The Impact of Opportunity Zones on Private Investment and Economic Activity

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Abstract

Place-based policies aim to stimulate economic development in disadvantaged areas with the goal of improving the well-being of residents. A provision of the Tax Cuts and Jobs Act aimed to spur private investment in low-income areas called Opportunity Zones (OZs). We evaluate the impact of OZs on investment using data on the near-universe of commercial retail, office, and industrial real estate investments in the United States and a regression discontinuity design that exploits randomness near the OZ eligibility threshold. From 2019-2022, we find economically small and statistically insignificant point estimates of the effect of OZ eligibility on census tract-level investment. Additional data from MasterCard show no robust evidence of increased business activity nor consumer spending. However, we find suggestive effects on investment in multi-family housing in certain years. We conclude that almost five years after the OZ policy was implemented, there is no evidence of a private investment response that has spread beyond multi-family housing, limiting the potential of the policy to stimulate broad economic development and improve the well-being of residents.

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1 Introduction

Rising geographic disparity in economic well-being in recent decades has focused attention on place-based policies as a tool to help people living in distressed areas. While in the past regions with low incomes would improve faster than regions with high incomes, this pattern of income convergence has stalled—or even reversed—in recent decades with reduced migration from low-income to high-income areas. Consequently, improvement in the economic conditions of distressed neighborhoods and the people who live in them may not occur naturally (Berry and Glaeser 2005; Ganong and Shoag 2017). Geographic disparity in well-being has long-term economic costs. For example, the neighborhoods in which children grow up have important consequences for their long-term well-being (e.g., Chetty et al. 2016; Chyn 2018). In light of these phenomena, place-based policies that target people living in distressed areas may be warranted.

In an attempt to address geographic disparities in well-being, the Tax Cuts and Jobs Act (TCJA) of 2017 included a provision that offered flexible tax incentives for investing capital into certain areas called Opportunity Zones (OZs), with the stated purpose of revitalizing economically distressed local economies. Only areas with sufficiently low median incomes or sufficiently high poverty rates were eligible for selection as OZs. Almost 9,000 census tracts—out of about 75,000 in the United States—were ultimately selected as OZs in the middle of 2018, and rules for investors followed later that year. Coyne and Johnson (2022) estimate from IRS data that investors made at least $48 billion of qualifying investments in OZs by the end of 2020. Whether this investment was caused by the OZ tax provisions and whether it boosted economic activity in OZs are key questions for determining whether OZs will ultimately improve the well-being of OZ residents and reverse trends in rising geographic disparity.

Recent studies have evaluated the impact of OZs on tract-level outcomes using a difference-in-differences approach that compares changes in outcomes in OZs to eligible but not selected OZs, though differing somewhat in how they define the control group.[1] The focus has primarily been on real estate prices and employment outcomes. For example, Sage et al. (2023) 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. Other studies focus on residential home prices. Using property-level transaction data from Zillow, Casey (2019) finds early and large home price impacts. By contrast, when controlling for the types of properties sold, Chen et al. (2023) and Council of Economic Advisers (2020) estimate smaller effects on home prices using repeat sales data from the Federal Housing Finance Agency. Wheeler (2022) considers the development of new commercial and residential properties using data on building permits in large cities, finding a 2.9 percentage point increase in development in OZs. 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. Freedman et al. (2023) find no statistically significant impact of OZ selection on employment, wages and poverty rates once controlling for pre-trend differences between selected and eligible but not selected tracts.

Overall, these studies present a mixed picture on the short-run impact of OZ selection and are generally unable to identify the mechanism driving effects on economic outcomes.

[1]For example, Sage et al. (2023) uses propensity score matching to identify similar tracts and Chen et al. (2023) use geographic neighbors in some specifications.

Of course, expectations of future development may be capitalized into housing prices, for example, but these outcomes could only be maintained with actual improvements in realized investment in these relatively low socioeconomic areas. More concerning is that these studies use a difference in differences design that relies on strong assumptions on the ability to control for different trends in census tracts prior to the implementation of the OZ policy.

Using the universe of commercial investment transactions above $2.5 million from 2010 through 2022 in the United States, we take a step back from other papers in the literature to examine whether the OZ policy increased investment, the primary objective of the policy from which other outcomes such as higher home prices and better employment outcomes would flow. We do so using a regression discontinuity approach that exploits randomness near the OZ eligibility threshold to identify the causal impact on investment, avoiding the strong assumptions needed to validate difference in differences approaches that fail to satisfy parallel pre-trend assumptions using non-adjusted data.

We make three main contributions to the literature. Our first and main contribution is to learn whether a broad, relatively unstructured, place-based policy incentivizes additional private investment. As we will describe below, OZs represent a new approach to place-based incentives that have typically been more structured and employment-focused. OZs represent the first place-based policy that allows uncapped private investment into areas throughout the United States. Thus, it is crucial to understand whether the primary investment objective is achieved in order to inform the likelihood of forthcoming secondary outcomes such as increased employment, higher wages, poverty reduction, and, ultimately, reversing the slower economic growth that has held back distressed neighborhoods in the United States.

Our second contribution is related to the time frame of the analysis. We take a longer perspective than any other study, taking our analysis out to the end of 2022. While the

financial incentives to invest were strongest for investments made by the end of 2019, the full extent of the investment response would likely take longer to be realized, given that OZs were not designated until halfway through 2018 and key regulations were not made public until several months later. It is possible that the mixed evidence from prior studies when looking at employment and housing outcomes is due to the relatively short time frame post-policy change that these studies use (Fikri and Glasner 2023). We will return to this point in our discussion of investments in multi-family housing.

Our third contribution is methodological. We differ from previous studies by using a multidimensional regression discontinuity approach that relies on weaker assumptions than the difference in differences approach taken in the prior literature. In fact, we find that commercial investment was rising faster in selected census tracts than in non-selected tracts prior to OZ selection in 2018, violating a necessary assumption for a difference in differences approach. We overcome this problem by instead relying on randomness around the eligibility thresholds in order to identify the effect of OZ eligibility on investment outcomes. In order to be eligible for selection as an OZ, a census tract must have had either a poverty rate exceeding 20 percent or a median family income below 80 percent of the area median. We construct a running variable that incorporates both the poverty and median income eligibility conditions such that tracts above the cutoff point of the running variable are eligible and tracts below the cutoff point are ineligible. By comparing investment outcomes in tracts just above the cutoff point with tracts just below, we can credibly estimate the causal effect of OZ eligibility on investment. Because the poverty and median income values used as eligibility conditions for each census tract are based on a U.S. Census Bureau survey conducted from 2011 through 2015—and were published before the OZ provision of the TCJA was passed—census tracts could not have manipulated the determinants of their eligibility for purposes of qualifying

as an OZ.

The regression discontinuity approach identifies the effect of OZ eligibility near the cutoff point, allowing us to estimate the effect of expanding the program on the margin. We find clear jumps in the probability of selection upon passing the eligibility threshold of about 11 to 13 percentage points and economically small impacts on commercial investment—OZ eligibility reduces the number of investments by 0.002 (0.6%) and decreases dollars of investment by $0.103 million (2.4%). While unexpectedly negative, neither estimate is statistically significant at conventional levels. This lack of a statistically significant investment response holds whether we use the change in average annual investment from the pre-OZ period (2014–2017) to the post-period (2019–2022) or when we look at commercial investment in each year individually. We also find no statistically significant effects when we analyze the types of investments (i.e., retail, industrial, and office) individually. We support these commercial investment results with additional data from MasterCard on economic activity and consumer spending. The MasterCard data show no statistically significant impact of OZ eligibility on new business growth, business loan growth, commercial diversity nor consumer spending, lending further credence to our results on investment.

The one exception to our general finding of a lack of investment response is multi-family housing. The change in investment in multifamily housing from 2014–2017 to 2019–2022 shows no statistically significant response, but this is masking heterogeneity in annual investment. We find a statistically significant jump in both the number and dollars of investment in 2018, a fall in 2019 (though statistically insignificant), followed by what looks like an increasing trend, and again a statistically significant positive impact in dollars of investment in 2022. The interpretation of these annual estimates is complicated by the COVID-19 pandemic that began in early 2020 but suggests that investors may have turned to multi-family

housing as a way to benefit from the policy.

Overall, we conclude that almost five years after the OZ policy was implemented, there is no evidence of a private investment response that has spread beyond multi-family housing. While it is plausible that investment in multi-family housing could spur investment in other sectors as well, we do not observe such an effect through 2022. In addition, we note that the development of multi-family housing is unlikely in the most distressed areas where a declining population has led to an excess of existing homes, suggesting the multi-family housing investment response is likely targeted at less distressed areas. Given the lack of a broader investment response, we do not expect that the OZ policy will meaningfully reduce geographic disparities and improve the economic well-being of residents living in distressed areas, on average.

2 Opportunity Zones Background

The OZ provision of 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, as a result, 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.[2]

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%; (b) have a median income below 80% of the median income in the state or metropolitan area; or (c) be contiguous with a selected 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 further in Section 4. In this light, Figure 2 presents four plots where each dot represents a single census tract.[3] The horizontal axis represents the census tract poverty rate. All census tracts to the right of 20% 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% of state or MSA median income (whichever is applicable for each census tract). Hence, the horizontal red line at zero reflects the point where the census tract median income exactly equals 80% 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.

Figure 2 panel (a) shows the breakdown of all census tracts by their eligibility status. About 57% of tracts are in the top-left quadrant, making them ineligible based upon conditions (a) and (b) above and the remaining 43% of tracts are eligible on at least one of the

[2]All of Puerto Rico was designated as eligible regardless of income and poverty threshold eligibility and is dropped throughout the entire analysis.

[3]For clarity in the figures, we drop the census tracts that qualify based upon the contiguity condition in panels (c) and (d). While 10,312 tracts could potentially qualify based on the contiguity condition, only 230 such tracts were actually selected.

poverty and income thresholds. The remaining panels in this figure break down the census tracts into three groups: Panel (b) contains the subset of census tracts that were ineligible. Panel (c) contains eligible but not selected census tracts. Panel (d) contains selected census tracts. As these panels show, 57% of eligible but not selected census tracts qualify based on both eligibility dimensions, while 72% of selected census tracts are eligible on both dimensions. This suggests that governors selected census tracts that were relatively more economically disadvantaged among all eligible census tracts.[4]

Months after OZs were officially designated and confirmed by the U.S. Treasury, it issued a preliminary rule in the final quarter of 2018 providing guidance to investors on how the OZ provision would function. Those who invest unrealized capital gains in OZs via so-called Qualified Opportunity Funds (QOF) are able to defer any taxes owed on those capital gains for as long as the investment remains in the QOF through the end of 2026. If the investment remains in the QOF for at least 5 years, then 10% of the original capital gain is excluded from taxation, and if the investment remains for at least 7 years, then 15% 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 QOF (above the original capital gain) are not subject to any taxation if the investment in the QOF is maintained for at least 10 years.

Individuals can invest an uncapped amount of funds into QOFs, and QOFs 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 QOFs to

[4]Note that a small share of eligible census tracts are in the top-left quadrant due to the additional eligibility criterion that allowed census tracts to qualify based on the 2012-2016 pooled American Community Survey data, as opposed to the 2011-2015 based values shown in the figures. Less than 0.6% of selected tracts qualified based on the 2012-2016 data but not the 2011-2015 data.

the Internal Revenue Service when filing their taxes.[5] According to U.S. Treasury rules, in order for a business to qualify as being in an OZ, it must have at least 70 percent of its property located in OZs (potentially more than one). As Coyne and Johnson (2022) estimate, real estate QOFs accounted for approximately 57 percent of the qualified OZ property held by QOFs in 2020—they argue that investments in commercial real estate are a relatively safer way to comply with the legislation's tax rules and have high probabilities of nominal appreciation.

OZs represent the first place-based policy that allows uncapped private investment into areas throughout the United States. 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, such as employment, as were many other previous efforts. 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. Thus, the success (or lack thereof) of this policy will be instrumental in informing the flexibility of investment incentives in future place-based policies.

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 (RCA), and (iii) tract-level credit card and point-of-interest data from MasterCard that relate to business activity and consumer spending.

[5]QOFs are required to report the amount of investment in each census tract using IRS form 8996, beginning in tax year 2019.

The ACS is an annual household survey conducted 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 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 the RCA commercial investment database that contains transaction level data for the entire United States from 2010 through 2022 on the full set of commercial investments valued at over $2.5 million and a subset of transactions below that threshold.[6] 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 the structure, type of transaction (e.g., new construction or sale of an existing structure), address, buyer objectives, buyer and seller information, and many

[6]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." About 12% of observations have prices below $2.5 million (conditional upon a positive sale).

details on the financing of the loans. The large majority of transactions are for investment objectives (84%), with another 12% dedicated towards redevelopment or renovation (and the remainder made for occupancy purposes). We aggregate investments to the yearly census tract level, focusing on outcomes such as the number of transactions and their sale prices.[7]

We note that any given transaction in the RCA data does not necessarily qualify as investment eligible for OZ tax benefits. Qualified Opportunity Fund investments must either be an "original use" of the property or must "substantially improve" the property after purchasing it. While the RCA data should capture such transactions that are followed by substantial improvement, the RCA data also include transactions that may not be followed by substantial improvements. Our inclusion of a broader set of investment transactions than solely those eligible for OZ tax benefits does not prevent us from detecting effects on the more narrow set of investments that are eventually improved and thus most likely to benefit from the OZ policy, as long as these transactions are included in our broader set of investments. Moreover, we are interested not only in the investment that is rewarded by OZs, but also in investment that is not rewarded by OZs yet still potentially beneficial to the economic development of a community. Thus, we prefer as broad a measure of investment as possible and therefore consider the full set of transactions recorded in the RCA data.

To measure business activity we use tract-level data from MasterCard's Center for Inclusive Growth. The data contain 18 metrics from multiple data sources (some based upon proprietary credit card data from MasterCard, while others are either outsourced to other data providers and made available by Mastercard or available publicly) in order to generate their final product called the "Inclusive Growth Score" for each census tract.[8] We make use

[7] All dollar values are adjusted for inflation using the Personal Consumption Expenditures price index.

[8] https://inclusivegrowthscore.com/. According to their website, MasterCard offers "policymakers and other stakeholders high-frequency, granulated data of social and economic indicators at the neighborhood level in order to uncover and prioritize opportunities for revitalization and assist in helping to identify areas

of only a small subset of these input variables that are directly related to business growth and household spending. In particular, we use measures of the percentage growth of net new businesses based on anonymized and aggregated location point of interest data (that is, new credit card machines), commercial diversity (percentage of all industries that are represented in the census tract), the percentage growth of the number of small business loans, and two measures of spending growth (aggregate and per capita) based upon proprietary MasterCard data.[9] The data are annual from 2017-2022 but not every variable is available in each year. Table B.1 provides additional details on the MasterCard variables.

Table 1 presents summary statistics related to census tract characteristics and our outcome variables for each of the three groups of census tracts, focusing on the years immediately prior to the 2018 designation of OZs when possible. By design, ineligible census tracts 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, and lower rates of labor force participation. 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.

Although selected tracts are worse off, they were more likely to receive investment than eligible tracts that were not selected prior to the OZ policy. This could reflect a preference by governors to select census tracts that are more likely to benefit from the OZ tax incentive. At the same time, sale prices were lower both in raw and size-adjusted terms in selected tracts, potentially reflecting their economic deprivation. Notably, industrial investments comprised

in need of economic development."

