

LOCALLY OPTIMAL PLACE-BASED POLICY: 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 exhibit a significant degree of heterogeneity across neighborhoods. Through a model of new development that accounts for location-heterogeneities, dynamics, and localized spillovers as well as 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 nearly doubled the effect. 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 (Gaubert et al., 2021; Reardon and Bischoff, 2011). Declining migration rates, and local impacts from aggregate shocks like the Great Recession (Yagan, 2019) and trade liberalization (Autor et al., 2016) have entrenched such differences (Moretti, 2011; Ganong and Shoag, 2017). Neighborhoods that struggle to attract well-paying jobs and infrastructure investment continue to decline (Glaeser and Gyourko, 2005). High-poverty neighborhoods worsen the health of adult residents (Ludwig et al., 2012) and can, in turn, have deleterious effects on the education, labor market outcomes, and criminal behaviour of children who grow up in them (Kling et al., 2005; Chetty et al., 2016; Chetty and Hendren, 2018a,b). Consequently, policymakers have shown interest in programs that encourage investment and employment in distressed areas (Austin et al., 2018).

What can place-based policies do that redistributive income transfers cannot? First, they can lead to efficiency gains from mitigating local market failures due to public goods provision, endogenous amenities, agglomeration economies, and labor market frictions (Kline and Moretti, 2014; Fajgelbaum and Gaubert, 2020). Second, they can reduce the efficiency cost of redistribution (Gaubert et al., 2019). However, the evidence on whether place-based policies can even 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 the program (i.e. which neighborhoods receive hiring credits, tax incentives, etc.) with its impacts.¹ These concerns are especially relevant when policies are implemented at fine geographic levels, where spatial inequality is more pronounced, the choice set of neighborhoods is large, and there is significant scope for attracting or deterring economic activity in nearby locations.

This paper studies the effectiveness and design of the recently implemented Opportunity Zone (OZ) program. Passed as part of the Tax Cuts and Jobs Act (2017), the OZ program provides a capital gains tax credit for investments made in more than 8,000 high-poverty neighborhoods across the U.S. Qualified investments could either be equity in businesses that largely operate in an OZ, or – the focus of this work – the development of properties located in OZs. The Congressional Joint Committee on Taxation estimates that the incentive will reduce tax revenue by an average \$3.4 billion per year from 2019-2023 (JCT, 2019), a cost significantly larger than previous and existing national place-based policies.² The program's scope and magnitude offer an ideal setting to study

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

²See for example Empowerment Zones (Busso et al., 2013) and the New Market Tax Credit (Freedman, 2012).

whether policies can drive investment into neighborhoods that have historically lacked it.

This paper contributes to the place-based policy discussion in several parts. First, I collect new data on the timing and location of development projects for large U.S. cities. Second, I present novel evidence that the OZ program has had a significant effect on new development in designated areas. Third, I document the existence of positive spillovers - that is, new development increases in nearby areas. Fourth, both impacts vary substantially across neighborhoods. Fifth, I build a spatial-equilibrium model of developers deciding whether to start 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 selections for the tax credit. In a final section, I describe the city policymaker's optimal problem to choose neighborhoods for OZ designation. I delineate how these optimal choices differ and improve upon which locations were actually designated for the tax credit.

Why focus on the new residential and commercial construction? First, new development constitutes a real form of investment explicitly targeted by the program and is the vast majority of OZ investment so far (Kennedy and Wheeler, 2021). Second, new housing investment is persistent across time and strongly correlated with urban growth (Glaeser and Gyourko, 2005; Hsieh and Moretti, 2019; Hornbeck and Keniston, 2017), making it a natural target to address spatial inequalities. These facts are even more pronounced within cities, as I discuss in Section 3. Third, market failures may arise in new developments through coordination failures (Owens III et al., 2020) and externalities (Fu and Gregory, 2019; Pennington, 2020).³ The tax credit could coordinate investment decisions through its salience, or ensure developers internalize these externalities.

To study this outcome, granular data on new construction in census tracts is necessary.⁴ 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. Construction data at the city-level (Glaeser and Gyourko, 2003) or for a single city have been used to study local housing markets before. However, compiling disparate data sources to track development across a large number of U.S. cities is, to the best of my knowledge, a contribution of this paper. At the level of neighborhood-month, new development is sparse. Thus, my main outcome throughout the paper is an indicator for whether a census tract has new construction for a

³This is consistent with a large literature finding localized spillovers in housing markets. The role of public housing (Diamond and McQuade, 2019), large market-rate apartment buildings (Asquith et al., 2019), new construction from building fires (Pennington, 2020), rent control (Autor et al., 2014), urban revitalization programs (Rossi-Hansberg et al., 2010), and foreclosures (Campbell et al., 2011) have all been studied.

⁴Census tracts is the geographic-level at which OZs were designated. Tracts that were approved for the tax-incentive are referred to as "Opportunity Zones."

residential or commercial building in a given month.⁵ The main estimation sample covers roughly four years prior and three and a half years after the program was announced.

I first document the direct effect of the tax credit on new development. I employ a difference-in-differences design, relying on other high-poverty neighborhoods that were eligible for the tax credit, but not designated, as a comparison group.⁶ The program's surprise announcement, the little time governors had to designate areas, and the limited guidance on its implementation, make it all the more likely that these comparison neighborhoods were trending similarly to OZs prior to the program.⁷ Reassuringly, there is no evidence of systematic differences in new construction between OZs and comparable areas in the years, quarters, and months leading up to the program.

After OZs were approved, I find a significant, large, and immediate effect of the tax credit on new development. The main estimate is a 2.9 percentage point (pp), or 20.5%, increase in the monthly probability of new development. These results 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. Using the same difference-in-differences design, I also find that median home values increased 3.4% in OZs by 2020, relative to 2017.

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 increases over time and with developable land and local supply elasticities, while declines with local home values. Furthermore, the policy effect exhibits an inverse U-shaped relationship in the amount of development happening prior to program implementation i.e. 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. New commercial developments improve local services and employment opportunities, which in turn increase demand for adjacent residential and commercial space. However, through en-

⁵The data contains information on the number of such projects, and for some cities, its square footage, number of new units, and estimated construction costs. However, the extensive margin appears to be the most important for studying the program response.

⁶This approach is taken in other early studies of the OZ program. See, for example [Chen et al. \(2019\)](#); [Atkins et al. \(2020\)](#); [Arefeva et al. \(2020\)](#); [Freedman et al. \(2021\)](#).

⁷By focusing on short-run outcomes, there are unlikely to be other time-varying factors that disproportionately impacted new construction in OZs relative to the control group.

couraging new development in targeted neighborhoods, 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). I adapt insights from Borusyak and Hull (2020) to generate quasi-experimental variation in nearby OZs. Having any nearby OZ is associated with a 1pp (6%) increase in new development within 2 kilometers of the OZ centroid; the effect decays towards zero after 3 kilometers. The evidence suggests that in this context, demand externalities far outweigh supply effects.⁸ The spillovers take a few years to set in, face diminishing returns in the number of nearby OZs, and like the direct effects of the program, are significantly declining in local home values.

Estimates of the program’s direct and indirect effects are not enough to move to counterfactual investment behavior. First, the indirect effects could attenuate our estimates of a neighborhood’s response to new development, and how that varies with neighborhood characteristics. This contamination complicates aggregating effects up to the city-wide response. Second, how the direct and indirect effects vary, with for example local home values, could reflect two sides of the same coin. Low home value areas could see a larger response to the tax credit because they have cheaper, under-utilized land, or because they are surrounded by other low home value areas, for which positive externalities are larger. The two possibilities have different implications: in the first, the policymaker would prefer to target low home values areas for the tax credit, while in the second, they will look for areas with many low home value neighborhoods nearby to magnify the spillovers. Third, while the empirical analysis uses variation in nearby OZs to estimate spillovers, the mechanism is through changes in nearby development itself rather than the tax credit. A model is needed to jointly summarize these reduced-form facts, and how they change in equilibrium, to be able to consider counterfactual policies.

I model new construction as arising from strategic decisions made by developers at locations within a city.⁹ Developer profits from building depend on prior new development, location fundamentals, the tax credit, and the behavior of other developers in the city.¹⁰ 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

⁸This 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).

⁹The model follows a small literature in treating developers as strategic agents interacting within the city (Henderson and Mitra, 1996).

¹⁰The role of heterogeneity, dynamics, and spillovers have long been discussed in explaining urban phenomena (Davis and Weinstein, 2002; Bleakley and Lin, 2012; Allen and Donaldson, 2018).

model provides a rich set of equilibrium interactions, including the possibility of multiple equilibria.¹¹ 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.¹² The model is flexible in its characterization of neighborhood responses to the tax credit, but tractable.¹³

I find a significant role in explaining new development through location fundamentals, prior development, spillovers, and the direct effect of the OZ tax credit. The model does well to rationalize several features of the data. First, it can replicate observed neighborhood heterogeneity in new development, as well as the difference-in-differences estimate of how the OZ program increased 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 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%.

There are efficiency gains from lowering federal capital gains taxes ([Harberger, 1962](#); [Cummins and Hubbard, 1994](#)). Under the OZ program, this increased investment is channeled into high-poverty neighborhoods. In the final section of this paper, I ask: how could city policymakers have more efficiently targeted neighborhoods for the tax credit? [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.¹⁴ Since I focus on the extensive margin response to the program, changes in equilibrium latent profits to building induced by the OZ tax credit should reflect changes in land values. While I do not know prices for unoccupied land, I do find that the change in equilibrium latent profits significantly predicts home value increases after the program is announced.¹⁵ Moreover, home value increases in OZs are entirely explained by this object.

The city planner's optimal policy problem can be stated as follows: which neighborhoods should be selected for the investment tax credit, given a fixed number of neighborhoods to select from a pool of program-eligible ones (i.e. sufficiently low-income and high-poverty), to maximize aggregate

¹¹Multiple 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](#)).

¹²Addressing 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.

¹³The estimation and identification of strategic games has been discussed in [Bajari et al. \(2010a\)](#), [Bajari et al. \(2010b\)](#), and [Bajari et al. \(2015\)](#).

¹⁴See, for example, ([Smith, 2020](#)).

¹⁵Reassuringly, the change in equilibrium latent profits is not predictive of home values prior to the program.

equilibrium developer latent profits, and by extension home values? This is a question that has largely been overlooked in the literature on place-based policies, in favor of efficiency considerations of a program altogether (Fajgelbaum and Gaubert, 2020) or redistributive motivations (Gaubert et al., 2019). However, this objective is fully consistent with the program’s intended purpose, to “drive private investment into our nation’s most distressed zip codes.”¹⁶ 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.¹⁷

I find that under the optimal policy, the aggregate impacts on city-wide new development would have nearly doubled and home values would have increased 1.1%. 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 returns in spillovers from 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 neighborhoods than was necessary under the optimal program. As an additional counterfactual, I find that the worst policy increases new development in cities by only 0.4%. This finding shows how critical the spatial design of place-based policies is to its impacts, and can rationalize the mixed evidence on place-based policy effectiveness in other contexts (Neumark and Simpson, 2015).¹⁸

A growing literature has explored the effects of the OZ program. While it is early to expect large labor market impacts from the policy, a few studies have found mixed results on wages and employment so far (Atkins et al., 2020; Arefeva et al., 2020; Freedman et al., 2021). Conflicting effects on local housing prices have been found as well (Casey, 2019; Chen et al., 2019).¹⁹ Sage et al. (2019) find that while property prices generally did not increase, they increased some 10-20% for redevelopment sites and vacant plots.²⁰ Like many of these works, I estimate the causal effect of the program using a difference-in-differences strategy relying on comparisons between OZs and neighborhoods that were eligible, but ultimately not designated with the tax credit. However, my work estimates the direct and indirect effects on a novel and policy-relevant outcome – new

¹⁶Taken from Senator Tim Scott’s website, one of the authors of the OZ program. <https://www.scott.senate.gov/opportunityzones>

¹⁷The optimal policy problem and context is similar to Fu and Gregory (2019)’s study of rebuilding susidies in the wake of Hurricane Katrina.

¹⁸See for example, (Freedman et al., 2021; Busso et al., 2013; Neumark and Kolko, 2010; Briant et al., 2015).

¹⁹Chen et al. (2019) find no effect on local housing price *growth* in OZs using Federal Housing Finance Agency data. They focus on the entire U.S., while I focus on a subset of large, urban areas where the effect is likely to be the strongest. They also find mixed evidence on residential permitting at the census place-level.

²⁰Two benefits of primarily focusing on new development projects are that (i) while prices should be forward-looking, they may be slow to adjust, and (ii) new development constitutes physical investment rather than market expectations of investment behaviour. However, I do find effects on home values as well.

development. Moreover, I focus on alternative program designs.

The rest of the paper is organized as follows. [Section 2](#) provides context for the Opportunity Zone program, while [Section 3](#) describes the data sources used. [Section 4](#) presents reduced-form evidence on the direct and indirect effects of the investment tax credit on new development. [Section 5](#) discusses the model of new development. [Section 6](#) presents the model estimates. [Section 7](#) describes the optimal policy framework and presents policy counterfactuals. [Section 8](#) concludes.

2 Opportunity Zones

Place-based policies have used various instruments to encourage economic activity in distressed areas. 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 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](#)). Starting in 2001, The New Markets Tax Credit (NMTC) provides tax benefits for investments in designated low-income communities, as facilitated by intermediaries known as Community Development Entities ([Freedman, 2012](#)). The Opportunity Zone program arose from the basic NMTC framework, but with several changes meant to improve its efficacy.

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 lead a bipartisan group of lawmakers in sponsoring the bill,²¹ 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 on average from 2019-2023 ([JCT, 2019](#)). Total investments claiming OZ tax credits is an order of magnitude larger than the predicted federal costs, with \$24.9 billion in 2019 alone ([Kennedy and Wheeler, 2021](#)).

The broad structure of the program is to provide tax incentives for reinvesting capital gains in distressed neighborhoods. The program provides three incentives: (i) a tax deferral on capital gains, (ii) a step-up in basis on reinvested capital gains, and (iii) the elimination of capital gains

²¹Their statement can be found [here](#).

taxes on the new investment if held for at least 10 years. Investments are coordinated through Qualified Opportunity Zone funds. The maximum tax benefits could be achieved for investments made in 2018 through 2021. To receive the credit, capital gains can either be invested directly in the equity of firms operating in OZs (Qualified 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 developments to be the first form of investment to take advantage of the program.²² 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 wave 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 to either make “substantial improvements” to the property or the “original use” of the property must begin 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.²³ 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 is 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 acquiring land.

The program was designed to encourage investment into 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.²⁴ 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

²²New York Times coverage can be found [here](#).

²³While 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.

²⁴For 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.

a state.²⁵ 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 eligible tracts for OZ designation. This nomination process varied across states. Some governors chose directly, some deferred to lower administrative units, while others required applications from local authorities.²⁶ From April until June, the IRS released lists of approved census tracts; virtually all of the nominated tracts were approved. [Figure A.1](#) includes maps for a few cities with their eligible neighborhoods and their chosen OZs.

3 Data

In order to study the new development response to the investment tax credit, I need 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 in census tract and OZ program characteristics.

3.1 Sources

Opportunity Zone Details: Eligible and chosen census tracts come from the CDFI fund. The month for each state when 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. 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 2016 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.

Building Permits: The main outcome is whether a permit for the construction of a new building is issued in a census tract in a given month. Towards that end, I have compiled data on millions of building and trade permits for 47 large cities covering more than 15% of the U.S. population. Permits that were cancelled or voided are excluded from the sample.²⁷ [Figure A.2](#) maps the cities in my sample, with geographic coverage ranging across the U.S. The data come from a

²⁵Practices on nominating LIC-contiguous tracts varied across states ([Wallwork and Schakel, 2018](#)).

²⁶[Frank et al. \(2020\)](#) find that political affiliation between governors and representatives effected OZ selection. On the other hand, [Duarte et al. \(2021\)](#) find that governors mainly rubber-stamped OZ recommendations from city mayors.

²⁷I require that cities have at least 50 different census tracts appear in their building permit data.

mixture of publicly available sources and FOIA requests.²⁸ Additional information about the data sources is included in [Table B.3](#). Additional information about the data construction is included in [Appendix C](#).

Applying for a permit is the last step in the building process, after financing, development plans, and contractors have been established. 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. In [Figure A.5](#), 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.²⁹ I find that each permit for a new building is associated with one additional address a year later and two addresses two years later. The dynamics are consistent with larger construction projects taking longer to complete.³⁰

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 Summary Statistics

The distribution of months with new developments is included in [Figure A.3](#). In my sample, 86% of neighborhoods have no new development in a given month, and 17% have no new buildings since 2014. [Figure A.4](#) plots the percent of neighborhoods with new development over time. The aggregate time series tracks the U.S. housing market - with increasing new development leading into the Great Recession, before crashing and slowly recovering since. New construction drops amid the COVID-19 pandemic, but recovers by 2021.

While some of the building permit histories go back into the 1990s, I will limit my sample to observations between January 2014 and June 2022. Not all cities will have a building permit history

²⁸To 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 the location of the building permit. Geolocating the permits was performed by a mix of directly provided coordinates, census tracts, or the assessor parcel number that could be mapped to auxiliary shapefiles containing parcel locations. Though the samples vary by city, almost all cover time periods up until June 2022.

²⁹I 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.

³⁰Additionally, the USPS collects information on “no-status” addresses, which can include those under construction but not occupied. In [Figure A.6](#), 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).

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 1](#).

The process by which states chose OZs varied. Differences between OZs and other eligible tracts are summarized in [Table B.1](#) for the entire U.S. and in [Table 2](#) 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. From the pool of eligible neighborhoods, governors and local policymakers tended to designate the tax credit to areas that were considerably more distressed. OZs also have lower home values, and are more diverse, less educated, and less populated. These patterns hold both for OZs nationally, and restricted to my sample of cities.

New housing investment is closely tied to urban growth ([Glaeser et al., 2006](#); [Hsieh and Moretti, 2019](#)). This fact is especially pronounced within cities. [Figure A.7](#) depicts a bin scatterplot of the average number of new buildings in a neighborhood from 2014 through 2017, against its 2015 log median family income, 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.8](#) performs the same analysis, with changes in median family income from 2015 to 2019; new development is clearly 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 cheap construction costs and availability of land not found in a city's more developed areas. [Figure A.9](#) provides a salient example, Brooklyn, to study this possibility. The figure plots the total number of new buildings over two-year horizons from 2008 to 2019 by census tract. 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 to this day. 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.10](#) 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. 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. This is a broad phenomenon across cities in my sample.

New development is highly correlated with neighborhood income and income growth, but this investment 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, but that it could be one important factor for lifting a neighborhood out of urban decline.

Did the OZ tax credit target neighborhoods that have historically lacked development? [Figure 1](#) plots the fraction of neighborhoods with new development since 2014. Two separate time series are included for OZs and eligible tracts that were not designated for the credit. To detrend the series and compare their magnitude relative to other neighborhoods, I normalize relative to the time series for 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 the new development response.

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. I also document significant, positive spillovers on nearby areas.

