# Opportunity Zones and Private Investment: Evidence from U.S. Commercial Real Estate*

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**Abstract**

Place-based policies seek to stimulate investment in economically distressed communities. We evaluate the effect of Opportunity Zone (OZ) designation on nonresidential and residential commercial real estate investment using nationwide transaction data from 2013–2023 and a regression discontinuity design based on statutory eligibility thresholds. Our design exploits quasi-random variation in OZ eligibility and avoids bias from differential pre-policy investment trends. We find economically small and statistically insignificant effects of OZ eligibility across a wide range of specifications. While moderate positive effects cannot be ruled out, the evidence provides little support for a large, widespread investment response to the OZ program.

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# 1 Introduction

The increasing 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. Although in the past, regions with low incomes would improve faster than regions with high incomes, this pattern of income convergence has stalled 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 where children grow up have important consequences for their long-term well-being (e.g., Chetty et al. 2016; Chyn 2018). These concerns have contributed to growing interest in policies that target poor places in addition to poor people (e.g., Austin et al. 2018). Gaubert et al. (2020) show that the scope for place-based redistribution, in the context of the current productivity disparities between regions and stunted migration rates in the United States, is substantial, with the optimal transfer per resident in the most distressed areas totaling $3,000 to $6,000. Some existing place-based policies provide subsidies roughly in this range (Gaubert et al. 2020), but the evidence for the effectiveness of existing policies in increasing resources to residents of distressed areas is mixed.

In an attempt to improve on previous efforts 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. Corinth et al. (2025) estimate from IRS data that investors made at least $82 billion of qualifying investments in OZs by the end of 2022. Whether this investment was caused by the OZ tax provisions is a key question for determining whether OZs will ultimately improve the well-being of OZ residents and reverse trends in rising geographic disparity.

Most 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. Glasner et al. (2026) focus specifically on housing development based on United States Postal Service data and find that OZs substantially boost the number of new addresses. Arefeva et al. (2024) find that OZ designation increased employment growth by 2 to 4 percentage points using private tract-level data on employment. Atkins et al. (2023) use zip code level data on job postings and salary

[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.

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. 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. From a methodological perspective, a limitation of these studies is their use of 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.

We make two main contributions to the literature. Our first 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 traditionally been more structured and employment-focused. OZs represent the first place-based policy that provides for uncapped government subsidies and therefore unlimited private investment in 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. Because our data extend through 2023, we are able to observe investment responses that took place more than 5 years after OZs were designated. 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.

Our second contribution is methodological. As mentioned, most prior work on OZs uses difference-in-differences designs and controls for the fact that the growth rates of many census tract characteristics (e.g. unemployment rates, median family income, housing prices) are not similar between selected and non selected tracts. Our test for parallel pre-trends in the growth rate of investment similarly fails. We find that commercial investment was already growing faster in selected tracts than in non-selected tracts before OZ selection took place in 2018, consistent with the findings of Glancy et al. (2026) that two-thirds of the higher growth in construction projects observed in OZs was a result of projects that had been planned prior to the policy's introduction. Rather than attempt to control for the differing trajectories of OZs, we turn to a multidimensional regression discontinuity design instead, which relies on weaker assumptions and uses randomness near the OZ eligibility threshold to identify the policy's effect. OZ eligibility was determined by a threshold rule: a tract qualified if its poverty rate exceeded 20 percent or its median family income fell below 80 percent of the area median. We combine both conditions into a single running variable, so that tracts just above the cutoff are eligible and tracts just below are not. Comparing investment on either side of that cutoff provides a credible estimate of what effect OZ eligibility actually had on investment. Of course, this approach comes with a cost in that we can only make conclusions that are local to the cutoff. In Section 5 we argue that our results likely represent an upper bound for the average effect across all eligible tracts.

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. As such, the determinants of census tracts' eligibility could not have been manipulated for purposes of qualifying as OZs.

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 12 percentage points. Using the universe of commercial investment transactions above $2.5 million in the United States from 2013 through 2023, we find economically small impacts on nonresidential commercial investment—OZ eligibility decreases the annual number of investments by 0.001 (0.3%) and annual dollars of investment by $43 thousand (1.0%). Neither estimate is statistically significant at conventional levels. This lack of a statistically significant investment response holds whether we use only transactions where the stated purpose was "redevelopment," drop control tracts that border treated tracts to minimize spillovers, break property types down by categories (industrial, office or retail), focus on subsets of tracts based on poverty level or use urban tracts only.

We also do not detect significant effects of OZ eligibility on residential investment. OZ eligibility decreases the annual number of multi-family transactions by 0.001 (0.6%) and increases annual dollars of investment per census tract by $55 thousand (1.8%) in the five years post-policy, with neither result statistically significant at conventional levels. This differs from studies reporting substantial OZ impacts on housing development using a difference-in-differences design (Wheeler 2022, Glasner et al. 2026). While reduced statistical precision in our RD design may partly explain this discrepancy, difference-in-differences designs also rely on stronger identifying assumptions. Indeed, while OZs experienced more residential investment growth than eligible but not selected tracts after the policy, this differential growth was

largely a continuation of pre-existing trends, leading us to favor the RD evidence despite its reduced precision. We also note that multi-family development is unlikely in the most distressed areas, where population decline has left an excess of existing homes; any residential investment response is therefore likely concentrated in less distressed areas. Given the lack of a broader investment response, we do not expect OZ designation to meaningfully reduce geographic disparities or improve economic well-being for residents of distressed areas, on average.

Overall, we conclude that more than five years after the OZ policy was implemented, there is no evidence of a widespread private investment response that is causally driven by the OZ legislation. At the same time, investments are lumpy and our estimates come with relatively wide confidence intervals. Our identification strategy has the key advantage of exploiting near-random variation around the eligibility cutoff but it has the drawback of less statistical provision. For this reason, it is important for further research to evaluate the causal impacts of OZs using different research designs and studying various outcomes. This is especially the case in light of the permanent extension of OZs in 2025, with a new round of OZ designations every 10 years. This new iteration makes various changes to the OZ provisions we study in this paper, including stronger incentives for investment in rural areas. It will be important compare the outcomes of the first iteration of OZs with future rounds, as different tracts are designated, investor knowledge grows, and the OZ policy changes.

# 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 condition (a) or (b), and have a median income below 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

[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.

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 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

[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.

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 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 held approximately 57 percent of all qualified OZ property owned by QOFs in 2020. They argue that commercial real estate offers a relatively safe way to comply with the legislation's tax requirements while maintaining a high likelihood of nominal appreciation.

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 policy, 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, including the permanent extension of OZs enacted in 2025.

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

# 3 Data

The major data sources we use are tract level data from the American Community Survey (ACS), which were used to define eligibility for OZ designation, and transaction-level investment data from Real Capital Analytics (RCA).

To measure investment we use the RCA commercial investment database that contains transaction level data for the entire United States from 2013 through 2023 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 details on the financing of the loans. The large majority of transactions are undertaken for investment purposes (88%), with another 9% dedicated to redevelopment or renovation, and the remainder intended for owner occupancy. We aggregate investments to the five years pre-legislation (2013-2017) and the five years post-legislation (2019-2023), dropping 2018 because the legislation and rules regarding OZ investment were clarified over 2018. For our outcomes, we focus on the number of transactions and the sum of their sale prices.[7] A primary reason for this aggregation across years is due to the Covid-19 pandemic that disrupted annual investment patterns. By focusing on five year windows, we hope to smooth through the pandemic's impact on the timing of investments but still capture any potential

[6]Once a property sells for at least $2.5 million, it remains in the RCA database even if a subsequent sale falls below that threshold. Where possible, RCA also backfills the property's earlier transaction prices. Consequently, approximately 12% of the transactions in our RCA sample have prices below $2.5 million. For comparison, a linear extrapolation from data collected by Reonomy (an alternative commercial real estate data source) implies about 16.6% of the value of transactions in 2017Q4 were below $2.5 million.

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

overall treatment effect.

We note that any given transaction in the RCA data that took place in OZs during the treatment period does not necessarily qualify for OZ tax benefits. QOF 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.

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

both eligible tracts that were not selected prior to the OZ policy, as well as ineligible tracts. 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, the average transaction price was lower in selected tracts, potentially reflecting their economic deprivation. [8]

# 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.

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 previously seen in Table 1. Trends in many census tract characteristics prior to OZ selection differed as well, with Frank et al. (2022) and Eldar and Garber (2023) finding that selected tracts were experiencing faster growth in median family income and housing prices than nonselected tracts. Freedman et al. (2023) offer

[8] While not reported in the table, industrial investments comprised 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.

evidence of differential trends between selected and non selected tracts for employment rates and poverty. Glancy et al. (2026) find that two-thirds of the higher growth in construction projects achieved by OZs after the policy was implemented was attributed to projects that had already been planned prior to the policy.

In Figure 3, panels (a) and (b), we show the trends in nonresidential 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 and 2023. However, the uptick in 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 that pre-trends differed across groups. This suggests that parallel trends in the post-period in the absence of the policy may not necessarily hold.

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.[9]

In the RD design, we exploit the sharp poverty rate and income eligibility cutoffs.[10] 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.[11]

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

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

[10]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.

[11]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 $i$.

Panel (e) 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.[12] Following Cattaneo et al. (2018), we test whether there is any evidence

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

of manipulation around the threshold of our combined running variable and find that we do not reject the null hypothesis of no systematic manipulation ($p > .94$).[13] We also conduct balance tests around the threshold on the basis of predetermined tract-level employment outcomes and industry shares in Figure 5. We do not find statistically significant changes in any of these outcomes when crossing the threshold. This provides empirical support for the identifying assumptions required for our RD design, but ultimately what is required is that unobservable determinants of investment are distributed similarly for places just above and below the cutoff, an assumption we cannot test. In the analysis that follows, we use this combined running variable to estimate the impact of OZ eligibility on commercial investment.[14]

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

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.

[13] 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.

[14] Figures B.10 and B.11 present complementary results for each threshold separately.

observable, pre-determined census tract characteristics that are correlated with the outcome 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. It is important to note that our estimates identify the effect of crossing the OZ eligibility threshold, as opposed to OZ selection. Estimating the latter would require a fuzzy RD design. Under the standard exclusion and monotonicity assumptions, a fuzzy RD estimate would identify the effect of OZ selection among tracts whose designation status is changed by crossing the eligibility threshold.

# 5 Nonresidential investment

This section focuses on nonresidential investment while the next section focuses on residential investment. We consider two primary 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, 2023) and the pre-period (January 1, 2013–December 31, 2017).[15] 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.[16]

[15]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.

[16]In addition, as we will discuss in Section 7, 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

The impact of OZ eligibility (intent-to-treat) on the change in total nonresidential commercial investment between the post and pre-period is shown graphically in Figure 6.[17] Panels (a) and (b) 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 parameter estimates in the corner of each figure that confirm the graphical evidence. 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 of the construction, manufacturing and retail industries, 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 0.001 (0.3%) and decreases dollars of investment by \$0.043 million (1.0%).[18] While some results are unexpectedly negative, they all point to a near-zero impact of OZ eligibility on nonresidential investment. Figures B.6 and B.8 show for total nonresidential investment that these null results are robust to variations in the fitting polynomial and bandwidth around the threshold, respectively. 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 0.038 (11.9%) more investments, and an additional \$1.04 million of investment (a 24.8% increase). While these upper bounds represent effects that are economically important, Figure B.12 shows that they are typical of the variation in investment that we see across all years, particularly those prior to OZ

on our outcome variables from this program.

[17] Total is the sum of the three major categories of retail, industrial and office buildings plus a fourth category of "other" that captures smaller categories of hotels and development sites.

[18] Percentage effects are derived from a comparison to the average value of the eligible group in the pre-period within the bandwidth throughout the paper as shown in Table 2.

implementation.[19] The lack of statistically significant investment effects in years prior to the policy also provides evidence that other factors aside from the OZ policy were not driving differences between eligible and ineligible tracts near the threshold.

