The impact of Opportunity Zones on commercial investment and economic activity *

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Abstract

A provision of the Tax Cuts and Jobs Act of 2017 offered tax incentives for investing in certain low income areas in the United States called Opportunity Zones (OZs). The goal of this provision was to spur private investment in OZs in order to improve the economic outcomes of their residents. This paper evaluates the impact of OZs during their first two years of implementation, using both difference in differences and a regression discontinuity design that address the fact that OZs may have differed from non-selected areas on unobserved characteristics. Using data on the universe of all significant commercial investments in the United States, we find that OZ designation led to little or no increase in the total amount of investment or the number of investments in OZs. Using smartphone tracking data, we find only slight increases in the market share of higher quality branded restaurants. Overall, our findings suggest that the impact of OZs in their first two years have been limited.

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1 Introduction

The Tax Cuts and Jobs Act (TCJA) of 2017—the largest federal tax reform package to pass in the United States since the mid-1980s—included a provision that offered tax incentives for investing capital into certain areas called Opportunity Zones (OZs). Only areas with sufficiently low median incomes or sufficiently high poverty rates were eligible for selection as OZs. The intended purpose of this provision of the TCJA was to spur greater private investment in struggling areas and ultimately to revitalize local economies and communities (Bernstein and Hassett 2015).

The OZ provision of the TCJA comes at a time of increased attention by policymakers and academics to geographic disparity in economic wellbeing, and to place-based policies that can help people living in distressed areas. For example, Chetty et al. (2018) develop an "Opportunity Atlas" that identifies how neighborhoods help shape the long-term outcomes of children, building on research indicating that the neighborhoods in which children grow up have important consequences for long-term well-being (e.g., Chetty et al. 2016; Chyn 2018). Others have shown that the usual historical pattern of income convergence among regions has stalled or even reversed in recent decades in conjunction with reduced migration from low-income to high-income areas, suggesting that improvement in the economic conditions of neighborhoods and the people who live in them may not occur naturally (Berry and Glaeser 2005; Ganong and Shoag 2017). In light of these phenomena, Austin et al. (2018) suggest that place-based policies that target people living in distressed areas may be warranted. In light of the growing research on the importance of place, as well as the demand among policymakers for tools that focus on improving distressed areas, empirical research that identifies how various place-based policies affect economic outcomes is needed.

Our paper provides new evidence on the effectiveness of place-based policies by evaluating the impact of OZs in their initial years of existence. We estimate the impact of OZs on investment and economic activity, two indicators for which positive impacts may be particularly likely to occur in the first couple years after the policy took effect. Our primary data used to measure these outcomes include the universe of significant commercial investments from Real Capital Analytics and a quality index of restaurant and other eating establishments that we generate from SafeGraph cell phone data. We use two different identification strategies to identify the impact of OZ designation. First, we use a difference-in-differences approach that compares changes in outcomes for OZs with changes in outcomes for census tracts that were eligible but not selected as OZs by state governors. Second, we use a fuzzy multidimensional regression discontinuity approach based on the poverty rate and median income cutoffs needed to be eligible for selection as an OZ. Under both the difference-in-differences and fuzzy multidimensional regression discontinuity approaches, we find that OZs did not significantly increase investment, and led to only small increases in restaurant quality.

Our results build on research that has evaluated the impact of past tax-related provisions directed at distressed areas. Major previous federal efforts include Empowerment Zones and the New Markets Tax Credit.

Empowerment Zone programs generally offered tax incentives for businesses that locate in specified areas or which hire employees who live in such areas. A number of states had their own Empowerment Zone programs before the federal government's Empowerment Zones and Enterprise Communities Act of 1993. Evidence on the effectiveness of Empowerment Zones has been mixed. Based on California's state-based program, O'Keefe (2004) found significant gains in employment resulting from Enterprise Zone designation, while Neumark and Kolko (2010) found no such effects when controlling for changes in zone borders over time. Focusing on other states in addition to California, Bondonio and Engberg (2000) and

Greenbaum and Engberg (2000) similarly find no effect on employment, with the latter also finding no effect on housing prices or home occupancy rates. Neumark and Young (2021) find no evidence of long-term impacts of state programs or impacts that are stronger for certain state programs. The evidence on the federal Empowerment Zones program is more positive. Ham et al. (2011) find positive effects for Empowerment Zones and Enterprise Communities, in addition to smaller effects for state-based programs. Busso et al. (2013) find that the federal Empowerment Zone program increased employment and wages of the people living in the zones, and that the program was an efficient use of funds.

Another major federal initiative intended to spur economic activity in distressed areas is the New Markets Tax Credit, a component of the Community Renewal Tax Relief Act of 2000. Like the OZ provision, the New Markets Tax Credit focuses on census tracts that have relatively low incomes and high poverty rates, and offers tax incentives for investment made in these areas. However, unlike the OZ provision, investments must be pre-approved by public authorities. Gurley-Calvez et al. (2009) find that the New Markets Tax Credit leads to additional investment that would not have otherwise been made, and Abravanel et al. (2013) similarly find that the program increased private investment while also encouraging public investment in designated areas. In terms of outcomes, Freedman (2012) finds modest impacts on increasing home values and reducing both poverty and unemployment, although some of the effect may be due to a changing population over time. Harger and Ross (2016) find that employment in retail and manufacturing sectors increased in eligible areas, while employment in other sectors decreased.

There are reasons to believe that Opportunity Zones could have different impacts than those found for previous efforts. First, much of the existing research is based on experiences during the 1990s or earlier (though some more recent evidence on the New Markets Tax

Credit studies the early 2000s), and so evidence on effectiveness decades later is needed. Second, OZs are focused on attracting private capital into distressed areas, as opposed to subsidizing employment, which is the focus of the most frequently studied programs. Third, OZ rules provide wide flexibility in terms of the type of investment and does not cap the amount of investment that is subject to tax-preferred treatment. Our results suggest that in their initial years of operation, OZs did not significantly increase investment, suggesting that the flexibility of OZ rules may have ultimately led to subsidization of investment that would have occurred anyway. While more research will be needed to identify impacts of OZs on resident outcomes in the longer term, the lack of an initial investment effect suggests that substantial impacts may be less likely.

In addition to building on the existing literature on previous place-based policies, our paper contributes to a small but growing literature evaluating the impact of OZs on various tract level outcomes. These studies have focused on real estate prices and employment outcomes.

Four studies have estimated the impact of OZs on real estate prices. Each uses a difference-in-differences methodology that compares changes in outcomes in OZs compared to eligible but not selected OZs, although Sage et al. (2019) uses propensity score matching to identify similar tracts and Chen et al. (2019) use geographic neighbors in some specifications. Sage et al. (2019) use commercial investment data and find that OZ designation increased prices for vacant land and redevelopment properties but not for existing properties, which they interpret as evidence that OZ benefits will simply be capitalized into higher prices without spurring additional investment. The remaining three studies focus on residential home prices. Using property-level transaction data from Zillow, Casey (2019) finds early and large home price impacts. By contrast and using a more sophisticated design to control for the types of properties sold, Chen et al. (2019) estimate little effect on home prices through the end of 2018 using repeat sales data from the Federal Housing Finance Agency. Council of Economic Advisers (2020) extends the Chen et al. (2019) data through the end of 2019 and find a modest effect of OZs on home prices.

Two studies have focused on the impact of OZs on employment outcomes, each using a difference in differences methodology. Arefeva et al. (2020) find that OZ designation increased employment growth by 2 to 4 percentage points using private tract-level data on employment. Atkins et al. (2020) use zip code level data on job postings and salary postings, finding that zip codes with OZs have fewer job postings and higher posted salaries than similar zip codes without OZs, but effects are small in magnitude and not consistently statistically significant.

Our paper is unique in focusing on the impact of OZs on both commercial investment, which is likely a necessary condition for downstream impacts on labor markets and other improvements in well-being of OZ residents, and economic development. We also employ a more holistic set of identification approaches than previous studies by considering both difference-in-differences and fuzzy multidimensional regression discontinuity approaches. These distinct approaches rely on different identifying assumptions and thus can help validate difference-in-differences results used in other studies. Further, we note that difference-in-differences estimates comparing OZs to eligible but not selected tracts may be biased in an unknown direction because the New Markets Tax Credit program began using the same eligibility criteria as that used for OZs starting at the end of 2017. Depending on whether the New Markets Tax Credit spurred investment and economic activity more or less in OZs compared with non-selected but eligible tracts, estimated impacts using a difference-in-differences approach could be either upward or downward biased. In contrast, regression discontinuity estimates are biased upwards because tracts eligible for OZs were also eligible for New Markets Tax Credit investment. Thus, supplementing difference-in-difference estimates with regression discontinuity estimates can improve our understanding of the true impact of OZs.

Using a difference in differences approach, we find that commercial investment—measured as the probability of any investment, the number of investments, or the total dollar amount of all investments—did not significantly increase in response to OZ designation, although as noted above, these estimates could potentially suffer from some unknown amount of bias due to the New Markets Tax Credit adopting the same eligibility criteria as those used for OZs starting in 2018. We next consider a multidimensional regression discontinuity approach that exploits the eligibility cutoff based on a census tract's poverty rate and median income level relative to that of the metropolitan area or state overall. We find that the probability of OZ selection increases sharply at the eligibility cutoff point over a constructed variable that combines the poverty rate and median income conditions. Our fuzzy regression discontinuity estimates imply that OZ designation had little or no impact on investment for tracts near the cutoff point. Since the confluence of the New Markets Tax Credit leads to upward bias of the estimated impact of OZs, our results suggest that OZs had little or no impact on investment. We apply the same two empirical approaches—difference-in-differences and regression discontinuity—to our measure of economic activity using smartphone tracking data and find a small positive effect.

