JUE Insight: The (Non-)Effect of Opportunity Zones on Housing Prices*

Jiafeng Chen  
Harvard University

Edward Glaeser  
Harvard University

David Wessel  
Brookings Institution

April 15, 2022

Abstract

Will the Opportunity Zones (OZ) program, America’s largest new place-based policy in decades, generate neighborhood change? We compare single-family housing price growth in OZs with price growth in areas that were eligible but not included in the program. We also compare OZs to their nearest geographic neighbors. Our most credible estimates rule out price impacts greater than 0.5 percentage points with 95% confidence, suggesting that, so far, home buyers don’t believe that this subsidy will generate major neighborhood change. OZ status reduces prices in areas with little employment, perhaps because buyers think that subsidizing new investment will increase housing supply. Mixed evidence suggests that OZs may have increased residential permitting.

*Chen: jchen@hbs.edu; Glaeser: eglaeser@harvard.edu; Wessel: dwessel@brookings.edu. We thank the editor and two anonymous referees for helpful comments that improved the paper substantially. We thank participants of AEA 2021 session “Do OZs Create Opportunities” and the Brookings Institution conference on OZs, as well as Isaiah Andrews, Fernando Ferreira, Amy Finkelstein, Amit Khandelwal, Mike Luca, Ashesh Rambachan, Jonathan Roth, Suproteem Sarkar, Liyang Sun, Susan Wachter, and Christopher Walker for helpful discussions. We especially thank Yanchen Jiang for superb research assistance. All errors are our own. All replication files are available at https://github.com/jiafengkevinchen/OZ-CGW.


1 Introduction

Opportunity Zones (OZs), established by the Tax Cuts and Jobs Act of 2017 (TCJA), arguably represent the U.S. government’s largest place-based policy innovation since Empowerment Zones were introduced in 1993. When capital gains are invested in OZs, taxes on the original gains are deferred and can be reduced; taxes on future gains from qualifying investments in OZs are largely eliminated. Will this significant tax-based subsidy lead to neighborhood improvements?

We test this hypothesis by examining whether areas that are designated as OZs subsequently experience an increase in residential real estate prices. If OZ designation is seen as a harbinger of future investment, then new buyers should presumably anticipate future neighborhood improvements and be willing to pay more for homes. If OZs are ineffective, or act primarily to generate more residential supply, then OZs will have little impact on price. Indeed, there is some empirical evidence that local housing prices respond to changes in neighborhood-level housing supply; see Asquith et al. (2019); Pennington (2021); Li (2019). It is too early to test the more interesting and important question of whether OZs impact people as well as place, and whether they positively impact the lives of neighborhood residents.\(^1\) Moreover, we only have price data available for 2018–2020, and the OZ tracts were designated by governors in the first few months of 2018 and officially posted in July.

A non-peer reviewed study of housing prices, done by Zillow (Casey, 2019), found a positive effect of OZs on prices when comparing OZ areas with areas that did not receive zone status, but there are two reasons to be cautious about this work. First, the pre-2017 trends in prices between their treatment and control samples do not appear to be parallel. They provide no tested pre-trends. Second, Casey (2019) results also do not control for changes in the quality of houses sold, since they include all arms’ length sales prices. Relatedly, another non-academic study by ATTOM Data Solutions (2020) observes that OZs found median home price increases, also using proprietary data. However, the study does not compare OZs to similar places without OZ designation, and thus its results cannot be interpreted causally. More recently, the CEA (2020) replicated our analysis from the working paper version of this paper (Chen et al., 2019).

We use the FHFA repeat sales-indices for single-family homes at the tract and ZIP code level to measure price changes (Bogin et al., 2019).\(^2\) We perform three different empirical exercises. First, we follow Casey (2019) and compare OZ areas with areas that were initially eligible for OZ status, but then not included as OZs, assuming and testing for parallel trends. Second, we use propensity-score weighting methods to include observed characteristics nonparametrically in the difference-in-difference design, making and testing a conditional parallel trends assumption. Third, we compare OZ areas with bordering areas. As many tracts have missing data, we perform the exercises at the ZIP code level in addition to at the tract level as well.

\(^1\)Busso and Kline (2008) provide a thorough analysis of the economic impact of national employment zones, which appear to have meaningfully impacts both housing prices and employment. Neumark and Kolko (2010) find fewer positive effects of state level Enterprise Zones.

\(^2\)Our data do not include multi-family housing or commercial real estate. See Sage et al. (2019) on possible impact of OZs on prices of commercial real estate and land.
All exercises yield a similar result: OZs appear to have a negligible price impact that is statistically indistinct from zero. Our results are sufficiently precise that we can generally rule out a tract-level price impact of 1.5 percentage points or more annually. Our point estimates are typically between 0 and 0.5 percentage points. This finding suggests that buyers do not believe that OZ status will generate a significant change in the economic fortunes of the neighborhood. Alternatively, buyers could be myopic, but that seems unlikely if the zone status attracts professional investors. One possible explanation for our null result is that the overall treatment effect combines a positive shock to housing demand and a positive shock to housing supply, since OZ status provides financial incentives that could encourage more residential construction. To test this possibility, we compare ZIP codes in predominantly residential areas with ZIP codes that are predominantly commercial areas. We hypothesized that the positive impact of OZs on housing supply should be stronger in already residential areas, and so the impact of OZ status on housing prices should be lower in those areas.

With an interaction specification, we do find that the treatment effect of zone status is weakly positive in non-residential areas, and the interaction between OZ status and being predominantly residential is negative. Our point estimates do indeed suggest that OZs appear to have increased prices in less residential areas and reduced prices in more residential areas. Yet the estimated effects are small and our standard errors are too large to rule out a zero impact in either type of area.

We supplement our primary work on prices by examining the impact of OZs on building permits, which is our best measure of new housing supply. Since places are a much coarser geography, we present results using a simple difference-in-difference methodology. We do find a small positive impact of OZ on the number of permits, but not on the number of permits relative to the stock of housing in 2010. Our results are compatible with the view that OZ status encouraged some construction, especially in smaller places.

This work does not imply that OZs were a mistake or that there are no benefits from these zones. These tax subsidies may generate neighborhood change in the future that buyers do not anticipate today. The costs of these subsidies may end up being so small that they are offset by even tiny price gains. Nonetheless, the absence of a visible price effect does suggest the limits of place-based policies, especially those that focus on investment in physical rather than human capital.

This paper proceeds as follows. Section 2 discusses the institutional context, policy details, and selection procedures for OZs. Section 3 discusses the various data sources used in our study. Section 4 introduces our three main empirical strategies. Section 5 discusses the empirical results. Section 7 concludes. We relegate a description of various data sources used in the study to Section 3.

---

3There is also mixed evidence on potential employment benefits; see Arefeva et al. (2021); Atkins et al. (2020); Freedman et al. (2021).
OZs were created in December 22, 2017, when President Trump signed the TCJA into law. They are intended to spur economic development in distressed communities. The law provides three benefits for investing capital gains in one of 8,762 Census tracts (12% of all tracts) across the country through intermediaries called Opportunity Funds: Tax on the initial capital gain is deferred until 2026 or when the asset is sold. For capital gains placed in an Opportunity Fund for at least five years, investors’ basis on the original investment increases by 10%; if invested for seven years, by 15%. For investments in Opportunity Funds held for at least 10 years, the gains on the investments in the zones are not subject to capital gains tax. Funds can be invested in commercial, residential and industrial real estate; infrastructure, and businesses. For real estate projects to qualify, the investment must result in the property being “substantially improved.”

The outlines of a proposal to create OZs was published by the Economic Innovation Group in April 2015 (Economic Innovation Group, 2015). Bills to create them were first introduced in Congress in April 2016 and re-introduced in February 2017, but got little attention initially. As in the bill that eventually became law, the proposals authorized governors to nominate as OZs 25% of the “low income communities” in their states; in states with fewer than 100 low income communities, the governor could choose 25. The definition of low income communities was borrowed from a 2000 law that created the New Markets Tax Credit: tracts were designated as low income if the poverty rate is at least 20%, or the median family income doesn’t exceed 80% of the statewide median for a tract outside the metropolitan area, or the median income doesn’t exceed 80% of the statewide median or the metro area statewide median for a tract inside a metropolitan area. Tracts contiguous with low income communities also are eligible, provided their median family income doesn’t exceed 125% of the contiguous low income community.

The OZ provision was not included in the House version of the TCJA, which was introduced on Nov. 2, 2017. With very little public attention, Sen. Tim Scott (R, S.C.), a member of the Senate Finance Committee, successfully pushed to include the OZ provision in the TCJA, which was introduced on Nov. 28, 2017. The first reference in the press to the OZ provision in the TCJA came on November 28, 2017, in South Carolina’s Post and Courier, according to the Factiva database (Lovegrove, 2017).

Governors had 90 days after the passage of the law—until March 21, 2018 unless they sought a 30-day extension—to nominate zones from a list of 31,866 eligible tracts prepared by the Treasury based on 2010-2015 American Community Survey data. The Treasury posted a list of all qualified OZs on July 9, 2018. Of the 8,764 OZs, 8,534 are low income communities and 230 are contiguous tracts. A map of the zones in the U.S. mainland is shown in Figure 4 in the appendix.

---

4Two additional tracts in Puerto Rico were added by subsequent legislation, bringing the total to 8,764. We do not have housing data for Puerto Rico.
5The 10% step-up in basis is only for OZ investments made by Dec. 31, 2021. The additional 5% step-up in basis is for investments by Dec. 31, 2019.
3 Data

Our measure of housing price growth is the annual change in the housing price index computed by the Federal Housing Finance Agency (FHFA). The housing price index is a weighted, repeat-sales price index of the movement of single-family house prices. Like all repeated-sales indices, it attempts to correct for quality changes. We use the annual house price indices by tract and by five-digit ZIP codes,\(^6\) and treat 2018 and 2019 as the treated years.

