The Impact of Opportunity Zones on Commercial Investment and Economic Activity*

Kevin Corinth† Naomi Feldman‡

This draft: January 17, 2023

Abstract

A provision of the Tax Cuts and Jobs Act of 2017 offered tax incentives for investing in certain low-income areas in the United States called Opportunity Zones. The goal of this provision was to spur private investment in OZs in order to improve the economic well-being of their residents. Using a regression discontinuity design and data on the universe of all significant commercial investments in the United States, we find that OZ eligibility led to no statistically significant increase in investment in OZs. We can rule out at the 95 percent confidence level an increase in the probability of investment of more than 1.3 percentage points per OZ (4.9%), an increase in the number of annualized investments of more than 0.01 per OZ (6.7%), and an increase in annualized dollars of investment of more than $0.16 million per census tract (8.2%). These findings are supported by data from Mastercard that show no evidence of increased business activity nor consumer spending. Overall, our findings suggest that the impact of the OZ place-based investment incentives on economic improvement has thus far been limited.

JEL Codes: H53, E22, D61
Keywords: Opportunity zones, investment, tax policy, poverty

*We thank Tim Bartik, Matt Freedman, Elena Patel and numerous conference and seminar participants from the Utah Tax Invitational Conference, NBER Business Taxation in a Federal System Conference, The Brookings Institution, the Online Public Finance Seminar, CREST-Institut Polytechnique de Paris, Institute of Fiscal Studies (London), Hebrew University, Oxford University Centre for Business Taxation, and Tulane University for helpful comments. Arjun Ramani provided excellent research assistance. This research was supported by The Israel Science Foundation (grant no. 1710/19) Declarations of interest: none.

†Harris School of Public Policy, University of Chicago and IZA. kcorinth@uchicago.edu
‡Department of Economics, Hebrew University of Jerusalem, Israel. naomi.feldman@mail.huji.ac.il.

Electronic copy available at: https://ssrn.com/abstract=4086056

1 Introduction

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

In an attempt to address geographic disparities in well-being, the Tax Cuts and Jobs Act (TCJA) of 2017 included a provision that offered flexible tax incentives for investing capital into certain areas called Opportunity Zones (OZs), with the stated purpose of revitalizing economically distressed local economies. Only areas with sufficiently low median incomes or sufficiently high poverty rates were eligible for selection as OZs. Almost 9,000 census tracts—out of about 75,000 in the United States—were ultimately selected as OZs in the middle of 2018, and final rules for investors followed later that year. By one estimate, investors made at least $20 billion of qualifying investment in OZs by the end of 2020.[1] Whether this investment was caused by the OZ tax provisions and whether it boosted economic activity in OZs are key questions for determining whether OZs will ultimately improve the well-being of OZ residents and reverse trends in rising geographic disparity.

[1] https://www.novoco.com/news/qofs-tracked-novogradac-surpass-20-billion-equity-raised

Electronic copy available at: https://ssrn.com/abstract=4086056

Recent studies have evaluated the impact of OZs on tract-level outcomes using a difference-in-differences approach that compares changes in outcomes in OZs to eligible but not selected OZs, though differ somewhat in the control group.[2] The focus has primarily been on house prices and employment outcomes. For example, Sage et al. (2019) use commercial investment data and find that OZ designation increased prices for vacant land and redevelopment properties but not for existing properties, which they interpret as evidence that OZ benefits will simply be capitalized into higher prices without spurring additional investment. 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. (2019) estimate little effect on home prices through the end of 2018 using repeat sales data from the Federal Housing Finance Agency. Council of Economic Advisers (2020) extends the Chen et al. (2019) data through the end of 2019 and finds a modest effect of OZs on home prices. Arefeva et al. (2020) find that OZ designation increased employment growth by 2 to 4 percentage points using private tract-level data on employment. Atkins et al. (2020) use zip code level data on job postings and salary postings, finding that zip codes with OZs have fewer job postings and higher posted salaries than similar zip codes without OZs, but effects are small in magnitude and not consistently statistically significant. Freedman et al. (2021) 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 papers present a mixed picture on the impact of OZ selection and are 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 the

[2]For example, Sage et al. (2019) uses propensity score matching to identify similar tracts and Chen et al. (2019) use geographic neighbors in some specifications.

Electronic copy available at: https://ssrn.com/abstract=4086056

persistence of such outcomes could only be realized with actual improvements in realized investment in these relatively low socioeconomic areas. Using the universe of commercial investments above $2.5 million from 2017 through 2020 in the United States, we take a step back from other papers in the literature to examine whether this primary investment objective is achieved. Understanding whether TCJA's broad, place-based policy incentivizes marginal investment is crucial for better understanding the impact on secondary outcomes such as housing, employment, poverty reduction and, ultimately, the slower economic growth that has held back distressed neighborhoods in the United States.

From a methodological standpoint, we differ from previous studies by using a multi-dimensional regression discontinuity approach that relies on weaker assumptions than the difference-in-differences approach taken in the literature until now. In fact, we find that commercial investment was rising faster in selected census tracts than non-selected tracts prior to OZ selection in 2018, violating a necessary assumption for a difference-in-differences approach. We overcome this problem by instead relying on randomness around the eligibility thresholds in order to identify the effect of OZ eligibility on investment outcomes. In order to be eligible for selection as an OZ, a census tract must have had either a poverty rate exceeding 20 percent or median family income below 80 percent of the area median. We construct a running variable that incorporates both the poverty and median income eligibility conditions such that tracts above the cutoff point of the running variable are eligible and tracts below the cutoff point are ineligible. By comparing investment outcomes in tracts just above the cutoff point with tracts just below, we can credibly estimate the causal effect of OZ eligibility on investment. Because the poverty and median income values used as eligibility conditions for each census tract are based on a U.S. Census Bureau survey conducted between 2011 through 2015—and were published before the OZ provision of the TCJA was

Electronic copy available at: https://ssrn.com/abstract=4086056

passed—census tracts could not have manipulated the determinants of their eligibility for purposes of qualifying as an OZ.

The regression discontinuity approach provides 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 16 percentage points but no statistically significant impact on our investment outcomes. Our estimates rule out with 95 confidence investment impacts greater than 1.3 percentage points for the probability of any investment (a 4.9% increase), 0.13 additional annualized investments (a 6.7% increase) and $0.16 million additional annualized dollars of investment (an 8.2% increase) per OZ.

In subsequent analysis, we scale up our regression discontinuity (intent-to-treat) estimates of eligibility using a fuzzy regression discontinuity design to determine the impact of OZ *selection* on commercial investment and economic activity. As expected given the modest share of eligible tracts actually selected as OZs, the effect of OZ selection on investment remains economically small and statistically insignificant, but the confidence bounds increase offering a less clear picture of the impact of OZ selection. Thus, our results speak most clearly to the lack of impact of the OZ policy in improving the investment outcomes of distressed areas as a whole. Our results are also consistent with a lack of impact for areas actually selected as OZs, but the necessary loss in statistical precision does not allow us to rule out potentially important effects. Finally, we support these commercial investment results with additional data from Mastercard on economic activity and consumer spending. The Mastercard data show no statistically significant impact of OZ eligibility nor selection on new business growth, business loan growth, commercial diversity nor consumer spending.

Taking our evidence as a whole, we conclude that the OZ policy has had, at best, minimal

Electronic copy available at: https://ssrn.com/abstract=4086056

impact on attracting marginal investment to distressed areas in the nearly three years after its passage, and thus, is unlikely to reduce geographic disparities and improve the economic welfare of residents in distressed areas. While our findings speak to a limited time period post-OZ implementation, the financial incentives to invest were strongest for investments made by the end of 2019 and thus it is reasonable to expect that future investments may be more limited.

The paper proceeds as follows: Section 2 provides background on OZs, including how they were selected, investment rules, and the tax benefits. Section 3 describes the American Community Survey data that underlie OZ eligibility determination, as well as the private data sources used to construct tract-level outcomes. Section 4 describes our methodology for identifying the impact of OZs on tract-level outcomes. Section 5 presents results on investment. Section 6 presents supplemental results on business activity and consumer spending. Section 7 discusses results and policy implications. Section 8 concludes.

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

Electronic copy available at: https://ssrn.com/abstract=4086056

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.[3]

In order for a census tract to be eligible for selection as an OZ, it was required to either (a) have an official poverty rate of at least 20 percent; (b) have a median income below 80 percent of the median income in the state or metropolitan area; or (c) be contiguous with a selected census tract meeting one of the conditions in (a) or (b), and have a median income less than 125 percent of the qualifying census tract. Because eligibility is essentially defined by the two dimensions of poverty and income (ignoring the contiguity criterion), we can visualize the eligibility of all census tracts by plotting each tract according to its poverty rate and median income. Notably, this also motivates our regression discontinuity design discussed further in Section 4. In this light, Figure 2 presents four plots where each dot represents a single census tract.[4] The horizontal axis represents the census tract poverty rate. All census tracts to the right of 20 percent are eligible to potentially be OZs because they meet the poverty rate criterion. The vertical axis represents the percent difference between census tract median income and 80 percent of state or MSA median income (whichever is applicable for each census tract). Hence, the horizontal red line at zero reflects the point where the census tract median income exactly equals 80 percent of state/MSA median income. All census tracts below this red line are eligible to potentially be OZs because they meet the median income criterion.

Figure 2 panel (a) shows the breakdown of all census tracts by their eligibility status.

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

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

Electronic copy available at: https://ssrn.com/abstract=4086056

Ineligible census tracts in the top-left quadrant represent about 50 percent of total census tracts and the remaining 50 percent of census tracts fall into one of the three remaining quadrants dependent upon the eligibility requirements they meet (poverty rate and/or median income). 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—census tracts with less than a 20 percent poverty rate and census tract median income above 80 percent of the MSA or state median. Panel (c) contains eligible but not selected census tracts. Panel (d) contains selected census tracts. As these panels show, the majority of eligible census tracts qualify based on both eligibility dimensions, although 72 percent of selected census tracts are eligible on both dimensions compared to 57 percent of eligible but not selected census tracts. This suggests that governors selected census tracts that were relatively more economically disadvantaged among all eligible census tracts.[5]

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 for 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 percent of the original capital gain is excluded from taxation, and if the investment remains for at least 7 years, then 15 percent of the original capital gain is excluded from taxation. After 2026, the capital gains must be realized and the appropriate portion subject to taxation. Furthermore, any capital gains accrued based on the investment in the QOF (above the original capital gain) are not subject

[5]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.

Electronic copy available at: https://ssrn.com/abstract=4086056

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.[6] 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).

Opportunity Zones represent the first place-based policy that allows uncapped private investment into areas throughout the United States. The OZ provision is also fairly broad in terms of the type of investments that receive preferential tax treatment and are untied to any particular outcome variable, such as employment, as were many other previous efforts. The most closely related effort, the New Markets Tax Credit, requires pre-approval and caps funds invested in designated areas, which are less evenly distributed throughout the country. Thus, the success (or lack thereof) of this policy will be instrumental in informing the flexibility of investment incentives in future place-based policies.

3 Data

The major data sources we use are (i) tract level data from the American Community Survey (ACS) which were used to define eligibility for OZ designation, (ii) transaction-level investment data from Real Capital Analytics, and (iii) tract-level credit-card and point-of-interest data from Mastercard that relate to business activity and consumer spending.

The ACS is an annual household survey conducted by the United States Census Bureau.

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

Electronic copy available at: https://ssrn.com/abstract=4086056

It samples about 2 million households per year in addition to people living in group quarters. The relatively large sample size allows Census to produce statistics at detailed geographic levels, especially when combining multiple survey years. Particularly important for our purposes, Census publishes census tract level poverty rates and median family income based on 5-year pooled samples of the ACS. These published poverty rates and median family income estimates were used to determine eligibility for OZ selection. Tracts could meet eligibility standards based on the 2011-2015 pooled sample or the later released 2012-2016 pooled sample, although in practice only 49 census tracts selected as OZs (out of over 8,700 selected OZs) were eligible on the basis of the 2012-2016 ACS but not the 2011-2015 ACS (see Internal Revenue Service 2018).

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

To measure investment we use the Real Capital Analytics (RCA) commercial investment database that contains transaction level data for the entire United States on commercial investments valued at over $2.5 million from 2010 through 2020 and a subset of transactions below that threshold.[7] RCA covers about 95% of all commercial real estate transactions above this threshold in the United States. The data contain numerous details on each transaction, such as price, age of structure, type of transaction (e.g., new construction or sale of existing structure), address, buyer objectives, buyer and seller information, and many details on financing of the loans. The large majority of transactions are for investment

[7]Once a property sells for $2.5 million it will stay in the database, even if it sells again in the future below this threshold. In addition, RCA backfills transaction prices, if possible, once a property hits $2.5 million threshold. Thus the data are not "truncated." About 12 percent of observations have prices below $2.5 million (conditional upon a positive sale).

Electronic copy available at: https://ssrn.com/abstract=4086056

objectives (86 percent), with another seven percent dedicated towards redevelopment or renovation (and the remainder made for occupancy purposes). We aggregate investments to the census tract level over different time periods, focusing on outcomes such as number of transactions and their sale prices.[8]

To measure business activity we use tract-level data from Mastercard's Center for Inclusive Growth. The data contain 18 metrics from multiple data sources (some based upon proprietary credit card data from Mastercard, while others are either outsourced to other data providers and made available by Mastercard or available publicly) in order to generate their final product called the "Inclusive Growth Score" for each census tract.[9] We make use of only a small subset of these input variables that are directly related to business growth and household spending. In particular, we use measures of the percentage growth of net new businesses based on anonymized and aggregated location point of interest data (that is, new credit card machines), commercial diversity (percentage of all industries that are represented in the census tract), percentage growth of the number of small business loans, and two measures of spending growth (aggregate and per capita) based upon proprietary Mastercard data.[10] The data are annual from 2017-2020 but not every variable is available in each year. Table B.1 provides additional details on the Mastercard variables.

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

[8] All dollars are adjusted for inflation using the Personal Consumption Expenditures price index.

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

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

Electronic copy available at: https://ssrn.com/abstract=4086056

better off economically than eligible census tracts. It is also clear from Table 1 that among the eligible census tracts, states chose tracts that are, on average, more distressed, with lower incomes, higher poverty rates, higher unemployment rates, and lower rates of labor force participation. This suggests that simply comparing outcomes in OZs to outcomes in eligible census tracts that were not chosen is likely to confound causal impacts of OZ designation with non-random selection.

Although selected tracts are worse off, they were more likely to receive investment than eligible tracts that were not selected prior to the OZ policy. This could reflect a preference by governors to select census tracts that are more likely to benefit from the OZ tax incentive. At the same time, sale prices were lower both in raw and size-adjusted terms in selected tracts, potentially reflecting their economic deprivation. Notably, industrial investments comprised 43.1 percent of all commercial investments in selected tracts, substantially higher than the share in other types of tracts.

The Mastercard data on business activity and consumer spending present a mixed picture. Selected tracts have on average a lower new business growth rate than eligible but not selected or ineligible tracts, but at the same time they have a similar growth rate of business loans to eligible but not selected tracts, a rate that is substantially higher than that of ineligible tracts. The average ranking of the growth rate of per capita spending for selected tracts is a bit higher than the other two groups but that of aggregate tract level spending is a bit lower.[11]

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

Electronic copy available at: https://ssrn.com/abstract=4086056

4 Research Design and Methods

TCJA allowed state governors to designate a subset of eligible low-income or high-poverty census tracts as OZs. While it appears that many states approached the selection process in a systematic way (Frank et al. 2020), many of the selected tracts were chosen based upon idiosyncratic factors that are 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 seen in Table 1. Trends prior to OZ selection may differ as well, with Frank et al. (2020) and Eldar and Garber (2020) finding that selected tracts were experiencing faster economic growth than eligible but not selected tracts prior to selection. In Figure 3, we show the trends in investment for each of our census tract groups. For each group of tracts, the presence of any investment, the number of investments, and dollars of investment increase until about 2015 and then flatten out, before dipping in 2020 in conjunction with the onset of the COVID-19 pandemic. However, the rate of growth 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 presence of investment and number of investments until 2016, at which point investment in selected tracts starts to grow more quickly. Selected tracts also begin to close the gap in terms of dollars of investment with ineligible tracts around the same time. Compared to eligible but not selected tracts—the

Electronic copy available at: https://ssrn.com/abstract=4086056

key comparison group for difference-in-difference designs—selected tracts see the presence and number of investments grow faster throughout the sample period, and faster growth in dollars of investment starting in 2013. To formalize these observations, in Figure 4 we plot annual differences between selected and eligible but not selected tracts, relative to their difference in 2017. The figure confirms the patterns visible in the raw trends, violating the parallel trends assumption needed for the validity of the difference-in-difference methodology.

In order to overcome the non-random selection of OZs, we instead use a multivariate regression discontinuity (RD) approach that takes advantage of how the eligibility of census tracts was determined, creating a natural experiment that assigned eligibility to some census tracts but not others based on arbitrary factors unrelated to outcomes of interest. The RD design relies on the qualification criteria for OZs—as noted earlier, census tracts were deemed eligible for OZ selection if they met at least one of the following conditions:

- had a poverty rate of at least 20 percent, or
- had a median family income below 80 percent of either the state median family income or the MSA median family income, or
- were contiguous with a selected tract based upon the first two qualifications, and had a median family income that does not exceed 125 percent of the median family income of at least one contiguous selected tract.[12]

In the RD design, we exploit the sharp poverty rate and income eligibility cutoffs.[13] Census tracts with poverty rates just below 20 percent or median family income just above

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

[13] 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.

