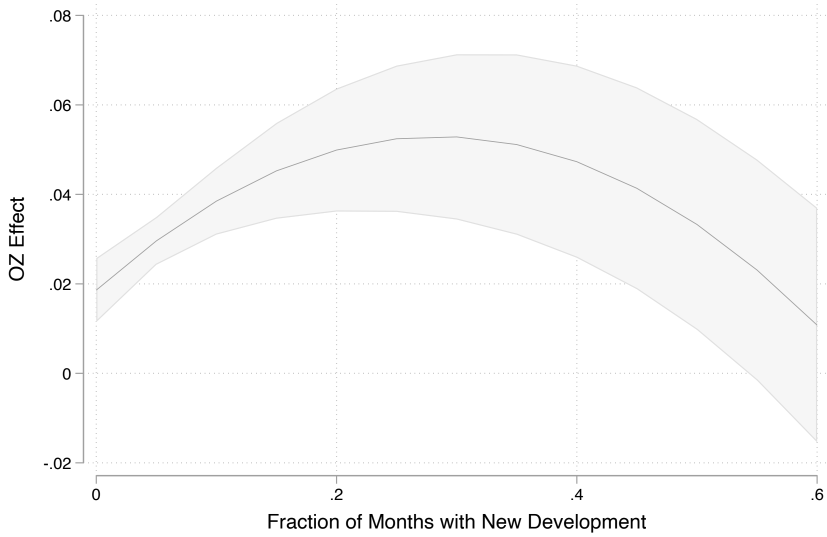
Note: This chart shows the distribution of point estimates from a series of placebo OZ programs. To implement, I simulate 100 different OZ programs by randomly drawing OZs from the population of eligible tracts (with probability equal to the fraction of eligible tracts that were actually chosen as OZs). 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 A.14: Intensive margins of response to OZ program



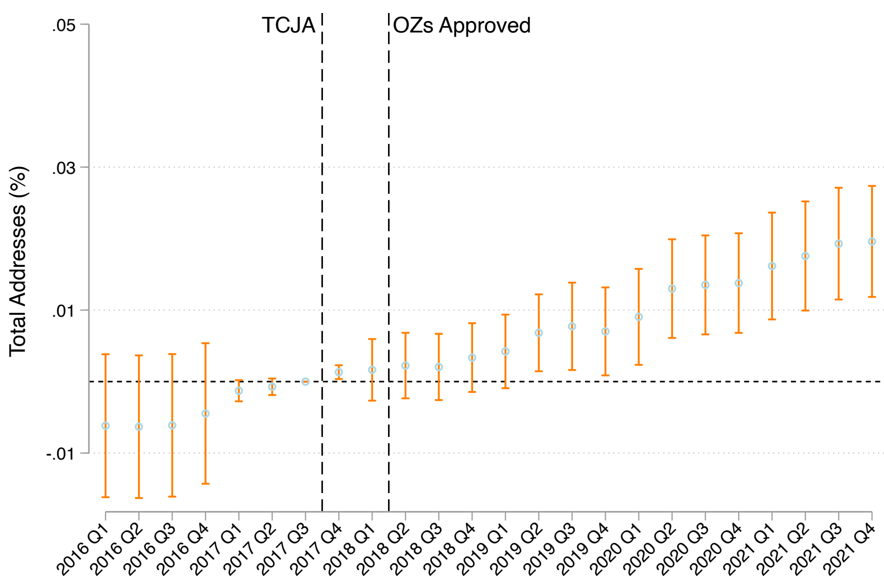
Note: These figures contain difference-in-differences estimates on various outcomes. The left hand chart runs these regressions on the full sample. The right hand chart conditions the sample to observations in which the outcome is greater than zero. All outcomes are transformed using the inverse hyperbolic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