[9]Spending includes all spending on MasterCard credit cards, debit cards, and pre-paid cards registered by businesses in the census tracts (not necessarily by residents of the census tract). The per capita measure takes the aggregate measure and divides it by the population of the census tract.

23.4% of all investments in selected tracts, substantially higher than the share in other types of tracts. Apartments comprised a larger share of investments in eligible tracts, and retail investments comprised a larger share of investments in ineligible tracts.

The MasterCard data on business activity and consumer spending present a mixed picture. Selected tracts have on average a lower new business growth rate than eligible but not selected or ineligible tracts, but at the same time, they have a similar growth rate of business loans to eligible but not selected tracts, a rate that is substantially higher than that of ineligible tracts. The average ranking of the growth rate of per-capita spending for selected tracts is a bit higher than the other two groups but that of aggregate tract-level spending is a bit lower.[10]

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. 2022), many of the selected tracts were chosen based on idiosyncratic factors that are unobserved to the econometrician. Multiple factors entered into the governors' selection criteria. In some states, governors sought geographic balance in their selections, with some states prioritizing a balance between rural and urban tracts. In other states, governors held a multi-step process whereby citizens could weigh in or preference was given to regions that were previously designated as high-priority areas and so were natural choices for OZ designation.

[10]MasterCard does not release its account-level proprietary spending data in raw form. Instead, it releases what it calls a "score" variable. This variable ranks from lowest to highest the growth in spending (per capita or aggregate) for each census tract within a state. Thus, the best we can do to capture (relative) improvements in spending is to measure improvements in (relative) ranking. Of course, this prevents us from making any more specific claims on actual growth rates.

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 selection may differ as well, with Frank et al. (2022) and Eldar and Garber (2020) finding that selected tracts were experiencing faster economic growth than eligible but not selected tracts prior to selection. In Figure 3, panels (a) and (b), we show the trends in total commercial investment for each of our census tract groups. For each group of tracts, the number and dollars of investments increased until about 2015, and then flattened out before dipping in 2020 in conjunction with the onset of the COVID-19 pandemic. Investments recovered in 2021 and again show a dip in 2022. However, the uptick in commercial investment in selected tracts exceeds that of other tracts prior to OZ selection in 2018. Selected tracts closely track ineligible tracts in terms of the number of investments until 2016, at which point investment in selected tracts starts to grow more quickly. Compared to eligible but not selected tracts—the key comparison group for difference in differences designs—selected tracts see the number of investments grow faster throughout the sample period, and faster growth in dollars of investment starting in 2013. To formalize these observations, in panels (c) and (d), we plot annual differences between selected and eligible but not selected tracts, relative to their difference in 2017. These panels confirm the patterns visible in the raw trends, violating the parallel trends assumption needed for the validity of the difference in differences methodology.

In order to overcome the non-random selection of OZs, we instead use a multivariate regression discontinuity (RD) approach that takes advantage of how the 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. The RD design relies on the qualification criteria for OZs—as noted earlier, census tracts were deemed

eligible for OZ selection if they met at least one of the following conditions:

- had a poverty rate of at least 20%, or
- had a median family income below 80% 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 had a median family income that does not exceed 125% of the median family income of at least one contiguous selected tract.[11]

In the RD design, we exploit the sharp poverty rate and income eligibility cutoffs.[12] Census tracts with poverty rates just below 20% or median family income just above 80 percent of the threshold are presumably 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").

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.[13]

One approach we adopt is to estimate separate specifications for each running variable, under the "frontier regression discontinuity" approach (see Appendix A). For example, we

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

[12] In his analysis of the impact of the NMTC on poverty and unemployment, Freedman (2012) focuses on only the income eligibility cutoff because the large majority of census tracts qualified based upon the income threshold.

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

focus first on the 20% poverty rate. In census tracts with median income above the 80% threshold, passing the 20% poverty rate threshold moves a census tract from ineligible to eligible. In contrast, if the census tract has median income below 80% of the threshold, passing the 20% poverty rate has no impact on OZ eligibility because being below the 80% income threshold, by definition, deems the census tract eligible. A similar idea applies to the median income threshold. In census tracts with a poverty rate below the 20% mark, passing from above the income threshold to below it moves a census tract from ineligible to eligible whereas passing this threshold in census tracts with poverty rates above 20% is irrelevant because the census tract is eligible regardless.

In Figure 4, we verify that crossing the poverty and income thresholds, when binding for determination of eligibility, leads to discrete increases in the probability of selection as an OZ. Groups of census tracts with approximately equal poverty rates are placed in bins using spacing estimators under the mimicking variance evenly-spaced method (Calonico et al. 2014), and we calculate the share that was selected as OZs. Poverty rates and implied probabilities of selection are plotted for each bin. Panel (a) considers only census tracts with median incomes below 80% of the MSA or state median. As seen in the figure, census tracts with poverty rates above 20% 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 eligibility (nor selection). Panel (b) considers only census tracts with median incomes above 80% of the MSA or state median. In this case, crossing over the 20% poverty rate threshold moves census tracts from ineligible to eligible and substantially increases the probability of being selected as an OZ. Census tracts with a slightly higher or slightly lower poverty rate than 20% are economically very similar, and so differences in outcomes between these two groups of tracts can be attributed to OZ eligibility, rather than other differences.

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% poverty rate and then using the 80% income threshold as the eligibility determinant. As shown in panel (c), passing the income threshold has no impact on OZ eligibility in census tracts with a poverty rate above 20%—census tracts on both sides of the income threshold are eligible. However, in census tracts with a poverty rate below 20%, passing from just above the income threshold to just below substantially increases the probability of being selected as an OZ (panel d).

An alternative 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, we construct the running variable r:

$$r_i \equiv \max\{\frac{P_i - 20}{20}, -\frac{I_i - 0.8 * I_m}{0.8 * I_m}\} \tag{1}$$

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 that contains census tract i.

Figure 5 displays the probability of selection as an OZ on both sides of the cutoff point for eligibility as determined by this combination running variable. Census tracts just above the cutoff point are substantially more likely to be selected as OZs compared to tracts just below the cutoff point. The probabilities are not zero below the cutoff point for three primary reasons: (1) 28 selected tracts that would not qualify based upon the cutoff criteria were nonetheless deemed eligible due to "technical corrections," (2) 49 selected tracts were eligible on the basis of the 2012-16 ACS but not the 2011-15 ACS, and (3) 197 selected tracts were eligible due to being contiguous with eligible tracts and have sufficiently low median income as described above.[14] Following Cattaneo et al. (2018), we test whether there is any

[14]It is unclear how Treasury determined these technical corrections. They could play a role in anecdotal

evidence of manipulation around the threshold of our combined running variable and find that we do not reject the null hypothesis of no systematic manipulation ($p > .94$). We also find no evidence for a discontinuity in tract-level employment outcomes and industry shares when crossing the eligibility threshold, providing further evidence for randomness around the threshold.[15]

In the analysis that follows, we use this combined running variable to estimate the impact of OZ eligibility on commercial investment and related commercial activity. Appendix B presents complementary results under the frontier regression discontinuity approach for each threshold separately.

In order to estimate the impact of OZ eligibility on investment and other economic outcomes, we estimate the following regression discontinuity model:

$$Y_i = f(r_i) + D_i\gamma + X_i\beta + \epsilon_i \tag{2}$$

$$D_i = 1[r_i \geq c] \tag{3}$$

where Y_i represents our outcomes of interest—number and dollars of investment or commercial activity in census tract i—typically specified as the change in the outcome variable before versus after the OZ provision went into effect; r_i is the running variable; f is a polynomial function; D_i is a binary indicator based on eligibility status; and X_i is the vector of observable, pre-determined census tract characteristics that are correlated with the outcome

stories of corruption in the OZ selection process. It is reassuring that these tracts based on technical corrections only represent around 0.6% of all selected tracts and our results are robust to excluding them from the analysis.

[15]Figure B.1 illustrates the histogram of the values of the combined running variable and the manipulation test using local polynomial density estimation. While not statistically significant, the distribution shows a drop in the fraction of census tracts that fall on the eligible side of the running variable, contrary to incentives. Figure B.2 plots employment outcomes and industry shares over the running variable and shows a discontinuity at the eligibility threshold.

of interest—labor force participation rate, employment to population ratio, unemployment rate and share of workers in construction, manufacturing and retail. Equation 3 reflects that census tracts with a value of the running variable r_i above the cutoff c are eligible to be selected as OZs. The dollars of investment outcome is winsorized at the 95th percentile within each year and investment type.

5 The impact of OZs on commercial investment

5.1 Regression discontinuity

We consider two primary commercial investment-related outcomes for our regression discontinuity analysis that identify the impact of OZ eligibility. These include the number and dollars of investment. We focus on changes in each outcome variable, taking the difference of average annual investment between the post-period (January 1, 2019–December 31, 2022) and the pre-period (January 1, 2014–December 31, 2017).[16] Differencing nets out any time-invariant census tract effects, which although is not necessary to obtain an unbiased estimate given our use of an RD design, can nonetheless increase precision.[17]

The impact of OZ eligibility (intent-to-treat) on the change in total commercial investment between the post and pre-period is shown graphically in Figure 6.[18] Panels (a) and (b)

[16]We exclude 2018 as the implementation was finalized over the first half of the year. Doing so overcomes any possible bias from investors knowing the identities of OZs and making investments in them before Treasury released the official list of OZs in July 2018. Our results are robust to including the first half of 2018 in the control period and the second half of 2018 in the post-period. We also show results for each year individually, allowing us to distinguish between effects before and after the COVID-19 pandemic.

[17]In addition, as we will discuss in Section 8, another place-based program called the New Markets Tax Credit existed continuously over this period. Thus, by differencing, we are also removing any level impact on our outcome variables from this program.

[18]Total is the sum of the three major categories here plus a fourth category of "other" that captures smaller categories of hotels and development sites.

present the results for all census tracts. There is no discernible jump at the discontinuity for either the number or dollars of investment, showing that investment is not higher in census tracts that are just eligible to be selected as OZs compared to those that are not eligible. We present parametric estimates in the upper left portion of each figure that confirm the graphical evidence. Table 2 repeats this information and also includes analogous RD estimates for each type of commercial investment, including industrial, office, retail, and other. All specifications include as control variables census tract-level measures of 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 pooled ACS. The estimated impacts of OZ eligibility are small and not statistically different from zero in any of the specifications. For example, OZ eligibility reduces the number of investments by 0.002 (0.6%) and decreases dollars of investment by \$0.103 million (2.4%).[19] While some results are unexpectedly negative, they all point to a near-zero impact of OZ eligibility on investment. Figures B.3 and B.4 show for total commercial investment that these null results are robust to variations in the fitting polynomial and bandwidth around the threshold, respectively. Figure B.5 shows similarly null results when using the individual income and poverty running variables without combining them into a single running variable.

We next consider two subsamples, urban tracts that make up about 75% of all census tracts within our bandwidth and investment that is specifically intended for redevelopment purposes, representing about 12% percent of all commercial investments within our bandwidth in the pre-period.[20] Results are presented in panels (c) through (f) of Figure 6. Urban

[19]Percentage effects are derived from a comparison to the average value of the ineligible group (left of cutoff) within the bandwidth throughout the paper.

[20]This is not to say that other transactions made for investment purposes would not also qualify under the OZ legislation but those designated as redevelopment properties are more obviously in line with OZ objectives.

tracts show no statistically significant impact of OZ eligibility with an increase of 0.013 (3.2%) in the number of commercial investments and a decrease of 0.027 (0.5%) in the dollars of investment. Similarly, we find no evidence that OZ eligibility increases investment in properties specifically designated as redevelopment projects, with a decrease of 0.005 (10.1%) in the number of commercial investments and a decrease of $0.164 million (37.0%) in investment. While redevelopment properties show substantial (though statistically insignificant) effects, they are in the opposite direction of what we may expect from the effect of OZ eligibility.

Returning to our results in panels (a) and (b), once we account for the standard errors around our estimated results, we can rule out at the 95 percent level of confidence estimates larger than 0.05 (15.4%) more investments, and an additional $1.3 million of investment (a 29.7% increase). While these upper bounds represent effects that are economically important, Figure 7 shows that they are typical of the variation in investment that we see across all years, particularly those prior to OZ implementation. Figure 7 repeats the same RD exercise for each individual year from 2010 through 2022 and for each category of commercial investment: industrial, office, retail and total. Thus, each black dot is the yearly estimate of the effect of OZ eligibility (or, in years prior to 2018, the effect of *future* OZ eligibility) at the threshold and the black vertical lines represent the 95% confidence interval. The outcome variable is the difference in investment in year t and investment in 2017. While there is some degree of variability from year to year in the RD point estimates, the important takeaway from this figure is that the pattern post-2018 is consistent with that prior to 2018, thus reflecting typical annual variation in commercial investment. It is also interesting to note that while the levels of investment were impacted by the COVID-19 pandemic (see Figure 3), there is nothing in the RD estimates to reflect differential impact of OZ eligibility. None of the

post-2018 RD estimates is statistically different from zero. Industrial investment prior to 2015 shows differences between eligible and non-eligible tracts, primarily in the dollars of investment, but this difference disappears by 2015. Dollars of office building investments is also statistically significant in 2016, presumably reflecting a random investment shock in a census tract or tracts that two years later will become just eligible for OZ designation.

5.1.1 Heterogeneity

The RD approach provides an estimate of the impact of OZ selection on investment locally around the cutoff point and allows us to conclude what would happen if the program were expanded on the margin but does not provide an overall assessment without an implicit assumption of homogeneous treatment effects across all tracts in the United States. The census tracts used to identify the effect of eligibility in our analysis are relatively better off in terms of poverty and relative median income than eligible tracts that are far from the cutoff points (see Table B.2 for census tract characteristics based on distance from the eligibility cutoff point). As such, one may imagine that because the census tracts in our RD sample are more attractive for investors than those facing more extreme levels of economic distress, they would likely have larger treatment effects. In fact, using proprietary IRS data, Kennedy and Wheeler (2022) document that only about 37% of selected tracts received positive investment by the end of 2020 from QOFs and that these tracts are better off in terms of observable characteristics (levels and growth rates) such as education, median home value, and median household income compared to selected OZ tracts that received no investment. Such tracts are more likely to be closer to the eligibility thresholds.

In addition, most investment activity in general occurs near the thresholds, and so treatment effects in tracts far from the thresholds are likely to be less relevant for place-based poli-

cies seeking to encourage private investment. Figure 8 shows the distribution of investment (number of investments and dollars of investment) over all census tracts in the pre-period (January 2014 through December 2017). As indicated by the brighter-colored pixels, these heat maps show that the majority of investment (62% of the number of transactions and 66% of dollars) is undertaken in census tracts that are ineligible (top left quadrant). Eligible tracts relatively near the poverty and income thresholds account for most of the remaining investment, with 25% of all transactions and 24% of dollars of investment undertaken in tracts within 5 percentage points of the poverty threshold or 20% of the income threshold.[21] The remaining 13% of transactions and 11% of dollars of investment are undertaken further out from the poverty and income thresholds, as indicated by the darker color pixels in the figure. As such, the null investment effects we estimate apply to those eligible census tracts where most investment tends to occur in the absence of OZ tax incentives. This lends support to the view that our results capture an upper bound of the average eligibility effect over all eligible tracts.

