Opportunity Zones and Housing Affordability

Roxana Gonzalez-Cortes*

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Abstract

The Opportunity Zone Program (OZP), signed into law in late 2017, granted attractive tax breaks for investments into distressed areas of the United States. Tax filings suggest that most OZP investment flowed into residential real estate, particularly into rental housing. Early assessments of the program have studied property values, but miss how rents change for targeted communities given the favoring of the rental housing industry by investors. This paper uses data from the American Community Survey to estimate the OZP's impact on the rental housing market in a differences-in-differences approach with propensity-score matching. I find a one to six percent decrease in rent costs in the post-policy period in areas with a high exposure to the OZP relative to areas with a low exposure to the program. Yet homeownership rates remain relatively stable. Together, the findings suggest that the program successfully eased housing costs for renters without crowding out homeownership.

1 Introduction

Poverty is a geographically-concentrated issue and these households struggle to afford their rent. The majority of households below the poverty line reside in Census tracts with poverty rates greater than 20%. Those in poverty spend a much larger share of their resources on housing than other households.

Incentives to develop poor areas through foreign investment can aid these households through improving their ability to afford their rent. Though poor communities experience the same rates of housing-cost burden as other communities, they two times more likely to experience rent cost mostly because they rent more. Place-based policies can help by lowering housing costs in high poverty communities through making marginal development areas more attractive to real estate developers.

*Roxana Gonzalez-Cortes, Darla Moore School of Business, University of South Carolina, roxanag@email.sc.edu

The Opportunity Zone Program (OZP) is a federal initiative aimed at stimulating economic development and job creation in distressed communities through tax incentives for investments in these areas. The bill was signed into law on December 28, 2017 as a provision in the Tax Cuts and Jobs Act with little preceding media attention. Opportunity Zones (OZs) became open for investment in 2018. The program is the largest place-based policy since the Empowerment Zone program of 1993 and was estimated to cost the federal government $8.2 billion dollars in foregone revenue in the 2020-2024 period.[1] Investors can defer and reduce tax liability owed on prior capital gains by rolling them over into Qualifying Opportunity Funds (QOFs) and holding them for at least five years.

Half a decade later, OZ investment has been highly concentrated in terms of industry and geography. Just one percent of Opportunity Zones received 42 percent of investment and just five percent of Zones received 78 percent of investments by 2019 (**?**). In addition to being extremely top heavy, investment has been highly concentrated by industry. The Joint Committee of Taxation reported that by 2021, two thirds of investment had gone into real estate (Joint Committee on Taxation Staff, 2023). Multifamily housing, specifically, accounted for well over half of all investment as of late 2021.[2] Rental costs are particularly important for low-income communities, who experience depressed rates of homeownership and high housing costs relative to other income groups. Given the strong favoring of rental housing as investments and the high share of renter-occupied tenure in low-income communities, it is key for policymakers to understand the effects of the OZP on rental costs. Such insights are essential for evaluating whether the program effectively addresses the housing affordability challenges faced by the targeted communities.

I leverage the natural experimentd designed by the policy to answer how the OZP impacted rents to address this gap in our understanding of the OZP's effects. This paper uses data from the American Community Survey (ACS) to study the OZP's effects on rents with a difference-in-differences framework. I also examine whether potential effects on rents crowd out homeownership and whether the policy impacted home values to anchor my results to prior studies. I compare changes in these housing outcomes with two empirical approaches, making use of the ACS's one- and 5-year estimates. The first comparison includes Census tracts designated as OZs and similar low-income tracts that were untreated whereas the second comparison uses public-use metropolitan areas (PUMAs) with high shares of residents in OZs to PUMAs with low shares of residents in OZs. I find that the in the post-investment period, median rents decreased by roughly one to six percent in treated areas. I find no evidence that the program decreased homeownership by more than a third of a percent. The results suggest that the policy effectively increased affordability for renters without crowding out homeownership.