Although we find null effects, we report in Figure B.13 the results of a placebo test, in which we vary the cutoff point of the running variable. We change the median income threshold from 80% of the MSA or state median to 100%, and we change the poverty threshold from 20% to 15%. Since eligibility was not based on these revised cutoff points, we expect not to find a statistically significant increase in investment at the revised cutoff of the running variable. That is the result obtained in Figure B.13, providing evidence that investment does not move discretely at an alternative set of cutoff points, and that any discrete jump at the true eligibility threshold we might have found would have been more likely attributable to the policy.

We next consider the subsample of investments that are specifically intended for redevelopment purposes, representing about 12% of all nonresidential commercial investments within our bandwidth in the pre-period.[20] Results are presented in panels (c) and (d) of Figure 6. We find no evidence that OZ eligibility increases investment in properties specifically designated as redevelopment projects, with the estimated results showing a decrease of 0.002 (1.1%) in the number of commercial investments and a decrease of $0.047 million (2.6%) in investment–quite similar to our main findings. Table 2 disaggregates nonresiden-

[19]Figure B.12 repeats the same RD exercise for each individual year from 2010 through 2022. 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 nonresidential commercial investment.

[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.

tial investment into industrial, office, retail and other, with no statistically significant effect found for any type.

## 5.1 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. If spillovers mainly occur in nearby eligible but not-selected tracts, our RD results would not be biased downwards as this would increase the amount of overall investment that occurs just over the eligibility threshold of the running variable and be included in our RD estimates. Thus, to the extent that there are spillovers to other eligible tracts, our conclusion that the OZ legislation did not lead to significant increases in nonresidential investment would not be altered. Alternatively, if spillovers occur in nearby *ineligible* census tracts then this could potentially lead us to understate the effect of OZ eligibility on investment. 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 dropping all ineligible tracts that border a selected tract. Thus, the ineligible census tracts just below the cutoff are at least one census tract removed from selected tracts just over the threshold, thus minimizing spillovers. Panels (e) and (f) of Figure 6 shows that the change in the number of investments at the threshold is equal to 0.000, almost identical to the main results while the change in the dollars of investment is equal to $97 thousand, larger than our main findings but still relatively small, and not statistically different from zero at conventional levels.

## 5.2 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. 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 absorb disproportionate amounts of OZ investment. 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 policies seeking to encourage private investment. Figure 7 shows the distribution of investment (number of investments and dollars of investment) over all census tracts in the pre-period (January 2013 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 B.4 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 subsamples. As Figure B.4 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.

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

# 6 Residential investment

Until now, we have focused our analysis on nonresidential investment (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 use residential investment, which captures commercial multifamily housing transactions, as our outcome variable, which is captured in the RCA data.

We begin by repeating the difference in differences analysis. Similar to the nonresidential investment figures, Figure 8 shows differing trends in residential 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 residential investment 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, suggesting greater concern over the identifying assumption for a difference-in-differences design that trends would have evolved similarly in the post-period absent the policy. In particular, selected tracts experienced faster growth in residential investment than eligible but not selected tracts in the period leading up to the designation of OZs in 2018. As was the case for nonresidential investment, this motivates the RD design that requires the weaker assumption of randomness around the eligibility threshold and balance in terms of unobserved variables that affect investment.

Figure 9 shows the effect of passing the eligibility threshold on the number of investments and dollars of residential investment. In subfigures (a) and (b), the outcome variable is the difference in annual average investment between the pre-period (2013-2017) and post-period (2019-2023). The number of residential investments falls by 0.001 (0.6%) and dollars

of residential investment increases by $0.055 million (1.8%), though, neither is statistically significant at conventional levels. We repeat the same spillover tests applied to the nonresidential investments and similarly find no evidence for a statistically significant increase in residential investment in subsample that excludes contaminated controls.

Finally, we consider the possibility that OZ incentives may have switched the composition of investments despite the lack of any clear evidence that the total amount of investment was impacted. In Figure 10 we plot the share of residential investment out of total commercial investment (nonresidential plus residential). While the point estimate is suggestively positive pointing to a shift towards residential investment, again, we cannot claim anything with statistical precision.

# 7 Discussion

Our analysis provides little evidence that OZ eligibility generated a large increase in commercial real estate investment through the end of 2023. Across a wide range of specifications, the estimated effects are economically small and generally centered near zero. For nonresidential investment, our preferred estimates imply a decrease of 0.001 investments per tract (0.3 percent) and a decrease of $0.043 million in investment dollars (1.0 percent). For residential investment, the corresponding estimates are -0.001 investments (0.6 percent) and +$0.055 million in investment dollars (1.8 percent). None of these estimates are statistically distinguishable from zero at conventional levels.

At the same time, our findings should not be interpreted as definitive evidence that OZs had no effect on investment. The confidence intervals surrounding many of our estimates remain relatively wide. For example, for nonresidential investment we cannot reject increases

as large as approximately 11.9 percent in the number of investments and 24.8 percent in investment dollars at the 95 percent confidence level. Investment activity is inherently lumpy, with a relatively small number of large transactions generating substantial year-to-year variation. Consequently, even if OZs induced additional investment in some locations, detecting those effects precisely in aggregate investment data is challenging.

This distinction between point estimates and statistical uncertainty is important. The estimates consistently fail to indicate large positive effects, and we find no specification that provides robust evidence of a substantial investment response. However, the confidence intervals imply that moderate positive effects remain plausible. Our results are therefore best interpreted as providing little evidence of a large, widespread investment response rather than as definitive evidence that the policy had no effect whatsoever.

The consistency of the findings across specifications is nevertheless notable. We obtain similar conclusions when examining redevelopment projects, alternative bandwidths and polynomial specifications, spillover-adjusted samples, urban tracts, and various heterogeneity analyses. Likewise, we find little evidence that OZ eligibility materially altered residential investment. Taken together, these results suggest that any average investment response to OZ eligibility was modest relative to the substantial underlying variation in investment activity across census tracts and over time.

One potential explanation for these findings is that the OZ incentive was not designed to maximize investment that would have not otherwise taken place. Corinth and Feldman (2024) show that the largest OZ tax subsidies were offered to investments with the highest private rate of return. This is because the largest share of the tax subsidy results from the elimination of capital gains tax on OZ investment held for at least ten years, implying that the higher the private return, the higher the subsidy. Thus, the same investments that

receive the highest subsidies are those that private investors would have been the most likely to undertake regardless of the OZ policy. Through 2022, $82 billion of qualifying investment had been made in OZs and thus was eligible for the OZ tax benefits (Corinth et al. 2025). The structure of the OZ incentive, together with our empirical estimates of the causal effect of OZs, suggests that much of the tax benefits may have accrued as a windfall to investors that did not necessarily change their investment decisions.

Apart from the design of OZ tax incentives that offered the highest rewards to investment with the highest private returns, another factor that could have stunted the causal impact of OZs is the difficulty in spurring investment in persistently distressed areas. In fact, a substantial share of OZ tracts exhibit persistent economic distress rather than temporary declines. As shown in Figure 11, more than 45 percent of selected OZ tracts had poverty rates above 20 percent both in 1980 and in the 2011–2015 ACS, while fewer than 2.5 percent of historically poor tracts had poverty rates that fell below 20 percent over the same period. These patterns highlight the remarkable persistence of geographic disadvantage in the United States and suggest that many OZs face structural challenges that extend well beyond a lack of investment capital.

Viewed through this lens, the absence of a precisely estimated investment response may not be entirely surprising. Tax incentives can increase the after-tax return to investment, but they may be insufficient to overcome the broader economic forces that have contributed to decades of persistent distress. If investors perceive weak local demand, declining population trends, limited agglomeration benefits, or other unfavorable economic fundamentals, tax preferences may have only a modest influence on investment decisions. While our analysis cannot directly identify these mechanisms, Figure 11 provides useful context for understanding why a policy focused primarily on capital incentives may generate smaller effects than

policymakers anticipated.

The persistence documented in this figure also has broader implications for place-based policy. If long-run economic decline reflects structural factors that evolve over decades, then policies aimed solely at increasing capital flows may have limited effects unless accompanied by complementary improvements in local economic conditions. This interpretation is consistent with the broader literature on place-based policies, which has produced mixed evidence regarding the ability of geographically targeted tax incentives to generate sustained economic improvements.

Finally, it is important to emphasize the scope of our estimates. The regression discontinuity design identifies the causal effect of OZ eligibility for census tracts near the eligibility thresholds and therefore informs what would happen if the program were expanded on the margin. It remains possible that OZ designation generated meaningful increases in investment in a subset of tracts, particularly those with characteristics that made them especially attractive to investors. However, such effects do not appear sufficiently widespread to generate a detectable average impact in the tracts where most investment activity occurs. Moreover, to the extent that OZs did affect local economies, there is a related concern that such improvements could have come at the expense of other low-income areas. Freedman et al. (2025) find that OZ designation does raise job creation within zones, but much of that gain is offset by losses in nearby low-income areas and the longer-run employment gains among OZ residents turn out to be driven mostly by new residents rather than the people who lived there when the zones were designated. In other words, OZs look less like a source of new economic activity and more like a mechanism for moving jobs and households around.

Taken together, the evidence indicates that OZ eligibility did not produce a clearly detectable increase in investment near the eligibility threshold during the first several years

of the policy. At the same time, the remaining uncertainty around the estimates cautions against strong conclusions regarding the complete absence of policy effects. The most defensible interpretation of the evidence is that any average effect was likely modest relative to the substantial variation in investment activity observed across places and over time, and that the design of the OZ tax incentives together with persistent economic conditions in many distressed communities may limit the ability of OZs alone to generate large changes in investment behavior.

# 8 Conclusion

The Opportunity Zone (OZ) policy was designed to encourage private investment in economically distressed communities through preferential tax treatment of capital gains. Using the universe of large commercial real estate transactions from 2013 through 2023 and a regression discontinuity design based on the statutory eligibility thresholds, we examine whether OZ eligibility increased investment activity.

We find little evidence that OZ eligibility generated a large increase in either nonresidential or residential real estate investment. Across a variety of specifications and robustness exercises, the estimated effects are generally small and statistically indistinguishable from zero. At the same time, investment activity is inherently volatile, and the confidence intervals around many estimates remain wide. As a result, our findings are better interpreted as evidence against a large, widespread investment response rather than definitive evidence of no effect.

More broadly, our results are consistent with the design of the OZ tax incentive which provided the highest rewards to investments with the highest private rate of return, suggest-

ing that windfalls for investors may have been more likely than inducing new investment that would not have otherwise occurred. Our findings also highlight the challenge of using tax incentives alone to stimulate investment in communities facing long-term economic distress. The persistence of poverty in many neighborhoods across the United States suggests that many regions face structural barriers that may limit the responsiveness of private investment to tax incentives.

Future research should continue to examine the longer-run effects of the policy. One fruitful avenue would be to apply a regression discontinuity approach to outcomes beyond investment. While investment is the primary channel through which other economic effects were posited to flow, more evidence on outcomes like employment and resident well-being using an approach with different identifying assumptions than difference-in-differences designs would add to the evidence base on OZs and inform future evaluation efforts of the new round of OZs implemented under the One Big Beautiful Bill Act. More generally, future research should also evaluate whether place-based investment incentives are more effective when designed to provide the largest subsidies to investments that would not have otherwise occurred, and when combined with policies that address the underlying sources of persistent economic disadvantage.