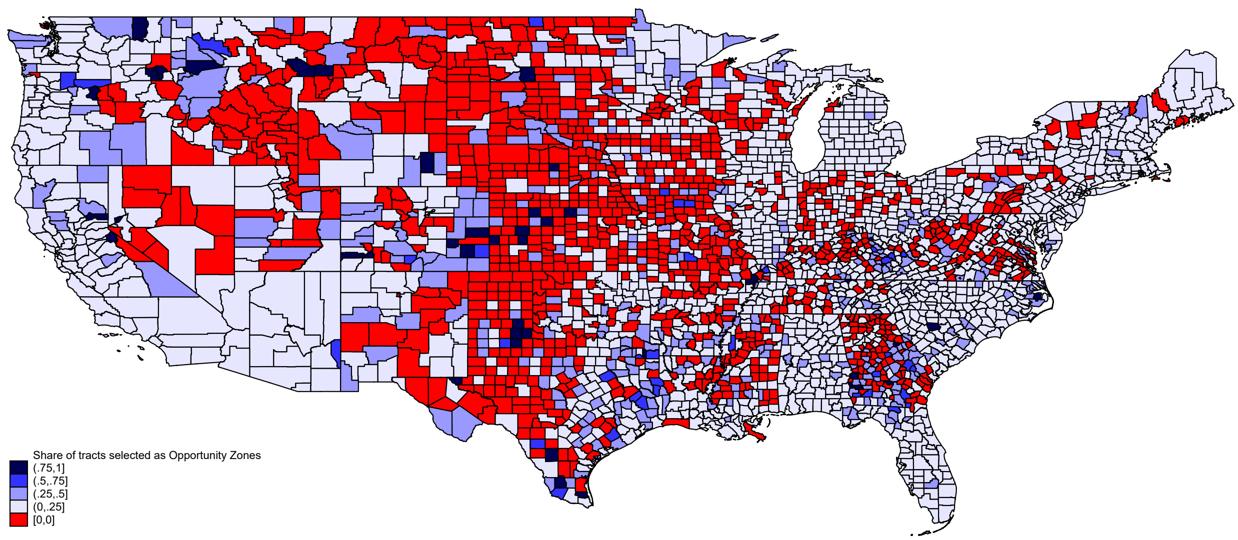
The paper proceeds as follows: Section 2 provides background on OZs, including how they were selected, investment rules, and the tax benefits. Section 3 describes the American Community Survey data that underlies OZ eligibility determination, as well as the private data sources used to construct tract-level outcomes. Section 4 describes our methodology for identifying the impact of OZs on tract-level outcomes. Section 5 presents results. Section 6 discusses results and policy implications. Section 7 concludes.

2 Opportunity Zones Background

The OZ provision of the TCJA allowed each state governor to designate up to 25 percent of eligible census tracts as OZs. The final list of designated OZs was officially published by the U.S. Treasury on July 9, 2018, although states' designations were often (publicly) made earlier in the year. Census tracts are designed to contain 1,200 to 8,000 residents, and so census tracts range in geographic area from the size of a neighborhood in densely populated parts of cities to much larger areas in rural parts of states. There are approximately 75,000 total census tracts in the United States. Of those, just over 42,000 were eligible to be OZs, and just over 8,700 were actually designated as OZs. Thus all U.S. census tracts fall into one of three groups: (1) not eligible, (2) eligible and not chosen and (3) eligible and chosen. Figure 1 shows a map of all counties in the United States, shaded based on the share of census tracts within the county that were selected as OZs. All states and two-thirds of counties have at least one census tract selected as an OZ.

In order for a census tract to be eligible for selection as an OZ, it was required to either (a) have an official poverty rate of at least 20 percent, (b) have a median income below 80 percent of the median income in the state or metropolitan area, or (c) be contiguous with a census tract meeting one of the conditions in (a) or (b) and have a median income less than 125 percent of the qualifying census tract. Because eligibility is essentially defined by the two dimensions of poverty and income (ignoring the contiguity criterion), we can visualize the eligibility of all census tracts by plotting each tract according to its poverty rate and median income. Notably, this also motivates our regression discontinuity design discussed

Figure 1: Share of census tracts designated as Opportunity Zones by county in the continental United States



Source: U.S. Department of the Treasury

further in Section 4. In this light, Figure 2 presents four plots where each dot represents a single census tract.[1] The horizontal axis represents the census tract poverty rate. All census tracts to the right of 20 percent are eligible to potentially be OZs because they meet the poverty rate criterion. The vertical axis represents the percent difference between census tract median income and 80 percent of state or MSA median income (whichever is applicable for each census tract). Hence, the red line at zero reflects the point where the census tract median income exactly equals 80 percent of state/MSA median income. All census tracts below this red line are eligible to potentially be OZs because they meet the median income criterion.

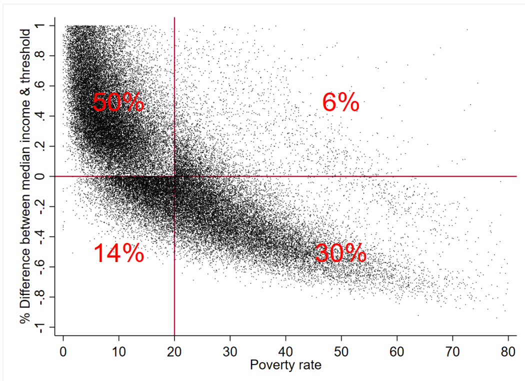
Thus, in Figure 2 panel (a), the 50 percent of census tracts in the top-left quadrant are ineligible, while the remaining 50 percent of census tracts in any of the other three quadrants

[1]For clarity in the figures, we drop the census tracts that qualify based upon requirement (c).

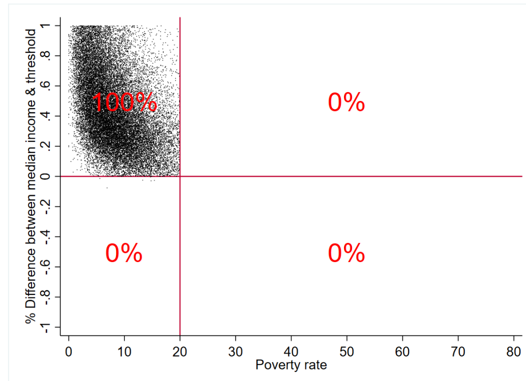
are eligible because they meet at least one of the poverty rate or median income criteria. Panel (b) illustrates the subset of census tracts that were ineligible—census tracts with less than a 20 percent poverty rate and census tract median income above 80 percent of the MSA or state median. Panel (c) illustrates the distribution of eligible but not selected census tracts. Panel (d) illustrates the distribution of selected census tracts. As these panels show, the majority of eligible census tracts qualify based on both eligibility dimensions, although 68 percent of selected census tracts are eligible on both dimensions compared to 56 percent of eligible but not selected census tracts. This suggests that governors may have selected census tracts that were relatively more economically disadvantaged among all eligible census tracts. Note that a small share of eligible census tracts are in the top-left quadrant, due to the additional eligibility criteria such as qualifying based on the 2012-2016 pooled American Community Survey data, as opposed to the 2011-2015 based values shown in the figures.

Months after OZs were offically designated and confirmed by Treasury, on October 19, 2018 the U.S. Treasury released a preliminary rule providing guidance to investors for how the OZ provision would function. Those who invest unrealized capital gains in OZs, via so-called Opportunity Funds, are able to defer any taxes owed on those capital gains for as long as the investment remains in the Opportunity Fund (through the end of 2026). If the investment remains in the Opportunity Fund for at least 5 years, then 10 percent of the original capital gain is excluded from taxation, and if the investment remains for at least 7 years, then 15 percent of the original capital gain is excluded from taxation. After 2026, the capital gains must be realized and the appropriate portion subject to taxation. Furthermore, any capital gains accrued based on the investment in the Opportunity Fund (above the original capital gain) are not subject to any taxation if the investment in the Opportunity Fund is maintained for at least 10 years.

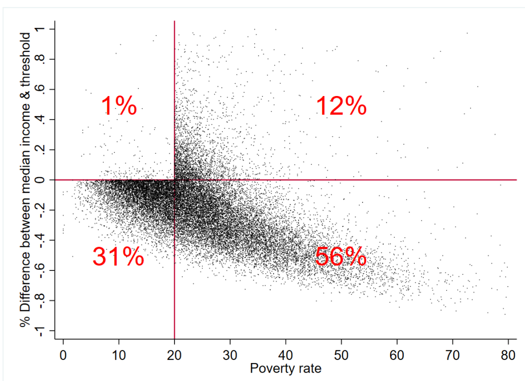
Figure 2: Poverty rate and percent difference between median income and threshold, by census tract type



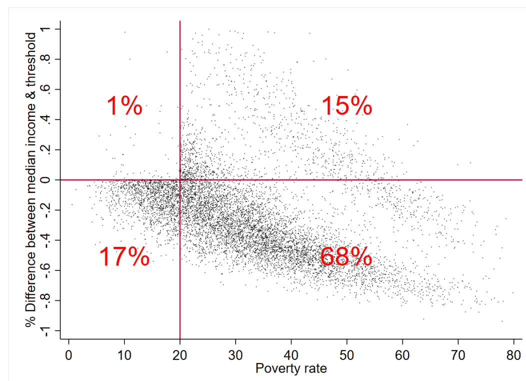
(a) All census tracts



(b) Ineligible census tracts



(c) Eligible but not selected census tracts



(d) Selected census tracts

Sources: American Community Survey, 2011-2015 5-year pooled sample; U.S. Department of the Treasury.
Notes: Tracts eligible based only on contiguity with eligible tracts are excluded from the figures. Some eligible and selected tracts are found in the top-left quadrant of the figures because they may have been eligible based on the 2012-2016 ACS data.

Individuals can invest an uncapped amount of funds into Opportunity Funds, and Opportunity Funds can invest an uncapped amount of funds into one or multiple OZs, across business and residential activities. Investors are simply required to declare the amount of capital gains invested into Opportunity Funds to the Internal Revenue Service when filing their taxes.[2] According to U.S. Treasury rules, in order for a business to qualify as being

[2]Qualified Opportunity Funds are required to report the amount of investment in each census tract using

in an OZ, it must have at least 70 percent of its property located in OZs (potentially more than one).

Opportunity Zones represent the first place-based policy that allows uncapped private investment into areas throughout the United States.[3] The OZ provision is also fairly broad in terms of the type of investments that receive preferential tax treatment and are untied to any particular outcome variable, such as employment, like many other previous efforts. Thus, the success (or lack thereof) of this policy will be instrumental in designing future policies not only in the U.S. but internationally as well.

3 Data

The major data sources we use are (i) tract level data from the American Community Survey (ACS) which were used to define eligibility for OZ designation, (ii) transaction-level investment data from Real Capital Analytics, and (iii) smartphone tracking data from SafeGraph related to economic activity. These data sources are used for causal identification and to measure tract-level outcomes.

The ACS is an annual household survey produced by the United States Census Bureau. It samples about 2 million households per year in addition to people living in group quarters. The relatively large sample size allows Census to produce statistics at detailed geographic levels, especially when combining multiple survey years. Particularly important for our purposes, Census publishes census tract level poverty rates and median family income based on 5-year pooled samples of the ACS. These published poverty rates and median family income estimates were used to determine eligibility for OZ selection. Tracts could meet

IRS form 8996, beginning in tax year 2019.

[3] The most closely related effort, the New Markets Tax Credit, requires pre-approval and caps funds invested in designated areas, which are less evenly distributed throughout the country.