Early assessments of the OZP have focused on outcomes unrelated to housing affordability or have only considered effects on single family home values. Perhaps unsurprisingly, given the lack of investment

[1] `https://www.urban.org/urban-wire/what-we-do-and-dont-know-about-opportunity-zones`
[2] `https://www.clarionpartners.com/insights/future-opportunity-zones`

into businesses, studies have found modest, if any, improvements in employment, earnings, or poverty of zone residents (**??**). Among housing market studies, the program has been shown to impact single family home prices modestly, with estimates ranging from a 0.5 to 6.8 percent increase in home values in OZs (**???**). These studies have overlooked the effects on rents though the rental housing industry was the only industry exhibiting a detectable increase in investment according to a working paper using data as recent as 2022 **?**. Together, early assessments of the program highlight an urgency in exploring the program's effects on a broader range of housing affordability metrics, as they may provide a more accurate understanding of the program's impact in affected communities.

The findings may be provide insights for policymakers in evaluating the role that place-based tax incentives play in addressing housing affordability challenges faced by low-income communities through. This research contributes to economics literature which study effects of the OZP and to broader research on the effects of place-based policies on local outcomes.

The rest of the paper proceeds as follows. Section 2 provides context on the Opportunity Zone Program. Section 3 describes data, key restrictions and limitations, and displays the summary statistics for my final sample. Section 4 details my empirical approach. My results are displayed and contextualized in Section 5. Finally, Section 6 concludes.

2 Background

Billed as a job creation and economic growth program, Opportunity Zones were established by the 2017 Tax Cuts and Jobs Act (TCJA). The bill received little attention prior to its introduction. The program is the largest place-based tax incentive program since the Empowerment Zone program of 1993, expected to cost anywhere from $11 to $15 billion in foregone tax revenue by 2026, the same year that the tax benefits, along with many other TCJA provisions expire (**??**).

The OZP works by stimulating long-term investment in distressed areas through deferred and reduced tax liability for any capital gains reinvested into OZs, especially for those held long-term. Under the policy, investors can roll over previously earned gains into Qualifying Opportunity Funds (QOFs) to reap taxes breaks on them and any new gains earned in the QOFs. To be a QOF, an investment vehicle must hold more than 90 percent of its assets in OZ property other than another QOF.[3] Capital gains placed in QOFs for five (seven) years increase the basis of the original investment by ten (fifteen) percent. For investments held in QOFs for ten years, any new gains on investments are not subject to any capital gains tax. QOFs can be invested into infrastructure, businesses, and real estate alike. However, for investment into existing residential real estate, properties must be "substantially improved" per the TCJA's guidelines.

[3] `https://taxfoundation.org/research/all/federal/opportunity-zones-what-we-know-and-what-we-dont/`

Qualifying Census tracts need to meet the low-income definition borrowed from a 2000 law that created the New Markets Tax Credit. These classifications were calculated using 2015 data from the American Community Survey (ACS). To be eligible for designation as an OZ, a tract needed to have a poverty rate of at least 20 percent, a median family income not exceeding 80 percent of the statewide median for a tract outside the metropolitan area, or a median income not exceeding 80 percent of the metro area median for a metropolitan tract. Eligible tracts could also be non-low-income so long as they were contiguous to a low-income tract and had a median income not exceeding 125 percent of the state's or metro area's median.[4]

The selection process for OZs began with state governors nominating up to 25 percent of eligible Census tracts, or up to 25 tracts for states with less than 100 tracts within 90 days of the law's passage. Once nominated, the U.S. Department of the Treasury reviewed and certified these areas as Opportunity Zones. In all, 8,762 tracts were designated, comprising twelve percent of all Census tracts. Roughly three percent of designated tracts were higher-income tracts contiguous to qualifying low-income tracts. States varied in their strategy for Zone nomination, with some prioritizing high activity areas and some prioritizing others in greater need (**?**). Political factors influenced selection of these zones (**??**), and the program disproportionately qualified gentrifying "high-value" neighborhoods for potential opportunity-zone designation (**??**).

The structure of the attractive tax breaks granted by the OZP incentivize long-term investment in distressed areas across the nation. By deferring and reducing capital gains taxes, the program can channel resources into businesses, infrastructure, and real estate in designated zones. While intended to spur job creation, investment has mostly funneled into housing, paving potential channels by which housing costs could be affected for local residents.