We directly test whether worse-off tracts (within the bandwidth of tracts near the thresholds) have stronger investment effects of OZ eligibility. Figure 9 reports the impacts of OZ designation by exploiting the income eligibility condition, but separately considering tracts with (i) poverty rates between 0 and 10 percent, and (ii) poverty rates between 10 and 20 percent. This breakdown allows us to perform a type of heterogeneity analysis by testing whether tracts that are further away from the poverty threshold (i.e., in the 0–10 percent range), yet within the bandwidth for the income threshold systematically respond differently to OZ eligibility than those closer to the poverty threshold (i.e., in the 10–20 percent range). We repeat our baseline RD analysis comparing the pre- and post-periods on these two sub-

[21] Or, conditional upon eligibility, 66% of all transactions and 69% of dollars of investment occur relatively near the poverty and income thresholds.

samples. As Figure 9 shows, both cases again show no statistically significant impact of OZ eligibility on investment. Within each outcome variable, estimates for each subsample are not statistically different from one another at conventional levels. The data are much thinner in the 0 to 10 percent poverty range and this is reflected in the noisier estimates. These results provide no evidence that OZ eligibility effects would be significantly different further away from the combined running variable threshold.

5.1.2 Spillover effects

One potential concern for the lack of any robust impact on investment is spillover effects. It is possible that when investment flows into OZs, the (expected) economic development generated by such investment may also incentivize investment in nearby, but non-selected areas. There is an important distinction to make here. If spillovers mainly occur in nearby eligible but not-selected tracts, our RD results would be biased upwards as this would increase the amount of overall investment that occurs just over the eligibility threshold of the running variable. Thus, to the extent that our findings are potentially upwardly biased by spillovers, this would strengthen our conclusion that the OZ legislation did not lead to significant increases in commercial investment. Alternatively, if spillovers occur in nearby *ineligible* census tracts then this could potentially invalidate the RD design. In order to address this concern, we focus on a subsample of tracts that minimizes the extent of contiguity between eligible and ineligible tracts by restricting to counties that contain only one type of tract. In other words, we exclude all counties that contain a mix of eligible and ineligible tracts and replicate our analysis on the roughly 2,000 census tracts that remain after this restriction. Thus, the ineligible census tracts just below the cutoff exist in counties without any eligible tracts and likewise, eligible census tracts that are just above the cutoff exist in counties

without any ineligible tracts, thus minimizing any spillovers.[22] The final row of Figure 9 shows that the point estimates are noisier due to the smaller sample size, not materially different than our baseline estimates, nor statistically different than zero at conventional levels.

6 The impact of OZs on business activity and consumer spending

Despite the consistent lack of positive and statistically significant impacts on commercial investment, we also estimate whether there are general improvements in other measures of economic development such as outcomes related to business formation and consumer spending. While the lower bound of $2.5 million per transaction to appear 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 to increased economic activity that may have occurred absent new investment. The MasterCard data address this gap by providing census tract-level aggregates of business-related activities and proprietary data on consumer spending as described in Table 1.

Figure 10 presents graphical evidence on the impact of OZ eligibility on business-related activities and consumer spending over the post-period of 2019-2022. The figure shows no discernible jumps in any of our outcome variables upon crossing the eligibility threshold.[23] The parametric point estimates are not statistically significant at conventional levels, further

[22] We recognize that this does not eliminate the potential spillover problem of adjacent eligible and non-eligible counties that happen to fall on either side of the threshold. However, the adjacency of individual eligible and non-eligible census tracts occurs less frequently within the subsample used here.

[23] Note that in unreported analysis, the COVID-19 pandemic appears to not have had any differential impact on OZ in terms of the economic activity variables considered here.

supporting the RD figures. In particular, OZ eligibility increases new business growth in the post period by 0.067 percentage points (1.0%), increases business loan growth by 0.25 percentage points (3.1%), increases commercial diversity by 0.23 percentage points (1.1%), improves the relative ranking of eligible census tracts in terms of per-capita spending growth by 0.52 percentage points (1.0%), and reduces overall spending growth by 0.04 percentage points (0.1%).

Taken as a whole, the results point to a near-zero impact of OZ eligibility on business activity and consumer spending. Upon taking into account the standard errors around our point estimates, we can rule out at the 95 percent level of confidence estimates larger than 0.94 percentage points (14.1%) for new business growth, 1.32 percentage points (16.5%) for business loan growth, 0.88 percentage points (4.1%) for commercial diversity, 1.6 percentage points (3.2%) in the relative ranking of spending growth per-capita and 1.0 percentage points (2.1%) in aggregate census tract level spending growth. While new business and small business loan growth may offer some foreshadowing of future positive business growth, these upper bounds point to otherwise a modest impact on business activity and consumer spending outcomes.

7 The impact of OZs on multifamily housing investment

Until now, we have focused our analysis on commercial investment—that is, investment in office, retail and industrial properties—as the stated motivation for the legislation was to spur economic growth and job creation in low-income communities by providing tax benefits to investors. As previously mentioned, the legislation takes a broad approach to the types

of property that qualify, including for residential properties. We repeat our baseline analysis but now replace commercial investment with multifamily housing investment as our outcome variable, which is captured in the RCA data.

We begin by repeating the difference in differences analysis. Similar to the commercial investment figures, Figure 11 shows differing trends in multifamily housing investment throughout the analysis period. From the raw trends in subfigures (a) and (b), it is apparent that the number of investments and dollars of investment in multifamily housing have increased over time, with an especially large increase in 2021 that faded somewhat in 2022. Subfigures (c) and (d) show that these trends in investment differed across tract types, violating the parallel trends assumption required for a difference in differences design. In particular, selected tracts experienced faster growth in multifamily housing investment than eligible but not selected tracts in the period leading up to the designation of OZs in 2018. As was the case for commercial investment, this motivates the RD design that requires the weaker assumption of randomness around the eligibility threshold.

Figure 12 shows the effect of passing the eligibility threshold on the number of investments and dollars of investment, for multifamily housing. In subfigures (a) and (b), the outcome variable is the difference in annual average investment between the pre-period (2014-2017) and post-period (2019-2022). The number of investments in multifamily housing falls by 0.002 (1.7%) and dollars of investment increases by \$0.175 million (6.4%), though, neither is statistically significant at conventional levels. We repeat the same robustness checks, heterogeneity analysis, and spillover tests applied to the commercial investments, and similarly find no evidence for increased multi-family housing investment based on these alternative specifications.[24]

[24]Figure B.6 presents results using different orders of polynomials. Figure B.7 presents results using different bandwidths. Figure B.8 presents results using the poverty and income running variables individually.

We next consider the effect on multifamily housing investment separately in each year. Given previous research finding potential effects of OZs on home prices soon after OZs were designated, as well as the surge in housing demand in 2021 and 2022, it is important to consider that any OZ effect on multifamily housing investment may have evolved over time. Subfigures (c) and (d) of Figure 12 show statistically significant effects on multifamily housing investment in 2018, both in terms of number of investments and dollars of investment. OZ eligibility increased the number of investments by 0.05 (39%) and dollars of investment increased by $1.7 million (51%) in 2018. The effect dissipated in 2019 and 2020 but then started to reemerge in 2021 and especially 2022, when OZ eligibility increased the number of investments by 0.02 (16%) and dollars of investment by a statistically significant $1.4 million (29%).

8 Discussion

In the almost five-year period since Opportunity Zones were implemented, we generally could not detect a positive impact on commercial investment—across industrial, office, and retail sectors. Nor could we detect a positive impact on related economic outcomes corresponding to business formation and consumer spending. However, there are potentially important effects on investment in multi-family housing, both immediately after OZs were designated in 2018 and again during the broader surge in housing demand following the COVID-19 pandemic.

Effects on multi-family housing are consistent with residential investment being the most common investment by Qualified Opportunity Funds (Sciarretti 2023). Our results suggest

Figure B.9 presents results testing for heterogeneity on the basis of different poverty rates and for spillover effects.

that the documented investment by Qualified Opportunity Funds in multi-family housing would not have entirely occurred otherwise. However, we find no evidence that increased investment in multi-family housing has led to increased investment in other sectors, limiting the potential for OZs to spur broad economic development. The limitation of positive investment response to multi-family housing also calls into question whether marginal investment caused by OZs is affecting the most distressed areas. Areas in economic decline typically face an excess of existing housing with high vacancy rates, given the durable nature of the housing stock and a declining population (Glaeser and Gyourko 2005). Thus, we should not expect investors to invest heavily in developing housing in areas facing economic decline. This should raise concerns that the investment response caused by the OZ policy is not taking place in the most distressed areas.

It is informative to place these results for OZs into the context of the broader literature that evaluates place-based policies attempting to address geographic disparities. Major previous federal efforts include Empowerment Zones and the New Markets Tax Credit (NMTC). Empowerment Zone programs generally offer tax incentives for businesses that locate in specified areas or that 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 on employment and economic well-being 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 NMTC, a component of the Community Renewal Tax Relief Act of 2000. Like the OZ provision, the NMTC focuses on census tracts that have relatively low incomes and high poverty rates and offers tax incentives for investments made in these areas.[25] However, unlike the OZ provision, tax credits are limited each year. Also, all investments must be made through community development entities that are approved by the U.S. Treasury. Gurley-Calvez et al. (2009) find that the NMTC leads to additional investment that would not have otherwise been made. Using telephone and online survey data from key participants in NMTC investments, Abravanel et al. (2013) find that about 30-40 percent of investment projects would not have happened without NMTC funding. In terms of outcomes, Freedman (2012) finds modest impacts on increasing home values and reducing both poverty and unemployment, although some of the effects 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. In sum, the results of previous place-based policies are mixed.

Unlike previous attempts at improving the economic health of distressed areas, OZ rules

[25]Estimated impacts of OZs could be biased upward due to the NMTC, which uses the same general eligibility conditions as OZs. Thus, any positive outcomes could be a result of OZs, NMTC, or a combination. Notably, estimates from difference in differences approaches would be biased in an unknown direction, since both selected and non-selected (but eligible) tracts can receive NMTC investment.

provide wide flexibility in the type of investment and do not cap the amount of investment that is subject to tax-preferred treatment. OZs thus provide unrestricted incentives for private investment that stand in contrast to the more highly vetted investments authorized in earlier programs that operated on a smaller scale. While the increased flexibility and uncapped funding of OZs could potentially increase the scale of effects, they could also lead to greater subsidies for investment projects that would have occurred in the absence of OZ tax incentives or increase investment that is less likely to improve the targeted outcomes among residents of distressed areas. Our results are consistent with this latter possibility, with the investment response we observe relegated to multi-family housing that is less likely to translate into broad economic development in the most distressed areas.

A focus on differences across place-based policies can help inform the design features that are more likely to increase investment and improve downstream outcomes. Two distinguishing factors of the NMTC are that investments are capped and must be approved by authorities. These factors may be successful in spurring investment that would not have otherwise occurred. The much greater degree of flexibility for OZ investment may do less to spur marginal investment. As our study is not designed to identify the specific aspects of OZs that reduced their effectiveness, future research should attempt to shed further light on this question. Nonetheless, our results are consistent with the idea that multi-family housing may have offered the safest way to comply with the tax legislation's rules and therefore may see increased investment, despite its limitations for broadly serving the most distressed areas and their residents.

While on average we find small and statistically insignificant commercial investment effects on industrial, office, and retail properties, it is important to emphasize that our results speak to the average effect of OZ eligibility on investment and economic activity on

the margin—it is possible that OZ designation had substantial positive effects in some subset of tracts but that these positive effects were outweighed by zero effects in a substantially larger number of other tracts. While we show that the large majority of investment in eligible tracts occurs near the eligibility threshold, the smaller share of tracts further away from the eligibility threshold may have been affected differently. To the extent that OZs were successful in driving investment to these tracts, this would suggest that the broad eligibility criteria for OZs could be hindering the overall success of the program.

Another potential contributing factor to the overall lack of commercial investment effects could be that persistent distress in many OZs makes them poor candidates for new investment regardless of tax incentives. Figure 13 breaks down census tracts into whether each is (a) persistently poor (poverty rate greater than 20% in both the 1980 Census and the 2011-2015 ACS); (b) newly poor (poverty rate less than 20% in the 1980 Census and greater than 20% in the 2011-15 ACS); (c) turned around (poverty rate greater than 20% in the 1980 Census and less than 20% in the 2011-15 ACS); and (d) never poor (poverty rate less than 20% in both the 1980 Census and the 2011-15 ACS). There are two key takeaways from this figure. First, over 45% of selected OZs are persistently poor, a higher fraction than eligible but not selected tracts (16 percent persistently poor). Thus, it is not simply that selected OZs are more economically distressed based upon the 2011-15 ACS (as we saw in Table 1) but they are more likely to be economically distressed for the decades leading up to the OZ legislation as well. Second, it is striking as to how few "turned around" tracts there are across all three groups. Less than 2.5% of census tracts that were poor in 1980 were no longer considered poor by 2015. This latter fact is somewhat discouraging given previous efforts with place-based policies and may provide insight into the fact that even providing generous tax-preferred incentives for more dollars to flow into these communities may be

insufficient to change the expected economic environment on a large scale that would lead investors to change their decisions.

The effect of OZs on investment through the end of 2022—almost five years since OZs were officially designated and just over four years since Treasury provided important guiding rules in October 2018—suggests that strong downstream impacts on OZ residents may not be forthcoming. This is all the more true given the financial incentives to invest were strongest when investment occurred prior to December 31, 2019. No clear effect on investment, except in multi-family housing, suggests that the natural channel through which employment and wages could rise is unlikely to materialize. This is exactly what Freedman et al. (2023) find—no statistically significant impact of OZ selection on employment, wages, and poverty rates once controlling for pre-trend differences between selected and eligible but not selected tracts, at least in the early period after OZs were designated.

