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

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This draft: August 1, 2022

Abstract

A provision of the Tax Cuts and Jobs Act of 2017 offered tax incentives for investing in certain low-income areas in the United States called Opportunity Zones (OZs). The goal of this provision was to spur private investment in OZs in order to improve the economic well-being of their residents. This paper uses a regression discontinuity design to evaluate the impact of OZs on commercial investment and economic activity. Using data on the universe of all significant commercial investments in the United States, we find that OZ selection led to practically no 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 21 percentage points per OZ, an increase in the number of investments of more than 0.31 per OZ, and an increase in investment of more than $2.3 million per OZ. These findings are supported by additional data from Mastercard that also show no evidence of increased business activity nor consumer spending. Overall, our findings suggest that the impact of OZs on economic improvement has thus far been limited.

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

*We thank Matt Freedman and numerous conference and seminar participants 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.

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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 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

Recent studies have also evaluated the impact of OZs on tract-level outcomes, primarily focused on real estate prices and employment. All of these studies use a difference in differences approach that compares changes in outcomes in OZs to eligible but not selected OZs, though differ somewhat in the final comparison control group.[2] 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 and using a more sophisticated design to control for the types of properties sold, Chen et al. (2019) estimate little effect on home prices through the end of 2018 using repeat sales data from the Federal Housing Finance Agency. Council of Economic Advisers (2020) extends the Chen et al. (2019) data through the end of 2019 and finds a modest effect of OZs on home prices.

A number of studies have focused on the impact of OZs on employment and poverty outcomes. 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.

[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.

Our paper contributes to the existing OZ literature by using a multidimensional regression discontinuity approach that relies on weaker assumptions than difference in differences approaches, and by using rich data on investment and other economic activity—outcomes that are likely necessary for potential downstream impacts on resident well-being. Our regression discontinuity approach is validated by a sharp increase in a census tract's probability of OZ selection when crossing the cutoff of our constructed running variable, based upon its poverty rate exceeding 20 percent or its median income falling below 80 percent of the area median. This approach overcomes the need to make unverifiable assumptions under a difference in differences design that OZs and non-OZs were affected similarly by unobserved factors once OZs were implemented. Since 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 passed—census tracts could not have manipulated the determinants of their eligibility for purposes of qualifying as an OZ. We then use a fuzzy regression discontinuity design to determine the impact of OZ designation on commercial investment and economic activity. Our investment data cover the universe of significant commercial investments in the United States, and data from Mastercard enable us to observe any related impacts on new business creation, new business loans, commercial diversity and consumer spending. Across all outcomes, we find that OZ designation has little or no effect on investment and economic activity, suggesting that downstream impacts on OZ resident well-being are not forthcoming, consistent with the results of Freedman et al. (2021). Our results therefore suggest that the tax advantages provided represent a straightforward transfer from the government to investors with no robust evidence that the investment that does occur in OZs would not have occurred otherwise. As a consequence, OZ legislation—as evaluated 2.5 years after its

passage—does not appear to achieve its goal of reducing geographic disparity, on average, by improving the economic well-being of residents in worse off areas. Ultimately, other policies may be needed to overcome geographic disparity in well-being.

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 which hire employees who live in such areas. A number of states had their own Empowerment Zone programs before the federal government's Empowerment Zones and Enterprise Communities Act of 1993. Evidence on the effectiveness of Empowerment Zones has been mixed. Based on California's state-based program, O'Keefe (2004) found significant gains in employment resulting from Enterprise Zone designation, while Neumark and Kolko (2010) found no such effects when controlling for changes in zone borders over time. Focusing on other states in addition to California, Bondonio and Engberg (2000) and Greenbaum and Engberg (2000) similarly find no effect on employment, with the latter also finding no effect on housing prices or home occupancy rates. Neumark and Young (2021) find no evidence of long-term impacts of state programs or impacts that are stronger for certain state programs. The evidence on the federal Empowerment Zones program is more positive. Ham et al. (2011) find positive effects 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 investment made in these areas. 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. 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.

Differences in the design of OZs compared to previous efforts could explain differences in their effects. 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 suggest that OZs were indeed not effective at spurring new investment. As a tool for reducing geographic disparity in well-being, OZs do not improve

on the mixed evidence of effectiveness from previous place-based policies.

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. Section 6 discusses results and policy implications. Section 7 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 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)

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

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. 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

[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.

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 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

[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.

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

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. As mentioned in the introduction, 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. 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. 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 (POI) 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. 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 data report that the large majority of transactions are for investment 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]

[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).

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

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 (POI) data, 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 A.1 provides additional descriptions of the Mastercard variables.

Table 1 presents summary statistics related to census tract characteristics and our outcome variables for each of the three groups of census tracts, focusing on the years immediately prior to the 2018 designation of OZs when possible. By design, ineligible census tracts are better off economically than eligible census tracts. It is also clear from Table 1 that among the eligible census tracts, states chose tracts that are, on average, more distressed, with lower incomes, higher poverty rates, higher unemployment rates, and lower rates of labor force participation. This suggests that simply comparing outcomes in OZs to outcomes in eligible

[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.

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. 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 present a bit of a mixed picture. Selected tracts have on average a lower new business growth rate than eligible but not selected or non-eligible 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 non-eligible 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]

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

[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.

into the governors' selection criteria. In some states, governors sought geographic balance in their selections, while in others a balance between rural and urban tracts was given priority. In yet other states, governors held a multi-step process whereby citizens could weigh in or preference was given to regions that were previously designated as high priority areas and so were natural choices for OZ designation. In sum, once eligible, governors were given significant leeway in the specific tracts chosen based upon the goals of the state.

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

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

- had a poverty rate of at least 20 percent, or

- had a median family income below 80 percent of either the state median family income or the MSA median family income, or

- were contiguous with a selected tract based upon the first two qualifications, and 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 80 percent of the threshold are arguably similar to census tracts that fall near but on the opposite side of the relevant threshold. Since in general only the latter tracts were eligible to be designated as OZs, we can estimate the treatment effect of OZ eligibility by comparing outcomes (or changes in outcomes) of tracts just below the cutoff to those just above ("intent to treat"). Furthermore, we can scale the effect upward in a fuzzy RD design based on the fact that only some eligible tracts were actually designated as OZs ("treatment effect on the treated").

With this institutional setting in mind, consider the following model:

$$Y_i = T_i\gamma + X_i\beta_1 + \epsilon_i \tag{1}$$

$$T_i = D_i\delta + X_i\beta_2 + \mu_i \tag{2}$$

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

where Y_i represents our outcomes of interest—investment or commercial activity in census

[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.

tract i—typically specified as the change in the outcome variable before versus after the OZ provision went into effect; T_i is a binary treatment indicator based on OZ selection; and X_i is the vector of observable, pre-determined census tract characteristics that are correlated with the outcome of interest—labor force participation rate, employment to population ratio, unemployment rate and share of workers in construction, manufacturing and retail. We allow for imperfect compliance in Equation 2 since not all eligible census tracts are selected as OZs: Here D_i is a binary indicator based on eligibility status. Equation 3 reflects that census tracts with a value of the running variable r_i above the cutoff c are eligible to be selected as OZs.

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. 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

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

because the census tract is eligible regardless. This is illustrated in Figure 3. Panel (a) considers only census tracts with median incomes below 80 percent of the MSA or state median. Groups of census tracts with approximately equal poverty rates are placed in bins using spacing estimators under the mimicking variance evenly-spaced method (Calonico et al. (2014)), and we calculate the share that were selected as OZs. Poverty rates and implied probabilities of selection are plotted for each bin. As seen in the figure, census tracts with poverty rates above 20 percent are not more likely to be selected because they already satisfy the income condition for eligibility. Thus, these tracts are not useful in identifying the impact of OZ designation. Panel (b) considers only census tracts with median incomes above 80 percent of the MSA or state median. In this case, crossing over the 20 percent poverty rate threshold substantially increases the probability of being designated an OZ. Census tracts with a slightly higher or slightly lower poverty rate than 20 percent are economically very similar, and so differences in outcomes between these two groups of tracts can be attributed to selection as an OZ, rather than other differences.

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).

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

we construct the running variable r:

$$r_i \equiv \max\{\frac{P_i - 20}{20}, -\frac{I_i - 0.8 * I_m}{0.8 * I_m}\} \tag{4}$$

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 4 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 5 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.

Table 2 presents parametric estimates of the first stage results from Equation (2) using

[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.

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 A.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.

5 The impact of OZs on investment and economic activity

In this section, we report results for two broad outcomes—investment and economic activity. For each outcome, we report reduced form results that show how the outcome differs when crossing the eligibility threshold, as well as second stage results that show how OZ designation itself affects the outcome using a fuzzy RD design.[16]

[16]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, and because the NMTC only started using the 2011-2015 ACS to determine eligibility starting in the period from November 2017 through October 2018.

5.1 Investment

We consider three primary investment related outcomes for our RD analysis. 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 post-designation compared to pre-designation. In particular, we take the difference of the annualized value between July 1, 2018 and December 31, 2020 and the annualized value between January 1, 2016 and June 30, 2018. 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. We also consider specifications of the outcome variables in level terms. These specifications would 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. See Table 3 for means of our non-transformed dependent variables.