Electronic copy available at: https://ssrn.com/abstract=4086056

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.[14]

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 focus first on the 20 percent poverty rate. In census tracts with median income above the 80 percent threshold, passing the 20 percent poverty rate threshold moves a census tract from ineligible to eligible. In contrast, if the census tract has median income below 80 percent of the threshold, passing the 20 percent poverty rate has no impact on OZ eligibility because being below the 80 percent income threshold, by definition, deems the census tract eligible. A similar idea applies to the median income threshold. In census tracts with a poverty rate below the 20 percent 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 5, we verify that crossing the poverty and income thresholds, when binding for determination of eligibility, lead to discrete increases in the probability of selection as an OZ. Groups of census tracts with approximately equal poverty rates are placed in bins

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

Electronic copy available at: https://ssrn.com/abstract=4086056

using spacing estimators under the mimicking variance evenly-spaced method (Calonico et al. (2014)), and we calculate the share that were 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 percent of the MSA or state median. As seen in the figure, census tracts with poverty rates above 20 percent 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 percent of the MSA or state median. In this case, crossing over the 20 percent 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 percent 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 percent poverty rate and then using the 80 percent 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 percent—census tracts on both sides of the income threshold are eligible. However, in census tracts with a poverty rate below 20 percent, passing from just above the income threshold to just below substantially increases the probability of being selected as an OZ (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

Electronic copy available at: https://ssrn.com/abstract=4086056

particular, we construct the running variable r:

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

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

Figure 6 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.[15] Following Cattaneo et al. (2018), we test whether there is any evidence of manipulation around the threshold of our combined running variable and find that we do not reject the null hypothesis of no systematic manipulation ($p > .94$). Figure 7 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.

In the analysis that follows, we first estimate the impact of OZ eligibility on investment

[15] It is unclear how Treasury determined these technical corrections. They could play a role in anecdotal stories of corruption in the OZ selection process. It is reassuring that these tracts based on technical corrections only represent around 0.6 percent of all selected tracts and our results are robust to excluding them from the analysis.

Electronic copy available at: https://ssrn.com/abstract=4086056

and other economic outcomes, and second, the impact of OZ selection on these same outcomes. The former answers the question of what impact the OZ policy had on distressed areas near the eligibility cutoff. The latter answers the question of what impact OZ selection had on selected OZs near the eligibility cutoff.

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—investment or commercial activity in census tract i—typically specified as the change in the outcome variable before versus after the OZ provision went into effect; r_i is the running variable; f is a polynomial function; D_i is a binary indicator based on eligibility status; and X_i is the vector of observable, predetermined 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 (or below in the case of median income) the cutoff c are eligible to be selected as OZs.

In order to estimate the impact of OZ *selection* on investment and other economic out-

Electronic copy available at: https://ssrn.com/abstract=4086056

comes outcomes, we estimate the following fuzzy regression discontinuity model:

$$Y_i = f(r_i) + T_i\gamma + X_i\beta_1 + \epsilon_i \tag{4}$$

$$T_i = g(r_i) + D_i\delta + X_i\beta_2 + \mu_i \tag{5}$$

$$D_i = 1[r_i \geq c] \tag{6}$$

where T_i is a binary treatment indicator based on OZ selection; and f and g are polynomial functions. We allow for imperfect compliance in Equation 5 since not all eligible census tracts are selected as OZs. Table 2 presents parametric estimates of the first stage results from Equation (5) using the three potential running variables—poverty, income, or the combination. For each running variable, the table reports the impact of crossing the cutoff point on the probability of being selected as an OZ. To do so, following the recommendation of Gelman and Imbens (2019), we fit a local linear polynomial below the cutoff, a local linear polynomial above the cutoff, and a dummy variable for the cutoff itself. Results confirm the graphical evidence of a substantial impact of crossing the threshold on the probability of selection. Tracts are between 11 and 16 percentage points more likely to be selected as an OZ when just crossing the cutoff point. We also document in Figure B.1 that there is no evidence of a discontinuity in our various control variables, suggesting that which side of the eligibility threshold census tracts fall on is as good as random.

Electronic copy available at: https://ssrn.com/abstract=4086056

5 The impact of OZs on investment

5.1 Regression discontinuity

We consider three primary investment-related outcomes for our regression discontinuity analysis that identify the impact of OZ eligibility. These include (i) whether any investment occurred, (ii) the number of investments, and (iii) dollars of investment. With the exception of the indicator variable for whether any investment occurred in the treatment period, we focus on changes in each outcome variable. In particular, we take the difference of the annualized value between July 1, 2018 and December 31, 2020 and the annualized value between July 1, 2015 and December 31, 2017.[16] Differencing nets out any time-invariant census tract effects, which although is not necessary to obtain an unbiased estimate given our use of an RD design, can nonetheless increase precision.[17] In later robustness tests, we also consider specifications of the outcome variables in level terms.

The impact of OZ eligibility (intent-to-treat) on the three investment outcomes is shown graphically in Figure 8. There is no discernible jump at the discontinuity for any of our investment variables, 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.

Table 3 presents parametric estimates that confirm the graphical evidence.[18] 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 em-

[16] We exclude the first half of 2018 as the implementation was finalized over this period. These specifications overcome 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 including the second half of 2018 in the post-period.

[17] 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 on our outcome variables from this program.

[18] Table B.2 presents the results without controls and shows they are little changed.

Electronic copy available at: https://ssrn.com/abstract=4086056

ployed in each industry, all based on the 2013-2017 ACS. Estimated impacts of OZ eligibility are generally small, occasionally negative, and not statistically different from zero in any of the specifications. For example, when using the combination running variable, OZ eligibility reduces the probability of investment by 1.1 percentage points (4.3%), reduces the number of investments by 0.003 (1.7%), and decreases dollars of investment by $0.07 million (3.8%). While the results are unexpectedly negative, they all point to a near-zero impact of OZ eligibility on investment. Once we take into account the standard errors around our estimated results, we can rule out at the 95 percent level of confidence estimates larger than a 1.3 percentage point (4.9%) increase in the probability of investment, 0.01 (6.7%) more investments, and an additional 0.16 million dollars of investment (an 8.2% increase). To further put these results in context, we calculate how much of the investment gap between eligible and non-eligible tracts is closed by our estimated treatment effects in Table 4. For example, the gap in dollars of investment is equal to $913,000 and the upper bound based upon the 95 percent confidence interval is equal to $156,000. Thus, we can rule out at the 95 percent confidence level that more than 17 percent of the investment gap is closed.

Focusing on the combination running variable, Figure B.2 and Table B.3 show that these null results are robust to variations in the bandwidth around the threshold and Figure B.3 and Table B.4 show similarly for variations in the fitting polynomial.[19] Appendix Figures B.4-B.9 and Table B.5 replicate the baseline estimation on various subsamples of our data. In particular, we consider different types of tracts (urban and rural), different types of investment (industrial, office and retail), different specifications of our outcome variables (levels in individual years, levels in the treatment period, growth in 2020 relative to 2019), and different sample restrictions (dropping contiguous tracts and dropping selected contiguous

[19] The first row of Table B.4 is the same as the final row of our main results table, Table 3 as our baseline is a linear specification (polynomial of order one).

Electronic copy available at: https://ssrn.com/abstract=4086056

tracts). In each case, we see no evidence of more investment when crossing the eligibility threshold.

5.1.1 Heterogeneity

The RD approach provides an estimate of the impact of OZ selection on investment locally around the cutoff point and allows us to conclude what would happen if the program were expanded on the margin but does not provide an overall assessment without an implicit assumption of homogeneous treatment effects across all tracts in the United States. The census tracts used to identify the effect of eligibility in our analysis are relatively better off in terms of poverty and relative median income than eligible tracts that are far from the cutoff points (see Table B.6 for census tract characteristics based on distance from the eligibility cutoff point). As such, one may imagine that because the census tracts in our RD sample are more attractive for investors than those facing more extreme levels of economic distress, they would likely have larger treatment effects. In fact, using proprietary IRS data, Kennedy and Wheeler (2022) document that only about 37% of selected tracts received positive investment by the end of 2020 from QOFs and that these tracts are better off in terms of observable characteristics (levels and growth rates) such as education, median home value, and median household income compared to selected OZ tracts that received no investment. Such tracts are more likely to be closer to the eligibility thresholds.

In addition, most investment activity in general occurs near the thresholds, and so treatment effects in tracts far from the thresholds are likely to be less relevant for place-based policies seeking to encourage private investment. Figure 9 shows the distribution of investment (presence of any investment, number of investments and dollars of investment) over all census tracts in the pre-period (July 2015 through December 2017). As indicated by the

Electronic copy available at: https://ssrn.com/abstract=4086056

brighter-colored pixels, these heat maps show that the majority of investment (62 percent of the number of transactions and 68 percent 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 percent of all transactions and 23 percent of dollars of investment undertaken in tracts within 5 percentage points of the poverty threshold or 20 percent of the income threshold. The remaining 13 percent of transactions and 10 percent 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 even without OZ tax incentives. This lends support to the view that our results capture an upper bound of the average treatment effect over all eligible tracts.

Finally, we directly test whether worse-off tracts (within the bandwidth of tracts near the thresholds) have stronger investment effects of OZ eligibility. Table 5 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), but 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 on these two subsamples. As the Table 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. 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

Electronic copy available at: https://ssrn.com/abstract=4086056

eligibility effects would be significantly different further away from the combined running variable threshold.

5.1.2 Spillover effects

One potential concern for the lack of any robust impact on investment is spillover effects. It is possible that when investment flows into OZs, the (expected) economic development generated by such investment may also incentivize investment in nearby, but non-selected areas. There is an important distinction to make here. If spillovers mainly occur in nearby eligible but not-selected tracts, our RD results would be biased upwards as this would increase the amount of overall investment that occurs just over the eligibility threshold of the running variable. Thus, to the extent that our findings are potentially upwardly biased by spillovers, this would strengthen our conclusion that the OZ legislation did not lead to significant increases in investment. Alternatively, if spillovers occur in nearby *ineligible* census tracts then this could potentially invalidate the RD design. In order to address this concern, we focus on a subsample of tracts that minimizes the extent of contiguity between eligible and ineligible tracts by restricting to counties that contain only one type of tract. In other words, we exclude all counties that contain a mix of eligible and ineligible tracts and replicate our analysis for the combination running variable on the roughly 2,000 census tracts that remain after this restriction. Thus, the ineligible census tracts just below the cutoff exist in counties without any eligible tracts and likewise, eligible census tracts that are just above the cutoff exist in counties without any ineligible tracts, thus minimizing any spillovers.[20] The final row of Table 5 shows that the point estimates turn positive, though are not statistically

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

Electronic copy available at: https://ssrn.com/abstract=4086056

different than our baseline estimates, nor are they statistically different than zero.

5.2 Fuzzy regression discontinuity

We next estimate treatment on the treated effects for our main results by using a fuzzy regression discontinuity design. This allows us to estimate the impact of OZ selection on investment outcomes. In particular, we estimate the second-stage equation (Equation 4) where the first stage estimates the probability of selection upon crossing the threshold (recall Table 2). Table 6 reports estimated effects for the same specifications shown in Table 3, with the only difference being that we use a fuzzy design based on the actual selection of tracts as OZs.[21] The point estimates from this exercise are, as expected, larger in absolute value but all remain statistically insignificant at conventional levels. For example, estimates for the combination running variable imply that OZ designation decreases the probability of any investment by 14.7 percentage points (56%), decreases the number of investments by 0.04 (21%), and decreases dollars of investment by $0.88 million (48%). In each of these cases, we would expect a positive result if OZ selection increased investment. Along with the larger point estimates (in absolute value), the standard errors do not allow us to claim precise null effects. For example, when using the combination running variable, we can rule out at the 95 percent confidence level an increase in the probability of investment of more than 9.7 percentage points per OZ (37%), an increase in the number of investments of more than 0.14 per OZ (70%), and an increase in investment of more than $1.3 million per OZ (70%). These upper bounds represent economically important effects. Thus, while the impact of OZ selection is not statistically different from zero, the effect cannot be precisely determined.

[21] See Table B.7 for estimates without control variables.

Electronic copy available at: https://ssrn.com/abstract=4086056

6 The impact of OZs on business activity and consumer spending

Despite the consistent lack of any positive and statistically significant impact on commercial investment, we also estimate whether there are general improvements in other measures of economic development such as outcomes related to business formation and consumer spending. While the lower bound of $2.5 million per transaction to appear in the RCA data is not particularly restrictive for commercial investment, we recognize that it does not capture small, perhaps numerous, investments as well as general improvements due to increased economic activity that may have occurred absent new investment. The Mastercard data address this gap by providing census tract-level aggregates of business-related activities and proprietary data on consumer spending as described in Table 1.

Figure 10 presents graphical evidence on the impact of OZ eligibility on business-related activities and consumer spending over 2019 and 2020. The figure shows no discernible jumps in any of our outcome variables upon crossing the eligibility threshold when using the combined running variable.[22] Table 7 further supports the RD figures.[23] For example, when using the combination running variable, OZ eligibility reduces new business growth in 2019 by 0.09 percentage points (0.6%), increases business loan growth by 1.28 percentage points (14%), increases commercial diversity by 0.07 percentage points (0.3%), improves the relative ranking of eligible census tracts in terms of per-capita spending growth by 0.89 percentage points (1.8%), and reduces overall spending growth by 0.72 percentage points (1.4%).

Taken as a whole, the results point to a near-zero impact of OZ eligibility on business

[22] Note that the Covid-19 pandemic appears to not have had any differential impact on OZ in terms of the economic activity variables considered here.

[23] See Table B.8 for regression discontinuity results without controls.

Electronic copy available at: https://ssrn.com/abstract=4086056

activity and consumer spending.[24] Once we take into account the standard errors around our point estimates, we find that we can rule out at the 95 percent level of confidence estimates larger than 0.81 percentage points (6%) for new business growth, 2.63 percentage points (29%) for business loan growth, 0.48 percentage points (2.3%) for commercial diversity, 2.8 percentage points (5.6%) in the relative ranking of spending growth per-capita and 1.0 percentage points (2.1%) in aggregate census tract level spending growth. These upper bounds point to at best a modest impact on the business activity and consumer spending outcomes. Table 8 presents fuzzy RD effects that are not statistically different from zero, though again measured with less precision.[25]

7 Discussion

It is informative to place estimated impacts of OZs into the context of the broader literature that evaluates place-based policies attempting to address geographic disparities. Major previous federal efforts include Empowerment Zones and the New Markets Tax Credit (NMTC). Empowerment Zone programs generally offered tax incentives for businesses that locate in specified areas or that hire employees who live in such areas. A number of states had their own Empowerment Zone programs before the federal government's Empowerment Zones and Enterprise Communities Act of 1993. Evidence on the effectiveness of Empowerment Zones has been mixed. Based on California's state-based program, O'Keefe (2004) found significant gains in employment resulting from Enterprise Zone designation, while Neumark and Kolko (2010) found no such effects when controlling for changes in zone borders over time. Focus-

[24] We note that business loan growth is statistically significant at the ten percent level and is arguably economically significant as well. It may offer some foreshadowing of future positive business growth. It is, however, one weak signal amongst much more evidence of the limited impact of OZs to improve economic well-being thus far.

[25] See Table B.9 for fuzzy regression discontinuity results without controls.

Electronic copy available at: https://ssrn.com/abstract=4086056

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

Another major federal initiative intended to spur economic activity in distressed areas is the NMTC, a component of the Community Renewal Tax Relief Act of 2000. Like the OZ provision, the NMTC focuses on census tracts that have relatively low incomes and high poverty rates, and offers tax incentives for investments made in these areas.[26] However, unlike the OZ provision, tax credits are limited each year. Also, all investments must be made through community development entities which are approved by the U.S. Treasury. Gurley-Calvez et al. (2009) find that the NMTC leads to additional investment that would not have otherwise been made. Using telephone and online survey data from key participants in NMTC investments, Abravanel et al. (2013) find that about 30-40 percent of investment projects would not have happened without NMTC funding. In terms of outcomes, Freedman (2012) finds modest impacts on increasing home values and reducing both poverty and unemployment, although some of the effect may be due to a changing population over time.

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

Electronic copy available at: https://ssrn.com/abstract=4086056

Harger and Ross (2016) find that employment in retail and manufacturing sectors increased in eligible areas, while employment in other sectors decreased. In sum, the results of previous place-based policies are mixed.

Unlike previous attempts at improving the economic health of distressed areas, OZ rules provide wide flexibility in the type of investment and do not cap the amount of investment that is subject to tax-preferred treatment. OZs thus provide unrestricted incentives for private investment that stands in contrast to the more highly vetted investments authorized in earlier programs that operated on a smaller scale. While the increased flexibility and uncapped funding of OZs could potentially increase the scale of effects, they could also lead to greater subsidies for investment projects that would have occurred in the absence of OZ tax incentives. Our results are consistent with this latter possibility.

A focus on differences across place-based policies can help inform the design features that are more likely to increase investment and improve downstream outcomes. Two distinguishing factors of the NMTC are that investments are capped and must be approved by authorities. These factors may be successful in spurring investment that would not have otherwise occurred. The much greater degree of flexibility for OZ investment may do less to spur marginal investment. As our study is not designed to identify the specific aspects of OZs that reduced its effectiveness, future research should attempt to shed further light on this question.

While on average we find small and statistically insignificant investment effects, it is important to emphasize that our results speak to the average effect of OZ eligibility and selection on investment and economic activity on the margin—it is possible that OZ designation had substantial positive effects in some subset of tracts but that these positive effects were outweighed by zero effects in a substantially larger number of other tracts. To the

Electronic copy available at: https://ssrn.com/abstract=4086056

extent that OZs were successful in driving investment to certain types of tracts, this would suggest that the broad eligibility criteria for OZs could be hindering overall success of the program.