Figure A.15: Heterogeneous policy response in prior development



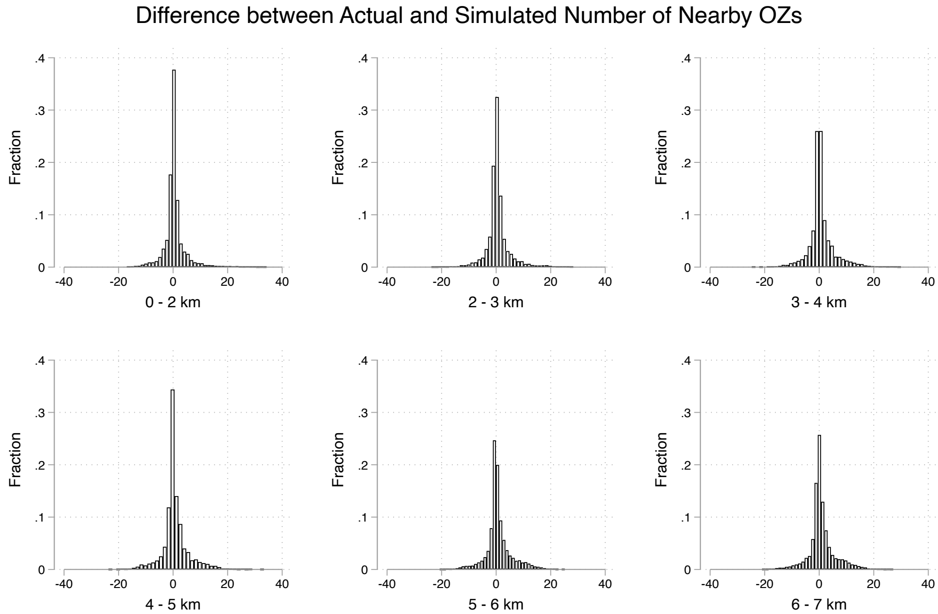
Note: This chart plots treatment effects from the regression model of Table 5. Errors are clustered at tract-level.

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



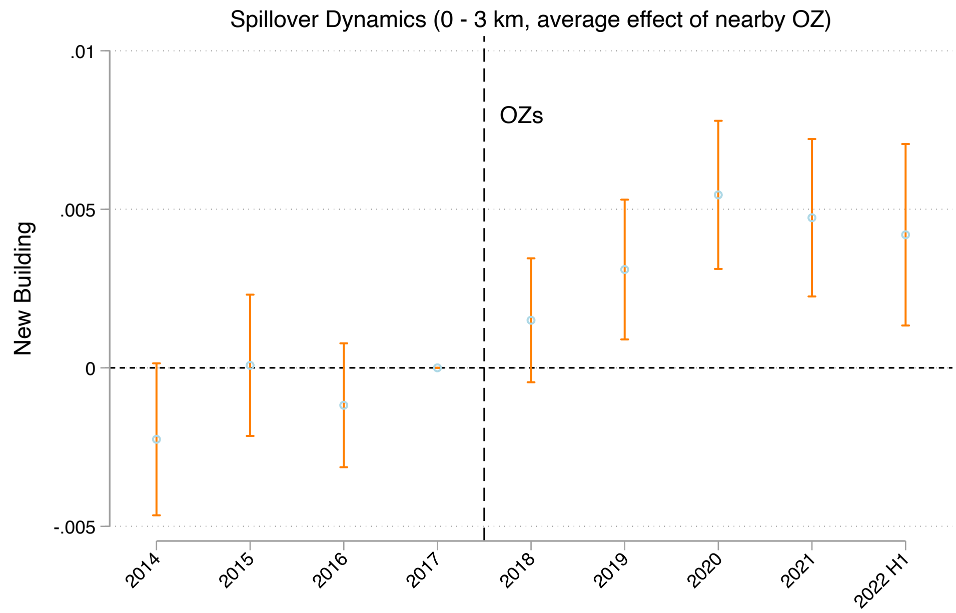
Note: This chart shows difference-in-differences coefficients from a poisson pseudo-maximum likelihood estimator. The outcome is total addresses that appear in a tract in a given quarter. Tract fixed effects, eligibility by month fixed effects, and city trends are included. All errors are clustered at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

