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The Impact of the Opportunity Zone Program on Residential Real Estate

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Abstract. ***Problem definition*:** Opportunity zones (OZs) are designated census tracts in which real estate investments can gain tax benefits. Introduced by the U.S. Tax Cuts and Jobs Act of 2017, the goal of the OZ program is to foster economic development in distressed neighborhoods. In this paper, we investigate and optimize the OZ selection process and examine the impact of OZs by exploiting two data sets: a proprietary real estate data set that includes 36.1 million residential transactions spanning all 50 U.S. states and census-tract demographics data between 2010 and 2019. ***Methodology/results*:** We show that census tracts with higher poverty and unemployment rates were more likely to be selected. Counterintuitively, however, tracts with a higher average real estate price were also more likely to be selected. We then apply difference-in-differences, synthetic control, and matching techniques to rigorously assess the impact of the OZ program on two key real estate metrics: price and transaction volume. We find that the OZ program increased real estate prices by 4.03%–6.13% but do not observe a significant effect on the transaction volume. We also find that investors primarily targeted the high-end real estate market, namely, exhibiting a cherry-picking behavior. To better fulfill its intended societal and economic goals, we propose an optimization framework with fairness considerations for OZ assignment decisions. We show that the OZs assigned from our fairness-aware optimization formulation can better serve distressed communities and mitigate investors' cherry-picking behavior. ***Managerial implications*:** Our paper underscores the importance of incorporating fairness in OZ designation to achieve a desirable real estate market reaction. Our large-scale empirical analysis provides a comprehensive assessment of the current government OZ assignment, and our fairness-aware optimization framework provides concrete recommendations for policy makers.

Supplemental Material: The online appendix is available at https://doi.org/10.1287/msom.2024.0746.

Keywords: opportunity zones • real estate • public policy • natural experiment • fairness-aware optimization

1. Introduction

As part of the U.S. Tax Cuts and Jobs Act of 2017, opportunity zones (OZs) were created as a way to spur economic development in distressed communities by encouraging investment in targeted census tracts (CTs) across the United States. Introduced by Senators Tim Scott (R-SC) and Cory Booker (D-NJ) and U.S. Representative Ron Kind (D-WI), the program provides tax incentives for commercial real estate investments in designated zones with benefits increasing for long-term investments until 2026. To this day, the OZ program is considered the most significant U.S. place-based incentive program in the last several decades. OZs have been a popular topic of discussion in the real estate investment community over the past few years with many large developers expressing interest in the program because of its capital gains tax benefits.

The OZ program is estimated to cost $1.6 billion between 2018 and 2027 (Eastman and Kaeding 2019). Given the ambitious goal and onerous cost of this program, one should naturally question its effectiveness in improving the standard of living of residents in economically distressed neighborhoods. Some common signals of community improvement include job creation, changes in household income, and resident inflows and outflows. In this paper, we study this problem by focusing on the residential real estate indicators. Residential real estate activities serve as a strong indication of the health and prosperity of a neighborhood and can be consistently measured across the entire country. To obtain a comprehensive assessment, we empirically examine the determinants of the OZ selection process and estimate the impact of the OZ program on the residential real estate market. We then construct a constrained optimization framework that incorporates fairness considerations for OZ assignment decisions to better achieve the intended societal and economic goals of the program.

Although the OZ program is directly targeted at commercial real estate, OZs are primarily located in residential areas in which asset classes that are traditionally considered as "commercial" are underrepresented. Whereas this might appear as a misalignment, it is actually not: in the past decade, the boundary between commercial and residential real estate has been blurring. Institutional investors are highly interested in single-family residential assets. Several commercial organizations, such as Invitation Homes, American Homes 4 Rent, and Pretium Partners manage portfolios of thousands of single-family houses.[1] With a great interest from many commercial real estate investors and the highest volume of transactions, the residential market is among the strongest indicators of the macroeconomic effects of the OZ program.

We highlight several aspects of the OZ program that motivate us to raise questions regarding its effectiveness. First, although subject to requirements mandated by the federal government on several characteristics, the final process of selecting census tracts as OZs was left to the states. Each state opted for different designation criteria and levels of transparency in this process. Second, according to our industry partner's records, there are many cases of large investors that acquired OZ properties with exorbitant prices in 2019. Although we can neither prove nor disprove that these investors plan to claim a tax deduction on their capital gains from these acquired properties, it seems reasonable to assume that these investors were fully aware of the OZ program. The following transactions are examples:

1. MG Properties, the real estate arm of McCourt Global (www.mccourt.com), acquired four properties for a total of $379M, the largest of which is a $186M luxury apartment complex in Oregon (see Online Figure A1 for an illustration of the interior and exterior).
2. The CIM Group (www.cimgroup.com) acquired a two-tower apartment complex in Virginia for a total of $254M.
3. An interesting case is the acquisition by Fairstead (www.fairstead.com) and Meadow Partners (www.meadowpartners.com) of a 125-unit apartment complex in Brooklyn, New York, for $67.25M. Although this landmark complex is located within the boundaries of an OZ, the median income of the residents in this zone increased from $31,591 in 2010 to $122,250 in 2019. Clearly, this zone has not been a low-income neighborhood for many years.

As for any large government program, especially one focused on real estate development, determining the program's effectiveness on a national scale requires large amounts of data. We aggregate 10 years of nationwide real estate transactions data from 2010 to 2019 with census demographics data to form a data set that comprises 36.1 million residential sales transactions. We leverage this data set with the goal to answer the following three research questions:

1. What role did demographics and real estate characteristics play in the OZ selection process?
2. What is the impact of the OZ program on residential real estate prices and transaction volume?
3. What is the value of applying fairness-aware optimization for OZ assignment decisions?

The first research question aims at determining whether there were consistent patterns in the OZ selection process by the government, looking beyond the policy-mandated demographic restrictions. We employ a logistic regression to investigate the roles of different factors, particularly real estate metrics. We find that, within the set of qualified census tracts, states had a tendency to select tracts with a higher average residential real estate price per square foot, namely, the process was biased toward high-end tracts. We then provide plausible explanations for this finding. Ultimately, it suggests that there may have been a lack of fairness in the OZ delegation process.

The second research question relates to the impact of the OZ program on residential real estate prices and volume. As key real estate indicators, these two metrics are proxies of investor behavior, and we use them to infer the dynamics of supply and demand in the real estate market. We conduct our analysis based on two empirical strategies: a difference-in-differences (DID) approach (which accounts for spillover effects) and the generalized synthetic control method. We compare the average real estate price and transaction volume across all three types of census tracts (tracts selected as OZs, tracts that were qualified but not selected, and tracts that were neither qualified nor selected). We also implement a propensity score matching (PSM) method to strengthen the validity of our comparisons. To alleviate the concerns of potential spillover effect of the program to nonopportunity zones (NOZs), we propose a spillover-robust difference-in-differences model to estimate the impact of the OZ program. We find that the OZ program, on average, has increased real estate prices by 4.03%–6.13% relative to NOZs, but we do not observe a significant effect on the transaction volume. A price surge without a concurrent volume increase implies that the OZ program had spurred investors' interest to invest and had boosted demand in the real estate market. We further estimate the heterogeneous impact of the OZ program with respect to preexisting demographics and real estate characteristics and show that the impact of the OZ program is stronger in more high-end OZs (i.e., OZs with higher real estate prices). This finding indicates that the demand increase is not uniform among assigned OZs, and investors primarily targeted high-end OZs. The cherry-picking behavior of investors naturally leads to fairness concerns.

The third research question seeks to prescribe actionable recommendations. We develop a mixed-integer program (MIP) that incorporates the notion of fairness to determine the OZ assignment. We show that the OZ assignment from our fairness-aware optimization model outperforms the actual OZ assignment by the government in multiple dimensions of fairness. For example, the average poverty rate is higher in the OZs selected by our model relative to those selected by the government, suggesting that the OZ assignment from our model can better serve distressed communities. More importantly, we show that pursuing a more fair OZ assignment with our optimization framework may not lead to significant economic loss and can successfully mitigate investors' cherry-picking behavior toward the higher end of the real estate market.

Our paper is organized as follows. We review the relevant literature in Section 2. Section 3 provides detailed explanations on the OZ qualification criteria and a full description of our data sources. Section 4 evaluates the factors that influenced the OZ selection process, and Section 5 examines the impact of the OZ program on residential real estate. Section 6 describes our fairness-aware optimization framework and demonstrates its value. Finally, we summarize our results and discuss their implications in Section 7. We conduct a series of robustness tests to showcase the consistency of our findings in the Online Appendix and e-companion.

2. Related Literature

This paper is related to the following four streams of literature: place-based policies (including the nascent literature on OZs), natural experiments in social sciences, fairness-aware and place-based optimization, and socially responsible operations.

2.1. Place-Based Policies and Opportunity Zones

Place-based policies refer to government efforts to provide economic incentives in order to boost the performance of economically challenged areas (see Neumark and Simpson 2015 for a comprehensive review for this topic). The OZ program is by far the most significant U.S. place-based policy, and prior research investigates the leading factors in the OZ selection process, such as the role of demographics and political affiliation (Frank et al. 2022), and the impact of the OZ program on job creation (Arefeva et al. 2023) and real estate metrics (Chen et al. 2019, Sage et al. 2023). We highlight that previous studies rely on aggregate data, such as the annual Housing Price Index provided by the Federal Housing Finance Agency, and are focused on commercial real estate metrics. In contrast, our paper examines the OZ selection process with a focus on the role of key residential real estate metrics, assesses the impact of the OZ program on these metrics by leveraging granular and comprehensive residential sales transactions data, and further proposes an optimization framework to select OZs.