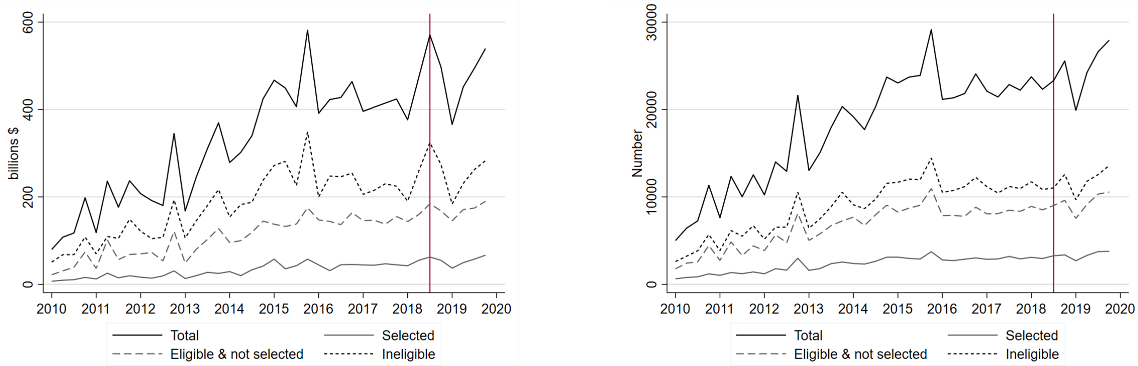
eligibility standards based on the 2011-2015 pooled sample or the later released 2012-2016 pooled sample, although in practice only 49 census tracts selected as OZs (out of over 8,700 selected OZs) were eligible on the basis of the 2012-2016 ACS but not the 2011-2015 ACS (see Internal Revenue Service 2018).

Our outcome data are assembled from comprehensive and up-to-date private data sources. While government collected data have important advantages, sources like the Census County Business Patterns dataset and the ACS are significantly lagged and are not necessarily available at the census tract level without combining survey years.

To measure investment we use Real Capital Analytics' (RCA) commercial investment database that contains transaction level data for the entire United States on commercial investments valued at over $2.5 million from 2010 through 2020 and a subset of transactions below that threshold.[4] RCA covers about 95% of all commercial real estate transactions above this threshold in the United States. The data contain numerous details on each transaction, such as price, age of structure, type of transaction (e.g., new construction or sale of existing structure), address, buyer objectives, buyer and seller information, and many details on financing of the loans. We aggregate investments to the census tract level, focusing on outcomes such as number of transactions and their prices. Figure 3 shows annualized quarterly aggregates of the number of dollars invested and the number of investments, by each type of census tract. In total, $540 billion over just under 28,000 investment transactions were made in the last quarter of 2019 on an annualized basis. Investment trended upward until 2015 before levelling off. Unsurprisingly, there was less total investment in selected OZs, since there are only about 8,700 OZs compared to over 30,000 each of ineligible tracts

[4]Once a property sells for $2.5 million it will stay in the database, even if it sells again in the future below this threshold. In addition, RCA backfills transaction prices, if possible, once a property hits $2.5 million threshold. Thus the data are not "truncated".

Figure 3: Annualized dollars and number of investments per quarter by tract type, 2010-2019



(a) Dollars of investment

(b) Number of investments

Sources: American Community Survey 2011-2015 5-year pooled sample; Real Capital Analytics; U.S. Department of the Treasury.
Notes: All dollars values are adjusted for inflation using the Personal Consumption Expenditures price index. Vertical line is drawn at 2018 Q3, the first quarter after which OZ designations were certified by the U.S. Treasury.

and eligible and not selected tracts.

We track economic development using SafeGraph data. SafeGraph collects anonymous location data from millions of Americans' smartphones. The data indicate the amount of time people spend at home and outside the home, how far they travel and the exact types of establishments they visit. We use these data to infer visits to restaurants and other establishments. In particular, we generate a "quality" index in order to measure improvements in restaurant quality in the years after OZ designation.

Table 1 presents summary statistics related to census tract characteristics and our outcome variables for each of the three groups of census tracts. By design, census tracts that were not eligible are better off economically than eligible census tracts. It is also clear from Table 1 that among the eligible census tracts, states chose tracts that are, on average, more distressed, with lower incomes, higher poverty rates, higher unemployment rates, lower rates of labor force participation and lower levels of educational attainment. This suggests that simply comparing outcomes in OZs to outcomes in eligible census tracts that were not chosen is likely to confound causal impacts of OZ designation with non-random selection.

Consistent with differences in community characteristics, selected tracts generally performed worse on outcome measures prior to designation. While selected tracts were more likely to receive commercial investment than eligible but not selected tracts, the prices were lower in raw terms and when adjusting for the size of a property. Notably, industrial investments comprised 40.9 percent of all commercial investments in selected tracts, substantially higher than the share in other types of tracts.

The final panel of Table 1 presents the average restaurant quality by tract type.[5] We focus on restaurants for a number of reasons. The primary reason is that restaurants, cafes and other eating establishments represent by far the largest category of establishments (out of 170 in total) in the SafeGraph data—comprising around 14 percent of all visits. Second, many restaurants have a national, regional or at least a state presence that provides variation in the census tract category (selected, eligible but not selected and ineligible) that allows us to generate quality weights. Unsurprisingly, as Table 1 shows, selected tracts have overall lower quality restaurants compared to eligible and ineligible census tracts.

4 Research Design and Methods

TCJA allowed state governors to designate a subset of eligible low-income or high poverty census tracts as OZs. While it appears that many states approached the selection process in a systematic way (Frank et al. 2020), many of the selected tracts were chosen based upon idiosyncratic factors that are unobservable to the econometrician. Multiple factors entered into the governor's selection criteria. In some states, governors sought geographic balance in

[5]See Appendix A for a description as to how these weights are generated.

Table 1: Summary Statistics

	Eligible		Not Eligible
	Selected	Not Selected	
Tract characteristics (American Community Survey)			
Median Income ($)	39,071	51,258	94,153
	(15,248)	(17,287)	(31,435)
Poverty Rate	32.51	21.66	7.58
	(14.5)	(11.9)	(4.4)
Unemployment Rate	14.4	10.3	6.1
	(8.1)	(5.9)	(3.2)
Labor Force Participation Rate	57.1	61.3	66.1
	(11.1)	(10.1)	(9.8)
Education			
Less than high school	.21	.16	.06
	(.12)	(.11)	(.05)
High school	.32	.31	.22
	(.09)	(.10)	(.11)
Some College	.29	.31	.30
	(.08)	(.09)	(.09)
College	.18	.21	.42
	(.13)	(.15)	(.19)
Investment statistics (Real Capital Analytics)			
At least one transaction (%)	41.3	35.5	43.5
Mean number of transactions	1.2	0.9	1.3
Mean building age	36.3	31.7	29.0
	(35.6)	(28.9)	(28.5)
Median Census Tract Level Price ($000)	4118.5	4575.0	5500.0
Median Price/sq ft, $	101.5	122.0	150.0
Property Type (%)			
Industrial	40.9	33.8	27.4
Office	26.8	26.0	33.1
Retail	32.3	40.2	39.5
Restaurant Quality (Safegraph)			
Quality Index	0.507	0.533	0.572
	(0.134)	(0.137)	(0.148)
Number of tracts	8,762	33,415	31,951

Sources: American Community Survey, 2011-2015, 5-year pooled sample; Real Capital Analytics, 2011-2015; Safegraph, Jan - Jun 2018; U.S. Department of the Treasury.

their selections, while in others a balance between rural and urban tracts was given priority. In yet other states, governors held a multi-step process whereby citizens could weigh in or preference was given to regions that were already previously designated as high priority areas and so were natural choices for OZ designation. In sum, once eligible, governors were given significant leeway in the exact tracts chosen based upon the goals of the state and how best to use the legislation to their advantage.[6]

As such, we take seriously the fact that governors did not select OZs randomly. It is clear that the observable characteristics of census tracts chosen by governors differ from census tracts that were not chosen, as seen in Table 1. Trends prior to OZ designation may differ as well, with Frank et al. (2020) finding that selected tracts were experiencing faster economic growth than eligible but not selected tracts prior to selection. While we can adjust for observable differences between selected and non-selected tracts, they presumably differ on unobservable factors as well. To the extent that the levels of unobservable factors are correlated with outcomes of interest, a simple comparison of the outcomes of OZs with those of other census tracts will not identify the causal effect of OZ designation. We take two approaches to resolve this identification problem.

4.1 Difference-in-Differences

We first use a difference-in-differences approach, comparing the changes in outcomes for tracts designated as OZs with the changes in outcomes for tracts eligible but not designated

[6] While difficult to quantify, because governors were given such flexibility in OZ designation, this has lead to some accusations of corruption in the OZ designation process. While this is certainly a concern for who benefits from the tax preferential investments, it is not obvious that this biases our estimates on outcomes. From the econometrician's point of view, OZ selection is problematic when the selection is based upon unobservable characteristics that impact our outcome variables, for example, potential for economic growth. If, instead, preferences for OZ designation are given to particular census tracts in order to benefit individuals based upon their political connections, then as long as these choices are orthogonal to the unobservable characteristics of concern, we can treat these choices as good as random.

as OZs. Taking as our sample only those census tracts that were eligible to potentially be selected as OZs, our baseline model is as follows:

$$y_{i,s,t} = \delta_i + \gamma_{s,t} + \beta D_{i,t} + \epsilon_{i,s,t} \tag{1}$$

for each census tract i in state s during quarter t. $y_{i,s,t}$ is the outcome of interest. δ_i is a time-invariant tract fixed effect, and $\gamma_{s,t}$ is a state by quarter interacted effect. Thus, we only rely on differential changes in outcomes among selected tracts in a state compared to eligible but not selected tracts in the same state to identify the impacts of OZs on outcomes. $D_{i,t}$ is an indicator variable equal to 1 if census tract i was selected as an OZ and quarter t is the quarter on or after the OZ policy took effect (which we take as the third quarter of 2018). The coefficient β represents the treatment effect of OZ designation. $\epsilon_{i,s,t}$ is the error term.

An important condition for the difference-in-differences approach to identify the causal impact of OZ selection is that pre-trends in each outcome variable are the same in selected and non-selected, though eligible, tracts. While we examine this assumption more rigorously in the context of our outcome variables in the next section, it is instructive to compare trends in other readily available measures of economic conditions in each type of tract. Figure 4a shows trends in median income and Figure 4b shows trends in poverty in selected and eligible but not selected census tracts, using 5-year samples of the ACS ending in the year shown, and with all values indexed to their 2010 level. While the levels of median income and poverty for selected and eligible but not selected census tracts differ, their trends since 2010 are similar. While we do not observe median income and poverty in years post-OZ designation (with the exception of a portion of the 2014-2018 and 2015-2019 pooled samples), and thus cannot reasonably estimate the impact on these variables post-OZ designation, these results suggest that the difference-in-differences approach could potentially provide unbiased causal estimates for other outcomes.