4.1 Opportunity Zone Effects

Empirical specification: To estimate the first-order impact of the tax credit on new development projects, I run several regression analyses. These analyses leverage comparisons in new development between OZs and eligible non-OZs. In a first set of regression results, I estimate the following linear probability model.

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

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 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 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.2](#).³¹ The x_{it} are additional controls for city time trends. In the robustness exercises, I include additional controls in x_{it} as well.

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 margins of development, like square footage, construction costs, and number of units, are considered in [Section 4.3](#). However, these features are not available for many cities in my sample. Given the discrete nature of building projects, y_{it} is also a natural decision to focus on when modelling developers' strategic behavior in [Section 5](#).

By including $\theta_{g(i)t}$, β is estimated from comparisons between OZs and eligible non-OZs.³² Eligible neighborhoods that did not receive OZ status offer a natural control group, given that they are similarly low-income and high-poverty. However, OZs tend to be more distressed than other eligible areas. To assess their comparability, I consider whether new development behavior differed between the two groups prior to the program. [Section 4.3](#) presents robustness exercises that further control for these differences.

Overall effect: The average effect β is summarized in [Table 4](#). 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

³¹There 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 interpreted as "partially" treated months.

³²I retain ineligible tracts in the main estimation sample, which contribute to estimating the city trends. However, the results are unchanged by their inclusion.

eligible tracts and non-eligible tracts within cities. The latter estimates β by comparing eligible non-OZs with OZs within the same city. All standard errors are clustered at the level of treatment – the census tract (Bertrand et al., 2004). 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.

4.2 Identification

Identification of the key parameter β requires that policy status $T_{it} = \text{OZ}_i \times \text{Post}_{it}$ is exogenous conditional on the model covariates. This is a plausible assumption for several reasons. First, states were given four months to nominate tracts and the the full extent of the OZ policy was not yet known at the time of nomination. So, 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.³³ Second, census tracts do not naturally correspond to local housing markets, limiting the ability of policymakers to target desirable neighborhoods. Third, 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 conditional exogeneity of T_{it} is that OZ and eligible non-OZ neighborhoods must have similar trends in outcomes before the policy comes into effect. To test for this, I estimate models of the following form.

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

The indicator $\tau_t(k)$ denotes that the time period is k . The β_k capture conditional differences across time in new development between OZs and eligible non-OZs.³⁴ If the common trends assumption is satisfied, β_k should be close to zero for years, quarters, and months prior to OZ implementation. Figure 2 documents the baseline estimates of β_k at the annual-level. Reassuringly, I cannot reject $\beta_k = 0$ for years before OZs were enacted (Figure 2a). Moreover, OZs and non-OZs are indistinguishable at longer horizons before the program was implemented than the program has been in

³³Consistent 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.

³⁴In practice, I separate β_{2018} into the months of 2018 prior to when OZs were announced, and the months after OZs were announced. This helps assess the possibility of anticipation effects between when the program was passed in December 2017, and OZs were formally designated in April-June 2018.

existence for. These results are not sensitive to the choice of time trends, as shown in Column (1) through Column (4) of [Table 3](#).

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 2b](#) show little evidence for this story from 2016 Q1 to 2018 Q2. Monthly dynamics in [Figure 2c](#) 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.

SUTVA violations: 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.³⁵

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” tracts from being near to other OZs. A primary motivation for the model presented in [Section 5](#) is to jointly estimate the direct effect of the program with spillovers on nearby development.

Reallocation effects are more difficult to parse. 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 1](#) 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

³⁵Either would constitute a failure of the Stable Unit Treatment Value Assumption (SUTVA) ([Rubin, 1990](#)).

(iii) ineligible areas. Column (1) of [Table B.4](#) shows estimates from a difference-in-differences design using investment in eligible tracts as the control group.³⁶ 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 a large role for reallocation effects.³⁷

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 concern of the research design is that OZs are lower-income, more-impooverished, 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 status 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,³⁸ 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 5](#), 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.6](#). At the bandwidths from [Calonico and Titunik \(2014\)](#) in Column (3), the pre-trends and policy effects look similar.

Propensity score and regression-adjustment: I run an inverse propensity score-reweighted

³⁶Specifically, I include developer by tract type fixed effects, and developer by time fixed effects.

³⁷The dataset construction and specification are discussed in more detail in [Appendix D](#).

³⁸In my sample, crossing the poverty and income thresholds increases the probability of being selected by 5% and 8%, respectively. The first stage is not strong. Moreover, the heterogeneity analysis in later in this section suggests that the largest effects on new development are away from the eligibility cutoffs.

(IPW) version of the annual interacted differences-in-differences specification in Column (2) of [Table 6](#). 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.³⁹ In a second specification, I also augment the inverse propensity score-reweighting with regression-adjustment (IPWRA) using the same set of ACS covariates.⁴⁰ These results are contained in Column (3) of [Table 6](#). This model is doubly-robust; consistent estimation of the OZ policy effect is guaranteed if either the propensity score specification is correct, or the outcomes model for new development is correct ([Sant'Anna and Zhao, 2018](#)).⁴¹ 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. \(2013\)](#).⁴² [Figure 3](#) contains a depiction of the model fit and the treatment effects with confidence intervals.⁴³ 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

³⁹Overlap in the propensity scores is shown in [Figure A.12](#). 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.

⁴⁰See [Acemoglu et al. \(2019\)](#) or [Suárez Serrato and Wingender \(2016\)](#) for examples of this procedure.

⁴¹I implement this model as a recentered-influence-function regression, via `rifhdreg` in Stata. This is a convenient command that allows for partialling out high-dimensional fixed effects ([Rios Avila, 2019](#)).

⁴²For each set of city OZs, I construct placebo synthetic controls from the remaining pool of city eligible non-OZs and city eligibles. 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.

⁴³While confidence intervals do not have a natural interpretation in the synthetic control framework, they are a convenient way to graphically represent the significance of the estimated treatment effects.

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 Extensions

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. I also explore heterogeneities in the policy effect.

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.⁴⁴ [Figure 4](#) 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% - nearly identical 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 or residential demolitions 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 occurring, 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 infor-

⁴⁴Where possible, I classify mixed-use buildings as commercial.

mation is available for most, but not all, cities in my sample. Difference-in-differences estimates in [Figure A.16](#) show large and significant increases along all margins.⁴⁵ Dropping observations with no new developments however, as in the right-side panel of [Figure A.16](#), shows a muted intensive response in 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 5](#).

Heterogeneity: 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 the OZ policy effect with the share of pre-program months with new development - a measure of the amount of prior investment. These results are contained in [Table 7](#). 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.⁴⁶ 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 5](#) and the optimal policy design of [Section 7](#).

As a final set of exercises, I explore neighborhood heterogeneities in the response to the tax credit. I interact 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: 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. These results are contained in [Table B.8](#). 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, potentially because of the availability of cheaper land. Areas 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).⁴⁷

⁴⁵I also include the fully-interacted difference-in-differences model in [Table B.7](#). The lack of pre-trends across various margins is reassuring for the empirical design.

⁴⁶These effects are depicted in [Figure A.17](#).

⁴⁷I also interact the OZ effect with municipality zoning and land use restriction data from [Gyourko et al. \(2008\)](#).

Additional outcomes: 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.18](#). I find no evidence of pre-trends and an increase of 2% by 2021 Q4 relative to 2017 Q3. This suggests that the tax credits have lead to a substantial change in the stock of residential and commercial housing - and this effect is likely to increase as more construction is finished.

Finally, I consider how prices have responded to the tax credit. Absent data on neighborhood land values, I rely on the ACS log of median home values to test how prices have changed. I also estimate the same difference-in-differences regression on the 25th 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.10](#). 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 2020.⁴⁸

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, likely for two reasons: (i) my sample contains all neighborhoods within the largest U.S. cities., for which I have already documented a strong new development response, and (ii) I focus on changes in the log level of home values rather than changes in the annual rate of housing price growth. Consistent with my work, [Sage et al. \(2019\)](#) find that vacant plots and redevelopment sites in OZs experienced price increases.

4.5 Spillovers

Consider two nearby city blocks, where due to arbitrary census tract boundaries, one is an OZ neighborhood, and the other is not. These two locations face the same demand from residents looking to live there, and businesses hoping to offer local services or open office space. The previous sections have shown that the tax credit will encourage new development in the OZ-designated block. What happens to new development in the other city block?

These results are presented in [Table B.9](#). 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 7](#) 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.

⁴⁸Home value increases and no effect on rents were also found in [Busso et al. \(2013\)](#)'s study of Empowerment Zones.

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 NMTC 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 Dominguez-Iino (2019)). One new OZ project in Bronx, NY was a charter school, which surely increases residential demand from parents seeking to locate near schools (Appendix D). Moreover, the new construction is 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: Either mechanism is possible, and it is an empirical question as to which force dominates. To measure 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 $\hat{\mathbb{E}}[N_i^m]$, 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.11 shows that 2015 to 2017 changes in tract-level

demographics are uncorrelated with the difference between realized and expected nearby OZs.⁴⁹

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.

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

By an abuse of notation, 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.⁵⁰ 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.⁵¹ 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 captured in [Table 8](#). 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 should decay towards zero. Reassuringly, the effects are insignificant after 2-3 km, and decline towards zero across the specifications.

To study how the spillover effects change over time, 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

⁴⁹Moreover, 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.19](#) plots distribution of $N_i^m - \widehat{\mathbb{E}}[N_i^m]$, demonstrating a reasonable amount of variation for estimating spillovers.

⁵⁰Note 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.

⁵¹The 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.

respective distance bands, to increase power. These coefficients are plotted in [Figure A.20](#). The coefficients can be interpreted 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 could play an important role for spillovers in this context. Seeing new construction in the next neighborhood over, or even finished construction by 2020, may be important for encouraging nearby development.

I test how spillovers vary with the number of nearby OZs through the following regression.

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

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.21](#) with 95% confidence intervals.⁵² The left hand figure plots these effects for 0-2 km. The figure demonstrates diminishing returns in spillovers; the effects are larger for a smaller number of nearby OZs before flattening out.⁵³

I finally consider how these spillovers vary across neighborhoods. 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.12](#). In Row (1), OZ status does not predict higher spillovers, suggesting contamination in the main policy effect estimates may be limited. As in the direct effect, developable land, supply elasticities, and low home values predict larger spillovers. However, including all interactions in Column (7), only home values remain significant. A higher college-share of the population, and lower poverty rates also induce larger spillovers.

These heterogeneities, in combination with the diminishing returns in nearby tax credits, will

⁵²The effect at 0 can be interpreted as the average spillover effect from having no nearby OZs at distance m , but having the average number of nearby OZs at other m .

⁵³The 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.

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 returns, and heterogeneity in [Section 5](#), and they play a key role in the optimal policy design of [Section 7](#).

5 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 display diminishing returns in the number of nearby OZs. Heterogeneities and dynamics seem to play an important role in both the direct and indirect investment responses. I now present a model that parsimoniously captures these features.

Beyond synthesizing the reduced-form evidence, the model is useful for three primary 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. 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 values, 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 7](#), 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. All of these reasons will allow 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.

5.1 Framework

The main outcome of interest is whether new development occurs in a location at a given time (as in Section 4). Developer's profits will depend, critically, 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 granularity of a parcel of land, this assumption is non-controversial. However, for estimation purposes and because my main outcomes of interest are neighborhood quantities, I abstract to the level of census tracts.⁵⁴ In what follows, 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, 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.

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

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

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

The S_i function is a weighted average of nearby development, with weights that decay towards zero in the distance between location i and j .⁵⁵ The speed of the decay is governed by parameter δ .

⁵⁴Because the model will include state-dependence, this will imply myopia in the monopolist developer's decisions: they do not internalize how their decisions influence future profits. Another way to motivate this 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.

⁵⁵In my estimation, distance will correspond to distances between census tract centroids. Figure A.22 plots the

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

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

The location-heterogeneity term 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 ensure neighborhoods are more or less infermarginal 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.

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 4.5](#)).

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 more or less profitable in OZ-eligible neighborhoods.⁵⁶ Both $\lambda(x_i)$ and $\beta(x_i)$ are allowed to vary with neighborhood observables as in the reduced-form evidence: the log of local home values, the log of median family income, the college share, and the poverty rate.⁵⁷

distribution of these distances across my sample. The median tract-to-tract distance is approximately 14 kilometers, the distribution is highly skewed towards zero.

⁵⁶I 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](#).

⁵⁷I normalize these covariates as follows. For the policy effect, I subtract off the mean within OZs within a city. For the spillovers, I subtract of 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 interpreted as the average direct effect and strength of spillovers.

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

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

Definition. A full-information rational-expectations (FIRE) equilibrium at time t occurs if $\mathbb{E}_{it}[y_{jt}|\omega_t] = \mathbb{E}[y_{jt}|\omega_t] = \mathbb{P}[y_{jt}|\omega_t], \forall i, j$.

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

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

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

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

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 $\mathbb{P}(\mathbf{y}_t|\omega_t) = \mathbf{G}_t(\mathbb{P}(\mathbf{y}_t|\omega_t))$.⁵⁸ 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.

5.2 Identification and Estimation

Equipped with probabilities for new development in every time period, I estimate the model through a maximum likelihood approach.⁵⁹ In particular, I treat the location heterogeneity terms α_i as

⁵⁸As shown in Brock and Durlauf (2001a), since $\mathbf{G}_t : [0, 1]^n \rightarrow [0, 1]^n$ is continuous in $\mathbb{P}(\mathbf{y}_t|\omega_t)$, a solution $\mathbb{P}_t^*(\omega_t) = (\mathbb{P}_{it}^*(\omega_t))_{i=1}^n$ exists by Brouwer's fixed point theorem. A simulation of this model is included in Appendix E to develop intuition for the data generating process.

⁵⁹It 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.

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.⁶⁰

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

$$\ln \mathcal{L}(y_{it}|\theta, \omega_t)[m_t] = y_{it} \ln \mathbb{P}_{it}^*(\theta, \omega_t)[m_t] + (1 - y_{it}) \ln (1 - \mathbb{P}_{it}^*(\theta, \omega_t)[m_t])$$

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.⁶¹ 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 ω_t .

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

This motivates the following constrained maximum likelihood estimator.

$$\begin{aligned} \hat{\theta}, \{\hat{m}_t\}_{t=1}^T &= \arg \max_{\theta, \{m_t\}_{t=1}^T} \sum_{t=1}^T \sum_{i=1}^n y_{it} \ln \mathbb{P}_{it}^*(\theta, \omega_t)[m_t] + (1 - y_{it}) \ln (1 - \mathbb{P}_{it}^*(\theta, \omega_t)[m_t]) \\ \text{s.t.} \quad \mathbb{P}^*[y_{it}|\omega_t] &= \Lambda(\pi_{it}(\mathbb{P}^*[\mathbf{y}_t|\omega_t])), \quad \forall i, t \end{aligned} \quad (\text{FIRE eq.})$$

In practice, each θ produces several equilibrium, of which I take the highest likelihood equilib-

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.

⁶⁰Moreover, 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.

⁶¹Fu 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 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).

rium gas the corresponding likelihood for θ .⁶² Comparisons across θ can then be readily made. 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* neighborhoods'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 4.5](#)) 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.

6 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.

⁶²For 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.

6.1 Estimates

Parameter estimates: The parameter estimates from my model are summarized in [Table 9](#). The first row contains the spillover parameters λ . The second row contains the policy parameters β . The third row contains the spillovers decay parameter δ and the state-dependence parameter γ , as well as the mean and standard deviation of the location heterogeneity term α_i .

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 1.2pp. 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 26%. A higher college-share also lowers spillovers significantly. A 1 standard deviation increase in the share of college-educated residents lowers spillovers locally by 18%. Median family incomes and poverty rates are not found to effect spillovers substantially. These results are 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.

The direct effect of the tax credit β_0 for an average neighborhood is significant (0.18***). The effect is declining in the share of the population that is college-educated (significant at the 10%-level) and median family incomes. A 1 standard deviation (among OZs) increase in the college share lowers the policy effect 24%. A 1 standard deviation increase in median family incomes halves the policy effect. Interestingly, β_{hval} is small and insignificant. This suggests that effect heterogeneity in local home values were primarily driven by spillovers.

The spillovers decay parameter δ is estimated to be 0.62. 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.

Model fit: [Figure A.24](#) assesses the model fit for location heterogeneity and dynamics. [Figure A.24a](#) 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.24b](#) 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 against the baseline probability of new development in a tract, as estimated by $\Lambda(\hat{\alpha}_i)$. While different, it is reasonable to think they will be closely related. [Figure A.23](#) shows that for sufficiently high $\hat{\alpha}_i$, 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{P}_{it}^* from my model.⁶³ I test whether this estimate is different the reduced-form estimates using new development data y_{it} . These results are captured in [Table B.14](#). 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, (i) I see how restrictive the linear functional form is, and (ii) 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 test, 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{P}_{it}^* , rather than y_{it} . These figures are contained in [Figure 5a](#) and [Figure 5b](#), respectively. The 45-degree line and associated p -value tests whether the estimates are different up to sampling error. I cannot reject the hypothesis that the two sets of estimates are the same. This finding suggests that policy heterogeneity in home values operates through spillover heterogeneity and the location heterogeneity terms α_i . Moreover, the model is able to replicate important sources of effect heterogeneity not targeted in estimation.

6.2 OZ Effect on New Developments

In a final exercise, I use the model to characterize the investment response to the tax credit. For simplicity, I study the stationary, equilibrium investment response. Let s denote all states of the city Markov chain i.e. all combinations of neighborhood development histories. First, for a given policy T , we can solve for $\mathbb{P}^*(T, s)$ - equilibrium probability functions that say for every state s ,

⁶³The state-dependence term requires at least 12 months of prior development decision. Because this results in an increasingly unbalanced sample in 2014, I run these tests for the main sample restricted to 2015-2022.

the probability that i has new development in the next period. Second, $\mathbb{P}^*(T, s)$ can be used to generate $\Pi(T)$ - the matrix of state transitions in the city. Third, $\Pi(T)$ is an irreducible transition matrix and so admits a unique, stationary distribution $q^*(T)$. Fourth, $q^*(T)$ can be mapped to stationary probabilities that neighborhood i has new development $\bar{\mathbb{P}}^*(T)$.⁶⁴

I study the objects $\bar{\mathbb{P}}^*(T)$ under the OZ program and under no OZ program i.e. $\Delta\bar{\mathbb{P}}^*(\text{OZ}) = \bar{\mathbb{P}}^*(\text{OZ}) - \bar{\mathbb{P}}^*(0)$, where OZ is the vector of indicators for tracts that were designated for the tax credit. I later adapt this procedure to the optimal policy framework, where the policymaker can choose how to allocate policy T across neighborhoods.

7 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.

7.1 What's Optimal?

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.

Moving from the model to welfare implications is not immediately clear. [Arnott and Stiglitz](#)

⁶⁴The state space is too large to solve analytically for the stationary distribution. I numerically estimate the stationary distribution through simulating the Markov chain and averaging the last several hundred elements of the chain. I start the chain at the state of new construction in the period prior to the OZ announcement. I focus on estimates of the percent change, given by $\Delta\bar{\mathbb{P}}^*(\text{OZ})/\bar{\mathbb{P}}^*(0)$. Additional estimation details are included in [Appendix E](#).