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



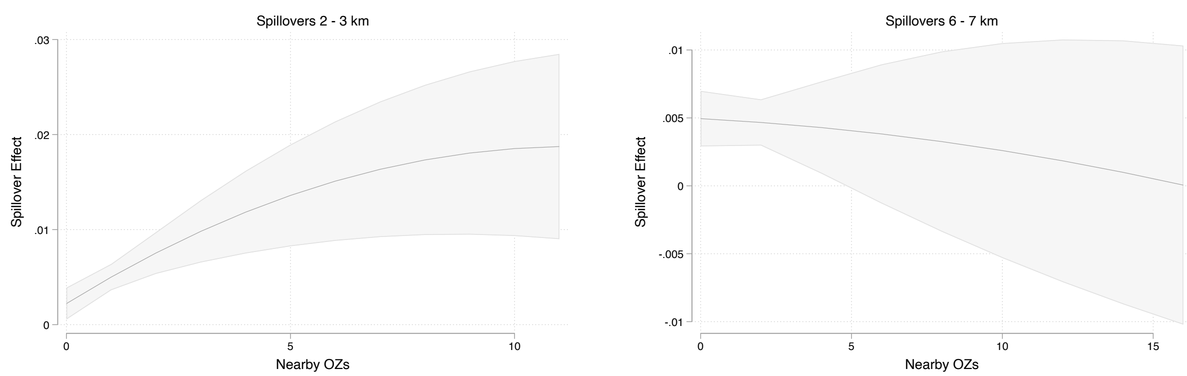
Note: This chart plots the distribution of differences between the actual number of OZs and the expected number of OZs across tracts and for different distance bands. The expected number of OZs is calculated by simulating OZ status among eligible tracts in a city according to the city-specific empirical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

Figure A.18: Spillover dynamics



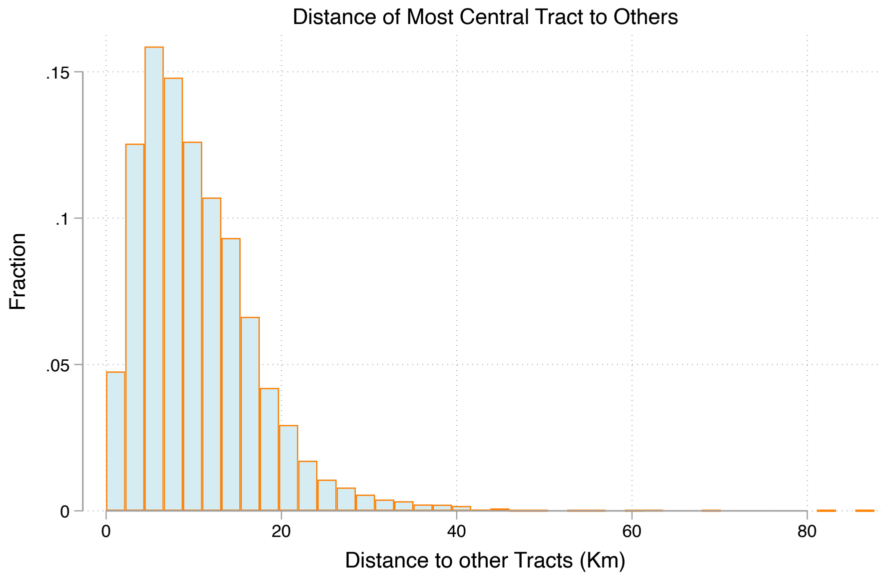
Note: This chart plots difference-in-difference coefficients from the main spillovers specification. The exposure to nearby OZs at various distances is interacted with year. I then scale (according to the average number of nearby OZs) and combine the coefficients for the distance bands 0-2 km and 2-3 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 A.19: Non-linearity in spillovers