2.2. Natural Experiments in Social Sciences

A natural experiment (as opposed to a field experiment) refers to the situation in which exposure to the treatment and control conditions is determined by nature or outside the control of the researchers. Natural experiments are useful for empirical research when the subpopulation exposed to an event (e.g., implementation of a public policy) can be clearly identified and changes in outcomes can be attributed to the event exposure. Examination of the impact of a specific event is at the core of empirical research in social sciences (see Dunning 2012 for more details). This type of empirical study has gained increasing attention in operations management in recent years (Terwiesch et al. 2020) encompassing a multitude of contexts, such as nurse staffing (Lu and Lu 2017), corporate social responsibility engagement (Li and Wu 2020), and online platform price promotions during initial public offerings (Cohen and Mitrofanov 2022). In our context, the launch of the OZ program in early 2018 is a natural experiment, and our granular data precisely identify the census tracts that were designated as OZs. Our paper adds to this literature through rigorously evaluating the impact of the OZ program on key real estate metrics.

2.3. Fairness-Aware and Place-Based Optimization

Fairness consideration has gained increasing attention in the operations management literature. Our work is closely related to the literature on fairness in resource allocation problems (Bertsimas et al. 2012, Breugem et al. 2022). The notion of fairness in this literature relates to distributive justice (e.g., Greenberg 1990), and different schemes have been developed for incorporating fairness considerations, such as embedding fairness in the objectives (Marsh and Schilling 1994) and constraints (Bertsimas et al. 2011). For example, Bertsimas et al. (2011) develop a framework that incorporates fairness considerations by imposing constraints on utility allocations across entities based on certain fairness schemes, and they investigate two fairness schemes: proportional fairness and max-min fairness. In our paper, we adopt a mean-variance framework as our objective and impose certain constraints to incorporate fairness. Our formulation aligns with the concept of distributive justice and also accounts for tractability and appropriateness to the goal of the OZ program.

Fairness concerns have been studied in strategic decision making based on game-theoretical models in a variety of contexts, such as strategic pricing (Li and Jain 2016) and compensation contracts (Li et al. 2020). In these studies, fairness is modeled as a utility term that depends on a reference point. Fairness can also be modeled as a constraint to achieve a fairer distribution of outcomes. Cohen et al. (2022) study the impact of imposing fairness constraints on pricing strategies and social welfare, in which the constraints ensure that a seller can only set similar prices, demand, consumer surplus, or no-purchase valuation for different customer groups. In the same spirit, many existing papers investigate how to address social injustice in different business scenarios (e.g., Cui et al. 2020 reduce racial discrimination in sharing economy). In our paper, beyond incorporating fairness considerations directly into the objective function, we also impose constraints to improve fairness from multiple dimensions to advance social justice.

Our paper is also related to the literature on optimization for public sector planning. In the domain of place-based policies, a variety of topics have been studied, such as political redistricting (Validi et al. 2022), housing mobility programs (Johnson and Hurter 2000), and refugee resettlement (Ahani et al. 2021). In our paper, we propose a framework to optimize the OZ assignment, incorporating fairness considerations into the optimization formulation.

2.4. Socially Responsible Operations

Social responsibility is an emerging area in operations management (Netessine 2022) that touches upon a wide range of nascent topics, such as gender disparity (Plambeck and Ramdas 2020), disparity of resource allocation in political voting (Cachon and Kaaua 2022), and disruption in small-size firms in emerging markets (Kundu et al. 2024). Our paper contributes to this literature by studying the impact of the largest U.S. place-based policy to date that aims to serve disadvantaged communities and by evaluating its fairness implications.

3. Setting and Data

In this section, we first discuss the OZ program. We then describe our data sets that comprise comprehensive real estate transactions and census-tract demographics.

3.1. OZ Program

OZs were introduced as part of the U.S. Tax Cuts and Jobs Act of 2017, an amendment to the Internal Revenue Service (IRS) code of 1986 that included several changes to the tax code and was signed into law in December 2017. The intent of the program was to spur economic development and job creation in low-income communities through tax incentives for real estate investment. To qualify for the tax benefits, investors must invest via a qualified opportunity fund; they must purchase and provide substantial improvement to a property located in designated census tracts,[2] also referred to as OZs; and the renovated property must be used to support a business in that tract.[3] The tax benefits are related to capital gains taxes: previously earned capital gains that are invested in these properties are deferred until the end of 2026 unless the investment is divested earlier. In addition, there is a potential tax reduction on any gain, proportional to the length of the investment, with the possibility to avoid paying taxes on any OZ investment gains if held longer than 10 years. More details on this topic can be found in IRS (2019) and Rapport (2020).

The OZ selection process is a decentralized process. The state executive officers (e.g., state governors) first nominated census tracts as OZs, and the U.S. Department of the Treasury then certified the nominations. According to the guidelines from the U.S. Department of the Treasury,[4] to be selected as an OZ, a census tract needed to first qualify as an OZ by satisfying either of the following:

- Qualifies as a low-income community (LIC) under the New Markets Tax Credit program.[5]
- Is contiguous to an LIC and the median family income is below 125% of its LIC neighbor.

Per the act, states had to designate 25% of qualifying census tracts as OZs by March 2018, and non-LICs could not account for more than 5% of the total. The detailed selection criteria adopted by each state demonstrate a significant lack of transparency apart from the general criteria specified in the federal guidelines. Some states that have disclosed their selection procedures also exhibit a significant degree of heterogeneity in the self-reported major factors used.[6] This decentralized selection process, although widely criticized by the public, has sparked academic research interest to empirically identify driving forces in selecting OZs across states (Frank et al. 2022).

Per the timeline of the decentralized designation process of OZs, the first wave of OZs was designated in 18 states on April 9, 2018, and the final wave of designations was completed on June 14, 2018. In total, 8,761 census tracts were designated as OZs, accounting for 12% of the total 73,057 census tracts in all 50 states and the District of Columbia in the United States.[7] Online Figure A2 presents a U.S. map showing all the OZs.

3.2. Data Description

We exploit two data sets in this paper: nationwide real estate data and census tract demographics data. The real estate data provide transaction-level tax and sales records for the entire United States across several asset types. This information is publicly available at county-level government offices. We note that the raw data collected from the county-level government present large discrepancies in their quality, depth, and coverage. This can result from a lack of nationwide real estate data reporting and collection standards. We source this information from a major data provider that regularly consolidates records from all U.S. county offices into a single harmonized proprietary data set. With additional proprietary data fields, our data provider was able to substantially reduce the discrepancies in this data set via data standardization. For example, our data provider standardizes the data to ensure that the same asset type is presented uniformly across states. As discussed, our study focuses on the residential real estate market. Thus, this data standardization step is important to ensure that we conduct reliable analyses on the right asset type using the right transactions across states.

Our real estate data set contains key fields related to property sales transactions, including the price and asset type. Three asset types are present in our data throughout the period spanning 2010 to 2019: (i) residential sales comprise 36.1 million transactions, (ii) commercial multifamily sales account for 0.45 million transactions, and (iii) office sales include 0.19 million transactions. In our context, residential real estate transactions pertain to single-family properties, whereas commercial real estate transactions include multifamily properties and offices. As discussed, we focus on the residential real estate market to study the impact of the OZ program because it is a strong indication of the development and growth of communities. In addition, residential sales are abundant and similar across neighboring census tracts, hence allowing us to consistently evaluate the policy impact. As we observe in our data, residential sales account for 98.9% of all real estate transactions. In contrast, the limited number of observations on commercial real estate transactions may hinder a rigorous statistical analysis. As a reference, we provide an analysis of the impact of the OZ program on commercial sales in Online Appendix EC3.4.8. For ease of exposition, our description of sales prices and transaction volume hereafter refers to residential properties.

Regarding the time frame, our real estate data set contains sales transactions over a span of 10 years from the beginning of 2010 to the end of 2019. This time span includes an eight-year period before the OZ program officially started in Q1 2018 and approximately two years after the program was implemented. We exclude the 2020 data from our data set because of the Covid-19 disruption.

With respect to census tracts, our real estate data set contains sales transaction data on 63,165 census tracts, which accounts for 86.46% of the total 73,057 U.S. census tracts. For the remaining 13.54% census tracts, we do not observe any sales in our data. This can be due to the nondisclosure policy in some states. We note that the transactions data collected at the county level are regulated at the state level, and they follow the reporting schemes of their states. One difference in the reporting scheme lies in the level of disclosure.[8] For nondisclosure states, either (i) property sales prices are not required to be submitted to the county recorder or (ii) the county recorder cannot release the property sales prices to the public. Although not compulsory, our data show that some transactions were still reported in nondisclosure states.