Information about the OZs is provided by the Urban Institute. The data includes whether a tract belongs to the 31,866 eligible tracts and to the selected 8,762 tracts and whether a tract is eligible for selection as a low income community or as contiguous tracts, which we use in our first and second empirical designs in Section 4. Characteristics of the Census tracts are from the American Community Survey (ACS) 2013–2017 5-year estimates. Among the 8,534 low-income selected tracts, 3,806 have price information in the sample period (2014–2020), of which only 2,917 have price information for the entire sample period. We discuss the representativeness of the various subsamples in Appendices A.2 and A.3, and our results are robust to using balanced versus unbalanced panels. Employment data at the tract level are available in the Longitudinal Employer-Household Dynamics data.

Geographical comparison between tracts and their non-selected geographical neighbors uses the TIGER 2018 geographic shapefiles provided by the Census. Aggregating tract-level data to ZIP-level data, implemented in Section 4.5, uses the geographical crosswalks between 5-digit USPS ZIP codes and Census tracts are provided by the Office of Policy Development and Research at the Department of Housing and Urban Development, where we use the data for the first quarter of 2017. Lastly, splitting ZIP codes by employment population in Table 2 uses population data at the ZCTA level in the 2012–2016 ACS 5-year estimates and employment data from the ZIP code level County Business Patterns in 2016.

We use the Building Permits Survey (BPS) for data on residential permitting. The finest geographical level that the BPS reports is the Census place, which is non-overlapping with tracts. As a result, we aggregate tracts up to places via crosswalk provided by the Missouri Census Data Center, which also provides estimates for housing units in 2010. Since New York City’s boroughs are different Census places but have the same FIPS identifier, we drop New York City from our analysis.

4 Empirical Strategy

We use three empirical strategies for the tract-level analyses. First, following Casey (2019), we compare OZs to tracts that are eligible for OZs but are not selected.\(^7\) In this case, we use a difference-

\(^6\)Links for data sources are provided in Appendix Table 7, accessed as of March 18, 2022.

\(^7\)In the results presented in the main text, we compare selected tracts to tracts that are eligible but not selected conditional on the eligibility criterion being low-income community, as tracts that are eligible for contiguity reasons are overrepresented in the non-selected group. Qualitative conclusions do not change if we remove this condition (Appendix B).
in-differences design that optionally incorporates observable tract-level covariates interacted with year fixed effects. We supplement the analysis in Casey (2019) with formal tests of pre-treatment trends. Second, we refine our analysis in the first design with semiparametric propensity score weighting methods in a difference-in-differences setting (Abadie, 2005; Callaway and Sant’Anna, 2018; Sant’Anna and Zhao, 2018). Third, we compare OZs with their geographical neighbors that are not selected.

The FHFA tract-level data covers only about half of the selected tracts. Concerned with the data attrition, we also aggregate tract-level data to the ZIP code level, and uses FHFA data at the ZIP code level, which has better coverage. We split the ZIP-code level data into quantiles by employment population so as to decompose the potential effects of OZs on supply and demand, noting that the positive supply effect has larger impact in residential areas. Lastly, we perform a similar analysis at the Census place level for the effect on permitting.

In the following description of methodology, we let $Y_{it}$ denote the observed outcome in geography $i$ and time $t$. We let $D_i$ denote OZ status and $D_{it}$ denote the interaction between OZ status and post-treatment time. We assume that the data across geography are i.i.d. sampled. As in the standard setting, the estimate target is the average treatment effect on the treated (ATT), mean-aggregated over post-treatment periods.

### 4.1 Baseline difference-in-differences (Casey, 2019)

In the first design, we compare tracts that are selected as OZs to tracts that are eligible but not selected. The parallel trends assumption allows us to identify the treatment effect in a difference-in-differences design. As we see in Figure 1(a), parallel trends is a more plausible assumption when comparing selected tracts to eligible but not selected tracts than when comparing selected tracts to all other tracts.

Our first estimation strategy uses a specification where we allow for tract-specific heterogeneity in levels and overall trends (Angrist and Pischke, 2008) via two-way fixed effects (TWFE): $Y_{it}(0) = \mu_i + \alpha_t + \epsilon^0_{it}$ and $Y_{it}(1) = \mu_i + \alpha_t + \tau + \epsilon^1_{it}$ with $E[\epsilon^0_{it} | D_i, \mu_i] = E[\epsilon^1_{it} | D_i, \mu_i] = 0$. The usual parallel trends assumption is implied by the outcome model, since $E[Y_{it}(0) - Y_{it}(1) | D_i = d, \mu_i] = \alpha_t - \alpha_{t-1}$, which does not depend on treatment status $d$. Thus $\tau$ is consistently estimated by $\hat{\tau}$ in the panel OLS regression

$$Y_{it}^{obs} = \mu_i + \alpha_t + \tau 1(t \geq t_0, D_i = 1) + \epsilon_{it}$$

### 4.2 Propensity-score-weighted difference-in-differences

Identification using the selection of OZs from eligible Census tracts faces the challenge that, selected tracts and tracts that are eligible but not selected differ in observable characteristics. Unbal-
anced characteristics suggests that parallel trends may not hold. We tackle this challenge with a propensity-score weighting approach (Abadie, 2005). Recent work by Callaway and Sant’Anna (2018) extends Abadie (2005) to settings with multiple periods, multiple treatment groups, and multiple treatment timings. Sant’Anna and Zhao (2018) extend Abadie (2005)’s two-period, two-group model and introduces a doubly-robust version of the semiparametric estimator in Abadie (2005). The ATT estimator is consistent if either the propensity score model or the outcome regression model is correctly specified.

4.3 Comparison of geographical neighbors

Our third empirical strategy uses geographic proximity to construct a paired sample of tracts. One may be concerned that certain unobserved, possibly time-varying, confounders drive selection of OZs—endangering causal interpretation of comparisons between OZs and their eligible-but-not-selected counterparts. If these unobserved factors affect geographically close regions similarly, then comparing OZs with their non-OZ geographical neighbors yields a more plausible natural experiment. On the other hand, geographical comparisons fall prey to potential spillover effects: If OZ selection has positive spillover effects, then comparisons with geographical neighbors understate treatment effects, and vice versa.

For each selected tract $i$, we construct its non-selected neighbor $\tilde{i}$ to be the tract that is (i) not selected as an OZ, (ii) closest to $i$ by distance between centroids, (iii) in the same state as $i$, and (iv) has no missing housing price data in 2018. Within each pair $t = (i, \tilde{i})$, we specify $Y_{it}(0) = \gamma_i + \alpha_{it} + \epsilon_{it}$, $Y_{\tilde{i}t}(0) = \tilde{\gamma}_i + \alpha_{\tilde{i}t} + \tilde{\epsilon}_{it}$ such that $Y_{it}(0) - Y_{\tilde{i}t}(0) = (\gamma_i - \tilde{\gamma}_i) + (\epsilon_{it} - \tilde{\epsilon}_{it})$, leading to the estimation procedure

$$Y_{it}^{\text{obs}} - Y_{\tilde{i}t}^{\text{obs}} = \tau 1(t \geq t_0, D_i = 1) + \mu_i + \eta_{it},$$

which is consistent assuming $\mathbb{E}[\eta_{it} | D_i, \mu_i] = 0$. The identification assumption in the third strategy is the pair, $i, \tilde{i}$ has the same trend $\alpha_{it}$, which is differenced away as we construct the estimator.

---

10 We do find substantial imbalance in covariate values in Tables 4 and 5 in the appendix. Technically, the identification assumption, (conditional) parallel trends, does not require covariate balance, since trends are only required to be parallel and may differ in levels.

11 In our design, we only have multiple pre-periods. There is a recent literature on the failure of the two-way fixed effect estimator (1) in situations with variable treatment timing and heterogeneous treatment effects, as the estimator becomes a weighted average of individual treatment effects with non-convex weights in large samples (Abraham and Sun, 2018; Athey and Imbens, 2018; Callaway and Sant’Anna, 2018; Goodman-Bacon, 2018; Imai and Kim, 2016; Borusyak and Jaravel, 2017; De Chaisemartin and D’Haultfoeuille, 2017). The issue is not as pertinent in our setting as we do not have variable treatment timing.

12 For instance, Hanson and Rohlin (2013) finds evidence that the Empowerment Zone program has significant spillover effects. Investigation of spillover effects is beyond the scope of this paper, but recent methodological work by Butts (2021) may further shed light.

13 The distance between centroids is calculated using the Haversine formula, assuming the Earth is a sphere with radius 6,371 kilometers. The average centroid distance between pairs is 2.662 (5.695) kilometers.
4.4 Heterogeneity analyses

We also use employment data from Longitudinal Employer-Household Dynamics Origin-Destination Employment Statistics (LODES) to classify tracts as either residential or commercial.\textsuperscript{14} We define residential tracts as those with the employment-to-population ratio (population employed in the tract to population residing in the tract) being below median. We then perform the analysis of Section 4.1, interacting the design with the residential indicator, in order to probe the potential heterogeneous effects of OZ designation. In a similar vein, we also conduct this heterogeneity exercise by classifying tracts as either having high or low price elasticity of housing supply, per the estimates of Baum-Snow and Han (2019).