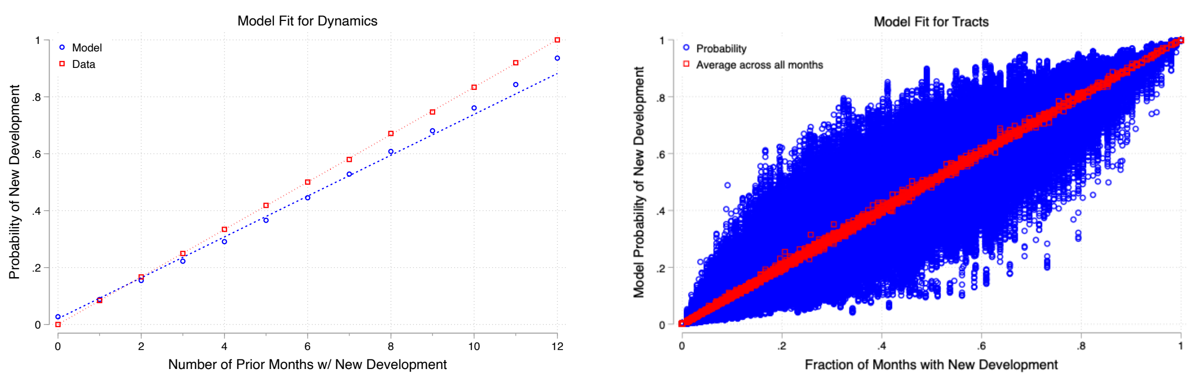
Note: This chart plots quadratic effects of having nearby OZs at 0-2 km (left) and 6-7 km (right). The main spillovers specification is augmented with a linear and quadratic term in the number of nearby OZs at various distances. Following Borusyak and Hull (2020), I control for the expected number of 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 A.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 A.21: Model fit