3 Data

The primary data in the analysis is from the ACS for the years 2012-2021. The ACS provides key information on housing, economic, and demographic characteristics at the tract and PUMA levels. The data include six years of estimates before the policy was effective and three years after. The data exclude estimates from 2020, as only unofficial, experimental estimates were released that year due to the COVID-19 pandemic. The main outcomes of interest are median home values, median rents, and homeownership rates. Key covariates from the ACS include a geographical unit's: total population, median income, poverty rate, employment rate, share of Black or Hispanic residents, share of population with a post-secondary degree. I keep units with complete information for all years of the data, reducing the total sample size by less than two percent.

[4] `https://www.cdfifund.gov/opportunity-zones`

I employ a tract-level analysis in the discrete treatment setup using 5-year estimates from the ACS. These are pooled average across five years. Thus, even the last year in my sample, 2021, contains data from the pre-policy period. Hence, the results produced by these data should be regarded as underestimates of the OZP's true effect. The benefit of using the 5-year estimates is their geographic granularity, which allows me to compare *fully treated* units to units with no exposure to the OZP. This is in contrast to the 1-year estimates which are only available at a much higher level of aggregation.

The ACS 1-year estimates are released at the PUMA level. A PUMA is a geographic area containing no fewer than 100,00 people and cleanly encloses multiple Census tracts with no fragmentation. The average PUMA contains 67 tracts. As OZs were designated at the tract-level, I create a continuous treatment variable, bound by 0 and 1, defined as the share of a PUMA's population residing in an OZ.

I identify Opportunity Zones as well as eligible but not designated tracts with data from the Community Development Financial Institutions (CDFI) Fund at the U.S. Department of Treasury.[5] I supplement my data on median home values with a tract-level home price index (HPI) from the Federal Housing Finance Agency, which is computed using weighted, repeat-sales (**?**).

Table 1 reports summary statistics for the treatment and control pools under no propensity score matching. Panel A compares OZs to non-designated low-income tracts for the pre- and post-policy periods. Panel B compares highly treated PUMAs to lowly-treated ones for the the pre- and post-policy periods. Panel A illustrates the main identification issue, stemming from state governors nominating tracts with higher poverty rates and lower median incomes. Opportunity Zones had a higher share of Black residents, a lower share of Hispanic residents, and a lower share of college graduates. Median rents and home values were lower in treated areas relative to the control units. Lastly, treated areas had depressed rates homeownership relative to their comparison group. The treatment and control pools in Panel B display similar patterns. Table 1 illustrates why in the absence of propensity-score matching, the selection in OZ designation could bias the results.

[5] `https://www.cdfifund.gov/opportunity-zones`

Table 1: Summary Statistics with P-values

Panel A: Discrete Treatment

Variable	Eligible LICs Pre-2018	OZs Pre-2018	Eligible LICs Post-2018	OZs Post-2018	Pre-2018 p-value
Poverty Rate	23.95 (10.91)	28.33 (12.48)	24.32 (11.61)	20.68 (10.48)	0.00
Median Household Income	39,293.57 (12,055.46)	34,573.21 (12,037.35)	41,942.54 (15,233.55)	47,388.58 (16,017.35)	0.00
Total Population	4,049.51 (1,809.03)	4,010.71 (1,896.59)	3,902.02 (1,810.59)	3,963.78 (1,747.34)	0.00
Share Black	21.40 (27.14)	27.33 (30.59)	26.64 (29.68)	21.15 (26.56)	0.00
Share Hispanic	48.60 (32.15)	44.73 (32.46)	44.17 (31.71)	47.34 (31.63)	0.00
Share College Grad	22.95 (14.26)	18.52 (12.31)	21.43 (13.45)	20.74 (15.37)	0.00

Panel B: Continuous Treatment

Variable	Less-Exposed PUMAs Pre-2018	More-Exposed PUMAs Pre-2018	Less-Exposed PUMAs Post-2018	More-Exposed PUMAs Post-2018	Pre-2018 p-value
Poverty Rate	12.29 (6.26)	23.60 (8.64)	19.36 (7.30)	10.64 (5.51)	0.00
Median Household Income	64,037.77 (17,599.50)	42,880.19 (13,110.58)	53,204.53 (16,382.66)	76,477.02 (20,476.72)	0.00
Total Population	133,508.36 (26,586.93)	138,160.59 (29,782.07)	140,097.76 (29,838.16)	138,370.35 (28,058.20)	0.00
Share Black	10.31 (13.08)	24.39 (23.42)	23.26 (22.30)	10.58 (13.60)	0.00
Share Hispanic	48.04 (21.97)	29.52 (21.26)	29.64 (22.37)	49.62 (22.49)	0.00
Share College Grad	29.45 (16.43)	21.87 (14.09)	24.68 (15.32)	27.92 (17.18)	0.00