4.5 Aggregating to ZIP codes

The FHFA tract-level data only covers less than half of all treated OZs. Moreover, the sample suffers from further attrition due to panel balance and missing Census covariates. To address the data availability concern, we include an alternative design by aggregating tracts to the ZIP code level. Mimicking our tract-level analysis, we drop ZIP codes that do not intersect with any Census tracts that are eligible to be selected as OZs.\textsuperscript{15} Each ZIP code $z$ is partitioned into tracts $i \in I_z$, with $\pi^z_i$ proportion of total addresses within tract $i$. We choose this aggregation method because crosswalks are readily provided by the Department of Housing and Urban Development and addresses are the most relevant measure for residential housing prices. For each variable $V$, we construct $V_z = \sum_{i \in I_z} \pi^z_i V_i$. The ZIP-code level treatment exposure $D_z$ now has a continuous distribution on $[0, 1]$. The analysis then proceeds as in the tract case. For weighting based estimator in Sant’Anna and Zhao (2018), we discretize $D_z$ by taking $\tilde{D}_z = 1(D_z \geq q)$ where $q$ is chosen such that the sample mean of $\tilde{D}_z$ equals the proportion of treated tracts among all tracts.

4.6 Effect on residential permitting

We study the effect of OZs on residential permitting in a similar manner. Our data for residential permitting, the Building Permits Survey (BPS), reports at the Census place level; therefore, we aggregate tracts up to Census places in a similar manner as in Section 4.5.

We define treatment as the binary variable which is one if a Census place intersects with a low-income community OZ, and zero otherwise; we restrict the sample to places that intersect with any eligible tract.\textsuperscript{16} About 50% of the sample are classified as treated, covering 5,162 OZs; the median treated place has 22% of its housing units falling into an OZ. In parts of our analysis, we also use a linear specification\textsuperscript{17} with the treatment intensity, defined as the proportion of housing units in a place falling into a low-income community OZ, as a robustness check.

\textsuperscript{14}Precisely speaking, we use the total non-federal employment population who works at the census tract in 2017, to match our population data from the 2017 ACS (WAC All Jobs Excluding Federal Jobs).

\textsuperscript{15}Our empirical results are not sensitive to this choice.

\textsuperscript{16}We have permit data for 7,241 Census places, where 3,613 intersect with eligible Census tracts.

\textsuperscript{17}The specification is entirely analogous to our TWFE analysis of ZIP-level aggregated data.
The BPS reports permitting data in terms of units, buildings, or dollar value, and, for each measure, the BPS reports by housing type (single family vs. multifamily, etc.). We use total\textsuperscript{18} units and value as our main variables of interest, and sum over different housing types.

We then perform a difference-in-difference analysis comparing treated places to untreated places. The building permits data is at a monthly level, which we aggregate every three months to smooth out some of the noise. We define December 2017 as the treatment time.\textsuperscript{19} Since there are many choices of the outcome variable (e.g., levels vs. logs), we supplement our analysis with a changes-in-changes (Athey and Imbens, 2006) analysis in Appendix D.2 that looks at the distribution of outcomes.\textsuperscript{20}

5 Results

The top panel of Table 1 provides tract-level results, corresponding to empirical strategies in Sections 4.1 to 4.3. The bottom panel shows ZIP code level results corresponding to Section 4.5. As discussed above, the key independent variable in the tract level regressions is an indicator variable that takes on a value of one if the tract is designated an OZ. In the ZIP code level regression, the key independent variable is the share of the addresses within each ZIP code that lie within an OZ.

The first two regressions in Table 1 show results where the treated tracts are compared with tracts that were eligible for inclusion within OZs, but were ultimately not included in the Zones. These results include time and tract fixed effects, and the estimated coefficient is 0.26, meaning that prices rose by about one-fourth of a percentage point annually on average. This coefficient is small in magnitude, statistically insignificant and precisely enough estimated so that we can rule out an effect of more than 0.6 percentage points. Table 1 of CEA (2020) performs a similar analysis, and obtains an estimated coefficient of 0.53 (0.19) for the 2018–2019 data. The size of the discrepancy is within the variation across specifications that we consider in Table 1. The source of the discrepancy is that (i) CEA (2020) uses the first difference of the FHFA price index, where we use successive ratios and (ii) we include data up to the year 2020.\textsuperscript{21}

In the second column, we show results allowing for interactions between tract-level characteristics and year, so that we estimate coefficients on tract income and other characteristics for each year. The tract level covariates do not change over time. With these added controls, the coefficient falls to 0.16. Again, the coefficient is small and insignificant. In this case, we can rule out effects of greater than 0.5 percentage points with 95% confidence.

Figure 1(a) shows the tract level housing price indices visually. The top two lines show annual growth rates from 2014 to 2020 for the treatment and control samples that are evaluated in the first

\textsuperscript{18}i.e., aggregating over all housing types; in Appendix D.1, we decompose outcome variables into unit types.

\textsuperscript{19}The choice corresponds to the passage of the TCJA and hence a reasonable estimate of the earliest time point where anticipation of treatment could occur; doing so also makes the permitting results comparable to the price results. Moreover, changing the treatment time would only change the baseline in the event study plot in Figure 2.

\textsuperscript{20}Of course, the assumptions required for differences-in-differences for different outcome variables and for changes-in-changes are different (Roth and Sant’Anna, 2020).

\textsuperscript{21}The first difference of the index is not invariant to the choice of base year. CEA (2020) uses 2013 as the base year.
(a) Raw trend of housing prices by treatment status

Figure 1: Raw trend and event study plot for top panel of Table 1
### Table 1: Estimation of ATT using FHFA Tract and ZIP-level data

|                      | TWFE (1) | TWFE (2) | Weighting CS (3) | Weighting DR (4) | Paired (5) | Paired (Linear Trend) (6) |
|----------------------|----------|----------|------------------|------------------|------------|--------------------------|
| **Tract-level data** |          |          |                  |                  |            |                          |
| \( \hat{\tau} \)   | 0.260 \([-0.034, 0.553]\) | 0.163 \([-0.130, 0.456]\) | 0.212 \([-0.164, 0.587]\) | 0.018 \([-0.374, 0.410]\) | 0.359 \([0.194, 0.523]\) | 0.285 \([-0.226, 0.796]\) |
| p-value              | (0.150)  | (0.144)  | (0.192)          | (0.200)          | (0.084)    | (0.261)                  |
| Pre-trend test p-value | 0.236    | 0.285    | 0.495            | —                | 0.010      | 0.274                    |
| \((N_1, N_0)\)      | (2917, 10962) | (2917, 10962) | (2917, 10962)   | (3095, 11500)   | (2867, 2867) | (2867, 2867)             |
| Covariates           | No       | Yes      | Yes              | Yes              | Yes        | Yes                      |
| Sample               | Balanced (2014–2020) | Balanced (2014–2020) | Balanced (2014–2020) | Balanced (2017–2020) | Balanced (2014–2020) | Balanced (2014–2020) |
| **ZIP-level data**   |          |          |                  |                  |            |                          |
| \( \hat{\tau} \)   | 1.181 \([0.799, 1.564]\) | 0.342 \([-0.041, 0.725]\) | —                | 0.181 \([-0.373, 0.735]\) |          |                          |
| p-value              | (0.195)  | (0.205)  |                  | (0.283)          |            |                          |
| Pre-trend test p-value | 0.021    | 0.653    |                  | —                | 0.521      |                          |
| \((N_1, N_0)\)      | (5957, 5758) | (5957, 5758) |                  | (1174, 9513)    |            |                          |
| Covariates           | No       | Yes      |                  |                  | Yes        |                          |
| Sample               | Balanced (2014–2020) | Balanced (2014–2020) |                  |                  | Balanced (2017–2020) |                          |

1. Standard errors are in parenthesis and 95% confidence intervals are in square brackets. Standard errors are clustered at the state level for the tract-level analysis (top panel) and clustered at the ZIP level for the ZIP-level analysis (bottom panel). Clustering the top panel at the tract level does not qualitatively change results.

2. Covariates include log median household income, total housing units, percent white, percent with post-secondary education, percent rental units, percent covered by health insurance among native-born individuals, percent below poverty line, percent receiving supplemental income, and percent employed. For Column (2), only including log median household income and percent white as covariates gives \(-0.025 (0.224)\) for the top panel and \(-0.060 (0.202)\) for the bottom panel.

3. Pretest for Column (2) interacts covariates with time dummies.

4. Years 2018 through 2020 are mean-aggregated in Column (4) since the doubly-robust estimation only handles two periods.

5. Discrete treatment in Column (4) is defined as the highest 88.3% of treated tract coverage, so as to keep the percentage of treated ZIPs the same as treated tracts.
two columns of Table 1. Both of these lines are quite distinct from the third line, which contains all of the tracts in the U.S. that were never eligible for OZ status. The top two lines lie essentially on top of one another. Pre-trends appear to be quite similar for the two top groups and quite different from the third group, which supports the finding of the pre-trends test reported in Table 1. There is also no visual change after the law is enacted in 2018. Both before and after the law is enacted, price growth in the two groups appears to be almost exactly the same.

The bottom panel of Table 1 shows results for the ZIP code analysis. In this case, the estimated coefficient represents the impact of moving from having no OZ tracts within the ZIP code to having 100 percent OZ tracts within the ZIP code.\textsuperscript{22} The coefficient in the first column is relatively large in the entire table, and it suggests that as the share of households that live in OZ tracts increase from zero to one, prices increased by 1.2 percentage points. As the standard errors suggest that the true coefficient could be as high as 1.5, this coefficient might be economically meaningful.