We show first the impact of OZ eligibility on the three investment outcomes in Table 4. These results are essentially intent-to-treat estimates because they only estimate the impact of being eligible for selection as an OZ rather than the impact of actually being selected. For each outcome, we show six total results—using each of our three running variables based on poverty, income and a combination, and with and without control variables. Estimated impacts of OZ eligibility are small and not statistically different from zero. For example, when using the combination running variable and including controls, OZ eligibility increases the probability of investment by 0.2 percentage points (0.6%), reduces the number of investments by 0.009 (2.4%), and decreases dollars of investment by \$0.18 million (3.3%). While the results are inconsistently signed, they all point to a near-zero impact of OZ eligibility on investment. Figure 6 shows these near-zero impacts of OZ eligibility graphically. Appendix

Figures A.2 - A.7 show the impacts based on different types of tracts (urban and rural), different types of investment (industrial, office and retail), different specifications of our outcome variables (levels in the treatment period, growth in 2020 relative to 2019, and levels in individual years), and different sample restrictions (dropping contiguous tracts and dropping selected contiguous tracts). In each case we see no evidence of more investment when crossing the eligibility threshold.

We next estimate treatment on the treated effects by using a fuzzy regression discontinuity design. This allows us to estimate the impact of OZ designation itself on investment outcomes. Table **??** reports estimated effects for the same specifications shown in Table 4, with the only difference being that we use a fuzzy design based on actual selection of tracts as OZs. Estimates are again small and not statistically different from zero, though standard errors are larger. Estimates for the combination running variable with controls implies that OZ designation decreases the probability of any investment by 0.4 percentage points (1.2%), increases the number of investments by 0.007 (1.9%), and decreases dollars of investment by $2.1 million (38%).[17] Table A.4 shows that these null results are robust to variations in the bandwidth around the threshold and Table A.5 shows similarly for variations in the fitting polynomial.[18] Finally, see Table A.6 for results based on various alternative specifications with different outcome variables and sample restrictions—estimates are generally small and not statistically different from zero.

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

[17]Notably, we observe total "initial" investment, which does not include the amount invested after the purchase of the property. This is important because an investment is only eligible for preferential tax treatment if it involves "substantial improvement" for the property.

[18]The first row of Table A.5 is the same as the final row of our main results table, Table **??** as our baseline is a linear specification (polynomial of order one).

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 eligible census tracts that we include in our analysis are relatively better off in terms of poverty and relative median income than tracts that are far from the cutoff points. As such, one may imagine that because the census tracts in our RD sample are more attractive for investors than those facing more extreme levels of economic distress, they would likely have larger treatment effects from OZ selection. In fact, using proprietary IRS data, Kennedy and Wheeler 2021 document that only about 16% of selected tracts received positive investment by the end of 2019 from QOFs and that these tracts look "better" on observable characteristics (levels and growth rates) such as education, median home value, and median household income compared to OZ selected tracts that received no investment. We posit that these tracts are more likely to be closer to the thresholds than those who are significantly further away.

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 8 shows the dispersion of investment (presence of any investment, number of investments and dollars of investment) over all census tracts in the pre-period (January 2016 through June 2018). As indicated by the 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 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. This suggests that the investment effects we estimate apply to those eligible census tracts where most investment occurs.

Finally, we directly test whether worse off tracts (within the universe of tracts near the thresholds) have stronger investment effects of OZ designation. Table A.8 reports 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 optimal bandwidth for the income threshold offer systemically respond differently to OZ selection than those closer to the poverty threshold (i.e. in the 10–20 percent range). We repeat our baseline analysis and both cases again show no statistically significant impact of OZ selection on investment. The data are a bit thinner in the former case and this is reflected in the noisier estimates but there is nothing that particularly stands out from this exercise that would suggest that potential OZ selection effects would be any different further away from the combined running variable threshold.

5.2 Business activity and consumer spending

Despite the lack of any positive and economically important impact on commercial investment, we also estimate whether there are general improvements in other measures of economic development 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 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 such as growth in business formation, new business loans, and business type variety, as well as proprietary data on consumer spending.

Tables 5 and 6 and Figure 7 summarize the results on our business activity outcomes. Table 5 present the impact of OZ eligibility on our economic activity outcomes and Table 6 presents the fuzzy regression discontinuity results of OZ selection on our economic activity outcomes. We primarily focus on outcomes in 2019 or changes from an earlier year to 2019 based upon data availability. The first two columns present growth rates of new businesses and new business loans from 2018 to 2019, column (3) presents the 2019 commercial diversity levels and columns (4) and (5) present the change in the ranking of per capita spending and aggregate spending growth from 2017 to 2019. In line with the previous findings on investment, there is no statistically significant impact of OZ eligibility or selection on any of our outcome variables at conventional levels of significance. Focusing on Table 6, we can see that many of the point estimates themselves are economically significant and show positive local average treatment effects of OZ selection on business loan growth, the level of commercial diversity and rankings in census tract spending per capita (for example, the final row, forth column estimates that OZs moved up 12 points in their relative ranking of census tracts by per capita spending growth). In contrast, new business growth and aggregate spending growth show negative impacts, though the point estimates themselves are rather small in the former.[19] Nonetheless, the confidence intervals are sufficiently large at typical

[19]We can reconcile the results in columns (4) and (5) by population growth, that is, these results suggest a negative impact of OZs on aggregate spending but once normalized by the number of residents, spending growth is positive. To be more specific, fewer people are spending more per capita but not in aggregate.

levels of significance such that we cannot rule out that there is no impact on any of our outcome variables. Figure 7 confirms these findings graphically and also include a number of subfigures ((f) - (i)) where we focus on 2020 outcomes. These subfigures are similar to the 2019 outcomes and show that that the Covid-19 pandemic appears to not have had any differential impact on OZ in terms of the economic activity variables considered here.

6 Discussion

The OZ provision of the TCJA was intended to help address the growing economic gap faced by disadvantaged communities in the United States. By providing tax incentives for private investment into high-poverty, low-income census tracts covering over one-tenth of the U.S. population, legislators hoped that OZ tax incentives would fuel private investment into disadvantaged communities which in turn would eventually improve the well-being of OZ residents through increased employment, increased incomes and reduced poverty. While it is still early to evaluate whether OZs will eventually successfully achieve these objectives, our results provide insight into early impacts with implications for expectations about future effects.

We find that through the end of 2020, three years after TCJA was passed and two and a half years since OZ selections were finalized, investment did not increase as a result of the tax incentives. This conclusion also holds when we examine 2019 alone, before investment and economic activity were impacted by the COVID-19 pandemic. More specifically, based on our fuzzy multivariate regression discontinuity estimates that account for the multivariate nature of the selection criteria, we find no statistically significant impact on the probability of any investment, the number of investments, nor on the overall transaction dollars for tracts

that are near the qualification cutoffs. These findings are further supported by Mastercard data that also show no statistically significant impact on new business and new business loan growth, commercial diversity (proxy for commercial variety) and consumer spending measures. Our results are consistent with the findings of Chen et al. (2019) who find little effect of OZ designations on home prices—if home buyers correctly anticipated little new investment as a result of OZs, then home prices should not have increased significantly once OZs designations were announced.

Comparing our null results to positive effects from some other place-based policies such as the NMTC 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 near-zero and statistically insignificant investment effects, it is important to emphasize that our results speak to the average effect of OZ selection on investment and economic activity–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 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 9 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. Second, it is striking as to how few "turned around" tracts there are across all three groups. Less that 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. The natural channel through which employment and wages could rise is through an increase in private investment that increases labor demand. No effect on investment suggests that these downstream effects are 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. Our results

are consistent with the lack of downstream effects, and moreover, suggest that we should not expect downstream effects to materialize in future years through lagged effects of investment.

It is important to emphasize important caveats for interpreting our results. First, our estimates extend 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. Investors could increase investment over time as they become more familiar with OZ rules, although the tax benefits from OZ investment diminish over time. Second, 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.

7 Conclusion

The persistence of economic disadvantage in some areas in the United States, combined with reduced geographic mobility, has led to renewed calls for policies that can improve the economic circumstances of residents in struggling regions. Our results suggest that at least in their first two and a half years of existence, there is not evidence that OZ tax incentives have significantly increased commercial investment in designated OZs. Similarly, business-related outcomes such as growth in new business formation, new business loans, commercial diversity and consumer spending have not substantially changed in OZs as a result of designation.

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.

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Table 1: Summary Statistics: Means and Standard Deviations

	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.

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

Running variable	**No controls**	**With controls**
Poverty	0.155	0.155
	(.017)	(0.017)
Number of tracts	7,720	7,720
Income	-0.113	-0.111
	(0.011)	(0.010)
Number of tracts	13,555	15,762
Combination	0.118	0.118
	(.008)	(0.008)
Number of tracts	22,436	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.