Table 1: Data come from American Community Survey for years 2012-2018. Year 2020 is omitted from all data. Panel A displays mean values and standard deviations for tract-level data. The treated units in Panel A are tracts designated as Opportunity Zones whereas the control units are low-income communities which were eligible for designation. Panel B displays mean values and standard deviations for PUMA-level data. The treated units in Panel B are PUMAs in which at least 20 percent of the population resides in an Opportunity Zone whereas control units are those PUMAs in which no more than 5 percent of the population reside in an Opportunity Zone.

4 Empirical Methodology

The main identification challenge stems from OZ designation not being random but instead a strategic decision of governors.[6]. To identify the program's impact on rental costs, we require a credible control group to serve as a viable counterfactual to those tracts selected as OZs. Given tract-level treatment assignment, ACS 5-year tract estimates offer optimal geographic precision for impact evaluation. However, these estimates' pooling structure contaminates the 2018-2021 treatment period with pre-policy observations. To address this limitation, I complement the analysis with ACS 1-year PUMA-level estimates in a continuous treatment framework, where treatment intensity varies across PUMAs, enabling comparison between high and low exposure areas.

I use both discrete and continuous treatment specifications in my difference-in-differences empirical approaches. In the discrete treatment specification, I compare outcomes in the pre- and post-policy periods for tracts designated as OZs with similar tracts that were eligible but not selected as OZs. In the continuous treatment specification, I compare outcomes in the pre- and post-policy periods for public use microdata areas (PUMAs) with high levels of exposure to the OZP with similar PUMAs which had low levels of exposure to the program. In both specifications, I use propensity-score matching to find control units that were similar to treated units in terms of covariates and trajectories in outcome variables in the pre-policy period.

4.1 Discrete Treatment

In the first specification, I compare tracts which were designated as OZs with similar low-income communities which were not designated, before and after the policy, using a difference-in-differences framework. This simple setup would provide a treatment-on-the-treated (TOT) estimate given data at the year-tract level. Since the ACS only provides tract-level *averages* which are pooled across a five year period, my estimates are instead an imprecise measure of the TOT effect of the Opportunity Zone Program on local housing affordability and homeownership. Equation 1 details the estimating equation.

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$$\mathrm{Y}_{it} = \alpha + \beta OZ * POST_{it} + X_{it} + \gamma_i + \eta_t + \epsilon_{it} \tag{1}$$

[6] https://www.brookings.edu/articles/the-early-results-of-states-opportunity-zones-are-promising-but-theres-still-room-fo

The left hand side variable (Y_{it}) denotes the value of the outcome variable for tract i in year t. The outcome variables are median home values, median monthly rent, and the local homeownership rate. The coefficient on $OZPOST_{it}$, β, captures the effect of the OZP for treated tracts after 2017. Vector X_{it} contains time-variant covariates for tract i in year t which could be correlated with housing affordability and homeownership outcomes such as employment rate, education level, income, share of Black residents, share of Hispanic residents, and total population. Tract fixed effects are denoted by γ_i and control for time-invariant idiosyncratic tract characteristics that could be correlated with OZ designation. Year fixed effects are denoted by η_t and control for unit-invariant changes across time. Errors are clustered at the tract-year level.

4.2 Continuous Treatment

In the second specification, I use a similar framework to compare public use microdata areas (PUMAs) with high levels of exposure to the policy to similar PUMAs with low levels of exposure before and after the policy. PUMAs are non-overlapping, statistical geographic areas containing no fewer than 100,000 people each and encapsulate groups of tracts with no fragmentation of a tract across multiple PUMAs.[7] This setup produces a treatment effect which exploits contrast in treatment intensity and should be interpreted as lower bounds or underestimates of the true effects of the OZP on local housing affordability and homeownership.