Yet there are two reasons to be cautious. First, the pre-trend test $p$-value suggests that the parallel trends assumption may be violated. ZIP codes with a higher share of addresses residing in OZs seem to diverge from ZIP codes with a lower share of such households prior to 2018. Moreover, when we allow for time varying effects of other tract level characteristics in the second column, the coefficient becomes 0.34. That second coefficient is estimated with enough precision to rule out a coefficient greater than 0.7 at conventional confidence levels. We interpret these results to suggest that the ZIP code level analysis also rules out large positive price impacts of OZ status in 2018.

In the third column, we show the tract-level analyses using the Callaway and Sant’Anna (2018) propensity score weighting method. The coefficient estimate is 0.21, and the upper bound on the confidence interval is 0.59. The fourth regression shows the doubly-robust coefficient estimate that follows Sant’Anna and Zhao (2018). The point estimate is 0.02 and the upper bound estimate is 0.4. The Callaway and Sant’Anna (2018) procedure tests for pre-trends, and we do not reject the null hypothesis that there is no pre-trend; the doubly-robust procedure (Sant’Anna and Zhao, 2018) follows a two-period model, and does not provide a pre-trend test. In both cases, the results imply that OZ status increased prices by less than one percentage point in 2018 related to previous years. ZIP-level analysis, based on discretization of the exposure variable, suggests effects of similar sizes.

In regressions (5) and (6), we match OZ tracts with the nearest tract that is not in an OZ. In some cases, the OZ tracts are matched with the same non-OZ tract. In regression (5), we find a coefficient of 0.36, which is statistically significant at conventional levels, but the result is not robust to inclusion of a linear time trend in regression (6).\textsuperscript{23} The coefficient falls to 0.28 and the 95%-confidence interval rules out a coefficient greater than 0.8.

Figure 1(b) shows year-by-year results that correspond to regressions (1), (2), (3), and (5). The first two sets of coefficients show no pre-trend, but only a small and statistically insignificant

\textsuperscript{22}The average ZIP code has 13.4\% of its addresses in a selected Opportunity Zone; the median ZIP code has 0.0\%; and the 75th percentile has 19.8\%.

\textsuperscript{23}The inclusion of a linear time trend is motivated by the observation in Figure 1 that the paired design seems to have nontrivial pre-trend.
increase in price in 2018. The third set of coefficients shows a statistically significant price increase in 2018, but similar-sized fluctuations are present in the pre-treatment period as well (e.g. 2014–2015). Our interpretation of these results is that OZ tracts did experience a modest increase in price in 2018–2019 relative to the nearest geographic neighbors, but that this could easily be a reflection of a pre-existing trend or a statistical fluke.

Taken together, these results suggest that if OZ status did generate a positive impact, that impact was quite small. There seems to be little possibility that home buyers anticipated that inclusion in an OZ would have a dramatic impact on the character of the neighborhood. This fact does not imply that the OZ program was a mistake, but rather that it is anticipated to have little effect on the neighborhood.

5.1 Heterogeneous Impacts of OZ Designation

OZ status confers subsidies to physical investment in a neighborhood. Such subsidies might have a different impact on housing prices if they largely work by subsidizing commercial space or if they largely work by subsidizing residential space. If a capital subsidy increases the presence of job-generating commercial properties, then standard urban theory predicts that the subsidy will increase residential prices. If the subsidy increases investment in residential properties, then the impact on housing prices could be negative.

Consider a subsidy that decreases the costs of adding residential density, but assume that existing homes must be bought to provide the land needed to build. If the new building generates supply but not externalities, then this should decrease the value of housing units. The value of lower density homes, which make up the bulk of the FHFA repeat sales properties, could still increase because they are providing land for future investment. If the new investment generates positive externalities, then there could be a positive price impact even if supply increases.

We test the hypothesis that OZ status may actually decrease prices by boosting residential supply by Census tracts in half based on the level of employment to residential population prior to 2017, where employment is in the LEHD-LODES data. Our core assumption is that OZ status will act primarily as a subsidy to commercial properties in the areas that initially have the higher levels of employment to population and that Zone status will act primarily as a subsidy to residential construction in the areas where employment to population begins at a lower level.

We test for this heterogeneity in the top panel of Table 2. As in the first two regressions in Table 1, the first column includes no covariates. The second and third columns include fixed covariates that are allowed to have a different coefficient in each year. In the second column, we control only for log of median income and total housing units. In the third column, we include a...
Table 2: Heterogeneous treatment effect by residential population and by housing elasticity

|                          | No Covariates   | Few Covariates | All Covariates  |
|--------------------------|-----------------|----------------|-----------------|
|                          | (1)             | (2)            | (3)             |
| Treatment × Post         | 0.286 [0.046, 0.526] | 0.035 [-0.204, 0.275] | 0.115 [-0.115, 0.345] |
| Treatment × Post × Residential | -0.580 [-1.024, -0.136] | -0.658 [-1.101, -0.215] | -0.229 [-0.643, 0.185] |
| Pretest p-value          | 0.800           | 0.269          | 0.903           |
| Treatment × Post         | 0.388 [0.139, 0.636] | 0.131 [-0.123, 0.386] | 0.297 [0.048, 0.547] |
| Treatment × Post × High Supply Elasticity | -0.996 [-1.475, -0.518] | -1.005 [-1.481, -0.528] | -0.729 [-1.192, -0.266] |
| Pretest p-value          | 0.345           | 0.286          | 0.663           |

1. The table reports the regression

\[ Y_{it}^{\text{obs}} = \mu_i + \alpha_{it} + \tau_0 \mathbb{1}(t \geq t_0, D_i = 1) + \tau_1 \mathbb{1}(t \geq t_0, D_i = 1, R_i = 1) + \gamma \mathbb{1}(t \geq t_0, R_i = 1) \]

and Treatment × Post reports \( \tau_0 \), while Treatment × Post × Residential reports \( \tau_1 \). Here \( \alpha_{it} = \alpha_i \) in the no-covariate specification and \( \alpha_{it} = \alpha_i X_i \) in the covariate specification. \( R_i \) is an indicator for whether the employment to residential population ratio is lower than median in the top panel, and is an indicator for whether the price elasticity of housing supply is above median in the bottom panel.

2. The employment to residential population ratio is reported as the ratio of the non-federal employment workforce (LEHD-LODES WAC files, 2017, column C000) in the tract divided by the population of the tract (ACS 2017 5-year estimates). Tracts are classified as residential if the ratio is lower than the median.

3. The price elasticity of housing supply is estimated and provided by Baum-Snow and Han (2019), using housing units.

4. Standard errors are in parentheses and 95% confidence intervals are in square brackets. Standard errors are clustered at the tract level.

5. “All covariates” consists of log median household income, total housing units, percent white, percent with post-secondary education, percent rental units, percent covered by health insurance among native-born individuals, percent below poverty line, percent receiving supplemental income, and percent employed. “Few covariates” consists of only log median household income and total housing units.

much wider set of covariates, and in all cases the estimated coefficients on the covariates are allowed to vary over time. The top row shows the overall coefficient on OZ status interacted with time, which should be interpreted as the treatment coefficient for non-residential areas. The second row interacts this variable with an indicator variable that takes on a value of one if the tract is above our sample median in the number of employees per capita in the residential population.

The first column shows a positive and significant coefficient overall and a negative and significant interaction term. These coefficients imply that prices grew by 0.29 percentage points in more commercial OZs, and by −0.3 percentage points in more residential OZs. The patterns are as expected, but even these coefficients are relatively modest in magnitude. In the second column, the positive effect for commercial tracts disappears when we allow for time varying effects of median income and the number of housing units. The negative interaction remains, suggesting the OZ status had a more negative impact on prices in more residential areas, but again the magnitude is quite modest. In the third column, we include a much wider range of controls. The overall

25The choice of covariates in the second column is more or less arbitrary—it is only meant to show the robustness of the result, consistent with a similar robustness check in Table 1.
effect is small in magnitude and statistically insignificant. The interaction is negative, but also not statistically significant at standard levels. The signs are as theory predicts, but the magnitudes are too small to meaningfully distinguish these effects from zero. Moreover, the standards errors are small enough to rule out truly large effects in either direction.

Similarly, the differential impact of the OZ designation should be more pronounced if we segment the sample by housing supply elasticity. Intuitively, areas with high supply elasticity respond to the changing incentives by building more housing, thereby we should expect housing prices to decrease, relative to areas with low supply elasticity. This is indeed what we observe in the bottom panel of Table 2: Across all three configurations of covariates, the price treatment effect for high elasticity tracts (per the estimates by Baum-Snow and Han (2019)) tends to be smaller than that for low elasticity tracts. The estimates continue to rule out large positive price effects, even for census tracts with low elasticity of supply.

Once again, there seems to be little evidence to support the view that OZ status generated the expectation, at least among home buyers, that these areas would transition from poverty to prosperity.

5.2 Effect on residential permitting

Another dimension of OZs’ effect on the housing market is through its effect on quantities, which we measure via housing permits. Large increases in permitting—and hence increase in housing supply—may explain the lack of price impact that we observe. New housing may indeed be evidence that the neighborhood is changing, although without price effects it is hard to new if the neighborhood is changing for the better. Presumably, part of the point of a capital subsidy is to induce new capital formation and housing permits provide our best indication of whether the OZ subsidy is generating new capital.