Table 3: Dependent Variable Means, Commercial Investment

	Eligible & selected	Eligible & not selected	Not eligible	All
Any investment: Share				
Jan. 1, 2016 to June 30, 2018	0.33	0.25	0.32	0.29
Jul. 1, 2018 to Dec. 31, 2020	0.33	0.24	0.31	0.28
Annualized number of investments				
Jan. 1, 2016 to June 30, 2018	0.37	0.25	0.35	0.31
Jul. 1, 2018 to Dec. 31, 2020	0.38	0.25	0.33	0.30
Annualized dollars of investment in millions				
Jan. 1, 2016 to June 30, 2018	4.18	2.73	4.57	3.68
Jul. 1, 2018 to Dec. 31, 2020	4.20	2.76	4.37	3.61
Number of tracts	7,727	33,131	30,813	71,671

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

Table 4: Impact of Opportunity Zones Eligibility on Commercial Investment, Combination Running Variable

Specification	Any investment	Number of investments	Millions of dollars
Regression Discontinuity Estimates			
No controls	0.001	-0.010	-0.168
	(0.014)	(0.014)	(0.246)
	27,801	32,948	26,560
With controls	0.002	-0.009	-0.159
	(0.014)	(0.014)	(0.245)
	28,328	32,130	26,756
Fuzzy Regression Discontinuity Estimates			
No controls	-0.011	0.001	-1.418
	(0.125)	(0.141)	(2.071)
	25,200	22,161	26,480
With controls	-0.004	0.007	-1.747
	(0.122)	(0.14)	(2.205)
	25,710	21,387	21,410

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

Notes: Table reports coefficient, standard error, and effective sample size for each specification and outcome variable. 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 (January 1, 2016 through June 30, 2018). 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 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.

Table 5: Impact of Opportunity Zones Eligibility on Economic Activity, Regression Discontinuity Estimates

Running variable	**New bus. growth**	**Business loan growth**	**Comm. div.**	**Spend growth per capita**	**Spend growth**
No controls					
Poverty	-0.207	0.751	0.105	0.941	-1.371
	(0.954)	(1.492)	(0.444)	(1.484)	(1.52)
Number of tracts	8,557	8,205	9,672	11,336	11,737
Income	0.304	0.422	0.276	1.964	-0.969
	(0.729)	(1.25)	(0.365)	(1.505)	(1.437)
Number of tracts	12,604	11,289	9,590	8,576	10,376
Combination	-0.068	1.285	0.103	0.901	-0.631
	(0.451)	(0.696)	(0.212)	(0.845)	(0.885)
Number of tracts	32,004	33,593	29,444	25,167	25,821
With controls					
Poverty	-0.125	0.942	0.041	0.837	-1.77
	(0.943)	(1.479)	(0.436)	(1.545)	(1.603)
Number of tracts	8,557	8,205	8,917	10,200	9,874
Income	0.24	0.479	0.14	1.906	-1.116
	(0.715)	(1.217)	(0.343)	(1.49)	(1.439)
Number of tracts	13,261	11,835	10,272	8,730	10,301
Combination	-0.088	1.284	0.067	0.893	-0.717
	(0.441)	(0.679)	(0.211)	(0.851)	(0.874)
Number of tracts	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: 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. We report observations for the final specification in which spending growth is the dependent variable. Standard errors (in parentheses) are estimated using the heteroskedasticity-robust nearest neighbor variance estimator.

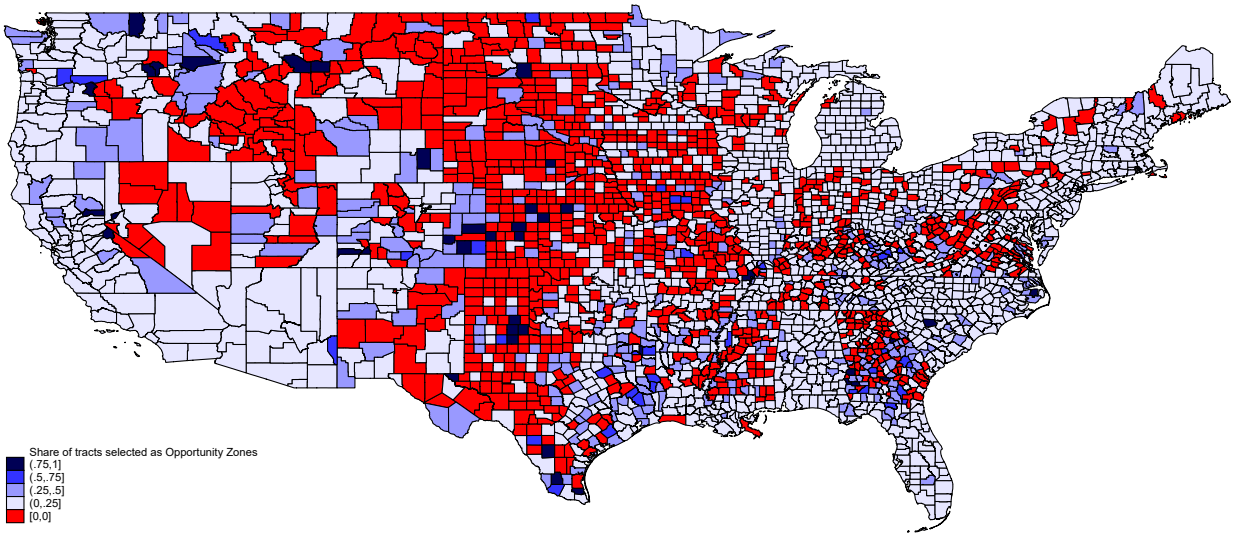
Table 6: Impact of Opportunity Zones Designation on Economic Activity, Fuzzy Regression Discontinuity Estimates

Running variable	**New bus. growth**	**Business loan growth**	**Comm. div.**	**Spend growth per capita**	**Spend growth**
No controls					
Poverty	-1.478	5.659	0.808	5.208	-7.309
	(5.189)	(10.099)	(2.83)	(8.446)	(8.359)
Number of tracts	12,347	7,197	8,917	11,158	12,130
Income	0.944	6.116	0.483	11.248	-8.431
	(6.228)	(9.908)	(2.622)	(10.325)	(10.246)
Number of tracts	14,735	15,540	15,675	13,721	14,990
Combination	-0.084	9.867	0.564	8.106	-5.446
	(4.352)	(6.267)	(1.817)	(7.201)	(7.694)
Number of tracts	21,933	25,484	24,194	22,549	21,416
With controls					
Poverty	-0.961	6.091	0.257	4.586	-7.876
	(5.037)	(9.505)	(2.72)	(8.442)	(8.096)
Number of tracts	12,783	8,205	8,731	10,570	12,528
Income	0.342	5.551	0.017	11.56	-9.563
	(5.898)	(10.166)	(2.571)	(10.441)	(10.513)
Number of tracts	15,831	14,757	15,465	13,519	14,170
Combination	-0.535	9.829	0.27	12.058	-7.823
	(4.123)	(6.441)	(1.724)	(7.758)	(8.172)
Number of tracts	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.

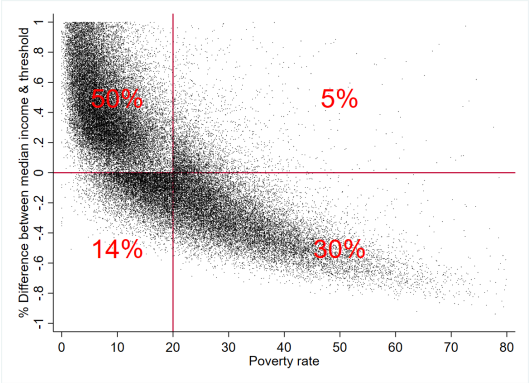
Notes: New business growth, business loan growth and commercial diversity are values as of 2019. Spending growth per capita and spending growth are values as of 2019 minus values as of 2017. When poverty is the running variable, the sample is restricted to census tracts where the income condition is 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. We report observations for the final specification in which spending growth is the dependent variable. Standard errors (in parentheses) are estimated using the heteroskedasticity-robust nearest neighbor variance estimator.

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

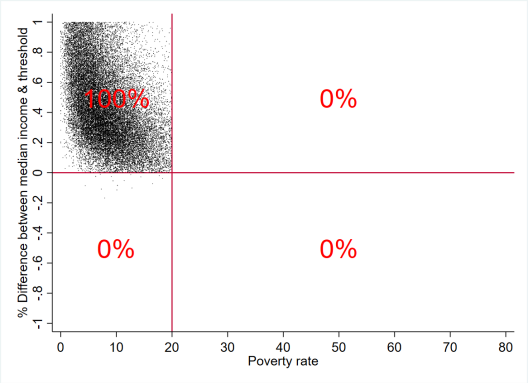


Source: U.S. Department of the Treasury; Authors' calculations

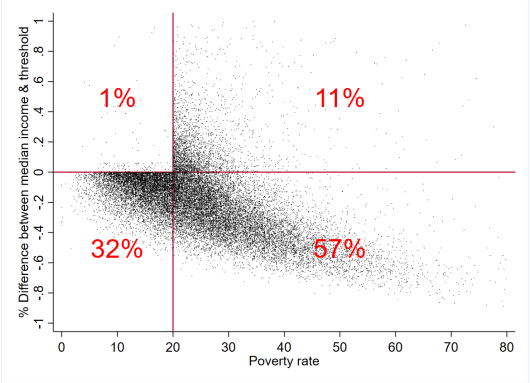
Figure 2: Poverty rate and percent difference between median income and threshold, by census tract type