I define treatment exposure as the proportion of a PUMA's population residing in an Opportunity Zone. The threshold for high treatment intensity was chosen based on a threshold high enough to differ substantially from the control units but not being so high that statistical power would be depleted. The resulting high treatment threshold was set as having at least 20 percent of a PUMA's population living in an OZ. I evaluate the validity of this control group through an event study analysis in Section 3.3, which tests for parallel pre-trends between treated and control PUMAs to assesses whether the control group exhibits comparable characteristics and trajectories to the treated units prior to policy. The control PUMAs' threshold was selected in a similar way and was set as having no more than 5 percent of a PUMA's population residing in an OZ. In all, the sample contained 154 units in each treatment group.[8] Equation 2 details the estimating equation.

$$Y_{jt} = \alpha + \beta TREAT * POST_{jt} + \chi_{jt} + \nu_j + \lambda_t + \xi_{jt} \tag{2}$$

Equation 2 follows the structure of (1) closely. Here, $TREATPOST_{jt}$ is an indicator equal to 1 if at least 20 percent of PUMA j's population lives in an OZ and equal to 0 if no more than 5 percent of its population resides in an OZ. (Y_{it}) denotes the median home values, median monthly rent, or local

[7] `https://www.census.gov/programs-surveys/geography/guidance/geo-areas/pumas.html`

[8] While the treated group contained 154 unique PUMAs, the "control" group contained just 115 unique PUMAs after matching methods described in section 3.3

homeownership rate of PUMA j in year t. Vector χ_{jt} contains the same time-variant covariates as in (1), but for PUMA j in year t. Unit fixed effects are now calculated at the PUMA level and denoted by ν_j. Year fixed effects are denoted by λ_t and control for unit-invariant changes across time. Errors are clustered at the PUMA-year level and are denoted by ξ_{jt}.

4.3 Control Unit Selection

I need a way to mitigate selection-into-treatment bias so I do PSM based on pre-period growth rate.

To construct more robust control groups for both model specifications, I deliberately omit units from the pools of potential control units based on specific criteria. These omissions result in smaller yet more comparable control groups for both the continuous and discrete treatment frameworks.

First, in constructing my control group for the discrete treatment specification, I start by omitting those tracts contiguous to designated OZs to avoid capturing any spillover effects of the policy on nearby housing markets. Though not directly affected by the OZP, an eligible low-income community could be indirectly impacted by changing property values in a neighboring OZ. In all, 11,563 tracts were excluded from the eligible pool of control tracts, reducing this pool to 11,769 tracts. I proceed with propensity-score matching for both this model and the continuous treatment specifications.

For both model specifications, I refine my groups of control units with propensity score matching based on pre-policy trajectories in values for outcome variables and 2017 values for covariates. Specifically, I use the nearest neighbor (NN) algorithm to match each treated unit to one control unit based on 2012-2015 growth rates in rents, home values, homeownership, and on 2017 values for the same covariates I use in (1) and (2). The NN algorithm allows for multiple control units to match to one treated unit. As such, I end up with 7,956 treated tracts and 7,723 control tracts in the discrete treatment specification; and 154 treated PUMAs and 115 control PUMAs in the continuous treatment specification. Control units which match to multiple treated units are weighted to reflect their match counts and are, at most, matched to three treated units.

5 Results

Table 2 reports the main results for both model specifications. Panel A reports estimates for the discrete treatment approach using tract-level data while Panel B reports the estimates for the continuous treatment approach using PUMA-level data. For the discrete treatment specification, column 1 of row 1 reports a $1,823.20 decrease in median home values for tracts designated as OZs after the policy relative to similar low-income communities whereas the continuous treatment approach reports a $34,102.88 decrease associated with the policy in PUMAs with higher treatment intensities (reported in row 1 of panel B). My results suggest that home values changed by no more than 1.3 percent, supporting prior

studies reporting minor changes to home values (**??**). The discrete treatment specification reports a $9.12 decrease in median rents in OZs in the post-policy period whereas the policy is associated with a $72.42 decrease in the alternative specification in highly-treated PUMAs. The third column reports that the policy was associated with little change in homeownership in both model specifications, with a decrease of less than a third of a percent.