Working with housing permits brings a few econometric challenges. Permits can either be measured in terms of units of housing or value of housing. Permitting is extremely sparse—more than half of place-by-month cells have zero permitting—and heterogeneous, as certain populous places have much more permitting than others. These challenges result in an ambiguity of the right response variable to use, and parallel trends assumptions on different response variables may not be internally consistent with each other (Roth and Sant’Anna, 2020). These concerns notwithstanding, we present event-study plots for a range of outcomes in Figure 2. Each subplot of Figure 2 is an event-study plot for a particular response variable showing the estimated coefficients for a given three-month period. For each response variable, we consider two specification (with and without adjusting for housing stock in 2010) and two estimators (Callaway and Sant’Anna (2018) and two-way fixed-effect). Qualitatively speaking, the four settings have similar estimated coefficients.

The left column of Figure 2 consists of event study plots for outcomes that are not normalized by city size. The first two rows of the left column are effects for monthly permitting units and value in levels. The results are consistent across the two measures of permitting volume, and point
Notes. Thick error bars are conventional pointwise (1.96 \times SE) Wald intervals, whereas the thin error bars are simultaneous \textit{(max-t)} confidence bands (Montiel Olea and Plagborg-Møller, 2019).

Figure 2: Event study plot as well as ATT estimates for effect on various measures of residential permitting.
towards a small positive effect, relative to the last untreated period, of 5 units or $900,000 in value per month, though it may be as low as 1 unit or $200,000 in value.\textsuperscript{26} Though, notably, these results are not very robust to adjusting for size of housing stock or to using a continuous exposure variable for treatment definition. In particular, many point estimates are negative when treatment is defined as the exposure to OZs.

This does suggest that OZs may have increased housing supply in treated areas, but unusual statistical features like sparse and long-tailed data make these results somewhat unreliable. As robustness checks we consider a range of measures that are better behaved statistically. First, we consider transforming value via the arcsinh function, which is an ad hoc approximation of the logarithm that is defined at zero. Doing so results in a confidence interval of $[-0.4, 0.3]$, which means that we cannot rule out zero, and our point estimate is negative.

On the right side of Figure 2, we consider units and value normalized by a measure of city size (number of housing units in 2010), motivated by concerns that the effect in pure quantity terms may be driven by large Census places. In this case, we cannot reject zero impact. A separate analysis for quantile treatment effects with changes-in-changes (Athey and Imbens, 2006; Callaway, 2016) in Figure 9 in Appendix D.2 similarly finds positive but small quantile effects.

Finally, in the bottom panel of the left column of Figure 2, we consider the response variable of non-zero permitting in a given three-month period, and we find a mildly negative point estimate of the ATT relative to the last untreated period, but it is clear that this effect is small relative to the uncertainty from the event-study plot.

As an aside, viewing the price and permitting results together, it also seems that the permitting effects—if they do exist—may be more persistent than the price effects. This is consistent with the fact that prices should adjust quickly, reflecting expectations, and so effects on price growth are naturally transitory. Quantities, on the other hand, adjust more slowly to changes in neighborhood policy.

We conclude that there is weak evidence that OZ status increased residential permitting. The basic effects suggest that OZs did encourage permitting, but the results were not robust enough for us to have much confidence. Moreover, since the results disappear when we looked at permits normalized for the 2010 stock, the effects are probably driven by a few cases of quite large bursts of permitting in relatively small places.

6 Cost-benefit analysis

We have too little information to fully assess the OZ program, but we can compare the program’s cost in foregone taxes with its impact on property values. Land value provides the most canonical welfare measure in an urban setting. Arnott and Stiglitz (1979) show that land value captures social welfare in a small open city when people are identical or when there are no infra-marginal residents.\textsuperscript{26} These estimates assume perfect parallel trends, which may be substantially attenuated if we consider effects relative to plausible pretrends à la Roth and Rambachan (2019).
These assumptions are unlikely to be met in reality, and our parameter estimates do not allow us to fully capture the impact of the OZ program on land values. OZ designation creates incentives for business-related investment and the employees, owners and customers of these businesses may well live outside of the OZ. Yet our estimates look only at the impact of the OZ program on property within the zone itself, and we look only at the impact of the program on residential housing, not on unoccupied or industrial land.

Nonetheless, we will now do two simple calculations estimating the total increase in housing wealth in OZ tract based on our estimates, and the value created by new housing construction. The increase in housing wealth will reflect both local amenities created by the program and the tax benefits reaped by home buyers who are participating in the OZ program. We will estimate benefits on a per unit basis and then scale those benefits up by the number of units. As we have discussed, 8,764 Census tracts were allocated to OZs. These tracts contained a total of 12.78 million housing units.

We estimated increases in housing price as being proportional the to the value of those units. The average value of owner-occupied housing in OZ tracts is $155,000, but only 44 percent of OZ units are owner-occupied. We do not have direct measures of the value of rental properties and those properties are likely to be less valuable than owner-occupied units. To address this issue, we ran a housing price hedonic using owner-occupied units from American Community Survey during the pre-period. We then used the estimated coefficients to price the rental units in OZs. Our predicted value for rental units is 60 percent of the predicted value relative to owner-occupied units. Consequently, we will assume that the average pre-treatment value of rental units equals $90,000 and that the treatment effect of OZ status as a percentage of value was the same for owner and rented properties. Taken together, these estimates suggest that the total housing value in OZs during the pre-period was $1.5 trillion.

Our different point estimates then suggest total increases in total residential housing value that are summarized in Figure 3. The preferred point estimates yield value increases that range from $6 to $15 billion, with $44 billion being a clear outlier. The upper bound point estimates provide value creation between $17–27 billion, with $60 billion being a clear outlier.

We can augment these values by considering the value of new housing created according to our permit data. We use the treatment effects per unit of housing stock estimates in Figure 2 and compute that the total value of new housing is $0.5–1 billion, with upper bounds that range from $2.5–4 billion. This value created is not a welfare calculation, but it does reflect a different way of calculating the effect of the housing.

To convert this new construction into a welfare calculation, we would need to go from this total output number to a measure of consumer welfare and producer profit and would need to convert

---

27Specifically, at the tract level, we regress median owner-occupied housing price on average household size, number of rooms, median year built, and median income of owner-occupied housing units. We then use the estimated regression coefficients to predict median renter-occupied housing price. Such a regression yields a reasonable approximation of value of rental units if the conditional relationships between median price and housing characteristics are similar across renter- and owner-occupied units.
Figure 3: Estimates of value impact on existing units

this annual flow into a sum of the life of the project. While industry rule of thumb calculations typically suggest a profit margin of 20 percent, that margin is better seen as reflecting accounting profits rather than true economic profits. If we took 1/5 of total production as reflecting total social welfare created by this new production, then this would imply annual profit flows ranging from $200 million to $800 million dollars.

This figure is much larger than the classic welfare triangle suggested by this calculation. The change in the number of units would range from 4,500 to 20,000 from our lower preferred estimate to our highest upper bound estimate. The largest change in price per unit point estimate is $4,500 over 2.5 years. By multiplying the change in price by the change in units by 1/2, we find a total “Harberger” triangle of $10 to $45 million. This is fairly negligible relative to the other figures, and so we will ignore it going forward.

So far, we have calculated the net price impact on existing quantities, \( q\Delta p \approx 10 \) billion. This corresponds to Marshallian surplus with perfectly inelastic housing supply. In general, the Marshallian surplus is, to a first-order, \( W = (1 + |\epsilon_S/\epsilon_D|)q\Delta p \), where \( \epsilon_S, \epsilon_D \) are supply and demand elasticities, respectively. Point estimates for changes in quantity and price suggest the supply elasticity is anywhere between 0.03 to 0.2, which is sensitive to estimation of the price impact. Baum-Snow and Han (2019) suggest the supply elasticity is about 0.4. On the other hand, modelling the OZ program as a demand shock implies that the demand elasticity is not identified from our data, but estimates by Hanushek and Quigley (1980) and Albouy et al. (2016) suggest \( \epsilon_D \) values ranging from −0.4 to −0.6. Thus, \( W \leq 2q\Delta p \) seems like a reasonably generous upper bound, which puts the consensus point estimate at about $20 billion, with consensus upper bounds going as high as $60 billion.

We caution that the benefit analysis above err on the side of optimism, and there are reasons to think they overstate the positive impact of the OZ program. First, the statistical uncertainty in our price estimates cannot reject a welfare impact of zero across many specifications. Second, as the identification strategy compares OZs to similar, untreated tracts, we cannot rule out the possibility...
that the price impact of OZs comes from displacement of investment that would have gone to the Census tracts that we use as controls. If investors substitute from these control tracts to OZs, welfare impacts from our estimates overstate the national net value creation of the program. Third, this analysis ignores heterogeneity, and there is evidence from Table 2 that the price impact for residential tracts—where existing residential units concentrate—may be smaller than the average price impact or even be negative. This means that the housing-unit level price impact may be lower than the tract-level price impact in Table 1 that we use for the benefit analysis.

6.1 The tax expenditure costs of the program

A report from the CEA (2020) “estimates that the Federal Government forgoes $0.15 for every $1 in capital gains invested in a Qualified Opportunity Fund before 2020, or about $11.2 billion for the $75 billion raised through the end of 2019.” Arefeva et al. (2021) suggest that this number should be increased five-fold because Qualified Opportunity Funds are responsible for only twenty percent of the investment in Opportunity Zones. Yet the Council of Economic Advisors estimate of $75 billion may also radically overstate the true level of Opportunity Zone investment, because it is based on scaling upward actually observed Qualified Opportunity Fund levels of $7.6 billion and $2.9 billion observed by Novogradac, an accounting firm, and the Securities and Exchange Commission respectively. Kennedy and Wheeler (2021) report only $18.9 billion in total OZ investments on tax records that were filed electronically by businesses in 2019. That report suggests that this represents 75 percent of all OZ investments, which would imply $25 billion in investment. If this $25 billion represents the total stock of investment than the total cost would be less than $4 billion. If that figure represents the annual flow of new investment in OZs, then the annual cost would come close to the $3.75 billion, which is reasonably close to a Joint Committee on Taxation figure of $3.5 billion per year cited in Kennedy and Wheeler (2021). Depending on the discount factor, $3.5 billion per year could be significantly higher than the $55 billion cited by Arefeva et al. (2021). If the costs of this program are more than $50 billion, then this seems significantly higher than most estimates of increase in housing values. The welfare associated with new construction is more difficult to assess, but it can be compared with the $3.5 billion flow discussion by Kennedy and Wheeler (2021).