9 Conclusion

The persistence of economic disadvantage in some areas of 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 nearly five years of existence, there is no strong evidence that OZ tax incentives have significantly increased commercial real estate investment in distressed areas that would ultimately lead to improved economic conditions. There are some indications that residential multi-family housing may be impacted but this may do less to spur job creation than some other types of investment, especially in areas in decline. Similarly, business-related outcomes such as growth in new business formation, new business loans, commercial diversity, and

consumer spending have not substantially changed in distressed areas as a result of the OZ policy. These findings are based on a multivariate regression discontinuity design that addresses potential biases of previous studies that rely on difference in differences approaches. Our results speak to the difficulties in designing policies that both encourage efficient and useful investments in low socioeconomic areas that work to improve these areas and actually attract the needed funds. 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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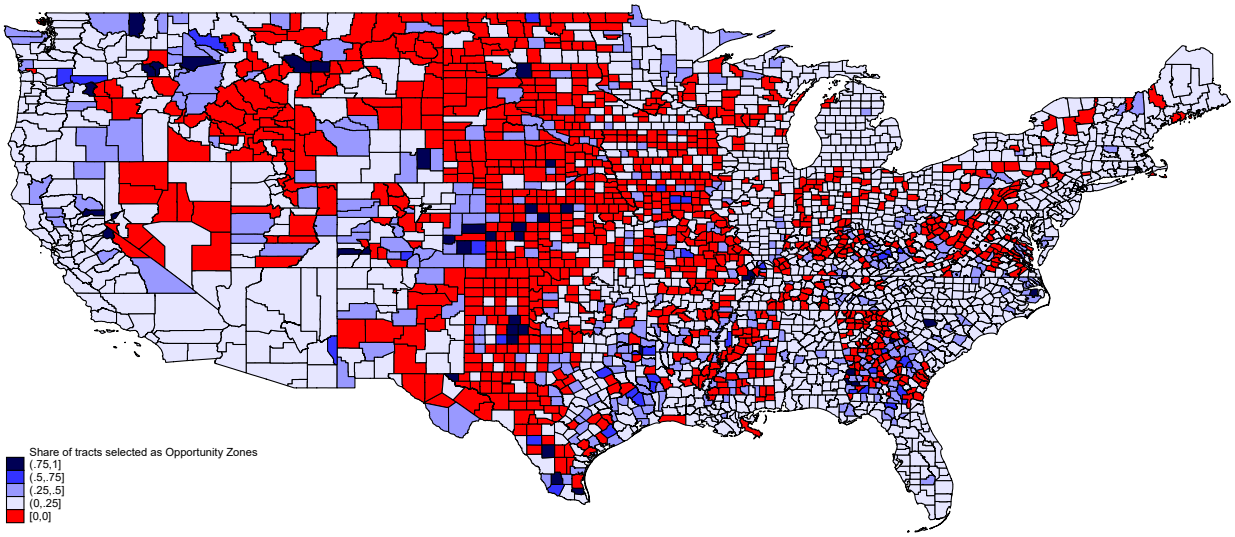
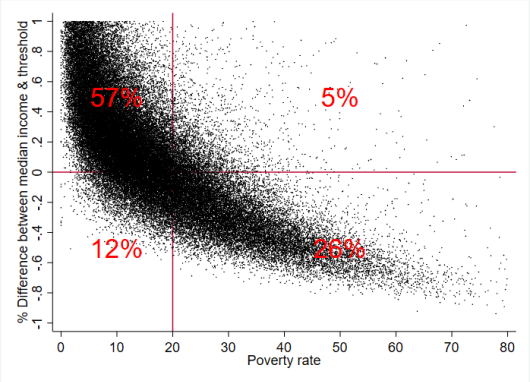
Figure 1: Share of census tracts designated as OZs by county in the continental United States

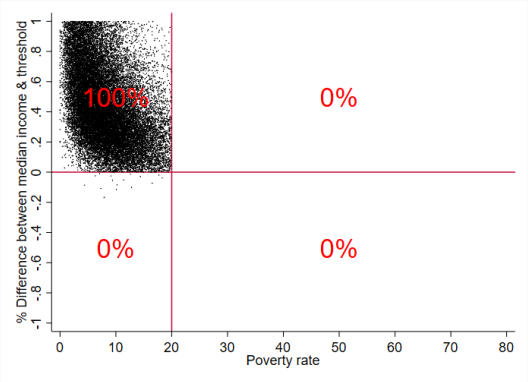


Source: U.S. Department of the Treasury; Authors' calculations
Notes: Map denotes unweighted share of census tracts in each county that were designated and confirmed by Treasury as Opportunity Zones in 2018.

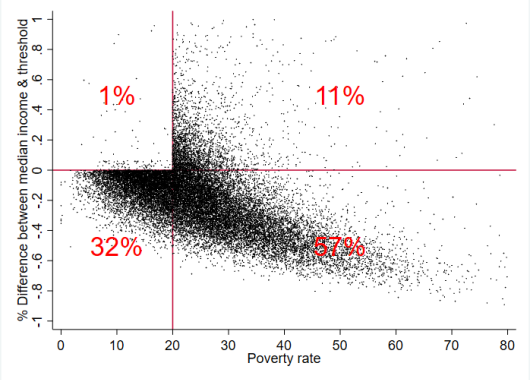
Figure 2: Poverty rate and percent difference between median income and threshold for each census tract, 2011-2015, by census tract eligibility and selection as OZ



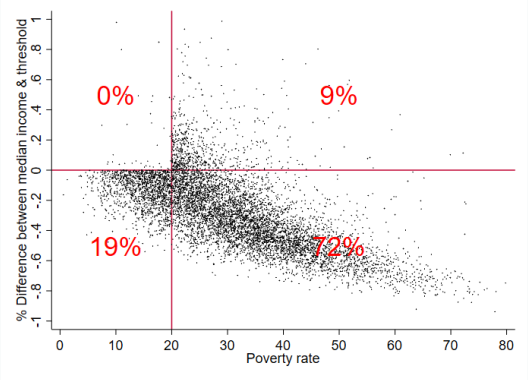
(a) All census tracts



(b) Ineligible census tracts



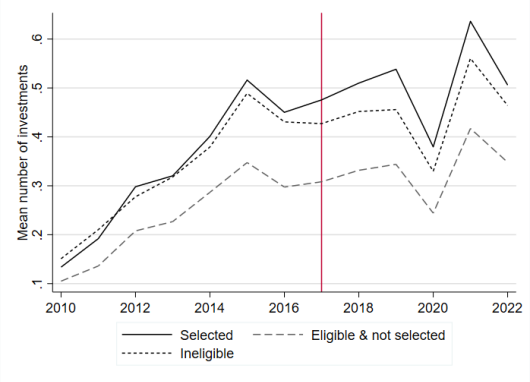
(c) Eligible but not selected census tracts



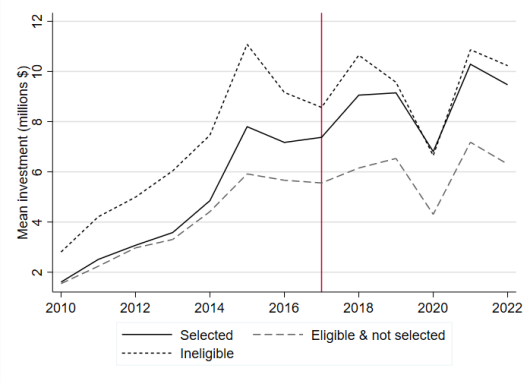
(d) Selected census tracts

Sources: American Community Survey, 2011-2015 5-year pooled sample; U.S. Department of the Treasury.
Notes: Each census tract in the United States is represented as a single dot, plotted according to its poverty rate and the percent difference between its median family income and the median family income of either the metropolitan statistical area or the broader state (if the tract is not in a metropolitan statistical area). Tracts eligible based only on contiguity with eligible tracts are excluded from the figures in panels (c) and (d). 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. A total of 71,671 census tracts are shown.

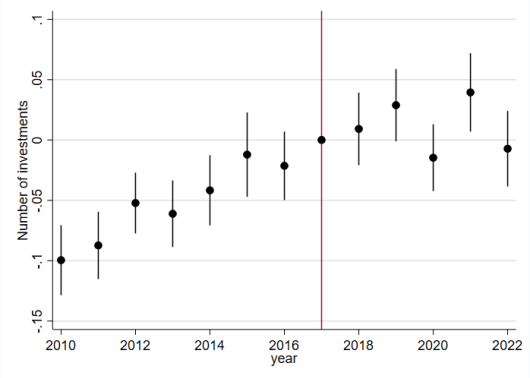
Figure 3: Mean number and dollars of commercial investments by census tract type and difference across census tract type, 2010-2022



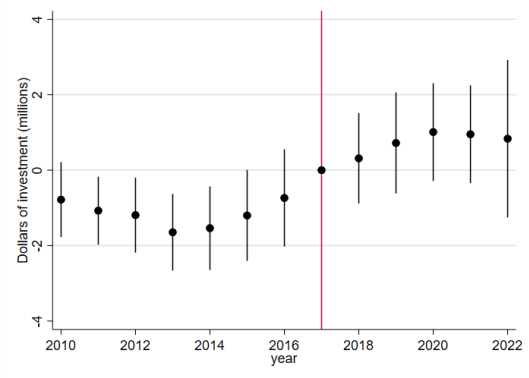
(a) Number of investments



(b) Dollars of investment



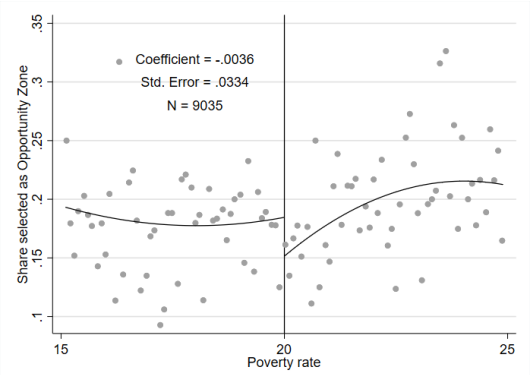
(c) Difference in number*



(d) Difference in dollars*

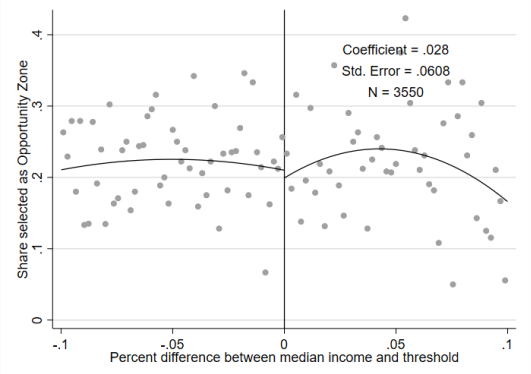
Sources: Real Capital Analytics; American Community Survey, 2011-2015 5-year pooled sample; U.S. Department of the Treasury.
Notes: Number of investments is number of commercial investments made during the year, and dollars of investment is the sum of all dollars of commercial investment (in millions) made during the year. Dollars values are adjusted for inflation using the Personal Consumption Expenditures price index. The vertical line at 2017 represents the last year prior to designation of Opportunity Zones. *In subfigures (c) and (d), the black dots represent the difference in investment between tracts selected as Opportunity Zones and tracts eligible but not selected as Opportunity Zones, relative to the difference in 2017. The black vertical lines centered at each black dot represent the 95 percent confidence interval corresponding to each difference in differences point estimate.

Figure 4: Share of census tracts designated as OZs in 2018 by 2011-2015 poverty rate and percent difference between median income and threshold

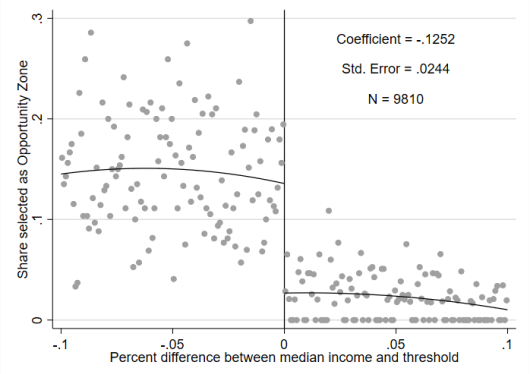


(a) Tracts with median income below 80% of MSA/state median income

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



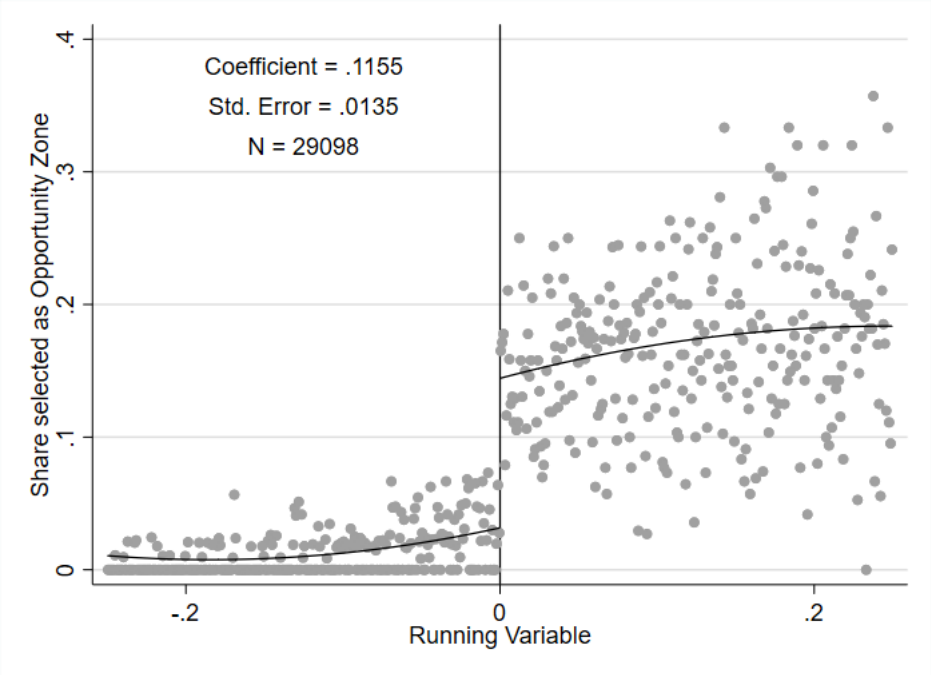
(c) Tracts with poverty rate above 20%



(d) Tracts with poverty rate below 20%

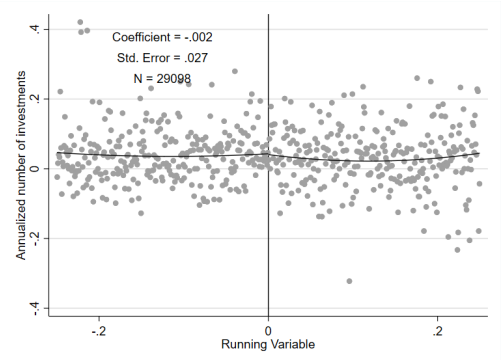
Sources: American Community Survey 2011-2015 5-year pooled sample; U.S. Department of the Treasury.
Notes: Each dot represents the sample average within each bin. Fitted lines are based on a polynomial of degree 2 fitted separately to points on either side of the cutoff. Coefficient represents the effect of crossing the eligibility threshold on the probability of being selected as an Opportunity Zone. Standard error and number of observations in the bandwidth are reported. The eligibility threshold is not binding in subfigures (a) and (c). The eligibility threshold is binding in subfigures (b) and (d).

Figure 5: Share of census tracts designated as OZs by constructed running variable

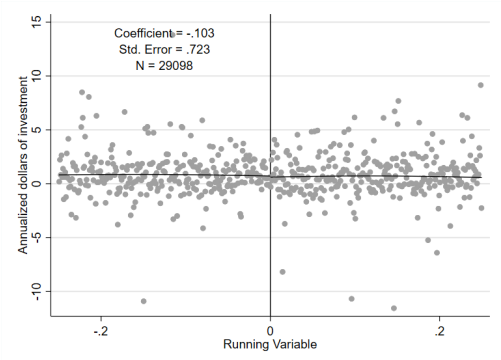


Sources: American Community Survey 2011-2015 5-year pooled sample; U.S. Department of the Treasury.
Notes: Each dot represents the sample average within each bin. Fitted lines are based on a polynomial of degree 2 fitted separately to points on either side of the cutoff. Running variable combines the poverty rate and median income eligibility conditions as defined in the text. The coefficient represents the effect of crossing the eligibility threshold on the probability of being selected as an Opportunity Zone. Standard error and number of observations in the bandwidth are reported.