(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.⁶⁵ 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](#).

[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 $\pi_{it}^*(\text{OZs}) - \pi_{it}^*(\text{no OZs})$ is predictive of home value increases. I construct this object, and average it for 2018 through 2020: $\overline{\Delta\pi_i^*}$. I then run the following regression.

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

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 6a](#). 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 $\overline{\Delta\pi_i^*}$ mediates the home value increases in OZs, I run the following regression.

$$\begin{aligned} \log(\text{median home values}_{it}) = & \sum_{k \neq 2017} \tilde{\beta}_k \cdot (\text{OZ}_i \times \tau_t(k)) + \\ & \sum_k (\overline{\Delta\pi_i^*} \times \tau_t(k) \cdot \eta_{1,k} + \overline{\Delta\pi_i^*}^2 \times \tau_t(k) \cdot \eta_{2,k}) + \alpha_i + \theta_{g(i)c(i)t} + \varepsilon_{it} \end{aligned}$$

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 6b](#). 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 π_{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. Given the link between housing supply and urban growth, documented in this paper and others, this is a natural

⁶⁵A recent example of such an approach is taken in [Smith \(2020\)](#).

objective for city planners.

7.2 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 $T(i)$ be a policy function assigning units of 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 optimal problem to choose the policy to maximize a weighted sum of latent developer profits is as follows.

$$\max_T \mathbb{E}_0 \sum_t \sum_i \rho^t \cdot \omega_i \cdot \pi_{it}^*(T, \theta, \mathbf{y}_0^{t-1}, \mathbf{x}_i) \quad (1)$$

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

$$\mathbf{y}_t \sim \text{Bernouilli}(\mathbb{P}_t^*(T, \theta, \mathbf{y}_0^{t-1}, \mathbf{x}_i)), \forall t \quad (3)$$

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

Equation 1 is the expected discounted sum of the weighted sum of neighborhood-specific latent profits (and by extension, median home values), 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}_{it}^* are interrelated across space. Also, implicitly $T(i)$ are either 0 or 1 for all neighborhoods.

In practice, solving for the optimal policy requires simulating all conditional distributions $\mathbf{y}_t | \mathbf{y}_{t-1}, \dots, \mathbf{y}_{t+1} | \mathbf{y}_{t-1}, \dots$, 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, as in [Section 6.2](#), I take a simpler approach and focus on the stationary distribution of investment. While there is fleixbility 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.⁶⁶

The city planner faces several trade-offs in this problem. Should they target neighborhoods that look like particularly good opportunities to see home value appreciation? Or areas, that through spillovers, can have a large response to the tax credit? Clustering the tax credit has diminishing returns in 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 and the choice set, and location, of neighborhoods that can be designated.

7.3 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 neighborhoods. 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. The solutions to Philadelphia's optimal policy problem are mapped in [Figure 7](#).

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 7a](#) and [Figure 7b](#), 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 4.6% and home values by 1.1% in the city.⁶⁷ The worst OZs increase investment by 0.08% and home values by 0.02%.

⁶⁶This already difficult problem is made worse by the fact that the objective function does not have a closed-form representation, and must be simulated. The stationary distribution is estimated via the same procedure as in [Section 6.2](#). 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](#).

⁶⁷These 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 0.19.

Given the critical role of home values in interpreting these findings, I map median home values in [Figure 7c](#) against the optimal designated tax credits in [Figure 7d](#). 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 returns in spillovers, the optimal designations are clustered, relying on larger direct and indirect effects of the policy to compensate. Second, the optimal policy prefers clustering in areas where the gradient moves from higher home values to lower home values. Here, neighborhoods are more inermarginal 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 6.6% 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. I now describe optimal OZs and generalize the above evidence for all neighborhoods in my sample.

Characterizing optimal OZs: [Table 10](#) correlates optimal OZs and actual OZs with 2011-2015 5-year ACS demographics among eligible tracts. Column (1) shows that optimal OZs tend to have a lower share of the population that is female, and are 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 27% of optimal OZs were actually selected for the tax credit. 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. This suggests that OZs had a higher share of the population that was college-educated than was necessary under the optimal policy.

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 0.4% and home values increase 0.08%. Under the optimal program, new development increases 4.7% and home values increase 1.1%. The worst policy demonstrates that a reasonable investment response to the program is not a given. However, the optimal program is a substantial improvement over the actual program.

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 8](#). These investment changes are plotted against a neighborhood's poverty rate. There is a strong positive relationship between a neighborhood's poverty rate and its response to the OZ program. The optimal program increases investment at all levels of the poverty distribution, but more so for higher-poverty neighborhoods.

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.

Cost-benefit analysis: The results 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 median home value \$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 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 7.1](#). This generates an aggregate increase in property values on the order of \$12.1 billion.⁶⁸

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 \$7.9 billion, and \$1.9 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). If we assume that the costs of the program scale with total investment, these net benefits would increase 75% under the optimal program.⁶⁹

⁶⁸This follows the conservative approach taken in [Busso et al. \(2013\)](#).

⁶⁹This is also conservative, since much of the increased investment will be in neighborhoods that were not designated for the tax credit.

8 Conclusion

The difference-in-differences results found large increases in the amount of new development occurring in OZs relative to neighborhoods that were eligible for designation, but not selected. The program also induced positive spillovers in new development on nearby areas. A model that accounts for these two impacts, and how they vary across neighborhoods, found that the actual program increased new development by 2.7% and home values by 0.6% in aggregate. These effects would have increased to 4.7% and 1.1% under the optimal OZ program.

The optimal program offers justification for clustering these tax credits. While there are diminishing returns in spillovers, spatial correlation in the magnitude of direct and indirect effects dominates. The optimal program favors clustering tax credits in neighborhoods outside the central downtown area. The optimal program in this paper suggests large opportunities for equity and efficiency gains in how place-based policies are implemented. Mixed evidence on the efficacy of prior place-based policies may, in part, reflect these differences in how they were implemented. 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 the property value gains from the program outweigh the federal costs through 2020. However, the approach in this paper is short-run and partial-equilibrium. 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. 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.

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Tables

Table 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.3.

Table 2: OZ descriptives for sample

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

	(1) New Building	(2) New Building	(3) New Building	(4) New Building
OZ and 2014	-0.00565 (0.00450)	-0.00672 (0.00445)	-0.00631 (0.00447)	-0.00725 (0.00443)
OZ and 2015	-0.000199 (0.00421)	-0.000642 (0.00415)	-0.00147 (0.00416)	-0.00107 (0.00414)
OZ and 2016	0.00279 (0.00370)	0.00260 (0.00369)	0.00321 (0.00370)	0.00243 (0.00368)
OZ and 2018 pre-OZ	0.00681 (0.00510)	0.00714 (0.00510)	0.00758 (0.00518)	0.00719 (0.00511)
OZ and 2018 post-OZ	0.0213*** (0.00442)	0.0216*** (0.00440)	0.0205*** (0.00439)	0.0217*** (0.00440)
OZ and 2019	0.0258*** (0.00441)	0.0263*** (0.00438)	0.0266*** (0.00439)	0.0266*** (0.00437)
OZ and 2020	0.0240*** (0.00453)	0.0247*** (0.00452)	0.0271*** (0.00454)	0.0250*** (0.00451)
OZ and 2021	0.0383*** (0.00512)	0.0393*** (0.00507)	0.0390*** (0.00502)	0.0398*** (0.00504)
OZ and 2022 H1	0.0331*** (0.00592)	0.0347*** (0.00582)	0.0326*** (0.00578)	0.0353*** (0.00579)
Observations	1,175,040	1,175,040	1,175,040	1,175,040
R²	0.303	0.305	0.311	0.306
Dep. Var. Mean	.1441	.1441	.1441	.1441
Tract FE	✓	✓	✓	✓
Elig. x Month FE	✓	✓	✓	✓
City x Season		✓		✓
City Linear Trend		✓		✓
City x Month			✓	
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 coefficients interact a time period with 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 4: 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*** (0.00346)	0.0294*** (0.00333)	0.0296*** (0.00335)	0.0300*** (0.00329)
Observations	1,175,040	1,175,040	1,175,040	1,175,040
<i>R</i> 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 5: Policy variation at the eligibility cutoffs (I)

	(1) New Building	(2) New Building	(3) New Building	(4) New Building
OZ and 2014	-0.00275 (0.00460)	-0.00317 (0.00456)	-0.00278 (0.00457)	-0.00251 (0.00456)
OZ and 2015	0.000439 (0.00427)	0.000397 (0.00427)	0.000787 (0.00427)	0.00104 (0.00427)
OZ and 2016	0.00336 (0.00382)	0.00302 (0.00381)	0.00340 (0.00381)	0.00346 (0.00381)
OZ and 2018 pre-OZ	0.00544 (0.00518)	0.00554 (0.00519)	0.00572 (0.00519)	0.00549 (0.00519)
OZ and 2018 post-OZ	0.0191*** (0.00452)	0.0191*** (0.00453)	0.0192*** (0.00452)	0.0190*** (0.00452)
OZ and 2019	0.0200*** (0.00455)	0.0197*** (0.00455)	0.0198*** (0.00454)	0.0196*** (0.00454)
OZ and 2020	0.0168*** (0.00464)	0.0156*** (0.00465)	0.0159*** (0.00464)	0.0158*** (0.00464)
OZ and 2021	0.0290*** (0.00519)	0.0276*** (0.00520)	0.0279*** (0.00520)	0.0276*** (0.00520)
OZ and 2022 H1	0.0248*** (0.00601)	0.0245*** (0.00602)	0.0247*** (0.00601)	0.0245*** (0.00601)
Observations	1,175,040	1,175,040	1,175,040	1,175,040
R²	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 6: Alternative specifications

	(1) New Building	(2) New Building	(3) New Building	(4) New Building
OZ and 2014	-0.00672 (0.00445)	0.00301 (0.00504)	-0.00299 (0.00496)	-0.0308 (0.0376)
OZ and 2015	-0.000642 (0.00415)	0.00361 (0.00477)	-0.000485 (0.00482)	0.0156 (0.0335)
OZ and 2016	0.00260 (0.00369)	0.00785* (0.00458)	0.00450 (0.00448)	0.0349 (0.0285)
OZ and 2018 pre-OZ	0.00714 (0.00510)	0.0134** (0.00670)	0.0108* (0.00646)	0.0478 (0.0352)
OZ and 2018 post-OZ	0.0216*** (0.00440)	0.0187*** (0.00510)	0.0189*** (0.00543)	0.133*** (0.0290)
OZ and 2019	0.0263*** (0.00438)	0.0190*** (0.00526)	0.0208*** (0.00522)	0.163*** (0.0291)
OZ and 2020	0.0247*** (0.00452)	0.0190*** (0.00525)	0.0165*** (0.00563)	0.186*** (0.0321)
OZ and 2021	0.0393*** (0.00507)	0.0264*** (0.00568)	0.0259*** (0.00568)	0.242*** (0.0335)
OZ and 2022 H1	0.0347*** (0.00582)	0.0184*** (0.00695)	0.0231*** (0.00672)	0.203*** (0.0375)
Observations	1,175,040	1,105,842	738,903	977,011
R²	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. Column (4) estimates the model via poisson pseudo-maximum likelihood estimation. For Column (4), the coefficients should be interpreted 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 7: Heterogeneity by share of pre-OZ months with new development

	(1)	(2)
	New Building	New Building
QOZ x Post x Dev. Shr.	-0.0478* (0.0255)	0.241*** (0.0588)
QOZ x Post x Dev. Shr. Sq.		-0.424*** (0.0790)
Observations	1,175,040	1,175,040
<i>R</i> 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 8: Spillovers

	(1) New Building	(2) New Building	(3) New Building	(4) New Building
Has QOZ (0-2 km) x Post	0.00982*** (0.00285)	0.0116*** (0.00284)	0.0125*** (0.00287)	0.0123*** (0.00287)
Has QOZ (2-3 km) x Post	0.00901*** (0.00305)	0.00941*** (0.00304)	0.00926*** (0.00303)	0.00909*** (0.00303)
Has QOZ (3-4 km) x Post	0.00543 (0.00349)	0.00588* (0.00350)	0.00517 (0.00350)	0.00493 (0.00350)
Has QOZ (4-5 km) x Post	0.00491 (0.00357)	0.00539 (0.00360)	0.00396 (0.00361)	0.00377 (0.00361)
Has QOZ (5-6 km) x Post	0.000241 (0.00357)	0.000672 (0.00356)	-0.00177 (0.00358)	-0.00218 (0.00357)
Has QOZ (6-7 km) x Post	0.00142 (0.00359)	0.00241 (0.00358)	-0.000550 (0.00358)	-0.000825 (0.00357)
Observations	1,174,782	1,174,782	1,174,782	1,174,782
R²	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 continuous 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 9: Model estimates

<i>Panel A: Spillovers</i>				
λ_0	λ_{hval}	λ_{col}	λ_{pov}	λ_{mfi}
1.03***	-0.27***	-0.19***	-0.00	0.14
(0.07)	(0.07)	(0.07)	(0.05)	(0.09)

<i>Panel B: Program effects</i>				
β_0	β_{hval}	β_{col}	β_{pov}	β_{mfi}
0.18***	-0.05	-0.07*	0.03	-0.15***
(0.02)	(0.04)	(0.04)	(0.03)	(0.05)

<i>Panel C: Other parameters</i>				
δ	γ	$\bar{\alpha}_i$	sd(α_i)	
0.62***	0.33***	-2.33	2.98	
(0.01)	(0.00)			

Note: This table contains parameter estimates from my baseline model in Section 5. 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 10: Characterizing optimal and actual OZs

	(1) OZs (optimal)	(2) OZ	(3) OZ	(4) OZ
OZs (optimal)		0.274*** (0.0193)	0.240*** (0.0199)	
Log Median Family Income	-0.128*** (0.0302)	-0.123*** (0.0316)		-0.0926*** (0.0303)
% Poverty, 2015	0.00416*** (0.000849)	0.00156* (0.000909)		0.000561 (0.000884)
Log Population, 2015	-0.0399*** (0.0142)	-0.0678*** (0.0147)		-0.0583*** (0.0144)
% Female, 2015	-0.00284** (0.00142)	-0.00636*** (0.00155)		-0.00568*** (0.00149)
% White, 2015	-0.000359 (0.000585)	-0.000217 (0.000596)		-0.000131 (0.000584)
% Black, 2015	0.000689 (0.000519)	0.00147*** (0.000525)		0.00130** (0.000512)
% High School, 2015	-0.00226*** (0.000876)	-0.00302*** (0.000978)		-0.00248** (0.000973)
% College, 2015	0.00121 (0.00106)	0.00406*** (0.00116)		0.00377*** (0.00115)
Log Median Home Value, 2015	0.0264 (0.0187)	0.0143 (0.0202)		0.00794 (0.0200)
Observations	3,801	3,801	3,801	3,801
R²	0.109	0.092	0.119	0.143
Dep. Var. Mean	.2125	.2137	.2137	.2137
Fixed Effects	City	City	City	City
Sample	Eligibles	Eligibles	Eligibles	Eligibles

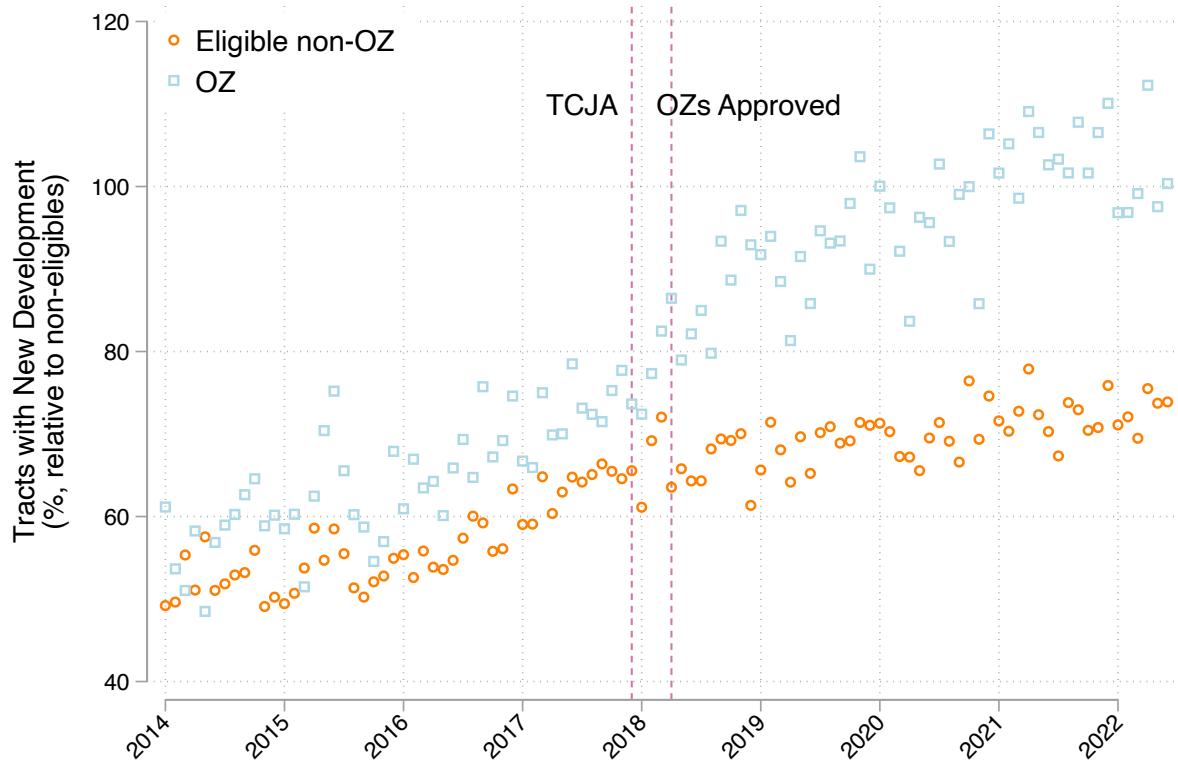
Robust standard errors in parentheses

*** 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.

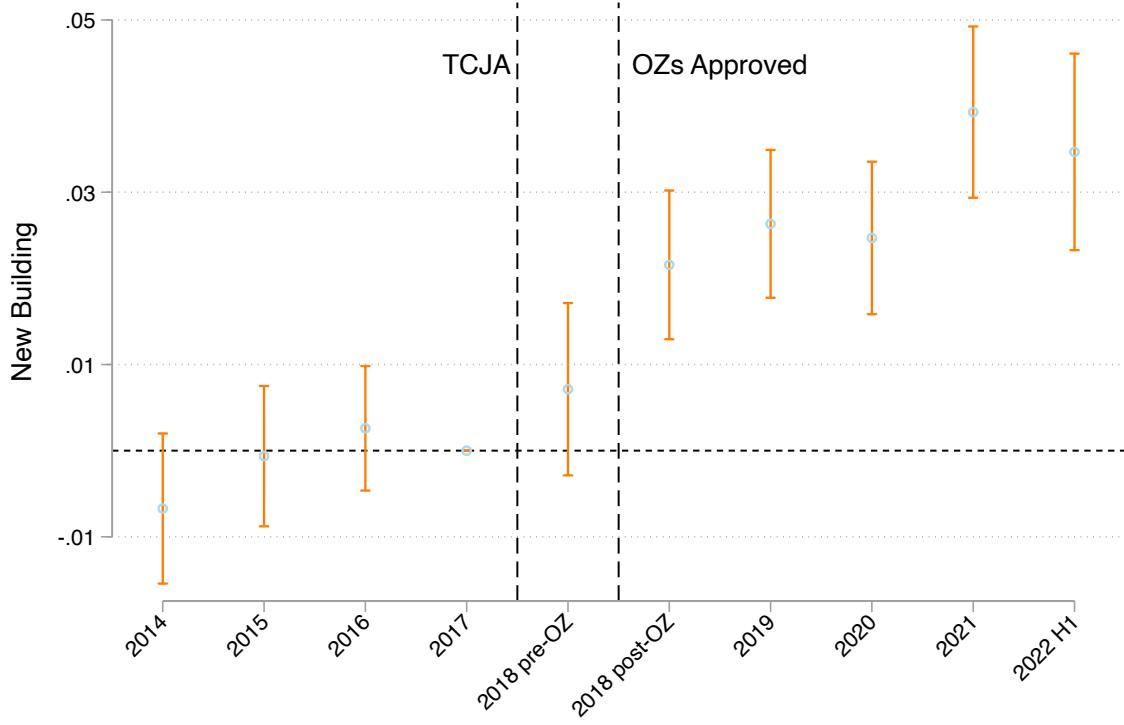
Figures

Figure 1: Time series for OZs and eligible non-OZs

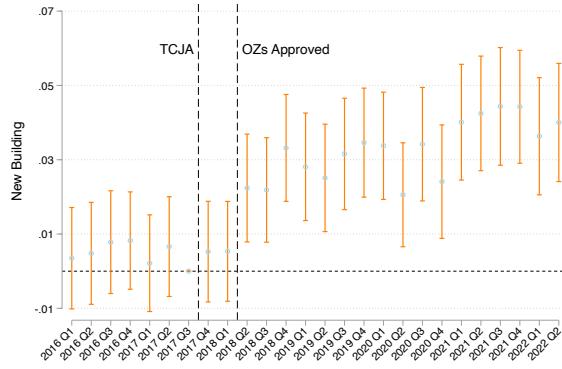


Note: This figure plots time series in new development projects for tracts that were eligible to be designated as OZs, but were not (blue), with those that were designated OZs (orange). The time series is the fraction of tracts in each tract type that have new development projects in a given month as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