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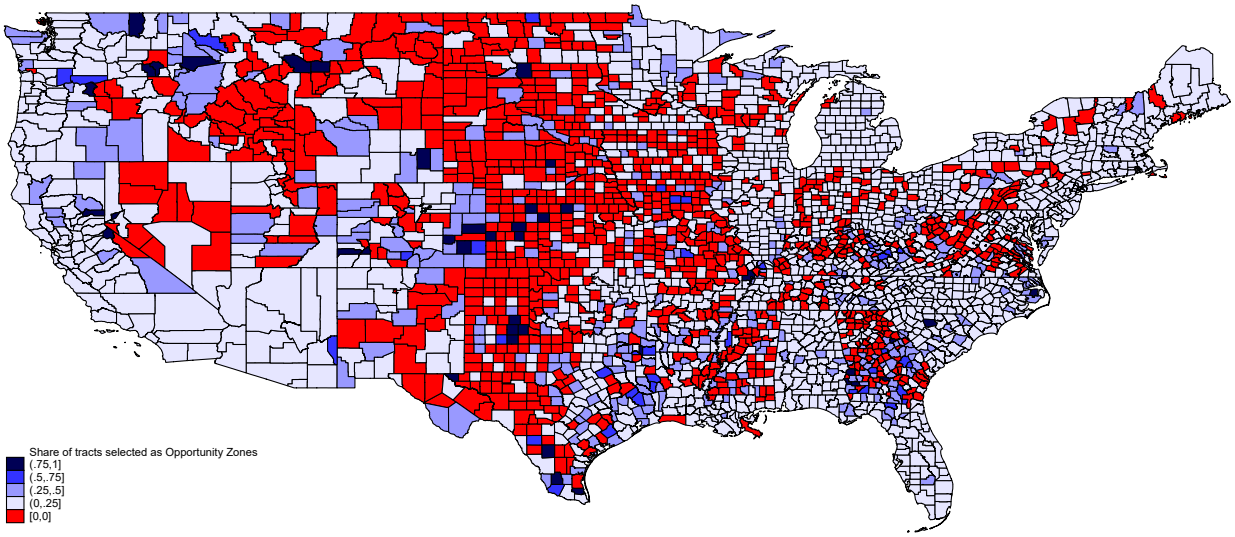
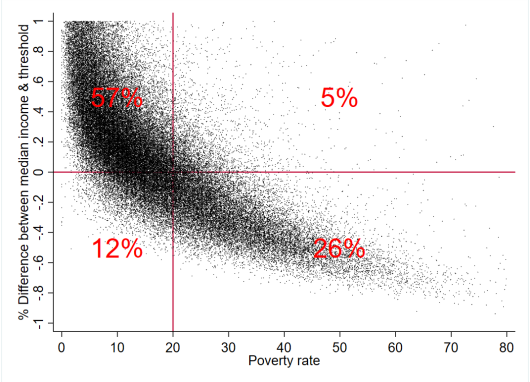
Figure 1: Share of census tracts designated as OZs by county in the continental United States, 2018

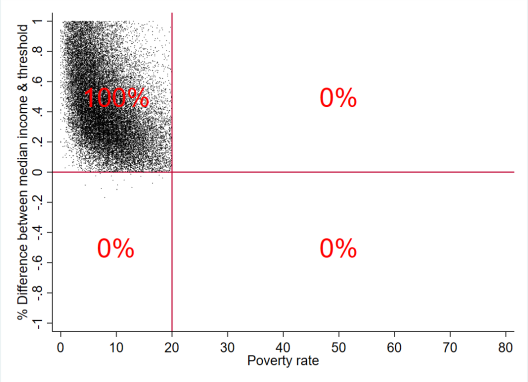



Source: U.S. Department of the Treasury; Authors' calculations
Notes: Map denotes unweighted share of census tracts in each county that were designated and confirmed by Treasury as Opportunity Zones in 2018.

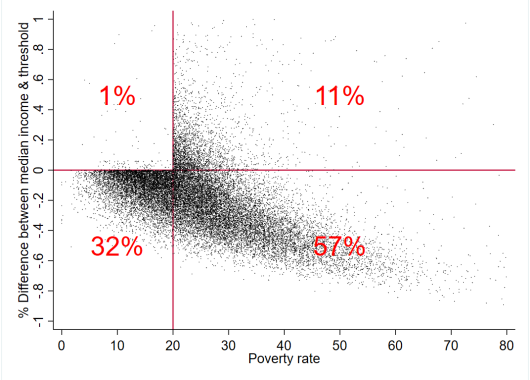
Figure 2: Poverty rate and percent difference between median income and threshold for each census tract, 2011-2015, by census tract eligibility and selection as OZ



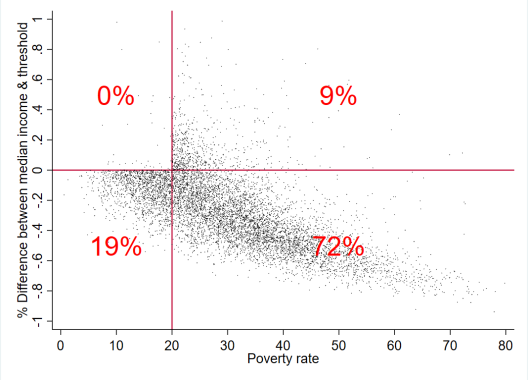

(a) All census tracts




(b) Ineligible census tracts



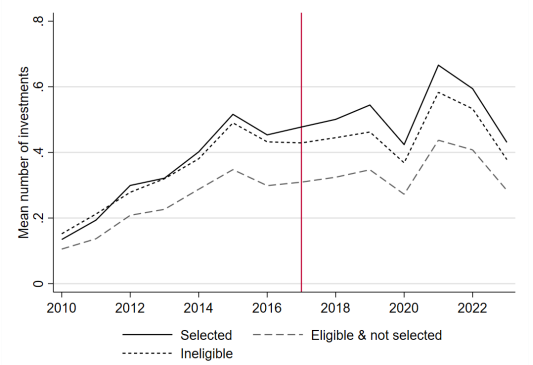

(c) Eligible but not selected census tracts



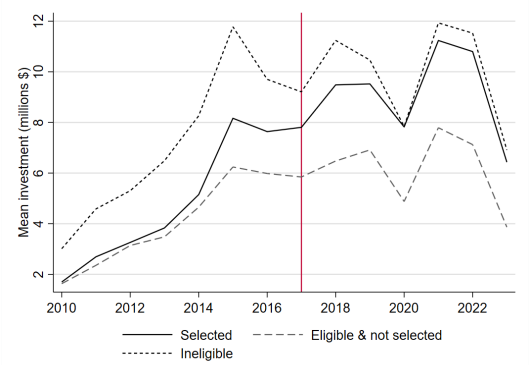

(d) Selected census tracts

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.

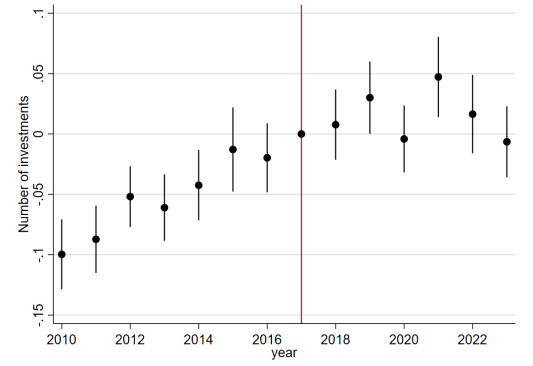
Figure 3: Mean annual number and dollars of nonresidential commercial investments by census tract type and difference across census tract type, 2010-2023



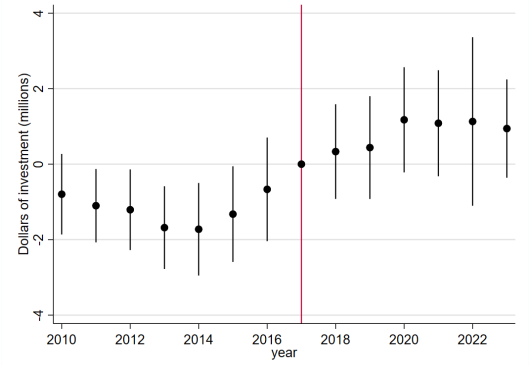

(a) Number of investments




(b) Dollars of investment



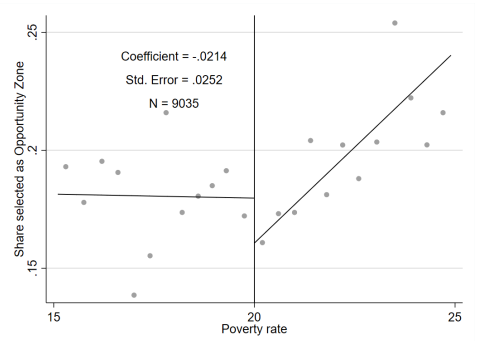

(c) Difference in number*



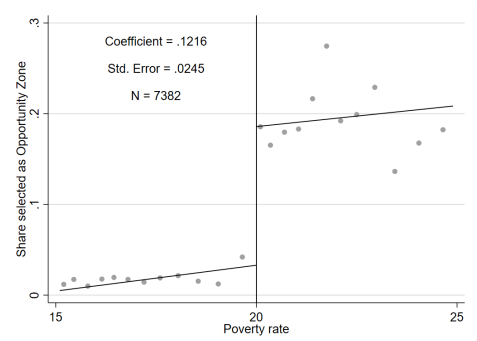

(d) Difference in dollars*

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 nonresidential investments made during the year, and dollars of investment is the sum of all dollars of nonresidential 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.

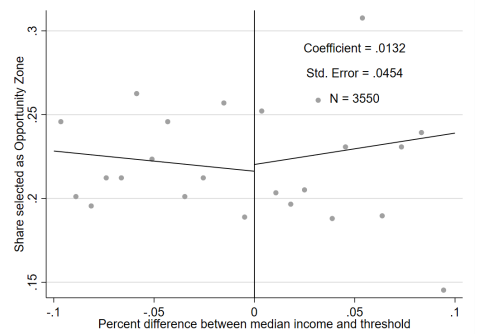
Figure 4: Share of census tracts designated as OZs in 2018 by poverty rate, percent difference between median income and threshold, and combined running variable