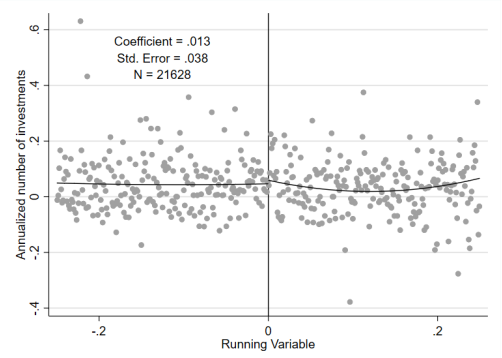
Figure 6: Total commercial investment, change from 2014-2017 to 2019-2022



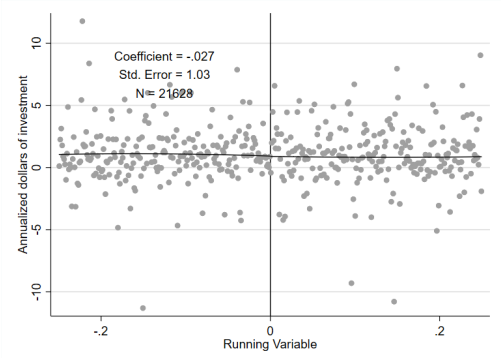
(a) All census tracts: Number of investments



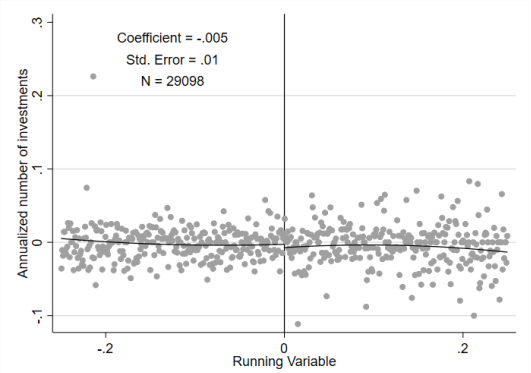
(b) All census tracts: Dollars of investment



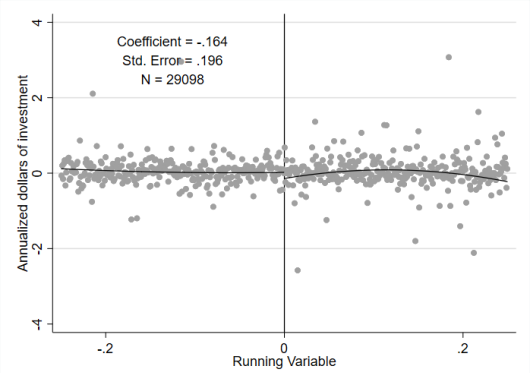
(c) Urban census tracts: Number of investments



(d) Urban census tracts: Dollars of investment



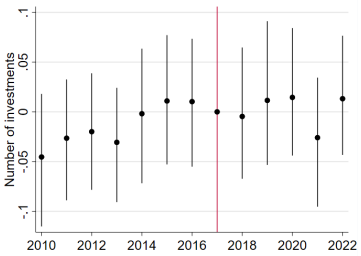
(e) Redevelopment properties: Number of investments



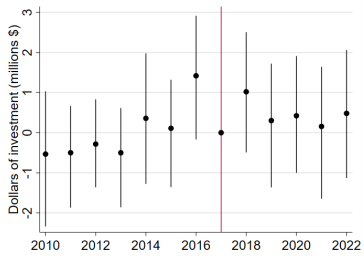
(f) Redevelopment properties: Dollars of investment

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 2 fitted separately to points on either side of the cutoff. Running variable combines the poverty rate and median income eligibility conditions as defined in the text. Coefficient represents the effect of crossing the eligibility threshold on the annual average number or dollars (in millions) of commercial investment. Standard error and number of observations in the bandwidth are reported. Dollars values are adjusted for inflation using the Personal Consumption Expenditures price index.

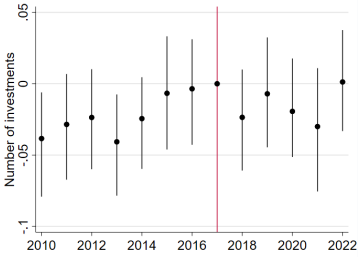
Figure 7: Annual RD estimates of the effect of OZ eligibility on the number and dollars of commercial investments in census tracts, by investment type



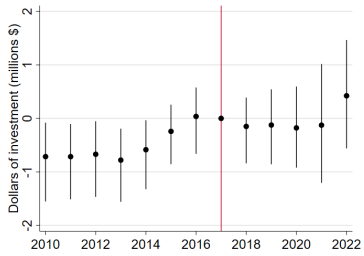
(a) Total (number)



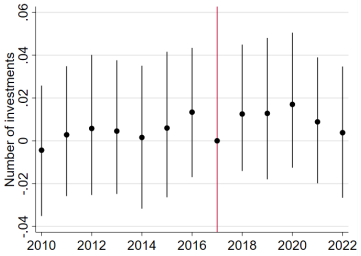
(b) Total (dollars)



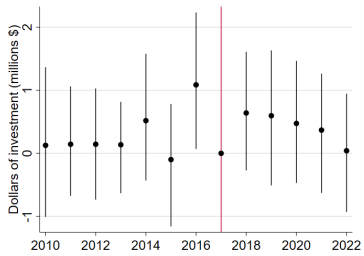
(c) Industrial (number)



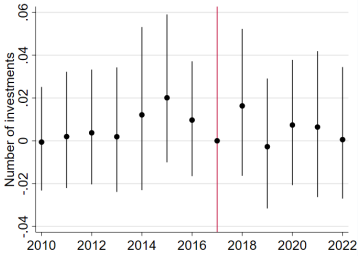
(d) Industrial (dollars)



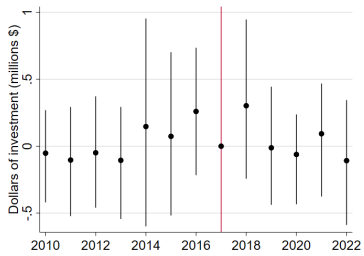
(e) Office (number)



(f) Office (dollars)



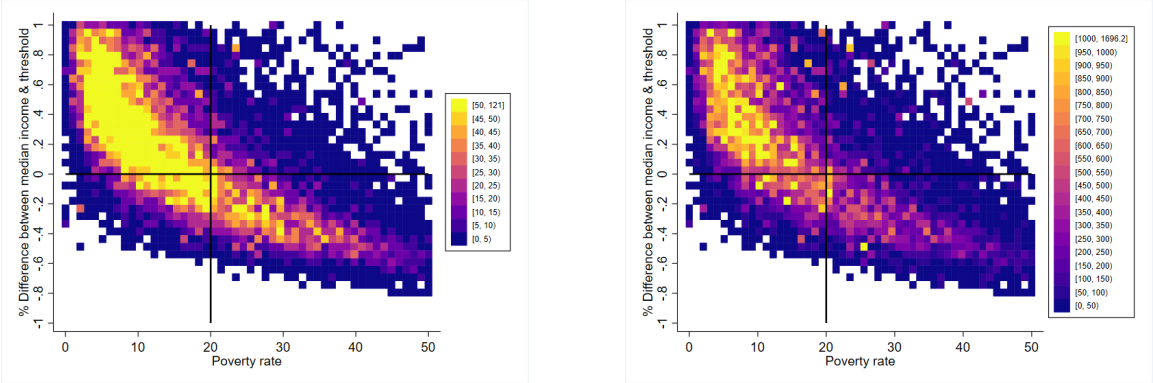
(g) Retail (number)



(h) Retail (dollars)

Sources: American Community Survey 2011-2015 5-year pooled sample; U.S. Department of the Treasury.
Notes: Each dot represents the estimated impact of Opportunity Zone eligibility on the number of investments or dollars of investment (in millions) in each year relative to investment in 2017. Vertical black lines represent the 95 percent confidence interval corresponding to each point estimate. Dollars values are adjusted for inflation using the Personal Consumption Expenditures price index.

Figure 8: Distribution of number and dollars of commercial investment by poverty rate and percent difference between median income and threshold, 2014–2017

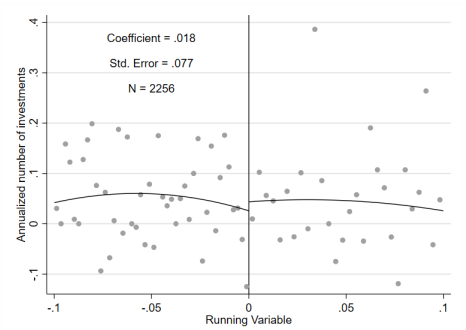


(a) Number of investments

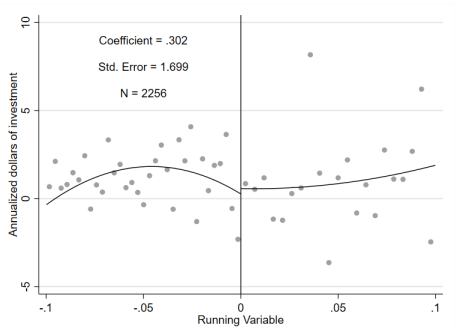
(b) Dollars of investment (millions)

Sources: American Community Survey, 2011-2015 5-year pooled sample; U.S. Department of the Treasury; Real Capital Analytics.
Notes: Values in the figure legends correspond to all census tracts within a given rectangle of the grid. There are 200 rectangles in each figure, each spanning 1 percentage point for the poverty dimension and 5 percent for the median income dimension. Brighter shading indicates more investment. Dollars values are adjusted for inflation using the Personal Consumption Expenditures price index.

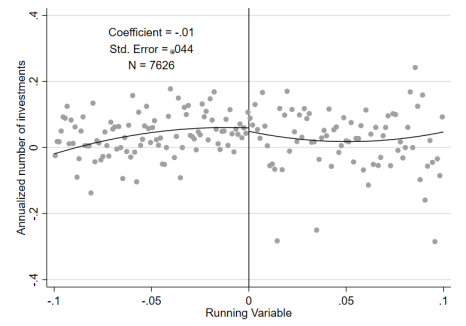
Figure 9: Total commercial investment by running variable, change from 2014-2017 to 2019-2022, various samples



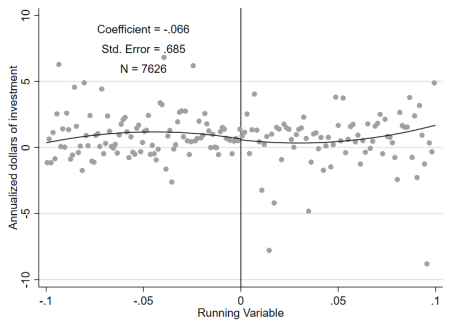
(a) Number of investments: Income running variable, poverty rate between 0% and 10%



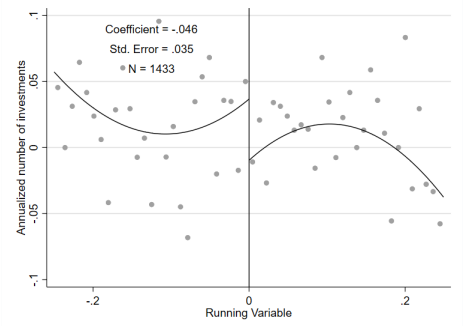
(b) Dollars of investment: Income running variable, poverty rate between 0% and 10%



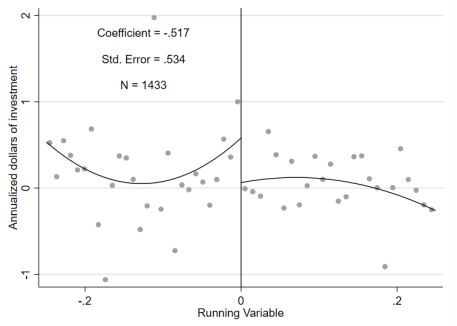
(c) Number of investments: Income running variable, poverty rate between 10% and 20%



(d) Dollars of investment: Income running variable, poverty rate between 10% and 20%



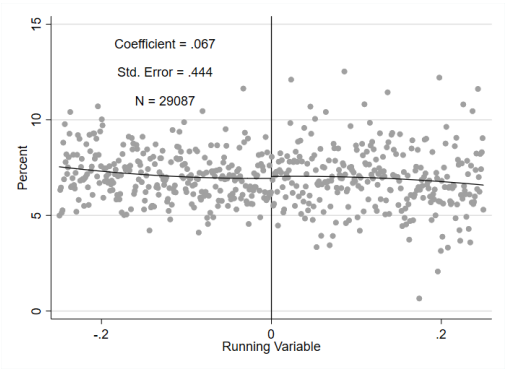
(e) Number of investments: Uniform counties



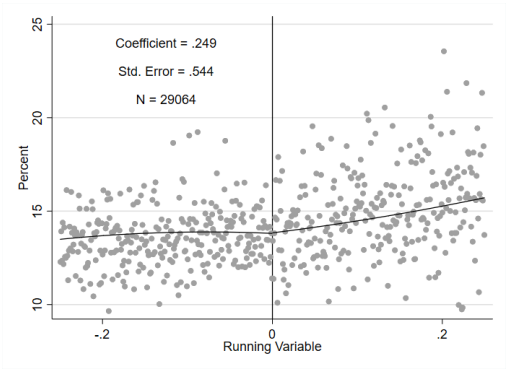
(f) Dollars of investment: Uniform counties

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 2 fitted separately to points on either side of the cutoff. Running variable in subfigures (e) and (f) combines the poverty rate and median income eligibility conditions as defined in the text. Subfigures (e) and (f) include only tracts in uniform counties, defined as counties which contain only eligible tract or only ineligible tracts. Coefficient represents the effect of crossing the eligibility threshold on the annual average number or dollars (in millions) of commercial investment. Standard error and number of observations in the bandwidth are reported. Dollars values are adjusted for inflation using the Personal Consumption Expenditures price index.

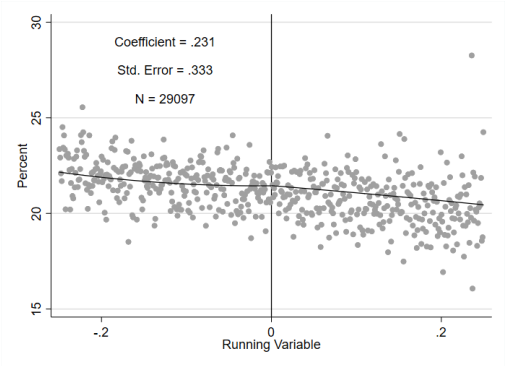
Figure 10: Economic activity outcomes from MasterCard business data by running variable, 2019-2022



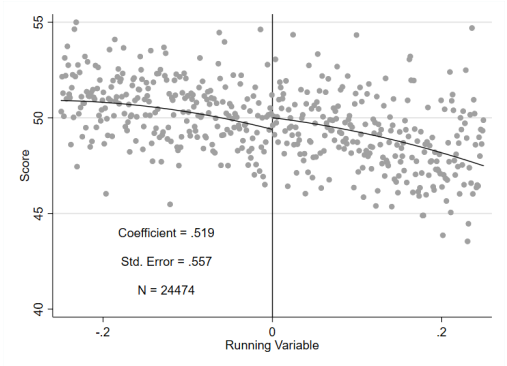
(a) New business growth



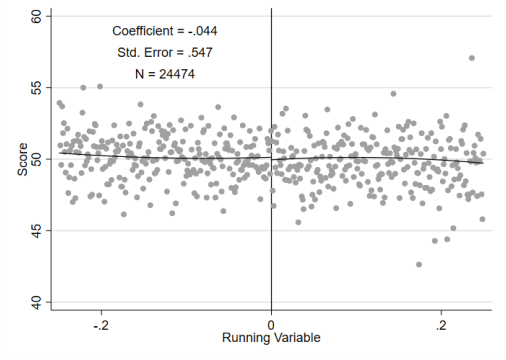
(b) Business loan growth



(c) Commercial diversity



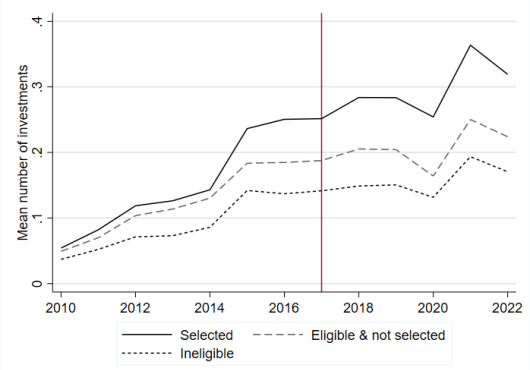
(d) Spending growth per capita



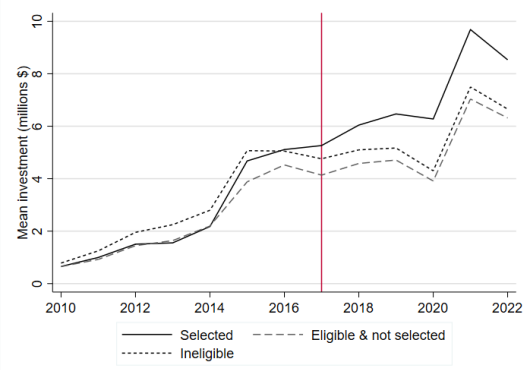
(e) Spending growth

Sources: Mastercard Center for Inclusive Growth, 2017-2020
Notes: Each dot represents the sample average within each bin. Fitted lines are based on a polynomial of degree 2 fitted separately to points on either side of the cutoff. Running variable combines the poverty rate and median income eligibility conditions as defined in the text. Coefficient represents the effect of crossing the eligibility threshold on the 2019-2022 mean of each outcome variable. Standard error and number of observations in the bandwidth are reported.