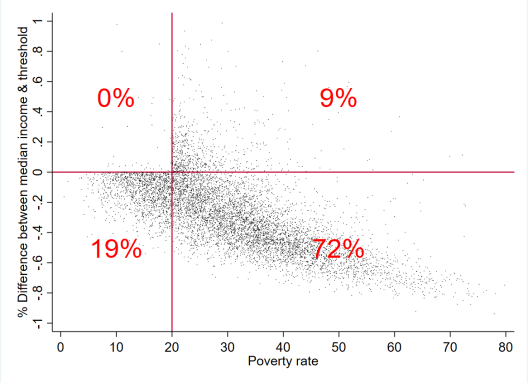
(a) All census tracts



(b) Ineligible census tracts



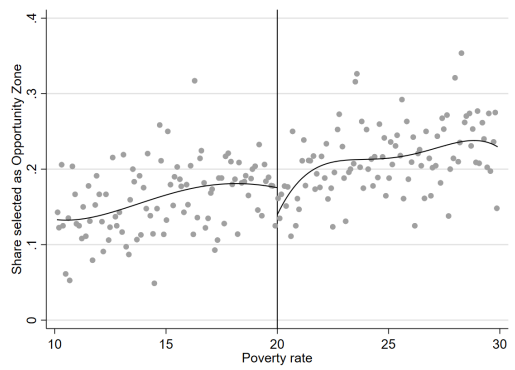
(c) Eligible but not selected census tracts



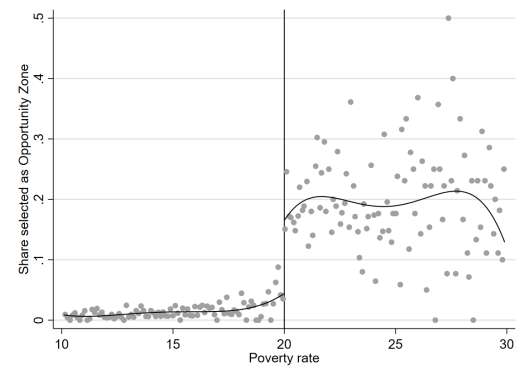
(d) Selected census tracts

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.

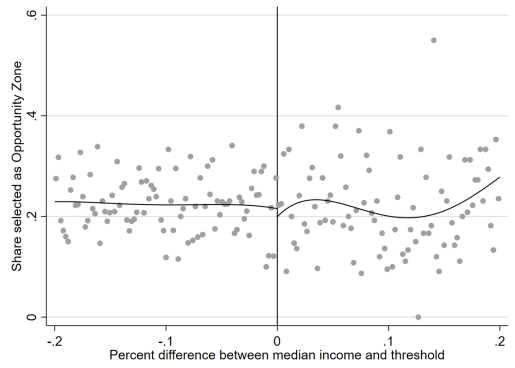
Figure 3: Share of tracts designated as Opportunity Zones by income and poverty thresholds



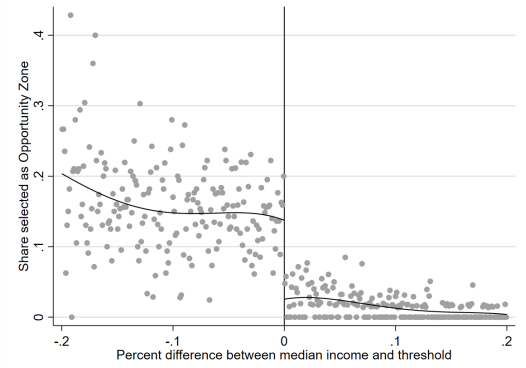
(a) Tracts with median income below 80% of MSA/state median income



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



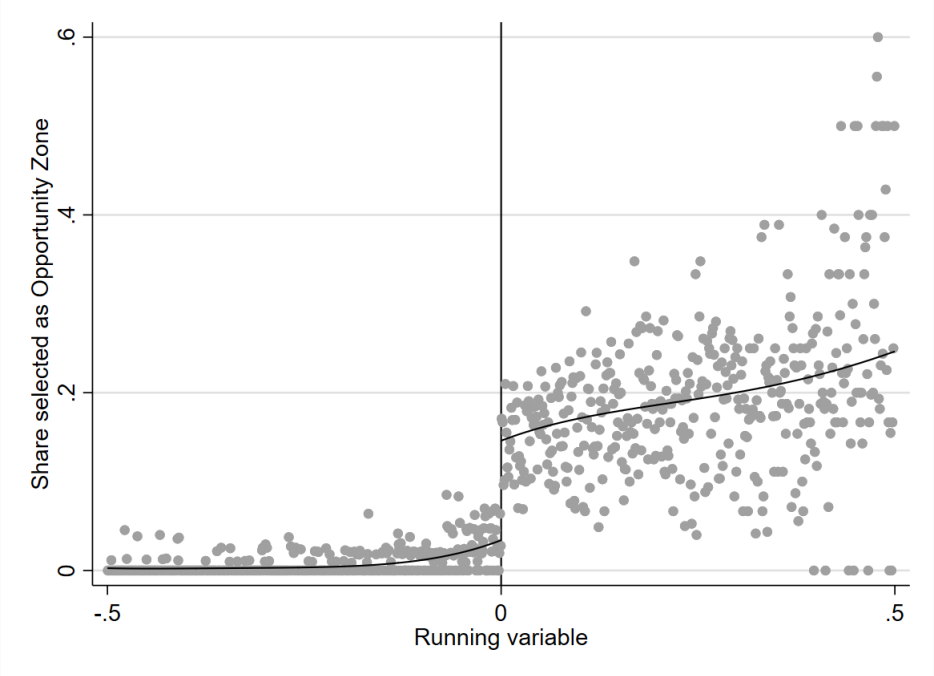
(c) Tracts with poverty rate above 20%



(d) Tracts with poverty rate below 20%

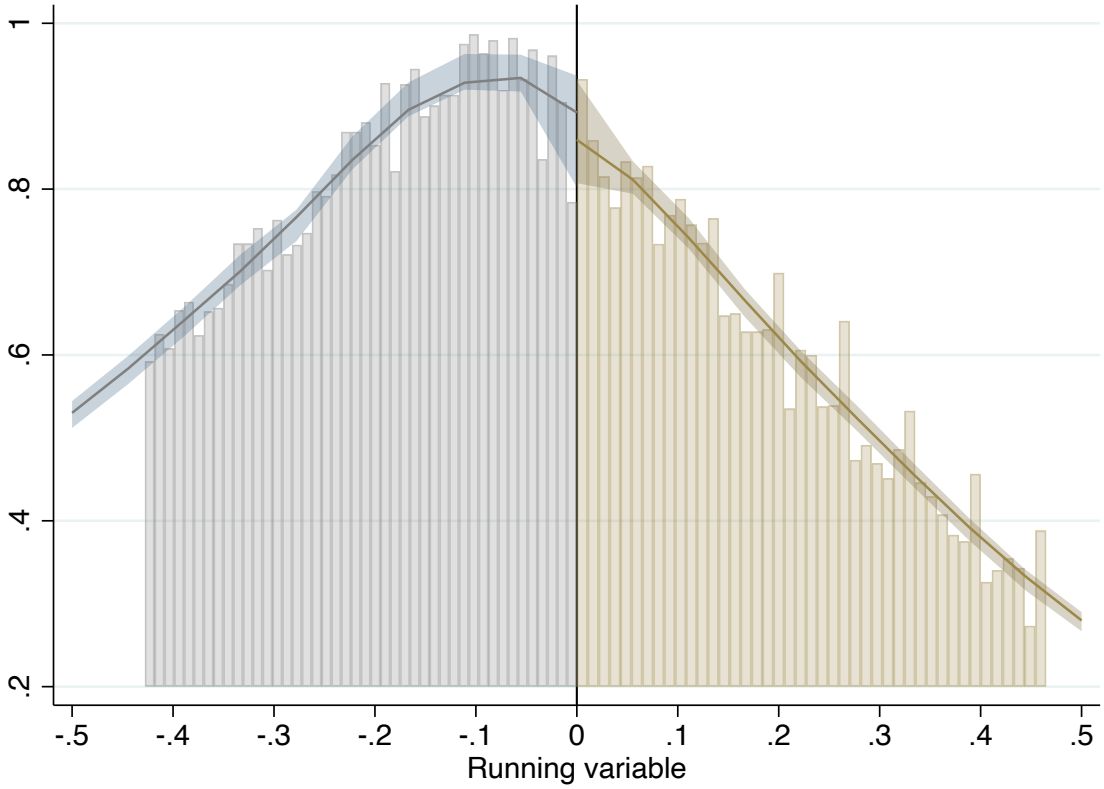
Sources: American Community Survey 2011-2015 5-year pooled sample; U.S. Department of the Treasury.
Notes: Each dot represents the sample average within each bin. Fitted lines are based on a polynomial of degree 4 fitted separately to points on either side of the cutoff.

Figure 4: Share of tracts designated as Opportunity Zones by constructed running variable



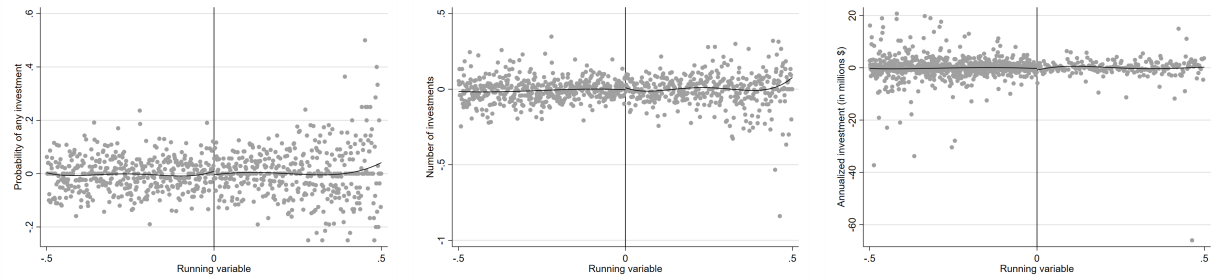
Sources: American Community Survey 2011-2015 5-year pooled sample; U.S. Department of the Treasury.
Notes: Each dot represents the sample average within each bin. Fitted lines are based on a polynomial of degree 4 fitted separately to points on either side of the cutoff.