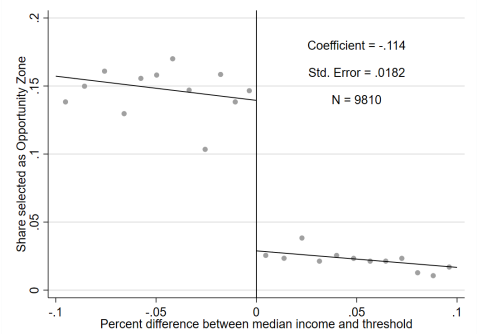

(a) Tracts with median income below 80% of MSA/state median income



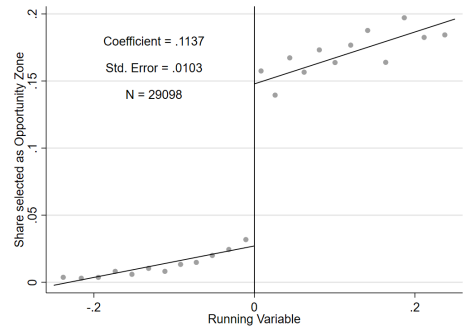

(b) Tracts with median income above 80% of MSA/state median income




(c) Tracts with poverty rate above 20%



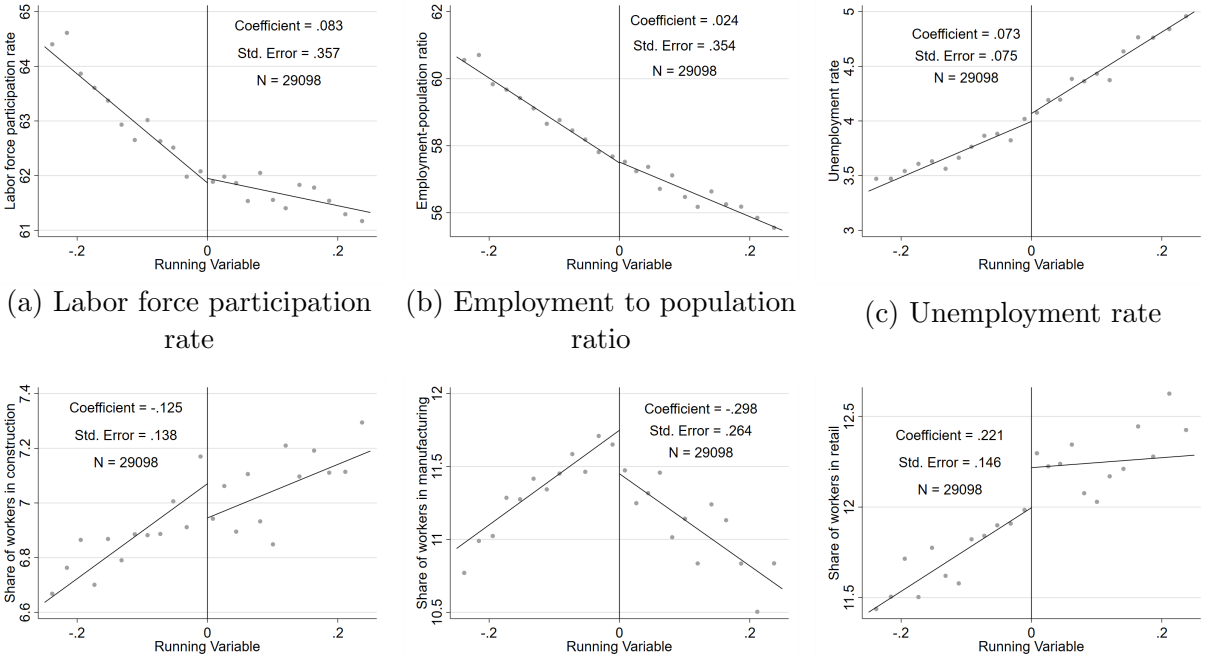

(d) Tracts with poverty rate below 20%




(e) All census tracts, combined 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 linear 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). Running variable in subfigure (e) combines the poverty rate and median income eligibility conditions as defined in the text.

Figure 5: Control variables in 2013-2017 by combined running variable




(a) Labor force participation rate

(b) Employment to population ratio

(c) Unemployment rate

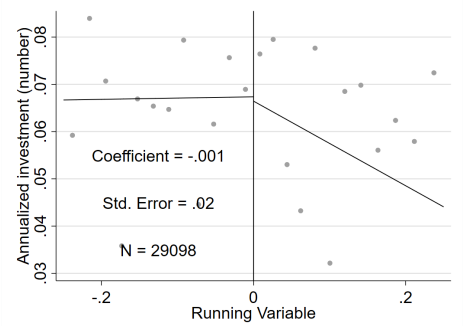
(d) Share of workers in construction

(e) Share of workers in manufacturing

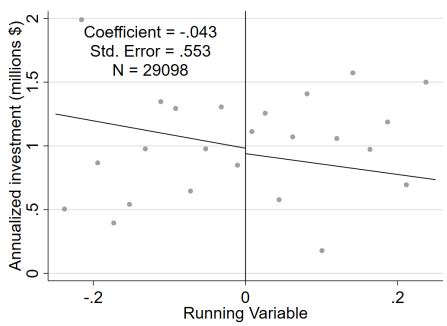
(f) Share of workers in retail

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 1 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.

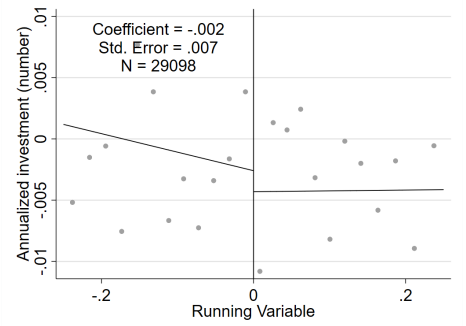
Figure 6: Difference in annualized nonresidential commercial investment between 2013-2017 and 2019-2023, by combined running variable



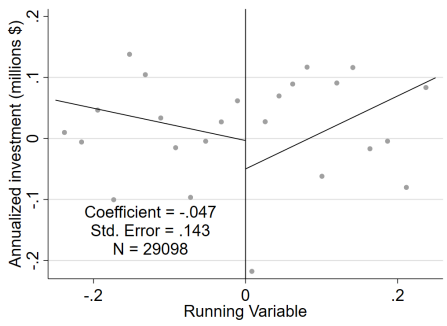

(a) Number of investments: All census tracts



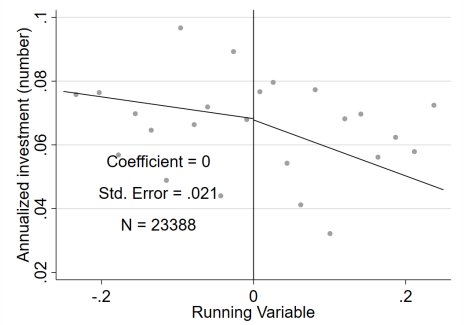

(b) Dollars of investment: All census tracts



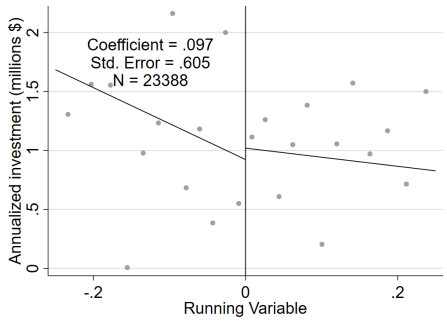

(c) Number of investments: Redevelopment properties




(d) Dollars of investment: Redevelopment properties



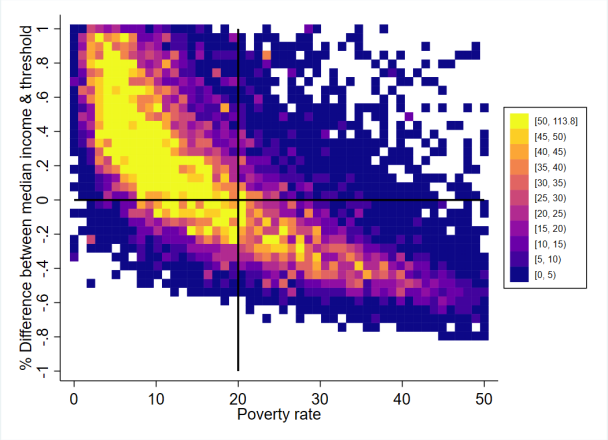

(e) Number of investments: Exclude contaminated controls



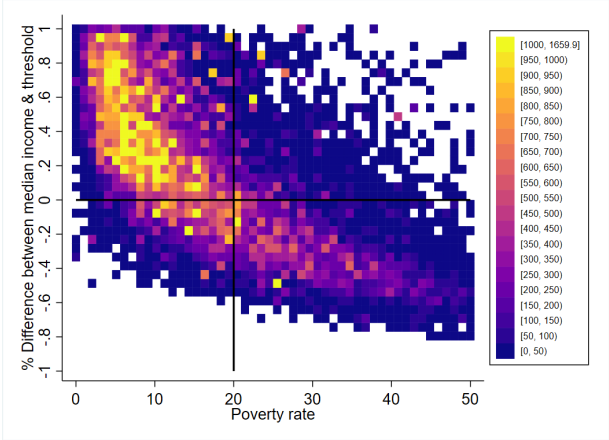

(f) Dollars of investment: Exclude contaminated controls

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 linear 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 nonresidential 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.

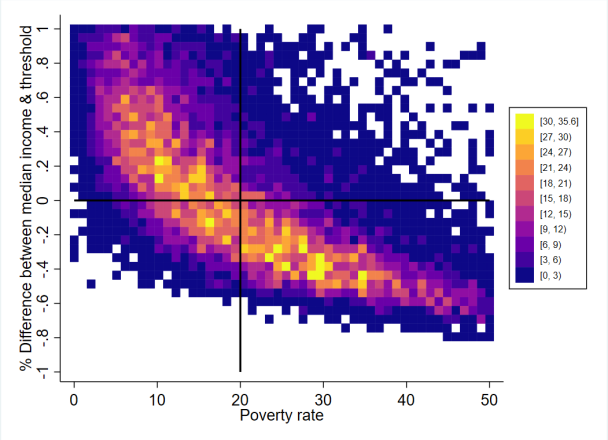
Figure 7: Distribution of annualized number and dollars of investment by poverty rate and percent difference between median income and threshold, 2013–2017



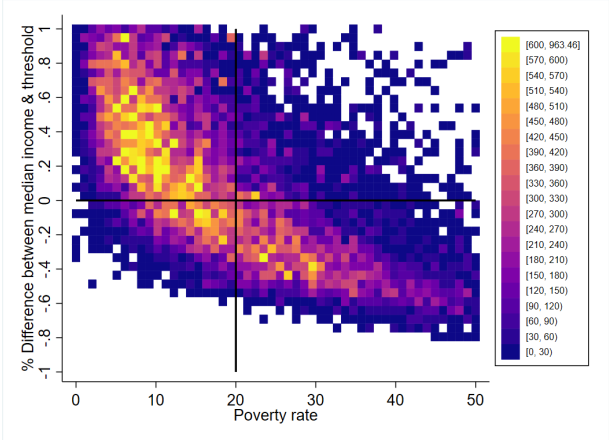

(a) Nonresidential: Number of investments




(b) Nonresidential: Dollars of investment (millions)



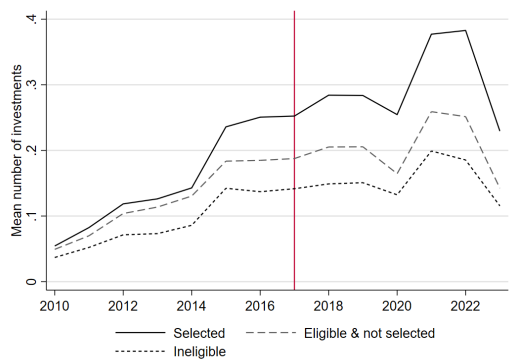

(c) Residential: Number of investments



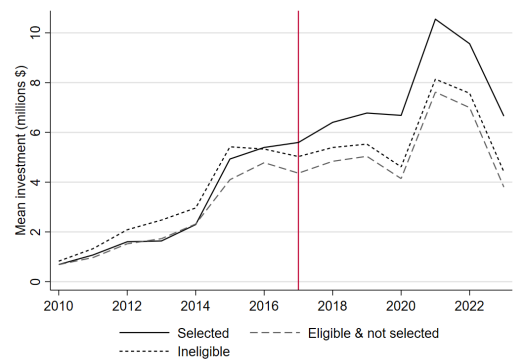

(d) Residential: Dollars of investment (millions)

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.