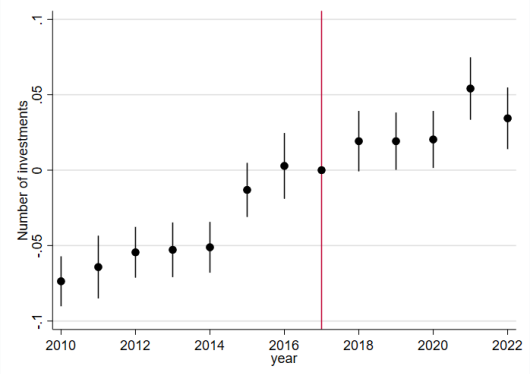
Figure 11: Mean number and dollars of multi-family housing investments by census tract type and difference across census tract type, 2010-2022



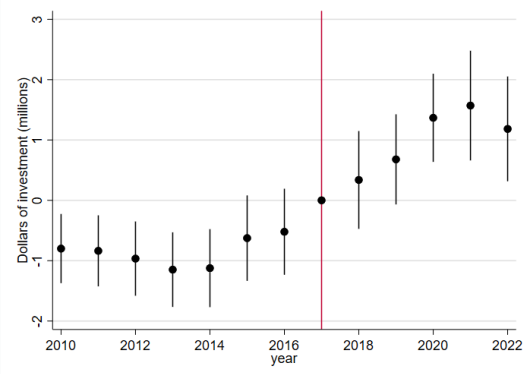
(a) Number of investments



(b) Dollars of investment



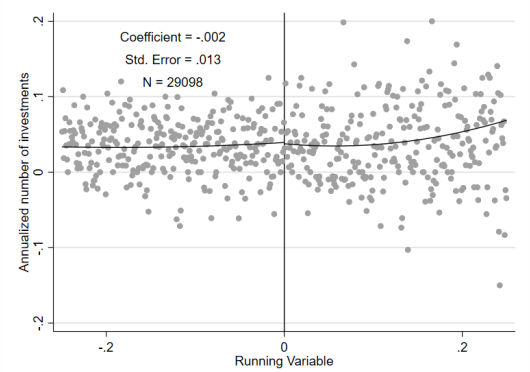
(c) Difference in number*



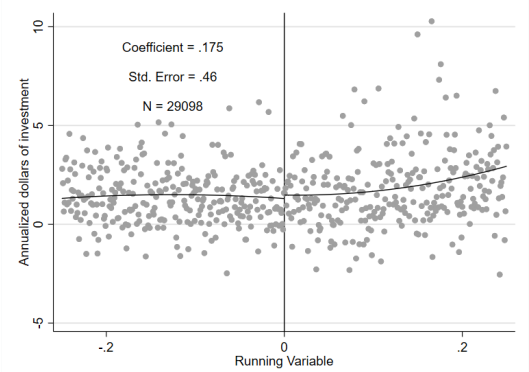
(d) Difference in dollars*

Sources: Real Capital Analytics; American Community Survey, 2011-2015 5-year pooled sample; U.S. Department of the Treasury.
Notes: Number of investments is number of multi-family housing investments made during the year, and dollars of investment is the sum of all dollars of multi-family housing investment (in millions) made during the year. Dollars values are adjusted for inflation using the Personal Consumption Expenditures price index. The vertical line at 2017 represents the last year prior to designation of Opportunity Zones. *In subfigures (c) and (d), the black dots represent the difference in investment between tracts selected as Opportunity Zones and tracts eligible but not selected as Opportunity Zones, relative to the difference in 2017. The black vertical lines centered at each black dot represent the 95 percent confidence interval corresponding to each difference in differences point estimate.

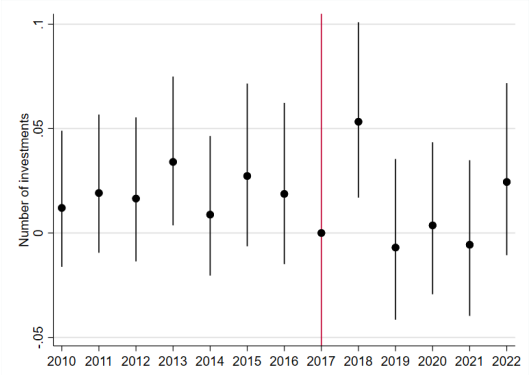
Figure 12: RD estimates of the effect of OZ eligibility on investment in multi-family housing



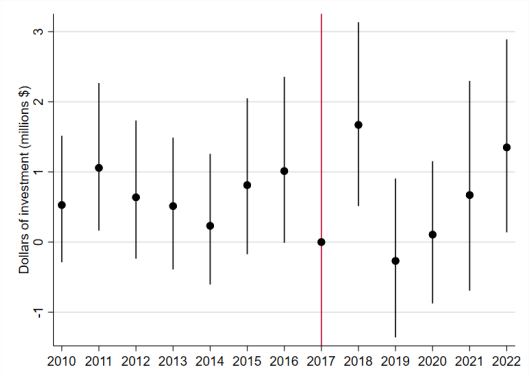
(a) 2019-2022 relative to 2014-2017: Number of investments



(b) 2019-2022 relative to 2014-2017: Dollars of investment



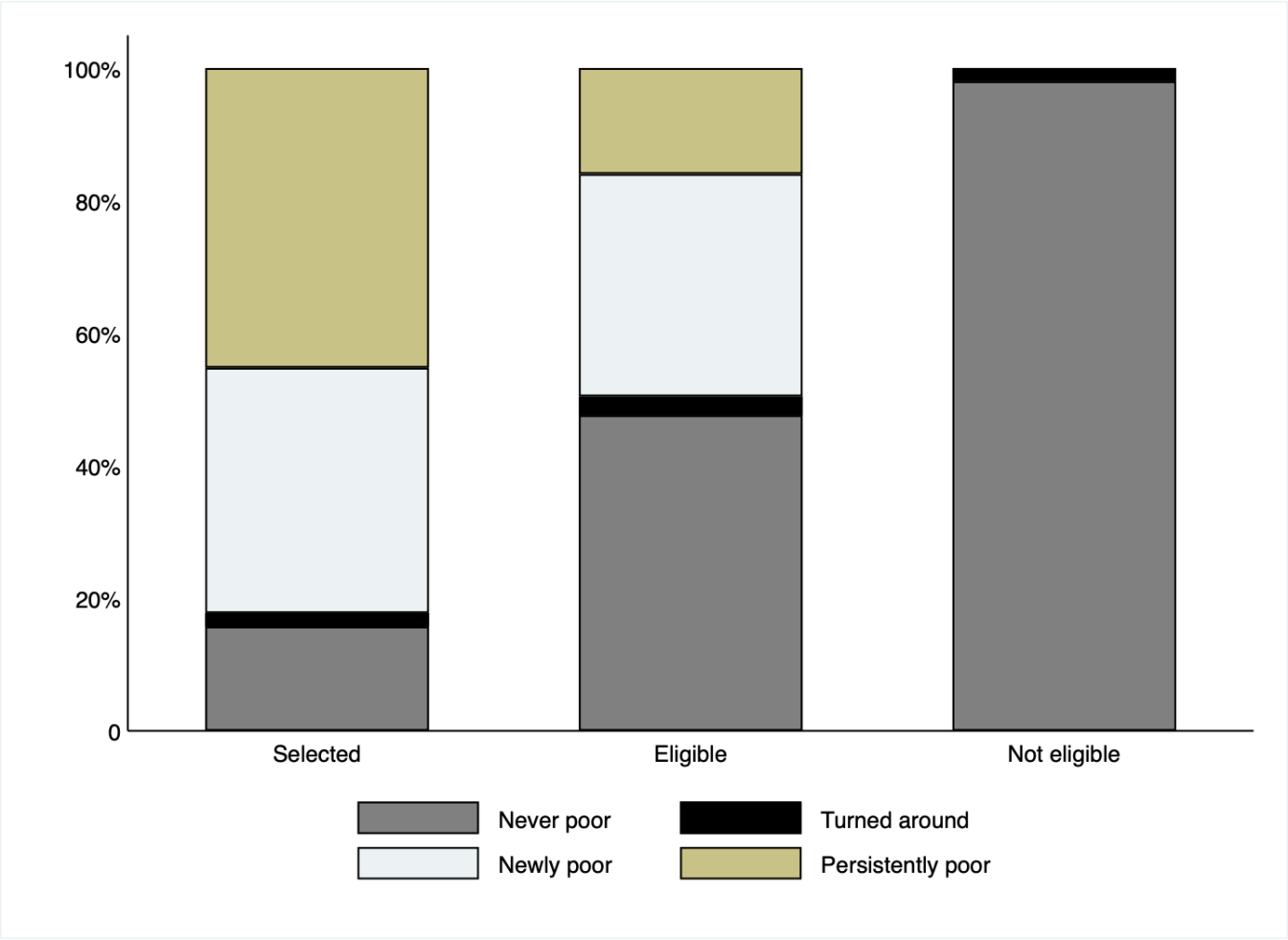
(c) Annual estimates: Number of investments



(d) Annual estimates: Dollars of investment

Sources: American Community Survey 2011-2015 5-year pooled sample; U.S. Department of the Treasury.
Notes: In subfigures (a) and (b), each dot represents the sample average within each bin. Fitted lines are based on a polynomial of degree 2 fitted separately to points on either side of the cutoff. Running variable combines the poverty rate and median income eligibility conditions as defined in the text. Coefficient represents the effect of crossing the eligibility threshold on the annual average number or dollars (in millions) of multi-family housing investment. Standard error and number of observations in the bandwidth are reported. In subfigures (c) and (d), each dot represents the estimated impact of Opportunity Zone eligibility on the number of investments or dollars of investment (in millions) in each year relative to investment in 2017. Vertical black lines represent the 95 percent confidence interval corresponding to each point estimate. Dollars values are adjusted for inflation using the Personal Consumption Expenditures price index.

Figure 13: Poverty of census tracts in 1980 versus 2011–2015



Sources: Economic Innovation Group, Census Bureau, U.S. Department of the Treasury.
Notes: A census tract is defined as poor if it has a poverty rate of at least 20%. Never poor means not poor in the 1980 Census and not poor in the 2011-15 ACS. Newly poor means not poor in 1980 Census and poor in the 2011-15 ACS. Turned around means poor in the 1980 Census and not poor in the 2011-15 ACS. Persistently poor means poor in both the 1980 Census and the 2011-15 ACS. Figure only includes only metropolitan area census tracts with a population of at least 500 and with less than 50 percent students. A total of 56,165 census tracts are included.

Table 1: Summary statistics, various years

	Eligible		Not Eligible
	Selected	Not Selected	
Tract characteristics (American Community Survey, 2013-2017)			
Median Household Income ($)	36,628	46,495	83,915
	(12,992)	(15,352)	(29,956)
Poverty Rate	28.6	20.2	7.4
	(12.8)	(11.1)	(4.5)
Unemployment Rate	6.3	4.9	3.2
	(3.6)	(2.9)	(1.6)
Labor Force Participation Rate	58.6	61.3	65.8
	(10.1)	(9.9)	(8.8)
Urban	0.89	0.90	0.97
Investment statistics (Real Capital Analytics, 2013-2017)			
At least one sale transaction (%)	57.2	47.2	51.1
Number of sale transactions*	4.6	4.2	4.3
	(6.9)	(6.7)	(6.6)
*Median Census Tract Level Price ($000)*†	5,688.1	6,070.3	7,278.8
	(15,487.9)	(23,525.9)	(54,584.0)
Median Census Tract Level Price/sq ft, $	135.4	144.4	173.1
	(165.0)	(168.9)	(1,027.4)
Property Type (%)			
Industrial	23.4	18.4	17.7
Office	14.0	13.4	21.2
Retail	16.9	20.3	24.4
Apartment	31.8	35.3	22.1
Business and spending activity (MasterCard, various years)			
New business growth rate (2018)	4.7	7.2	9.2
	(18.4)	(22.4)	(21.0)
Small business loan growth rate (2017)	11.6	11.2	6.6
	(36.1)	(36.3)	(23.6)
Commercial diversity (2019)	22.0	19.4	22.6
	(8.5)	(7.4)	(7.2)
Per-capita spending growth rate (rank, 2017)	46.9	47.7	53.3
	(22.6)	(22.9)	(22.4)
Tract-level spending growth rate (rank, 2017)	51.8	50.4	49.3
	(20.7)	(21.4)	(20.9)
Number of tracts	7,727	33,131	30,813

Sources: American Community Survey, 2013-2017, 5-year pooled sample; Real Capital Analytics, 2013-2017; Mastercard Center for Inclusive Growth, 2017-2019.
Notes: *Conditional upon having at least one transaction over 2011-2015. †Prices conditional upon sale. Excludes Puerto Rico.

Table 2: Impact of OZ eligibility on annualized number and dollars of commercial investment

Dependent variable	Total	Industrial	Office	Retail	Other
Number of Investments	-0.002	-0.004	0.006	-0.007	0.003
	(0.027)	(0.016)	(0.010)	(0.012)	(0.009)
Dollars of Investment	-0.103	0.189	-0.079	-0.169	-0.044
(millions $)	(0.723)	(0.559)	(0.291)	(0.216)	(0.144)

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

Notes: All results are estimated using a regression discontinuity design. Number of investments is the annualized number of investments made from 2019–2022 relative to 2014–2017. Dollars of investment is the total dollars of annualized investment made from 2019–2022 relative to 2014-2017. Running variable combines the poverty rate and median income eligibility conditions as defined in the text. All specifications include as census tract-level controls 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 dollar values are adjusted for inflation using the Personal Consumption Expenditures price index. Standard errors (in parentheses) are estimated using the heteroskedasticity-robust nearest neighbor variance estimator. Number of observations is 29,098.