Our demographics data set is based on public sources from the 2010 U.S. census as well as updates via the American Community Survey that are collected at the census tract level and updated yearly for the period 2010–2019. The demographics data contain the factors outlined in the government official guidelines that shaped the OZ designation—poverty rate and median family income—and several additional fields, such as unemployment rate. As discussed in Section 3.1, 8,768 census tracts were designated as OZs. For the remaining census tracts not designated as OZs, we further identified 33,350 of them that were qualified as LICs but were not selected by the program based on the demographics data. Thus, we segment the census tracts into three categories: (i) OZ (i.e., tracts that were qualified and selected as OZs), (ii) QNS (i.e., tracts that were qualified according to the OZ program guidelines but ultimately not selected), and (iii) NQ (i.e., tracts that were not qualified as OZs).

We report that, among the total 73,057 census tracts, 10.8% are OZs, 56.6% are QNS tracts, and 32.6% are NQ tracts; for the 63,165 census tracts on which we have sales transaction data, 10.8% are OZs, 56.3% are QNS tracts, and 32.9% are NQ tracts. We observe that the descriptive statistics of this subset of 63,165 census tracts are fairly close to that of the entire set of 73,057 census tracts. Specifically, when averaging at the census tract level for the OZ group, the poverty rate, unemployment rate, and median household income are 12.2%, 8.9%, and 56.8K, respectively, and for the 63,165 census tracts, they are 12.3%, 9.1%, and 58.3K, respectively, for the 73,057 census tracts. Thus, without loss of generality, we discard the census tracts without transaction data from our analysis.

We aggregate the sales transaction data for each census tract at the quarterly level and then merge the real estate data set and the demographics data set to form a single data set for our analysis. As mentioned, this merged data set encompasses 63,165 census tracts. We apply the following filters (either before or after merging both data sets) to remove outliers and noisy observations and alleviate data sparsity issues. First, we exclude all sales transactions with a price per square foot below $10 or higher than $2,000; they account for approximately 5% and 0.3% of all transactions, respectively.[9] We treat these transactions as non–market-based outliers because such transactions are unlikely to provide a reasonable representation of the real estate market price. Second, we retain only tracts with an average of at least 10 transactions per quarter before the OZ program launch at the beginning of 2018. We apply this filter for two reasons: (i) it is hard to accurately estimate the price of real estate for census tracts with a small number of sales, and (ii) price and volume estimates are more sensitive to noise if we have a small number of sales for a specific census tract in a particular quarter. We acknowledge that the second filter removes a significant number of observations (43% of census tract–quarter pairs, 49% of the census tracts, and 14.6% of residential sales transactions are removed). However, through conducting a series of robustness analyses (see Online Sections A2 and EC3.4), we alleviate the concerns of introducing bias in preprocessing our data for our analysis. Thus, for ease of exposition, we use this final filtered data set for our analysis in Sections 4 and 5.

Our final filtered data set consists of 1,211,874 census tract–quarter pairs. Each pair is associated with transaction volume and an average price. The data set covers 30,374 census tracts and 28.15 million residential transactions. We report that, in our final filtered data set, the proportions of OZ, QNS, and NQ tracts are 5.9%, 43.1%, and 51%, respectively; when averaging at the census tract level, the poverty rate is 8.7%, the unemployment rate is 8.1%, and the median household income is 67.1K. We report descriptive statistics of our data in Online Section EC1.

4. OZ Selection Process

In this section, we investigate the process of selecting OZs. Our goal is to identify which demographics and real estate characteristics played a role in the selection process. We model the probability of a census tract being selected as an OZ using a logistic regression. Let indices i and j denote a state and a census tract, respectively. The probability of census tract CT_j being selected as an OZ can be modeled as follows:

$$\Pr(\mathrm{CT}_j = 1) = \mathrm{Logistic}(\alpha_i + \mathrm{X_j}\boldsymbol{\beta}), \quad (1)$$

where the dependent variable, CT_j, equals one if census tract j is selected as an OZ and zero otherwise. We account for unobserved heterogeneity in the OZ selection process by including the state fixed effects as captured by the intercept parameter α_i. The vector $\mathrm{X_j}$ contains one or multiple of the following covariates:

- *Price*: average price per square foot in a census tract.
- *Volume*: average number of sales transactions in a census tract.
- *Income level*: average median income in a census tract.
- *Poverty rate*: percentage of individuals under the poverty threshold in a census tract.
- *Unemployment rate*: percentage of individuals unemployed in a census tract.

The variables above are averaged over the quarters of the pretreatment period defined as the period from the beginning of 2010 to the end of 2017.

We estimate the model in Equation (1) using the sample that includes all OZs and all the QNS tracts and present the results in Tables 1 and 2. In Table 1, Models (1)–(5) include a single covariate at a time, whereas Model (6) includes all the covariates simultaneously. Table 2 shows the importance of the covariates in the OZ selection process, in which each column provides the pseudo R^2 and the likelihood ratio (LR) statistics when we exclude a specific covariate from Model (6) in Table 1.

Table 1 shows that, for the demographic covariates, we observe positive and statistically significant coefficients for the poverty and unemployment rates and a negative and statistically significant coefficient for income level. These results show that OZs were more likely to be selected in less privileged neighborhoods. Regarding real estate variables, we find that the estimated coefficient corresponding to the price variable becomes positive and statistically significant in Model (6), whereas the coefficient for volume is negative throughout. One possible explanation for the change of sign in the price variable from Model (1) to Model (6) is that the real estate

Table 1. Logistic Regression to Examine How Qualified Census Tracts Were Selected as Opportunity Zones

	Model (1)	Model (2)	Model (3)	Model (4)	Model (5)	Model (6)
Price	−0.003***					0.002***
	(−6.948)					(4.805)
Volume		−0.015***				−0.005**
		(−6.044)				(−2.150)
Poverty rate			0.083***			0.033***
			(29.062)			(7.638)
Unemployment rate				0.134***		0.057***
				(24.050)		(8.293)
Income level					−0.068***	−0.040***
					(−26.301)	(−11.128)
Const	−1.523***	−1.542***	−3.177***	−3.325***	1.102***	−1.374***
	(−29.946)	(−28.358)	(−55.314)	(−46.811)	(10.249)	(−6.416)
Number of observations	13,097	13,097	13,097	13,097	13,097	13,097
Pseudo R^2	0.005	0.004	0.084	0.056	0.082	0.105

Notes. The values in parentheses are t statistics. We follow this convention for all tables throughout the paper unless noted otherwise.
*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

Table 2. Significance of Each Component in Explaining Selection of OZs Among Qualified Census Tracts

		Price	Volume	Poverty rate	Unemployment rate	Income level
Qualified	Pseudo R^2	0.125	0.126	0.120	0.120	0.109
	LR statistic	8.341	0.024	59.619	62.433	175.970

variables are correlated with demographics variables. For instance, we find a correlation of 0.39 between price and income level as opposed to 0.29 between volume and income level in the sample. Online Table A1 reports the full Pearson correlation matrix.

Table 2 shows that we can rank the covariates by decreasing importance in explaining the OZ selection process in the following order: income level, unemployment rate, poverty rate, price, and volume. This finding is consistent for both metrics (pseudo R^2 and LR statistics) and indicates that demographics variables were the major determinants of the OZ selection process, compared with real estate variables.

The results from Tables 1 and 2 offer interesting insights into the OZ selection process. Overall, we find that the selection process mainly focused on demographics variables, also taking into account real estate variables. Notably, the sign of the price coefficient in Model (6) is counterintuitive: it suggests that, other conditions being equal, the government selected census tracts with a higher average property price to be OZs. This finding implies that the selected OZs may not be the most distressed areas (given the high positive correlation between real estate price and median income). We highlight some anecdotal evidence that supports this finding. Trickey (2020) observes that the OZ designation in Cleveland selected areas were not the most distressed, but some distance away from the most distressed neighborhoods. We also find that the qualitative insights with respect to both demographics and real estate variables remain the same across all model specifications when we include the NQ tracts, and we detail this analysis in Online Section A2.1.

To further validate our finding that selected OZs are not necessarily census tracts with low real estate prices, we conduct additional analyses that involve two main steps: (i) apply the PSM technique[10] to select OZs and QNS tracts with similar demographic characteristics and form matched subsamples and (ii) compare these two matched subsamples in terms of residential real estate price per square foot.[11] We conduct PSM based on the demographic covariates used in Equation (1), namely, poverty rate, unemployment rate, and income level, and our matched data set consists of 1,513 QNS tracts and 1,513 OZ tracts. The matched subsamples have similar demographic characteristics (see Online Section A2.2 for a detailed comparison). We also note that the number of OZs in our matched data corresponds to 17.3% of all designated OZs, and as expected, this fraction is relatively small because of the matching criteria. Our finding that tracts designated as OZs had a higher average price relative to tracts with similar demographics that were not selected as OZs is further supported from estimating Equation (1) using the matched subsamples, and we detail this analysis in Online Section A2.2. One interpretation of this finding is that the policy makers intentionally selected the more high-end areas in the real estate market to be OZs because they believed that these areas had a better chance of attracting investors. As described in Trickey (2020), the policy makers may resort to this practice because investors typically rely on the OZ program for market rate–type housing projects. Intuitively, high-end areas can be more attractive to investors given their higher returns.

We acknowledge that the aforementioned analyses may suffer from bias of an omitted variable that can be correlated with the average real estate price. For example, one possible scenario is that the policy makers preferred selecting OZs from adjacent neighborhoods to form contiguous opportunity enclaves, and the adjacent neighborhoods happened to be more high-end in the real estate market. Although we cannot prove empirically whether the policy makers selected more high-end areas as OZs intentionally, in either case, our finding shows that the most distressed communities have been left out, thus suggesting a misalignment between the OZ designation process and the intended goals of the OZ program.