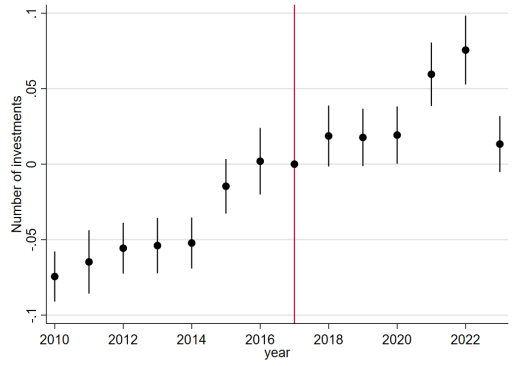
Figure 8: Mean annual number and dollars of residential investments by census tract type and difference across census tract type, 2010-2023



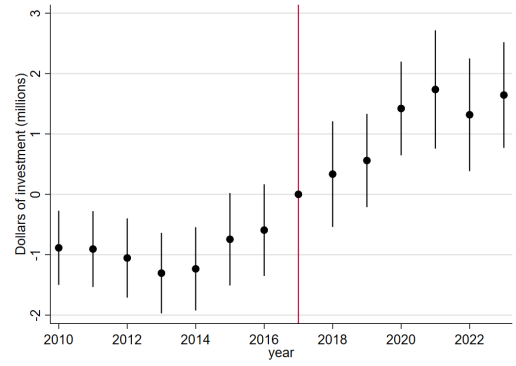

(a) Number of investments




(b) Dollars of investment



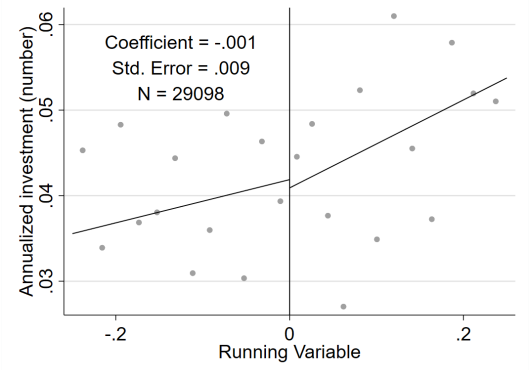

(c) Difference in number*



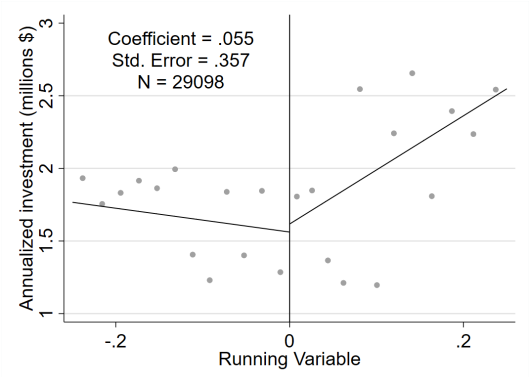

(d) Difference in dollars*

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 residential investments made during the year, and dollars of investment is the sum of all dollars of residential 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.

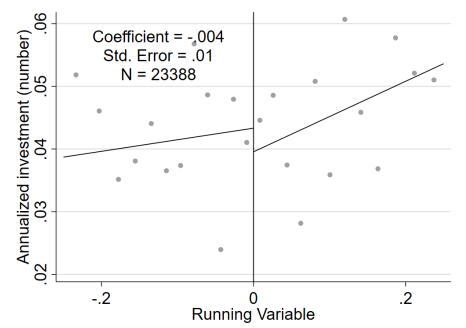
Figure 9: Difference in annualized residential investment between 2013-2017 and 2019-2023, by combined running variable



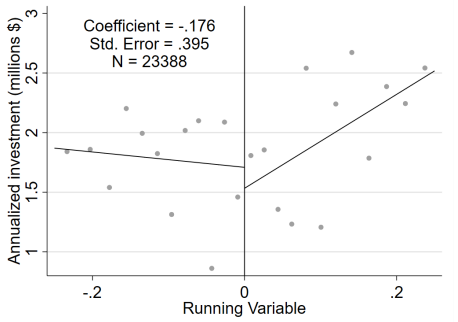

(a) Number of investments: All census tracts




(b) Dollars of investment: All census tracts



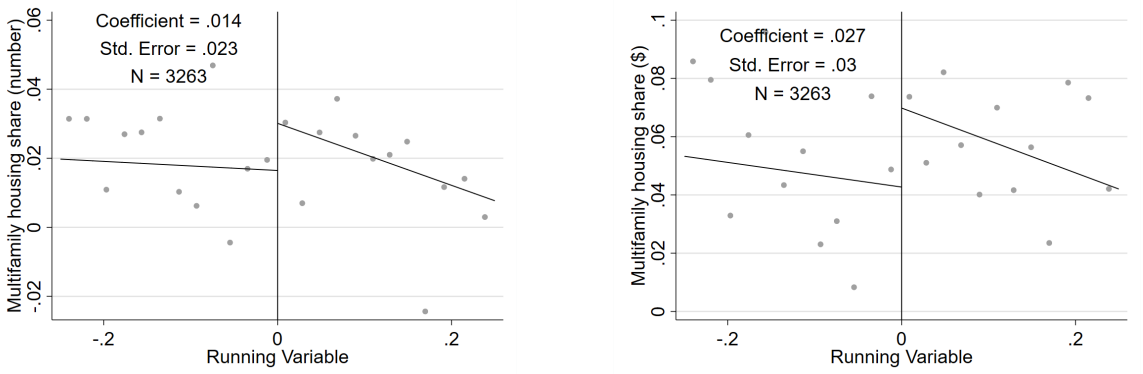

(c) Number of investments: Exclude contaminated controls




(d) Dollars of investment: Exclude contaminated controls

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 linear 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 residential 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: Difference in residential share of investment between 2013-2017 and 2019-2023, by combined running variable

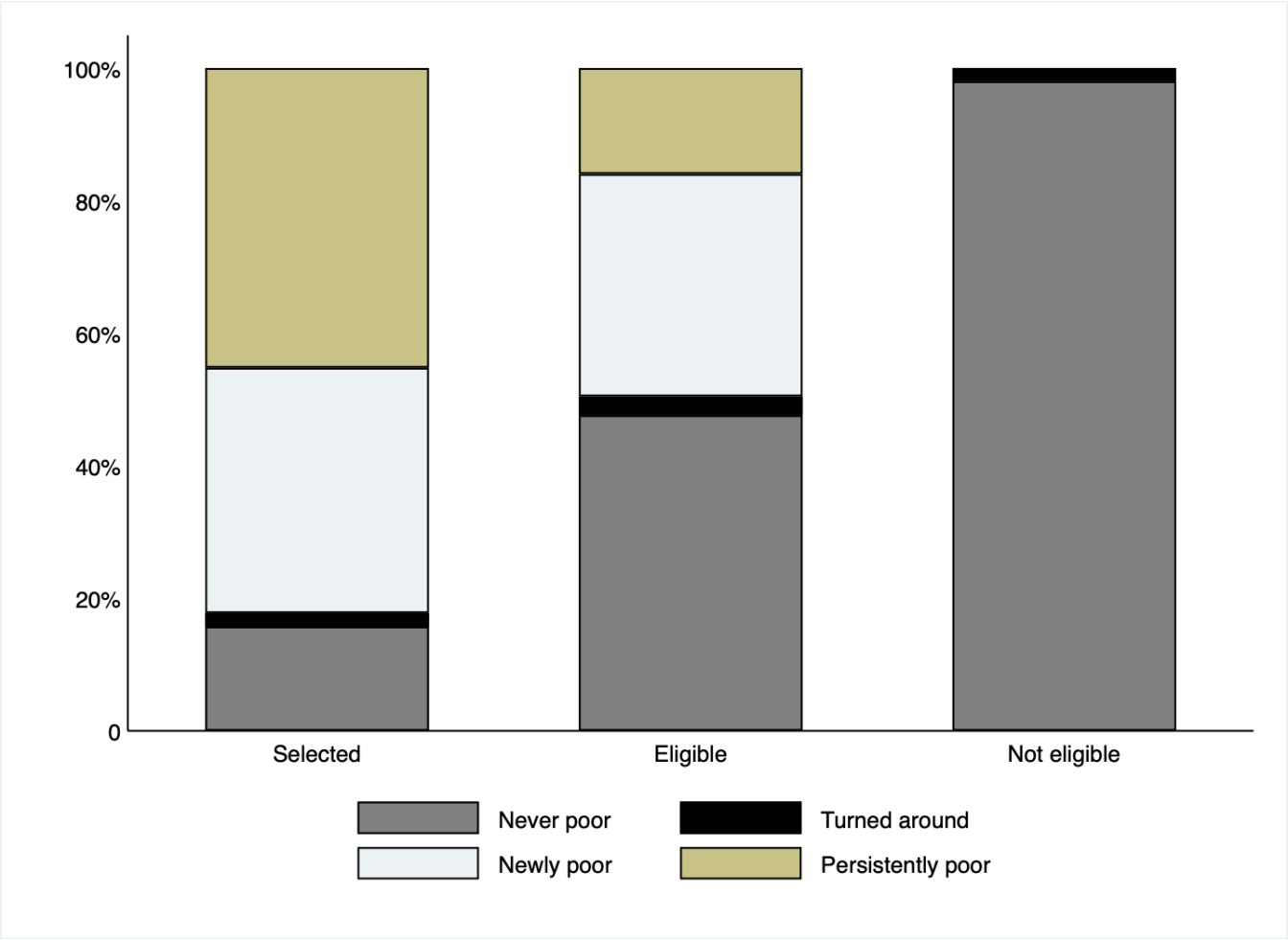



(a) Number of investments

(b) 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 linear 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 residential share of investment, where the share is defined in terms of either number of transactions or total dollars across all transactions. Census tracts with no investment in 2013-2017 or 2019-2023 are dropped. Standard error and number of observations in the bandwidth are reported. Dollar values are adjusted for inflation using the Personal Consumption Expenditures price index.

Figure 11: 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: Tract-level summary statistics, various years

| | Eligible | | Not Eligible |
|---|---|---|---|
| | Selected | Not Selected | |
| **Tract characteristics (American Community Survey, 2013-2017)** | | | |
| *Median Household Income ($)* | 40,824 | 51,258 | 94,178 |
| *Poverty Rate* | 0.306 | 0.214 | 0.076 |
| *Unemployment Rate* | 0.063 | 0.049 | 0.032 |
| *Labor Force Participation Rate* | 0.586 | 0.613 | 0.658 |
| *Urban* | 0.763 | 0.785 | 0.902 |
| **Investment statistics, 2013-2017 (Real Capital Analytics)** | | | |
| *Nonresidential* | | | |
| *At least one transaction (%)* | 0.503 | 0.387 | 0.470 |
| *Mean number of transactions* | 2.170 | 1.470 | 2.052 |
| *Mean aggregate value of transactions (millions $)* | 26.4 | 19.7 | 32.0 |
| *Residential* | | | |
| *At least one transaction (%)* | 0.356 | 0.298 | 0.258 |
| *Mean number of transactions* | 1.008 | 0.800 | 0.580 |
| *Mean aggregate value of transactions (millions $)* | 17.6 | 15.3 | 17.2 |
| **Investment statistics, 2019-2023 (Real Capital Analytics)** | | | |
| *Nonresidential* | | | |
| *At least one transaction (%)* | 0.569 | 0.440 | 0.506 |
| *Mean number of transactions* | 2.659 | 1.749 | 2.325 |
| *Mean aggregate value of transactions (millions $)* | 36.0 | 23.8 | 36.0 |
| *Residential* | | | |
| *At least one transaction (%)* | 0.481 | 0.370 | 0.328 |
| *Mean number of transactions* | 1.528 | 1.023 | 0.783 |
| *Mean aggregate value of transactions (millions $)* | 36.5 | 25.2 | 26.9 |
| Number of tracts | 7,727 | 33,131 | 30,813 |

Sources: American Community Survey, 2013-2017, 5-year pooled sample; Real Capital Analytics, 2013-2023.
Notes: All census tracts in each group, regardless of inclusion in bandwidth for regression discontinuity analysis, are included in the table. Excludes Puerto Rico.

Table 2: Impact of OZ eligibility on difference in annualized commercial investment between 2013-2017 and 2019-2023

| | | Nonresidential | | | | Residential |
|---|---|---|---|---|---|---|
| | Total | Industrial | Office | Retail | Other | Housing |
| **Number of investments** | | | | | | |
| Coefficient | -0.001 | -0.004 | 0.007 | -0.004 | 0.001 | -0.001 |
| Standard error | (0.020) | (0.011) | (0.007) | (0.008) | (0.007) | (0.009) |
| Mean (eligible, 2013-2017) | 0.320 | 0.096 | 0.066 | 0.095 | 0.063 | 0.168 |
| **Dollars of investment (millions $)** | | | | | | |
| Coefficient | -0.043 | -0.039 | 0.121 | -0.099 | -0.250 | 0.055 |
| Standard error | (0.553) | (0.381) | (0.268) | (0.151) | (0.106) | (0.357) |
| Mean (eligible, 2013-2017) | 4.185 | 1.084 | 1.420 | 0.910 | 0.771 | 3.140 |

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 2019–2023 relative to 2013–2017. Dollars of investment is the total dollars of annualized investment made from 2019–2023 relative to 2013-2017. 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.[22]

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.