Appendix A: Frontier regression discontinuity

More formally, let D be the eligibility status, T be the selection status, and R_i be the running variable (i = median income (1) or poverty rate (2)). We can define the potential outcomes $T(d)$ and $Y(d)$ and normalize the cutoffs of the running variable to zero. We consider the simplified example where $D = 1 - \mathbb{1}(R_1 > 0, R_2 < 0)$, that is, if median income is above its threshold and the poverty rate is below 20 percent, the census tract is not eligible.[26]

Note that

$$\begin{aligned}\mathbb{E}[T|R_1 = r_1] &= \mathbb{E}[T|R_1 = r_1, R_2 > 0]P(R_2 > 0|R_1 = r_1) \\ &\quad + \mathbb{E}[T|R_1 = r_1, R_2 < 0]P(R_2 < 0|R_1 = r_1)] \\ &= \mathbb{E}[T(1)|R_1 = r_1, R_2 > 0]P(R_2 > 0|R_1 = r_1)] \\ &\quad + \begin{cases} \mathbb{E}[T(1)|R_1 = r_1, R_2 < 0]P(R_2 < 0|R_1 = r_1)]r_1 < 0 \\ \mathbb{E}[T(0)|R_1 = r_1, R_2 < 0]P(R_2 < 0|R_1 = r_1)]r_1 > 0 \end{cases}\end{aligned} \tag{A.1}$$

Assuming $r_1 \to P(R_2 < 0|R_1 = r_1), r_1 \to E[T(\cdot)|R_1 = r_1, R_2 < 0]$ are continuous functions, we have that the first-stage RD at $R_1 = 0$:

$$E[T(1) - T(0)|R_1 = 0, R_2 < 0]P(R_2 < 0|R_1 = 0) \tag{A.2}$$

Likewise, the reduced-form RD gives

$$E[Y(1) - Y(0)|R_1 = 0, R_2 < 0]P(R_2 < 0|R_1 = 0), \tag{A.3}$$

where their ratio can be shown to be the LATE, under a monotonicity assumption.

$$E[Y(1) - Y(0)|R_1 = 0, R_2 < 0, T(1) > T(0)] \tag{A.4}$$

More specifically, Equation A.4 is the average treatment effect of those tracts which are ineligible on the poverty requirement, have median income at exactly at the cutoff and are selected as an OZ if and only if they are eligible. The flip case where poverty is exactly at the threshold and ineligibility on the median income requirement would look quite similar.

These thresholds from Equation A.2 are illustrated in Figure 4.

[26] For purposes of this exercise, we ignore the contiguity option for selection.

Appendix B: Tables and figures

Table B.1: Mastercard variable descriptions

Variable	Description	Source	Years available
New business growth	percentage growth of net new businesses based upon anonymized and aggregated location data $(year_t - year_{t-1})/year_{t-1}$	Mastercard Places	2018-2022
Small business loans	percentage growth of the number of small business loans $(year_t - year_{t-1})/year_{t-1}$	Federal Financial Institutions Examination Council	2017-2022
Commercial diversity	percentage of industries represented	Mastercard Point of Interest provider	2019-2022
Spend growth	percentage growth of spending based upon anonymized and aggregated transaction data. Within state $rank_t - rank_{t-1}$ (100 = top rank)	Mastercard GeoInsights	2017-2022
Spending per capita	percentage growth of average spending per account based upon anonymized and aggregated transaction data. Total spending within census tract (by residents and nonresidents) divided by number of residents. Within state $rank_t - rank_{t-1}$ (100 = top rank)	Mastercard GeoInsights	2017-2022

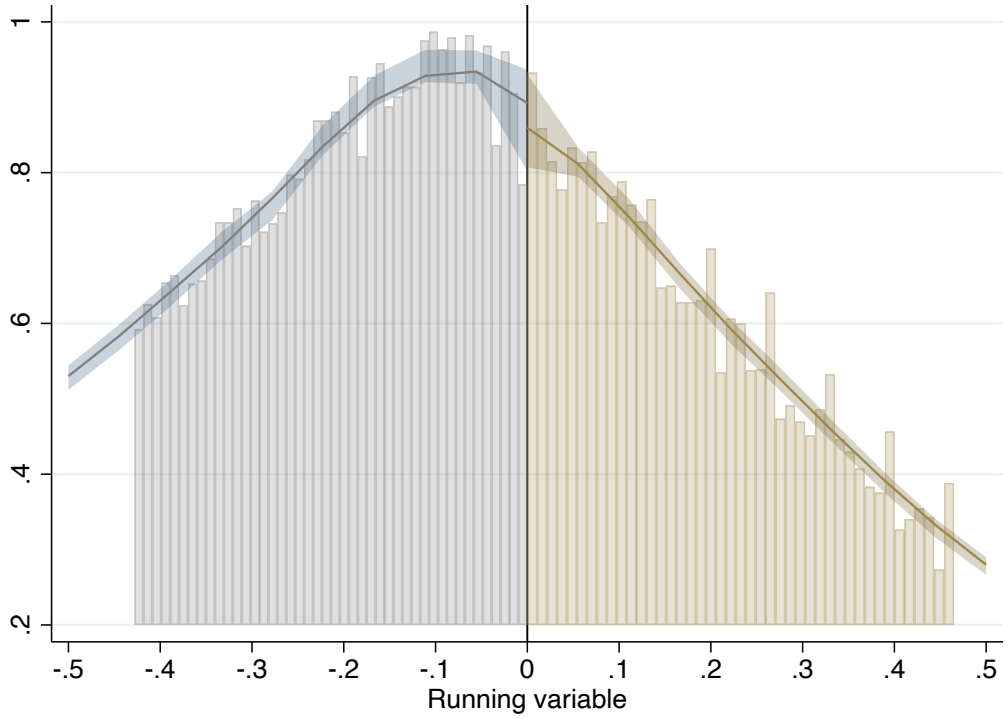
Sources: Mastercard's Center for Inclusive Growth

Table B.2: Summary statistics of census tracts by various intervals (lower and upper bound) of combination running variable

Lower bound	None	-0.75	-0.50	-0.25	0	0.25	0.50	0.75
Upper bound	-0.75	-0.50	-0.25	0	0.25	0.50	0.75	None
Med HH Income ($)	122,877	92,903	72,678	58,273	48,015	40,819	35,208	27,132
Poverty Rate	3.3	5.9	8.3	11.9	16.8	23.2	29.6	41.1
Unemp. Rate	2.6	3	3.3	3.7	4.5	5.5	6.3	7.7
LFP Rate	66.5	66.4	65.7	63.1	61.6	61.7	59.8	56.2
Share urban	1	0.99	0.96	0.88	0.87	0.92	0.93	0.96
Number of Tracts	5,152	8,053	11,774	16,287	12,900	7,296	3,600	6,609

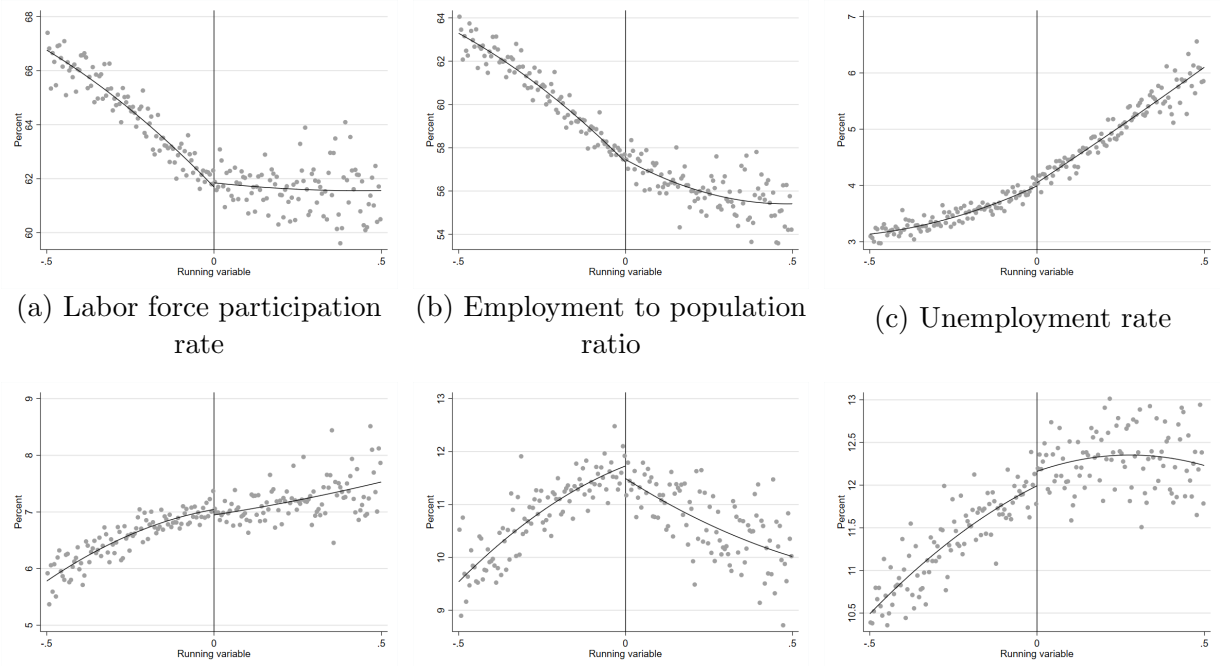
Sources: American Community Survey 2011-2015; 2013-2017; U.S. Department of the Treasury.
Notes: Table shows the median household income, mean poverty rate, mean unemployment rate, mean labor force participation rate, and share of tracts that are urban, all based on the 2013-2017 ACS, among census tracts grouped by their value of the combination running variable calculated on the basis of the 2011-2015 ACS. Running variable combines the poverty rate and median income eligibility conditions as defined in the text. The lower bound and upper bound of each running variable interval are shown in the top two rows of the table.

Figure B.1: Histogram of combined running variable and manipulation test



Source: American Community Survey 2011-2015 5-year pooled sample; U.S. Department of the Treasury; Authors' calculations
Notes: Running variable combines the poverty rate and median income eligibility conditions as defined in the text. Figure represents the density of tracts on each side of the cutoff point for eligibility as an Opportunity Zone, along with smoothed lines enabling a manipulation test.

Figure B.2: Control variables in 2013-2017 by constructed running variable



(a) Labor force participation rate

(b) Employment to population ratio

(c) Unemployment rate

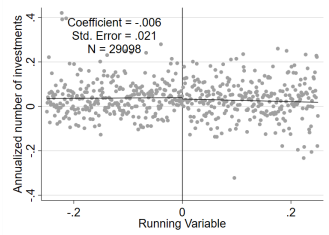
(d) Share of workers in construction

(e) Share of workers in manufacturing

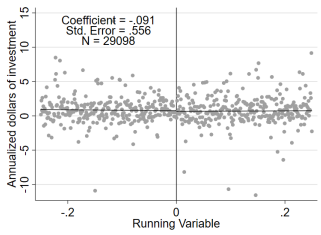
(f) Share of workers in retail

Sources: Real Capital Analytics; American Community Survey, 2011-2015; 2013-2017; 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 2 fitted separately to points on either side of the cutoff. Each control variable is based on the 2013–2017 ACS, while the running variable is constructed on the basis of the 2011–2015 ACS. Running variable combines the poverty rate and median income eligibility conditions as defined in the text.

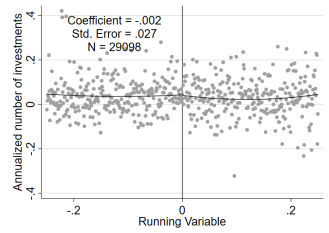
Figure B.3: Commercial investment outcomes by constructed running variable, 2019-2022 relative to 2014-2017, various polynomials



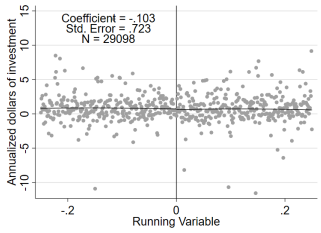
(a) Number of investments: Polynomial Order 1



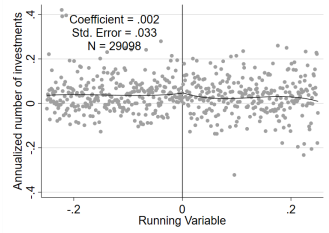
(b) Dollars of investment: Polynomial Order 1



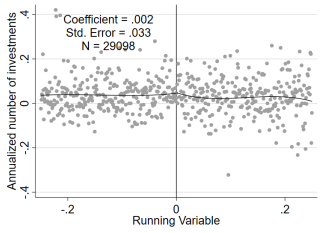
(c) Number of investments: Polynomial Order 2



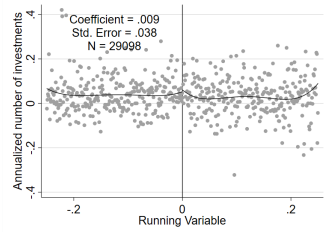
(d) Dollars of investment: Polynomial Order 2



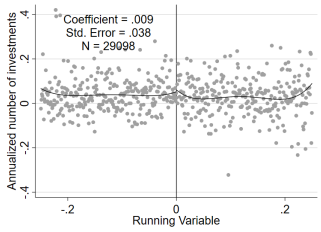
(e) Number of investments: Polynomial Order 3



(f) Dollars of investment: Polynomial Order 3



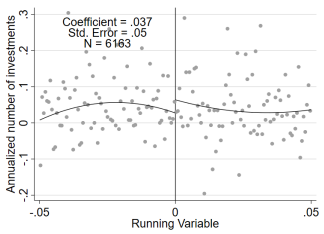
(g) Number of investments: Polynomial Order 4



(h) Dollars of investment: Polynomial Order 4

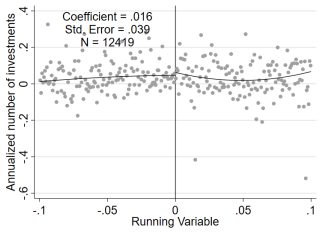
Sources: Real Capital Analytics; American Community Survey, 2011-2015 5-year pooled sample; U.S. Department of the Treasury.
Notes: Each dot represents the sample average within each bin. Fitted lines are based on a polynomial of various degrees fitted separately to points on either side of the cutoff. See Figure 6 for variable definitions.