4.2 Regression Discontinuity

In order to complement the difference-in-differences approach, we additionally implement a multivariate regression discontinuity (RD) approach that does not rely on similar pre-trends in selected versus eligible but not selected tracts. The RD approach takes advantage of how eligibility of census tracts was determined, creating a natural experiment that assigned eligibility to some census tracts but not others based on arbitrary factors unrelated to outcomes of interest. While causal identification is stronger in the RD approach, we are generally limited to estimating treatment effects for specific subsets of OZs.

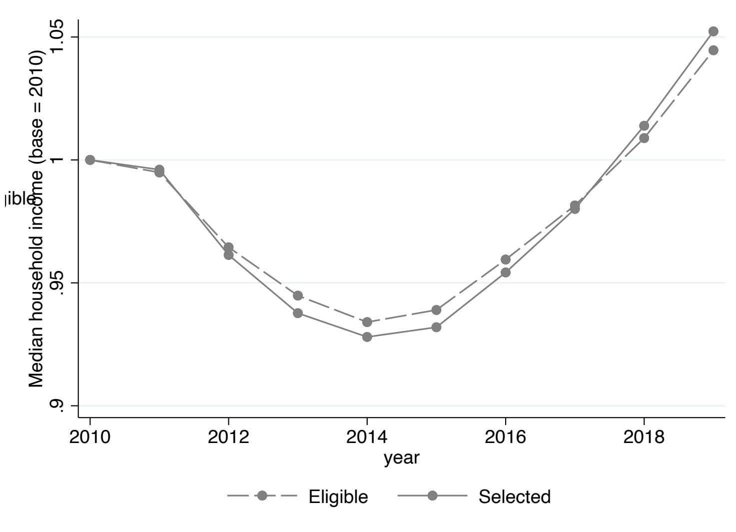
The RD design relies on the qualification criteria for OZs—as noted earlier, census tracts were deemed eligible for OZ designation if they met at least one of the following conditions:

- had a poverty rate of at least 20 percent, or
- had a median family income below 80 percent of either the state median family income or the MSA median family income, or
- were contiguous with a selected tract based upon the first two qualifications and with a median family income that does not exceed 125 percent of the median family income of at least one contiguous selected tract.[7]

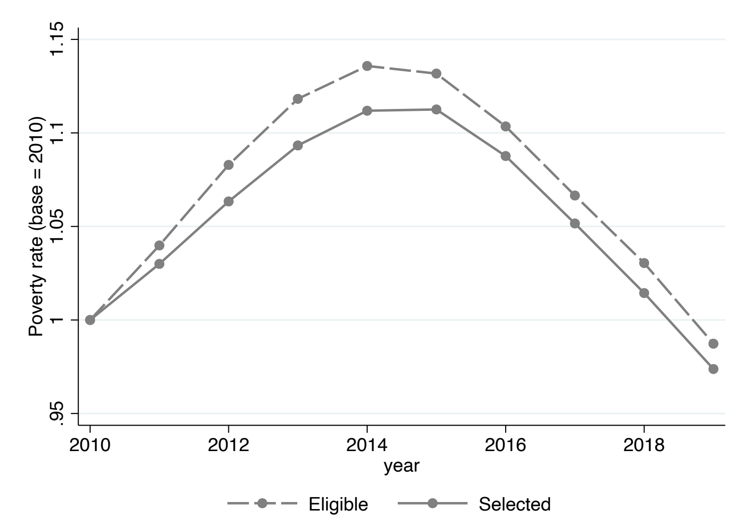
In the RD design, we exploit the sharp poverty rate and income eligibility cutoffs. Census tracts with poverty rates just below 20 percent or median family income just above 80 percent

[7] Contiguous tracts account for nearly 25 percent of eligible tracts but were less than 3 percent of those actually selected.

Figure 4: Poverty rate and median income trends, 2010 - 2019



(a) Mean of census tract median household income indexed to 2010, by census tract type



(b) Mean of census tract poverty rate indexed to 2010, by census tract type

Sources: American Community Survey, 2006-2010; 2007-2011; 2008-2012; 2009-2013; 2010-2014; 2011-2015; 2012-2016; 2013-2017; 2014-2018; 2015-2019 5-year pooled sample; U.S. Department of the Treasury.

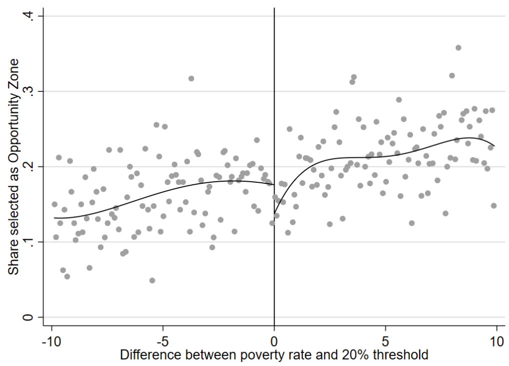
of the threshold are arguably similar to census tracts that fall near but on the opposite side of the relevant threshold. Since in general only the latter tracts were eligible to be designated as OZs, we can estimate the treatment effect of OZ eligibility by comparing outcomes (or changes in outcomes) of tracts just below the cutoff to those just above ("intent to treat"). Furthermore, we can scale the effect upward in a fuzzy RD design based on the fact that only some eligible tracts were actually designated as OZs ("treatment effect on the treated").

Because eligibility is conditioned on both the poverty rate and median income relative to the MSA/state median, we rely on regression discontinuity approaches that incorporate multiple running variables. Reardon and Robinson (2012) suggest several such approaches, which have been used frequently in education-related research.[8] One approach we adopt is to estimate separate specifications for each running variable, under the "frontier regression discontinuity" approach. For example, we focus first on the 20 percent poverty rate. In census tracts with median income above the 80 percent threshold, passing the 20 percent poverty rate threshold moves a census tract from ineligible to eligible. In contrast, if the census tract has median income below 80 percent of the threshold, passing the 20 percent poverty rate has no impact on OZ eligibility because being below the 80 percent income threshold, by definition, deems the census tract eligible. Taking these institutional details into account of how census tracts are deemed eligible allows us to further push the analysis to estimate the impact in passing the 20 percent threshold. While, a priori, there should be no impact on our outcome variables when passing the 20 percent threshold for the latter case, we can use census tracts with median income above the 80 percent threshold as a control group to those below.

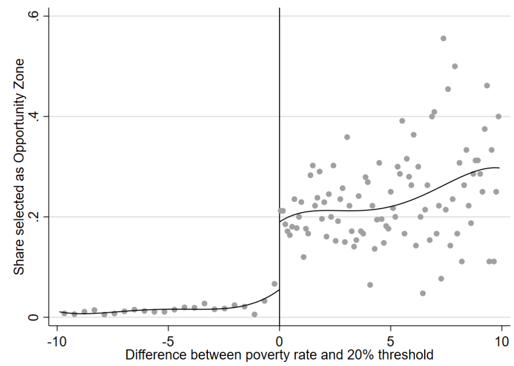
This is illustrated in Figure 5. Panel (a) considers only census tracts with median incomes

[8] Also see for example Wong et al. (2013).

Figure 5: Share of tracts designated as Opportunity Zones by difference between poverty rate and threshold



(a) Tracts with median income below 80% of MSA/state median income

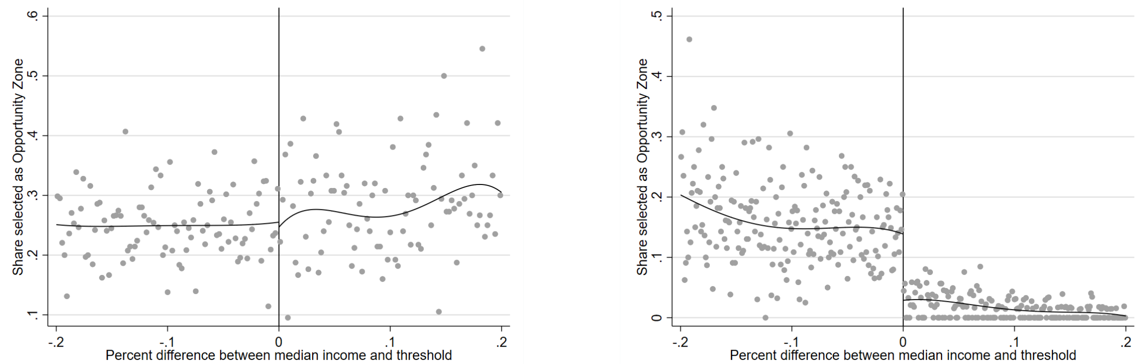


(b) Tracts with median income above 80% of MSA/state median income

Sources: American Community Survey 2011-2015 5-year pooled sample; U.S. Department of the Treasury.
Notes: Each dot represents the sample average within each bin. Fitted lines are based on a polynomial of degree 4 fitted separately to points on either side of the cutoff.

below 80 percent of the MSA or state median. Groups of census tracts with approximately equal poverty rates are placed in bins, and we calculate the share that were selected as OZs. Poverty rates and implied probabilities of selection are plotted for each bin. As seen in the figure, census tracts with poverty rates above 20 percent (normalized to zero in the figures) are not more likely to be selected because they already satisfy the income condition for eligibility. Thus, these tracts are not useful in identifying the impact of OZ designation. Panel (b) considers only census tracts with median incomes above 80 percent of the MSA or state median. In this case, crossing over the 20 percent poverty rate threshold substantially increases the probability of being designated an OZ. Census tracts with a slightly higher or slightly lower poverty rate than 20 percent are economically very similar, and so differences in outcomes between these two groups of tracts can be attributed to selection as an OZ, rather than other differences.

Figure 6: Share of tracts designated as Opportunity Zones by difference between median income and threshold



(a) Tracts with poverty rate above 20%

(b) Tracts with poverty rate below 20%

Sources: American Community Survey 2011-2015 5-year pooled sample; U.S. Department of the Treasury.
Notes: Each dot represents the sample average within each bin. Fitted lines are based on a polynomial of degree 4 fitted separately to points on either side of the cutoff.