5. Impact of the OZ Program

In this section, we investigate the impact of the OZ program on two key real estate metrics: residential real estate price and transaction volume. We view the designation of the OZs by the U.S. Department of the Treasury as a natural intervention and assess the impact of such treatment using a quasi-experimental research design. Specifically, we apply a DID approach and focus on estimating the average treatment effect on the treated (ATT). For conciseness, we use "average treatment effect" to refer to ATT hereafter. We also investigate whether the treatment effect is heterogeneous over time or across census tracts because of preexisting census tract characteristics.

We focus on the two real estate metrics, price and volume, because we aim to infer whether the OZ program spurred investors' interest in investing in OZs (i.e., boosted demand). Price and volume are major indicators of the demand and supply dynamics of the real estate market. Following the canonical relationship between demand and supply, if both the price and transaction volume remained unchanged (respectively, changed in certain directions), then we can infer that the OZ program had no impact (had an impact) on the demand or on the supply. We note that we can only infer the demand and supply dynamics from the price and volume metrics because the demand and supply data of the real estate market are not available (e.g., we do not have demand proxy data, such as the number of investors or the number of investment funds targeted at OZs).

5.1. Estimation of Average Treatment Effect on the Treated

Following a DID approach, we use a two-way fixed effects (TWFE) model to estimate the average treatment effect of the OZ program, based on the specification in Equation (2):

$$Y_{it} = \lambda_i + \sigma_t + \boldsymbol{X_{it}} \cdot \boldsymbol{\beta} + \beta_e \cdot D_{it} + \epsilon_{it}, \qquad (2)$$

where the indices i and t correspond to a census tract and a specific quarter, respectively. The outcome variable Y_{it} is either price or volume as defined in Section 4. The variable D_{it} is an indicator of the treatment status of census tract i in quarter t, and

$$D_{it} = \begin{cases} 1; & \text{if observation } i \text{ at time period } t \text{ is post intervention,} \\ 0; & \text{otherwise.} \end{cases}$$

The variable λ_i captures CT-level fixed effects, and variable σ_t captures quarter-level time fixed effects. The vector $\boldsymbol{X_{it}}$ consists of real estate variables volume (when Y_{it} is price) or price (when Y_{it} is volume); the demographic variables income level, poverty rate, and unemployment rate as defined in Section 4; and the following three additional demographic covariates:

- *Population:* number of individuals living in a census tract.
- *Density:* population density in a census tract.
- *Area:* average square footage of the real estate properties in a census tract.

We highlight several characteristics of our DID design and our TWFE estimation model. First, the treatment adoption times for all census tracts fall into the second quarter of 2018. Thus, our DID approach is not based on a staggered treatment adoption design, in which the treatment adoption times are in different periods. Furthermore, once a census tract is assigned to the treatment group, it remains in that group in the context of our analyses. Second, we apply a TWFE model because it is more suitable for a setting that involves multiple groups and multiple time periods.

We acknowledge that our estimate from Equation (2), $\hat{\beta_e}$, could be subject to potential biases if we were to shift our data aggregation from a quarterly to a monthly level as this adjustment would lead to a staggered treatment adoption design. This potential issue is discussed in Online Section EC3.1. However, as we mention above, we do not need to address this issue because we have a nonstaggered DID design in our analyses when aggregating the data quarterly. This approach not only addresses the inherent sparsity of real estate data but also aligns with the existing literature that typically aggregates data on a quarterly (or annual) level when studying the impact of the OZ program.

In our context, all the OZs are treatment units, and all the NOZs (including all the QNS tracts and NQ tracts) are control units. To improve comparability between the treatment and control groups and obtain an unbiased average treatment effect, we focus on control units that are QNS tracts. We further applied the PSM technique to select OZs and QNS tracts that are matched on observed demographics and real estate characteristics in the pretreatment period into the treatment and control groups. We included all six demographic variables in vector $\boldsymbol{X_{it}}$ and both real estate variables as covariates for matching, and we used the nearest neighbor matching algorithm. Our matched data have 1,470 OZs and 1,470 QNS tracts and are sufficiently comparable among observed characteristics. We detail the steps to implement PSM and show the comparison of several major covariates between the treatment and control groups before and after the matching procedure in Online Section A3.1.

We present below the average treatment effect estimated from the TWFE model in Equation (2) using the aforementioned matched data and present the results in Table 3, in which Model (1) uses price for Y_{it} and Model (2) uses volume for Y_{it}. Table 3 shows that the estimated coefficient for variable D_{it} is positive and statistically significant when the outcome variable is price, thus suggesting that the OZ program had a positive impact on the price per square foot in OZs. Specifically, the results show that the OZ program, on average, increased prices in OZs by 4.03%–6.13% relative to the control units. In contrast, we do not find consistent evidence

Table 3. Effect of OZ Program on Price and Volume Using Matched Data

	Model (1) Price	Model (2) Volume
ATT	4.395***	−0.065
	(3.076)	(−0.505)
Number of observations	117,282	117,282
R^2	0.009	0.003
CT fixed effects	Yes	Yes
Time fixed effects	Yes	Yes
Controls	Yes	Yes

Notes. The values in parentheses are t statistics obtained from clustering the standard errors at the CT level. We follow this convention for all tables in this section or related to this section unless noted otherwise.

$^*p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

regarding the impact of the OZ program on the transaction volume.

The parallel trends assumption is an essential condition for the validity of the DID approach. We present a formal test of the parallel trends assumption in Online Section A3.2, and our results validate that the parallel trend assumption holds in our context.

We also obtained consistent qualitative insights when using other matching criteria, such as genetic matching, and matching based on all OZs and all NOZs; the results are omitted for conciseness. Whereas matching allows us to mitigate estimation bias, we report that the DID results using the original unmatched data present the same qualitative insights as the matched data, and we detail this analysis in Online Section EC3.2.

5.1.1. Spillover-Robust DID. We previously demonstrated, using the conventional DID estimation outlined in Equation (2), that the OZ program had a statistically (and economically) significant positive effect on real estate prices but did not exhibit any statistically significant influence on the number of real estate transactions. In this section, we revisit this finding and examine how robust it is to potential spillover effects.[12]

In a quasi-experiment, researchers often use geographical boundaries to divide units of analyses into two groups: a treatment group that has access to a program or an intervention and a control group that does not. In our study, we use census tracts as geographical boundaries. Census tracts are designated as OZs or NOZs. Only investments in OZs qualify for the tax benefits offered by the OZ program. However, the effects of the program may spill over to NOZs, especially those that are close to OZs. There are two main driving forces for the spillover effects of the OZ program on NOZ areas. First, investments in OZs may make nearby NOZ areas more attractive to investors because of their proximity to OZs, leading to an underestimation of our average treatment effect size if the estimation model does not account for spillover effects. The central assumption in this context is that real estate properties become more appealing when neighboring areas are anticipated to experience an increase in infrastructure development. Second, investors often have a fixed budget to invest in a specific broad geographic area. This means that investments in OZs may come at the expense of investments in nearby NOZ areas, which could lead to an overestimation of the impact of the OZ program if we do not account for spillover effects. Therefore, we reevaluate the impact of the program by using a DID model that accounts for potential spillover effects. We note that it is not feasible to identify whether both of these opposing driving forces contribute to the spillover effects of the OZ program, but we can pinpoint the major driving force if spillover effects are found to be significant. Most importantly, we can determine whether spillover effects lead to an overall overestimation or underestimation of the true effect size, and it is also possible that spillovers may not be substantial enough to introduce a bias.

We follow the framework developed by Clarke (2017) to create a spillover-robust DID estimation by grouping control units based on their distance to treatment units. Similar to Clarke (2017), we determine proximity using geographic distances, placing each control unit into a predefined distance bin. This approach is justified by the nature of our study, in which the closer a control unit is to a treatment unit, the higher the likelihood of it being affected by spillover effects. To disentangle the spillover effects, Clarke (2017) proposed a method for estimating spillover effects by including distance bin dummy variables for a full sample of all control units in a single regression. Our approach builds on this framework, but we also use the matching algorithms to create matched subsamples of treatment and control units for each distance bin. This allows us to continuously estimate the average treatment effect across different distance bins, enabling us to obtain more unbiased results by integrating the matching technique into our analyses. Our approach consists of the following key steps:

- Step 1: We leverage the census data set that contains pairwise census tract distances to gauge the proximity of a control unit relative to the treatment units by measuring the distance between the control unit and its nearest neighboring treatment units.[13]
- Step 2: We calculate proximity quantiles, which divide the set of control units into K subsets of roughly equal sizes, designating the closest subset of control units as segment 1. For instance, in our data encompassing 11,314 QNS tracts, the proximity quantiles that divide them into three equal-sized subsets are represented by the distances $d_1 = 1.699$ miles (i.e., meaning that the first third of the control units fall within a distance between 0 and 1.699 miles) and $d_2 = 4.303$ miles (i.e., meaning that the second third of the control units fall within a distance between 1.699 and 4.303 miles). We focus on quantiles instead of defining arbitrary thresholds to ensure an equitable distribution of control units across the different segments. We then repeat steps 3–5 below for k ranging from $k = 2$ to $k = K$. This allows us to estimate the spillover effect under different distances from the treatment units.
- Step 3: We remove the segments 1 to $k - 1$ from the pool of control units in our analyses.
- Step 4: We use propensity score matching to select a group of control units from the remaining pool that is similar to the group of treatment units.
- Step 5: Using a DID model specification, we estimate the average treatment effect based on the matched treatment and control samples.