Figure 5: 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

Figure 6: Investment outcomes by constructed running variable



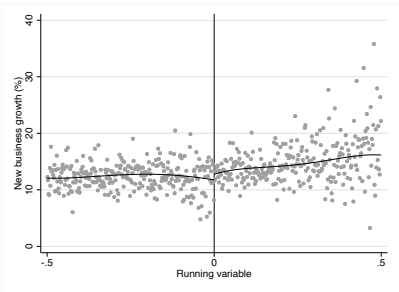
(a) Any investment

(b) Number of investments

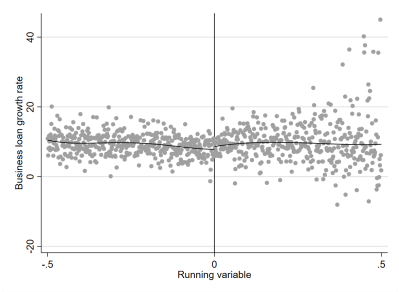
(c) Dollars (millions) of investment

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 the treatment period value (July 1, 2018 through December 31, 2020) and the control period value (January 1, 2016 through June 30, 2018). Dollars of investment are winsorized by replacing transactions 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.

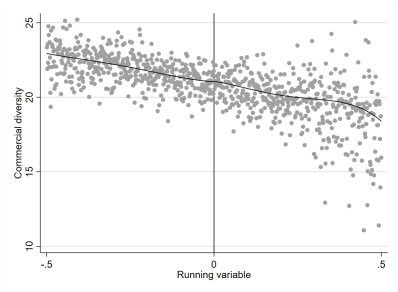
Figure 7: Mastercard business data by constructed running variable



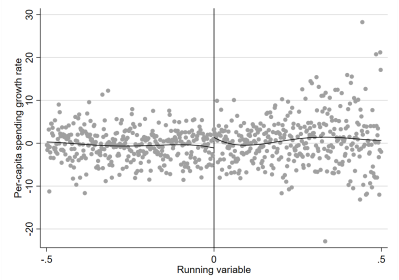
(a) New business growth 2019



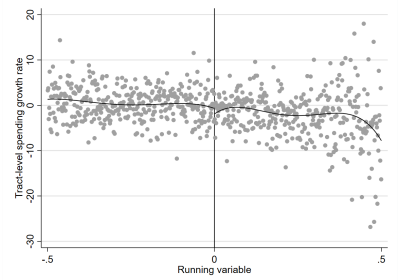
(b) Business loan growth 2019



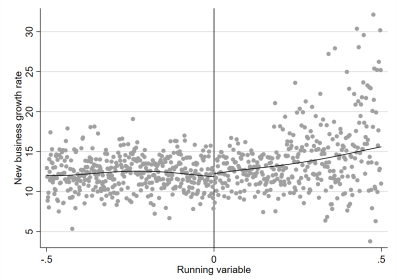
(c) Commercial diversity 2019



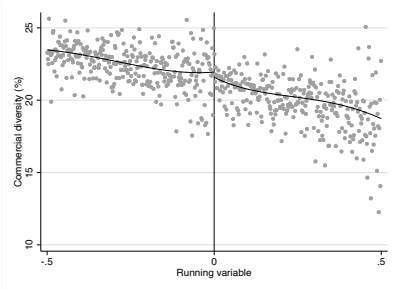
(d) Spending growth per capita (change in rank, 2017-2019)



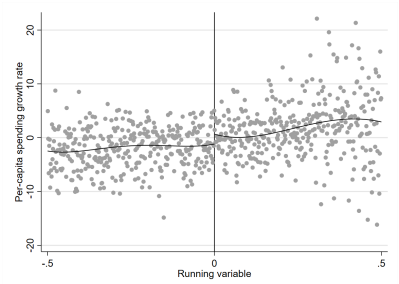
(e) Spending growth (change in rank, 2017-2019)



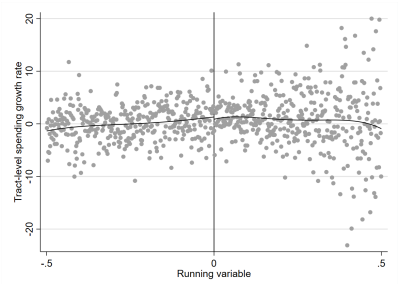
(f) Business loan growth 2020



(g) Commerical diversity 2020



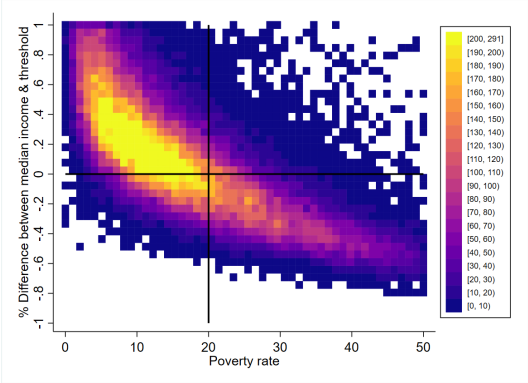
(h) Spending growth per capita (change in rank, 2017-2020)



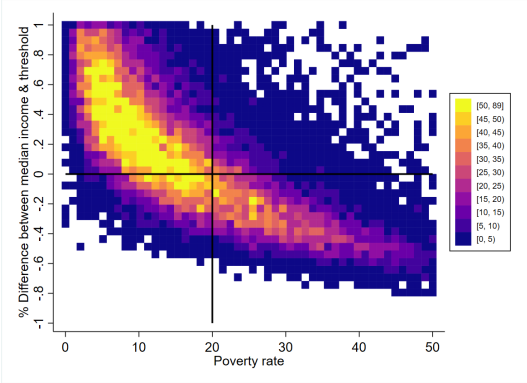
(i) Spending growth (change in rank, 2017-2020)

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.

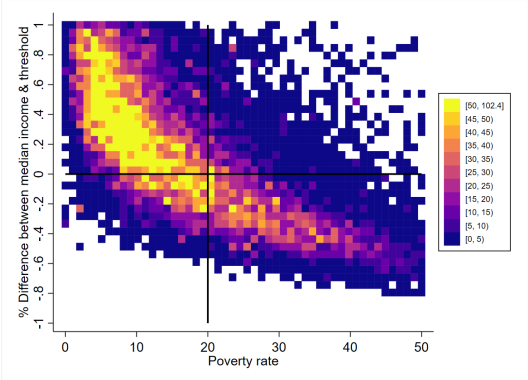
Figure 8: Investment outcomes by poverty rate and percent difference between median income and threshold, control period



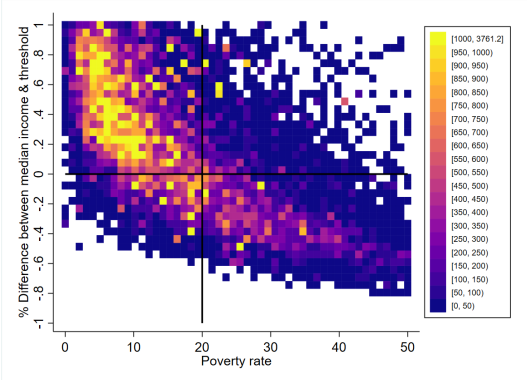
(a) Number of census tracts



(b) Number of census tracts with any investment



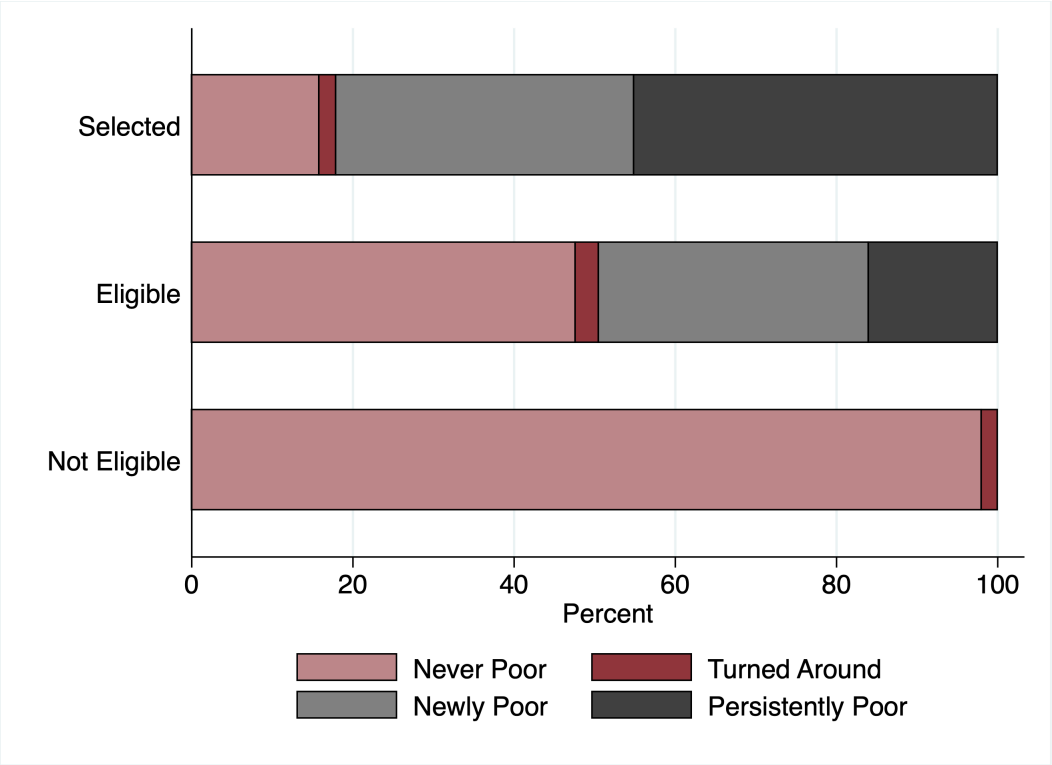
(c) Number of investments



(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.

