The Effect of Tax Incentives on Local Private Investments and Entrepreneurship: Evidence from the Tax Cuts and Jobs Act of 2017

Jiajie Xu
Tippie College of Business
University of Iowa
jiajie-xu@uiowa.edu
(857) 361-9833

Current Version: March 2026

* For helpful feedback, I thank conference participants at the Western Finance Association, NBER Entrepreneurship SI, NBER Business Taxation in a Federal System, AEA, NASMES, European Finance Association, Midwest Finance Association, Northern Finance Association, ICEA-Taxation and Innovation Conference, Urban Economic Association, conference discussants including Alina Arefeva, Maryann Feldman, Marlene Koffi, Laura Lindsey, Xinxin Wang, Nils Wehrhofer, Ting Xu, Jason Zein, as well as seminar participants at Boston College, ESCP Business School, IE Business School, University of Iowa, University of Hong Kong, University of Tennessee, University of Arkansas, Hong Kong Polytechnic University. This paper is based on chapter one of my dissertation at Boston College and I thank my dissertation committee, Thomas Chemmanur, Francesco D'Acunto, Ran Duchin, Hassan Tehranian, Xuan Tian, for their guidance and support. I also want to thank *OpenCorporates* for providing data on business registration. I am responsible for any remaining errors and omissions.

1 Introduction

New business formation is crucial for economic growth, job creation, and wealth equality (Davis and Haltiwanger, 1992; Decker et al., 2014; Herranz et al., 2015). With the recent decline of new firms started in the U.S. (Pugsley and Şahin, 2019), both policymakers and academic researchers have studied how public policies, including taxes and subsidies, influence local entrepreneurship and business dynamics (Lerner, 2020).[1] Place-based tax incentives targeting certain regions have emerged as a significant policy instrument in regional economic development, but their effects on local private investments and entrepreneurial activity are unclear (Bartik, 2020). Previous literature generally finds that policy-induced increases in private investments positively impact firms' entrepreneurial activities, but concerns remain regarding the efficiency of tax-induced investments (Hall and Van Reenen, 2000). Specifically, when a region experiences capital inflows induced by place-based tax incentives, will local entrepreneurship surge because of relaxed financial constraints, or is there a possibility that capital inflows may benefit existing firms and hurt local entrepreneurship? This paper contributes to this debate by studying the impact of a recent place-based tax credit policy targeting low-income communities in the U.S., the Opportunity Zone program of 2017, and shows that despite a surge in local private investments, overall local new business formation declines, and there is negative real economic impact on local employment and market competition.

The Opportunity Zone program was introduced in 2017, and 8,762 out of 42,160 eligible census tracts, primarily low-income communities, were designated. The original purpose of

[1] Several states have implemented or are exploring tax credit programs aimed at supporting early-stage firms. For instance, Massachusetts has recently modified its corporate tax incentive program to prioritize early-stage companies that are not yet profitable (Boston Business Journal, April 2025). Additional examples include Maryland's Biotechnology Investment Incentive Tax Credit and New York's Life Sciences Research and Development Tax Credit Program.

the program was to encourage entrepreneurship and entrepreneurial financing in neighborhoods with high poverty (Eldar and Garber, 2022),[2] despite being broadly conceived as a policy to draw private investments to these neighborhoods for long-term economic growth and employment. Investors who reinvest their capital gains from out-of-zone businesses into in-zone businesses through Opportunity Zone Funds for a qualified period can enjoy tax deferral and tax benefits. More than $40 billion equity has been raised to invest in the Opportunity Zone program as of April 2025, according to Novogradac, making the program one of the largest-scale place-based policies in US history. Studying the impact of Opportunity Zones on local private investments and entrepreneurship is especially important because it takes a more "market-based" approach than previous place-based policies and involves minimal government intervention—only zone designation and tax incentives.[3] Therefore, there is no guarantee that investors will indeed fund new ventures in low-income areas that the policy targets.

To examine the policy's impact, I use a difference-in-differences (DiD) approach and compare census tracts designated as Opportunity Zones with those eligible but not designated before and after the policy's implementation from 2015 to 2019. I show that areas with lower income, higher poverty, and a greater fraction of minority groups were more likely to become Opportunity Zones, suggesting that the selection of the treated areas is, in general, aligned with the policy's target. Additionally, pre-policy growth in investments and entrepreneurship does not predict the designation of Opportunity Zones, alleviating the concerns about reverse causality.

Using novel datasets compiled from Form D filings and official business registration records

[2]US Senate Republican Policy Committee, "One significant handicap for these communities has been the lack of access to loans, grants, and venture capital needed to start or expand a small business. Opportunity zones were devised to address this gap."

[3]Previous place-based policies have more government intervention, such as selecting firms, providing infrastructure, and monitoring the employment or taxes of the region.

from state government registrars, I show that the tax incentives offered by the Opportunity Zone program experienced a 10.5% increase in the number of private investment deals and a 16.1% increase in the total dollar amount of investment in the treated economically distressed census tracts. Also, the increase in private investments was larger in older firms than in the newly formed companies. When examining the policy's impact on local entrepreneurship, I observe a significant 1.8% larger decline in the number of new firms formed in treated tracts following the designation, compared to non-treated tracts. The decline in entrepreneurship was concentrated in the non-tradable sector (e.g., grocery stores and restaurants), where most firms compete locally. In contrast, there was no significant impact on the formation of businesses in the tradable or construction sectors. The results support the interpretation that additional financial resources drawn by the policy help incumbents strengthen their market position, thus deterring potential entrepreneurs from entering.