Likewise, we can flip the analysis to estimate the impact of OZ eligibility by dividing our sample into those census tracts that are above or below the 20 percent poverty rate and then using the 80 percent income threshold as the eligibility determinant. As shown in Figure 6, passing the income threshold has no impact on OZ eligibility in census tracts with a poverty rate above 20 percent—census tracts on both sides of the income threshold are eligible. However, in census tracts with a poverty rate below 20 percent, passing from just above the income threshold to just below substantially increases the probability of being selected as an OZ.[9]

A third approach is to consider all census tracts but combine the poverty and income variables into a single running variable with a single cutoff point for eligibility. In particular

[9]We should note that the probability of being selected isn't exactly zero when neither of the poverty nor median income thresholds hold for two primary reasons: (1) the data include contiguous tracts and the eligibilty requirements are slightly different as described above and (2) a small number of census tracts used the 2012-16 ACS and their poverty and/or median income slightly differs from that of the 2011-15 ACS.

we construct the running variable r:

$$r_{i,m} \equiv \max\{\frac{P_i - 20}{20}, -\frac{I_i - 0.8 * I_m}{0.8 * I_m}\} \tag{2}$$

where P_i is the poverty rate and I_i is the median income in census tract i, and I_m is the median income in MSA or state m.

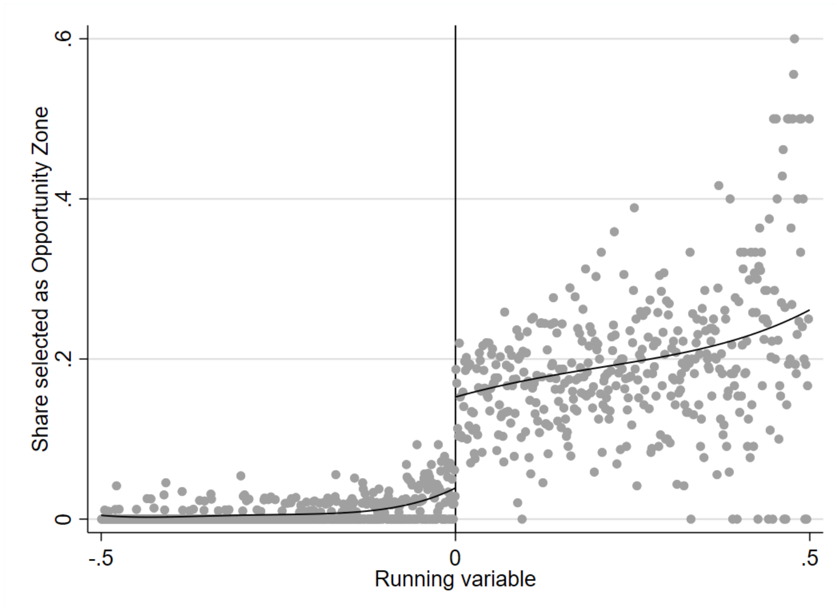
Figure 7 shows the probability of selection as an OZ on both sides of the cutoff point for eligibility as determined by the running variable as defined above. Census tracts just above the cutoff point are substantially more likely to be selected as an OZ as tracts just below the cutoff point. The probabilities are not zero below the cutoff point due to the possibility of being classified as eligible on the basis of being contiguous to a selected census tract and having a median income no higher than 125% of the contiguous tract's median income. The probabilities are not one above the cutoff point because not all eligible tracts were selected as OZs.

Table 2 shows first stage results using the three potential running variables—poverty, income, or a combination—and predicts the probability of being selected as an OZ depending on a flexible polynomial below the cutoff, a flexible polynomial above the cutoff, and a dummy variable for the cutoff itself. Results confirm the graphical evidence of a substantial impact of crossing the threshold on the probability of selection. Tracts are between 11 and 16 percentage points more likely to be selected as an OZ when just crossing the cutoff point.

As noted earlier, an important issue for estimating the impact of OZ designation is that the New Markets Tax Credit (NMTC) has continued to operate during the implementation of OZs. The NMTC provides federal tax credits to entities who invest in Community Development Entities (CDEs). CDEs then make qualified investments in eligible census tracts.[10]

[10] See Congressional Research Service (2019) for an overview of the NMTC program.

Figure 7: Share of tracts designated as Opportunity Zones by constructed running variable



Sources: American Community Survey 2011-2015 5-year pooled sample; U.S. Department of the Treasury.
Notes: Each dot represents the sample average within each bin. Fitted lines are based on a polynomial of degree 4 fitted separately to points on either side of the cutoff.

The U.S. Treasury allocated just over $3.5 billion in tax credits during the 2019 calendar year, the intention of which is to fuel private investment to these areas that otherwise would not have occurred.

Census tracts' eligibility conditions for the NMTC are identical to those for OZs. Tracts must have a poverty rate above 20 percent or median income below 80 percent of the area median. While the NMTC eligibility conditions between 2012 and 2017 were based on the 2006-2010 ACS (and before 2012 used the Decennial 2000 Census), the program transitioned to using the 2011-2015 ACS between November 2017 and October 2018, overlapping with the period during which OZs were designated.

As a result, any change in outcomes in eligible tracts starting 2018 could be a result of eligibility for OZ investment, eligibility for NMTC investment, or an interaction of the two

Table 2: First Stage Regression Discontinuity Estimates: Impact of Cutoff Point on Designation as Opportunity Zone

	No controls		**With controls**	
Running variable	**Estimates**	**Observations**	**Estimates**	**Observations**
Poverty	0.163	45,826	0.162	45,786
	(.017)		(0.017)	
Income	0.113	49,944	0.112	49,942
	(.011)		(0.010)	
Combination	0.120	73,142	0.120	73,101
	(.008)		(0.008)	

Sources: 2011-2015; 2013-2017 5-year pooled sample; U.S. Department of the Treasury.
Notes: Poverty, income and combination running variables are defined in the text. Control variables include the labor force participation rate, employment to population ratio, the unemployment rate, and the share of workers employed in each industry, all based on the 2013-2017 American Community Survey.

programs. This affects both difference-in-difference estimates and regression discontinuity estimates, though in different ways. Difference-in-difference estimates of the impact of OZ designation comparing differences in selected versus eligible but not selected census tracts will be biased in an unknown direction. If NMTC investment tended to increase investment and drive outcomes more in eligible tracts actually designated as OZs, the treatment effect of OZs will be biased upward. If NMTC investment instead tended to increase investment and drive outcomes in eligible tracts not designated as OZs, the treatment effect of OZs will be biased downward. Regression discontinuity estimates of the impact of OZ eligibility or designation will be biased upward, since eligible tracts may receive both NMTC and OZ investment. Thus, regression discontinuity estimates will form upper bounds on the impact of OZ designation on outcomes.

5 Results

We discuss results for investment and economic development using our restaurant quality index separately. For both sets of results, we consider the treatment period to start on July 1, 2018.

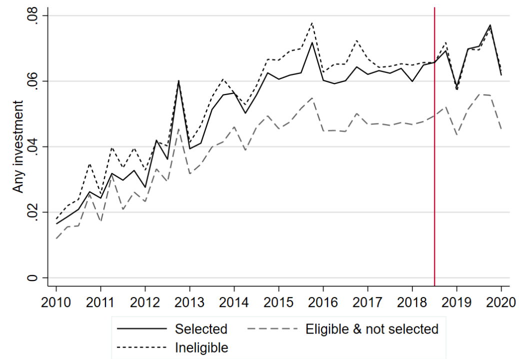
5.1 Investment

We consider four primary investment related outcomes for both the difference-in-differences and regression discontinuity approaches. These include (i) whether any investment occurred, (ii) the normalized number of investments, (iii) dollars of investments, and (iv) normalized dollars of investment. Our normalization for the number (dollars) of investment is taken as the ratio of the number (dollars) of investments in a tract in a given period to the mean number (dollars) of investments per tract/period in the same county over the entire time-frame. This normalization essentially scales effects based on normal amounts of investment in a given county, which may be important if some counties have much higher levels of investment than other counties. It also circumvents the problem of using the natural logarithmic transformation in the presence of a substantial number of zero values for our outcome variables.

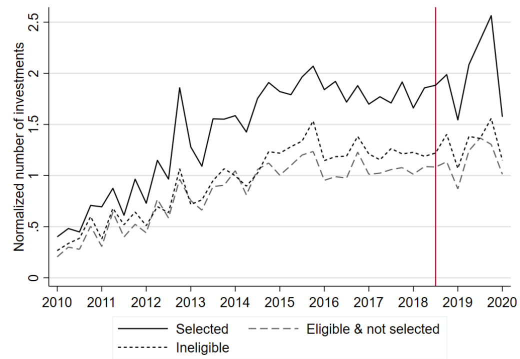
Figure 8 shows the mean of each of the four investment outcome variables at quarterly levels, expressed as increases compared to four quarters prior, for each of the three groups of tracts. As seen earlier, investment increases until about 2015 before flattening out. Levels and trends are similar for selected tracts and eligible but not selected tracts.

We next show coefficients for variables interacting an indicator for OZ selection with each quarter of the study time period (the estimates of β in Equation 1). To the extent

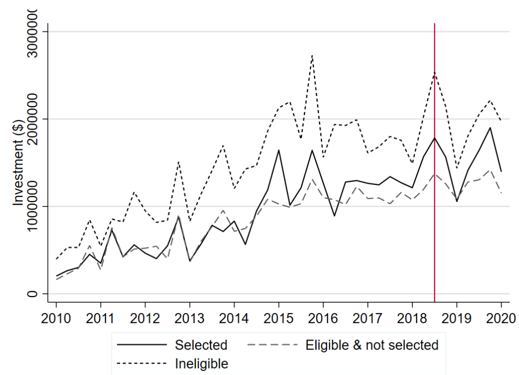
Figure 8: Trends in various investment outcomes, by tract type