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My preferred specification is the discrete treatment approach as it more cleanly isolates the impact of the OZP, though it uses 5-year averages. The estimates from panel B should be interpreted as a lower bounds of the true values as the partial treatment intensity results in an attenuation toward zero. However, in the average PUMA only about 30 percent of the population resided inside an OZ. Thus, it is less obvious that changes in the mean outcome levels post-policy are attributable to the OZP. Still, a consistent finding across the specification is the negligible change in the homeownership rate. This finding suggests that regardless of the change in local housing costs, the policy did not result in a crowding out of homeownership through the influx of investment into new or existing rental housing.

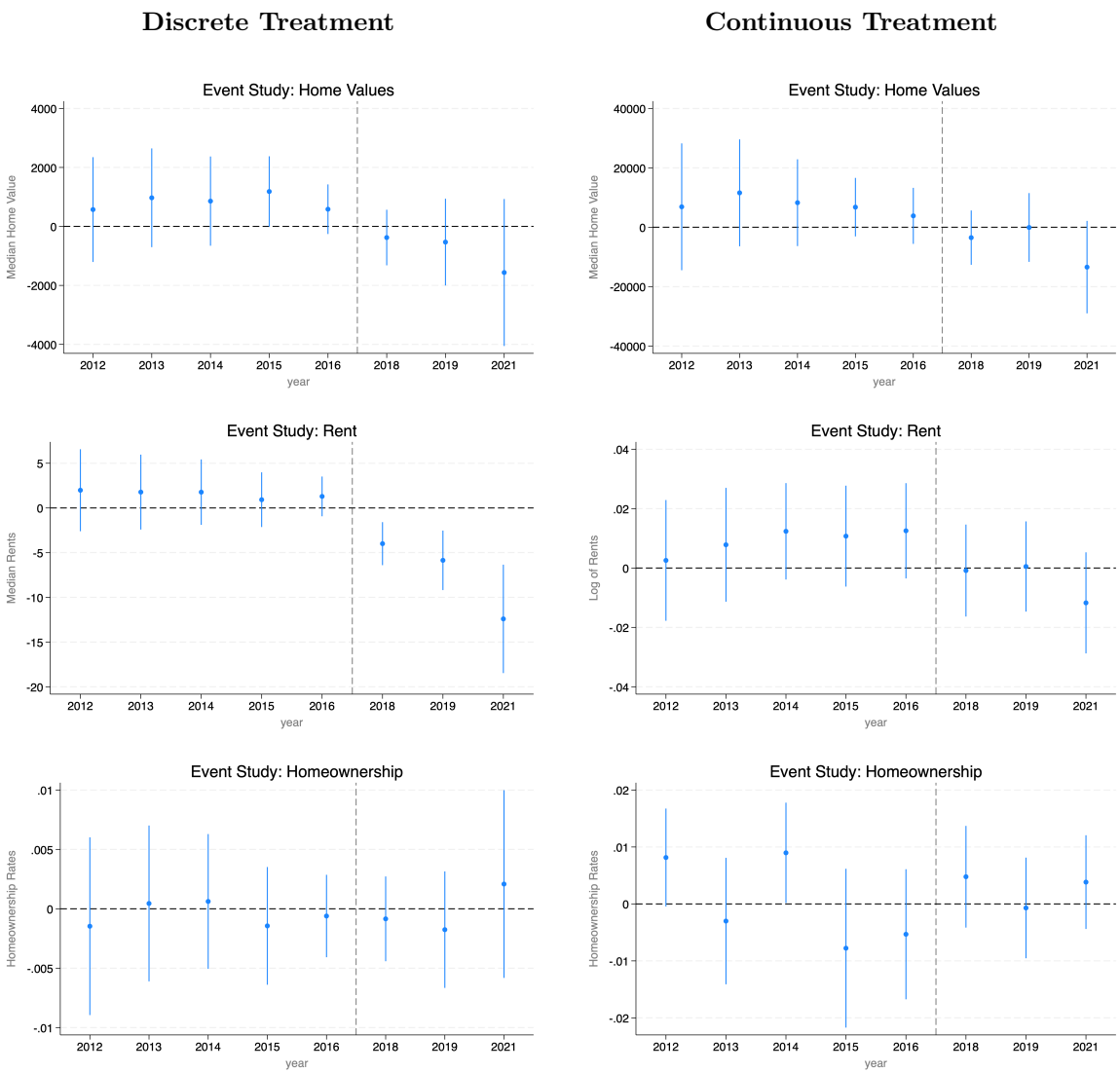
6 Robustness

To establish internal validly, I first provide evidence that trends for the treated and control units did not vary significantly for any of the outcomes in both specifications. I test for differing pre-trends with an event study on my outcomes. Equation 3 describes the event study equation.