Table 4 presents the results when control units are categorized into three equal-sized segments (i.e., $K = 3$) based on their distance to the treatment units with the

Table 4. Effect of OZ Program on Price Using Matched Data from Spillover-Robust Estimation

	(1) Full sample	(2) Segment 1 removed	(3) Segments 1 and 2 removed
ATT	4.395***	5.007***	5.802**
	(3.076)	(2.238)	(2.345)
Number of observation	117,282	110,938	93,210
R^2	0.009	0.024	0.021
CT fixed effects	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes
Controls	Yes	Yes	Yes

dependent variable being the price.[14] To begin, column (3) in Table 4 shows that the impact of the OZ program on price is significantly positive even when we only consider the control units that are the farthest from the treatment units (i.e., when both segments 1 and 2 are excluded). We note that the estimated value derived from this specific subset is anticipated to be the least affected by spillover effects and is, thus, more likely to offer an unbiased estimation of the true average treatment effect size (because the control units are farthest from the treatment units).

Furthermore, in column (2) in Table 4, the estimate remains significantly positive when only segment 1 is removed. The magnitude of the coefficient consistently increases from the full-sample estimation to the subsample after excluding only segment 1 and further to the subsample after removing both segments 1 and 2. This trend strongly suggests that the spillover effect of the OZ program on price is positive. It also indicates that the primary factor contributing to the spillover effect is the enhanced attractiveness of nearby NOZ areas to investors because of their proximity to OZs. Comparing the results in Tables 3 and 4, we find that the standard DID leads to a rather conservative estimate of the OZ program's positive impact on price (i.e., an underestimate of the positive effect).

In summary, our spillover-robust estimation method effectively separates the spillover effect from the unbiased average treatment effect. As shown in Tables 3 and 4, both the standard and spillover-robust DIDs consistently indicate a significant positive average treatment effect on real estate prices in OZs. Notably, the standard DID estimation tends to provide a more conservative estimate of the positive price effect relative to the spillover-robust DID estimation, whereas the findings are qualitatively the same. We also provide a detailed analysis of spillover effects on the volume of transactions in Online Section A3.3. We find that the estimate of the average treatment effect on volume using the spillover-robust DID is the same as under the standard DID estimation (i.e., the effect is not statistically significant). To show the robustness of our results, we also conducted the analyses when using a 1:2 matching with replacement (i.e., each treated unit is matched to two control units), and we obtained qualitatively the same results as detailed in Online Section EC3.3. Thus, for conciseness, we continue to exploit the standard DID estimation as our baseline for the rest of the analyses.

5.2. Heterogeneous Treatment Effects

Our key finding from the standard DID estimation in Equation (2) shows that the average treatment effect of the OZ program on price is significantly positive. This finding has immense implications in investor responses from the real estate market post the OZ program. In this section, we further investigate whether the average treatment effect on price is heterogeneous. We consider two dimensions: (i) heterogeneity based on preexisting demographics or real estate characteristics in a census tract, and (ii) heterogeneity based on years post the treatment.

5.2.1. Heterogeneity Based on CT Characteristics. We assess whether the impact of the OZ program on price varies across census tracts by estimating the following model:

$$Y_{it} = \lambda_i + \sigma_t + \boldsymbol{X_{it}} \cdot \boldsymbol{\beta} + \beta_e \cdot D_{it} + \beta_h \cdot High_i \cdot D_{it} + \epsilon_{it}. \tag{3}$$

Equation (3) includes the indicator variable $High_i$, which is equal to one if census tract i's specific characteristic exceeds the median of this characteristic across all census tracts in the data set before the launch of the OZ program in 2018. We consider price as Y_{it}, controlling for CT- and quarter-level time fixed effects, and the vector $\boldsymbol{X_{it}}$ consists of variables median income level, poverty rate, unemployment rate, population, density, and area.

The effect of the OZ program for the low (high) type is captured by β_e ($\beta_e + \beta_h$). We also note that coefficient β_h captures the moderating effect of the census tract characteristic we consider. We highlight our findings when the census tract characteristic is price when defining $High_i$ and report the estimates of β_e and β_h in the rows "Low (base)" and "High" in column (1) in Table 5, respectively. We also consider other census tract characteristics when defining $High_i$, and we report the results in Online Section A3.4.

Column (1) in Table 5 shows that the impact of the OZ program on price is heterogeneous with respect to the property price in a census tract. The significantly positive

Table 5. Heterogeneous Effect of OZ Program on Price Using Matched Data Based on Qualified Census Tracts

	(1) CT-based heterogeneity	(2) Yearly heterogeneity
Low (base)	−11.845***	
	(−7.576)	
High	32.266***	
	(15.649)	
Year_2018_ATT		1.484
		(0.739)
Year_2019_ATT		7.038***
		(4.093)
Number of observations	117,282	117,282
CT fixed effects	Yes	Yes
Time fixed effects	Yes	Yes
Controls	Yes	Yes

estimate of β_h suggests that the positive average treatment effect on price is driven by OZs with high-end properties. Furthermore, the magnitude of the positive effect is higher for the high-end compared with the entire OZ pool (because $\beta_e + \beta_h = 20.421$ in column (1) of Table 5 is greater than $\beta_e = 4.395$ in Table 3), which indicates that the positive effect on price is more salient for high-end real estate markets.

5.2.2. Heterogeneity Based on Years Post Treatment. We assess whether the impact of the OZ program on price varies over the years posttreatment by estimating the following model:

$$Y_{it} = \lambda_i + \sigma_t + \boldsymbol{X_{it}} \cdot \boldsymbol{\beta} + \sum_{k=2018}^{2019} \beta_k \cdot year_{kt} \times D_{it} + \epsilon_{it}. \quad (4)$$

In Equation (4), we consider price as Y_{it}, controlling for CT- and quarter-level time fixed effects; $\boldsymbol{year_{kt}}$ equals one if the quarter t is in year k and zero otherwise, and hence, the yearly effect of the OZ program in the posttreatment years, 2018 and 2019, is captured by β_k. In column (2) of Table 5, we can see that the effect of the OZ program on price shows a positive sign but is not statistically significant in 2018, whereas the positive effect of the OZ program on price becomes more salient and significant in 2019. This suggests the existence of a response time from the real estate market post launching the OZ program so that the positive effect of the program on price grows stronger over time.

As a robustness check, we implement the generalized synthetic control method (as proposed by Xu 2017) to evaluate the heterogeneity in the treatment effect over time, and we find consistent results. Proposed by Abadie et al. (2010), the synthetic control method is a commonly used empirical strategy for policy impact evaluation based on constructing a synthetic control unit as comparable as possible to each treated unit (see, e.g., Abadie et al. 2015). To assess the average treatment effect in multiple posttreatment periods (i.e., years 2018 and 2019) and handle more properly multiple treatment units as in our setting, we use the generalized synthetic control method. We detail this analysis in Online Section A3.5.

5.3. Discussion of Main Results

Our empirical analysis in Section 5.1 shows that the OZ program had a positive effect on real estate prices and no statistically significant effect on the volume of transactions. We also find consistent results by using a spillover-robust estimation. Based on this finding, we infer that the OZ program had induced a positive demand shock and a negative supply shock to the real estate market, that is, the demand increased and supply decreased as a result of the OZ program. This is because, for the equilibrium price to increase and the transaction volume to remain the same, the only possible scenario is that the demand curve shifted to the right (e.g., from D1 to D2) and the supply curve shifted to the left (e.g., from S1 to S2) as illustrated in Online Figure A10. Thus, our results suggest that the OZ program spurred investors' interest to invest and has ultimately boosted demand. The decrease in supply can be explained by property owners' expectation to sell their properties at a premium as they anticipated the willingness to pay of buyers to rise because of the tax benefits offered by the OZ program.

Upon further investigation in Section 5.2.1, we find that the demand increase is more salient in the more high-end real estate market among the assigned OZs. We show this by estimating the heterogeneous treatment effect of the OZ program on price, and we find that the price increase is significantly higher in census tracts where the real estate price level is high. We also find that the positive impact of the OZ program on price grows stronger over time, which implies the existence of response time from the real estate market.

Our finding that the demand increase is more salient in the more high-end real estate market suggests that investors exhibit a cherry-picking behavior when choosing between the designated OZs: the investors primarily targeted and competed for the high-end OZs, and the low-end OZs were left behind in obtaining investment. This finding indicates that the OZ program served better the high-end OZs and raises fairness concerns. Fairness considerations are a critical element in place-based government policies such as the OZ program. Indeed, if the program served distressed communities disproportionately, it would defeat its purpose. Incorporating fairness considerations into the OZ program would enhance its social responsibility dimension by benefiting the communities that are in need and providing them with a fair treatment. Thus, we next investigate how policy makers can improve the fairness of the OZ program from a design perspective. Specifically, because the government

OZ delegation process lacks transparency, and we empirically find in Section 4 that high-end census tracts were more likely to be selected as OZs, we focus on developing a data-driven approach with fairness considerations to guide the OZ assignment.