Figure B.4: Commercial investment outcomes by constructed running variable, 2019-2022 relative to 2014-2017, various bandwidths

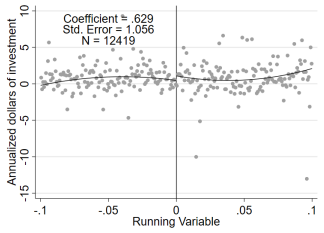


(a) Number of investments: Bandwidth 0.05

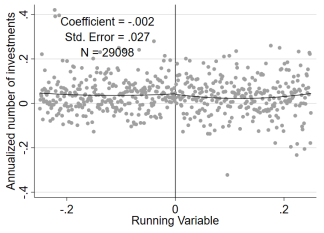
(b) Dollars of investment: Bandwidth 0.05



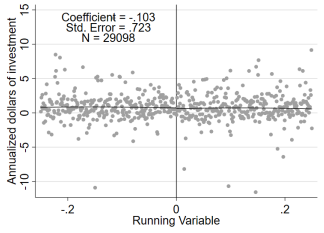
(c) Number of investments: Bandwidth 0.10



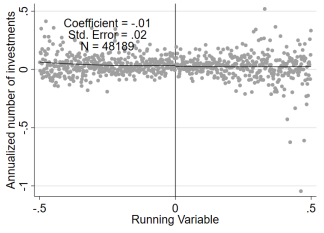
(d) Dollars of investment: Bandwidth 0.10



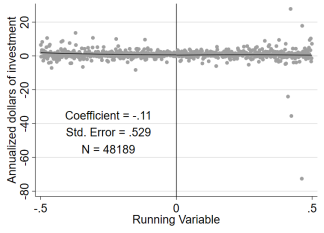
(e) Number of investments: Bandwidth 0.25



(f) Dollars of investment: Bandwidth 0.25



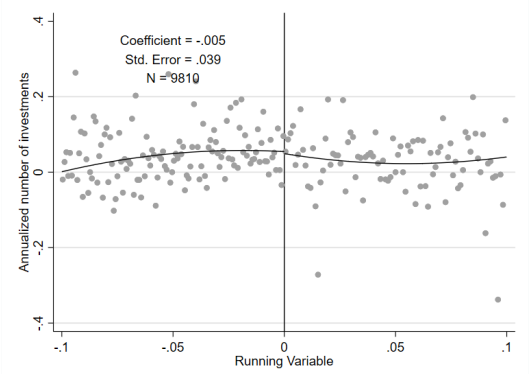
(g) Number of investments: Bandwidth 0.50



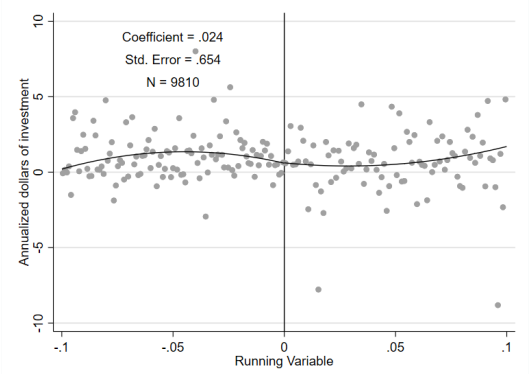
(h) Dollars of investment: Bandwidth 0.50

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 2 fitted separately to points on either side of the cutoff, using different bandwidths. See Figure 6 for variable definitions.

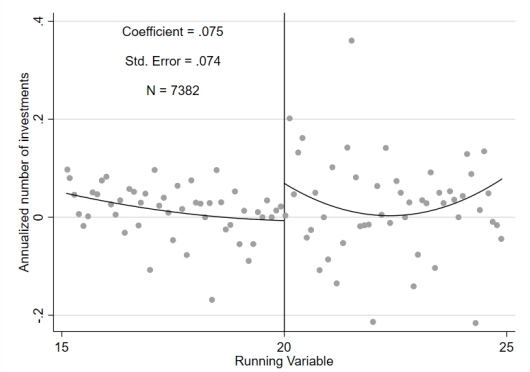
Figure B.5: Commercial investment outcomes by alternative running variables, 2019-2022 relative to 2014-2017



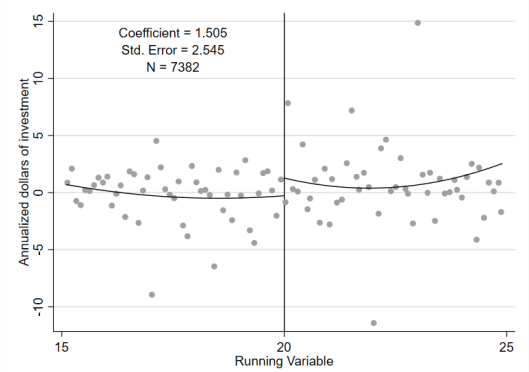
(a) Income running variable: Number of investments



(b) Income running variable: Dollars of investment



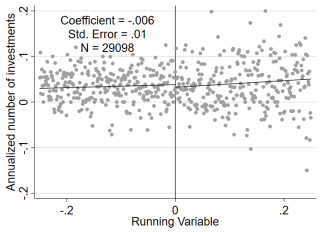
(c) Poverty running variable: Number of investments



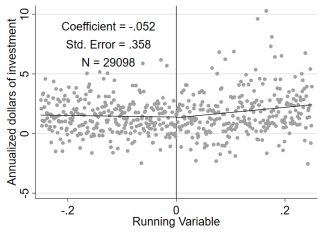
(d) Poverty running variable: Dollars of investment

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 2 fitted separately to points on either side of the cutoff. See Figure 6 for variable definitions.

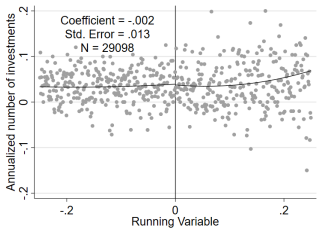
Figure B.6: Multi-family housing investment outcomes by constructed running variable, 2019-2022 relative to 2014-2017, various polynomials



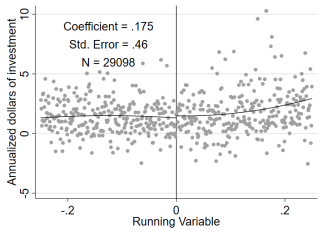
(a) Number of investments: Polynomial Order 1



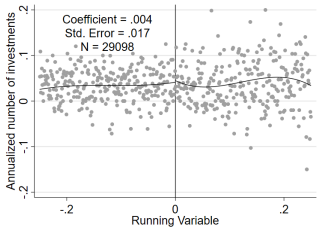
(b) Dollars of investment: Polynomial Order 1



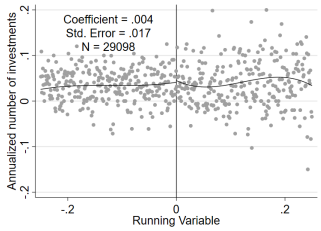
(c) Number of investments: Polynomial Order 2



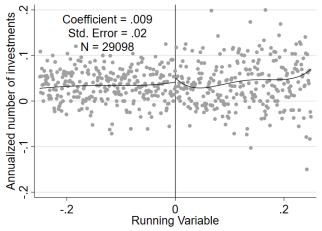
(d) Dollars of investment: Polynomial Order 2



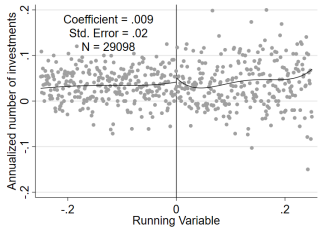
(e) Number of investments: Polynomial Order 3



(f) Dollars of investment: Polynomial Order 3



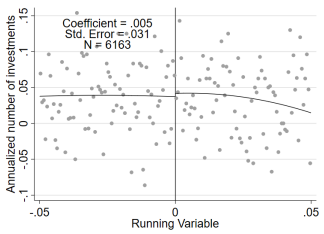
(g) Number of investments: Polynomial Order 4



(h) Dollars of investment: Polynomial Order 4

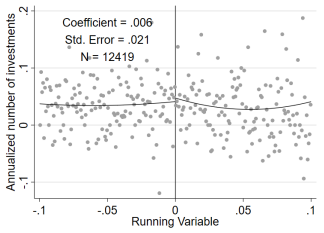
Sources: Real Capital Analytics; American Community Survey, 2011-2015 5-year pooled sample; U.S. Department of the Treasury.
Notes: Each dot represents the sample average within each bin. Fitted lines are based on a polynomial of various degrees fitted separately to points on either side of the cutoff. See Figure 6 for variable definitions.

Figure B.7: Multi-family housing investment outcomes by constructed running variable, 2019-2022 relative to 2014-2017, various bandwidths

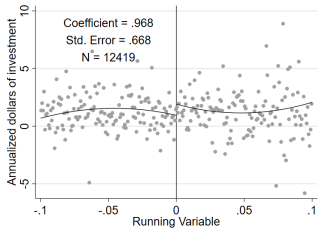


(a) Number of investments: Bandwidth 0.05

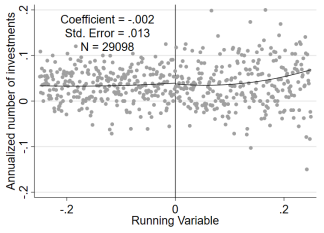
(b) Dollars of investment: Bandwidth 0.05



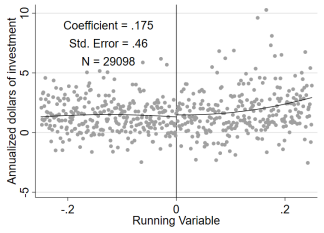
(c) Number of investments: Bandwidth 0.10



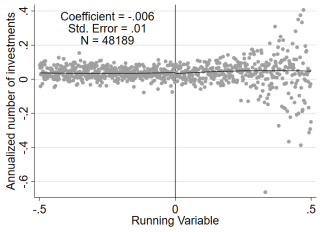
(d) Dollars of investment: Bandwidth 0.10



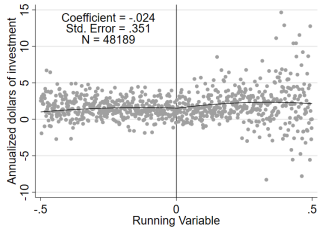
(e) Number of investments: Bandwidth 0.25



(f) Dollars of investment: Bandwidth 0.25



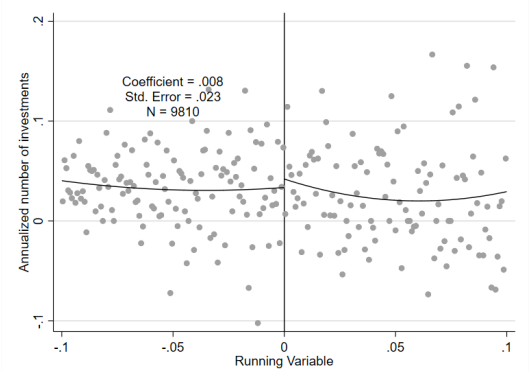
(g) Number of investments: Bandwidth 0.50



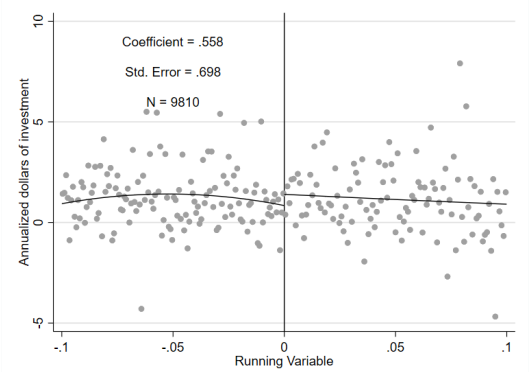
(h) Dollars of investment: Bandwidth 0.50

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 2 fitted separately to points on either side of the cutoff, using various bandwidths. See Figure 6 for variable definitions.

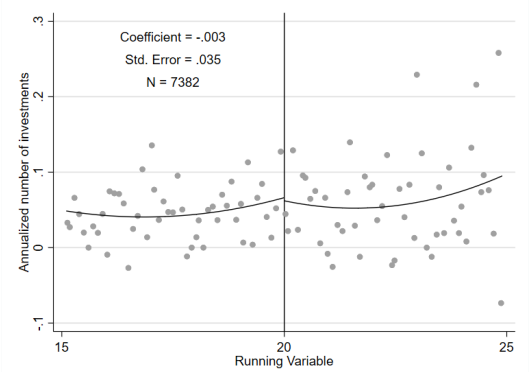
Figure B.8: Multi-family housing investment outcomes by alternative running variables, 2019-2022 relative to 2014-2017



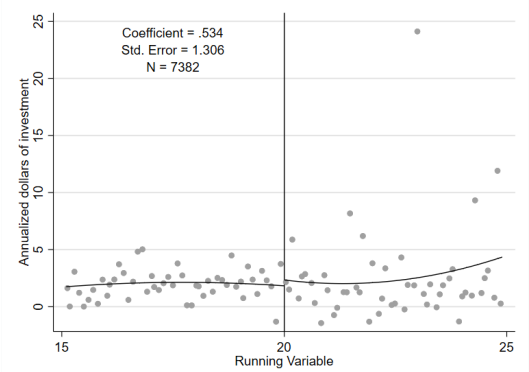
(a) Income running variable: Number of investments



(b) Income running variable: Dollars of investment



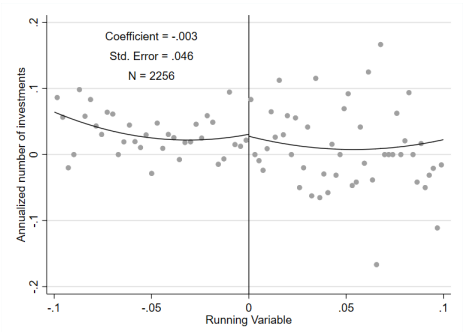
(c) Poverty running variable: Number of investments



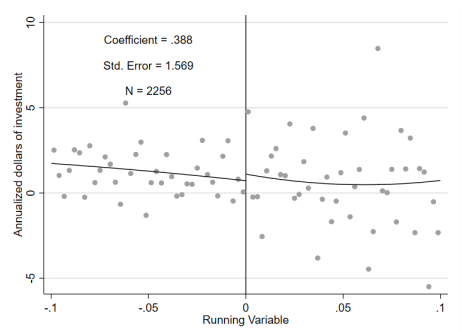
(d) Poverty running variable: Dollars of investment

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 2 fitted separately to points on either side of the cutoff. See Figure 6 for variable definitions.

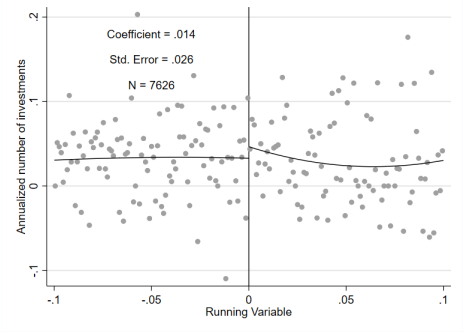
Figure B.9: Multi-family housing investment outcomes by constructed running variable, various samples



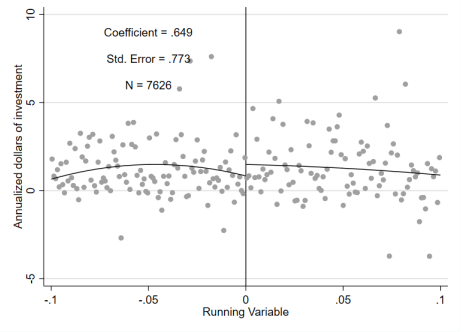
(a) Number of investments: Income running variable, poverty rate between 0% and 10%



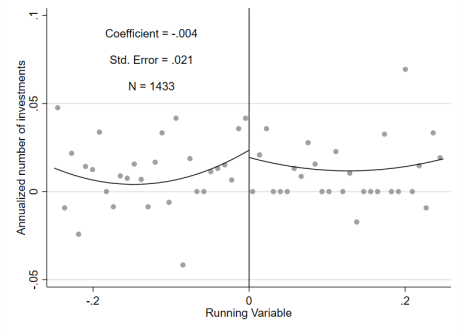
(b) Dollars of investment: Income running variable, poverty rate between 0% and 10%



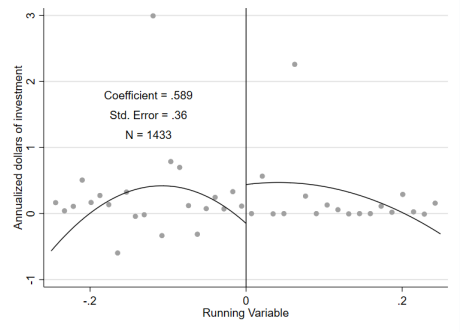
(c) Number of investments: Income running variable, poverty rate between 10% and 20%



(d) Dollars of investment: Income running variable, poverty rate between 10% and 20%



(e) Number of investments: Uniform counties



(f) Dollars of investment: Uniform counties

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 degrees 2 fitted separately to points on either side of the cutoff. See Figure 9 for variable definitions.