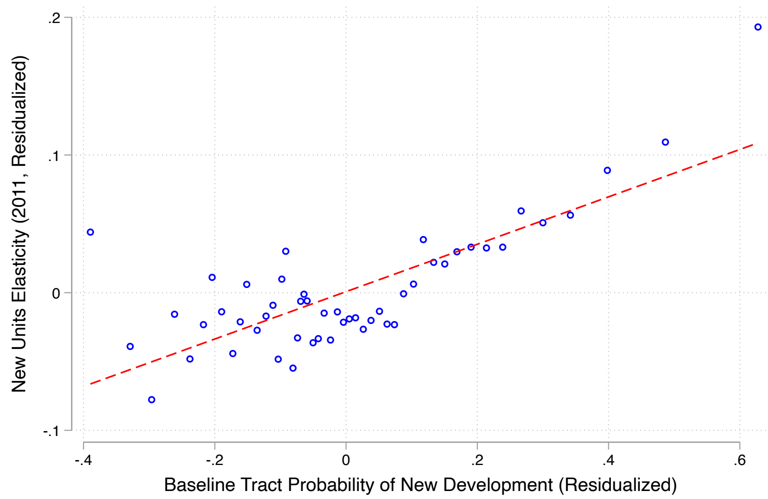


(a) Dynamics

(b) Heterogeneity

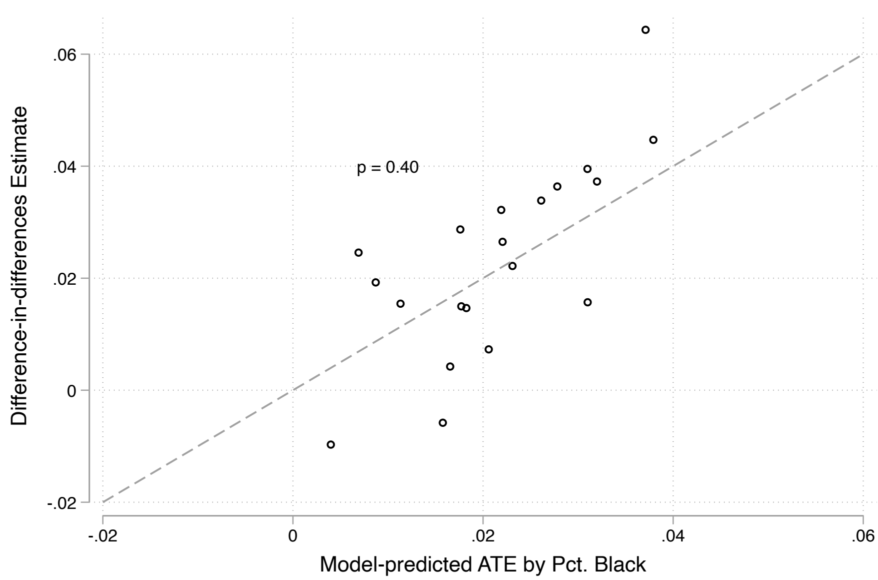
Note: This figure assesses the fit of the model to the data. Panel (a) plots the fraction of all months with new development (data, in red) and the model's estimated equilibrium probability of new development (model, in blue) against the number of prior months with new development. These probabilities 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 A.22: Model comparison with Baum-Snow and Han (2019)



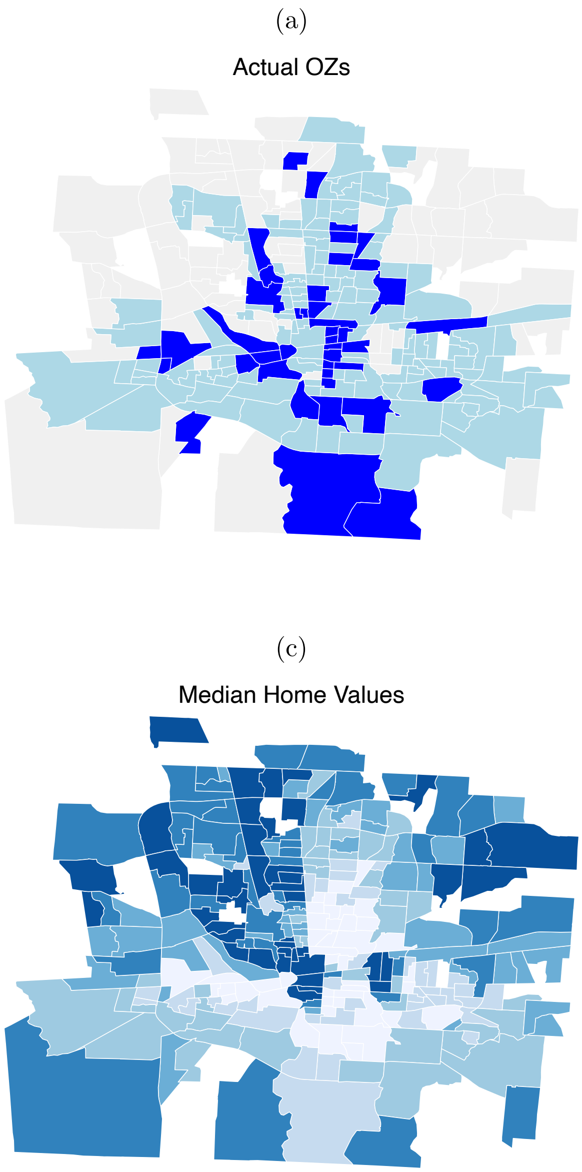
Note: This table compares housing supply elasiticity estimates from Baum-Snow and Han (2019) with baseline estimates of a tract's propensity to develop, calculated as the logit function applied to the model estimates of tract-heterogeneity. I use the elasticity with respect to new units, estimated via their "linear, IV" specification. Both are residualized on city fixed effects. I then plot a line of best fit.