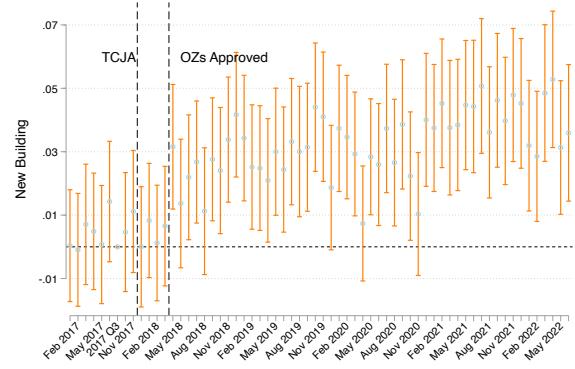
Figure 2: Difference-in-differences estimates



(a) Annual



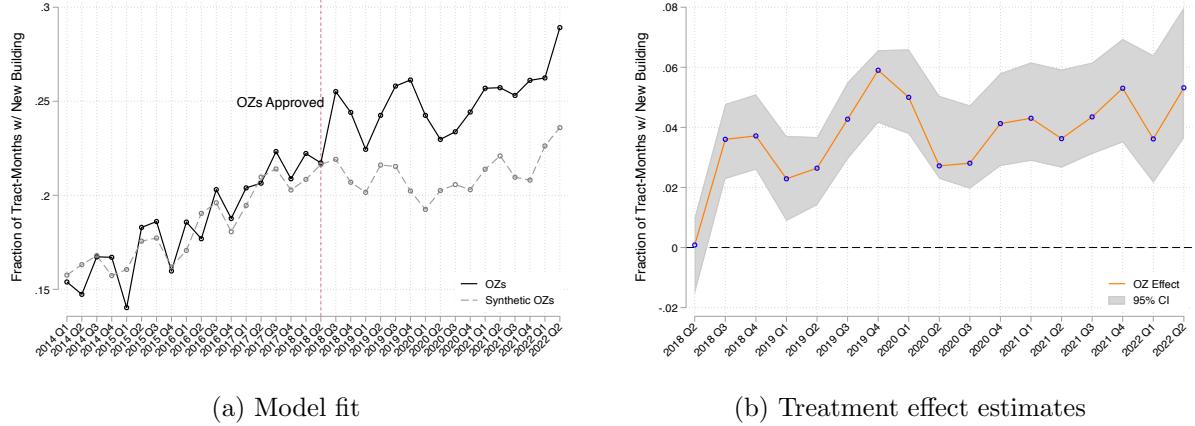
(b) Quarterly



(c) Monthly

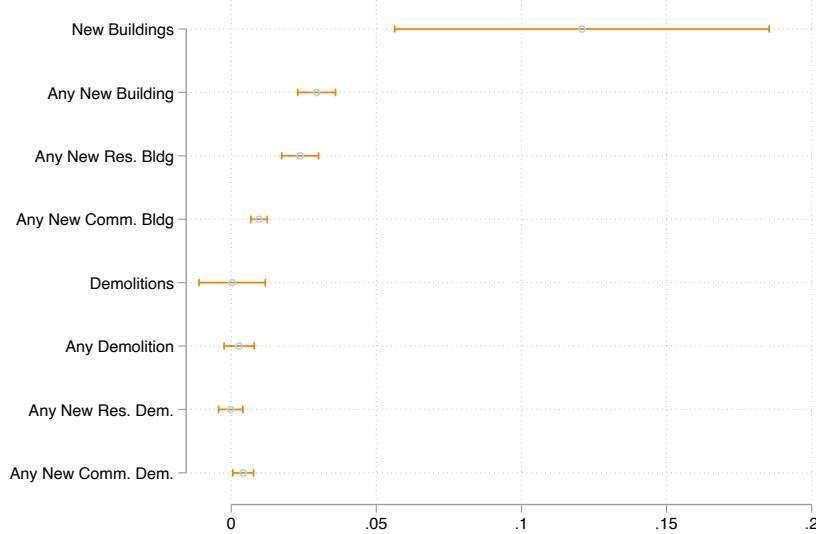
Note: This chart contains estimates from a linear probability model including tract, month, and eligibility by month fixed effects, as well as city linear and seasonal trends. The outcome variable is an indicator for whether a tract had a permit issued for the construction of a new building in a month. The coefficients correspond to OZ status interacted with various time periods. Panel (a) depicts annual interactions with OZ status. These estimates can also be found in Column (2) of [Table 3](#). 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 3: Synthetic control design



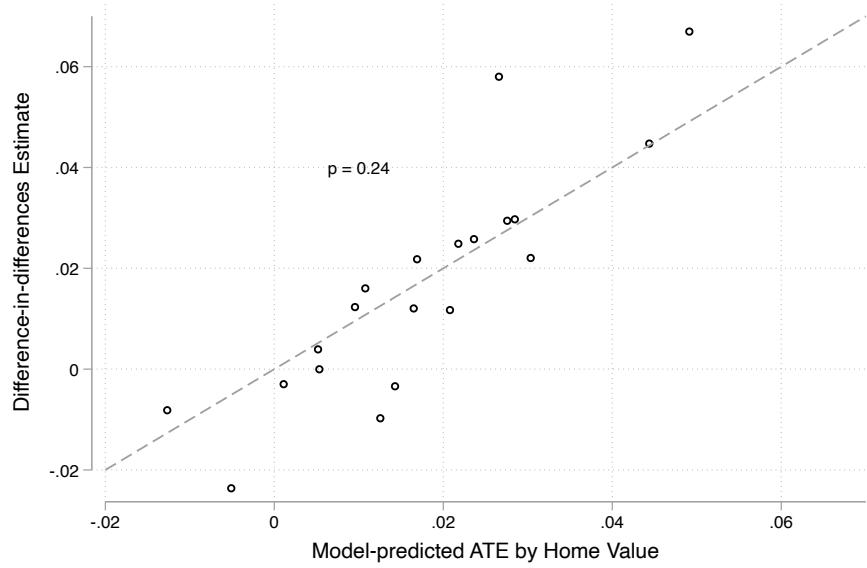
Note: This figure presents model fit and treatment effect estimates using a synthetic control method. The data is first collapsed to average number of tract-months with new development in a quarter in a city by tract type (where tract type can be OZ, eligible but not OZ, or ineligible). A synthetic control 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 4: Other development responses



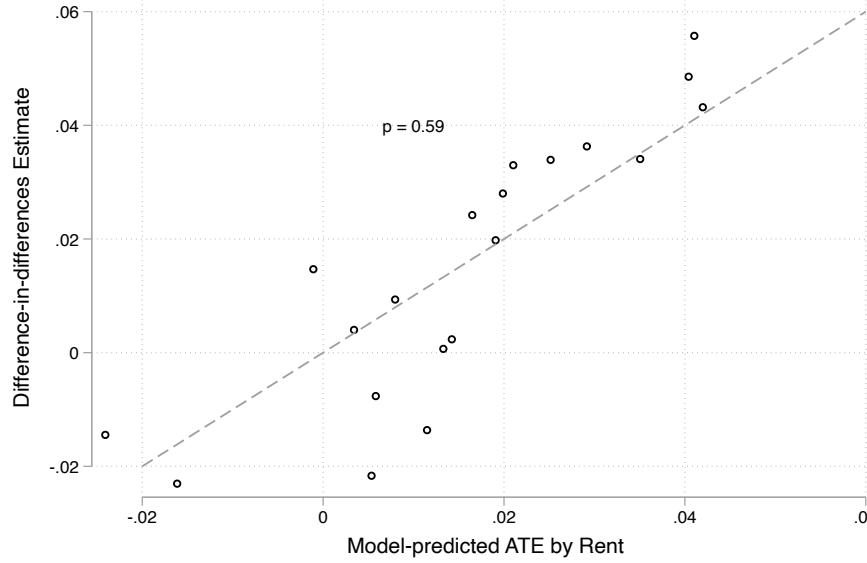
Note: This figure contains estimates of the OZ effect using the baseline difference-in-differences model on various outcomes. The top row uses as an outcome the number of new buildings, rows 2 through 4 use as outcomes an indicator for a new building, and whether its residential or commercial. Rows 5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

Figure 5: Model-predicted effects versus design-based effects



(a) Median home values

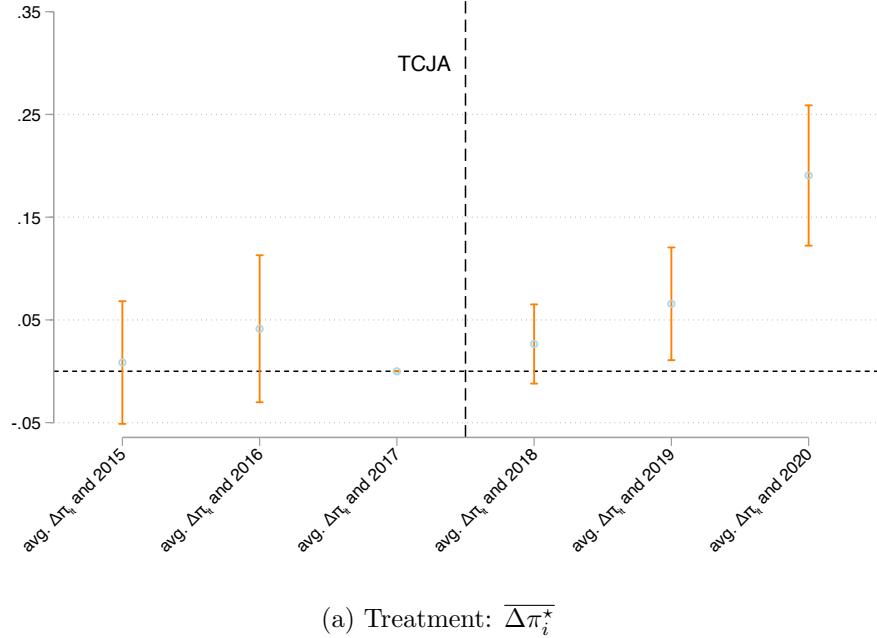
Note: This figure compares model-based estimates of the OZ effect by median home value vingtile with those from an interacted difference-in-differences model. The dashed line corresponds to the 45 degree line. The p-value comes from a test of the hypothesis that the difference-in-differences estimates 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.



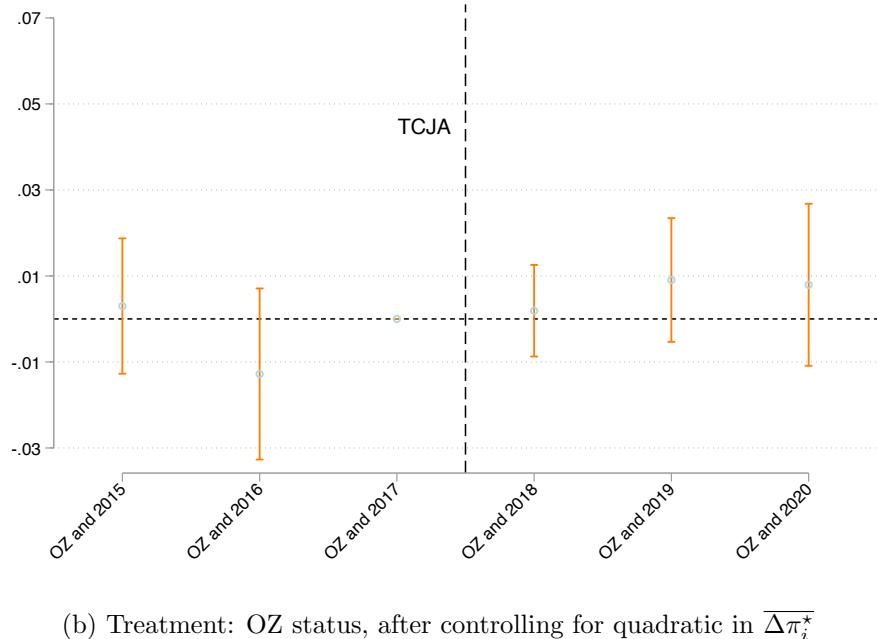
(b) Rents

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 6: Log median home value changes

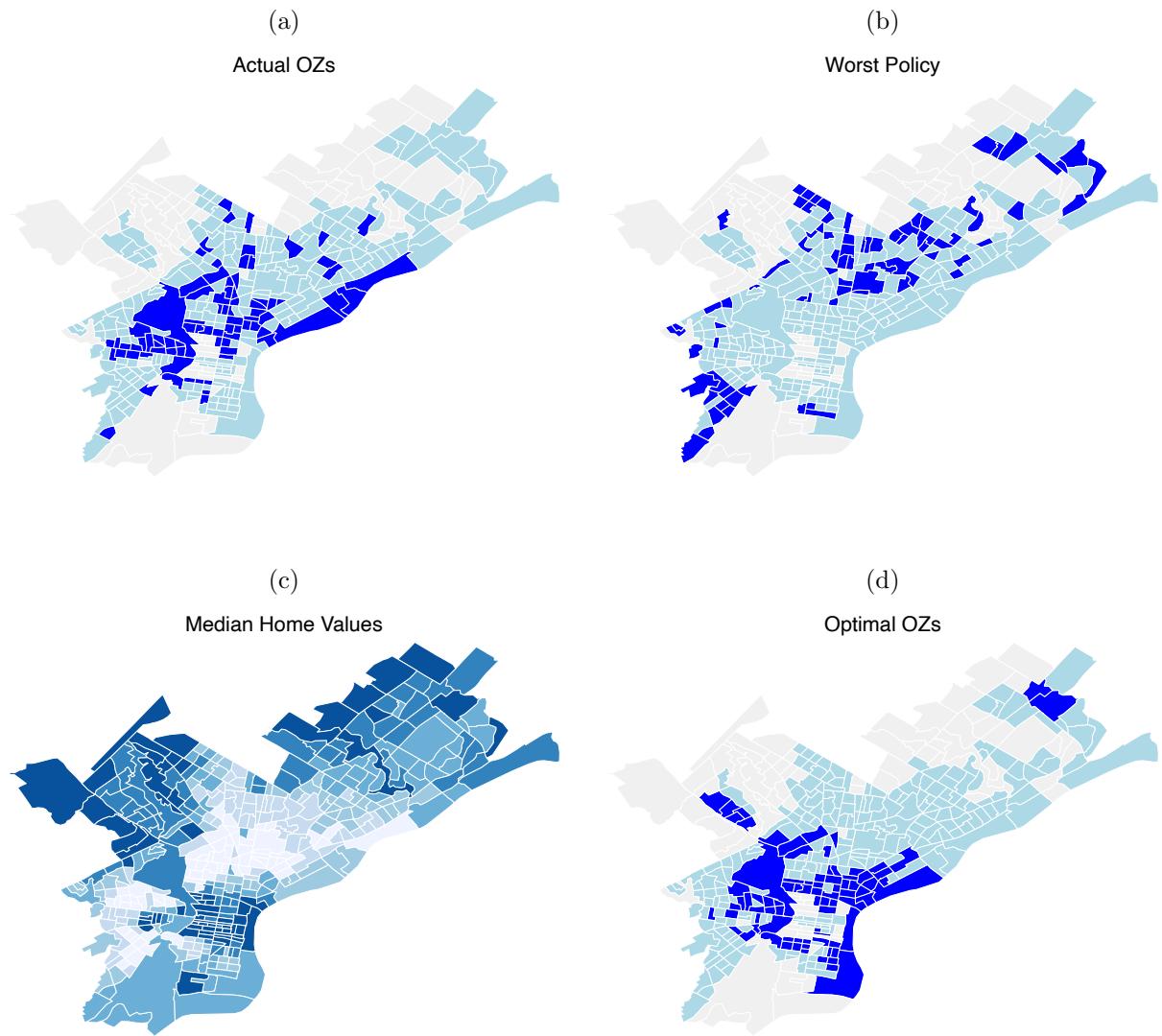


Note: This figure contains estimates from a difference-in-differences model where treatment is $\bar{\Delta}\pi_i^*$. The sample only includes census tracts with median home value data for all years. The sample covers years 2015 through 2020. Errors are clustered at tract-level.