One potential contributing factor to the overall lack of investment effects could be that persistent distress in many OZs makes them poor candidates for new investment regardless of tax incentives. Figure 11 breaks down census tracts into whether each is (a) persistently poor (poverty rate greater than 20 percent in both the 1980 Census and the 2011-2015 ACS); (b) newly poor (poverty rate less than 20 percent in the 1980 Census and greater than 20 percent in the 2011-15 ACS); (c) turned around (poverty rate greater than 20 percent in the 1980 Census and less than 20 percent in the 2011-15 ACS); and (d) never poor (poverty rate less than 20 percent in both the 1980 Census and the 2011-15 ACS). There are two key takeaways from this figure. First, over 45 percent of selected OZs are persistently poor, a higher fraction than eligible but not selected tracts (16 percent persistently poor). Thus, it is not simply that selected OZs are more economically distressed based upon the 2011-15 ACS (as we saw in Table 1) but they are more likely to be economically distressed for the decades leading up to the OZ legislation as well. Second, it is striking as to how few "turned around" tracts there are across all three groups. Less than 2.5 percent of census tracts that were poor in 1980 were no longer considered poor by 2015. This latter fact is somewhat discouraging given previous efforts with place-based policies and may provide insight into the fact that even providing for generous tax-preferred incentives for more dollars to flow into these communities may be insufficient to change the expected economic environment on a large scale that would lead investors to change their decisions.

The limited effect of OZs on investment through the end of 2020 suggests that strong downstream impacts on OZ residents may not be forthcoming. While our estimates extend

Electronic copy available at: https://ssrn.com/abstract=4086056

only through 2020, allowing only two and a half years since OZs were officially designated and just over two years since Treasury provided important guiding rules in October 2018, the tax benefits from OZ investment diminish over time. In particular, the financial incentives to invest are strongest when investment occurs prior to December 31, 2019. No effect on investment suggests that the natural channel through which employment and wages could rise is unlikely to materialize. This is exactly what Freedman et al. (2021) 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.

It is important to emphasize an important caveat for our analysis. It is not clear how the COVID-19 pandemic will affect OZ investment in the longer term, or if the pandemic could spur OZ policy changes such as extending the window for tax-favored investments in OZs. While we see no evidence of it thus far, it is possible that OZs would have a different impact in the post-COVID environment than in the pre-COVID environment. For these reasons, it will be important to continue to evaluate the impact of OZs on investment, economic development, home prices, and the well-being and labor market outcomes of OZ residents.

8 Conclusion

The persistence of economic disadvantage in some areas in the United States, combined with reduced geographic mobility, has led to renewed calls for policies that can improve the economic circumstances of residents in struggling regions. Our results suggest that at least in their first two and a half years of existence, there is no evidence that OZ tax incentives have significantly increased commercial investment in distressed areas. Similarly, business-related outcomes such as growth in new business formation, new business loans, commercial

Electronic copy available at: https://ssrn.com/abstract=4086056

diversity and consumer spending have not substantially changed in distressed areas as a result of the OZ policy. These findings are based on a multivariate regression discontinuity design that addresses potential biases of previous studies that rely on difference in differences approaches. Future research should continue to monitor the effects of OZs on investment and other outcomes such as employment, property values and other measures of economic activity in the years to come.

Electronic copy available at: https://ssrn.com/abstract=4086056

References

Abravanel, M. D., Pindus, N. M., Theodos, B., Bertumen, K., Brash, R., and McDade, Z. (2013). New markets tax credit (nmtc) program evaluation. *Urban Institute*.

Arefeva, A., Davis, M. A., Ghent, A. C., and Park, M. (2020). Who benefits from place-based policies? Job growth from opportunity zones. *Working paper*. Available at https://papers.ssrn.com/sol3/papers.cfm?abstract¨id=3645507.

Atkins, R. M. B., Hernandez-Lagos, P., Jara-Figueroa, C., and Seamans, R. (2020). What is the impact of opportunity zones on employment outcomes? *Working paper*. Available at https://papers.ssrn.com/sol3/papers.cfm?abstract¨id=3673986.

Berry, C. R. and Glaeser, E. (2005). The divergence of human capital levels across cities. *Papers in Regional Science*, 84(3):407–444.

Bondonio, D. and Engberg, J. (2000). Enterprise zones and local employment: Evidence from the states' programs. *Regional Science and Urban Economics*, 30(5):519–549.

Busso, M., Gregory, J., and Kline, P. (2013). Assessing the incidence and efficiency of a prominent placed based policy. *American Economic Review*, 103(2):897–947.

Calonico, S., Cattaneo, M., and Titiunik, R. (2014). Robust data-driven inference in the regression discontinuity design. *Stata Journal*, 14(4):909–946.

Casey, A. (2019). Sale prices surge in neighborhoods with new tax break. *Zillow Research*. Available at https://www.zillow.com/research/prices-surge-opportunity-zones-23393/.

Cattaneo, M., Jansson, M., and Ma, X. (2018). Manipulation testing based on density discontinuity. *The Stata Journal*, 18(1):234–261.

Electronic copy available at: https://ssrn.com/abstract=4086056

Chen, J., Glaeser, E. L., and Wessel, D. (2019). The (non-) effect of opportunity zones on housing prices. *NBER Working paper No. 26587*.

Chetty, R., Hendren, N., and Katz, L. F. (2016). The effects of exposure to better neighborhoods on children: New evidence from the moving to opportunity experiment. *American Economic Review*, 106(4):855–902.

Chyn, E. (2018). Moved to opportunity: The long-run effects of public housing demolition on children. *American Economic Review*, 108(10):3028–3056.

Council of Economic Advisers (2020). The impact of opportunity zones: An initial assessment. *White House*. Available at https://www.whitehouse.gov/wp-content/uploads/2020/08/The-Impact-of-Opportunity-Zones-An-Initial-Assessment.pdf.

Eldar, O. and Garber, C. (2020). Does government play favorites? Evidence from opportunity zones. *Duke Law School Public Law & Legal Theory Series*.

Frank, M. M., Hoopes, J. L., and Lester, R. (2020). What determines where opportunity knocks? Political affiliation in the selection of opportunity zones. *Working paper*. Available at https://papers.ssrn.com/sol3/papers.cfm?abstract˙id=3534451.

Freedman, M. (2012). Teaching new markets old tricks: The effects of subsidized investment on low-income neighborhoods. *Journal of Public Economics*, 96(11-12):1000–1014.

Freedman, M., Khanna, S., and Neumark, D. (2021). The impacts of opportunity zones on zone residents. *NBER Working Paper No. 28573*.

Ganong, P. and Shoag, D. (2017). Why has regional income convergence in the U.S. declined? *Journal of Urban Economics*, 102:76–90.

Electronic copy available at: https://ssrn.com/abstract=4086056

Gelman, A. and Imbens, G. (2019). Why high-order polynomials should not be used in regression discontinuity designs. *Journal of Business Economic Statistics*, 37(3):447–456.

Greenbaum, R. and Engberg, J. (2000). An evaluation of state enterprise zone policies. *Review of Policy Research*, 17(2-3):29–45.

Gurley-Calvez, T., Gilbert, T. J., Harper, K., Marples, D. J., and Daly, K. (2009). Do tax incentives affect investment? An analysis of the new markets tax credit. *Public Finance Review*, 37(4):371–398.

Ham, J. C., Swenson, C., Imrohoroglu, A., and Song, H. (2011). Government programs can improve local labor markets: Evidence from state enterprise zones, federal empowerment zones and federal enterprise community. *Journal of Public Economics*, 95(7-8):779–797.

Harger, K. and Ross, A. (2016). Do capital tax incentives attract new business? Evidence across industries from the new markets tax credit. *Journal of Regional Science*, 56(5):733–753.

Internal Revenue Service (2018). Bulletin no. 2018–28. Available at https://www.irs.gov/pub/irs-drop/n-18-48.pdf.

Kennedy, P. and Wheeler, H. (2022). Neighborhood-level investment from the U.S. opportunity zone program: Early evidence. *Working Paper, University of California Berkeley.*

Neumark, D. and Kolko, J. (2010). Do enterprise zones create jobs? Evidence from California's enterprise zone program. *Journal of Urban Economics*, 68:1–19.

Neumark, D. and Young, T. (2021). Heterogeneous effects of state enterprise zone programs in the shorter run and longer run. *Economic Development Quarterly*, pages 1–17.

Electronic copy available at: https://ssrn.com/abstract=4086056

O'Keefe, S. (2004). Job creation in California's enterprise zones: A comparison using a propensity score matching model. *Journal of Urban Economics*, 55(1):131–150.

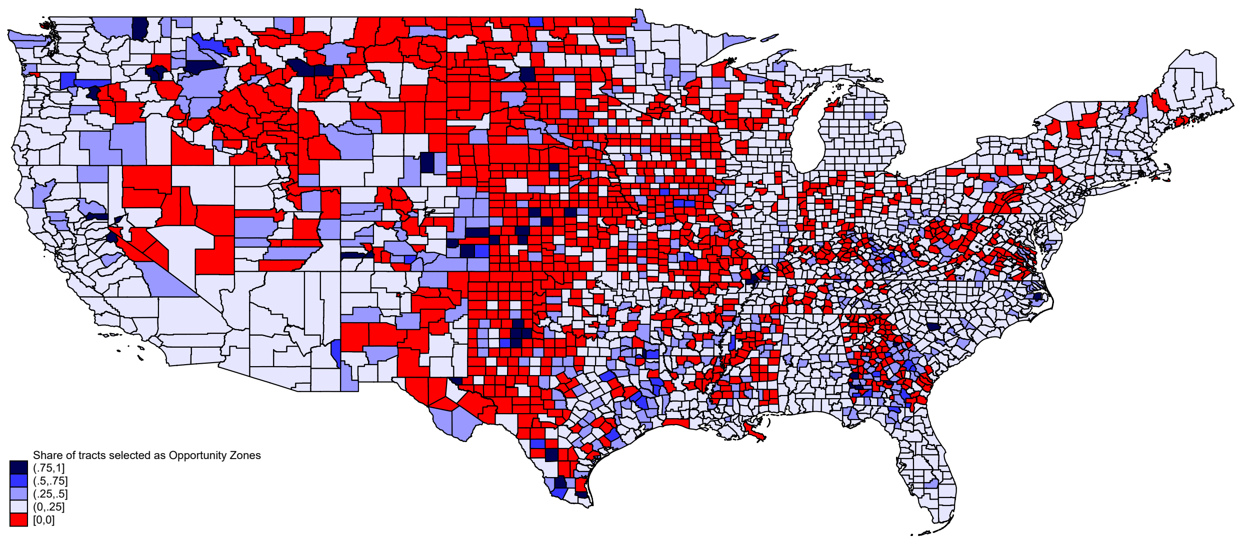
Reardon, S. F. and Robinson, J. P. (2012). Regression discontinuity designs with multiple rating-score variables. *Journal of Research on Educational Effectiveness*, 5(1):83–104.

Sage, A., Langen, M., and de Minne, A. V. (2019). Where is the opportunity in opportunity zones? Early indicators of the opportunity zone program's impact on commercial property prices. *Working paper*. Available at https://ssrn.com/abstract=3385502 or http://dx.doi.org/10.2139/ssrn.3385502.

Wong, V. C., Steiner, P. M., and Cook, T. D. (2013). Analyzing regression discontinuity designs with multiple assignment variables: A comparative study of four estimation methods. *Journal of Educational and Behavioral Statistics*, 38(2):107–141.

Electronic copy available at: https://ssrn.com/abstract=4086056

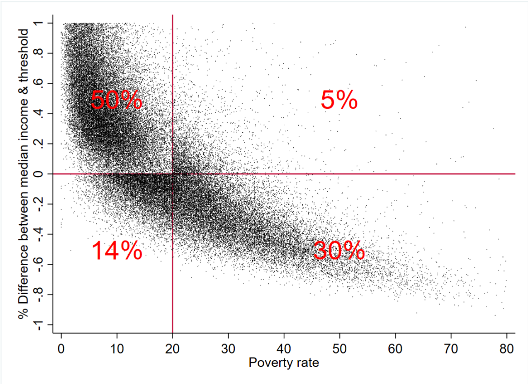
Figure 1: Share of census tracts designated as Opportunity Zones by county in the continental United States



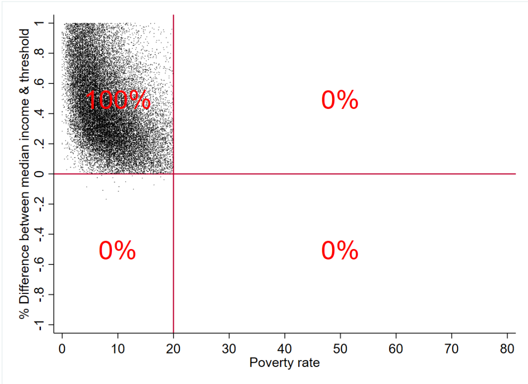
Source: U.S. Department of the Treasury; Authors' calculations

Electronic copy available at: https://ssrn.com/abstract=4086056

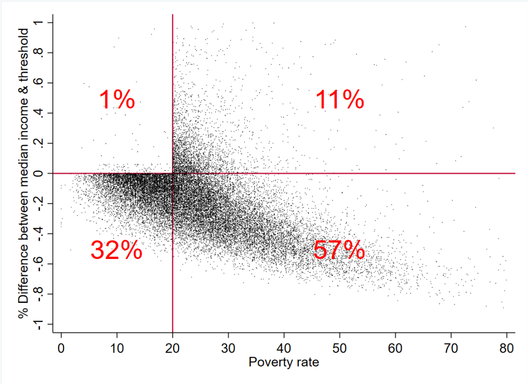
Figure 2: Poverty rate and percent difference between median income and threshold, by census tract type



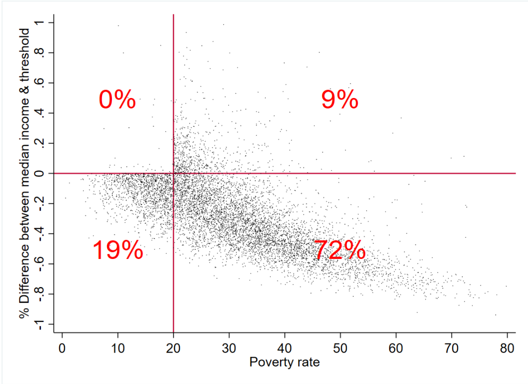
(a) All census tracts



(b) Ineligible census tracts



(c) Eligible but not selected census tracts

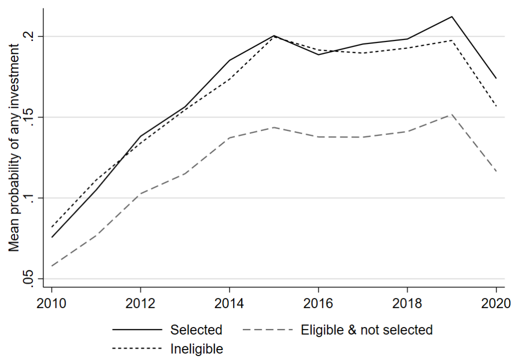


(d) Selected census tracts

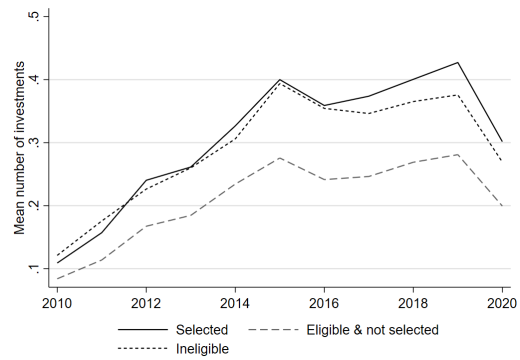
Sources: American Community Survey, 2011-2015 5-year pooled sample; U.S. Department of the Treasury.
Notes: Tracts eligible based only on contiguity with eligible tracts are excluded from the figures. Some eligible and selected tracts are found in the top-left quadrant of the figures because they may have been eligible based on the 2012-2016 ACS data. Total tracts = 61,410.

Electronic copy available at: https://ssrn.com/abstract=4086056

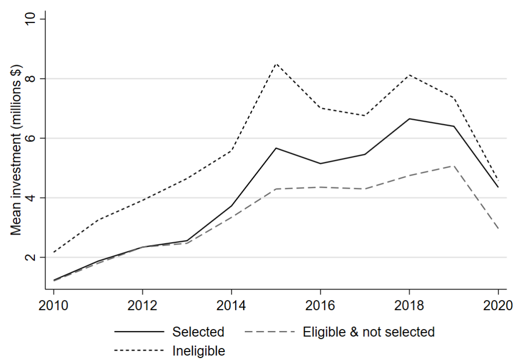
Figure 3: Investment outcomes by census tract type, 2010-2020



(a) Any investment



(b) Number of investments

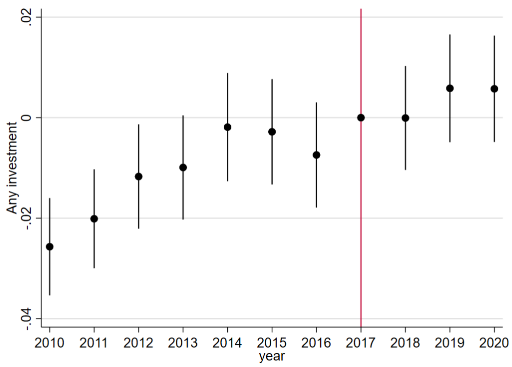


(c) Dollars of investment

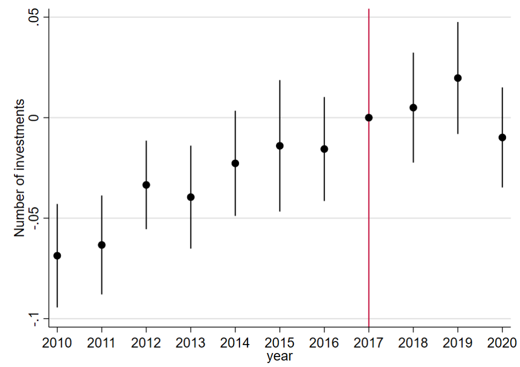
Sources: Real Capital Analytics; American Community Survey, 2011-2015 5-year pooled sample; U.S. Department of the Treasury.
Notes: Any investment is an indicator variable equal to 1 if at least one investment was recorded. Number of investments is number of investments made, and dollars of investment is the annual sum of investment amounts across all investments in millions. Dollars values are adjusted for inflation using the Personal Consumption Expenditures price index.