Fairness considerations are paramount when evaluating the OZ program, and hence, it is desirable to achieve a fairer and more socially responsible OZ assignment through data-driven optimization. However, it is unclear whether a fairer OZ assignment would induce a significant economic loss because it hinders opportunistic investor behavior. Thus, we assess the economic impact of using a fairer OZ assignment. Specifically, we evaluate the potential trade-off between economic efficacy and fairness by proposing a metric motivated by our heterogeneous treatment effect analysis from Section 5.2.1.

6. Fairness-Aware Optimization

In this section, we propose a fairness-aware constrained optimization model based on an MIP to identify an OZ assignment. We further evaluate the OZ assignment derived from our optimization framework and compare it with the government OZ assignment via multiple dimensions, including implications on measures of fairness and economic benefits. As discussed in Section 2, fairness-aware models and algorithms have received considerable attention in both academia and public policy. Fundamentally, fairness is an ethical or legal question, and the views and interpretations of fairness can be different depending on the context, political environment, and moral foundations. Two key dimensions of fairness are equity and equality. "Equality suggests providing everyone with the same experience. Equity means working to overcome the historical legacy of discrimination, marginalization, and underinvestment that disadvantages specific groups of people" (See Minow 2021, p. 173, for a detailed discussion on the difference between equity and equality). In our setting, we consider both dimensions in our optimization formulation.

6.1. Constrained Optimization Formulation

We formulate the OZ assignment as a constrained optimization problem. For conciseness, hereafter we illustrate our optimization framework using the state-level analysis in which we optimize the OZ assignment for each state.[15] We relegate the discussion on county-level analysis to Online Section EC4.3.

6.1.1. OZ Assignment Optimization: MIP. We first introduce some notation for a given state: I is a set of all qualified census tracts in the state, i denotes a qualified census tract in the state such that $i \in I$, I_c is a set of qualified census tracts in the state that are contiguous to an LIC and the median family income is below 125% of its LIC neighbor, $I \setminus I_c$ is a set of qualified census tracts in the state that satisfy the LIC criteria. Let k and k_c denote the number of census tracts that are assigned to be OZs in sets I and I_c, respectively. Let x_i be the OZ assignment decision variable that corresponds to a specific census tract i in the state such that $x_i = 1$ ($x_i = 0$) if census tract i is selected (not selected) to be an OZ. Note that I, I_c, k, and k_c are inputs to our optimization problem. Finally, we formulate the OZ assignment problem as an MIP as follows:

$$\textbf{MIP}: \min_{\boldsymbol{x},\boldsymbol{\mu},\boldsymbol{\sigma}^2} \alpha_1\mu_{MI} + \alpha_2\sigma^2_{MI} + \beta_1\mu_{NPR} + \beta_2\sigma^2_{NPR}, \tag{5}$$

$$\textbf{where } \mu_{MI} = (\Sigma_{i\in I} Income_i \cdot x_i)/k, \tag{6}$$

$$\sigma^2_{MI} = (\Sigma_{i\in I}(Income_i - \mu_{MI})^2 \cdot x_i)/k, \tag{7}$$

$$\mu_{NPR} = (\Sigma_{i\in I} NonPoverty_i \cdot x_i)/k, \tag{8}$$

$$\sigma^2_{NPR} = (\Sigma_{i\in I}(NonPoverty_i - \mu_{NPR})^2 \cdot x_i)/k, \tag{9}$$

$$\textbf{subject to}: x_i \in \{0,1\}, \quad \forall i \in I, \tag{10}$$

$$\Sigma_{i\in I} x_i = k, \tag{11}$$

$$\Sigma_{i\in I_c} x_i \le k_c, \tag{12}$$

$$(\Sigma_{i\in I} IG_i x_i)/k \le IG\_Quartile, \tag{13}$$

$$(\Sigma_{i\in I} BP_i x_i)/k \ge BP\_Quartile, \tag{14}$$

where $\boldsymbol{\mu}$ (resp. $\boldsymbol{\sigma}^2$) is the vector that consists of two elements μ_{MI} (resp. σ^2_{MI}) and μ_{NPR} (resp. σ^2_{NPR}). We denote the median family income and poverty rate by *Income* and *PovertyRate* and measure them in thousands of dollars and percentages, respectively. For ease of exposition, we let *NonPoverty* denote $100 - PovertyRate$, which stands for the nonpoverty rate. We let μ_{MI} and σ_{MI} (resp. μ_{NPR} and σ_{NPR}) denote the mean and variance of the median income (resp. nonpoverty rate), during the pre-assignment period for the subset of census tracts assigned to be OZs, respectively. Formally, μ_{MI}, σ_{MI}, μ_{NPR}, and σ_{NPR} are defined by Equations (6)–(9). The parameters α_1, α_2, β_1, and β_2 in Equation (5) are normalizing factors to ensure that the objective function has a consistent unit of measurement, in which the inverse of α_1 and α_2 (resp. β_1 and β_2) are the mean and variance of the median income (nonpoverty rate) of the qualified census tracts in the state, respectively. To compute α_1, α_2, β_1, and β_2, we first use Equations (6)–(9) to compute μ_{MI}, σ^2_{MI}, μ_{NPR}, and σ^2_{NPR}, where we set x_i to be equal to one for all $i \in I$ and then take the inverse.

Our main goal is to find the optimal OZ assignment $\boldsymbol{x}$ that minimizes the objective function, which is a weighted sum of the mean and variance of the median income and nonpoverty rate computed for the census tracts that are assigned as OZs. We emphasize that we base our objective function on the census tract median family income and poverty rate to follow the guideline criteria behind the government OZ selection as described in Section 3.1. For conciseness, we relegate the detailed justifications of our objective function and constraints to Online Section A4.

6.1.2. Quadratic and Linear Reformulations. After formulating the MIP, we shift our focus to the computational aspect of solving this optimization problem. The MIP formulated above is nontractable as the objective function is a cubic function of x_i. Thus, our main goal in this section is to reformulate the aforementioned MIP into a more tractable optimization framework so that we can exploit state-of-the-art solvers. We incorporate new variables $\boldsymbol{p}$ and $\boldsymbol{q}$ into the optimization problem and impose the additional constraints in Equations (20) and (21), in which M is a sufficiently large number. It is straightforward to verify that the set of inequalities (20) ensures that $p_i = \mu_{MI}^2 x_i$ for all $i \in I$, and the set of inequalities (21) ensures that $q_i = \mu_{NPR}^2 x_i$ for all $i \in I$ given that $\boldsymbol{x}$ is a binary variable. Thus, the original MIP in Equations (5)–(14) is equivalent to the optimization problem in Equations (15)–(21), in which the latter problem is a mixed integer quadratic programming (MIQP). The reformulated MIQP is a much more tractable formulation than the original problem with the additional merit that its decision variables and constraints scale linearly with I:

$$\textbf{MIQP}: \min_{\boldsymbol{x},\boldsymbol{p},\boldsymbol{q},\boldsymbol{\mu},\boldsymbol{\sigma}^2} \alpha_1\mu_{MI} + \alpha_2\hat{\sigma}_{MI}^2 + \beta_1\mu_{NPR} + \beta_2\hat{\sigma}_{NPR}^2, \tag{15}$$

$$\textbf{where } \mu_{MI} = (\Sigma_{i\in I} Income_i \cdot x_i)/k, \tag{16}$$

$$\hat{\sigma}_{MI}^2 = (\Sigma_{i\in I} Income_i^2 x_i - 2Income_i \cdot \mu_{MI} \cdot x_i + p_i)/k, \tag{17}$$

$$\mu_{NPR} = (\Sigma_{i\in I} NonPoverty_i \cdot x_i)/k, \tag{18}$$

$$\hat{\sigma}_{NPR}^2 = (\Sigma_{i\in I} NonPoverty_i^2 x_i - 2NonPoverty_i \cdot \mu_{NPR} \cdot x_i + q_i)/k, \tag{19}$$

$$\textbf{subject to}: \boldsymbol{x} \text{ satisfies (10)–(14)},$$

$$p_i \le M\cdot x_i, p_i \le \mu_{MI}^2 + M\cdot(1-x_i), \quad p_i \ge 0, p_i \ge \mu_{MI}^2 - M\cdot(1-x_i),\ \forall i \in I, \tag{20}$$

$$q_i \le M\cdot x_i, q_i \le \mu_{NPR}^2 + M\cdot(1-x_i), q_i \ge 0, \quad q_i \ge \mu_{NPR}^2 + M\cdot(1-x_i),\ \forall i \in I. \tag{21}$$

In Online Section A5, we also show how one can represent this optimization problem as a mixed-integer linear problem. In this case, both the decision variables and the constraints scale quadratically with I.

6.2. Benefits of the Optimal Solution

We solved the MIQP from Section 6.1.2 using Python (version 3.8) with Gurobi (version 9.1.2) as the optimization engine and ran it on a 3.0-Ghz processor with 32 GB of RAM. For the state-level analysis, we ran the optimizer independently for each of the 50 states and the District of Columbia by setting the time limit to 10 minutes. The average precision (i.e., optimality gap) of our solution was satisfactory at a level of 0.15% (i.e., $\varepsilon = 0.15\%$) even though we have not solved most of the instances to optimality with this time limit. In what follows, we highlight several characteristics of the optimal assignment obtained after solving the MIQP at the state level.