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

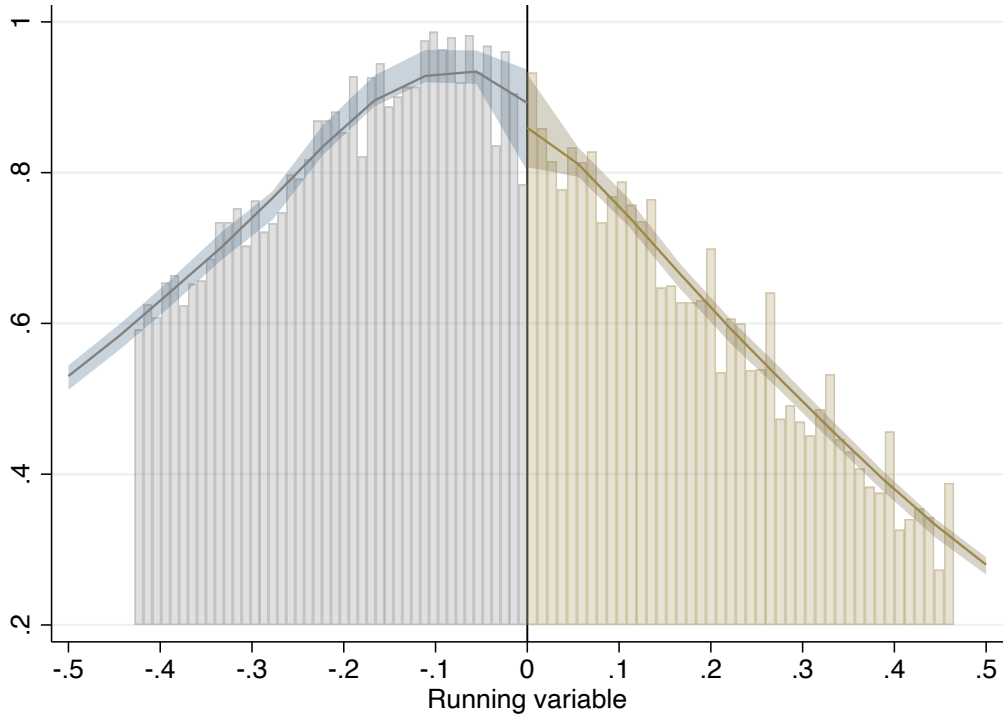
# Appendix B: Tables and figures

Table B.1: Means of dependent variables, eligible census tracts in the bandwidth, 2013-2017

| | Nonresidential | | Residential | |
|---|---|---|---|---|
| Specification | Number | Dollars | Number | Dollars |
| All | 0.32 | 4.16 | 0.14 | 2.80 |
| Urban | 0.40 | 5.37 | 0.18 | 3.73 |
| Bandwith: 0.05 | 0.30 | 4.02 | 0.11 | 2.30 |
| Bandwith: 0.10 | 0.32 | 4.28 | 0.12 | 2.55 |
| Bandwith: 0.50 | 0.34 | 4.28 | 0.17 | 3.21 |
| Poverty running variable | 0.38 | 6.23 | 0.16 | 3.97 |
| Income running variable | 0.30 | 3.82 | 0.11 | 2.38 |
| Income running variable (0-10% poverty) | 0.32 | 4.32 | 0.11 | 3.08 |
| Income running variable (10-20% poverty) | 0.30 | 3.71 | 0.11 | 2.24 |
| Redevelopment only | 0.19 | 1.78 | | |

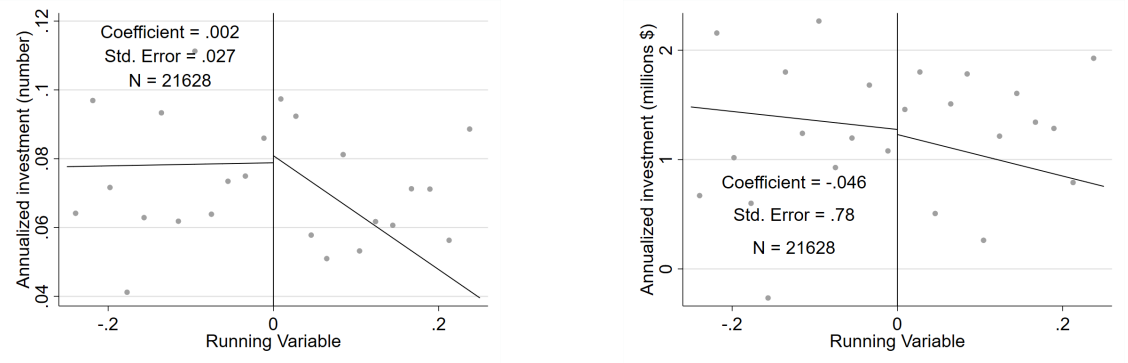
Sources: Real Capital Analytics; American Community Survey, 2011-2015; 2013-2017, 5-year pooled sample; U.S. Department of the Treasury.
Notes: All means are annualized. All dollar values are in millions and adjusted for inflation using the Personal Consumption Expenditures price index. Sample is all eligible census tracts in the bandwidth for the particular specification.

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: Difference in annualized nonresidential investment in urban census tracts between 2013-2017 and 2019-2023, by combined running variable

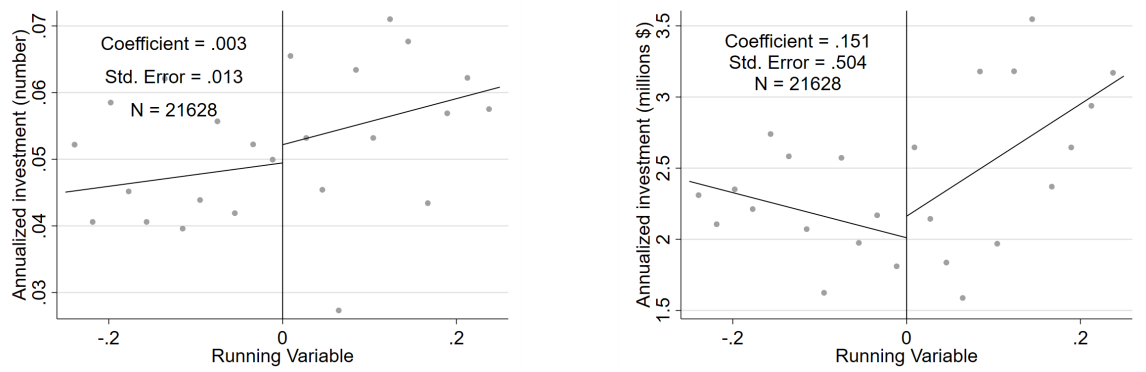



(a) Number of investments

(b) 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 linear 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 nonresidential investment. Standard error and number of observations in the bandwidth are reported. Dollar values are adjusted for inflation using the Personal Consumption Expenditures price index.

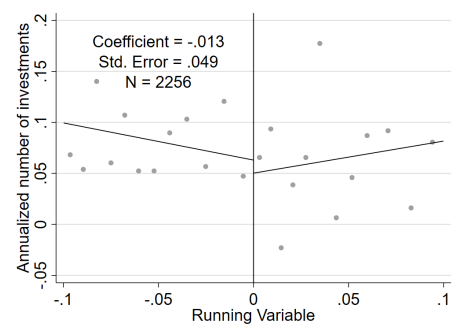
Figure B.3: Difference in annualized residential investment in urban census tracts between 2013-2017 and 2019-2023, by combined running variable



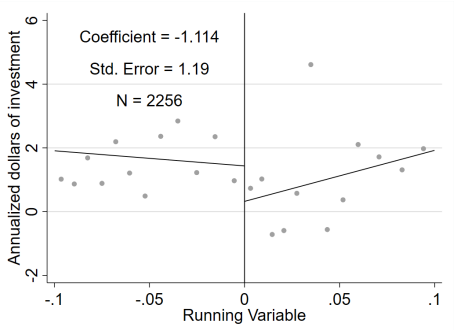

(a) Urban census tracts: Number of investments (b) Urban census tracts: Dollars of investment

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 linear 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 residential investment. Standard error and number of observations in the bandwidth are reported. Dollar values are adjusted for inflation using the Personal Consumption Expenditures price index.

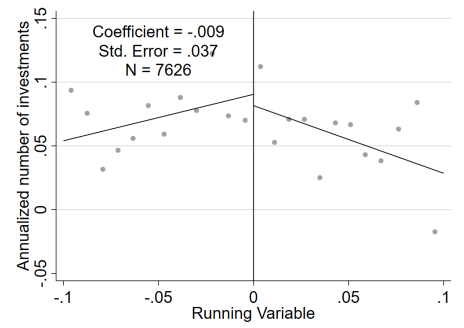
Figure B.4: Difference in annualized nonresidential investment between 2013-2017 and 2019-2023, by income running variable, samples based on poverty rate



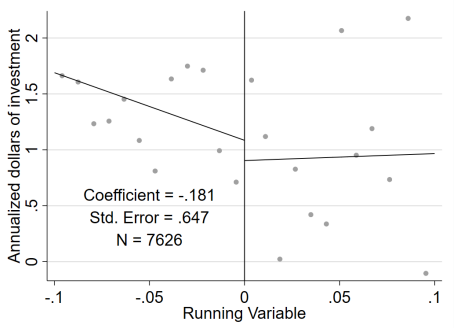

(a) Number of investments: Income running variable, poverty rate between 0% and 10%




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



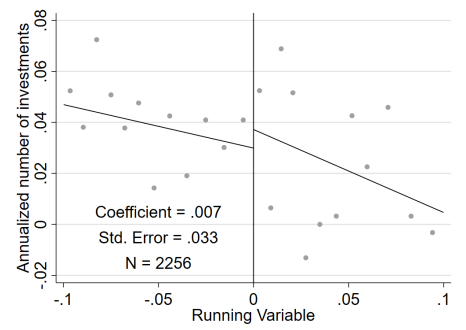

(c) Number of investments: Income running variable, poverty rate between 10% and 20%



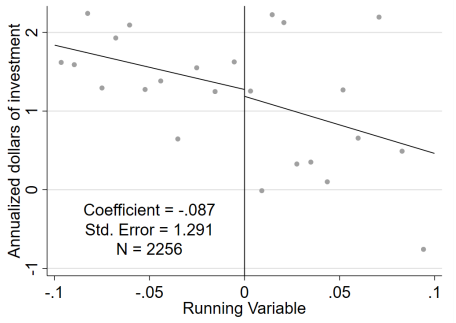

(d) Dollars of investment: Income running variable, poverty rate between 10% and 20%

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 linear fitted separately to points on either side of the cutoff. Coefficient represents the effect of crossing the eligibility threshold on the annual average number or dollars (in millions) of nonresidential investment. Standard error and number of observations in the bandwidth are reported. Dollar values are adjusted for inflation using the Personal Consumption Expenditures price index.