Figure 9: Poverty 1980 vs. poverty 2011-15



Num. 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

Appendix Tables and Figures

Table A.1: Descriptions of Mastercard Variables

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

Table A.2: Impact of Opportunity Zones Eligibility on Commercial Investment, Poverty Running Variable

Specification	Any investment	Number of investments	Millions of dollars
Regression Discontinuity Estimates			
No controls	0.042	0.040	0.112
	(0.029)	(0.037)	(0.530)
	8,557	7,536	10,256
With controls	0.042	0.039	0.149
	(0.029)	(0.037)	(0.537)
	8,384	7,536	9,672
Fuzzy Regression Discontinuity Estimates			
No controls	0.276	0.257	0.701
	(0.181)	(0.214)	(3.319)
	8,917	8,917	10,256
With controls	0.286	0.241	0.931
	(0.178)	(0.202)	(3.319)
	9,310	11,107	9,672

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

Notes: Table reports coefficient, standard error, and effective sample size for each specification and outcome variable. 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 (January 1, 2016 through June 30, 2018). Dollars of investment are winsorized by replacing observations with values above the 95th percentile with the 95th percentile. The sample is restricted to census tracts where the income eligibility condition is not satisfied. 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.

Table A.3: Impact of Opportunity Zones Eligibility on Commercial Investment, Income Running Variable

Specification	Any investment	Number of investments	Millions of dollars
Regression Discontinuity Estimates			
No controls	-0.004	0.009	-0.226
	(0.023)	(0.025)	(0.337)
	10,405	9,058	13,526
With controls	-0.005	0.009	-0.218
	(0.023)	(0.025)	(0.334)
	10,568	9,112	13,861
Fuzzy Regression Discontinuity Estimates			
No controls	-0.096	0.096	-2.330
	(0.18)	(0.195)	(2.796)
	13,846	12,289	15,900
With controls	-0.093	0.068	-2.295
	(0.184)	(0.193)	(2.784)
	14,076	13,733	15,882

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

Notes: Table reports coefficient, standard error, and effective sample size for each specification and outcome variable. 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 (January 1, 2016 through June 30, 2018). Dollars of investment are winsorized by replacing observations with values above the 95th percentile with the 95th percentile. The sample is restricted to census tracts where the poverty eligibility condition is not satisfied. 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.

Table A.4: Impact of Opportunity Zones Selection on Commercial Investment, Fuzzy Regression Discontinuity Estimates, Various Bandwidths

Bandwidth	Any investment	Number of investments	Millions of dollars
0.05	0.080	0.148	0.694
	(0.315)	(0.319)	(4.908)
Number of tracts	6,163	6,163	6,163
0.10	-0.049	0.076	-2.397
	(0.229)	(0.241)	(3.776)
Number of tracts	12,419	12,419	12,419
0.25	0.016	-0.043	-1.230
	(0.139)	(0.150)	(2.359)
Number of tracts	29,098	29,098	29,098
0.50	0.047	-0.069	-0.421
	(0.094)	(0.102)	(1.641)
Number of tracts	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: 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. Unless otherwise specified, these variables are expressed as differences between the treatment period value (July 1, 2018 through December 31, 2020) and the control period value (January 1, 2016 through June 30, 2018). Dollars of investment are winsorized by replacing observations with values above the 95th percentile with the 95th percentile. The constructed running variable is used for all specifications. 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.

Table A.5: Impact of Opportunity Zones Selection on Commercial Investment, Fuzzy Regression Discontinuity Estimates, Various Polynomials

Order of polynomial	**Any investment**	**Number of investments**	**Millions of dollars**
1	-0.004	0.007	-1.747
	(0.122)	(0.140)	(2.205)
Number of tracts	25,710	21,387	21,410
2	-0.030	0.064	-1.966
	(0.143)	(0.169)	(2.680)
Number of tracts	35,782	33,011	32,462
3	-0.079	0.006	-2.443
	(0.156)	(0.172)	(2.705)
Number of tracts	47,550	48,089	48,418
4	-0.098	0.095	-1.946
	(0.174)	(0.195)	(3.179)
Number of tracts	51,810	51,626	48,361

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

Notes: Any investment is an indicator variable equal to 1 if at least one investment was recorded. Number of investments is number of investments made, and millions of dollars is the the annualized sum of investment amounts across all investments. Unless otherwise specified, these variables are expressed as differences between the treatment period value (July 1, 2018 through December 31, 2020) and the control period value (January 1, 2016 through June 30, 2018). Dollars of investment are winsorized by replacing transactions with values above the 95th percentile with the 95th percentile. The constructed running variable is used for all specifications. 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.

Table A.6: Impact of Opportunity Zones Selected on Commercial Investment, Fuzzy Regression Discontinuity Estimates, Various Specifications

Outcome or sample	**Any investment**	**Number of investments**	**Millions of dollars**
Urban only	-0.005	0.052	-2.194
	(0.17)	(0.196)	(3.009)
Rural only	-0.156	-0.111	-1.028
	(0.18)	(0.088)	(0.765)
Type: Industrial	-0.087	-0.092	-2.719
	(0.085)	(0.079)	(1.357)
Type: Office	-0.007	0.094	1.001
	(0.09)	(0.071)	(1.558)
Type: Retail	-0.02	0.006	-0.269
	(0.122)	(0.085)	(0.948)
Levels: Treatment period	0.043	0.297	6.729
	(0.121)	(0.227)	(3.056)
Levels: 2014	0.158	0.122	3.318
	(0.1)	(0.25)	(2.880)
Levels: 2015	0.156	0.284	4.242
	(0.097)	(0.248)	(3.378)
Levels: 2016	0.094	0.376	13.610
	(0.079)	(0.219)	(3.640)
Levels: 2017	0.085	0.208	5.326
	(0.088)	(0.261)	(3.177)
Levels: 2019	0.003	0.251	7.089
	(0.103)	(0.276)	(3.638)
Levels: 2020	0.109	0.235	2.149
	(0.088)	(0.195)	(2.919)
Difference: 2020 vs. 2019	0.076	0.022	-5.2052
	(0.097)	(0.223)	(3.412)

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

Notes: Any investment is an indicator variable equal to 1 if at least one investment was recorded. Number of investments is number of investments made, and millions of dollars is the the annualized sum of investment amounts across all investments. Unless otherwise specified, these variables are expressed as differences between the treatment period value (July 1, 2018 through December 31, 2020) and the control period value (January 1, 2016 through June 30, 2018). Dollars of investment are winsorized by replacing observations with values above the 95th percentile with the 95th percentile. The constructed running variable is used for all specifications. 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.

Table A.7: Impact of Opportunity Zones Selection on Commercial Investment, Fuzzy Regression Discontinuity Estimates, Heterogeneity Analysis

Poverty range	**Any investment**	**Number of investments**	**Millions of dollars**
0% to 10%	-0.083	0.180	-18.837
	(0.579)	(0.597)	(11.581)
Number of tracts	3,261	2,326	3,740
10% to 20%	-0.068	0.062	0.191
	(0.180)	(0.202)	(2.773)
Number of tracts	11,977	11,105	12,290

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

Notes: Any investment is an indicator variable equal to 1 if at least one investment was recorded. Number of investments is number of investments made, and millions of dollars is the the annualized sum of investment amounts across all investments. Unless otherwise specified, these variables are expressed as differences between the treatment period value (July 1, 2018 through December 31, 2020) and the control period value (January 1, 2016 through June 30, 2018). Dollars of investment are winsorized by replacing observations with values above the 95th percentile with the 95th percentile. The running variable for all specifications is the percent difference between the census tract median income and the threshold median income needed to qualify for OZ eligibility. 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.