First, we observe that our optimal OZ assignment significantly deviates from the government OZ assignment. The last column in Online Table EC28 shows the overlap ratio of the OZ assignment, which is defined as the number of census tracts shared between our solution and the government assignment, divided by the total number of assigned OZs. As an example, the state of Alabama has an overlap ratio of 34.8%: in total, we have 158 assigned OZs out of 1,170 qualified census tracts, and only 55 qualified census tracts are selected as OZs by both our optimization model and the state governor. Across all 50 states and the District of Columbia, the average (median) overlap ratio is 36.3% (34.8%).

Second, we focus on the objective function value in Equation (15) to evaluate the suboptimality of the OZ assignment chosen by the governors. We compute the optimality ratio by dividing the value of the objective function under our optimal assignment by the value under the government assignment. The optimality ratio can theoretically vary between zero and one, and the lower the optimality ratio, the more suboptimal the government OZ assignment. The first column in Online Table EC29 shows that the optimality ratio is less than one for 96% of all the states and the mean of the optimality ratio across all the states is 0.836.

We next evaluate the percentage difference for the mean and coefficient of variation (CV) of the median income and poverty rate and the percentage difference for the average proportion of African American population and average investment growth. CV is the ratio of the standard deviation to the mean, hence capturing the extent of variability of data points relative to the mean; the higher the CV, the greater the dispersion. The percentage difference is defined as the relative change in the value of a specific variable of interest computed over all OZs in the optimal assignment compared with the value of the variable computed over all OZs in the government assignment.

Online Table EC29 shows that the percentage difference of the mean of poverty rate is positive for 94% of the states with a mean of 41.34% (see the second column); the percentage difference for the mean of the median income is negative for 94% of the states with a mean of −20.2% (see the third column). These two observations show that the OZs selected by our optimal solution have a higher poverty rate and a lower median income than those selected by the governors, thus suggesting that our optimal assignment performs better at serving distressed communities. We also observe in Online Table EC29 that the percentage difference for the CV of poverty rate is negative for 96% of all the states with a mean value of −37.9% (see the fourth column); the percentage difference for the CV of median income is negative for 94% of the states with a mean of −25% (see the fifth column). The decrease in CV in these two observations implies that our assignment advances equality through appointing qualified census tracts of more comparable and homogeneous economic levels as OZs.

Our optimal solution also advances equity in the OZ assignment as a result of the constraints we imposed in the formulation. Online Table EC29 shows that the percentage difference for the average proportion of African American population is positive for 88% of the states with a mean of 51.6% (see the sixth column), and the percentage difference for the average investment growth is negative for 81% of the states with a mean of −23.4% (see the seventh column). These two observations suggest that the optimal OZ assignment includes census tracts with a higher fraction of African Americans and a lower investment growth compared with the government assignment.

Our qualitative insights remain largely the same when we omit these two additional fairness constraints (13) and (14). As shown in Online Tables EC30 and EC31, our objective function alone can improve fairness measured in poverty rate, median income, and proportion of African Americans, but adding these two constraints into our MIP can better fulfill the OZ program's goal of serving distressed areas (compared with the government OZ delegation), preventing opportunistic investment behavior of investors (i.e., improving fairness measured in investment growth).

6.3. Cost of Fairness

In Section 6.2, we show that our fairness-aware optimization framework achieves a fairer assignment compared with the governor's assignment. As discussed, fairness considerations are an integral part of the OZ assignment. Nonetheless, achieving a higher level of fairness can potentially jeopardize the economic advantages of the OZ program.[16] This might occur if the chosen OZs become less attractive to investors in a fairer allocation, hence leading to reduced attention from real estate investors. In this section, our main goal is to quantify the loss in terms of economic benefits because of fairness, which we refer to as the cost of fairness.

We characterize the loss in terms of economic benefits as the loss of interest from real estate investors, which is proxied by the price variable. We describe the procedure to obtain the cost of fairness metric, which we denote by C_F, as follows. First, the computation of C_F involves estimating the real estate price under both the realized governor's OZ assignment and under our optimal OZ assignment. To this end, let S denote the set of census tracts to be assigned to OZ or NOZ. For each census tract $i \in S$, our goal is to estimate its price Y_{it} at a time period post the assignment $t \in T$ (time period t is defined at the quarter level) either when it is assigned as OZ or NOZ denoted by a binary variable D_{it} (i.e., census tract i is assigned to OZ if and only if $D_{it} = 1$). We achieve this goal with the following specification:

$$Y_{it} = \lambda_t + \sigma_i + \boldsymbol{X_{it}} \cdot \boldsymbol{\beta} + \beta_e \cdot D_{it} + \sum_{k \in F} \beta_{hk} \cdot High_{ik} \cdot D_{it} + \epsilon_{it}, \tag{22}$$

where $D_{it} = Gov_{it}$ under the realized assignment by the governors and $D_{it} = Opt_{it}$ under the assignment that resulted from our fairness-aware optimization formulation. The variables λ_t and σ_i capture the time and census tract fixed effects, respectively. Then, F is the set of demographic and real estate characteristics, motivated by our heterogeneity analyses from Section 5.2.1, that can moderate the effect of the treatment on price (namely, median income, poverty rate, unemployment rate, area, density, population, price per square foot, and volume). Then, the indicator variable, $High_{ik}$, is equal to one if and only if census tract i's characteristic k exceeds the median of this characteristic across all census tracts in the data set before the launch of the OZ program in 2018. We denote the price estimate from Equation (22) as $\hat{Y}_{it}$ under the governors' OZ assignment (i.e., $D_{it} = Gov_{it}$) and $\hat{y}_{it}$ under the optimal OZ assignment (i.e., $D_{it} = Opt_{it}$). Thus, we can compute C_F as follows:

$$\begin{aligned} C_F &:= \frac{1}{|S|} \sum_{t \in T} \sum_{i \in S} (\hat{Y}_{it} - \hat{y}_{it}) \\ &= \frac{1}{|S|} \sum_{t \in T} \beta_e \cdot \sum_{i \in S} (Gov_{it} - Opt_{it}) \\ &\quad + \frac{1}{|S|} \sum_{t \in T} \sum_{k \in F} \beta_{hk} \cdot \sum_{i \in S} High_{ik} \cdot (Gov_{it} - Opt_{it}) \\ &= \frac{1}{|S|} \sum_{t \in T} \sum_{k \in F} \beta_{hk} \cdot \sum_{i \in S} High_{ik} \cdot (Gov_{it} - Opt_{it}), \end{aligned} \tag{23}$$

where the second equality follows from the fact that we have the same number of OZs under either the governors' or our optimal OZ assignments.

As mentioned, we focus on the optimal assignment obtained from our state-level analysis. After estimating the model in Equation (22) and plugging the estimates into Equation (23), we estimate the cost of fairness to be \$0.173 (i.e., $C_F = 0.173$). One interpretation of this result is that, when fairness is factored into the optimal OZ assignment, it can potentially lead to some adverse economic outcomes. However, we believe that this cost of fairness is rather marginal in our setting. First, the \$0.173 decline in real estate price per square foot is just 0.15% of the average real estate price in OZs (\$115.3). Second, compared with the 4.03%–6.13% increase in the real estate price (which equates to an increase of \$4.647–\$7.068 based on the average price) induced by the governors' OZ assignment (which was done presumably without explicit fairness considerations), the decrease of the effect size under our optimal OZ assignment with fairness considerations by \$0.173 accounts only for 3.72%–2.45% of the effect size. This ultimately suggests that striving for a fairer OZ assignment through our optimization framework would not result in a substantial reduction in economic gains.

6.4. Regression Analysis

We further statistically estimate the impact of our fairness-aware optimization framework on the investors' behavior by conducting the following regression analysis for key real estate indicators:

$$Y_i = \beta_0 + \beta_1 Optimality_i + \alpha_i + \epsilon_i, \tag{24}$$

where index i corresponds to a county. We conduct our regression analysis at the county level in order to have a higher signal-to-noise ratio.

The independent variable $Optimality_i$ in Equation (24) is the optimality ratio defined as the ratio between the value of the objective function in Equation (15) under the optimal assignment and under the government assignment. Thus, a county with a smaller value of $Optimality_i$ corresponds to the case when the government assignment was more suboptimal relative to the assignment from our optimization framework. The independent variable α_i captures state-level fixed effects.

We consider two constructs for the dependent variable Y_i. They are the mean and the interquartile range (IQR) of the intertemporal percentage difference of the following three real estate metrics computed across all the assigned OZs by governors in a specific county i:

- *Investment*: total dollar value invested in residential real estate properties.
- *Price*: average price per square foot of residential real estate based on real estate sales.
- *Volume*: total number of residential real estate sales transactions.

We define the intertemporal percentage difference of the variable of interest as the percentage change of the postdesignation two-year average level (i.e., years 2018 and 2019) relative to the predesignation two-year average level (i.e., years 2016 and 2017). Thus, the intertemporal percentage difference measures the growth of our real estate metrics for the OZs following the launch of the program. By using both the mean and the IQR, we assess the average growth and the dispersion of the growth of the real estate indicators. We choose IQR, defined as the difference between the 75th and 25th percentiles of the data, because it is a standard statistical measure of the data spread. However, our results are robust to alternative definitions of statistical dispersion (e.g., the difference between the 90th and 10th percentiles).