Electronic copy available at: https://ssrn.com/abstract=4086056

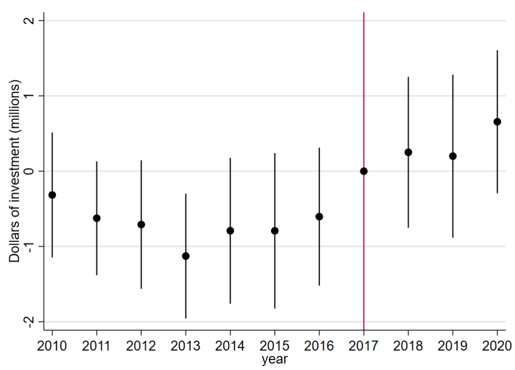
Figure 4: Difference in investment outcomes between census tracts selected as Opportunity Zones and census tracts eligible but not selected as Opportunity Zones (relative to difference in 2017), 2010-2020



(a) Any investment



(b) Number of investments

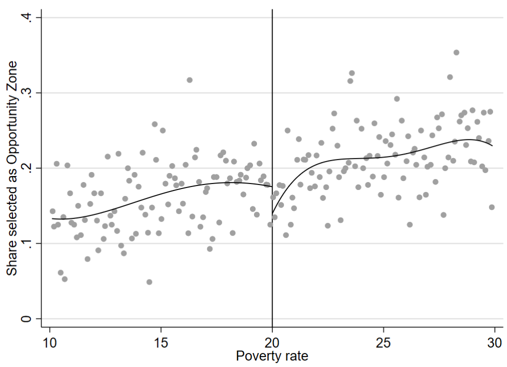


(c) Dollars of investment

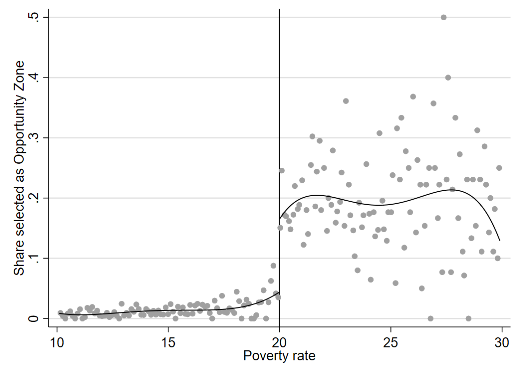
Sources: Real Capital Analytics; American Community Survey, 2011-2015 5-year pooled sample; U.S. Department of the Treasury.
Notes: Any investment is an indicator variable equal to 1 if at least one investment was recorded. Number of investments is number of investments made, and dollars of investment is the annual sum of investment amounts across all investments in millions. Dollars values are adjusted for inflation using the Personal Consumption Expenditures price index. Differences are estimated with a two-way fixed effects regression model, including census tract fixed effects and year fixed effects. Plotted differences are the coefficient estimates on interactions of year dummies and a dummy variable indicating whether the census tract was selected as an Opportunity Zone. Vertical lines represent 95 percent confidence intervals around the point estimates. The base year is 2017, the last year before Opportunity Zones were selected.

Electronic copy available at: https://ssrn.com/abstract=4086056

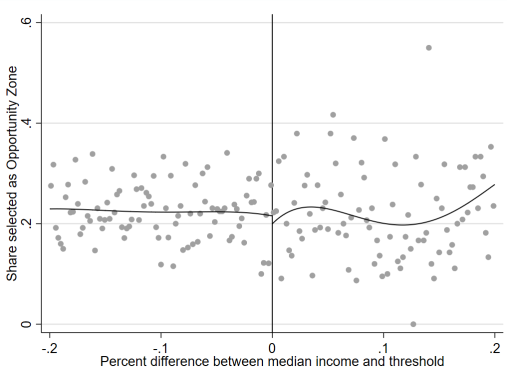
Figure 5: Share of tracts designated as Opportunity Zones by income and poverty thresholds



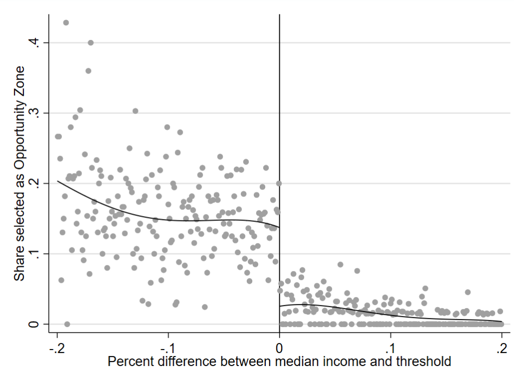
(a) Tracts with median income below 80% of MSA/state median income



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



(c) Tracts with poverty rate above 20%

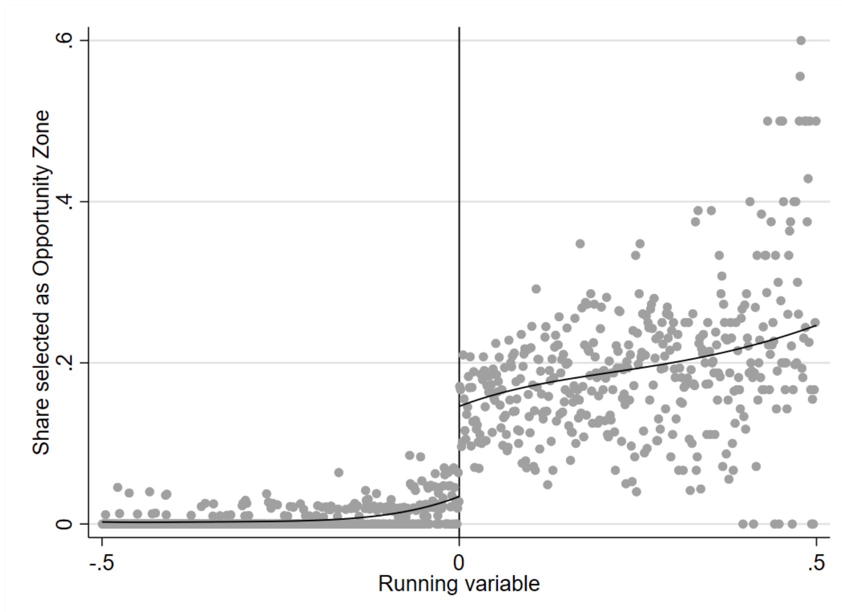


(d) Tracts with poverty rate below 20%

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

Electronic copy available at: https://ssrn.com/abstract=4086056

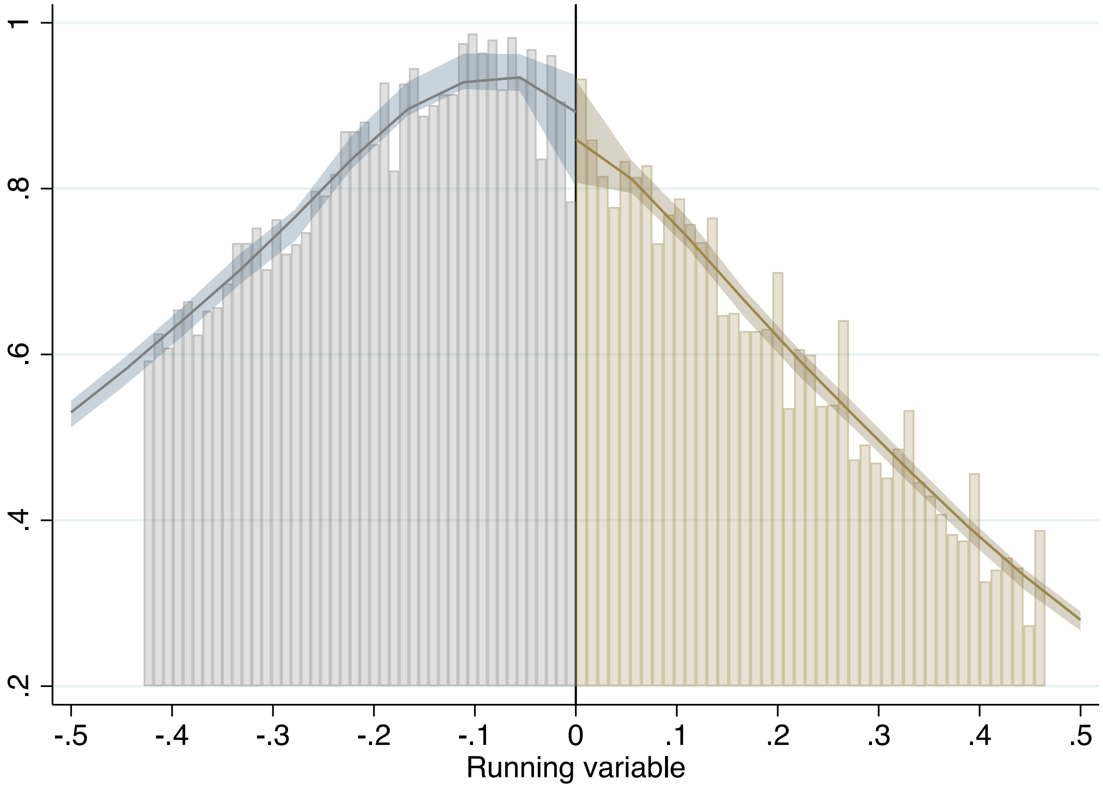
Figure 6: Share of tracts designated as Opportunity Zones by constructed running variable



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

Electronic copy available at: https://ssrn.com/abstract=4086056

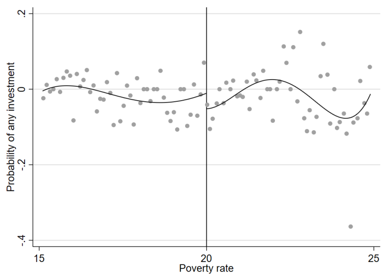
Figure 7: Histogram of combined running variable and manipulation test



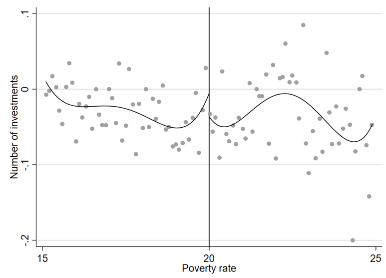
Source: American Community Survey 2011-2015 5-year pooled sample; U.S. Department of the Treasury; Authors' calculations

Electronic copy available at: https://ssrn.com/abstract=4086056

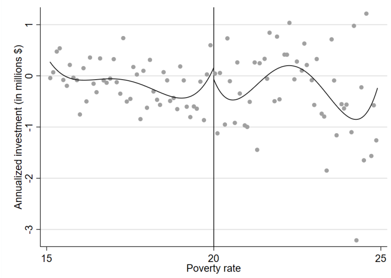
Figure 8: Investment outcomes by various running variables



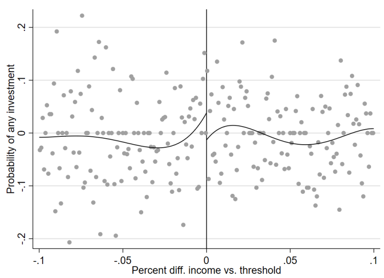
(a) Any investment



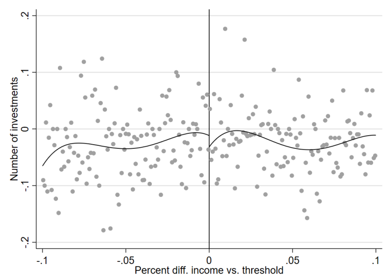
(b) Number of investments



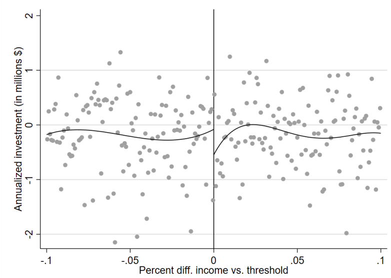
(c) Dollars (millions) of investment



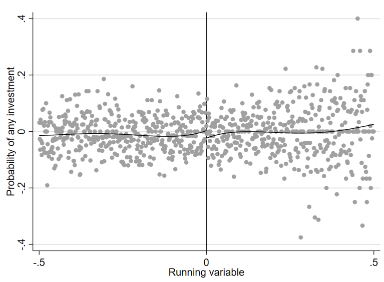
(d) Any investment



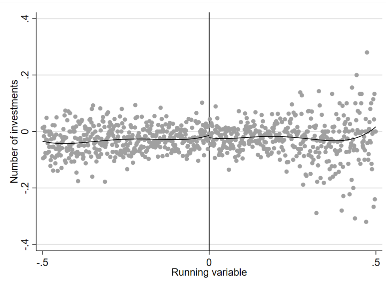
(e) Number of investments



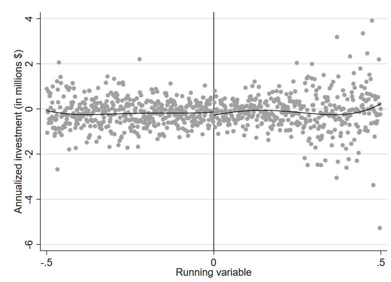
(f) Dollars (millions) of investment



(g) Any investment



(h) Number of investments

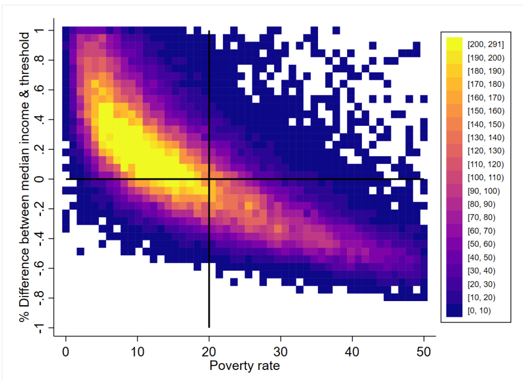


(i) Dollars (millions) of investment

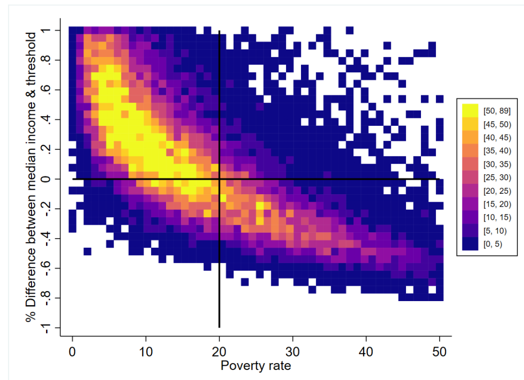
Sources: Real Capital Analytics; American Community Survey, 2011-2015 5-year pooled sample; U.S. Department of the Treasury.
Notes: Each dot represents the sample average within each bin. Fitted lines are based on a polynomial of degree 4 fitted separately to points on either side of the cutoff. Any investment is an indicator variable equal to 1 if at least one investment was recorded. Number of investments is number of investments made, and millions of dollars is the annualized sum of investment amounts across all investments. These variables are expressed as differences between the treatment period value (July 1, 2018 through December 31, 2020) and the control period value (July 1, 2015 through December 31, 2017). Dollars of investment are winsorized by replacing observations (before differencing) with values above the 95th percentile with the 95th percentile. See text for the definition of the constructed running variable. All dollars values are adjusted for inflation using the Personal Consumption Expenditures price index.