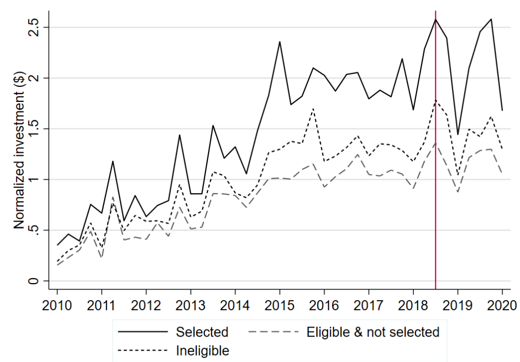
(a) Any investment



(b) Normalized number of investments



(c) Dollars of investment

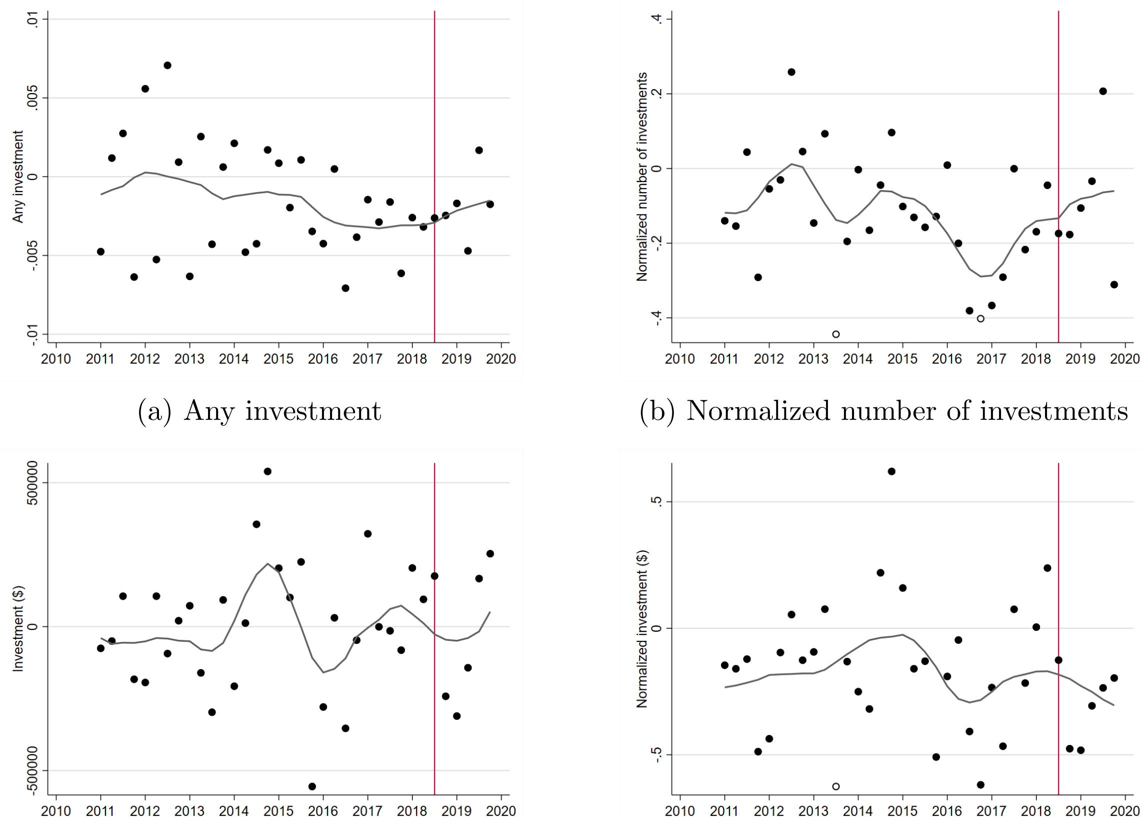


(d) Normalized dollars of investment

Sources: Real Capital Analytics; American Community Survey, 2011-2015 5-year pooled sample; U.S. Department of the Treasury.
Notes: Outcome variable is expressed as the quarterly value minus the value four quarters prior. Any investment is an indicator variable equal to 1 if at least one investment was recorded in the census tract during the quarter. Normalized number of investments is the ratio of the number investments in a tract in a given quarter to the average number of investments per tract in the same county over the entire sample period. Dollars of investments is the sum of prices for all investment transactions during the quarter. Normalized dollars of investment is the ratio of the dollars of investment in a tract in a given quarter to the average dollars of investment per tract in the same county over the entire sample period. Sample period is 2010Q1 through 2019Q4. All dollars values are adjusted for inflation using the Personal Consumption Expenditures price index. Vertical line is drawn at 2018 Q3, the first quarter after which OZ designations were certified by the U.S. Treasury.

that OZ designation increases investment, we would expect estimates to be higher starting in the third quarter of 2018. In addition, for the assumption of parallel pre-trends to hold, estimates should be near zero prior to 2018. Figure 9 shows these coefficients for each of the

Figure 9: Treatment effect for various investment outcomes, difference in differences estimates



(a) Any investment

(b) Normalized number of investments

(c) Dollars of investment

(d) Normalized dollars of investment

Sources: Real Capital Analytics; American Community Survey, 2011-2015 5-year pooled sample; U.S. Department of the Treasury.
Notes: Dependent variable is expressed as the quarterly value minus the value four quarters prior. Any investment is an indicator variable equal to 1 if at least one investment was recorded in the census tract during the quarter. Normalized number of investments is the ratio of the number investments in a tract in a given quarter to the average number of investments per tract in the same county over the entire sample period. Dollars of investments is the sum of prices for all investment transactions during the quarter. Normalized dollars of investment is the ratio of the dollars investments in a tract in a given quarter to the average dollars of investment per tract in the same county over the entire sample period. Sample period is 2010Q1 through 2019Q4. All dollars values are adjusted for inflation using the Personal Consumption Expenditures price index. Vertical line is drawn at 2018 Q3, the first quarter after which OZ designations were certified by the U.S. Treasury.

four investment outcome variables. None of the coefficients are statistically different from zero, either before or after OZ designations were made. This suggests investment was not substantially affected by OZ designation.

Table 3: Impact of Opportunity Zones Eligibility on Commercial Investment Growth, Difference in Differences Estimates

	Any investment	Normalized number of investments	Dollars of investment	Normalized dollars of investment
Mean quarterly level	0.0480	1.0000	1,160,379	1.0000
Mean 4-quarter change	0.0044	0.1092	131,024	0.1172
Treatment effect	-0.0006	0.0310	13,008	-0.0674
	(0.0013)	(0.0569)	(93,140)	(0.1019)
Census tract fixed effects	X	X	X	X
State*quarter fixed effects	X	X	X	X
Number of observations	1,560,549	1,560,549	1,560,549	1,560,549

Sources: Real Capital Analytics; American Community Survey, 2011-2015 5-year pooled sample; U.S. Department of the Treasury.
Notes: Dependent variable is expressed as the quarterly value minus the value four quarters prior. Any investment is an indicator variable equal to 1 if at least one investment was recorded in the census tract during the quarter. Normalized number of investments is the ratio of the number investments in a tract in a given quarter to the average number of investments per tract in the same county over the entire sample period. Dollars of investments is the sum of prices for all investment transactions during the quarter. Normalized dollars of investment is the ratio of the dollars of investment in a tract in a given quarter to the average dollars of investment per tract in the same county over the entire sample period. Sample period is 2010Q1 through 2019Q4. All dollars values are adjusted for inflation using the Personal Consumption Expenditures price index.

Finally, we show in Table 3 estimates of the treatment effect of OZ designation based on the difference-in-differences approach. Effects are small. For example, OZ designation increased investment growth by $13,008. The mean quarterly level of investment is $1.16 million and the mean 4-quarter increase is $131,024.

We next show regression discontinuity results for the same four investment outcomes. With the exception of the indicator variable for whether any investment occurred in the treatment period, for our dependent variables we focus on changes in each outcome variable post-designation compared to pre-designation. In particular, we take the difference of the annualized value between July 1, 2018 and December 31, 2019 and the annualized value between January 1, 2016 and December 31, 2017. Since TCJA was signed at the end of 2017, this ensures that the pre-designation period is not contaminated while remaining recent enough to be as informative as possible. See Table 4 for means of our non-transformed dependent variables.

Table 4: Summary Statistics, Commercial Investment

	Eligible & selected	Eligible & not selected	Not eligible	All
Any investment: Share				
Jul. 1, 2018 to Dec. 31, 2019	0.24	0.19	0.25	0.22
Annualized number of investments: Mean (standard deviation)				
Jan. 1, 2016 to Dec. 31, 2017	0.33	0.25	0.25	0.30
	(1.03)	(0.91)	(0.98)	(0.95)
Jul. 1, 2018 to Dec. 31, 2019	0.38	0.28	0.37	0.33
	(1.25)	(1.04)	(1.13)	(1.11)
Annualized dollars of investment in millions: Mean (standard deviation)				
Jan. 1, 2016 to Dec. 31, 2017	4.43	3.89	6.81	5.22
	(22.34)	(39.90)	(56.87)	(46.61)
Jul. 1, 2018 to Dec. 31, 2019	6.16	5.08	8.02	6.48
	(38.45)	(49.86)	(59.82)	(53.28)
Number of tracts	8,762	33,415	31,951	74,128

Sources: American Community Survey, 2011-2015, 5-year pooled sample; Real Capital Analytics, 2011-2015; U.S. Department of the Treasury.

We show first the impact of OZ eligibility on the four investment outcomes in Table 5. These results are essentially intent-to-treat estimates they only estimate the impact of being eligible for selection as an OZ rather than the impact of actually being selected. For each outcome, we show six total results—using each of our three running variables based on poverty, income and a combination, and with and without control variables. Estimated impacts of OZ eligibility are small and not statistically different from zero. For example, when using the combination running variable and including controls, OZ eligibility increases the probability of investment by 0.8 percentage points, reduces the number of investments by 14 percent of normal county levels, increases dollars of investment by \$0.2 million, and decreases dollars of investment by by 4 percent of normal county levels. While the final two results are inconsistently signed, they both point to a near-zero impact of OZ eligibility on dollars of investment. Figure 10 shows these near-zero impacts of OZ eligibility graphically.

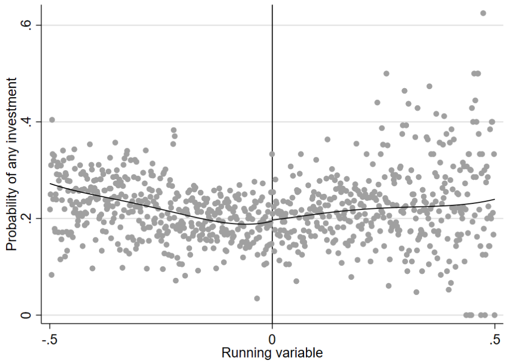
Table 5: Impact of Opportunity Zones Eligibility on Commercial Investment, Regression Discontinuity Estimates

Running variable	**Any investment**	**Normalized number of investments**	**Dollars of investment (millions)**	**Normalized dollars of investment**	**Obs.**
No controls					
Poverty	0.024	0.013	0.875	0.399	45,826
	(0.025)	(0.270)	(0.838)	(0.446)	
Income	0.008	-0.067	-0.024	-0.217	49,944
	(0.019)	(0.258)	(0.473)	(0.344)	
Combination	0.011	-0.138	0.223	-0.037	73,142
	(0.011)	(0.145)	(0.374)	(0.207)	
With controls					
Poverty	0.020	0.001	0.942	0.411	45,786
	(0.024)	(0.266)	(0.858)	(0.452)	
Income	0.017	-0.070	-0.016	-0.220	49,942
	(0.020)	(0.257)	(0.474)	(0.341)	
Combination	0.008	-0.138	0.207	-0.044	73,101
	(0.011)	(0.145)	(0.370)	(0.209)	

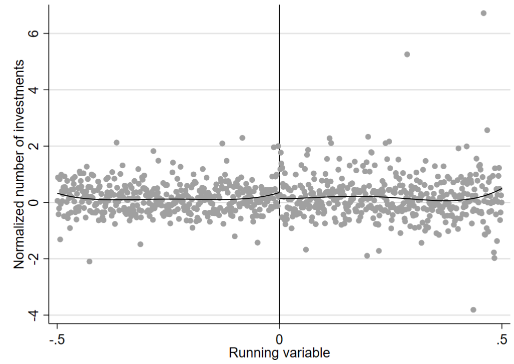
Sources: Real Capital Analytics; American Community Survey, 2011-2015; 2013-2017, 5-year pooled sample; U.S. Department of the Treasury.