$$(\mathrm{Y}_{it}) = \sum_{t=2012, t\neq 2020}^{2021} \beta \cdot \mathrm{TREAT}_i \times \mathrm{YEAR}_t + X_{it} + \eta_i + \epsilon_i \tag{3}$$

Here, Y_{it} is the outcome of interest for unit i in year t. Unit i is a tract or a PUMA, depending on the model specification. $TREAT_i$ is an indicator that takes a value of 1 if unit i is a an OZ in the tract-level specification or a PUMA with at least 20 percent of the population residing in an OZ in the PUMA-level specification. β is the coefficient of interest and captures whether treated units experience significantly different changes in (Y_{it}) in year t on average. Estimates are relative to the omitted year value, 2017 and to the control units. Vector X_{it} contains the same time-variant covariates as in (1) and (2). Unit fixed effects are denoted by η_i and control for time-invariant unit characteristics that could be correlated with treatment. Errors are clustered at the tract or PUMA level, according to the specification.

Figure 1: Event Study



The left column displays the event studies for the discrete treatment approach using the tract-level data. The right column displays the event studies for the continuous treatment approach which uses the PUMA-level data. The point estimates are relative to the pre-policy year, 2017, and to the control group. Controls include employment rate, share of college graduates, share of Black residents, and total population. Errors are clustered at the tract or PUMA level, depending on the specification.

Figures 1-6 display the event studies for median home values, median rents, and homeownership rates. The point estimates are relative to the omitted year, 2017, and to the control group's values. The left column displays the event studies for the discrete treatment approach using the tract-level data whereas the right column displays the event studies for the continuous treatment approach which uses the PUMA-level data. The treated and control units for all outcomes exhibit similar pre-trends for both model specification suggesting that the identifying assumption needed for the difference-in-differences estimation is satisfied.

In both approaches, home values do not differ much prior to 2018, but appear to trend downward

after 2018 for the treated group, though the point estimates are not delectably different from zero. Rent costs trends also do not deviate delectably across the treated and control groups in both the discrete and continuous approaches and significantly decrease after 2018 for the treated group in the discrete specification. In both approaches, homeownership rates in the treated group does not deviate from the control group before or after 2018. Homeownership rates for the treated group in the treatment approach appear to trend differently in some pre-policy years for the continuous treatment approach. Though not statistically detectable changes, results for this outcome in this specification should be interpreted cautiously.

7 Discussion/Conclusion

This paper examines the impact of the Opportunity Zone Program on housing costs and homeownership in designated distressed communities. I use both 5-year estimates from the ACS at the tract level and 1-year estimates at the PUMA level. Using a difference-in-differences framework with data from the American Community Survey, this study provides insights into how the OZ Program influences residential property values, rents, and homeownership rates.

Prior findings on the effects of the program indicated mixed effects on local housing markets. On one hand, there is evidence of increased investment in rental housing, suggesting that tax incentives under the program may have successfully attracted investors to certain types of real estate. Other studies find modest to null effect on singe-family homes. The impact on rents and homeownership remain understudied. These mixed findings in previous literature and the one in this paper highlight the complexity of place-based economic policies and the varying effects they can have on different segments of the housing market.

In my preferred specification, the main results suggest that the OZP's successfully stabilized housing affordability for renters without crowding out homeownership. The negligible effects on local homeownership rates remain consistent across both specifications. Overall, the evidence from this study contributes to the ongoing debate about the effectiveness of the Opportunity Zone Program and place-based policies. While the OZP has shown some promise in attracting investment and potentially improving the housing stock, the implications for homeownership and housing costs remain mixed. Policymakers should consider these nuanced outcomes when designing and implementing similar place-based tax incentive programs in the future. Future research could explore longer-term effects of the program, particularly as investments mature and as economic conditions change.

8 Annotated Bibliography

References