Electronic copy available at: https://ssrn.com/abstract=4086056

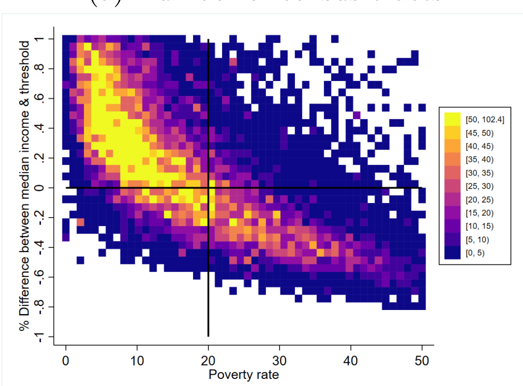
Figure 9: Investment outcomes by poverty rate and percent difference between median income and threshold, control period



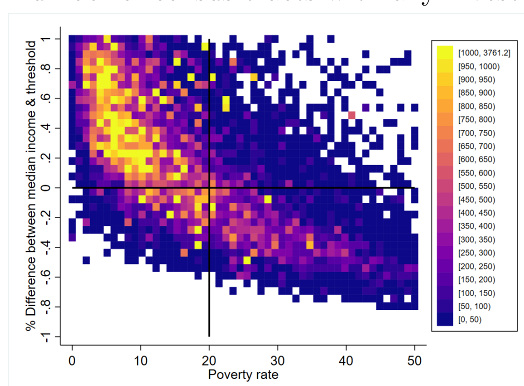
(a) Number of census tracts



(b) Number of census tracts with any investment



(c) Number of investments

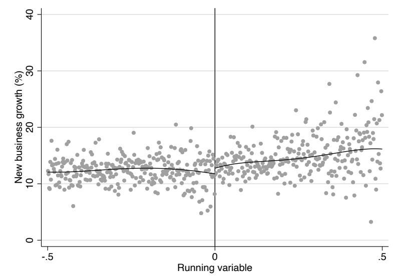


(d) Dollars of investment (millions)

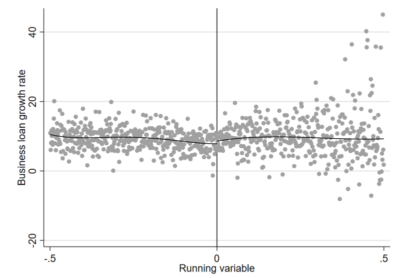
Sources: American Community Survey, 2011-2015 5-year pooled sample; U.S. Department of the Treasury; Real Capital Analytics.
Notes: Values in the figure legends correspond to all census tracts within a given rectangle of the grid. There are 200 rectangles in each figure, each spanning 1 percentage point for the poverty dimension and 5 percent for the median income dimension.

Electronic copy available at: https://ssrn.com/abstract=4086056

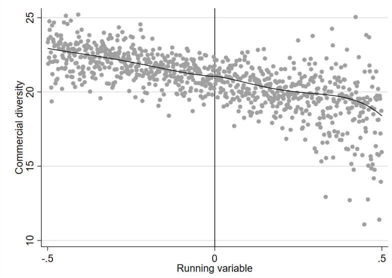
Figure 10: Mastercard business data by constructed running variable



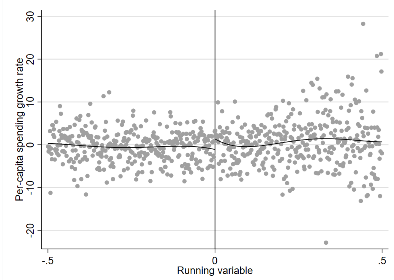
(a) New business growth 2019



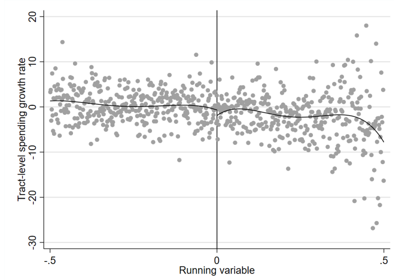
(b) Business loan growth 2019



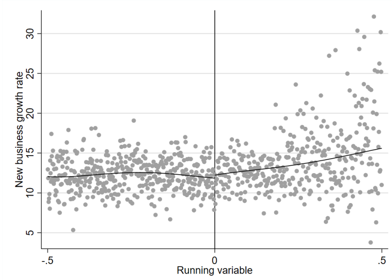
(c) Commercial diversity 2019



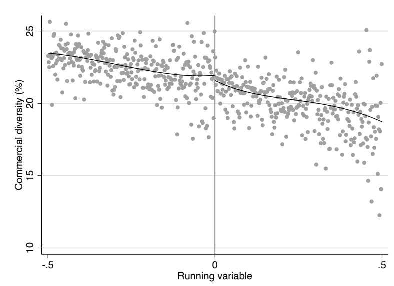
(d) Spending growth per capita (change in rank, 2017-2019)



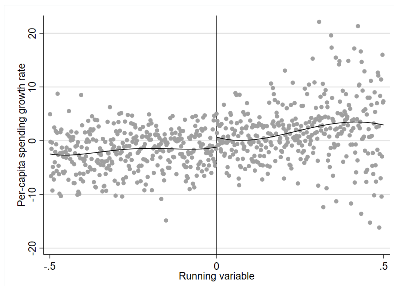
(e) Spending growth (change in rank, 2017-2019)



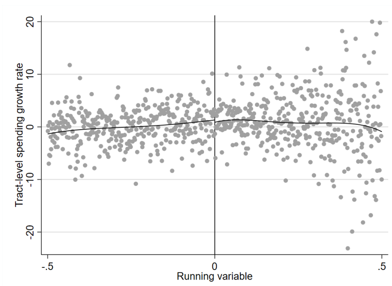
(f) Business loan growth 2020



(g) Commerical diversity 2020



(h) Spending growth per capita (change in rank, 2017-2020)

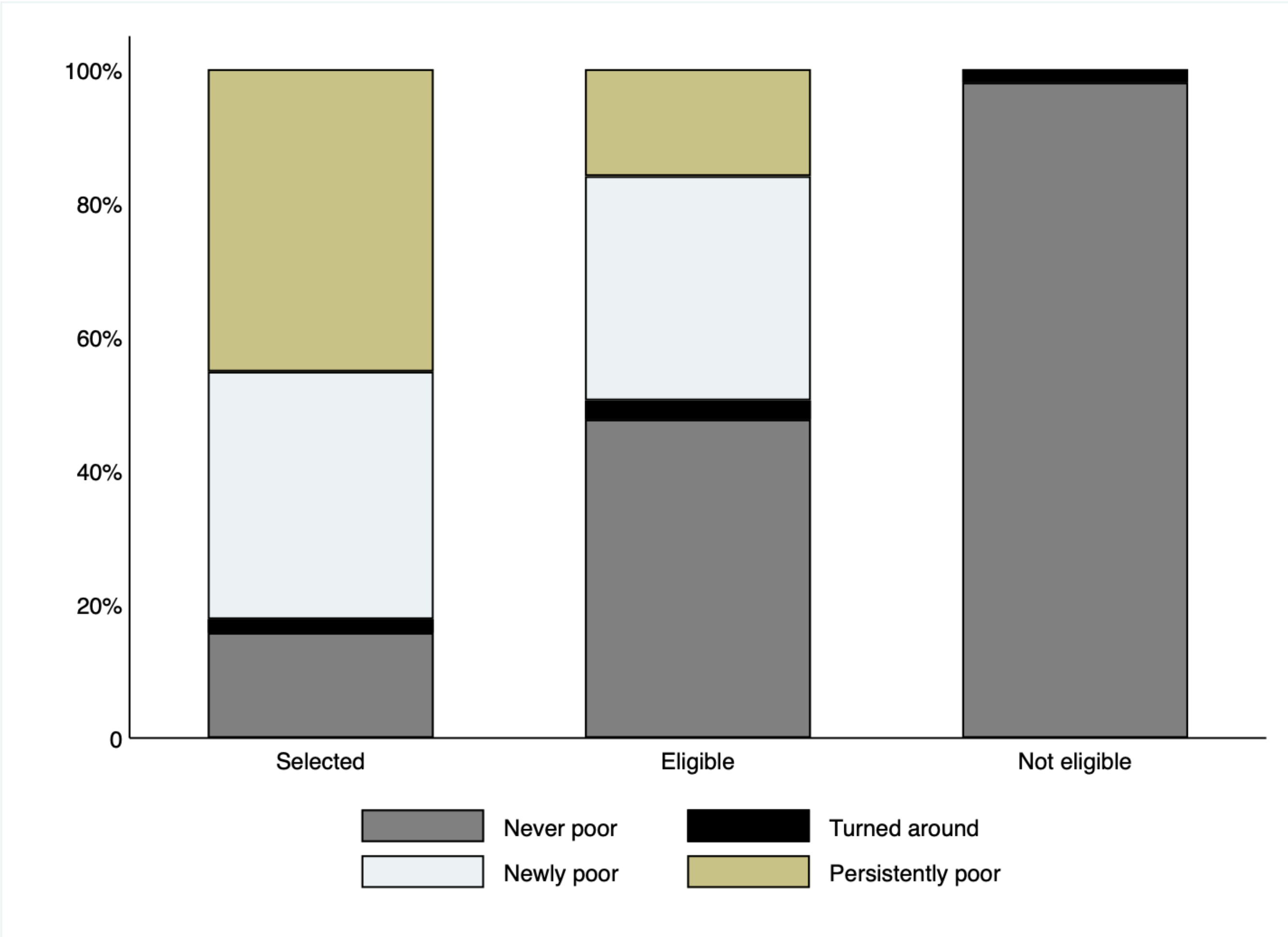


(i) Spending growth (change in rank, 2017-2020)

Sources: Mastercard Center for Inclusive Growth, 2017-2020
Notes: Each dot represents the sample average within each bin. Fitted lines are based on a polynomial of degree 4 fitted separately to points on either side of the cutoff. Left figure: Net growth rate includes both new businesses and closures.

Electronic copy available at: https://ssrn.com/abstract=4086056

Figure 11: Poverty 1980 vs. poverty 2011-15



Observations: 56,165. Poor = poverty rate $\geq$ 20 percent. Never poor: not poor in 1980 Census and 2011-15 ACS. Newly poor: not poor in 1980 Census and poor in 2011-15 ACS. Turned around: poor in 1980 Census and not poor in 2011-15 ACS. Persistently poor: poor in both 1980 Census and 2011-15 ACS. Includes only metropolitan area census tracts with a population of at least 500 (and less than 50 percent students).
Sources: Economic Innovation Group, Census Bureau, U.S. Department of the Treasury

Electronic copy available at: https://ssrn.com/abstract=4086056

Table 1: Summary statistics

	Eligible		Not Eligible
	Selected	Not Selected	
Tract characteristics (American Community Survey, 2013-2017)			
Median Household Income ($)	36,628	46,495	83,915
	(12,992)	(15,352)	(29,956)
Poverty Rate	28.6	20.2	7.4
	(12.8)	(11.1)	(4.5)
Unemployment Rate	6.3	4.9	3.2
	(3.6)	(2.9)	(1.6)
Labor Force Participation Rate	58.6	61.3	65.8
	(10.1)	(9.9)	(8.8)
Urban	0.89	0.90	0.97
Investment statistics (Real Capital Analytics, 2013-2017)			
At least one construction start (%)	7.8	5.1	8.4
Number of construction starts*	1.5	1.5	1.5
	(1.1)	(1.2)	(1.1)
At least one sale transaction (%)	42.1	33.0	40.8
Number of sale transactions*	3.6	3.2	3.6
	(6.1)	(6.0)	(5.8)
*Median Census Tract Level Price ($000)*†	8,767.6	8,896.0	11,555.7
	(17,830.6)	(17,880.7)	(25,998.3)
Median Census Tract Level Price/sq ft, $	192.8	219.5	251.5
	(185.3)	(214.8)	(247.4)
Property Type (%)			
Industrial	43.1	35.2	28.0
Office	25.8	25.8	33.4
Retail	31.1	39.0	38.6
Business and spending activity (Mastercard, various years)			
New business growth rate (2018)	8.9	12.2	15.6
	(18.5)	(21.3)	(20.8)
Business loan growth rate (2017)	10.1	9.8	6.3
	(29.9)	(30.6)	(22.0)
Commercial diversity (2019)	22.0	19.4	22.6
	(8.5)	(7.4)	(7.2)
Per-capita spending growth rate (rank, 2017)	46.9	47.8	53.3
	(22.3)	(22.7)	(22.1)
Tract-level spending growth rate (rank, 2017)	51.9	50.5	49.2
	(20.7)	(21.4)	(20.9)
Number of tracts	7,727	33,131	30,813

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

Electronic copy available at: https://ssrn.com/abstract=4086056

Table 2: First stage RD estimates: impact of cutoff on OZ selection

Running variable	**Statistic**	**Estimate**
Poverty	OZ eligible	0.155
	Standard error	(0.017)
	N	7,720
Income	OZ eligible	-0.111
	Standard error	(0.010)
	N	15,762
Combination	OZ eligible	0.118
	Standard error	(0.008)
	N	22,178

Sources: 2011-2015; 2013-2017 5-year pooled sample; U.S. Department of the Treasury.
Notes: For each running variable and presence of controls, we report the point estimate, standard error, and number of tracts within the optimally selected bandwidth. Poverty, income and combination running variables are defined in the text. Control variables include the labor force participation rate, employment to population ratio, the unemployment rate, and the share of workers employed in each industry, all based on the 2013-2017 American Community Survey. Standard errors (in parentheses) are estimated using the heteroskedasticity-robust nearest neighbor variance estimator.

Electronic copy available at: https://ssrn.com/abstract=4086056

Table 3: Impact of OZ eligibility on commercial investment, (ITT)

Running variable	**Statistic**	**Any Investment**	**Annualized Number of Investments**	**Annualized Dollars of Investment (Millions)**
Poverty	OZ eligible	0.017	0.009	0.156
	Standard error	(0.030)	(0.022)	(0.271)
	95% CI (lb)	-0.043	-0.036	-0.330
	95% CI (ub)	0.074	0.050	0.731
	Mean	0.282	0.213	2.095
	N	7,889	7,720	8,384
Income	OZ eligible	-0.016	0.0001	-0.1043
	Standard error	(0.023)	(0.016)	(0.182)
	95% CI (lb)	-0.059	-0.030	-0.457
	95% CI (ub)	0.029	0.033	0.256
	Mean	0.254	0.190	1.792
	N	11,065	10,347	13,348
Combination	OZ eligible	-0.011	-0.003	-0.072
	Standard error	(0.015)	(0.010)	(0.134)
	95% CI (lb)	-0.045	-0.025	-0.369
	95% CI (ub)	0.013	0.013	0.156
	Mean	0.262	0.197	1.889
	N	23,887	26,689	23,698

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. Table reports coefficient, standard error, lower and upper bounds of the 95 percent confidence interval, sample mean during the control period, and effective *N* for each running variable and outcome variable. Any investment is an indicator variable equal to 1 if at least one investment was recorded in the entire 2.5 year period from July 1, 2018 through December 31, 2020. Annualized number of investments is number of investments made from July 1, 2018 through December 31, 2020, divided by 2.5. Annualized dollars of investment is the total dollars of investment made from July 1, 2018 through December 31, 2020, divided by 2.5. These variables are expressed as differences between the treatment period value (July 1, 2018 through December 31, 2020) and the control period value (July 1, 2015 through December 31, 2017). Dollars of investment and number of investments in the control period and treatment period are each winsorized (before differencing) by replacing observations with values above the 95th percentile with the 95th percentile. See text for the definition of the combination running variable. 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 dollars 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.

Electronic copy available at: https://ssrn.com/abstract=4086056

Table 4: Investment gap between eligible and non-eligible census tracts

	Mean			**Upper bound**	**Max share**
	Eligible	**Ineligible**	**Gap**	**effect**	**of gap (%)**
Any investment (%)	0.267	0.329	0.062	0.013	20.7
Number of investments	0.202	0.267	0.065	0.013	20.5
Investment ($ millions)	1.944	2.856	0.913	0.156	17.0
N	40,841	30,830			

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

Notes: Means for eligible and ineligible census tracts are based on data from July 1, 2015 through December 31, 2017. Gap is the difference in means for eligible and ineligible tracts. Upper bound effects are take from Table 3. Maximum share of gap explained is the upper bound effect divided by the gap.

Electronic copy available at: https://ssrn.com/abstract=4086056

Table 5: Impact of OZ eligibility on commercial investment: heterogeneity and spillover analysis, (ITT)

Data subsample	**Statistic**	**Any Investment**	**Annualized Number of Investments**	**Annualized Dollars of Investment (Millions)**
Poverty range: 0% to 10%				
	OZ eligible	0.001	0.002	-0.265
	Standard error	(0.050)	(0.036)	(0.500)
	N	2,085	2,132	2,330
Poverty range: 10% to 20%				
	OZ eligible	-0.018	-0.001	-0.071
	Standard error	(0.025)	(0.018)	(0.198)
	N	8,914	8,237	9,818
Spillovers: Uniform counties				
	OZ eligible	0.018	0.012	0.126
	Standard error	(0.044)	(0.021)	(0.303)
	N	1,927	2,004	1,930

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. Table reports coefficient, standard error, and effective *N* for each bandwidth and outcome variable. Any investment is an indicator variable equal to 1 if at least one investment was recorded in the entire 2.5 year period from July 1, 2018 through December 31, 2020. Annualized number of investments is number of investments made from July 1, 2018 through December 31, 2020, divided by 2.5. Annualized dollars of investment is the total dollars of investment made from July 1, 2018 through December 31, 2020, divided by 2.5. These variables are expressed as differences between the treatment period value (July 1, 2018 through December 31, 2020) and the control period value (July 1, 2015 through December 31, 2017). Dollars of investment and number of investments in the control period and treatment period are each winsorized (before differencing) by replacing observations with values above the 95th percentile with the 95th percentile. The running variable for the poverty range specifications is the percent difference between the census tract median income and the threshold median income needed to qualify for OZ eligibility. The running variable for the spillovers analysis is the combination. 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 dollars 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.