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



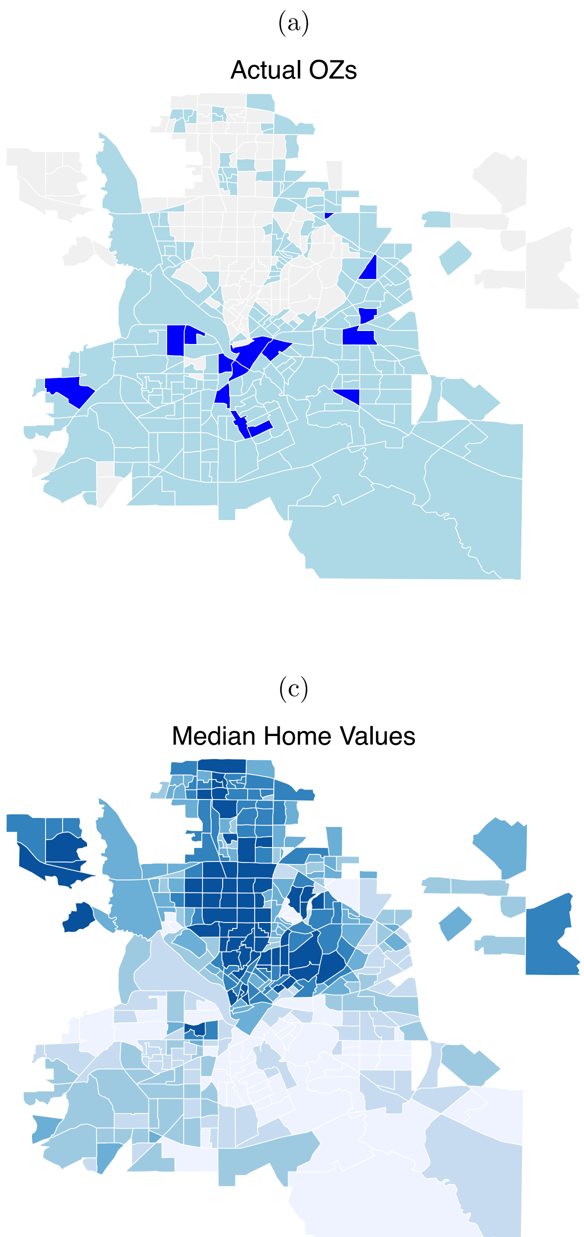
Note: This figure compares model-based estimates of the OZ effect by black population share vingtile with those from an interacted difference-in-differences model. The dashed line corresponds to the 45 degree line. The p-value comes from a test of the hypothesis that the difference-in-differences estimates 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 A.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 A.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 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) New Projects	(2) New Projects	(3) 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 Appendix D. 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) New Building	(2) New Building	(3) New Building	(4) 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, 2019), 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) Log Home Value (Q25)	(2) Log Home Value (Q50)	(3) Log Home Value (Q75)	(4) 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. (2019) comparison using log-levels

	(1) Log (HPI)	(2) Log (Home Val. Q50)	(3) 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. (2019) comparison using first-differences

	(1) Δ Log (HPI)	(2) Δ Log (Home Val. Q50)	(3) 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. (2019), 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) Δ Log MFI	(2) Δ Log Pop.	(3) Δ Log Home Value	(4) Δ % Poverty	(5) Δ % College	(6) Δ % High School	(7) Δ % White	(8) 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, 2019), 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.

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 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.

C 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 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.[2] 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 involved 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

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

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.[3] 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 A.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.[4] 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.[5]

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

[4] 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.

[5] Additionally, the USPS collects information on "no-status" addresses, which can include those under construction but not occupied. In Figure A.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).

D 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.[6] I can identify developers or contractors for 37 of the 46 cities in my sample.[7]

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

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

[7] 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.

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.

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

α_{ig} denote developer-tract type fixed effects and η_{it} denote developer-month fixed effects. T_i is an indicator for the "treatment" group, which varies across specifications. β 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 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 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 A.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 4.1 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 A.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 A.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.[8]

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

E 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.[9] 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 θ, and so can be dropped from the estimate for the asymptotic variance. The remaining term gives an estimator for the information matrix as

[9] 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.

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

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

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 δ 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, θ_k denotes an iterative guess of θ, not the kth 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` (θ_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` (θ_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`(θ_{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.