7 Conclusion

OZs are America’s most important new national spatial policy since the Empowerment Zone program began during the Clinton era. They are intended to spur investment in high poverty areas. The hope of this program is that it would generate neighborhood revival, yet we find little evidence to support this view at this early date. Housing prices may have gone up in OZ areas after their enactment in 2018, but if they did the overall price impact seems to have been less than one percentage point. We find suggestive evidence that OZ status increased prices in more commercial areas and reduced prices in more residential areas, presumably because Zone status generated a
subsidy for building new homes. We also found some evidence suggesting that OZs did encourage new building. Of course, even if OZs did induce new building in treated areas, it is not obvious if that outcome is socially desirable.

The designation of the OZ tracts was only made public in the summer of 2018. Consequently, our results reflect 27 months of subsequent data, during which the world experiences a once-in-a-century global pandemic. These features of our results should make us cautious about any interpretation. We are at an early point and home price effects can, at best, capture the expectations about neighborhood change held by recent home buyers. These buyers could be wrong: In the future OZ status could end up correlated with neighborhood upgrading.

Our preliminary estimates suggest that a generous welfare estimate of OZ’s impact over 2.5 years is $20 billion, versus about $3.75 billion of cost per annum estimated by Kennedy and Wheeler (2021)—aggregating to about $10 billion over 2.5 years. Under such an optimistic scenario, it does seem that the OZ program is beneficial. However, the uncertainty in these welfare means that we cannot reject a welfare impact of zero. There is also considerable uncertainty in the cost estimates: Our welfare estimates are generally comparable to the low-end of cost estimates, but are much less than the high end. Moreover, much of the estimated welfare increase comes from increasing value of the housing stock, but we may not expect the price effect to persist if it mainly reflects buyers’ expectations, whereas the costs of the program continue to accrue.
References

Abadie, A. (2005). Semiparametric difference-in-differences estimators. *The Review of Economic Studies* 72(1), 1–19.

Abraham, S. and L. Sun (2018). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Available at SSRN 3158747*.

Albouy, D., G. Ehrlich, and Y. Liu (2016). Housing demand, cost-of-living inequality, and the affordability crisis. Technical report, National Bureau of Economic Research.

Angrist, J. D. and J.-S. Pischke (2008). *Mostly harmless econometrics: An empiricist’s companion*. Princeton university press.

Arefeva, A., M. A. Davis, A. C. Ghent, and M. Park (2021). Job growth from opportunity zones. *Available at SSRN 3645507*.

Arnott, R. J. and J. E. Stiglitz (1979). Aggregate land rents, expenditure on public goods, and optimal city size. *The quarterly journal of economics* 93(4), 471–500.

Asquith, B., E. Mast, and D. Reed (2019). Supply shock versus demand shock: The local effects of new housing in low-income areas. *Available at SSRN 3507532*.

Athey, S. and G. W. Imbens (2006). Identification and inference in nonlinear difference-in-differences models. *Econometrica* 74(2), 431–497.

Athey, S. and G. W. Imbens (2018). Design-based analysis in difference-in-differences settings with staggered adoption. Technical report, National Bureau of Economic Research.

Atkins, R., P. Hernandez-Lagos, C. Jara-Figueroa, and R. Seamans (2020). What is the impact of opportunity zones on employment outcomes? *Available at SSRN*.

ATTOM Data Solutions (2020). Home prices continue rising in two-thirds of opportunity zones in first quarter of 2020.

Baum-Snow, N. and L. Han (2019). The microgeography of housing supply. *Work in progress, University of Toronto*.

Bogin, A., W. Doerner, and W. Larson (2019). Local house price dynamics: New indices and stylized facts. *Real Estate Economics* 47(2), 365–398.

Borusyak, K. and X. Jaravel (2017). Revisiting event study designs. *Available at SSRN 2826228*.

Busso, M. and P. Kline (2008). Do local economic development programs work? evidence from the federal empowerment zone program.
Butts, K. (2021). Difference-in-differences estimation with spatial spillovers. *arXiv preprint arXiv:2105.03737*.

Callaway, B. (2016). Quantile treatment effects in r: The qte package. Technical report, working paper, Temple University, Philadelphia.

Callaway, B. and P. H. Sant’Anna (2018). Difference-in-differences with multiple time periods and an application on the minimum wage and employment. *arXiv preprint arXiv:1803.09015*.

Casey, A. (2019). Sale prices surge in neighborhoods with new tax break. [https://www.zillow.com/research/prices-surge-opportunity-zones-23393/](https://www.zillow.com/research/prices-surge-opportunity-zones-23393/). Accessed: 2019-11-14.

CEA (2020). The impact of opportunity zones: An initial assessment.

Chen, J., E. L. Glaeser, and D. Wessel (2019, December). The (non-) effect of opportunity zones on housing prices. Working Paper 26587, National Bureau of Economic Research.

De Chaisemartin, C. and X. D’Haultfœuille (2017). Fuzzy differences-in-differences. *The Review of Economic Studies 85*(2), 999–1028.

Economic Innovation Group (2015). Unlocking private capital to facilitate economic growth in distressed areas. [https://eig.org/wp-content/uploads/2015/04/Unlocking-Private-Capital-to-Facilitate-Growth.pdf](https://eig.org/wp-content/uploads/2015/04/Unlocking-Private-Capital-to-Facilitate-Growth.pdf). Accessed: 2022-03-18.

Freedman, M., S. Khanna, and D. Neumark (2021). Jue insight: The impacts of opportunity zones on zone residents. *Journal of Urban Economics*, 103407.

Goodman-Bacon, A. (2018). Difference-in-differences with variation in treatment timing. Technical report, National Bureau of Economic Research.

Hanson, A. and S. Rohlin (2013). Do spatially targeted redevelopment programs spillover? *Regional Science and Urban Economics 43*(1), 86–100.

Hanushek, E. A. and J. M. Quigley (1980). What is the price elasticity of housing demand? *The Review of Economics and Statistics*, 449–454.

Imai, K. and I. S. Kim (2016). When should we use linear fixed effects regression models for causal inference with longitudinal data? *Unpublished working paper. Retrieved September 19, 2018.*

Kennedy, P. and H. Wheeler (2021). Neighborhood-level investment from the us opportunity zone program: Early evidence. Technical report, Working Paper.

Li, X. (2019). Do new housing units in your backyard raise your rents. *NYU Wagner and NYU Furman Center, Job Market Paper 57*.
Lovegrove, J. (2017). Sens. scott, graham are big arm-twisters in tax overhaul as gop scrambles to lock down votes. https://www.postandcourier.com/politics/sens-scott-graham-are-big-arm-twisters-in-tax-overhaul/article_3e414eb2-d453-11e7-9ea8-93ddcd71dd65.html. Accessed: 2022-03-18.

Montiel Olea, J. L. and M. Plagborg-Møller (2019). Simultaneous confidence bands: Theory, implementation, and an application to svars. *Journal of Applied Econometrics* 34(1), 1–17.

Neumark, D. and J. Kolko (2010). Do enterprise zones create jobs? evidence from california’s enterprise zone program. *Journal of Urban Economics* 68(1), 1–19.

Pennington, K. (2021). Does building new housing cause displacement?: The supply and demand effects of construction in san francisco. *The Supply and Demand Effects of Construction in San Francisco* (June 15, 2021).

Roth, J. and A. Rambachan (2019). An honest approach to parallel trends.

Roth, J. and P. H. Sant’Anna (2020). When is parallel trends sensitive to functional form? *arXiv preprint arXiv:2010.04814*.

Sage, A., M. Langen, and A. Van de Minne (2019). Where is the opportunity in opportunity zones? early indicators of the opportunity zone program’s impact on commercial property prices. *Early Indicators of the Opportunity Zone Program’s Impact on Commercial Property Prices* (May 1, 2019).

Sant’Anna, P. H. and J. B. Zhao (2018). Doubly robust difference-in-differences estimators. *Available at SSRN 3293315*. 
A Covariates, summary statistics, and balance

A.1 Variable definitions

Variable definitions for covariates and simple calculations, along with their associated code used in the Census API, are shown in Table 8. Summary statistics and covariate balance are shown in Tables 4 and 5. We see that compared to the control group, the selected OZs are less populated, less employed, less likely to attain higher education, have more rental units, and are less wealthy. Similar trends persist when compared to their non-selected geographical neighbors in Table 5. While the covariate non-balance threatens identification by making the (conditional) parallel trends assumption less plausible, it does not reject parallel trends either. Since identification requires only trends to be parallel and allows for level differences, it also allows for level differences in observed or unobserved characteristics, so long as the trends are the same (conditionally).

A.2 Estimation with unbalanced panel

The counterpart to the first two columns of Table 1 with unbalanced panel is shown in Table 3, which does not qualitatively change conclusions.