Electronic copy available at: https://ssrn.com/abstract=4086056

Table 6: Impact of OZ selection on commercial investment, (LATE)

Running variable	Statistic	Any Investment	Annualized Number of Investments	Annualized Dollars of Investment (Millions)
Poverty	OZ selected	0.104	0.084	0.873
	Standard error	(0.191)	(0.123)	(1.550)
	95% CI (lb)	-0.256	-0.104	-1.866
	95% CI (ub)	0.494	0.379	4.211
	Mean	0.282	0.215	2.113
	N	7,720	10,461	10,461
Income	OZ selected	-0.149	-0.004	-0.926
	Standard error	(0.180)	(0.124)	(1.4844)
	95% CI (lb)	-0.498	-0.224	-3.908
	95% CI (ub)	0.209	0.262	1.910
	Mean	0.253	0.188	1.801
	N	14,534	14,434	15,793
Combination	OZ selected	-0.147	-0.041	-0.883
	Standard error	(0.140)	(0.095)	(1.211)
	95% CI (lb)	-0.450	-0.234	-3.452
	95% CI (ub)	0.097	0.137	1.295
	Mean	0.261	0.195	1.866
	N	18,903	19,724	19,856

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 fuzzy regression discontinuity design. Table reports coefficient, standard error, lower and upper bounds of the 95 percent confidence interval, sample mean during the control period, and effective *N* for each running variable and outcome variable. Any investment is an indicator variable equal to 1 if at least one investment was recorded in the entire 2.5 year period from July 1, 2018 through December 31, 2020. Annualized number of investments is number of investments made from July 1, 2018 through December 31, 2020, divided by 2.5. Annualized dollars of investment is the total dollars of investment made from July 1, 2018 through December 31, 2020, divided by 2.5. These variables are expressed as differences between the treatment period value (July 1, 2018 through December 31, 2020) and the control period value (July 1, 2015 through December 31, 2017). Dollars of investment and number of investments in the control period and treatment period are each winsorized (before differencing) by replacing observations with values above the 95th percentile with the 95th percentile. See text for the definition of the combination running variable. 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 dollars 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.

Electronic copy available at: https://ssrn.com/abstract=4086056

Table 7: Impact of OZ eligibility on economic activity, (ITT)

Running variable	**Statistic**	**New bus. growth**	**Bus. loan growth**	**Comm. div.**	**Spend growth per capita**	**Spend growth**
Poverty	OZ eligible	-0.125	0.942	0.041	0.837	-1.770
	Std. error	(0.943)	(1.479)	(0.436)	(1.545)	(1.603)
	95% CI (lb)	-2.061	-1.663	-0.774	-2.089	-5.194
	95% CI (ub)	1.637	4.133	0.935	3.966	1.091
	Mean	14.455	8.718	21.716	50.090	50.473
	N	8,557	8,205	8,917	10,200	9,874
Income	OZ eligible	0.240	0.479	0.140	1.906	-1.116
	Std. error	(0.715)	(1.217)	(0.343)	(1.490)	(1.439)
	95% CI (lb)	-0.999	-2.160	-0.4342	-0.5757	-4.0874
	95% CI (ub)	1.803	2.611	0.910	5.264	1.555
	Mean	13.804	8.793	20.843	49.622	49.866
	N	13,261	11,835	10,272	8,730	10,301
Comb.	OZ eligible	-0.088	1.284	0.067	0.893	-0.717
	Std. error	(0.441)	(0.680)	(0.211)	(0.851)	(0.874)
	95% CI (lb)	-0.914	-0.031	-0.341	-0.538	-2.377
	95% CI (ub)	0.814	2.633	0.484	2.799	1.047
	Mean	13.972	9.179	21.033	49.607	50.147
	N	32,957	34,716	28,583	24,816	26,342

Sources: Mastercard; 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. Table reports coefficient, standard error, lower and upper bounds of the 95 percent confidence interval, sample mean, and effective *N* for each running variable and outcome variable. New business growth, business loan growth and commercial diversity are values as of 2019. Spending growth per capita and spending growth are values as of 2019 minus values as of 2017. When poverty is the running variable, the sample is restricted to census tracts where the income condition is not satisfied. When income is used as the running variable, the sample is restricted to census tracts where the poverty condition is not satisfied. See text for the definition of the combination running variable. Control variables include the labor force participation rate, employment to population ratio, the unemployment rate, and the share of workers employed in each industry, all based on the 2013-2017 American Community Survey. All dollars values are adjusted for inflation using the Personal Consumption Expenditures price index. Standard errors (in parentheses) are estimated using the heteroskedasticity-robust nearest neighbor variance estimator.

Electronic copy available at: https://ssrn.com/abstract=4086056

Table 8: Impact of OZ selection on economic activity, (LATE)

Running variable	**Statistic**	**New bus. growth**	**Bus. loan growth**	**Comm. div.**	**Spend growth per capita**	**Spend growth**
Poverty	OZ selected	-0.961	6.091	0.257	4.586	-7.876
	Std. error	(5.038)	(9.505)	(2.720)	(8.442)	(8.096)
	95% CI (lb)	-10.127	-10.478	-5.005	-11.335	-24.972
	95% CI (ub)	9.620	26.780	5.655	21.757	6.763
	Mean	14.433	8.718	21.714	50.139	50.479
	N	12,783	8,205	8,731	10,570	12,528
Income	OZ selected	0.342	5.551	0.017	11.561	-9.563
	Std. error	(5.898)	(10.166)	(2.571)	(10.441)	(10.513)
	95% CI (lb)	-10.680	-17.057	-4.807	-4.620	-32.538
	95% CI (ub)	12.439	22.793	5.273	36.308	8.673
	Mean	13.854	8.798	20.864	49.744	49.967
	N	15,831	14,757	15,465	13,519	14,170
Comb.	OZ selected	-0.535	9.829	0.270	12.058	-7.823
	Std. error	(4.123)	(6.441)	(1.724)	(7.758)	(8.172)
	95% CI (lb)	-8.488	-2.549	-3.033	-0.927	-24.719
	95% CI (ub)	7.675	22.699	3.724	29.482	7.315
	Mean	13.988	8.97	21.020	49.588	50.222
	N	26,018	24,427	25,220	17,860	17,465

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

Notes: All results are estimated using a fuzzy regression discontinuity design. Table reports coefficient, standard error, lower and upper bounds of the 95 percent confidence interval, sample mean, and effective N for each running variable and outcome variable. New business growth, business loan growth and commercial diversity are values as of 2019. Spending growth per capita and spending growth are values as of 2019 minus values as of 2017. When poverty is the running variable, the sample is restricted to census tracts where the income condition is not satisfied. When income is used as the running variable, the sample is restricted to census tracts where the poverty condition is not satisfied. See text for the definition of the combination running variable. Control variables include the labor force participation rate, employment to population ratio, the unemployment rate, and the share of workers employed in each industry, all based on the 2013-2017 American Community Survey. All dollars values are adjusted for inflation using the Personal Consumption Expenditures price index. Standard errors (in parentheses) are estimated using the heteroskedasticity-robust nearest neighbor variance estimator.

Electronic copy available at: https://ssrn.com/abstract=4086056

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.[27]

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 5.

Appendix B: Tables and figures

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

Electronic copy available at: https://ssrn.com/abstract=4086056

Table B.1: Mastercard variable description

Variable	Description	Source	Years available
New business growth	percentage growth of net new businesses based upon anonymized and aggregated location data $(year_t - year_{t-1})/year_{t-1}$	Mastercard Places	2018-2020
Small business loans	percentage growth of the number of small business loans $(year_t - year_{t-1})/year_{t-1}$	FFIEC	2017-2019
Commercial diversity	percentage of industries represented	Mastercard POI provider	2019-2020
Spend growth	percentage growth of spending based upon anonymized and aggregated transaction data. Within state $rank_t - rank_{t-1}$ (100 = top rank)	Mastercard GeoInsights	2017-2020
Spending per capita	percentage growth of average spending per account based upon anonymized and aggregated transaction data. Total spending within census tract (by residents and nonresidents) divided by number of residents. Within state $rank_t - rank_{t-1}$ (100 = top rank)	Mastercard GeoInsights	2017-2020

Sources: Mastercard's Center for Inclusive Growth

Electronic copy available at: https://ssrn.com/abstract=4086056

Table B.2: Impact of OZ eligibility on commercial investment, various running variables: no controls

Running variable	**Statistic**	**Any Investment**	**Annualized Number of Investments**	**Annualized Dollars of Investment (Millions)**
Poverty	Coefficient	0.016	0.0084	0.1388
	Standard error	(0.0297)	(0.0221)	(0.2705)
	95% CI (lower bound)	-0.0422	-0.0366	-0.3481
	95% CI (upper bound)	0.074	0.0501	0.7122
	Mean	0.2821	0.2132	2.0949
	N	8,049	7,720	8,384
Income	Coefficient	-0.0152	0.0006	-0.1109
	Standard error	(0.0229)	(0.0162)	(0.1854)
	95% CI (lower bound)	-0.0583	-0.0291	-0.468
	95% CI (upper bound)	0.0312	0.0342	0.2588
	Mean	0.2535	0.1893	1.7964
	N	10,866	10,162	12,926
Combination	Coefficient	-0.012	-0.0038	-0.0796
	Standard error	(0.0147)	(0.0097)	(0.134)
	95% CI (lower bound)	-0.0452	-0.0252	-0.3756
	95% CI (upper bound)	0.0124	0.0128	0.1498
	Mean	0.2625	0.1983	1.8891
	N	23,880	27,404	23,793

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. Table reports coefficient, standard error, lower and upper bounds of the 95 percent confidence interval, sample mean during the control period, and effective N for each running variable and outcome variable. Any investment is an indicator variable equal to 1 if at least one investment was recorded in the entire 2.5 year period from July 1, 2018 through December 31, 2020. Annualized number of investments is number of investments made from July 1, 2018 through December 31, 2020, divided by 2.5. Annualized dollars of investment is the total dollars of investment made from July 1, 2018 through December 31, 2020, divided by 2.5. These variables are expressed as differences between the treatment period value (July 1, 2018 through December 31, 2020) and the control period value (July 1, 2015 through December 31, 2017). Dollars of investment and number of investments in the control period and treatment period are each winsorized (before differencing) by replacing observations with values above the 95th percentile with the 95th percentile. See text for the definition of the combination running variable. All specifications include no control variables. All dollars 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.

Electronic copy available at: https://ssrn.com/abstract=4086056

Table B.3: Impact of OZ eligibility on commercial investment: various bandwidths, combination running variable

Bandwidth	**Any investment**	**Number of investments**	**Millions of dollars**
0.05	-0.016	-0.002	0.055
	(0.039)	(0.026)	(0.340)
N	6,163	6,163	6,163
0.10	-0.021	-0.005	-0.077
	(0.026)	(0.018)	(0.233)
N	12,419	12,419	12,419
0.25	-0.007	-0.003	-0.058
	(0.017)	(0.012)	(0.149)
N	29,098	29,098	29,098
0.50	0.002	0.001	0.026
	(0.012)	(0.009)	(0.120)
N	48,189	48,189	48,189

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. Table reports coefficient, standard error, and effective sample size for each bandwidth and outcome variable. Any investment is an indicator variable equal to 1 if at least one investment was recorded in the entire 2.5 year period from July 1, 2018 through December 31, 2020. Annualized number of investments is number of investments made from July 1, 2018 through December 31, 2020, divided by 2.5. Annualized dollars of investment is the total dollars of investment made from July 1, 2018 through December 31, 2020, divided by 2.5. These variables are expressed as differences between the treatment period value (July 1, 2018 through December 31, 2020) and the control period value (July 1, 2015 through December 31, 2017). Dollars of investment and number of investments in the control period and treatment period are each winsorized (before differencing) by replacing observations with values above the 95th percentile with the 95th percentile. The combination running variable is used for all specifications (see text for details). 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 dollars 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.

Electronic copy available at: https://ssrn.com/abstract=4086056

Table B.4: Impact of OZ eligibility on commercial investment: various polynomials, combination running variable

Order of polynomial	Any investment	Number of investments	Millions of dollars
1	-0.011	-0.003	-0.072
	(0.015)	(0.009)	(0.134)
N	23,887	26,689	23,698
2	-0.013	-0.006	-0.099
	(0.015)	(0.010)	(0.159)
N	42,669	42,695	32,257
3	-0.021	-0.003	-0.123
	(0.019)	(0.014)	(0.163)
N	42,073	39,777	44,979
4	-0.021	-0.003	-0.079
	(0.022)	(0.016)	(0.204)
N	46,545	42,237	42,891

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. Table reports coefficient, standard error, and effective sample size for each bandwidth and outcome variable. Any investment is an indicator variable equal to 1 if at least one investment was recorded in the entire 2.5 year period from July 1, 2018 through December 31, 2020. Annualized number of investments is number of investments made from July 1, 2018 through December 31, 2020, divided by 2.5. Annualized dollars of investment is the total dollars of investment made from July 1, 2018 through December 31, 2020, divided by 2.5. These variables are expressed as differences between the treatment period value (July 1, 2018 through December 31, 2020) and the control period value (July 1, 2015 through December 31, 2017). Dollars of investment and number of investments in the control period and treatment period are each winsorized (before differencing) by replacing observations with values above the 95th percentile with the 95th percentile. The combination running variable is used for all specifications (see text for details). 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 dollars 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.

Electronic copy available at: https://ssrn.com/abstract=4086056

Table B.5: Impact of OZ eligibility on commercial investment: various data subsamples, combination running variable

Outcome or sample	**Any investment**	**Number of investments**	**Millions of dollars**
Urban only	-0.007	-0.002	-0.055
	(0.016)	(0.011)	(0.150)
Rural only	-0.038	-0.030	-0.258
	(0.033)	(0.017)	(0.152)
Industrial Only	-0.010	-0.004	-0.030
	(0.008)	(0.003)	(0.027)
Office Only	-0.002	-0.001	0.026
	(0.009)	(0.004)	(0.029)
Retail Only	-0.001	-0.002	-0.039
	(0.011)	(0.005)	(0.043)
Levels: 2017	0.010	0.011	0.116
	(0.010)	(0.014)	(0.140)
Levels: 2019	0.005	0.010	0.094
	(0.010)	(0.014)	(0.125)
Levels: 2020	0.013	0.013	0.097
	(0.010)	(0.010)	(0.082)
Levels: Treatment period (2019-2020)	0.012	0.014	0.194
	(0.012)	(0.010)	(0.133)
Difference: 2020 vs. 2019	0.009	0.004	-0.008
	(0.012)	(0.014)	(0.125)

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. Table reports coefficient and standard error for each outcome or sample. Except when the outcome variable is expressed as levels in a given year or the difference between 2020 and 2019: Any investment is an indicator variable equal to 1 if at least one investment was recorded in the entire 2.5 year period from July 1, 2018 through December 31, 2020. Annualized number of investments is number of investments made from July 1, 2018 through December 31, 2020, divided by 2.5. Annualized dollars of investment is the total dollars of investment made from July 1, 2018 through December 31, 2020, divided by 2.5. These variables are expressed as differences between the treatment period value (July 1, 2018 through December 31, 2020) and the control period value (July 1, 2015 through December 31, 2017). Dollars of investment and number of investments in the control period and treatment period are each winsorized (before differencing) by replacing observations with values above the 95th percentile with the 95th percentile. The combination running variable is used for all specifications (see text for details). 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 dollars 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.

Electronic copy available at: https://ssrn.com/abstract=4086056

Table B.6: Summary statistics of census tracts by various intervals (lower and upper bound) of combination running variable

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

Sources: American Community Survey 2011-2015; 2013-2017 5-year pooled sample; U.S. Department of the Treasury.
Notes: Table shows the median household income, mean poverty rate, mean unemployment rate, mean labor force participation rate, and share of tracts that are urban, among census tracts grouped by their value of the combination running variable. The lower bound and upper bound of each running variable interval are shown in the top two rows of the table.

Electronic copy available at: https://ssrn.com/abstract=4086056

Table B.7: Impact of OZ selection on commercial investment, various running variables: no controls

Running variable	Statistic	Any Investment	Annualized Number of Investments	Annualized Dollars of Investment (Millions)
Poverty	Coefficient	0.106	0.0738	0.8104
	Standard error	(0.1873)	(0.13)	(1.5839)
	95% CI (lower bound)	-0.2442	-0.1416	-1.9449
	95% CI (upper bound)	0.4902	0.368	4.2638
	Mean	0.2825	0.2153	2.0952
	N	8,206	8,917	10,071
Income	Coefficient	-0.1501	-0.0034	-0.9789
	Standard error	(0.1782)	(0.1248)	(1.4889)
	95% CI (lower bound)	-0.4902	-0.2234	-3.9169
	95% CI (upper bound)	0.2083	0.2658	1.9194
	Mean	0.2532	0.1889	1.8026
	N	14,270	13,528	16,341
Combination	Coefficient	-0.1251	-0.0345	-0.6663
	Standard error	(0.1339)	(0.0916)	(1.1462)
	95% CI (lower bound)	-0.4254	-0.2189	-3.1723
	95% CI (upper bound)	0.0994	0.14	1.3206
	Mean	0.2622	0.1962	1.8884
	N	21,731	22,632	23,878

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 fuzzy regression discontinuity design. Table reports coefficient, standard error, lower and upper bounds of the 95 percent confidence interval, sample mean during the control period, and effective sample size for each running variable and outcome variable. Any investment is an indicator variable equal to 1 if at least one investment was recorded in the entire 2.5 year period from July 1, 2018 through December 31, 2020. Annualized number of investments is number of investments made from July 1, 2018 through December 31, 2020, divided by 2.5. Annualized dollars of investment is the total dollars of investment made from July 1, 2018 through December 31, 2020, divided by 2.5. These variables are expressed as differences between the treatment period value (July 1, 2018 through December 31, 2020) and the control period value (July 1, 2015 through December 31, 2017). Dollars of investment and number of investments in the control period and treatment period are each winsorized (before differencing) by replacing observations with values above the 95th percentile with the 95th percentile. See text for the definition of the combination running variable. All specifications include no control variables. All dollars 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.