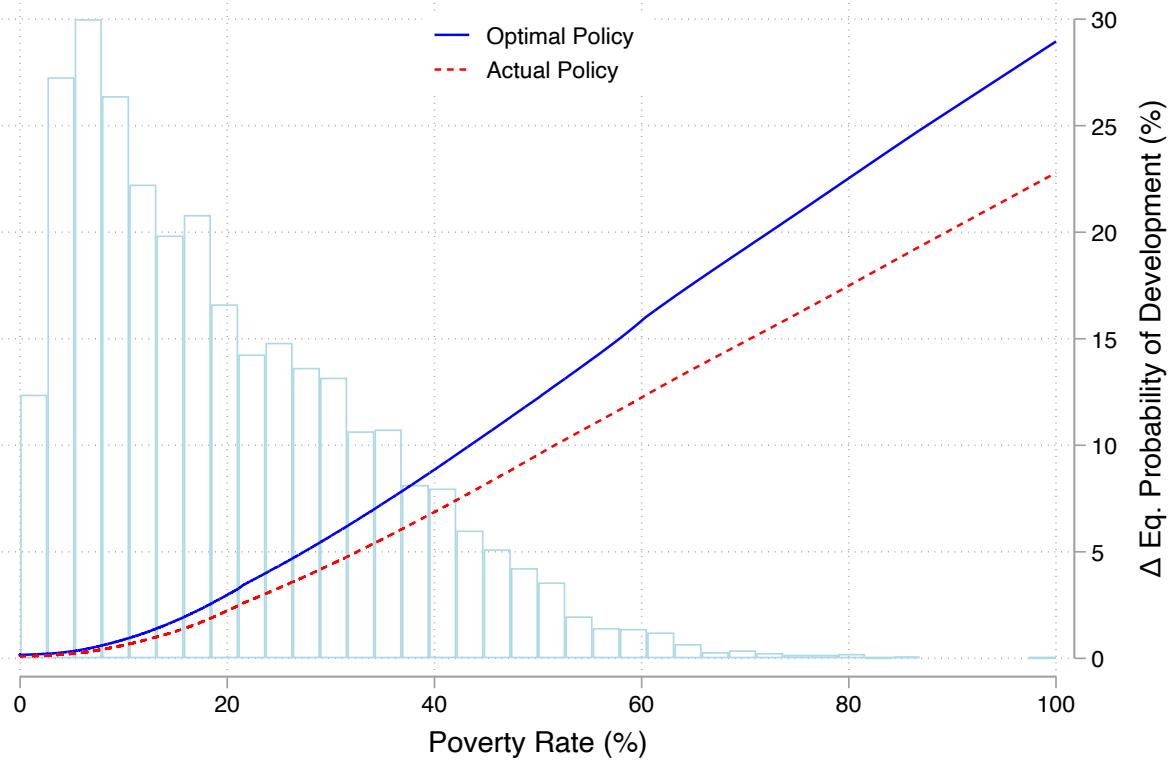
Note: This figure contains estimates from a difference-in-differences model where treatment is OZ status. I control for a quadratic in $\bar{\Delta}\pi_i^*$ interacted with year. The sample only includes census tracts with median home value data for all years. The sample covers years 2015 through 2020.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

Figure 7: 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 8: Actual vs. optimal policy effects by poverty rate



Note: This figure shows estimates of the percentage change in equilibrium development $100 \times (\bar{P}^*(T) - \bar{P}^*(0)) / \bar{P}^*(0)$ 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 the tract poverty rate from the 2011-2015 ACS. The lines depict predictions from a locally-weighted regression via lowess smoothing. A histogram of the poverty rate is included in the background in light blue.

Locally Optimal Place-Based Policy:
Evidence from Opportunity Zones

Online Appendix

Harrison Wheeler - UC Berkeley¹

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¹Email: wheeler@berkeley.edu.

A Additional Figures

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

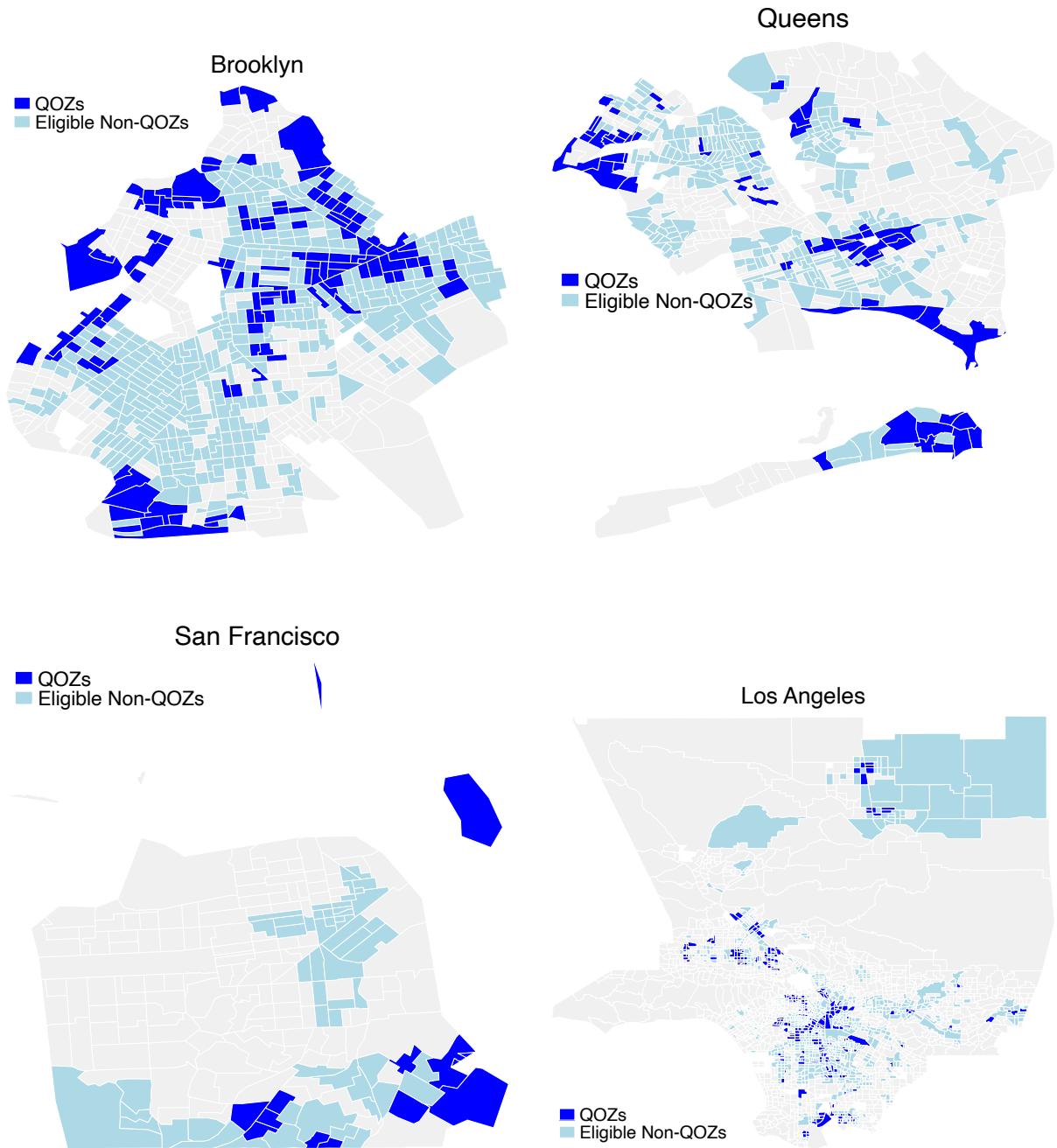
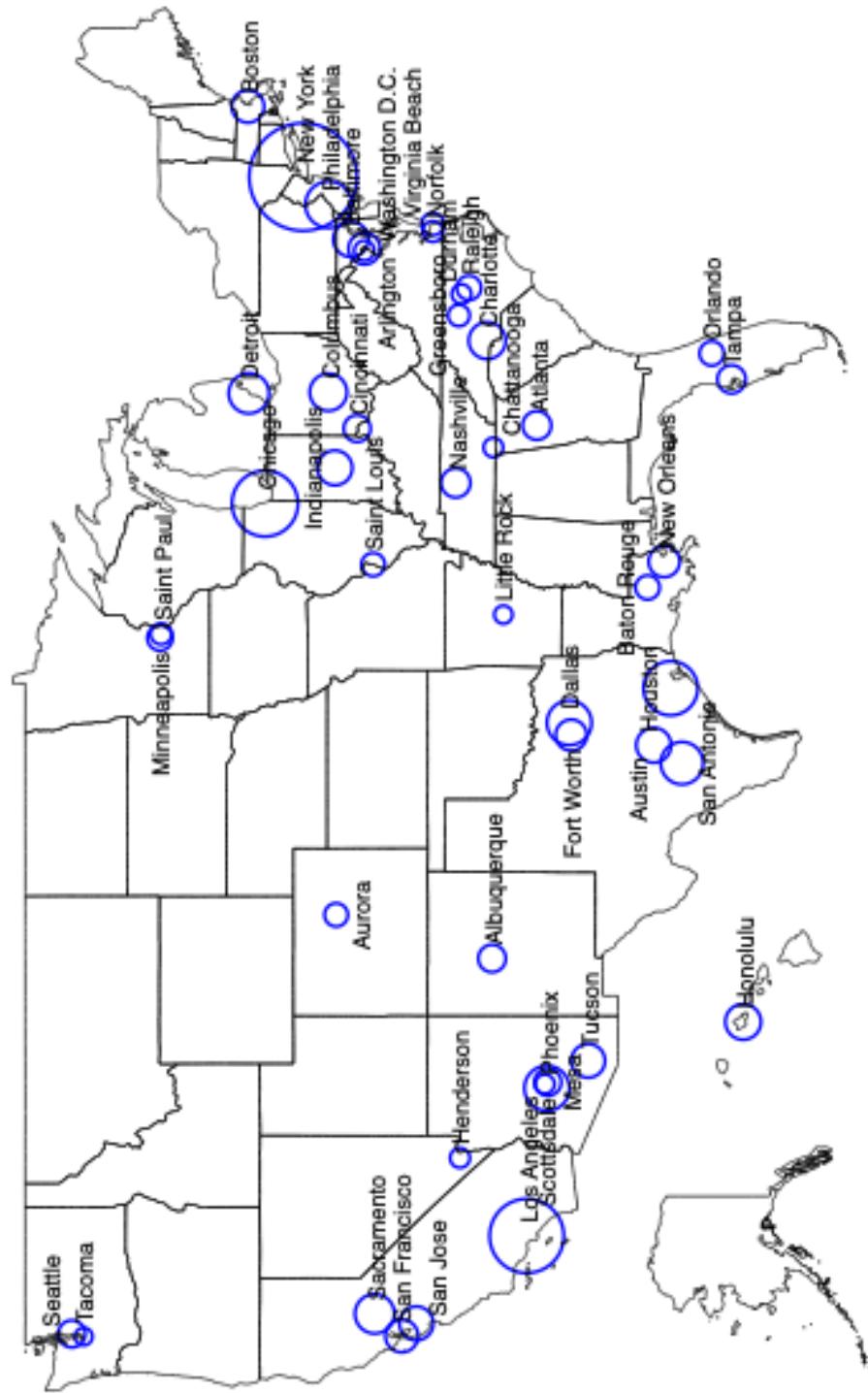
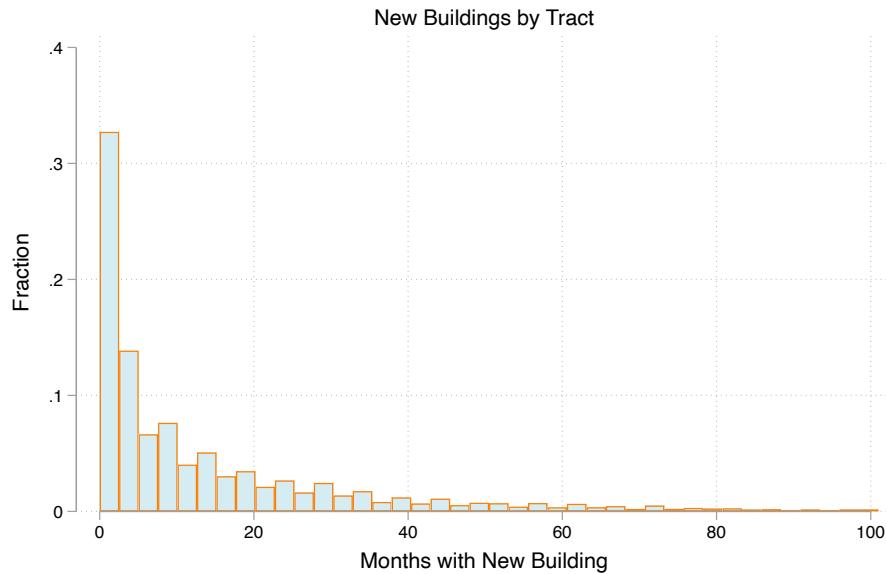


Figure A.2: Cities in main sample



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 A.3: Distribution of new development



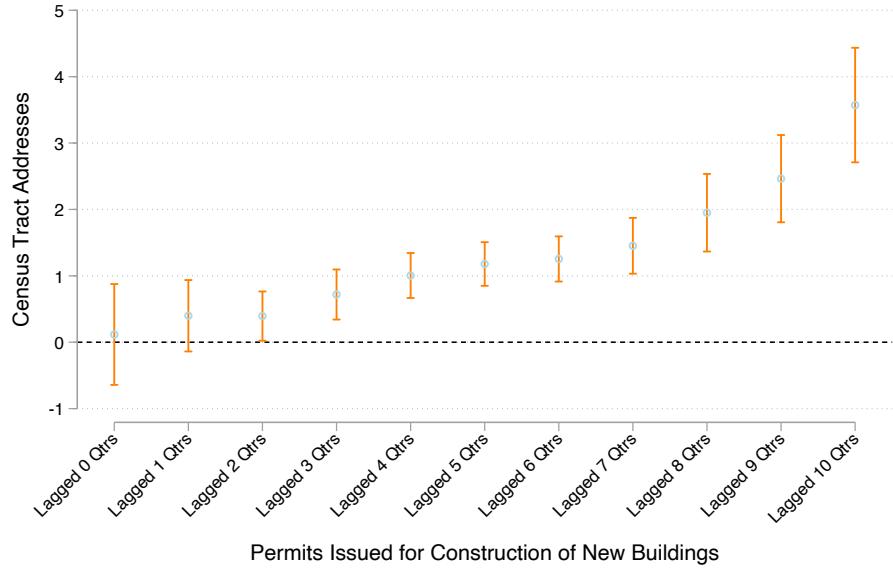
Note: This histogram plots the distribution of number of months with new developments for each census tract in the sample. The time coverage is January 2014 to June 2022. The sample includes 11,936 total tracts.

Figure A.4: Aggregate time series



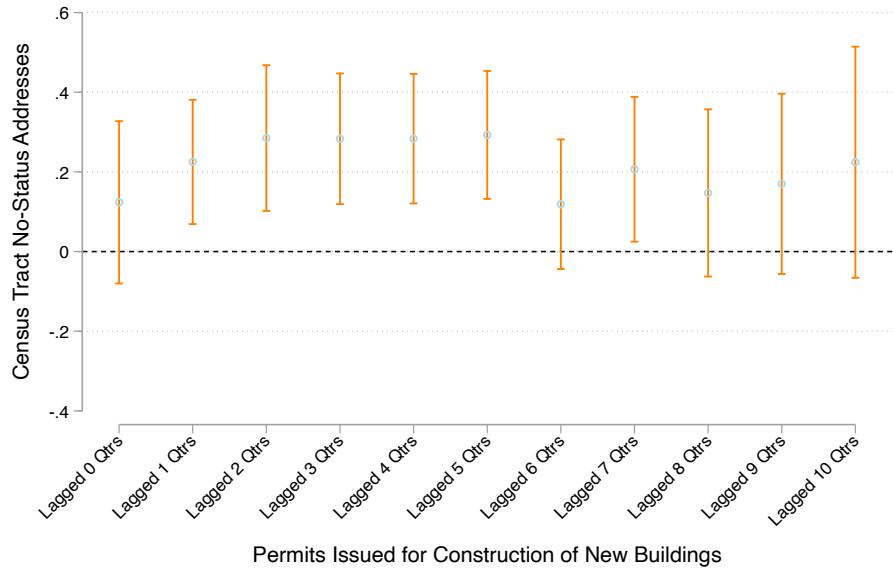
Note: This figure plots the fraction of tracts with new development in each month over time. The underlying sample of cities changes depending on availability of building permit data (Table B.3).

Figure A.5: Correlation of new construction measure and tract addresses



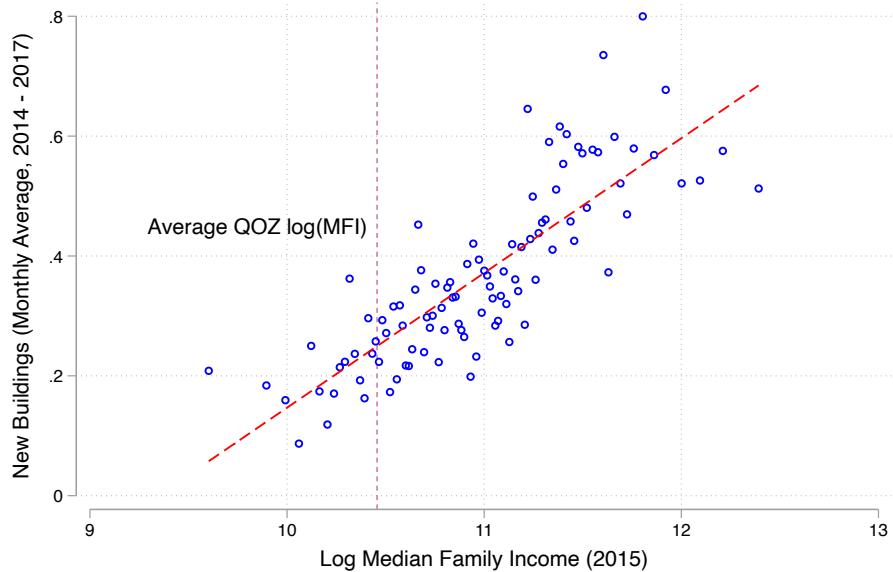
Note: This chart shows coefficients from a regression of total addresses in a census tract quarter on lags of number of permits issued for the construction of new buildings. The address data comes from HUD's USPS vacant addresses data. The regression includes tract and date fixed effects. Errors are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

Figure A.6: Correlation of new construction measure and tract “no-status” addresses



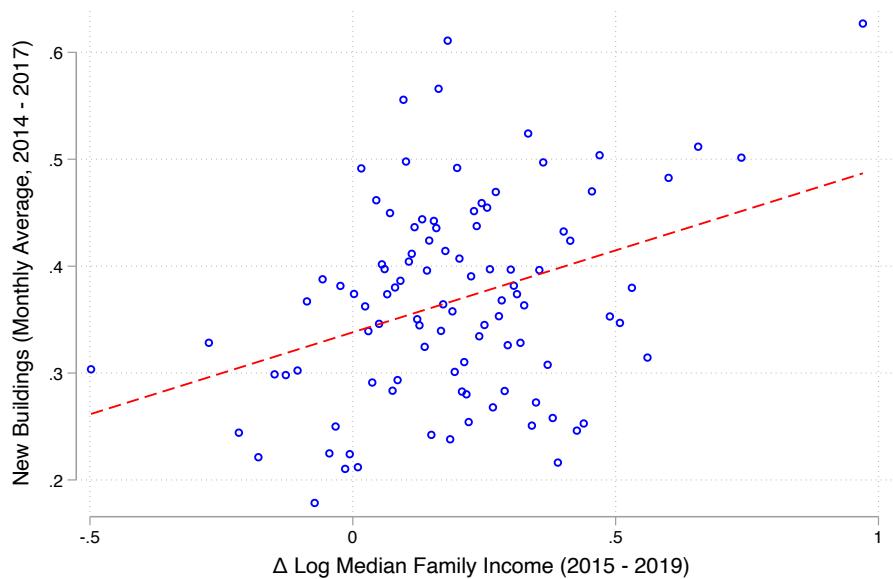
Note: This chart shows coefficients from a regression of “no-status” addresses in a census tract quarter on lags of number of permits issued for the construction of new buildings. The address data comes from HUD's USPS vacant addresses data. The regression includes tract and date fixed effects. Errors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