A.3 Representativeness of the tract sample

We plot the distribution of a few covariates in the overall sample of selected OZs\textsuperscript{28} versus various subsamples with price data in Figure 5; we show the covariate means in Table 6. We see that, roughly speaking,

- Treated tracts with price data tend to be more populous (by about 25% on average), richer (by 8% / $2,000 in median earnings), whiter (by 12 percentage points), less renter-occupied (by 7 percentage points), and more employed (by 3 percentage points).

\textsuperscript{28}Among which 7,617 have ACS covariate data coverage.
Table 3: TWFE results with unbalanced panel of tract-level data

|                | TWFE      | TWFE      |
|----------------|-----------|-----------|
|                | (1)       | (2)       |
| $\hat{\tau}$  | 0.219 [-0.062, 0.499] | 0.099 [-0.181, 0.380] |
|                | (0.143)   | (0.147)   |
| p-value        | 0.133     | 0.502     |
| Pre-trend test p-value | 0.980  | 0.746     |
| $(N_1, N_0)$   | (3806, 13983) | (3806, 13983) |
| Covariates     | No        | Yes       |
| Sample         | Unbalanced (2014–2020) | Unbalanced (2014–2020) |

1. Standard errors are in parenthesis and 95% confidence intervals are in square brackets. Standard errors are clustered at the state level for the tract-level analysis (top panel). Clustering the top panel at the tract level does not qualitatively change results.

2. Covariates include log median household income, total housing units, percent white, percent with post-secondary education, percent rental units, percent covered by health insurance among native-born individuals, percent below poverty line, percent receiving supplemental income, and percent employed.

3. Pretest for Column (2) interacts covariates with time dummies.

- The distributions of the covariates in the subsample with price data still has substantial overlap with the overall distribution (Figure 5), as opposed to the distribution in the subsample being some truncation of the overall distribution.

- Tracts in the unbalanced panel versus the subsample of tracts in the balanced panel look quite similar.

- Tracts covered by the ZIP code level data in 2018 (we have covariate data for 6984 out of 6988) look quite similar to the overall sample of selected OZ tracts.

- Tracts covered by the Census place level data (we have covariate data for 5160 out of 5162) look likewise similar to the overall sample, perhaps somewhat less so than the ZIP-level data. It tends to skew less white and more rent-occupied than the overall population.

Similarly, we show the covariate distribution discrepancies in the control group (eligible, but not selected census tracts) in Figure 6. The qualitative patterns are similar to that of the treated group.

B Including all OZs

In the main text, we compare treated zones to control zones that are strictly low-income. We remove this restriction in this section. We regenerate Figure 1 with Figure 7, Table 1 with Table 9, and Table 2 with Table 11. The modification does not change our qualitative results.
Table 4: Balance of selected opportunity zones and eligible census tracts

|                               | Mean Not Selected (1) | Mean Selected (2) | Diff. (3) | Not Selected SE (4) | Selected SE (5) | t (6) |
|--------------------------------|-----------------------|-------------------|-----------|---------------------|-----------------|-------|
| Population                     | 4084.088              | 4018.494          | −65.594   | 12.424              | 22.582          | −2.545|
| Employed pop.                  | 1197.027              | 1085.639          | −111.388  | 4.288               | 7.356           | −13.082|
| Avg. commute (min)             | 37.864                | 37.190            | −0.675    | 0.135               | 0.223           | −2.590|
| Median household income        | 26076.123             | 24150.852         | −1925.272 | 47.183              | 81.187          | −20.503|
| Median earnings                | 41606.969             | 36041.271         | −5565.698 | 86.632              | 145.624         | −32.847|
| Total housing                  | 1478.728              | 1458.415          | −20.313   | 4.352               | 7.820           | −2.270|
| Median gross rent              | 897.552               | 822.828           | −74.724   | 1.953               | 3.053           | −20.616|
| % White                        | 0.624                 | 0.568             | −0.057    | 0.002               | 0.003           | −14.558|
| % Higher ed.                  | 0.144                 | 0.129             | −0.014    | 4.790 × 10⁻⁴        | 7.471 × 10⁻⁴    | −15.677|
| % Rent                         | 0.490                 | 0.557             | 0.067     | 0.001               | 0.003           | 22.981 |
| % Healthcare                   | 0.886                 | 0.878             | −0.007    | 3.976 × 10⁻⁴        | 7.260 × 10⁻⁴    | −9.043 |
| % Poverty                      | 0.207                 | 0.249             | 0.043     | 6.371 × 10⁻⁴        | 0.001           | 30.347 |
| % Supplemental income          | 0.101                 | 0.120             | 0.019     | 4.143 × 10⁻⁴        | 8.284 × 10⁻⁴    | 20.923 |
| % Employed                     | 0.290                 | 0.266             | −0.024    | 5.056 × 10⁻⁴        | 8.789 × 10⁻⁴    | −23.720|

"Not Selected" refers to eligible but not selected opportunity zones. Difference is selected minus not selected. Two-sample *t*-statistic reported.

Table 5: Covariate balance between geographical pairs (treated minus untreated)

|                               | N (1) | Mean (2) | Standard Err. (3) | t-statistic (4) |
|--------------------------------|-------|----------|-------------------|-----------------|
| Population                     | 7,814 | −563.655 | 28.437            | −19.821         |
| Employed pop.                  | 7,814 | −389.183 | 9.982             | −38.988         |
| Avg. commute (min)             | 3,278 | 2.104    | 0.229             | 9.202           |
| Median household income        | 7,796 | −6989.912| 120.019           | −58.240         |
| Median earnings                | 7,800 | −17255.649| 232.043          | −74.364         |
| Total housing                  | 7,814 | −240.748 | 10.283            | −23.413         |
| Median gross rent              | 7,739 | −131.982 | 3.101             | −42.562         |
| % White                        | 7,814 | −0.134   | 0.003             | −49.533         |
| % Higher ed.                  | 7,814 | −0.060   | 0.001             | −56.080         |
| % Rent                         | 7,808 | 0.168    | 0.003             | 66.720          |
| % Healthcare                   | 7,813 | −0.027   | 6.906 × 10⁻⁴      | −39.012         |
| % Poverty                      | 7,814 | 0.097    | 0.001             | 71.050          |
| % Supplemental income          | 7,814 | 0.048    | 8.911 × 10⁻⁴      | 53.717          |
| % Employed                     | 7,814 | −0.058   | 0.001             | −55.363         |
Figure 5: Distribution of covariates in different subsamples of the tract data (Middle 95% of each covariate shown)
Figure 6: Distribution of covariates in different subsamples of the untreated tract data (Middle 95% of each covariate shown)
Figure 7: Figure 1 with all eligible tracts as control group
Table 6: Covariate means by subsample in the tract data (Covariate data from ACS 2017)

| level_1 | Most (7617/8532) | Unbalanced (3806/8532) | Balanced (2917/8532) | ZIP (6984/8532) | Places (5160/8532) |
|---------|------------------|------------------------|----------------------|-----------------|---------------------|
|         | (1)              | (2)                    | (3)                  | (4)             | (5)                 | (6)                 |
| population | mean           | 4018.494               | 4743.423              | 4933.514        | 4055.862            | 4022.165            |
| population | std            | 1970.423               | 1889.225              | 1900.284        | 1940.016            | 2053.094            |
| population | count           | 7,617                  | 3,806                 | 2,917           | 6,984               | 5,160               |
| population | SE              | 22.582                 | 30.623                | 35.184          | 23.217              | 28.587              |
| median_earnings | mean           | 24150.852              | 25629.700             | 26037.611       | 24081.051           | 23873.037           |
| median_earnings | std            | 7076.359               | 6203.688              | 6253.787        | 6952.603            | 7337.118            |
| median_earnings | count           | 7,617                  | 3,806                 | 2,917           | 6,984               | 5,160               |
| median_earnings | SE              | 81.187                 | 100.558               | 115.791         | 83.290              | 102.270             |
| pct_white | mean           | 0.568                  | 0.676                 | 0.699           | 0.577               | 0.510               |
| pct_white | std            | 0.299                  | 0.246                 | 0.230           | 0.294               | 0.292               |
| pct_white | count           | 7,617                  | 3,806                 | 2,917           | 6,984               | 5,160               |
| pct_white | SE              | 0.003                  | 0.004                 | 0.004           | 0.004               | 0.004               |
| pct_rent | mean           | 0.557                  | 0.488                 | 0.479           | 0.555               | 0.615               |
| pct_rent | std            | 0.222                  | 0.181                 | 0.176           | 0.215               | 0.208               |
| pct_rent | count           | 7,617                  | 3,806                 | 2,917           | 6,984               | 5,160               |
| pct_rent | SE              | 0.003                  | 0.003                 | 0.003           | 0.003               | 0.003               |
| pct_employed | mean        | 0.266                  | 0.289                 | 0.292           | 0.267               | 0.267               |
| pct_employed | std            | 0.077                  | 0.064                 | 0.065           | 0.076               | 0.080               |
| pct_employed | count           | 7,617                  | 3,806                 | 2,917           | 6,984               | 5,160               |
| pct_employed | SE              | 8.789 \times 10^{-4}   | 0.001                 | 0.001           | 9.072 \times 10^{-4} | 0.001               |
| Data Source                          | URL (Accessed 2022-03-18)                                      |
|-------------------------------------|----------------------------------------------------------------|
| FHFA Housing Price Index            | https://www.fhfa.gov/DataTools/Downloads/Pages/House-Price-Index-Datasets.aspx |
| Urban Institute OZ Data             | https://www.urban.org/policy-centers/metropolitan-housing-and-communities-policy-center/projects/opportunity-zones |
| Longitudinal Employer-Household Dynamics | https://lehd.ces.census.gov/data/                          |
| TIGER Geographic Shapefiles        | https://datacatalog.urban.org/dataset/longitudinal-employer-household-dynamics-origin-destination-employment-statistics-lodes |
| Census Business Patterns            | https://www.census.gov/geographies/mapping-files/time-series/geo/tiger-line-file.html |
| Building Permits Survey            | https://www.census.gov/construction/bps/                 |
| Missouri Census Data Center, Geocorr 2000 | https://www.census.gov/construction/bps/       |
|                                     | https://mcdc.missouri.edu/applications/geocorr2000.html   |
Table 8: Variable definitions, ACS codes, descriptions, and transformations