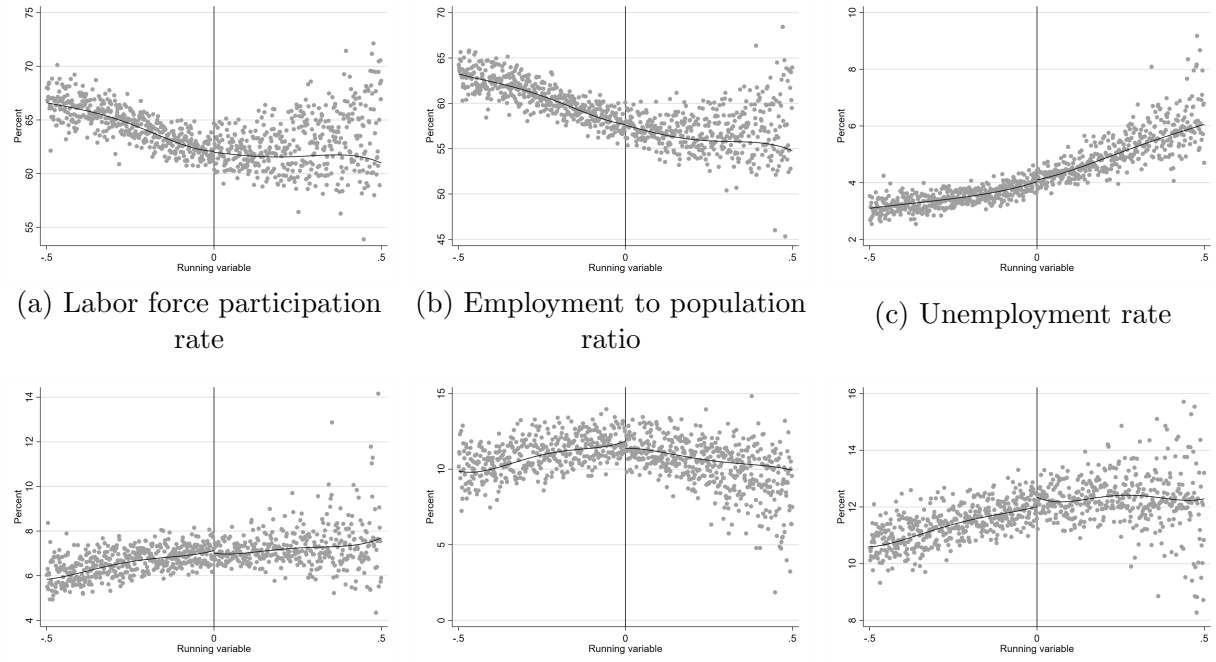
Table A.8: Impact of Opportunity Zones Selection on Commercial Investment, Counties with Uniform Eligibility Status of all Tracts

Model	**Any investment**	**Number of investments**	**Millions of dollars**
Regression Discontinuity	0.012	0.010	0.150
	(0.044)	(0.023)	(0.314)
	1,927	2,004	1,930
Fuzzy Regression	-0.103	-0.060	0.056
Discontinuity	(0.210)	(0.136)	(1.690)
	1,953	1,910	1,910

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

Notes: Tracts are included in the sample only if every tract within the county has the same OZ eligibility status. 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. Unless otherwise specified, these variables are expressed as differences between the treatment period value (July 1, 2018 through December 31, 2020) and the control period value (January 1, 2016 through June 30, 2018). Dollars of investment are winsorized by replacing observations with values above the 95th percentile with the 95th percentile. 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.

Figure A.1: Control variables by constructed running variable



(a) Labor force participation rate

(b) Employment to population ratio

(c) Unemployment rate

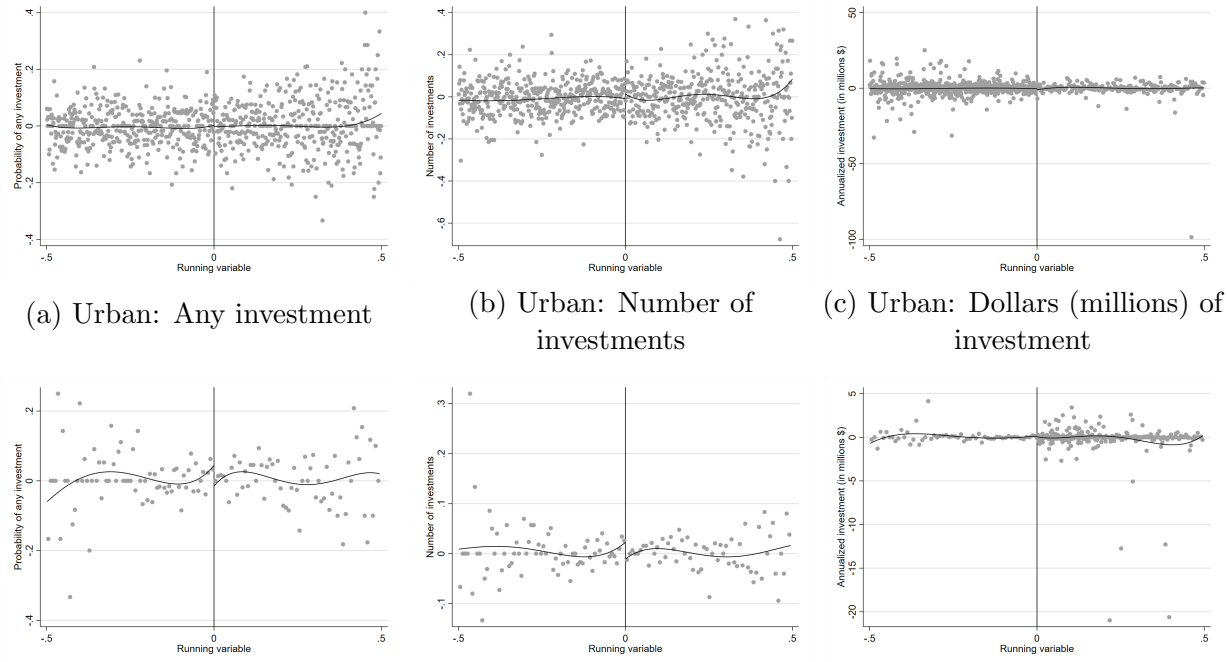
(d) Share of workers in construction

(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.

Figure A.2: Investment outcomes by constructed running variable, Urban and Rural



(a) Urban: Any investment

(b) Urban: Number of investments

(c) Urban: Dollars (millions) of investment

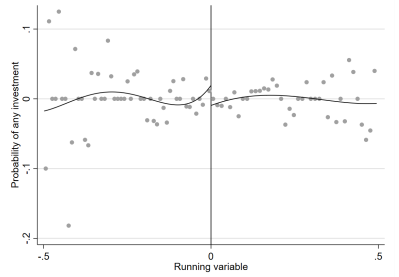
(d) Rural: Any investment

(e) Rural: Number of investments

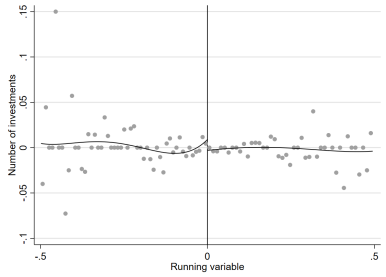
(f) Rural: Dollars (millions) of investment

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 (January 1, 2016 through June 30, 2018). Dollars of investment are winsorized by replacing transactions 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.

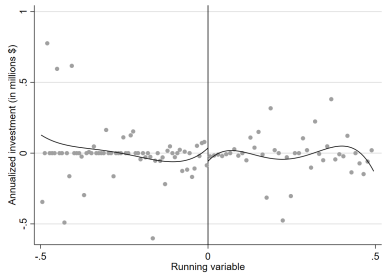
Figure A.3: Investment outcomes by constructed running variable, by sector



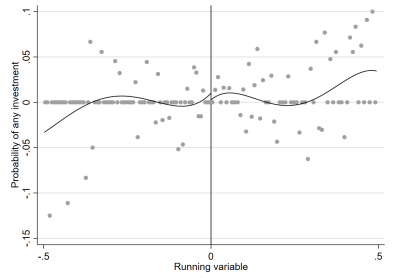
(a) Industrial: Any investment



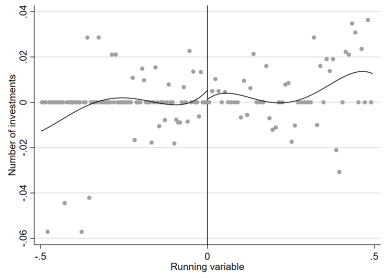
(b) Industrial: Number of investments



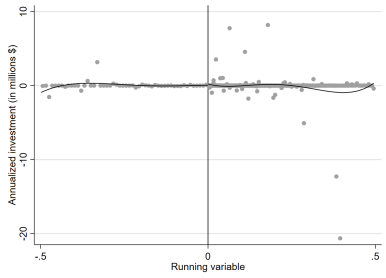
(c) Industrial: Dollars (millions) of investment



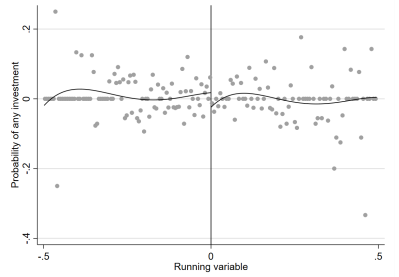
(d) Office: Any investment



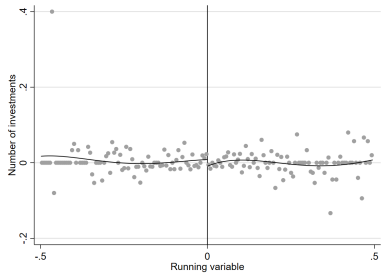
(e) Office: Number of investments



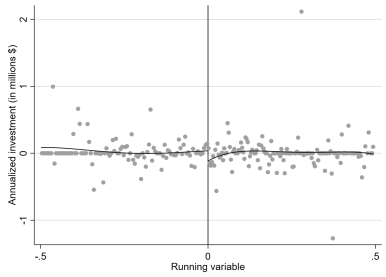
(f) Office: Dollars (millions) of investment



(g) Retail: Any investment



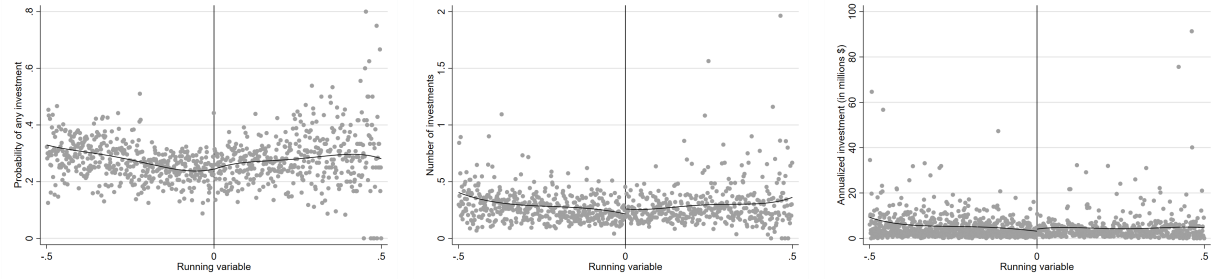
(h) Retail: Number of investments



(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 (January 1, 2016 through June 30, 2018). Dollars of investment are winsorized by replacing transactions 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.