We estimate Equation (24) and report the results in Table 6. First, we find that all the estimated coefficients of the optimality ratio are not statistically significant when Y_i is set to the mean of the growth of real estate indicators. It, thus, shows that being fairer (i.e., closer to our optimal assignment) does not come at the expense of investors' interest. This finding is consistent with the insights obtained from the cost of fairness analyses (see Section 6.3).

Second, we find that all the estimated coefficients of the optimality ratio are negative and both statistically and economically significant when focusing on IQR as the output variable. Specifically, in counties with a smaller optimality ratio, on average, we observe a larger discrepancy in the growth of the key real estate indicators within the assigned OZs. This shows that being less fair (i.e., a larger deviation from our optimal assignment) would induce a more imbalanced growth of the real estate indicators. This imbalanced growth can be seen as evidence of investors' cherry-picking behavior. Thus, this finding demonstrates that incorporating fairness considerations in the OZ selection process can mitigate investors' cherry-picking behavior. Intuitively, when we have certain OZs that are more attractive among all assigned OZs, it is not surprising that investors will mostly invest in these more attractive OZs. This rationale is also supported by empirical evidence. For example,

Table 6. Relationship Between Optimality Ratio and Real Estate Market Indicators

	Investment	Investment	Price	Price	Volume	Volume
Mean						
Optimality	−9.672	−11.938	−0.413	−7.944	−4.375	−3.413
	(−0.985)	(−1.258)	(−0.089)	(−1.514)	(−0.838)	(−0.722)
Const	38.900***		21.566***		10.694**	
	(−4.843)		(−5.588)		(−2.552)	
R^2	0.004	0.006	0.000	0.008	0.002	0.002
IQR						
Optimality	−48.530***	−46.040***	−28.180***	−34.940***	−18.740***	−26.460***
	(−4.010)	(−2.930)	(−4.190)	(−4.810)	(−3.070)	(−3.840)
Const	79.310***		43.340***		35.220***	
	(−7.290)		(−7.010)		(−6.380)	
R^2	0.046	0.033	0.049	0.067	0.032	0.051
State fixed effect	No	Yes	No	Yes	No	Yes
Number of observations	303	303	303	303	303	303

Wiley and Nguyen (2022) show that, among designated OZs, significant premiums are concentrated in the most desirable tiers of available assets. Our findings in this section also provide a possible explanation for the empirical evidence of investors' cherry-picking behavior found in Section 5, namely, the governors had not systematically incorporated fairness in their OZ selection process.

Our result is consistent across regression models either with or without state-level fixed effects, hence enhancing the robustness of our finding. We point out that this regression analysis has two limitations: (i) it relies on a small sample size, and (ii) it uses only a two-year period to construct the intertemporal percentage difference variables. We also find that, in the regression analysis based on the formulation without fairness constraints, the qualitative insights remain the same; that is, all the estimated coefficients are insignificant when the dependent variable is the mean and are negative and statistically (as well as economically) significant when the dependent variable is the IQR as shown in Online Table EC32. Moreover, the magnitudes of all variables (beyond the price variable with fixed effects) in absolute values have decreased. This shows that we would achieve a more significant mitigation in investors' cherry-picking behavior when we reduce the deviation of the government assignment from the optimal assignment that is generated from a formulation with fairness constraints. This further demonstrates the value of including the fairness constraints in the optimization model. In summary, our results in this section underscore the value of exploiting fairness-aware optimization in identifying an OZ assignment that can excel in fairness without inducing a significant loss in terms of economic benefits.

We acknowledge that our analysis on the fairness implications of using a data-driven fairness-awareness optimization for the OZ assignment is a first step in understanding the real estate market response of a fairer OZ assignment counterfactually. To perform a rigorous counterfactual analysis, one would need to develop a structural model that formulates the investors' decision-making process (e.g., by using a discrete choice model) and estimates their value functions. We also note that there are alternative definitions of the cost of fairness. For example, Bertsimas et al. (2011) define the cost of fairness as the relative loss of system utilities when an optimization model incorporates fairness considerations. Following their framework, the cost of fairness would not rely on a counterfactual analysis of the market response, but instead measure the loss of efficiency induced by the rules imposed by the fairness scheme. We leave these as a future research avenue.

7. Discussion and Conclusion

This paper presents an attempt to evaluate the effectiveness of the OZ program in achieving its intended goals of helping distressed communities. To this end, we analyze the impact of the OZ program on the residential real estate market and investigate and optimize the OZ selection process. By leveraging large amounts of proprietary real estate data from our industry partner, we present the largest scale study of the OZ program to date to our knowledge.

We next highlight our key findings. First, we show that the demographic characteristics play a key role in the OZ selection process. Specifically, OZs have a higher poverty rate, higher unemployment rate, and lower median income relative to NOZs. Counterintuitively, however, we find that tracts with a higher average real estate price were more likely to be selected as OZs. This finding suggests a lack of fairness and room for improvement in the OZ designation process. Second, we show that the OZ program had a positive effect on the real estate price and no statistically significant effect on the volume of transactions. This provides evidence that the OZ program spurred investors' interest in investing in assigned OZs. However, we find that the demand increase is imbalanced, prompting investors to primarily target high-end areas within assigned OZs. This investor cherry-picking behavior raises fairness concerns and implies that the OZ program may have not achieved its full potential to be socially responsible under the current government assignment. Motivated by our empirical findings, we further construct a constrained optimization framework with fairness considerations to make the OZ assignment. We demonstrate the value of fairness-aware optimization for OZ designation in serving the most distressed areas and mitigating investors' cherry-picking behavior. We also show that pursuing a fairer OZ assignment may not necessarily lead to significant economic loss.

Admittedly, this research has limitations. Although our transaction data on real estate price and volume are informative and comprehensive, we acknowledge that whether these investments were made via a qualified opportunity fund (and, hence, eligible for tax benefits) is unverifiable. A second limitation is that we focus our analysis on the pre-Covid period. Future research may study the impact of the OZ program with the influence of the pandemic and based on a longer horizon. Another concern is the potential adverse impact of the OZ program on renters of vulnerable groups in OZs, who may not reap significant benefits from property value appreciation and instead experience rent increases that could lead to their displacement from these areas. In this study, we were not able to account for this aspect because of data limitations (e.g., we do not have access to the rent information). To mitigate this issue, we advocate for the implementation of regulatory measures as part of the OZ policies. These measures could include rent control mechanisms, requirements for affordable housing, or incentives for developers to create housing options that cater to renters with various income levels within OZs.

By preserving affordability, these initiatives seek to safeguard vulnerable populations from displacement because of skyrocketing living expenses.

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Endnotes

[1] See https://www.cbre.com/investor-hub/single-family-rental-housings-phenomenal-year?article=b2aec371-5623-46b7-bd2e-f16bc77ee347.

[2] According to the IRS guidelines, the expectation for substantial improvement is an investment in the property that is roughly of the same magnitude as the original sale price and must be completed within 30 months of purchase.

[3] All these requirements are for new investments (i.e., after December 31, 2017), and the business must earn more than 50% of its gross income within the OZ.

[4] For detailed entries in the federal guidelines, see https://www.irs.gov/pub/irs-drop/rp-18-16.pdf.

[5] The poverty rate should be at least 20% or the median family income should not exceed 80% of the metropolitan area median family income (New Markets Tax Credit Coalition 2019).

[6] For example, New Jersey chose tracts based on the Municipal Revitalization Index (see New Jersey Opportunity Zones Resource Center 2018). Illinois selected tracts based on a three-stage selection process that started with the needs-based indexing (Illinois Department Commerce and Economic Activity 2018). Washington allowed local entities to nominate census tracts to one of three pools (Washington Department of Commerce 2018). New York selected tracts based on recommendations from economic development councils (Empire State Development 2018).

[7] For more details, see the press release from the U.S. Department of the Treasury on February 8, 2018, and its attached guidelines (https://home.treasury.gov/news/press-release/sm0283) as well as the press releases on April 9, June 14, and October 19, 2018 (https://home.treasury.gov/news/press-releases?title=opportunity+zone&publication-start-date=&publication-end-date=).

[8] The following are the current full or partial nondisclosure states: Alaska, Idaho, Kansas, Louisiana, Mississippi, Missouri (some counties), Montana, New Mexico, North Dakota, Texas, Utah, and Wyoming (GeoData Plus 2019).

[9] Sales transactions with a price per square foot below $10 are such lower end transactions that are often non–market priced, such as transfers of deeds between family members.

[10] PSM is a widely used technique in academic research (e.g., Li and Wu 2020).

[11] To make meaningful comparisons, we run the PSM method for each state separately.

[12] We thank the anonymous reviewer for suggesting this analysis.

[13] The data set can be accessed via https://www.nber.org/research/data/tract-distance-database.

[14] The results in Table 4 are from the nearest neighborhood matching method with replacement and 1:1 matching, and the caliper is 0.005. Our insights remain consistent when using other criteria.

[15] The state-level analysis is motivated by the fact that the OZ assignment in each state was determined independently by state governors. Specifically, state governors nominated a limited number of eligible tracts for official designation. The certification and designation of an opportunity zone then comes from the secretary of the treasury via delegation of authority to the IRS.

[16] We thank the anonymous referee for suggesting this analysis.

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