Notes: Any investment is an indicator variable equal to 1 if at least one investment was recorded in the census tract between July 1, 2018 and December 31, 2019. Number (dollars) of investments is the difference in annualized number (dollars) of investments made during the period July 1, 2018 and December 31, 2019 and annualized number (dollar) of investments made between January 1, 2016 and December 31, 2017. Number (dollars) of investments are normalized by dividing the annualized number (dollars) of investments by the mean annualized number (dollars) of investments in all tracts within the same county between January 1, 2010 and December 31, 2019. Extreme values of the dependent variable greater than $100 million in absolute value are replace with $100 million, for the dollars of investment dependent variable only. When poverty is the running variable, the sample is restricted to census tracts where the income condition is not satisfied. When income is used as the running variable, the sample is restricted to census tracts where the poverty condition is not satisfied. See text for the definition of the combination running variable. Control variables include the labor force participation rate, employment to population ratio, the unemployment rate, and the share of workers employed in each industry, all based on the 2013-2017 American Community Survey. All dollars values are adjusted for inflation using the Personal Consumption Expenditures price index.

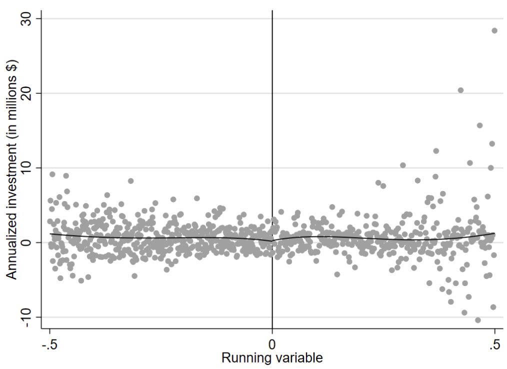
Figure 10: Investment outcomes by constructed running variable



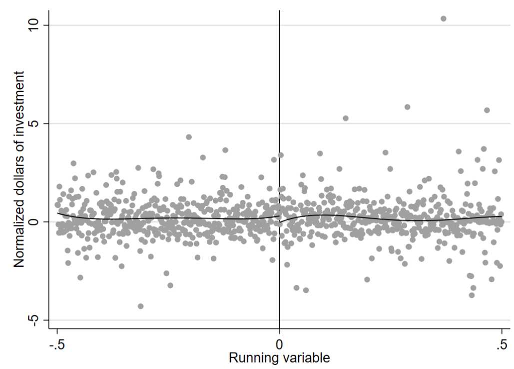
(a) Any investment during post-treatment period



(b) Increase in normalized number of investments in post-treatment period



(c) Increase in dollars of investment in post-treatment period



(d) Increase in normalized dollars of investment in post-treatment period

Sources: Real Capital Analytics; American Community Survey, 2011-2015 5-year pooled sample; U.S. Department of the Treasury.
Notes: Each dot represents the sample average within each bin. Fitted lines are based on a polynomial of degree 4 fitted separately to points on either side of the cutoff. In panel (a), the dependent variable is a binary variable equal to one if there was at least one investment in the tract between July 1, 2018 and December 31, 2019, and zero otherwise. In panel (b), the dependent variable is the annualized number of investments between July 1, 2016 and December 31, 2017 divided by the average annual number of investments per tract in the same county between January 1, 2010 and December 31, 2019, minus the annualized number of investments between January 1, 2016 and December 31, 2017 divided by the average annual number of investments per tract in the same county between January 1, 2010 and December 31, 2019. In panel (c), the dependent variable is the annualized total dollars of investment between July 1, 2018 and December 31, 2019 minus the annualized total dollars of investment between January 1, 2016 and December 31, 2017. In panel (c), extreme values of the dependent variable greater than $100 million in absolute value are replace with $100 million. In panel (d), the dependent variable is the same as in panel (b), except dollars of investment are used instead of number of investments. All dollars values are adjusted for inflation using the Personal Consumption Expenditures price index.

Finally, we estimate treatment on the treated effects by using a fuzzy regression discontinuity design. This allows us to estimate the impact of OZ designation itself on investment outcomes. Table 6 shows estimated effects for the same specifications shown in Table 5, with the only difference that we use a fuzzy design based on actual selection of tracts as OZs. Estimates are again small and not statistically different from zero, though standard errors are larger. Estimates for the combination running variable with controls implies that OZ designation increases the probability of any investment by 4.3 percentage points, reduces the number of investments by more than normal county levels, increases dollars of investment by $1.6 million, and reduces the dollars of investment by 6.3 percent of normal county levels.

5.2 Restaurant Quality

Despite the lack of any positive and economically important impact on commercial investment, we also estimate whether there are general improvements in other measures of economic development. While the lower bound of $2.5 million in the RCA data is not particularly restrictive for commercial investment, we recognize that it does not capture small, perhaps numerous, investments as well as general improvements due increased economic activity. We focus on restaurant quality as a proxy of these improvements for a number of reasons. First, "Restaurants and Other Eating Places" is the largest category in the SafeGraph database representing 830,000 of the roughly 6 million establishments that it follows.[11] Second, we rely upon branded restaurants—30 percent of establishments—in order to generate our quality index. That is, the presence of these brands in multiple census tracts, sometimes regional and often national, provide the necessary variation that allow us to predict how useful a brand is in predicting census tract type. The idea is that selected census tracts should begin

[11] "Religious organizations" is the second largest category representing around 400,000 establishments.

Table 6: Impact of Opportunity Zones Designation on Commercial Investment, Fuzzy Regression Discontinuity Estimates

Running variable	Any investment	Normalized number of investments	Dollars of investment (millions)	Normalized dollars of investment	Obs.
No controls					
Poverty	0.136	0.039	5.821	2.544	45,826
	(0.165)	(1.689)	(6.273)	(3.217)	
Income	0.022	-1.200	0.351	-2.260	49,944
	(0.152)	(1.877)	(4.267)	(2.837)	
Combination	0.055	-0.747	1.799	0.395	73,142
	(0.114)	(1.105)	(2.963)	(1.417)	
With controls					
Poverty	0.110	-0.012	5.707	2.493	45,786
	(0.165)	(1.759)	(6.746)	(3.267)	
Income	0.030	-1.156	-0.881	-2.272	49,942
	(0.151)	(1.909)	(3.894)	(2.918)	
Combination	0.043	-1.262	1.606	0.063	73,101
	(0.112)	(1.248)	(2.710)	(1.525)	

Sources: Real Capital Analytics; American Community Survey, 2011-2015; 2013-2017, 5-year pooled sample; U.S. Department of the Treasury.

Notes: Any investment is an indicator variable equal to 1 if at least one investment was recorded in the census tract between July 1, 2018 and December 31, 2019. Number (dollars) of investments is the difference in annualized number (dollars) of investments made during the period July 1, 2018 and December 31, 2019 and annualized number (dollar) of investments made between January 1, 2016 and December 31, 2017. Number (dollars) of investments are normalized by dividing the annualized number (dollars) of investments by the mean annualized number (dollars) of investments in all tracts within the same county between January 1, 2010 and December 31, 2019. Extreme values of the dependent variable greater than \$100 million in absolute value are replace with \$100 million, for the dollars of investment dependent variable only. When poverty is the running variable, the sample is restricted to census tracts where the income condition is not satisfied. When income is used as the running variable, the sample is restricted to census tracts where the poverty condition is not satisfied. See text for the definition of the combination running variable. Control variables include the labor force participation rate, employment to population ratio, the unemployment rate, and the share of workers employed in each industry, all based on the 2013-2017 American Community Survey. All dollars values are adjusted for inflation using the Personal Consumption Expenditures price index.

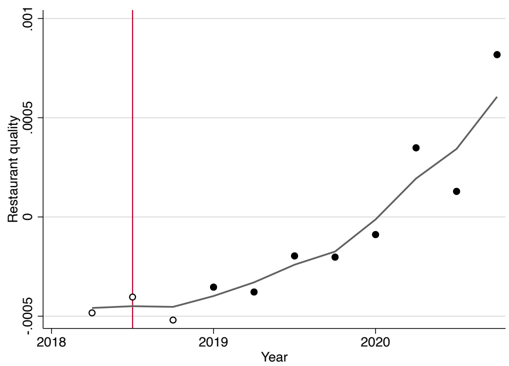
to look more like ineligible tracts over time in terms of the types of branded restaurants and the foot traffic to these establishments if OZ designation is associated with improved economic conditions.

Figure 11 presents the estimated treatment effects of selection into OZ designation from a difference-in-differences estimation where we regress census tract level restaurant quality index on the interaction between selected and quarter (the treatment effect), the interaction of state and quarter and census tract fixed effects with standard errors clustered at the census tract level. The base category is the first quarter of 2018 and we consider the "pre" period to be the first two quarters of 2018. Unfortunately, due to data limitations, we cannot go further back than January 2018. As the left side of the figure shows, there is general improvement in restaurant quality over time; however, it is not statistically significant at conventional levels. Moreover, the estimated treatment effect is economically negligible. As shown in Table 1, the average restaurant quality index was 0.507 for selected tracts and 0.572 for ineligible tracts for a difference of 0.065. Thus, the point estimates of around 0.0005 represent less than a percent change in closing the quality gap. The right hand side of the figure shows the RD estimation similar to Figure 10. The figure shows no visible change in restaurant quality at the discontinuity.