| Variable          | Description                                      |
|-------------------|--------------------------------------------------|
| B01003_001E       | population                                       |
| B02001_002E       | white_population                                 |
| C24020_001E       | employed_population                              |
| B08131_001E       | minutes_commute                                 |
| B09010_002E       | supplemental_income                              |
| B15003_021E       | associate                                        |
| B15003_022E       | bachelor                                         |
| B15003_023E       | master                                           |
| B15003_024E       | professional_school                              |
| B15003_025E       | doctoral                                         |
| B16009_002E       | poverty                                          |
| B18140_001E       | median_earnings                                  |
| B19019_001E       | median_household_income                          |
| B25011_001E       | total_housing                                    |
| B25011_026E       | renter_occupied                                  |
| B25031_001E       | median_gross_rent                                |
| B27020_002E       | native_born                                      |
| B27020_003E       | native_born_hc_covered                           |
| pct_white         | white_population / population                    |
| minutes_commute   | minutes_commute / employed_population             |
| pct_higher_ed     | (associate + bachelor + professional_school + doctoral) / population |
| pct_rent          | renter_occupied / total_housing                  |
| pct_native_hc_covered | native_born_hc_covered / native_born             |
| pct_poverty       | poverty / population                             |
| log_median_earnings | log(median_earnings)                           |
| log_median_household_income | log(median_household_income)            |
| log_median_gross_rent | log(median_gross_rent)                      |
| pct_supplemental_income | supplemental_income / population                |
| pct_employed      | employed_population / population                 |
Table 9: Estimation of ATT using FHFA Tract and ZIP-level data

|                  | TWFE (1) | TWFE (2) | Weighting CS (3) | Weighting DR (4) | Paired (Linear Trend) |
|------------------|----------|----------|------------------|------------------|-----------------------|
| **Tract-level data** |          |          |                  |                  |                       |
| $\hat{\tau}$     | 0.155 [−0.242, 0.551] | 0.188 [−0.209, 0.584] | 0.216 [−0.160, 0.592] | −0.000 [−0.386, 0.385] | 0.359 [0.194, 0.523] |
| $p$-value         | (0.202)  | (0.154)  | (0.192)          | (0.197)          | (0.084)               |
| Pre-trend test $p$-value | 0.448  | 0.228  | 0.260          | 0.998          | $1.900 \times 10^{-5}$ |
| $(N_1, N_0)$      | (3055, 18468) | (3055, 18468) | (3055, 18468) | (3234, 19148) | (2867, 2867) |
| Covariates        | No       | Yes      | Yes             | Yes             | Yes                   |
| Sample            | Balanced (2014–2020) | Balanced (2014–2020) | Balanced (2014–2020) | Balanced (2017–2020) | Balanced (2014–2020) |
| **ZIP-level data** |          |          |                  |                  |                       |
| $\hat{\tau}$     | 1.000 [0.641, 1.360] | 0.364 [0.004, 0.723] | 0.038 [−0.480, 0.556] |                  |                       |
| $p$-value         | (0.184)  | (0.195)  |                  | (0.264)          |                       |
| Pre-trend test $p$-value | 5.107 $\times 10^{-8}$ | 0.063 |                  | 0.885 |                       |
| $(N_1, N_0)$      | (6105, 8223) | (6105, 8223) |                  | (1505, 11356) |                       |
| Covariates        | No       | Yes      |                  | Yes             |                       |
| Sample            | Balanced (2014–2020) | Balanced (2014–2020) |                  | Balanced (2014–2020) |                       |

1. Standard errors are in parenthesis and 95% confidence intervals are in square brackets. Standard errors are clustered at the state level for the tract-level analysis (top panel) and clustered at the ZIP level for the ZIP-level analysis (bottom panel). Clustering the top panel at the tract level does not qualitatively change results.

2. Covariates include log median household income, total housing units, percent white, percent with post-secondary education, percent rental units, percent covered by health insurance among native-born individuals, percent below poverty line, percent receiving supplemental income, and percent employed. For Column (2), only including log median household income and percent white as covariates gives $−0.147$ (0.262) for the top panel and $−0.023$ (0.190) for the bottom panel.

3. Pretest for Column (2) interacts covariates with time dummies.

4. Years 2018 through 2020 are mean-aggregated in Column (4) since the doubly-robust estimation only handles two periods.

5. Discrete treatment in Column (4) is defined as the highest 88.0% of treated tract coverage, so as to keep the percentage of treated ZIPs the same as treated tracts.
C  Heterogeneous price effects at the ZIP level

In a previous version of the analysis, we consider the price effect by residential population analysis aggregated at the ZIP code level using the Census Business Patterns data for employment, much as we do in Table 2. We reproduce the results below in Table 10 and Table 11.

Table 10: Heterogeneous treatment effect by residential population

|                     | No Covariates (1) | Few Covariates (2) | All Covariates (3) |
|---------------------|-------------------|--------------------|-------------------|
| Treatment × Post    | 1.879 [1.291, 2.367] | 0.033 [−0.471, 0.537] | 0.419 [−0.072, 0.909] |
|                     | (0.249)           | (0.257)            | (0.250)           |
| Treatment × Post × Residential | −1.293 [−2.071, −0.515] | −0.686 [−1.463, 0.091] | −0.342 [−1.104, 0.420] |
|                     | (0.397)           | (0.396)            | (0.389)           |
| Pretest p-value     | 6.379 × 10⁻⁴      | 0.439              | 0.795             |

1. The table reports the regression

\[ Y_{it}^{obs} = \mu_i + \alpha_{it} + \tau_0 \mathbb{1}(t \geq t_0, D_i = 1) + \tau_1 \mathbb{1}(t \geq t_0, D_i = 1, R_i = 1) + \gamma \mathbb{1}(t \geq t_0, R_i = 1) \]

and Treatment × Post reports \( \tau_0 \), while Treatment × Post × Residential reports \( \tau_1 \). Here \( \alpha_{it} = \alpha_t \) in the no-covariate specification and \( \alpha_{it} = \alpha_t'X_i \) in the covariate specification. \( R_i \) is an indicator for whether the employment to residential population ratio is lower than median.

2. Standard errors are in parenthesis and 95\% confidence intervals are in square brackets. Standard errors are clustered at the ZIP level.

3. “All covariates” consists of log median household income, total housing units, percent white, percent with post-secondary education, percent rental units, percent covered by health insurance among native-born individuals, percent below poverty line, percent receiving supplemental income, and percent employed. “Few covariates” consists of only log median household income and total housing units.

D  Permitting analysis

D.1  Decomposing units

We carry out the analysis in the top-left panel of Figure 2, except we decompose permits for total units into single-family units and multi-family units. The resulting event study plots are shown in Figure 8. We see that most of our results are driven by differences in single-family units.
### Table 11: Heterogeneous treatment effect by residential population

|                                | No Covariates | Few Covariates | All Covariates |
|--------------------------------|---------------|----------------|---------------|
|                                | (1)           | (2)            | (3)           |
| Treatment × Post               | 1.752 [1.314, 2.190] | 0.068 [−0.384, 0.520] | 0.502 [0.061, 0.943] |
|                                | (0.223)       | (0.231)        | (0.225)       |
| Treatment × Post × Residential | −1.464 [−2.214, −0.714] | −0.993 [−1.743, −0.244] | −0.551 [−1.289, 0.188] |
|                                | (0.383)       | (0.382)        | (0.377)       |
| Pretest p-value                | 9.489 × 10^{-5} | 0.060          | 0.532         |

1. The table reports the regression

$$Y_{it}^{\text{obs}} = \mu_i + \alpha_{it} + \tau_0 \mathbb{1}(t \geq t_0, D_i = 1) + \tau_1 \mathbb{1}(t \geq t_0, D_i = 1, R_i = 1) + \gamma \mathbb{1}(t \geq t_0, R_i = 1)$$

and Treatment × Post reports $\tau_0$, while Treatment × Post × Residential reports $\tau_1$. Here $\alpha_{it} = \alpha_t$ in the no-covariate specification and $\alpha_{it} = \alpha_t'X_i$ in the covariate specification. $R_i$ is an indicator for whether the employment to residential population ratio is lower than median.

2. Standard errors are in parenthesis and 95% confidence intervals are in square brackets. Standard errors are clustered at the ZIP level.

3. “All covariates” consists of log median household income, total housing units, percent white, percent with post-secondary education, percent rental units, percent covered by health insurance among native-born individuals, percent below poverty line, percent receiving supplemental income, and percent employed. “Few covariates” consists of only log median household income and total housing units.

### D.2 Changes in changes

In this section we detail how we computed changes-in-changes estimates for the quantile treatment effects in permitting units and value (Athey and Imbens, 2006). We use the implementation of Callaway (2016), which is suitable for a two-period setting. As a result, we engineer a two-period dataset by aggregating pre-treatment and post-treatment months for each Census place. The resulting estimates are shown in Figure 9.
Figure 9: Changes-in-changes estimates of quantile treatment effects for permitting results