Electronic copy available at: https://ssrn.com/abstract=4086056

Table B.8: Impact of OZ eligibility on economic activity, various running variables: no controls

Running variable	**Statistic**	**New bus. growth**	**Bus. loan growth**	**Comm. div.**	**Spend growth per capita**	**Spend growth**
Poverty	Coefficient	-0.2073	0.7513	0.1046	0.9413	-1.3708
	Std. error	(0.9536)	(1.492)	(0.4439)	(1.4843)	(1.52)
	95% CI (lb)	-2.1603	-1.8969	-0.7617	-1.7926	-4.5675
	95% CI (ub)	1.5776	3.9516	0.9784	4.0258	1.3907
	Mean	14.4553	8.7177	21.7122	-0.2455	0.1496
	N	8,557	8,205	9,672	11,336	11,737
Income	Coefficient	0.3038	0.422	0.2763	1.9639	-0.9687
	Std. error	(0.7289)	(1.2501)	(0.365)	(1.5053)	(1.4373)
	95% CI (lb)	-0.9464	-2.3207	-0.3357	-0.5471	-3.8859
	95% CI (ub)	1.9107	2.5794	1.0952	5.3537	1.7481
	Mean	13.7575	8.6465	20.8298	-0.6233	-0.4813
	N	12,604	11,289	9,590	8,576	10,376
Comb.	Coefficient	-0.0676	1.2847	0.1035	0.9011	-0.6309
	Std. error	(0.4515)	(0.6958)	(0.2121)	(0.8452)	(0.8849)
	95% CI (lb)	-0.9371	-0.0983	-0.2907	-0.5224	-2.349
	95% CI (ub)	0.8328	2.6291	0.5407	2.7908	1.1196
	Mean	13.9721	9.1816	21.0465	-0.2221	-0.5045
	N	32,004	33,593	29,444	25,167	25,821

Sources: Mastercard; 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. Table reports coefficient, standard error, lower and upper bounds of the 95 percent confidence interval, sample mean, and effective sample size for each running variable and outcome variable. New business growth, business loan growth and commercial diversity are values as of 2019. Spending growth per capita and spending growth are values as of 2019 minus values as of 2017. When poverty is the running variable, the sample is restricted to census tracts where the income condition is not satisfied. When income is used as the running variable, the sample is restricted to census tracts where the poverty condition is not satisfied. See text for the definition of the combination running variable. No control variables are included. All dollars 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.

Electronic copy available at: https://ssrn.com/abstract=4086056

Table B.9: Impact of OZ selection on economic activity, various running variables: no controls

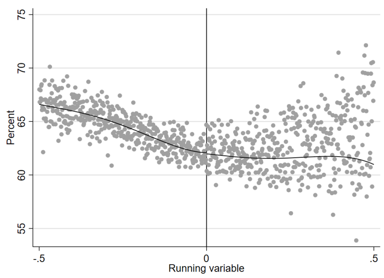
Running variable	**Statistic**	**New bus. growth**	**Bus. loan growth**	**Comm. div.**	**Spend growth per capita**	**Spend growth**
Poverty	Coefficient	-0.1251	0.9424	0.0409	0.8374	-1.7703
	Std. error	(0.9433)	(1.4786)	(0.436)	(1.5446)	(1.6033)
	95% CI (lb)	-2.0609	-1.6627	-0.7744	-2.0887	-5.194
	95% CI (ub)	1.6368	4.1333	0.9349	3.9659	1.0907
	Mean	14.4553	8.7177	21.7161	-0.049	0.0764
	N	8,557	8,205	8,917	10,200	9,874
Income	Coefficient	0.2403	0.479	0.1404	1.9055	-1.1159
	Std. error	(0.7148)	(1.217)	(0.3429)	(1.4898)	(1.4393)
	95% CI (lb)	-0.999	-2.1595	-0.4342	-0.5757	-4.0874
	95% CI (ub)	1.8029	2.6109	0.9099	5.2644	1.5546
	Mean	13.8036	8.7933	20.8426	-0.5911	-0.5182
	N	13,261	11,835	10,272	8,730	10,301
Comb.	Coefficient	-0.0884	1.2837	0.0669	0.8929	-0.7167
	Std. error	(0.4409)	(0.6795)	(0.2105)	(0.8513)	(0.8735)
	95% CI (lb)	-0.9141	-0.031	-0.341	-0.5376	-2.3768
	95% CI (ub)	0.814	2.6325	0.4843	2.7994	1.0472
	Mean	13.9716	9.1785	21.0334	-0.2282	-0.5155
	N	32,957	34,716	28,583	24,816	26,342

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

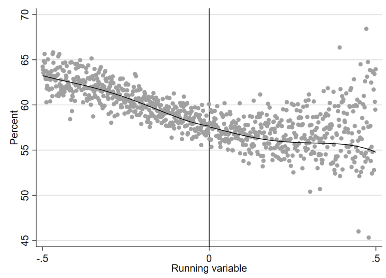
Notes: All results are estimated using a fuzzy regression discontinuity design. Table reports coefficient, standard error, lower and upper bounds of the 95 percent confidence interval, sample mean, and effective sample size for each running variable and outcome variable. New business growth, business loan growth and commercial diversity are values as of 2019. Spending growth per capita and spending growth are values as of 2019 minus values as of 2017. When poverty is the running variable, the sample is restricted to census tracts where the income condition is not satisfied. When income is used as the running variable, the sample is restricted to census tracts where the poverty condition is not satisfied. See text for the definition of the combination running variable. No control variables are included. Standard errors (in parentheses) are estimated using the heteroskedasticity-robust nearest neighbor variance estimator.

Electronic copy available at: https://ssrn.com/abstract=4086056

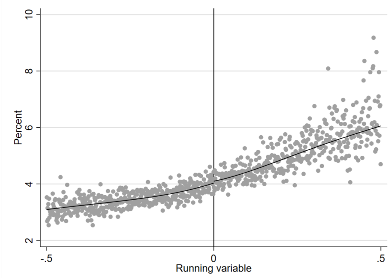
Figure B.1: Control variables by constructed running variable



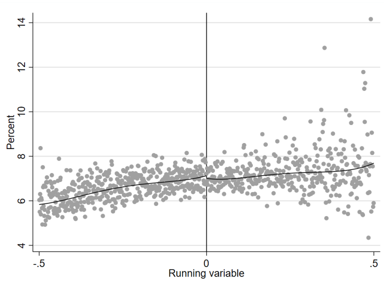
(a) Labor force participation rate



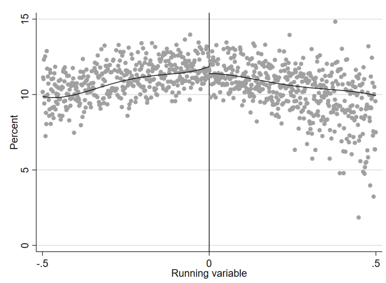
(b) Employment to population ratio



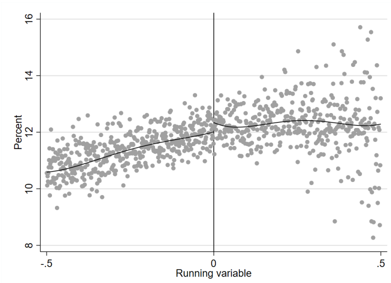
(c) Unemployment rate



(d) Share of workers in construction



(e) Share of workers in manufacturing

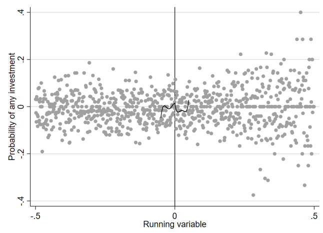


(f) Share of workers in retail

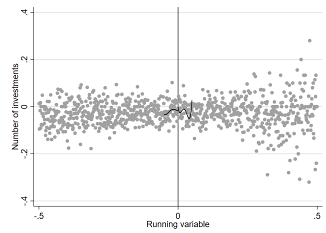
Sources: Real Capital Analytics; American Community Survey, 2011-2015 5-year pooled sample; U.S. Department of the Treasury.
Notes: Each dot represents the sample average within each bin. Fitted lines are based on a polynomial of degree 4 fitted separately to points on either side of the cutoff.

Electronic copy available at: https://ssrn.com/abstract=4086056

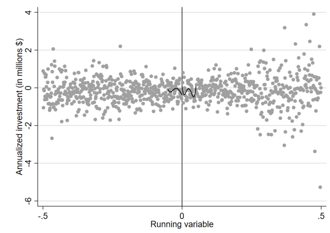
Figure B.2: Investment outcomes by combined running variable, various bandwidths



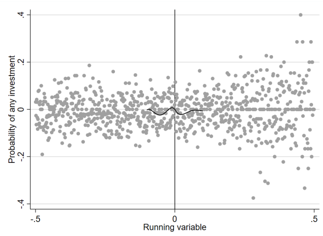
(a) Any investment: Bandwidth 0.05



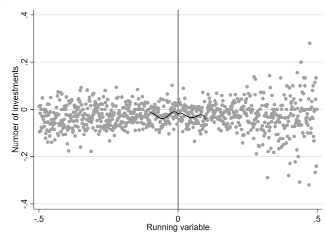
(b) Number of investments: Bandwidth 0.05



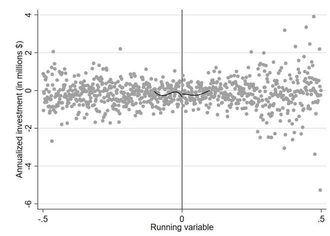
(c) Dollars (millions) of investment: Bandwidth 0.05



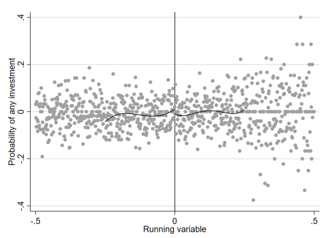
(d) Any investment: Bandwidth 0.10



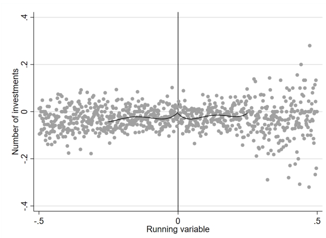
(e) Number of investments: Bandwidth 0.10



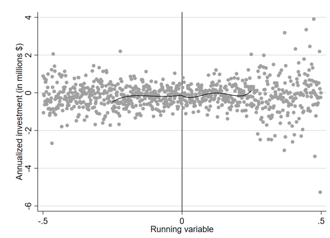
(f) Dollars (millions) of investment: Bandwidth 0.10



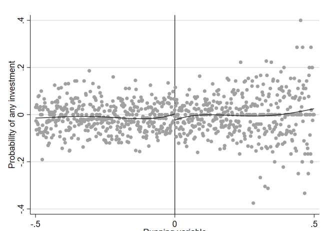
(g) Any investment: Bandwidth 0.25



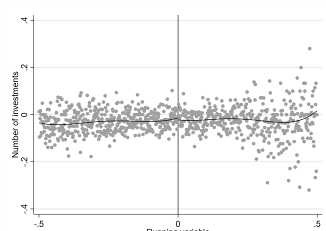
(h) Number of investments: Bandwidth 0.25



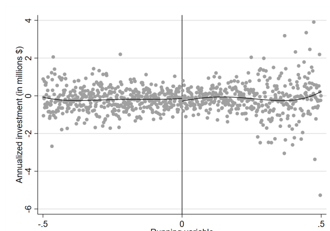
(i) Dollars (millions) of investment: Bandwidth 0.25



(j) Any investment: Bandwidth 0.50



(k) Number of investments: Bandwidth 0.50

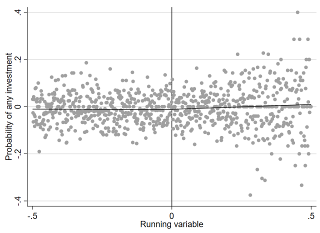


(l) Dollars (millions) of investment: Bandwidth 0.50

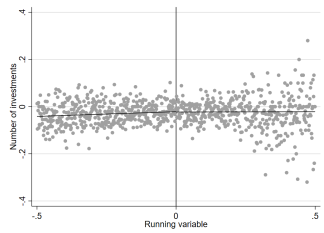
Sources: Real Capital Analytics; American Community Survey, 2011-2015 5-year pooled sample; U.S. Department of the Treasury.
Notes: Each dot represents the sample average within each bin. Fitted lines are based on a polynomial of degree 4 fitted separately to points on either side of the cutoff. See Figure 6 for variable definitions.

Electronic copy available at: https://ssrn.com/abstract=4086056

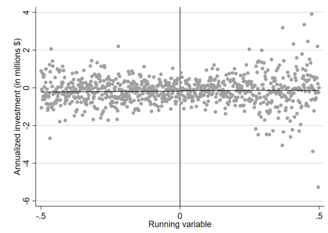
Figure B.3: Investment outcomes by constructed running variable, various polynomials



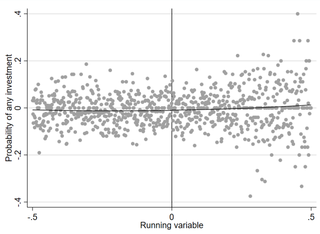
(a) Any investment: Polynomial Order 1



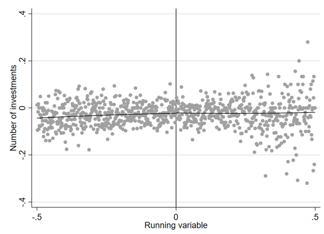
(b) Number of investments: Polynomial Order 1



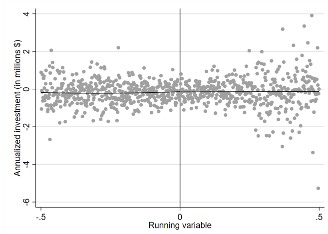
(c) Dollars (millions) of investment: Polynomial Order 1



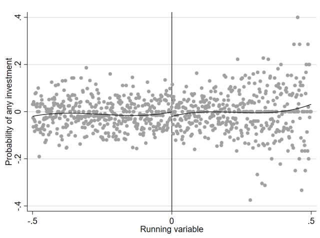
(d) Any investment: Polynomial Order 2



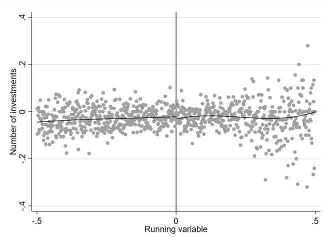
(e) Number of investments: Polynomial Order 2



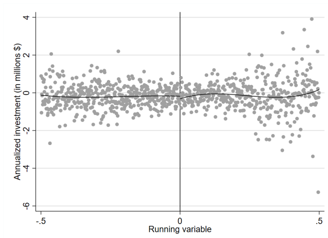
(f) Dollars (millions) of investment: Polynomial Order 2



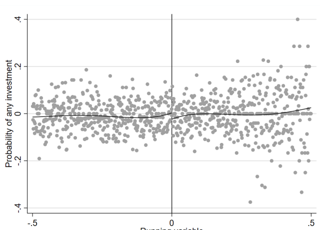
(g) Any investment: Polynomial Order 3



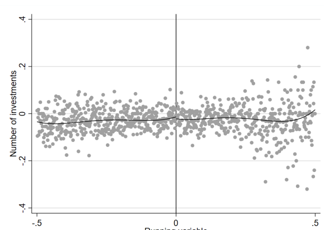
(h) Number of investments: Polynomial Order 3



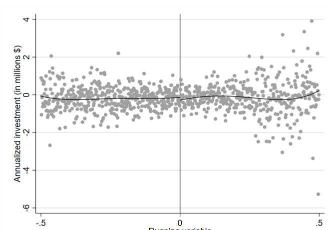
(i) Dollars (millions) of investment: Polynomial Order 3



(j) Any investment: Polynomial Order 4



(k) Number of investments: Polynomial Order 4

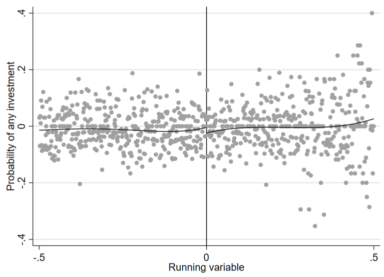


(l) Dollars (millions) of investment: Polynomial Order 4

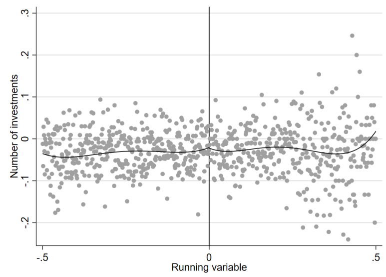
Sources: Real Capital Analytics; American Community Survey, 2011-2015 5-year pooled sample; U.S. Department of the Treasury.
Notes: Each dot represents the sample average within each bin. Fitted lines are based on a polynomial of various degrees fitted separately to points on either side of the cutoff.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

Electronic copy available at: https://ssrn.com/abstract=4086056

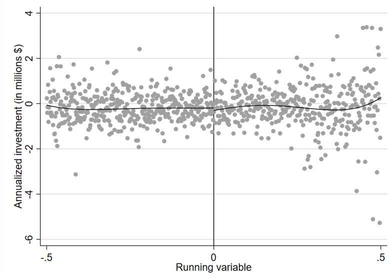
Figure B.4: Investment outcomes by constructed running variable, urban and rural



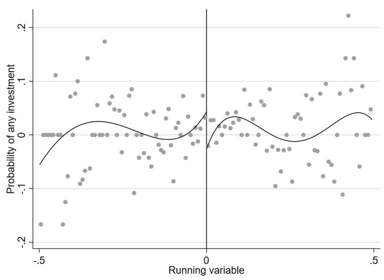
(a) Urban: Any investment



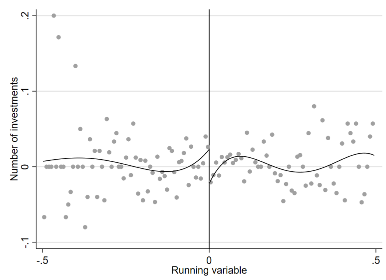
(b) Urban: Number of investments



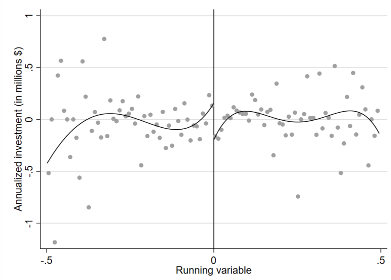
(c) Urban: Dollars (millions) of investment



(d) Rural: Any investment



(e) Rural: Number of investments



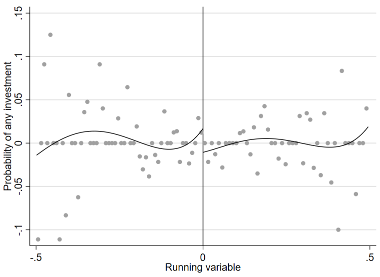
(f) Rural: Dollars (millions) of investment

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

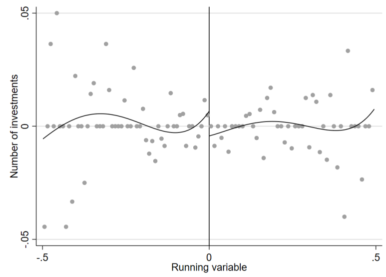
Notes: Each dot represents the sample average within each bin. Fitted lines are based on a polynomial of degree 4 fitted separately to points on either side of the cutoff. Any investment is an indicator variable equal to 1 if at least one investment was recorded. Number of investments is number of investments made, and millions of dollars is the the annualized sum of investment amounts across all investments. These variables are expressed as differences between the treatment period value (July 1, 2018 through December 31, 2020) and the control period value (July 1, 2015 through December 31, 2017). Dollars and number of investments are winsorized by replacing observations (before differencing) with values above the 95th percentile with the 95th percentile. See text for the definition of the constructed running variable. All dollars values are adjusted for inflation using the Personal Consumption Expenditures price index.