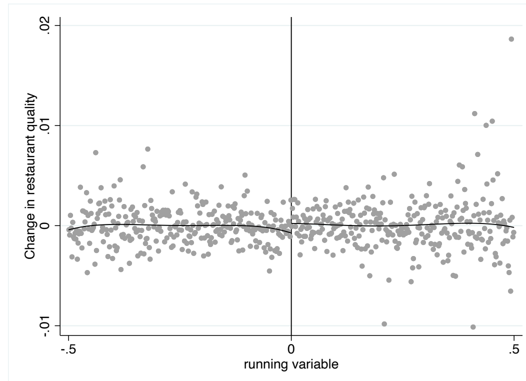
6 Discussion

The OZ provision of the TCJA was intended to help address the growing economic gap faced by disadvantaged communities in the United States. By providing tax incentives for private investment into high-poverty, low-income census tracts covering over one-tenth of the U.S. population, legislators hoped that OZ tax incentives would fuel private investment into

Figure 11: Restaurant quality post OZ designation



(a) Difference-in-difference: restaurant quality index (census tract level)



(b) Regression discontinuity: restaurant quality index (census tract level)

Sources: Source: SafeGraph, July 2018 - November 2020
Notes: Each dot represents the sample average within each bin. Left figure: Outlined circles represent point estimates that are statistically different from zero at a 5% level of significance, filled in dots represent point estimates that are not statistically different from zero at a 5% level of significance. Right figure: Fitted lines are based on a polynomial of degree 4 fitted separately to points on either side of the cutoff.

disadvantaged communities which in turn would eventually improve the well-being of OZ residents through increased employed, increased incomes and reduced poverty. While it is still too early to evaluate whether OZs will successfully achieve these objectives, our results provide insight into early impacts with implications for expectations about future effects.

We find that through the end of 2019, two years after TCJA was passed and a year and half since OZ designations were finalized, OZs did not have a statistically significant impact on investment. Our point estimates are for the most part economically small or negative: Based on our difference in differences estimates, OZs slightly decreased both the probability of any investment and the quarterly growth (over the same quarter four years ago) of investments relative to the typical amount of investment in tracts in the same county. Based on our fuzzy multivariate regression discontinuity estimates, the probability of investment grew by 4.3 percentage points and dollars of investment increased by 6.3 percent of the typical level of investment in tracts in the same county. Across both empirical approaches and all four investment outcomes, point estimates are small or negative, with the exception of a modest effect on non-normalized dollars of investment under the fuzzy regression discontinuity approach, although even in this case the effect is not statistically different from zero.[12]

Our two empirical approaches complement one another by relying on different identification strategies and estimating treatment effects on different populations of OZs. While regression discontinuity effects are less precisely estimated, they confirm the limited effects estimated based on the difference in differences approach. The modest impact under both empirical approaches on our restaurant quality index provides further evidence of a limited initial economic development response to OZs. Our results are also consistent with Chen et al. (2019) who find limited impacts of OZ designation on home prices. If OZs had led to an increase in private investment, we may have expected to see a positive impact on home prices as home buyers are forward-looking regarding future impacts on economic conditions such as higher wages and higher quality amenities.

The limited effect of OZs on investment through the end of 2019 suggests that strong downstream impacts on OZ residents may not be forthcoming. The natural channel through which employment and wages would have risen would be through an increase in private investment that increased labor demand, although it is possible that public investments that were paired OZ incentives in some federal programs and in some states could have some impact.

However, it is important to emphasize important caveats for interpreting our results.

[12]Under the (non-normalized) dollars of investment outcome using fuzzy regression discontinuity approach, the point estimate, though not statistically different from zero, suggests that OZs increased investment by \$1.6 million for OZs near the eligibility cutoff. If we assumed the same impact applied to all the approximately 8,700 OZs, this would imply a total effect of about \$21 billion over the 1.5 years between July 1, 2018 and December 31, 2019. For comparison, Council of Economic Advisers (2020) estimates that Qualified Opportunity Funds raised about \$75 billion for investment into OZs through the end of 2019.

First, our estimates extend only through 2019, allowing only one and a half years since OZs were officially designated and just over one year since Treasury provided important guiding rules in October 2018. Investors could increase investment over time as they become more familiar with OZ rules, although the tax benefits from OZ investment diminish over time. Second, we do not capture most smaller investments of less than $2.5 million, and so we cannot rule out impacts of OZs on small investments. Third, it is not clear how the COVID-19 pandemic will affect OZ investment, or if the pandemic could spur OZ policy changes such as extending the window for tax-favored investments in OZs. It is possible that OZs would have a different impact in the post-COVID environment than in the pre-COVID environment. For all of these reasons, it will be important to continue to evaluate the impact of OZs on investment, economic development, home prices, and the well-being and labor market outcomes of OZ residents.

7 Conclusion

The persistence of economic disadvantage in some areas in the United States, combined with reduced geographic mobility, has led to renewed calls for policies that can improve the economic circumstances of residents in struggling regions. Our results suggest that at least in their first two years of existence, there is not evidence that OZ tax incentives have significantly increased commercial investment in designated OZs. Similarly, restaurant quality, a proxy for increased smaller scale investment and more general improvements, has not substantially risen in OZs as a result of designation. These findings are consistent across two distinct identification strategies—difference in differences and regression discontinuity designs. Future research should continue to monitor the effects of OZs on investment and other outcomes such as employment, property values and other measures of economic activity in the years to come.

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A Restaurant quality weights

The category of "Restaurant and other Eating Places" (henceforth "Restaurants") captures just under 830,000 establishments out of the roughly 6 million in the SafeGraph database.[13] We focus on "branded" restaurants in order to establish our quality measure. These branded (or chain) restaurants comprise 30 percent of total Restaurant establishments. We use these visits over the first six months of 2018 to calibrate restaurant quality.[14] We first obtain the top 100 brands (weighted by visits) for each census tract. Next, we run a simple linear regression of these brand dummies on a binary indicator for ineligibility of OZ designation (that is, the census tracts with poverty rates below 20 percent and median income above 80 percent of the MSA or state median.) without including a constant term. The estimated coefficients from this regression provide the weights for each brand. Figure A.1 presents the full distribution of the quality weights. The mass points at 0 and 1 reflect that some brands are exclusively in either eligible or ineligible tracts, respectively. Furthermore, we estimate, for example, a weight of 0.48 for McDonalds and 0.80 for Peet's Coffee and Tea. Intuitively, this means that McDonalds is not terribly informative as to whether a census tract is of higher socio-economic status but Peet's Coffee and Tea is, relatively speaking, more so. In general, a weight of 0.50 means that a particular brand is just as likely to be in a noneligible tract as compared to an eligible tract. Table **??** provides a number of illustrative examples of the weights allocated to various brands. After calculating the weights for each of the brands based up Jan-Jun 2018 establishment visits, we then match the brand quality weights to the monthly data from July 2018 - November 2020, further weighting by contemporaneous establishment visits. Finally, we calculate the weighted average quality at the census tract level. We then look at changes in this weighted average quality over time to proxy for changes in economic improvement. There are two main ways for the quality weight to increase over time. First, the introduction of higher quality (above the previous mean) restaurants into the census tract and second, a relative shift of establishment visits towards pre-existing higher quality restaurants.

[13] The next largest category is "Religious Organizations" at less than half that number.

[14] Ideally we would use the pre-period of 2017 (prior to the passage of TCJA in December 2017) but the Safegraph data is only available beginning in January 2018. Thus, the first six months of 2018 are used to calibrate the quality of restaurants prior to any changes due to OZ designation. This calibration is reasonable assuming that restaurant quality does not immediately adjust post OZ designation.

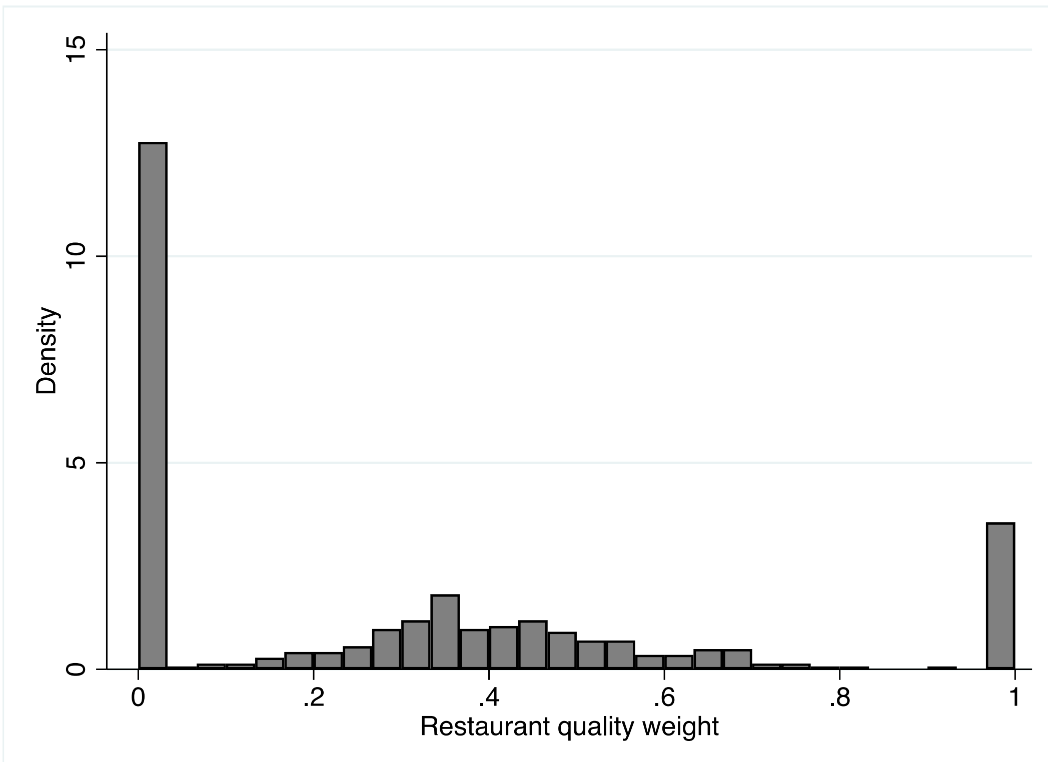

Figure A.1: Quality weights by restaurant brand

Table A.1: Examples of restaurant brand weights

Weight	Brand
1	Blue Bottle Coffee
	Chopt
	Philz Coffee
	Pressed Juicery
	Zinburger Wine and Burger Bar
0.8 - 0.99	Manhattan Bagel
	Planet Smoothie
	Rosati's Chicago Pizza
0.6 - 0.79	Pret-a-manger
	In-N-Out Burger
	Chipotle
	Starbucks
0.5 - 0.59	TGI Friday's
	Potbelly Sandwich Works
	Arby's
	Pizza Hut
	Taco Bell
	Dairy Queen
0.4 - 0.49	Domino's Pizza
	McDonald's
	Denny's
	KFC
	White Castle
0.2 - 0.39	Einstein Bros
	Long John Silver
	Old Country Buffet
	Fuddruckers
	Cottage Inn Pizza
.01 - 0.19	Church's Chicken
	Lebanese Tavern
	Apollo Burgers
0	Pizza Loca
	Pollo Camporo
	Atomic Wings
	Jersey Giant Subs

Source: Safegraph, Jan-Jun, 2018. No. brands: 971