Figure A.7: Median family income vs. new development projects



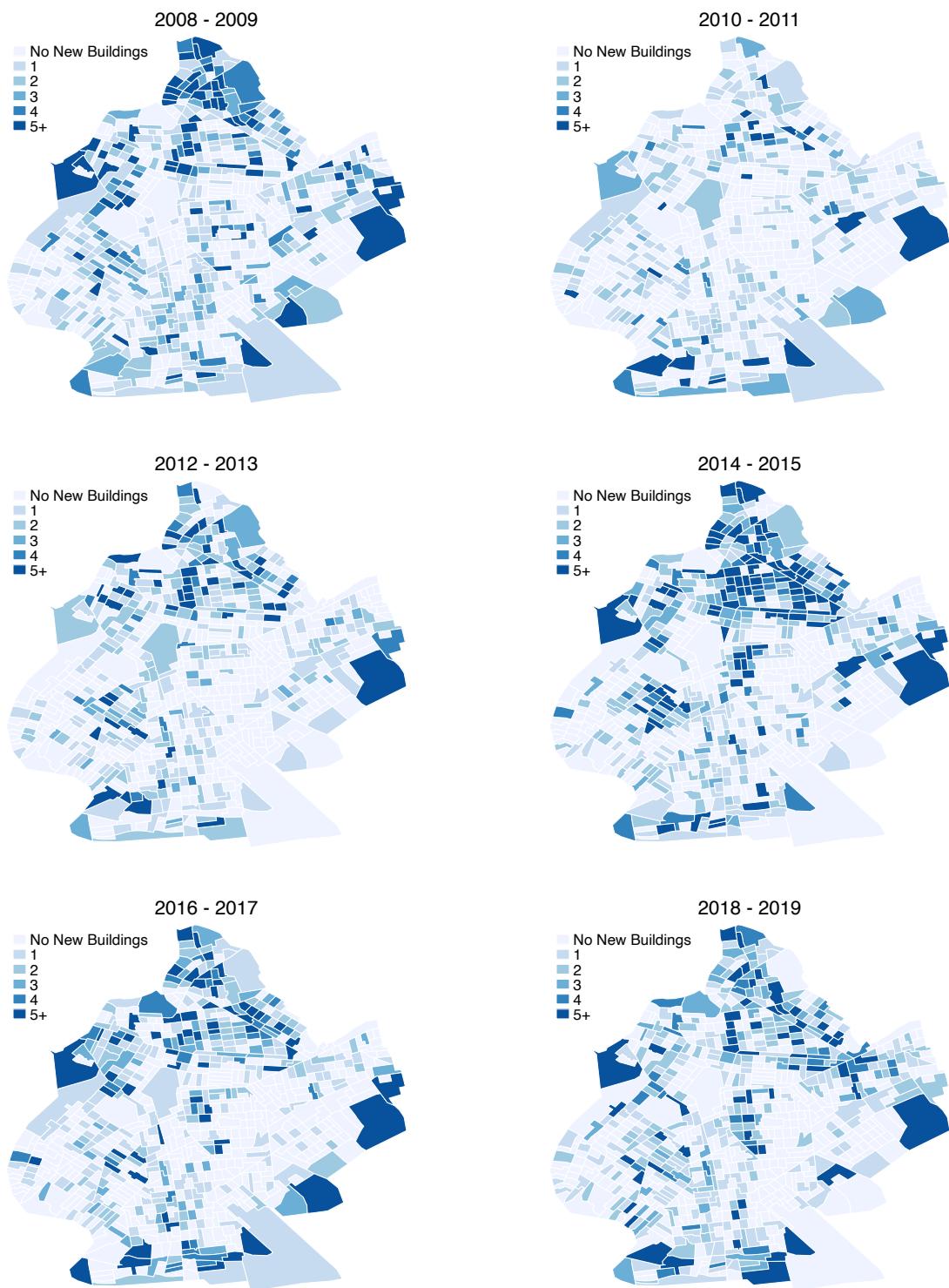
Note: This bin scatterplot shows ACS 2011-2015 tract-level log median family income against the monthly average of new buildings permitted for from 2014 to 2017. The dotted line denotes the average log median family income of Opportunity Zones. A line of best fit is depicted in red.

Figure A.8: Change in median family income vs. new development projects



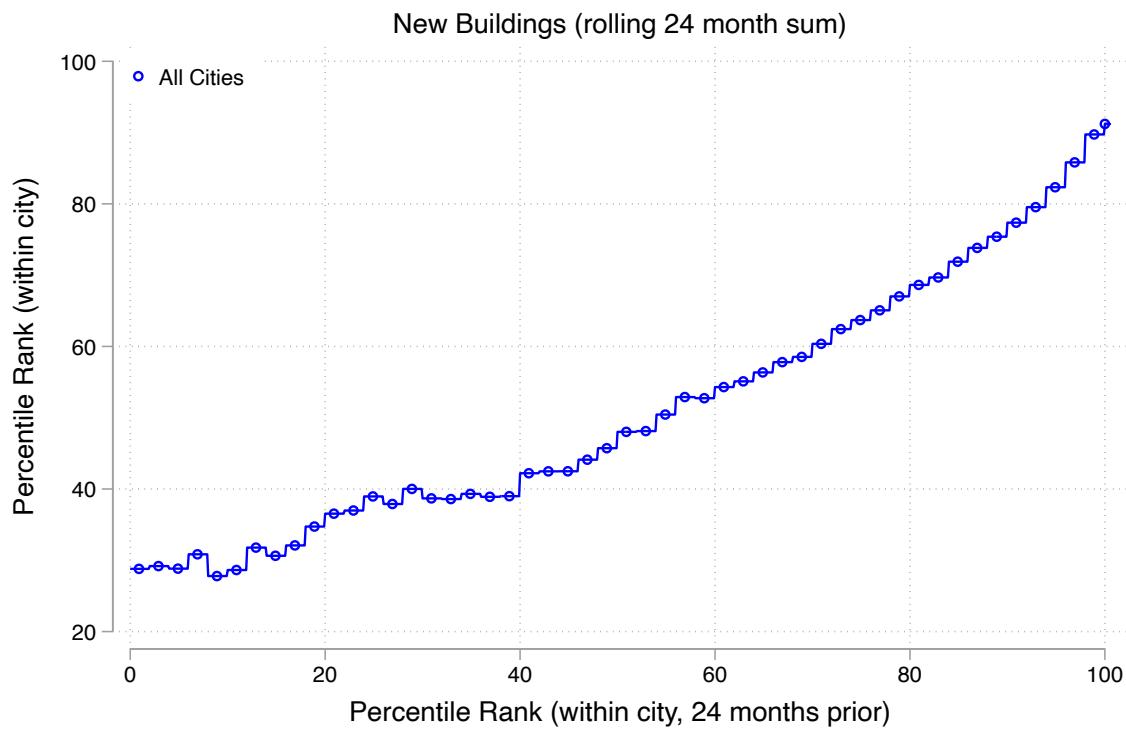
Note: This bin scatterplot shows the change in the log median family income from the 2015 to 2019 ACS against the monthly average of new buildings permitted for from 2014 to 2017. A line of best fit is depicted in red.

Figure A.9: New developments case study: Brooklyn



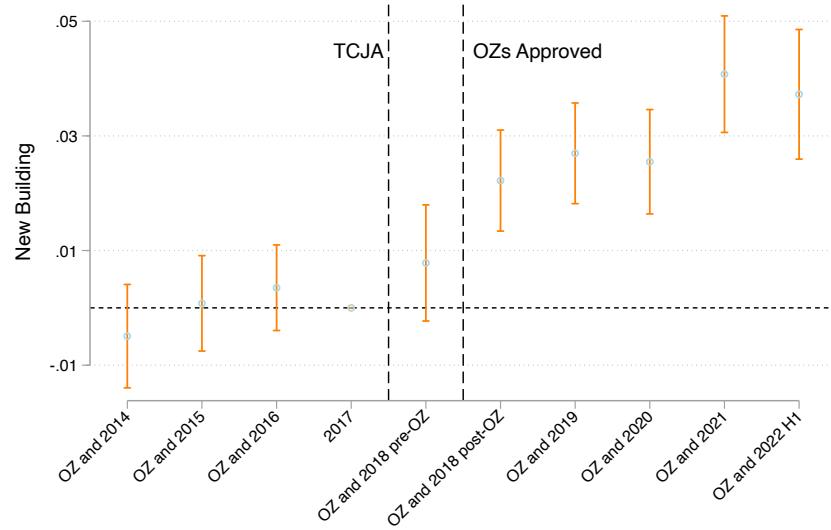
Note: These maps shows the number of new buildings over 2 year horizons for census tracts in Brooklyn.

Figure A.10: Persistence in new development



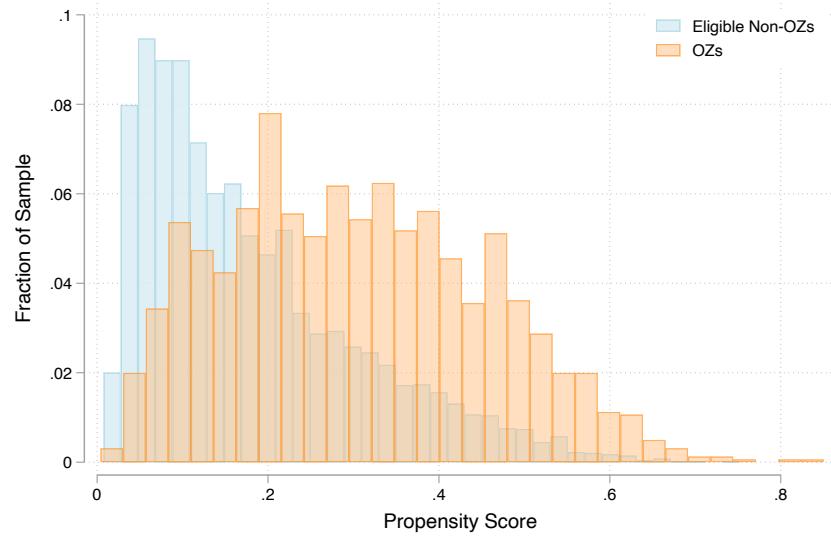
Note: These figures are produced by ranking tracts within cities in terms of the number of new buildings with permits issued in the previous 24 months. I then plot this percentile rank on its 24 month lag, and aggregate within 2-percentile bins across months.

Figure A.11: Difference-in-difference estimates balancing sample



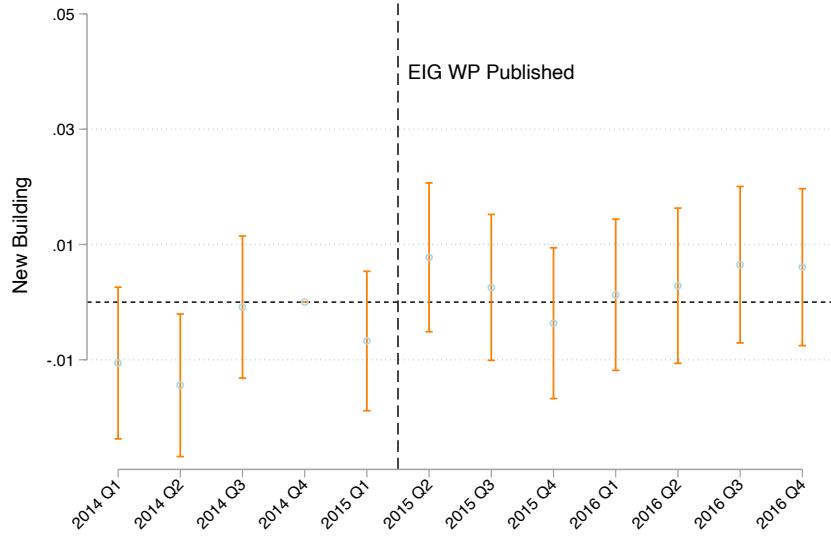
Note: This figure plots the annual version of the main difference-in-differences coefficients. However, a logistic model is run between any time period and right before the policy is implemented to estimate how ACS covariates affect whether the tract is in the sample or not. Observations are then reweighted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

Figure A.12: Overlap of propensity scores between OZs and eligible non-OZs



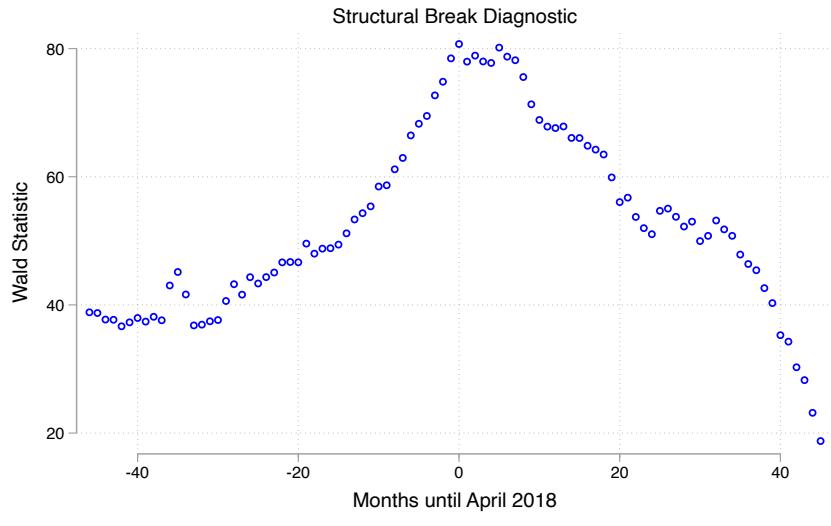
Note: This chart plots propensity scores for OZs against eligible non-OZs. Propensity scores were estimated via a logit model with 2015 5-year ACS tract-level demographics and local housing market covariates as predictors.

Figure A.13: Placebo using EIG white paper release date



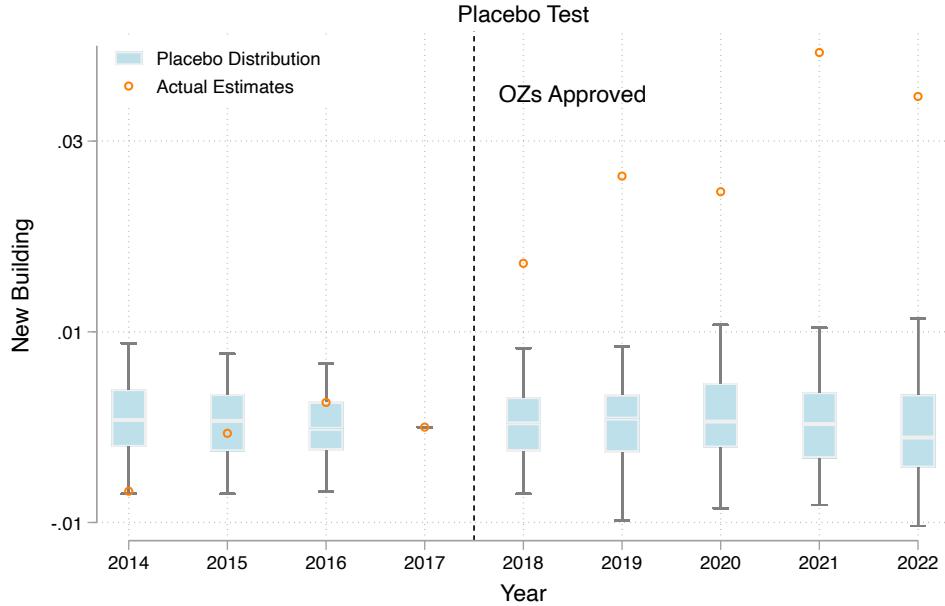
Note: This figure plots difference-in-differences coefficients from a version in which May 2015 (the publication date of the EIG white paper proposing the OZ tax credit) is the program implementation date. The model uses the same controls as the baseline specification: city linear trends, city seasonal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

Figure A.14: Andrews (1993, 2003) test for a structural break



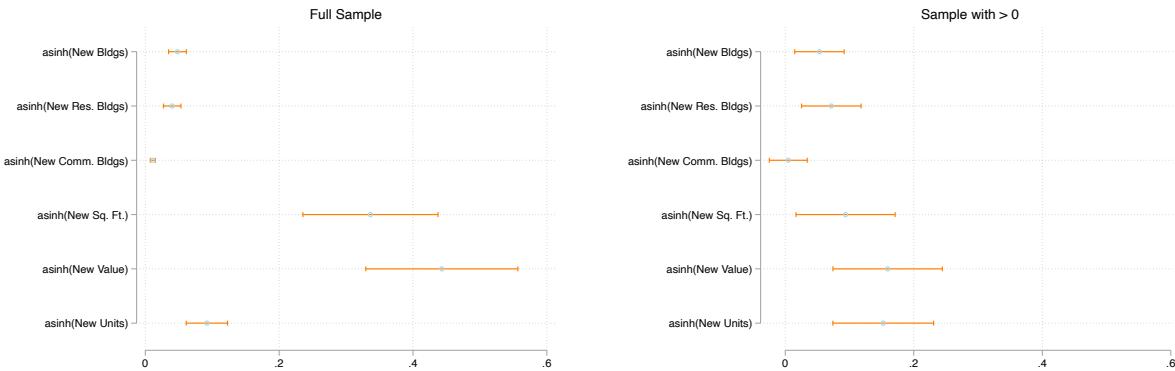
Note: This chart plots the Wald statistic for testing the null hypotheses that the coefficient on $\mathbb{1}\{i \text{ is a OZ}\} \cdot \mathbb{1}\{t \text{ is after } j\}$ in the baseline specification is zero, for each j from Jun 2014 to October 2019. All errors are clustered at tract-level.