Figure A.4: Investment outcomes by constructed running variable, Outcomes expressed in levels during treatment period



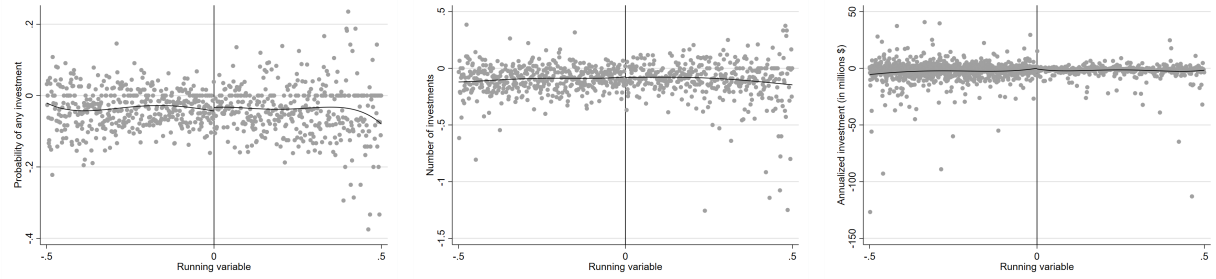
(a) Any investment

(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 transactions 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.

Figure A.5: Investment outcomes by constructed running variable, growth in 2020 relative to 2019



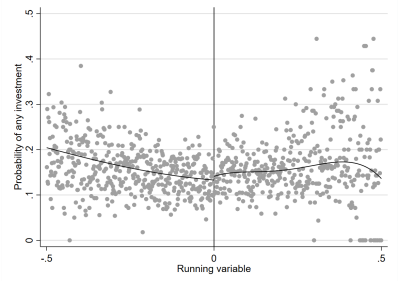
(a) Any investment

(b) Number of investments

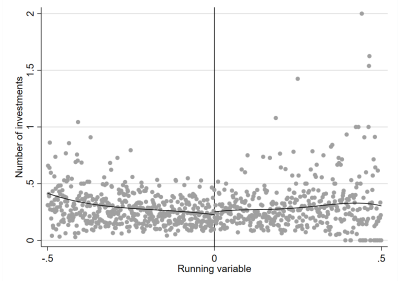
(c) Dollars (millions) of investment

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 transactions 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.

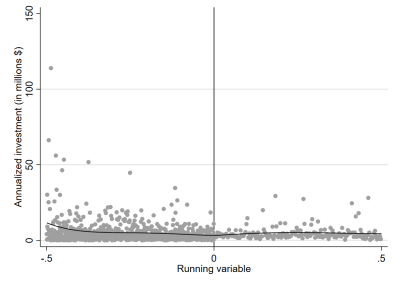
Figure A.6: Investment outcomes by constructed running variable, by year



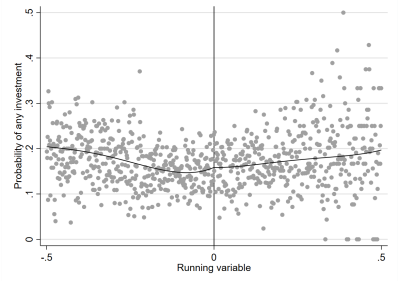
(a) 2017: Any investment



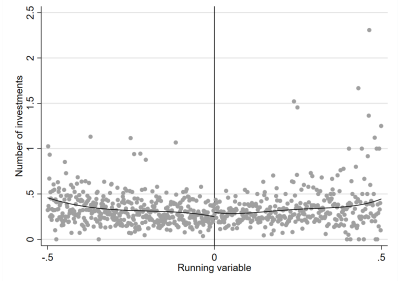
(b) 2017: Number of investments



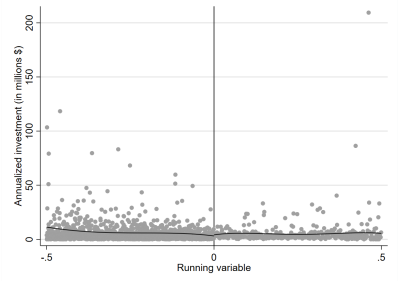
(c) 2017: Dollars (millions) of investment



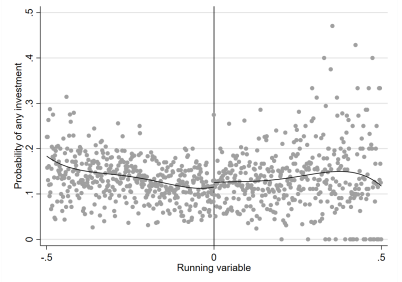
(d) 2019: Any investment



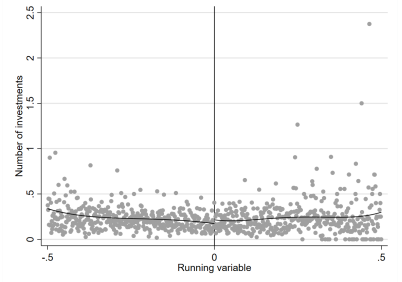
(e) 2019: Number of investments



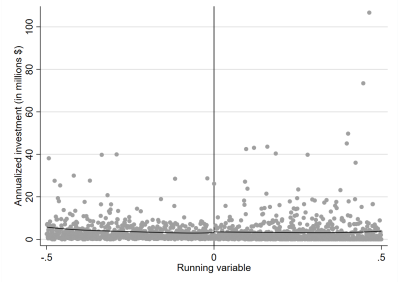
(f) 2019: Dollars (millions) of investment



(g) 2020: Any investment



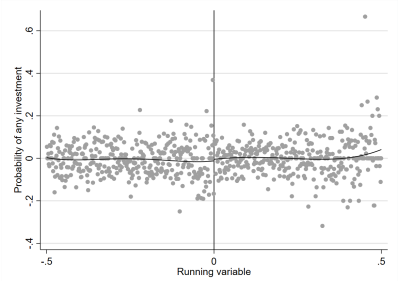
(h) 2020: Number of investments



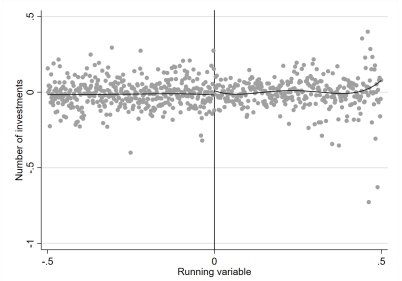
(i) 2020: Dollars (millions) of investment

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 transactions 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.

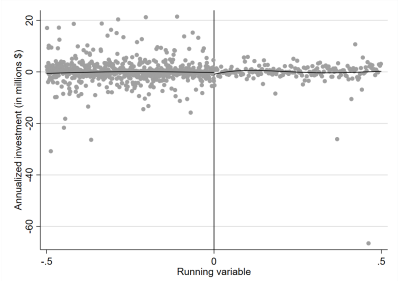
Figure A.7: Investment outcomes by constructed running variable, sample restrictions of contiguous tracts



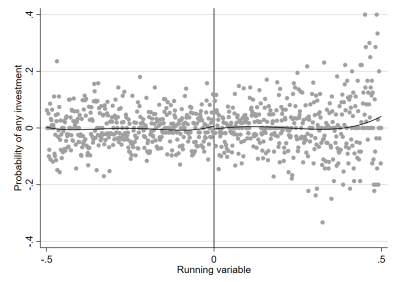
(a) Drop contiguous: Any investment



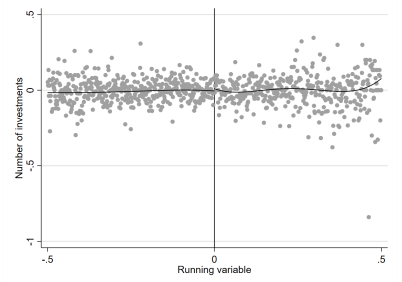
(b) Drop contiguous: Number of investments



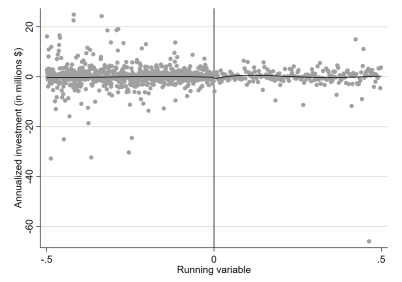
(c) Drop contiguous: Dollars (millions) of investment



(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 (January 1, 2016 through June 30, 2018). Dollars of investment are winsorized by replacing transactions 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.