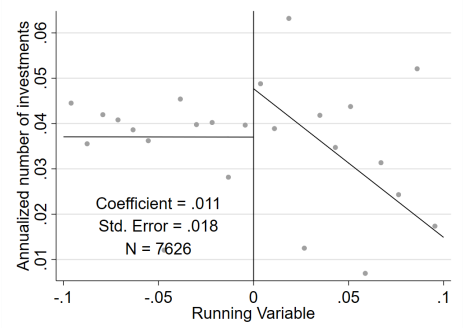
Figure B.5: Difference in annualized residential investment between 2013-2017 and 2019-2023 by income running variable, samples based on poverty rate



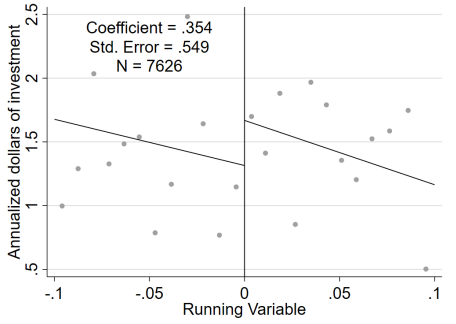

(a) Number of investments: Income running variable, poverty rate between 0% and 10%




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



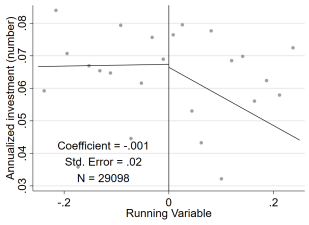

(c) Number of investments: Income running variable, poverty rate between 10% and 20%



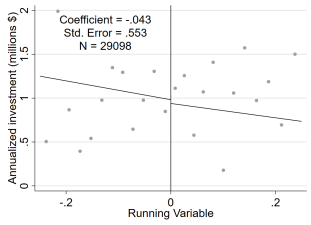

(d) Dollars of investment: Income running variable, poverty rate between 10% and 20%

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 linear fitted separately to points on either side of the cutoff. Coefficient represents the effect of crossing the eligibility threshold on the annual average number or dollars (in millions) of residential investment. Standard error and number of observations in the bandwidth are reported. Dollar values are adjusted for inflation using the Personal Consumption Expenditures price index.

Figure B.6: Difference in annualized nonresidential investment between 2013-2017 and 2019-2023, various polynomials

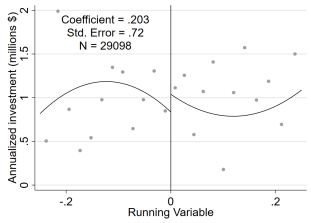



(a) Number of investments: Polynomial Order 1

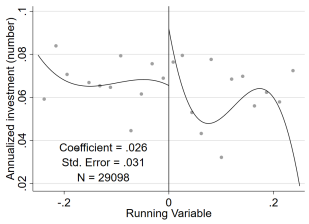



(b) Dollars of investment: Polynomial Order 1

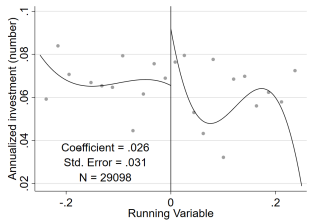

(c) Number of investments: Polynomial Order 2



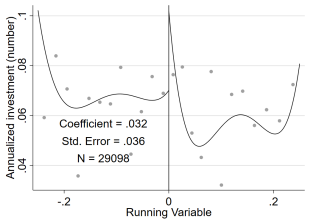

(d) Dollars of investment: Polynomial Order 2



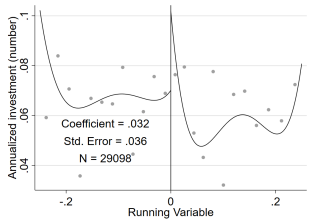

(e) Number of investments: Polynomial Order 3




(f) Dollars of investment: Polynomial Order 3




(g) Number of investments: Polynomial Order 4

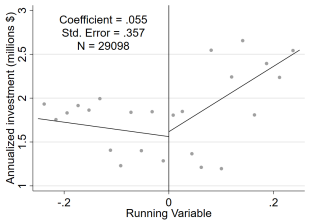



(h) Dollars of investment: Polynomial Order 4

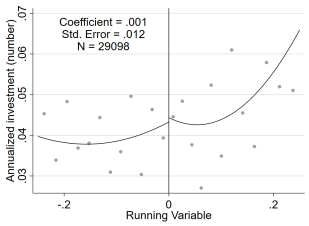
Sources: Real Capital Analytics; American Community Survey, 2011-2015 5-year pooled sample; U.S. Department of the Treasury.
Notes: Each dot represents the sample average within each bin. Fitted lines are various polynomials fitted separately to points on either side of the cutoff. Coefficient represents the effect of crossing the eligibility threshold on the annual average number or dollars (in millions) of nonresidential investment. Standard error and number of observations in the bandwidth are reported. Dollar values are adjusted for inflation using the Personal Consumption Expenditures price index.

Figure B.7: Difference in annualized residential investment between 2013-2017 and 2019-2023, various polynomials

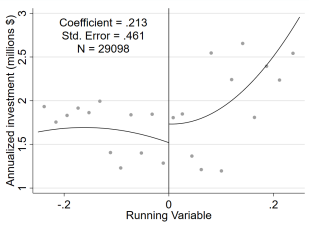

(a) Number of investments: Polynomial Order 1



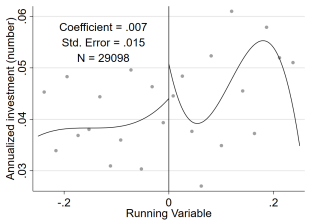

(b) Dollars of investment: Polynomial Order 1



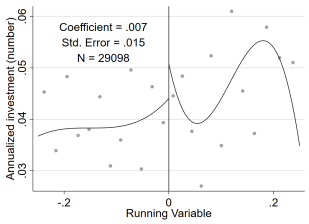

(c) Number of investments: Polynomial Order 2



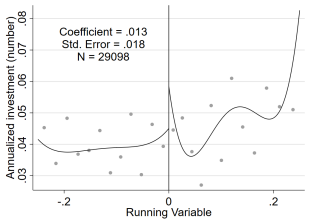

(d) Dollars of investment: Polynomial Order 2



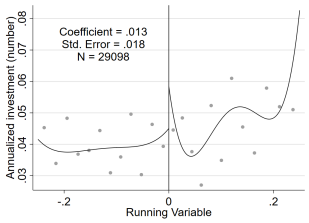

(e) Number of investments: Polynomial Order 3




(f) Dollars of investment: Polynomial Order 3



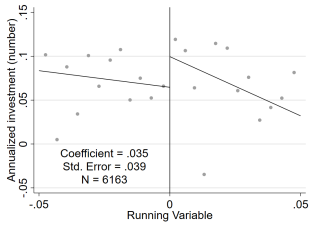

(g) Number of investments: Polynomial Order 4



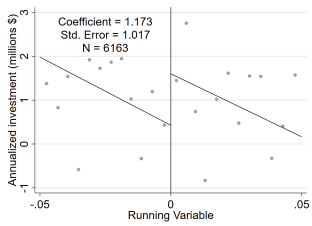

(h) Dollars of investment: Polynomial Order 4

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 various polynomials fitted separately to points on either side of the cutoff. Coefficient represents the effect of crossing the eligibility threshold on the annual average number or dollars (in millions) of residential investment. Standard error and number of observations in the bandwidth are reported. Dollar values are adjusted for inflation using the Personal Consumption Expenditures price index.

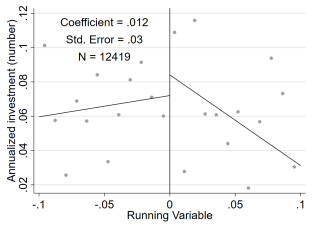
Figure B.8: Difference in annualized nonresidential investment between 2013-2017 and 2019-2023, various bandwidths



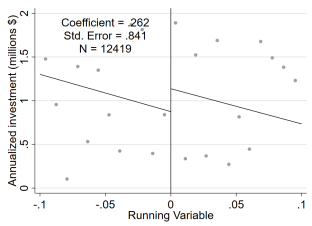

(a) Number of investments: Bandwidth 0.05



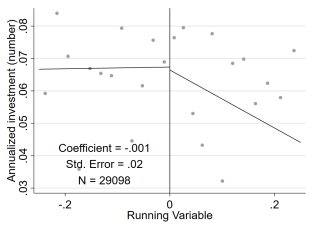

(b) Dollars of investment: Bandwidth 0.05



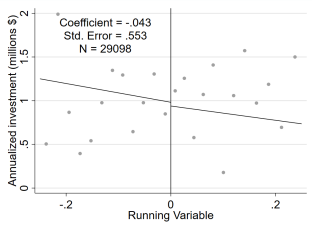

(c) Number of investments: Bandwidth 0.10



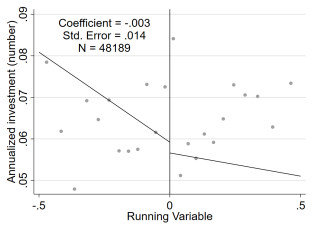

(d) Dollars of investment: Bandwidth 0.10



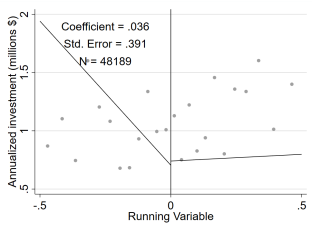

(e) Number of investments: Bandwidth 0.25




(f) Dollars of investment: Bandwidth 0.25



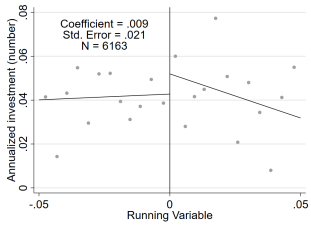

(g) Number of investments: Bandwidth 0.50



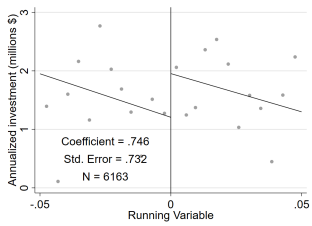

(h) Dollars of investment: Bandwidth 0.50

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 linear fitted separately to points on either side of the cutoff, using different bandwidths. Coefficient represents the effect of crossing the eligibility threshold on the annual average number or dollars (in millions) of nonresidential investment. Standard error and number of observations in the bandwidth are reported. Dollar values are adjusted for inflation using the Personal Consumption Expenditures price index.

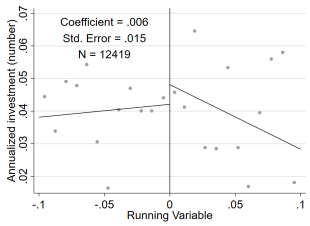
Figure B.9: Difference in annualized residential investment between 2013-2017 and 2019-2023, various bandwidths



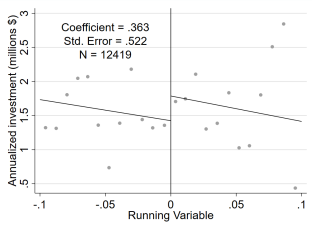

(a) Number of investments: Bandwidth 0.05



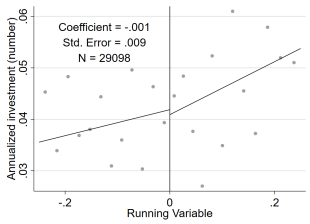

(b) Dollars of investment: Bandwidth 0.05



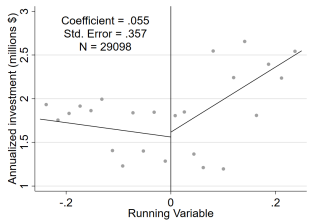

(c) Number of investments: Bandwidth 0.10



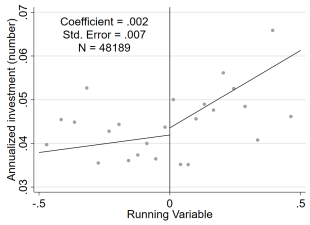

(d) Dollars of investment: Bandwidth 0.10



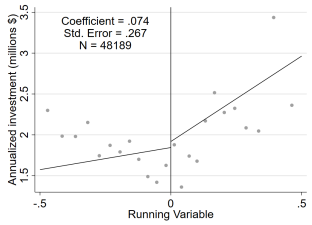

(e) Number of investments: Bandwidth 0.25




(f) Dollars of investment: Bandwidth 0.25



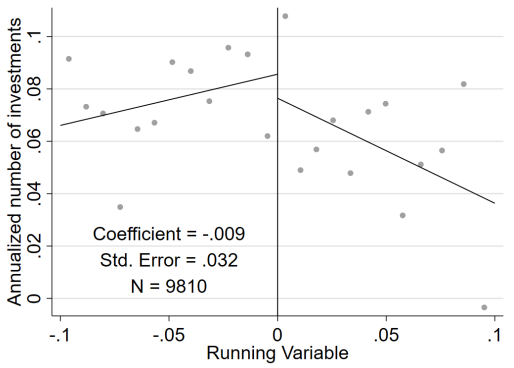

(g) Number of investments: Bandwidth 0.50



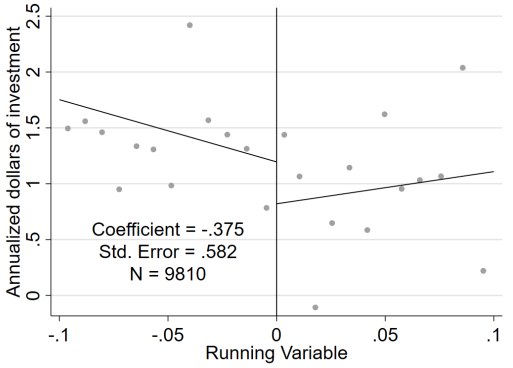

(h) Dollars of investment: Bandwidth 0.50

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 linear fitted separately to points on either side of the cutoff, using different bandwidths. Coefficient represents the effect of crossing the eligibility threshold on the annual average number or dollars (in millions) of residential investment. Standard error and number of observations in the bandwidth are reported. Dollar values are adjusted for inflation using the Personal Consumption Expenditures price index.