Figure A.15: Placebo Tests



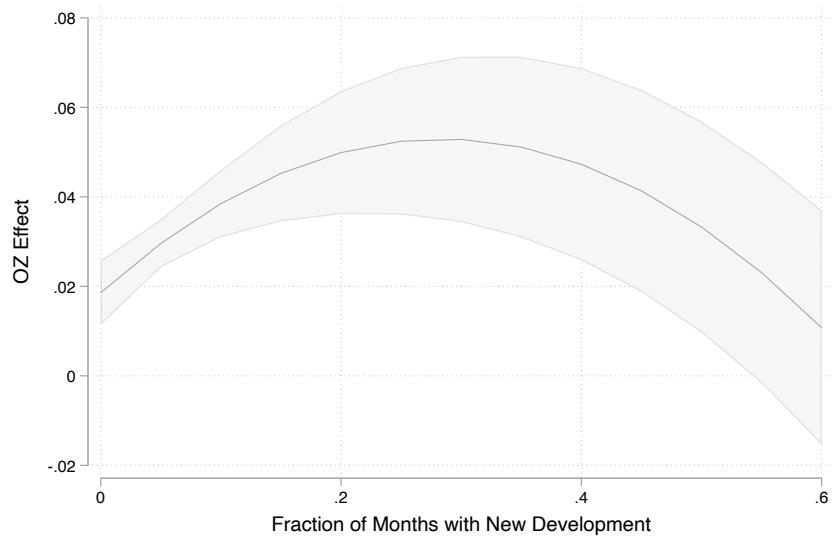
Note: This chart shows the distribution of point estimates from a series of placebo OZ programs. To implement, I simulate 100 different OZ programs by randomly drawing OZs from the population of eligible tracts (with probability equal to the fraction of eligible tracts that were actually chosen as OZs). 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.16: Intensive margins of response to OZ program



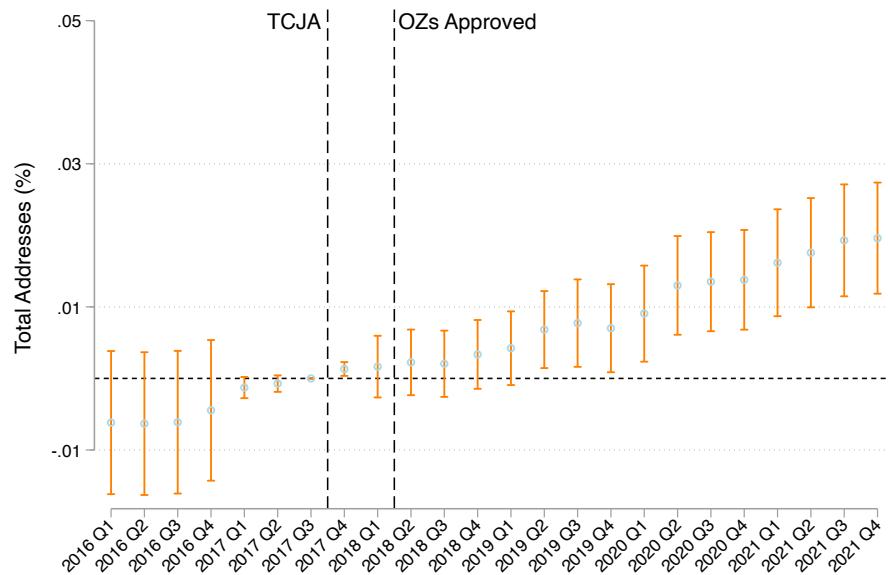
Note: These figures contain difference-in-differences estimates on various outcomes. The left hand chart runs these regressions on the full sample. The right hand chart conditions the sample to observations in which the outcome is greater than zero. All outcomes are transformed using the inverse hyperbolic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

Figure A.17: Heterogeneous policy response in prior development



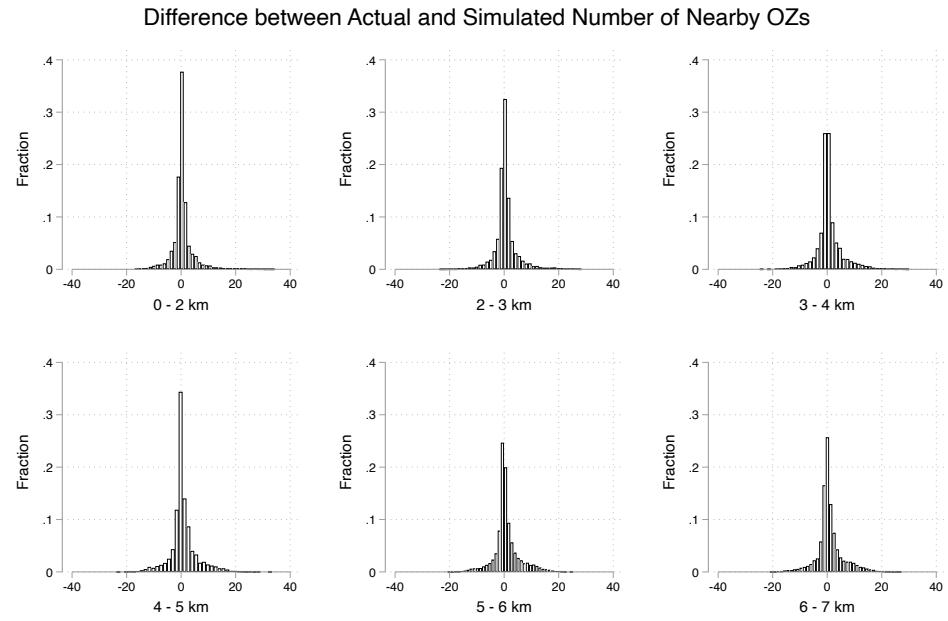
Note: This chart plots treatment effects from the regression model of Table 7. Errors are clustered at tract-level.

Figure A.18: Difference-in-differences with addresses



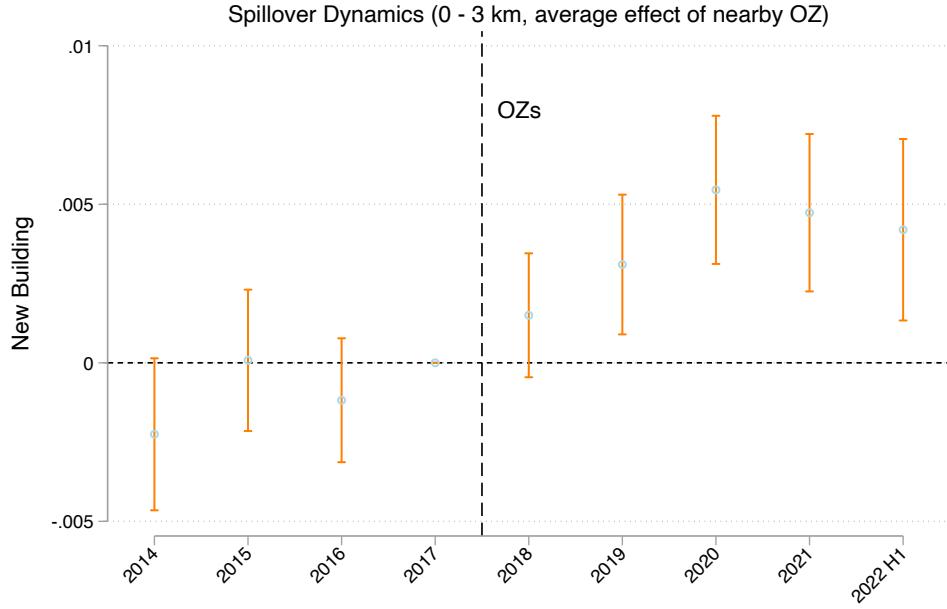
Note: This chart shows difference-in-differences coefficients from a poisson pseudo-maximum likelihood estimator. The outcome is total addresses that appear in a tract in a given quarter. Tract fixed effects, eligibility by month fixed effects, and city trends are included. All errors are clustered at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

Figure A.19: Distribution of $N_i^k - \hat{\mu}_i^k$ for distance bands k



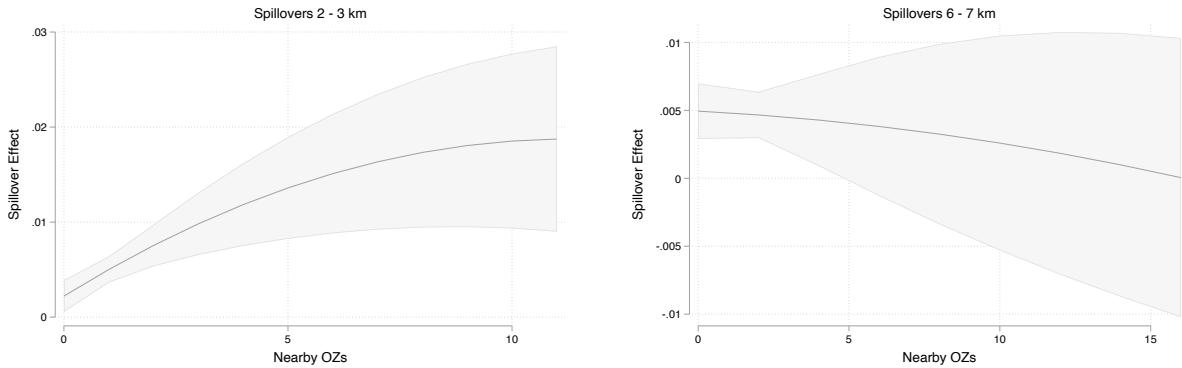
Note: This chart plots the distribution of differences between the actual number of OZs and the expected number of OZs across tracts and for different distance bands. The expected number of OZs is calculated by simulating OZ status among eligible tracts in a city according to the city-specific empirical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

Figure A.20: Spillover dynamics



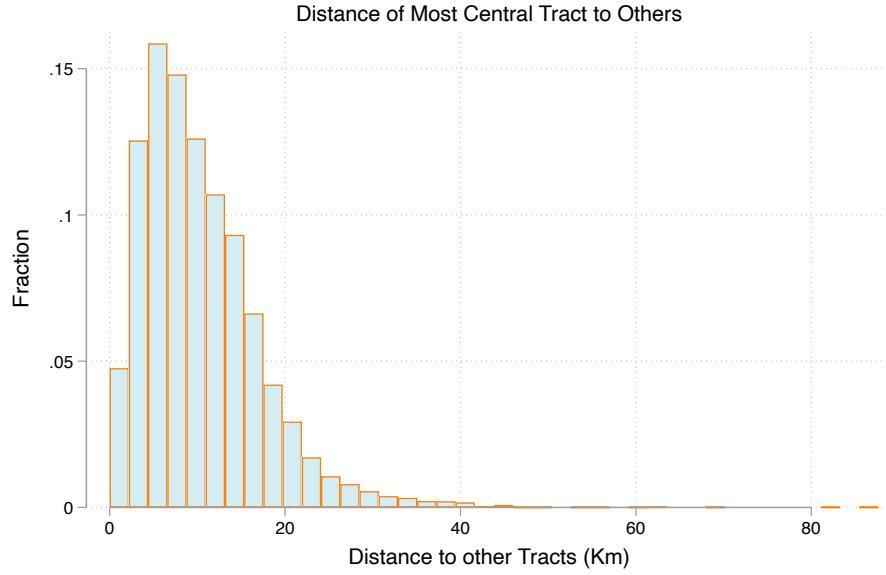
Note: This chart plots difference-in-difference coefficients from the main spillovers specification. The exposure to nearby OZs at various distances is interacted with year. I then scale (according to the average number of nearby OZs) and combine the coefficients for the distance bands 0-2 km and 2-3 km. These are the distances where I detect a positive spillover effect. Thus, the coefficient can be interpreted 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.21: Non-linearity in spillovers



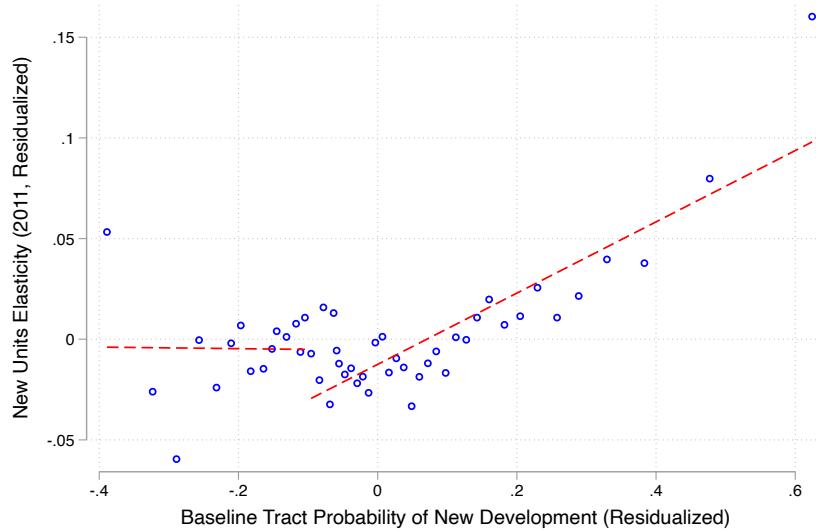
Note: This chart plots quadratic effects of having nearby OZs at 0-2 km (left) and 6-7 km (right). The main spillovers specification is augmented with a linear and quadratic term in the number of nearby OZs at various distances. Following [Borusyak and Hull \(2020\)](#), I control for the expected number 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.22: Distribution of tract-tract distances



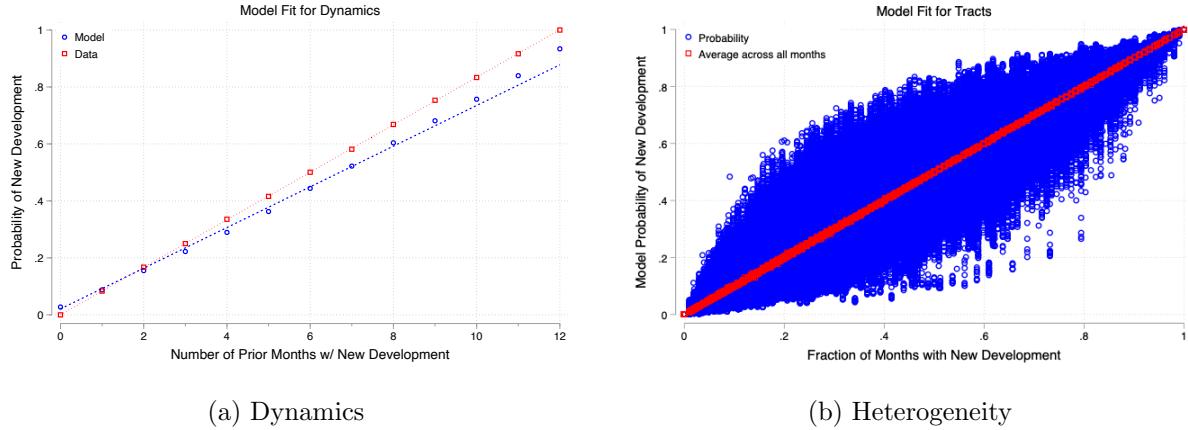
Note: This chart plots the distribution of distances from the centroid of the most central tract to all other tracts within the city.

Figure A.23: Model comparison with [Baum-Snow and Han \(2019\)](#)



Note: This table compares housing supply elasticity 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 separately for residual values less than -0.2 and greater than -0.2 .

Figure A.24: Model fit



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.

B Additional Tables

Table B.1: 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 (2,083)	4,066 (1,819)	4,033 (1,901)	-33	0.19
Rural	0.17 (0.37)	0.19 (0.39)	0.24 (0.43)	0.05	0.00
Median Age	39.1 (7.5)	36.1 (7.3)	35.2 (7.2)	-0.9	0.00
% White	0.74 (0.25)	0.63 (0.28)	0.58 (0.30)	-0.05	0.00
% Black	0.13 (0.22)	0.21 (0.27)	0.27 (0.30)	0.06	0.00
% Foreign	0.06 (0.08)	0.09 (0.10)	0.09 (0.10)	0.00	0.00
% High School	0.86 (0.11)	0.79 (0.12)	0.77 (0.12)	-0.02	0.00
% College	0.29 (0.19)	0.18 (0.13)	0.16 (0.11)	-0.02	0.00
Median Family Income	69,156 (33,613)	45,487 (14,502)	40,492 (14,084)	-4995	0.00
% Poverty Rate	0.16 (0.12)	0.25 (0.11)	0.29 (0.12)	0.04	0.00
Median Home Value (1000s)	225 (196)	157 (129)	141 (117)	-16	0.00
Household Gini	0.42 (0.06)	0.44 (0.06)	0.45 (0.06)	0.01	0.00
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.2: 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.3: 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.necnc.gov/
Chattanooga, TN	Dec 2008 - May 2020	https://internal.chattadata.org/Economy/All-Permit-Data/v7br-pei3
Chicago, IL	Jan 2006 - Jun 2022	https://data.cityofchicago.org/
Cincinnati, OH	Jan 2010 - Jun 2022	https://data.cincinнатi.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://data.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.fortworthtx.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	Nov 2016 - Jun 2022	https://opendata.minneapolismn.gov/
Nashville, TN	Jan 2012 - Jun 2022	https://data.nashville.gov/
New Orleans, LA	Jan 1990 - Jun 2022	https://datadriven.nola.gov/home/
New York, NY	Jul 2016 - Jun 2022	https://opendata.cityofnewyork.us/
Norfolk, VA	Jul 1997 - 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/1i-building-permits
Phoenix, AZ	Jan 1997 - Jun 2022 (no 2002)	https://apps-secure.phoenix.gov/PDF/Search/IssuedPermit
Raleigh, NC	Apr 2000 - Jun 2022	https://data.ral.opendata.arcgis.com
Sacramento, CA	Jan 2007 - Jun 2022	https://data.ral.opendata.arcgis.com
San Antonio, TX	May 2003 - Mar 2020	FreedomofInformationRequest
San Francisco, CA	Dec 1981 - Jun 2022	https://data.ral.opendata.arcgis.com
San Jose, CA	Jan 2005 - Jun 2022	https://sjspermits.org/
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.state.mn.us/
Tacoma, WA	Jan 2015 - Jun 2022	https://wspdmap.cityoftacoma.org/websit/PDS/permits/
Tampa, FL	Jan 2010 - Jun 2022	http://www.cividata.com/dataset/tampa_permit_standard_permits_v11_11419
Tucson, AZ	Mar 1997 - Jun 2022	http://gisdata.fucsonaz.gov/datasets/permits-planning-and-development-services-open-data
Virginia Beach, VA	Jan 2016 - Jul 2020	https://data.vbgov.com/

Table B.4: OZ effect using developer-level variation

	(1) New Projects	(2) New Projects	(3) New Projects
T x Post	0.0125*** (0.00163)	0.0147*** (0.00165)	0.00217 (0.00196)
Observations	1,494,392	1,494,392	1,494,392
<i>R</i> 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.5: Robustness to trends

	(1) New Building	(2) New Building	(3) New Building	(4) New Building
OZ and 2014	-0.00672 (0.00445)	-0.00701 (0.00447)	-0.00442 (0.00450)	-0.00342 (0.00457)
OZ and 2015	-0.000642 (0.00415)	-0.000360 (0.00416)	0.000880 (0.00420)	0.000721 (0.00425)
OZ and 2016	0.00260 (0.00369)	0.00275 (0.00370)	0.00387 (0.00373)	0.00370 (0.00380)
OZ and 2018 pre-OZ	0.00714 (0.00510)	0.00721 (0.00511)	0.00594 (0.00514)	0.00490 (0.00521)
OZ and 2018 post-OZ	0.0216*** (0.00440)	0.0216*** (0.00441)	0.0204*** (0.00444)	0.0193*** (0.00452)
OZ and 2019	0.0263*** (0.00438)	0.0260*** (0.00439)	0.0238*** (0.00442)	0.0208*** (0.00454)
OZ and 2020	0.0247*** (0.00452)	0.0234*** (0.00453)	0.0200*** (0.00455)	0.0184*** (0.00464)
OZ and 2021	0.0393*** (0.00507)	0.0380*** (0.00508)	0.0356*** (0.00509)	0.0306*** (0.00520)
OZ and 2022 H1	0.0347*** (0.00582)	0.0342*** (0.00583)	0.0311*** (0.00585)	0.0260*** (0.00600)
Observations	1,175,040	1,175,040	1,175,040	1,175,040
R²	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.6: Difference-in-difference at eligibility cutoffs