Electronic copy available at: https://ssrn.com/abstract=4086056

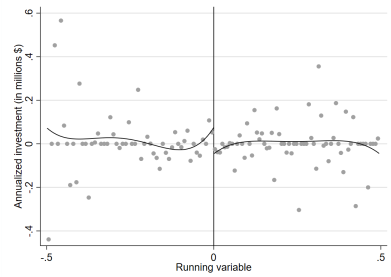
Figure B.5: Investment outcomes by constructed running variable, by sector



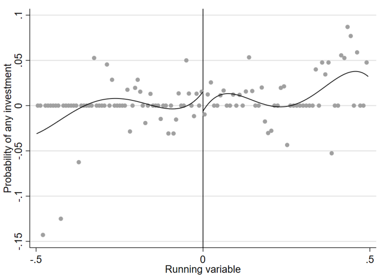
(a) Industrial: Any investment



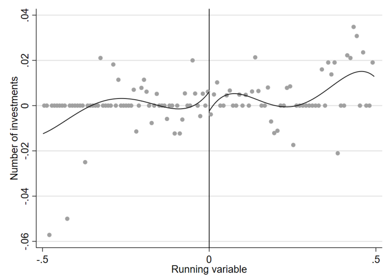
(b) Industrial: Number of investments



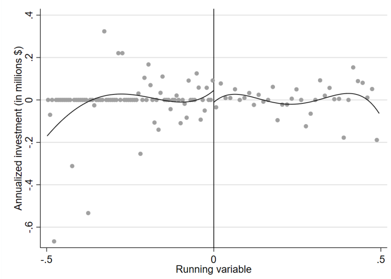
(c) Industrial: Dollars (millions) of investment



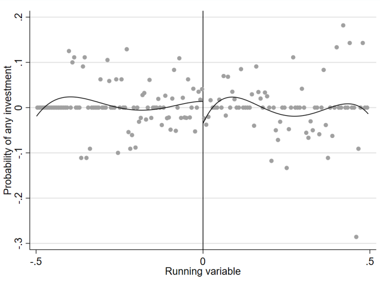
(d) Office: Any investment



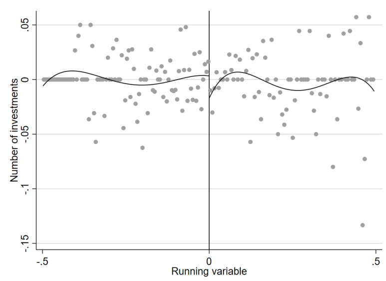
(e) Office: Number of investments



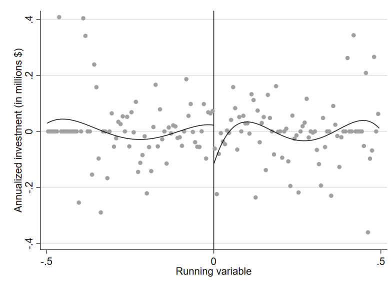
(f) Office: Dollars (millions) of investment



(g) Retail: Any investment



(h) Retail: Number of investments

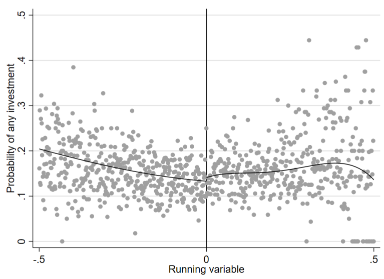


(i) Retail: Dollars (millions) of investment

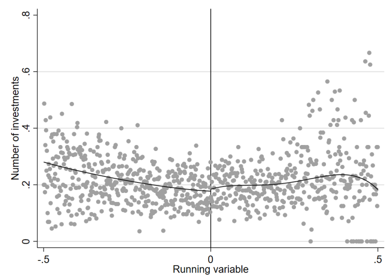
Sources: Real Capital Analytics; American Community Survey, 2011-2015 5-year pooled sample; U.S. Department of the Treasury.
Notes: Each dot represents the sample average within each bin. Fitted lines are based on a polynomial of degree 4 fitted separately to points on either side of the cutoff. Any investment is an indicator variable equal to 1 if at least one investment was recorded. Number of investments is number of investments made, and millions of dollars is the the annualized sum of investment amounts across all investments. These variables are expressed as differences between the treatment period value (July 1, 2018 through December 31, 2020) and the control period value (July 1, 2015 through December 31, 2017). Dollars and number of investments are winsorized by replacing observations (before differencing with values above the 95th percentile with the 95th percentile. All dollars values are adjusted for inflation using the Personal Consumption Expenditures price index.

Electronic copy available at: https://ssrn.com/abstract=4086056

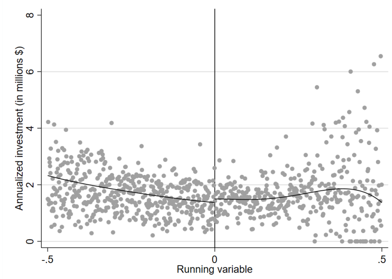
Figure B.6: Investment outcomes by constructed running variable, by year



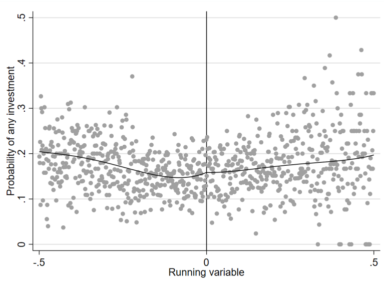
(a) 2017: Any investment



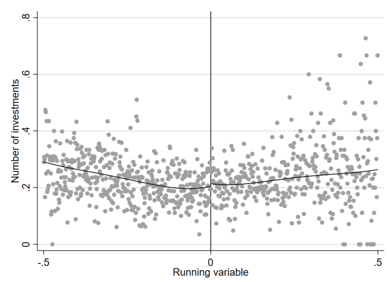
(b) 2017: Number of investments



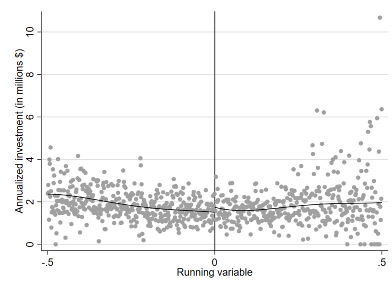
(c) 2017: Dollars (millions) of investment



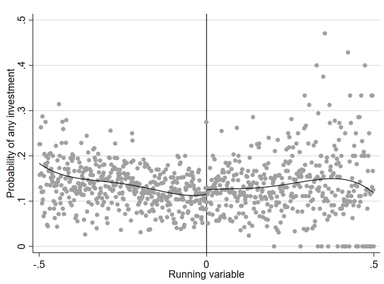
(d) 2019: Any investment



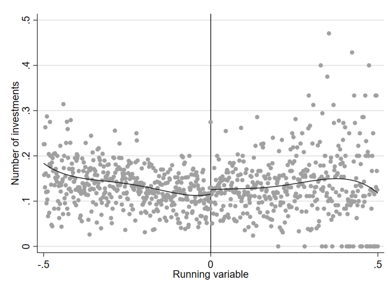
(e) 2019: Number of investments



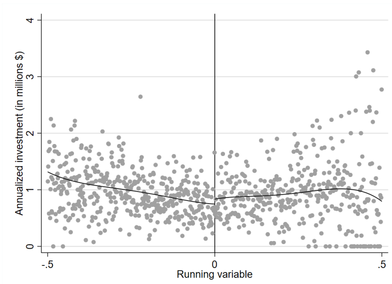
(f) 2019: Dollars (millions) of investment



(g) 2020: Any investment



(h) 2020: Number of investments

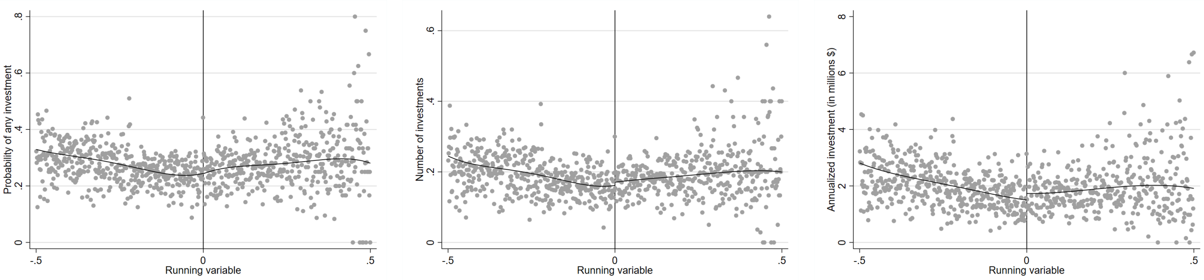


(i) 2020: Dollars (millions) of investment

Sources: Real Capital Analytics; American Community Survey, 2011-2015 5-year pooled sample; U.S. Department of the Treasury.
Notes: Each dot represents the sample average within each bin. Fitted lines are based on a polynomial of degree 4 fitted separately to points on either side of the cutoff. Any investment is an indicator variable equal to 1 if at least one investment was recorded. Number of investments is number of investments made, and millions of dollars is the the annualized sum of investment amounts across all investments. These variables are expressed as levels in the year indicated. Dollars of investment are winsorized by replacing observations with values above the 95th percentile with the 95th percentile. See text for the definition of the constructed running variable. All dollars values are adjusted for inflation using the Personal Consumption Expenditures price index.

Electronic copy available at: https://ssrn.com/abstract=4086056

Figure B.7: Investment outcomes by constructed running variable, outcomes expressed in levels during treatment period



(a) Any investment

(b) Number of investments

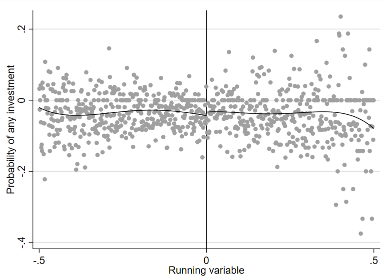
(c) Dollars (millions) of investment

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

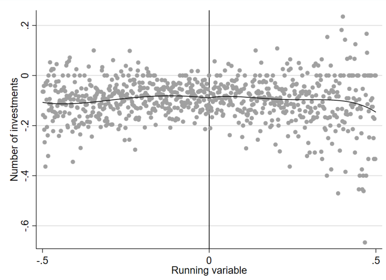
Notes: Each dot represents the sample average within each bin. Fitted lines are based on a polynomial of degree 4 fitted separately to points on either side of the cutoff. Any investment is an indicator variable equal to 1 if at least one investment was recorded. Number of investments is number of investments made, and millions of dollars is the the annualized sum of investment amounts across all investments. These variables are expressed in levels during the treatment period (July 1, 2018 through December 31, 2020). Dollars of investment are winsorized by replacing observations with values above the 95th percentile with the 95th percentile. See text for the definition of the constructed running variable. All dollars values are adjusted for inflation using the Personal Consumption Expenditures price index.

Electronic copy available at: https://ssrn.com/abstract=4086056

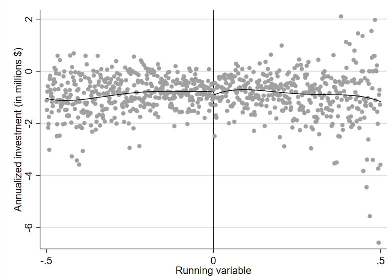
Figure B.8: Investment outcomes by constructed running variable, growth in 2020 relative to 2019



(a) Any investment



(b) Number of investments

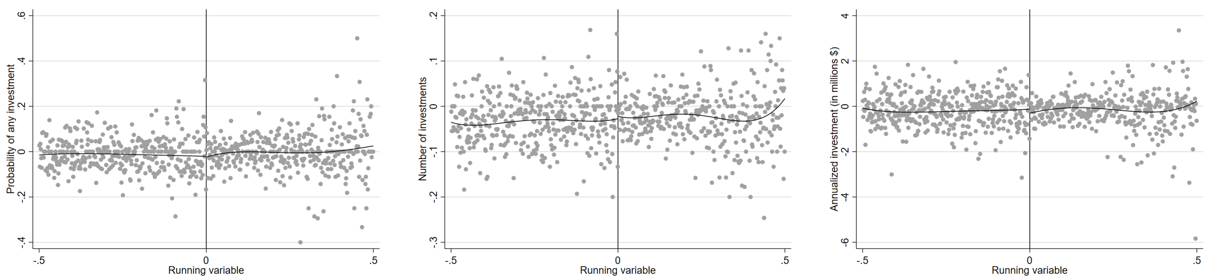


(c) Dollars (millions) of investment

Sources: Real Capital Analytics; American Community Survey, 2011-2015 5-year pooled sample; U.S. Department of the Treasury.
Notes: Each dot represents the sample average within each bin. Fitted lines are based on a polynomial of degree 4 fitted separately to points on either side of the cutoff. Any investment is an indicator variable equal to 1 if at least one investment was recorded. Number of investments is number of investments made, and millions of dollars is the the annualized sum of investment amounts across all investments. These variables are expressed as differences between 2020 and 2019. Dollars of investment are winsorized by replacing observations with values above the 95th percentile with the 95th percentile. See text for the definition of the constructed running variable. All dollars values are adjusted for inflation using the Personal Consumption Expenditures price index.

Electronic copy available at: https://ssrn.com/abstract=4086056

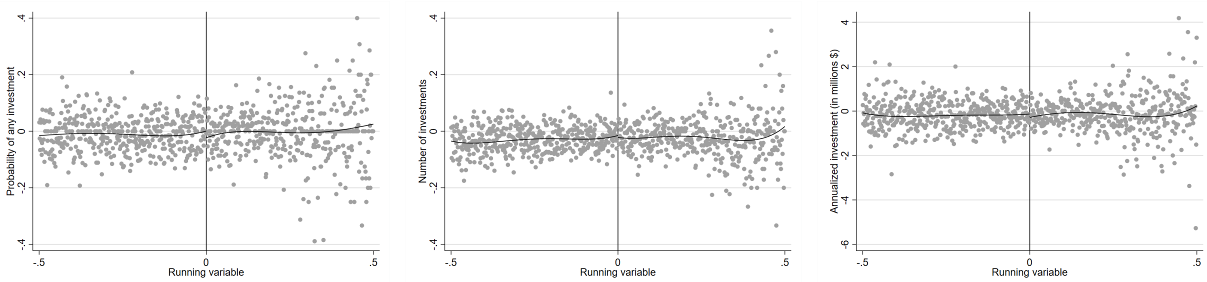
Figure B.9: Investment outcomes by constructed running variable, sample restrictions of contiguous tracts



(a) Drop contiguous: Any investment

(b) Drop contiguous: Number of investments

(c) Drop contiguous: Dollars (millions) of investment



(d) Drop selected contiguous: Any investment

(e) Drop selected contiguous: Number of investments

(f) Drop selected contiguous: Dollars (millions) of investment

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

Notes: Each dot represents the sample average within each bin. Fitted lines are based on a polynomial of degree 4 fitted separately to points on either side of the cutoff. Any investment is an indicator variable equal to 1 if at least one investment was recorded. Number of investments is number of investments made, and millions of dollars is the the annualized sum of investment amounts across all investments. These variables are expressed as differences between the treatment period value (July 1, 2018 through December 31, 2020) and the control period value (July 1, 2015 through December 31, 2017). Dollars and number of investments are winsorized by replacing observations (before differencing) with values above the 95th percentile with the 95th percentile. See text for the definition of the constructed running variable. All dollars values are adjusted for inflation using the Personal Consumption Expenditures price index.

Electronic copy available at: https://ssrn.com/abstract=4086056