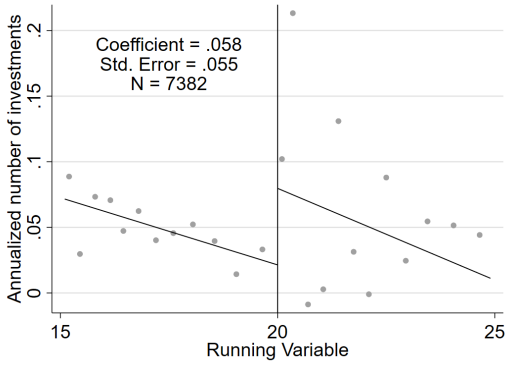
Figure B.10: Difference in annualized nonresidential investment between 2013-2017 and 2019-2023, by alternative running variables



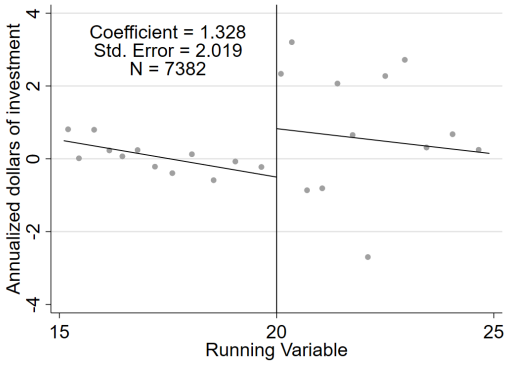

(a) Income running variable: Number of investments




(b) Income running variable: Dollars of investment



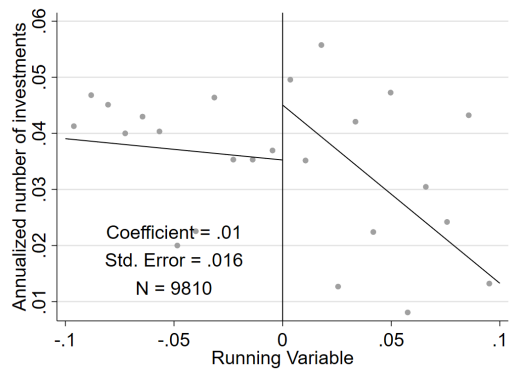

(c) Poverty running variable: Number of investments



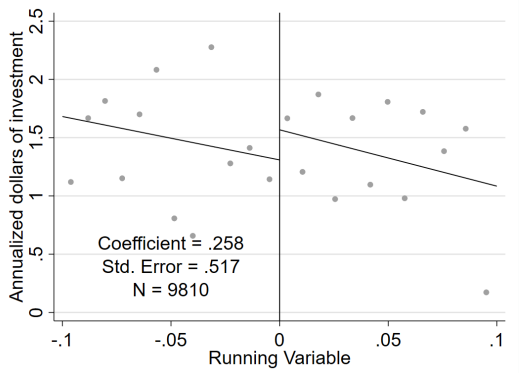

(d) Poverty running variable: Dollars of investment

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 linear fitted separately to points on either side of the cutoff. Coefficient represents the effect of crossing the eligibility threshold on the annual average number or dollars (in millions) of nonresidential investment. Standard error and number of observations in the bandwidth are reported. Dollar values are adjusted for inflation using the Personal Consumption Expenditures price index.

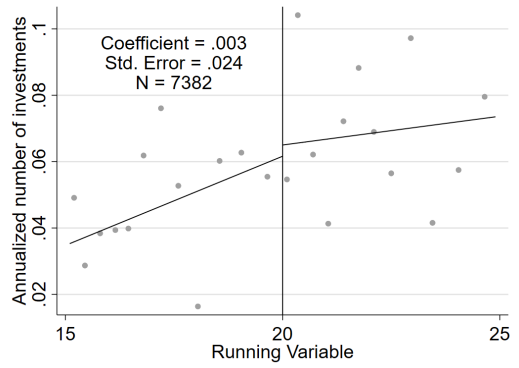
Figure B.11: Difference in annualized residential investment between 2013-2017 and 2019-2023, by alternative running variables



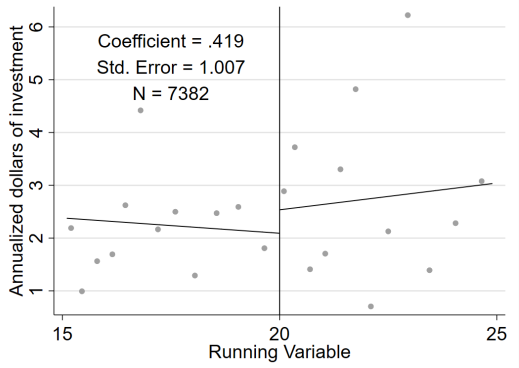

(a) Income running variable: Number of investments




(b) Income running variable: Dollars of investment



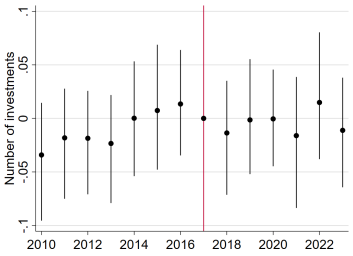

(c) Poverty running variable: Number of investments



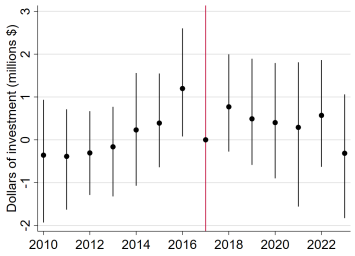

(d) Poverty running variable: Dollars of investment

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 linear fitted separately to points on either side of the cutoff. Coefficient represents the effect of crossing the eligibility threshold on the annual average number or dollars (in millions) of residential investment. Standard error and number of observations in the bandwidth are reported. Dollar values are adjusted for inflation using the Personal Consumption Expenditures price index.

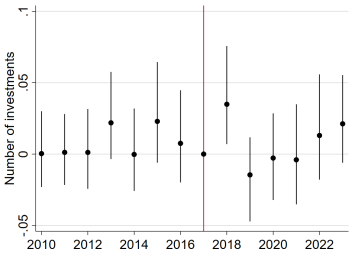
Figure B.12: Annual regression discontinuity estimates of the effect of Opportunity Zone eligibility on the number and dollars of investments in census tracts, by investment type



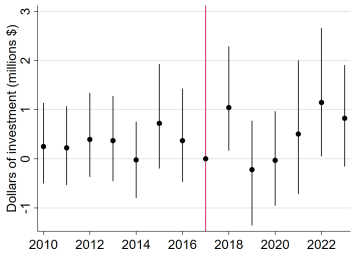

(a) Nonresidential: Number of investments




(b) Nonresidential: Dollars of investment



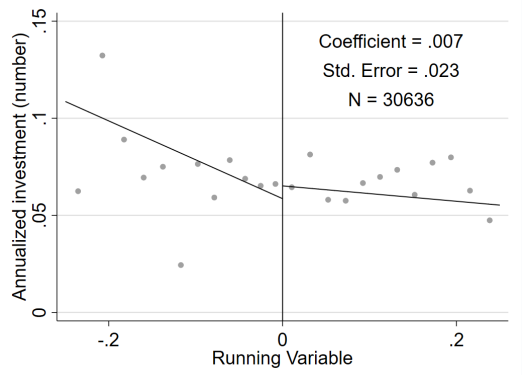

(c) Residential: Number of investments



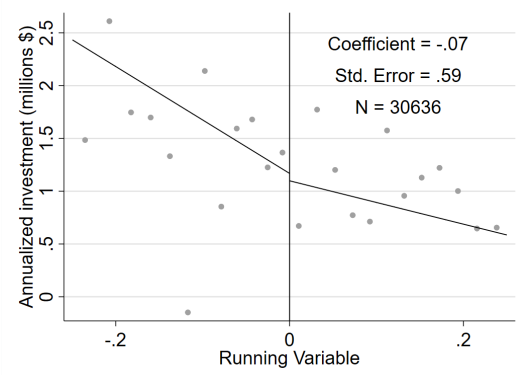

(d) Residential: Dollars of investments

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. Dollar values are adjusted for inflation using the Personal Consumption Expenditures price index.

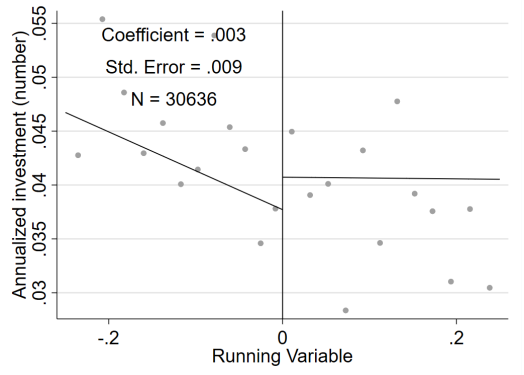
Figure B.13: Difference in annualized investment between 2013-2017 and 2019-2023, placebo running variable



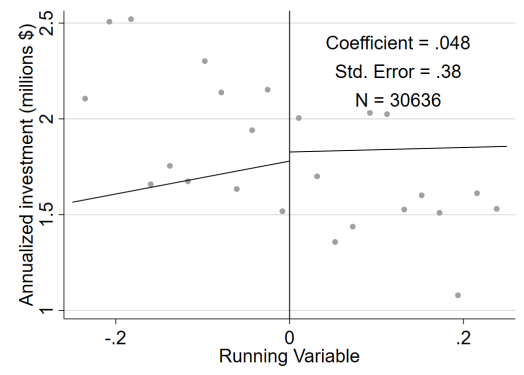

(a) Nonresidential: Number of investments




(b) Nonresidential: Dollars of investment




(c) Residential: Number of investments




(d) Residential: Dollars of investment

Sources: American Community Survey 2011-2015 5-year pooled sample; U.S. Department of the Treasury.
Notes: For purposes of this placebo test, running variable uses a median income cutoff of 100% of MSA or state median income (instead of 80%), and a poverty cutoff of 15% (instead of 20%). Each dot represents the sample average within each bin. Fitted lines are based on a polynomial of degree 1 fitted separately to points on either side of the cutoff. Coefficient represents the effect of crossing the eligibility threshold on the annual average number or dollars (in millions) of nonresidential or residential investment.