	(1) New Building	(2) New Building	(3) New Building	(4) New Building
OZ and 2014	-0.0251*** (0.00510)	-0.0129* (0.00744)	-0.00763 (0.00883)	0.00328 (0.0105)
OZ and 2015	-0.0182*** (0.00476)	-0.0119* (0.00699)	-0.00979 (0.00837)	0.000556 (0.0101)
OZ and 2016	-0.00857** (0.00409)	-0.00290 (0.00608)	7.16e-05 (0.00729)	0.0117 (0.00903)
OZ and 2018 pre-OZ	0.0177*** (0.00556)	0.0219*** (0.00840)	0.0297*** (0.0102)	0.0315** (0.0126)
OZ and 2018 post-OZ	0.0293*** (0.00485)	0.0352*** (0.00735)	0.0388*** (0.00869)	0.0427*** (0.0106)
OZ and 2019	0.0417*** (0.00490)	0.0349*** (0.00729)	0.0289*** (0.00839)	0.0393*** (0.0104)
OZ and 2020	0.0521*** (0.00520)	0.0365*** (0.00765)	0.0293*** (0.00879)	0.0372*** (0.0107)
OZ and 2021	0.0675*** (0.00572)	0.0457*** (0.00831)	0.0366*** (0.00962)	0.0410*** (0.0117)
OZ and 2022 H1	0.0618*** (0.00659)	0.0453*** (0.00951)	0.0385*** (0.0113)	0.0410*** (0.0137)
Observations	563,848	244,493	161,501	106,492
R²	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.	[-∞,∞]	[-10,10]	[-7,7]	[-5,5]
MFI BW 1000s	[-∞,∞]	[-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.7: 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 (0.00826)	-0.0120 (0.00784)	-0.000410 (0.00305)	-0.101 (0.0684)	-0.0397 (0.0796)	-0.0236 (0.0189)
OZ and 2015	0.000693 (0.00737)	-0.00169 (0.00702)	0.000731 (0.00266)	-0.0144 (0.0623)	0.00473 (0.0723)	0.00694 (0.0170)
OZ and 2016	0.00463 (0.00634)	0.00117 (0.00598)	0.000629 (0.00268)	0.00435 (0.0544)	0.00881 (0.0634)	0.00380 (0.0148)
OZ and 2018 pre-OZ	0.00749 (0.00804)	0.00593 (0.00747)	0.000508 (0.00347)	0.0270 (0.0757)	0.0853 (0.0886)	0.0146 (0.0195)
OZ and 2018 post-OZ	0.0317*** (0.00739)	0.0234*** (0.00713)	0.00916*** (0.00289)	0.289*** (0.0653)	0.329*** (0.0764)	0.0500*** (0.0166)
OZ and 2019	0.0421*** (0.00782)	0.0340*** (0.00763)	0.00853*** (0.00270)	0.254*** (0.0658)	0.348*** (0.0755)	0.0734*** (0.0176)
OZ and 2020	0.0457*** (0.00867)	0.0378*** (0.00862)	0.00836*** (0.00271)	0.241*** (0.0691)	0.389*** (0.0809)	0.0885*** (0.0188)
OZ and 2021	0.0625*** (0.0100)	0.0500*** (0.00980)	0.0167*** (0.00306)	0.402*** (0.0800)	0.587*** (0.0930)	0.112*** (0.0228)
OZ and 2022 H1	0.0601*** (0.0114)	0.0454*** (0.0110)	0.0185*** (0.00398)	0.472*** (0.0870)	0.679*** (0.104)	0.155*** (0.0270)
Observations	1,174,851	1,174,851	1,174,851	617,340	848,197	497,613
<i>R</i> 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 interpreted 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.8: 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*** (0.0145)						0.000979 (0.0242)
T x Elasticity (New Units)		0.0758*** (0.0116)					0.0393* (0.0210)
T x Log Home Value			-0.0227*** (0.00395)				-0.0168*** (0.00486)
T x Log MFI				-0.0140* (0.00798)			-0.0192 (0.0138)
T x College Shr					-0.123*** (0.0281)		-0.0308 (0.0337)
T x Poverty Shr						0.0107 (0.0254)	-0.0747* (0.0401)
Observations	1,175,040	1,175,040	1,175,040	1,175,040	1,175,040	1,175,040	1,175,040
<i>R</i> 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.9: 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 (0.00232)					-0.000426 (0.00309)
T x Local Zoning Approval		-0.0182*** (0.00432)				-0.0220*** (0.00614)
T x Local Project Approval			-0.00382 (0.00248)			0.00910** (0.00401)
T x Density Restrictions				0.0259** (0.0119)		0.0289** (0.0122)
T x Approval Delay					-0.00433*** (0.000656)	-0.00349*** (0.000778)
Observations	1,108,024	1,108,024	1,108,024	1,108,024	1,108,024	1,108,024
<i>R</i> 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.10: Home value and rents

	(1)	(2)	(3)	(4)
	Log Home Value (Q25)	Log Home Value (Q50)	Log Home Value (Q75)	Log Rent
OZ and 2015	-0.000199 (0.00536)	-0.00112 (0.00484)	-0.00942* (0.00546)	-0.00199 (0.00274)
OZ and 2016	-0.00469 (0.00809)	-0.00127 (0.00733)	0.000173 (0.00738)	0.00246 (0.00423)
OZ and 2018	0.0151*** (0.00452)	0.00651* (0.00356)	0.0151*** (0.00394)	-0.00239 (0.00224)
OZ and 2019	0.0231*** (0.00706)	0.0157*** (0.00500)	0.0200*** (0.00524)	-0.00184 (0.00311)
OZ and 2020	0.0435*** (0.00833)	0.0338*** (0.00631)	0.0336*** (0.00646)	0.00411 (0.00411)
Observations	59,418	62,592	62,358	58,788
<i>R</i> 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 test 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.11: Balance table for spillovers analysis

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$\Delta \text{Log MFI}$	$\Delta \text{Log Pop.}$	$\Delta \text{Log Home Value}$	$\Delta \% \text{ Poverty}$	$\Delta \% \text{ College}$	$\Delta \% \text{ High School}$	$\Delta \% \text{ White}$	$\Delta \% \text{ Black}$
$N^0 - \hat{\mu}_i^0$	0.000912 (0.000642)	0.000210 (0.000375)	-9.29e-05 (0.000590)	-0.0126 (0.0202)	0.000235 (0.0181)	0.0188 (0.0162)	0.0431** (0.0202)	-0.0185 (0.0154)
$N^2 - \hat{\mu}_i^2$	0.000230 (0.000683)	1.63e-05 (0.000396)	0.000840 (0.000645)	0.00550 (0.0205)	0.0182 (0.0171)	0.0150 (0.0169)	-0.00756 (0.0219)	-0.00775 (0.0160)
$N^3 - \hat{\mu}_i^3$	-0.000594 (0.000622)	0.000171 (0.000372)	-0.000338 (0.000587)	-0.0151 (0.0189)	0.0172 (0.0167)	0.00776 (0.0156)	0.00264 (0.0206)	0.0169 (0.0154)
$N^4 - \hat{\mu}_i^4$	-0.000453 (0.000581)	-0.000500 (0.000369)	0.000372 (0.000508)	0.000379 (0.0170)	-0.00286 (0.0154)	0.0238 (0.0161)	0.00748 (0.0193)	-0.0182 (0.0144)
$N^5 - \hat{\mu}_i^5$	0.000404 (0.000549)	0.000398 (0.000312)	-0.000288 (0.000515)	0.00728 (0.0165)	-0.00591 (0.0144)	-0.0198 (0.0138)	0.0415** (0.0184)	-0.00129 (0.0137)
$N^6 - \hat{\mu}_i^6$	0.000136 (0.000445)	-0.000188 (0.000255)	-0.000353 (0.000391)	-0.00140 (0.0129)	-0.00488 (0.0121)	-0.00142 (0.0114)	-0.00300 (0.0154)	0.0167 (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 interpreted 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.12: 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 (0.00797)							0.000413 (0.00823)
T x Developable Land Shr. (Low)		0.0305*** (0.00817)						0.00575 (0.0123)
T x Elasticity (New Units)			0.0294*** (0.00846)					-0.00189 (0.0132)
T x Log Home Val.				-0.0182*** (0.00213)				-0.0173*** (0.00267)
T x Log MFI					-0.0181*** (0.00284)			-0.00814 (0.00548)
T x College Shr.						-0.00181 (0.0112)		0.0390** (0.0152)
T x Pov. Rate							-0.0177 (0.0154)	-0.0635*** (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
<i>R</i> 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.13: Simulation of model

Parameters [$\lambda, \delta, \gamma, \beta$]	Effect of OZ at (2,2) on Stationary Distribution (%)	Effect of OZ at (1,3) on Stationary Distribution (%)
[0, 1, 0, 0.5]	$\begin{bmatrix} +0.00\% & +0.00\% & +0.00\% \\ +0.00\% & +12.44\% & +0.00\% \\ +0.00\% & +0.00\% & +0.00\% \end{bmatrix}$	$\begin{bmatrix} +0.00\% & +0.00\% & +10.86\% \\ +0.00\% & +0.00\% & +0.00\% \\ +0.00\% & +0.00\% & +0.00\% \end{bmatrix}$
[2, 1, 0, 0.5]	$\begin{bmatrix} +3.59\% & +2.13\% & +1.73\% \\ +5.22\% & +12.67\% & +2.13\% \\ +5.40\% & +5.22\% & +3.59\% \end{bmatrix}$	$\begin{bmatrix} +0.95\% & +0.74\% & +5.75\% \\ +1.17\% & +0.98\% & +0.74\% \\ +1.17\% & +1.17\% & +0.95\% \end{bmatrix}$
[2, 1, 1, 0.5]	$\begin{bmatrix} +2.31\% & +1.09\% & +0.84\% \\ +4.12\% & +9.99\% & +1.09\% \\ +4.39\% & +4.12\% & +2.31\% \end{bmatrix}$	$\begin{bmatrix} +0.38\% & +0.25\% & +3.41\% \\ +0.57\% & +0.41\% & +0.25\% \\ +0.59\% & +0.57\% & +0.38\% \end{bmatrix}$
[2, 0.5, 1, 0.5]	$\begin{bmatrix} +1.56\% & +0.78\% & +0.69\% \\ +2.68\% & +8.89\% & +0.78\% \\ +2.77\% & +2.68\% & +1.56\% \end{bmatrix}$	$\begin{bmatrix} +0.45\% & +0.25\% & +3.65\% \\ +0.69\% & +0.45\% & +0.25\% \\ +0.70\% & +0.69\% & +0.45\% \end{bmatrix}$

Note: This table shows simulated stationary distributions for a city given by the following heterogeneity terms: $\alpha_{(1,2)} = \alpha_{(1,3)} = \alpha_{(2,3)} = -1$, $\alpha_{(1,1)} = \alpha_{(2,2)} = \alpha_{(3,3)} = -0.25$, and $\alpha_{(2,1)} = \alpha_{(3,1)} = \alpha_{(3,2)} = 0.5$. The coordinates of each location in the city are given by its matrix index. The table contains estimates of how the (exact) stationary distribution changes with two OZ policies: (i) where location (2, 2) receives OZ status, (ii) location (1, 3) receives OZ status. Simulation details and discussion are given in [Appendix E](#).

Table B.14: Model fit to reduced-form effects

	(1)	(2)	(3)
	Data	Model	Diff.
QOZ x Post	0.0273*** (0.00330)	0.0240*** (0.00140)	0.00329 (0.00238)
Observations	1,029,840	1,029,840	2,059,680
R²	0.310	0.937	0.464
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.3](#).

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. In all, these decisions likely introduced some measurement error into the geolocation procedure, which would tend to bias down my estimates of the OZ effect on new development.

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

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

cities, I was also able to identify permits for demolitions as well.

I drop permits that were rejected. I use the date of permit approval for the new development. For a few permits, if the approval date was missing, I would use the date of submission. In all, these decisions were meant to mitigate measurement error, but if anything would bias my results towards finding a smaller OZ effect.

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.

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

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 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. Whether these are investments that would have happened without OZ tax credits is unclear. In the near-term, there is no evidence of an immediate increase in developments followed by a subsequent decline - in fact, the OZ policy effects grow over time. I present evidence that OZs were not experiencing heightened new development prior to the program either. 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 2014.⁴ I can identify developers or contractors for 34 of the 46 cities in my sample.⁵

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

⁴The latter I do to avoid names that cities use when they lack information about the specific developer.

⁵I have no developer information for Aurora, Durham, Detroit, Indianapolis, Honolulu, Henderson, Little Rock, Norfolk, Seattle, San Francisco, Tacoma, Tampa, Tucson, and Virginia Beach.

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

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

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

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.4](#). 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.5](#). 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.6](#) 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 6](#). The coefficients can be interpreted 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.13](#). 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.14](#) 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 \(2003\)](#). While not necessary for a credible difference-in-differences design, this test combined with positive OZ impacts across most cities in ?? demonstrate a wide-spread 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.15](#). 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.⁶

⁶These placebo tests can alternatively be viewed as demonstrating the treatment effects are significant under exact inference ([Hagemann, 2019](#)).

E Model Details

Simulation

A city is given by a 3×3 matrix of heterogeneity terms, with a gradient of low to high fundamentals for new development running from North-East to South-West. For a city of this size, I can calculate the exact stationary distribution of new development. Under different parameters, I calculate the difference in the stationary probability of new development for each location under an OZ program relative to no OZ program. The OZ program corresponds to choosing one of the nine “locations” as an OZ. The calculations are included in [Table B.13](#).

The first simulation turns off externalities and state-dependence. The model already has treatment effect heterogeneity through variation in location fundamentals; and as can be seen, targeting the “more-marginal” (closer to zero latent developer profits) location (2, 2) produces a higher policy impact than the less-marginal location (1, 3).

As we turn on externalities, two effects happen. First, the externalities will tend to lower the probability of new construction given the limited construction in this city. Second, through spillovers, surrounding areas will now increase new construction as well, as a function of their and their neighbors underlying heterogeneity. The high-fundamental southwest corner of the city sees the largest spillovers. Targeting location (1, 3) with low heterogeneity and surrounded by low fundamental locations will tend to mute the spillover effects.

Positive state-dependence, in a city that does not engage in new construction regularly, will suppress the stationary probability that a given tract engages in new construction. This will tend to push the policy impact down purely through the effect on latent developer profits. However, the policy impact will be magnified across time through its dynamics. Finally, decreasing δ allows the externality to operate more evenly across space. Latent profits will now be higher for low fundamental locations and lower for high fundamental locations - in effect, flattening the gradient in latent developer profits. Consequently, the policy impact and spillovers are larger relative to more localized spillovers (large δ) if we target the low-fundamental location (3, 1) but are smaller if we target the higher-fundamental location (2, 2).

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

equilibrium rational expectations. More details are given below. Due to the computational burden of this problem, I opt to estimate this model separately by the C cities⁷ in my sample to produce estimates $\hat{\theta}_k^*$ (a $C \times 1$ vector of the k th component of θ). When discussing the model estimates, I aggregate the city-specific estimates through the following procedure.

$$\hat{\theta}_k = \Omega'_k \hat{\theta}_k^*$$

I perform this pooled estimate for the set of common parameters: $\lambda, \delta, \gamma, \beta$. In my preferred specification, $\Omega_{kc} \propto 1/\mathbb{V}(\hat{\theta}_{kc}^*)$, the variance of the parameter estimate. This efficiently combines my city-specific parameter estimates, and approximates how the true joint maximum likelihood estimator would aggregate. Equally and tract-weighted versions perform similarly. 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.

$$\mathbb{P}_t^*(\theta, \omega_t) = \mathbf{G}_t(\mathbb{P}_t^*(\theta, \omega_t))$$

\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 $\mathbb{P}_t^*(\theta, \omega_t) - \mathbf{G}_t(\mathbb{P}_t^*(\theta, \omega_t)) = 0$. By the Implicit Function Theorem (where I_n is a $n \times n$ identity matrix)

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

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

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

A second derivative yields two set of terms. The first contains the residual $y_{it} - \mathbb{P}_{it}^*(\hat{\theta}, \omega_t)$, which

⁷ Additionally, I estimate New York separately by its five boroughs, as well as Los Angeles and Chicago separately by their Northern and Southern regions.

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

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

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

Model Estimation

All calculations were performed using Python version 3.7.0. 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 interior root finding can lead to flat regions in the likelihood function i.e. small changes in the parameter values may not have a large impact on the equilibrium expectations, in part due to numerical approximations in the root-finding. To help, I include a regularization term; I add $\kappa \cdot \|\theta - \tilde{\theta}\|^2$ to the likelihood. This adds concavity to the likelihood function which aids the estimation. $\tilde{\theta}$ is the vector which I am “shrinking” my parameter estimates too, and is taken to be zero for all parameters except the location heterogeneity terms, which I shrink towards their average. This penalty term makes the procedure look like ridged-logistic regression. I set $\kappa = 0.1/(NT)$.

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-exact” in function `minimize`). Analytic gradients are calculated and used in the root-finding and opti-

mization 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 k th component of k . `root_solver` refers to the inner loop – the rational expectations solver. `local_maximizer` refers to the outer loop – the likelihood maximization procedure. `global_maximizer` refers to the stochastic optimization that wraps the entire estimation procedure, re-estimating at stochastically chosen initial values and stopping when some criterion is achieved for the local maxima.

Pseudo-code

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

```

1:  $j = 0$ 
2:  $\theta_0 = \theta_0^* = 0$ 
3:  $L_{-1} = -10^3$ 
4:  $tol_1 = tol_2 = 10^3$ 
5: while  $tol_1 > \varepsilon_1$  do
6:    $k = 0$ 
7:   while  $tol_2 > \varepsilon_2$  do
8:      $P = \{\mathbb{P}_t^*(\theta_k)\} \leftarrow \text{root\_solver}(\theta_k, t)$ 
9:      $L_k \leftarrow \max_P \mathcal{L}$ 
10:     $tol_2 \leftarrow |L_k - L_{k-1}|$ 
11:    if  $tol_2 \leq \varepsilon_2$  then
12:      return  $\theta_{j+1}^* \leftarrow \theta_k$ 
13:    end if
14:     $\theta_{k+1} \leftarrow \text{local\_maximizer}(\theta_k)$ 
15:     $k \leftarrow k + 1$ 
16:  end while
17:   $tol_1 \leftarrow \|\theta_{j+1}^* - \theta_j^*\|$ 
18:  if  $tol_1 \leq \varepsilon_1$  then
19:    return  $\hat{\theta} \leftarrow \theta_{j+1}^*$ 
20:  end if
21:   $\theta_0 \leftarrow \text{global\_maximizer}(\theta_{j+1}^*)$ 
22:   $j \leftarrow j + 1$ 
23: 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 time and eligibility by year effects from 2019, 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 (option “df-sane” in function `root`). 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.