In addition, the decrease in local new business formation was greater in Opportunity Zones with positive private investments during the sample period. Using the ZIP Codes Business Pattern data as an alternative measurement for local entrepreneurship, I confirm that there was a decrease in the net creation of establishments and find that the decline was mainly in firms with a smaller employment size. I also conduct a series of additional tests to confirm the robustness of the results. The estimation of the coefficient dynamics provides supportive evidence of parallel trends in the pre-shock periods. I perform a propensity score matching based on census tracts' observable characteristics and find that the findings are robust to using the matched sample. The results are also robust to excluding areas near colleges and universities. Similar estimates when splitting control group tracts based on their distances to an Opportunity Zone suggest a limited spillover effect from treated areas to nearby control areas. The results are also similar when

census tracts with headquarters or multiple branches of multinational companies are excluded, alleviating the concern that other contemporaneous corporate tax deductions provided by the TCJA drive the main findings.

Next, I examine the Opportunity Zone policy's real effects on the local economy. I find that total employment in Opportunity Zones decreased by 1.7% after the policy's implementation. The decline was greater in firms with higher risk profiles and less information available to investors, such as those that were newly formed and independent. I further show that market competition, as measured by the concentration ratios and the Herfindahl–Hirschman Index (HHI), in the non-tradable sector decreased significantly after the policy's implementation. In comparison, the tradable sector did not experience significant changes in local market competition. In addition, the likelihood of dissolution of older firms in the treated areas decreased compared to the control areas after the policy's implementation. The declines in local employment and market competition in Opportunity Zones underscore the critical role of start-ups and young firms in local job creation and economic growth, particularly in contrast to older, incumbent firms (Adelino et al., 2017; Haltiwanger et al., 2013). These results highlight the importance of studying the impact of the Opportunity Zone policy on local entrepreneurship.

Lastly, to better understand how tax-induced private investments could discourage local entrepreneurship and employment, I construct a stylized model where an investor decides to invest in existing or new local firms in the policy-targeted areas. The model predicts that in sectors where firms compete locally with limited information available on the quality and risk of local private firms (Ivković and Weisbenner, 2005; Seasholes and Zhu, 2010), the investment in existing firms increases much more than in new firms, thus discouraging local entrepreneurship. Furthermore, in cases where potential new businesses' ability to generate jobs is sufficiently

high compared to existing businesses, tax-induced capital inflows could negatively impact local employment.

With the increasing adoption of place-based tax incentives in the United States and globally (Bartik, 2020), this paper has important policy implications. With almost no restrictions imposed by the Opportunity Zone program on investment allocation, investors preferred existing firms over new ones, and this widened funding gap discouraged local new business formation. Hence, future policymakers must carefully consider the potential distributional effects when offering tax incentives across businesses with varying levels of risk. A potential improvement to investor tax credit policies could involve differentiating tax incentives based on firm risk profiles. For instance, programs might offer higher incentives to new businesses or non-real-estate firms, which are more likely to generate long-term local employment.

The rest of the paper is organized as follows. Section 2 discusses the related literature and this paper's contribution. Section 3 introduces the institutional background of the Opportunity Zone policy. Section 4 describes the data sources and variable construction. Section 5 shows the empirical strategy and results on the impact on local private investments and entrepreneurship. Section 6 examines the real economic impact of the policy on local employment and market competition, and discusses the economic mechanism. Section 7 discusses its policy implications and Section 8 concludes the paper.

2 Related Literature and Contribution

This paper contributes to several strands of literature. First, this paper builds on the literature related to the real effects of tax policies, specifically on the local economy. Previous papers have studied the impact of corporate tax and personal income tax on corporate decision-making

and risk-taking (Coles et al., 2022), R&D spending and innovation (Mukherjee et al., 2017; Wilson, 2009), establishments and employment (Giroud and Rauh, 2019). Regarding the effect of tax policies on entrepreneurial activity, the literature generally finds that lowering corporate or personal income taxes is positively related to becoming an entrepreneur (Cullen and Gordon, 2007; Curtis and Decker, 2018). Other studies show that providing capital gain credits increases business formation and investment in high-growth startups as well as affects IPO underpricing and proceeds (Chen and Farre-Mensa, 2025; Edwards and Todtenhaupt, 2020; Guenther and Willenborg, 1999). Denes et al. (2023) find that tax credits offered to angel investors increase investments toward entrepreneurial firms but do not have any positive impact on boosting high-growth entrepreneurship. My paper contributes to this literature by showing that tax incentives can, under certain circumstances, *negatively* affect local entrepreneurship through unevenly distributed investment inflows between established and newly-formed firms.

Second, this paper contributes to the literature surrounding the role of financial constraints as barriers to entrepreneurship and innovation (Cagetti and De Nardi, 2006; Evans and Jovanovic, 1989; Hurst and Lusardi, 2004).[4] Many government programs have been created to address market failures associated with entrepreneurial finance (Akcigit et al., 2022; Bayar et al., 2025; Hall, 2002; Hall and Lerner, 2010). This paper adds to this literature by studying the place-based tax incentives to potential investors in entrepreneurial firms. As the paper shows that new private investments brought in by the policy might favor existing businesses and discourage entrepreneurship under certain circumstances, it adds to the discussion on the efficiency of using tax instruments to promote entrepreneurship (Poterba, 1989).

[4]There is a large literature showing the empirical evidence of how financial constraints affect potential entrepreneurs' decisions, some examples include Bellon et al. (2021), Holtz-Eakin et al. (1994), Kerr and Nanda (2009), Krishnan et al. (2015), Robb and Robinson (2014), Schmalz et al. (2017).

Finally, this paper is related to the literature on the effects of place-based policies, as the Opportunity Zone program targets economically distressed areas in the US. Existing studies on such place-based policies have generated mixed findings regarding their impact on local economic growth and employment (see Austin et al. (2018) for a summary of the studies) but largely overlooked the impact on local entrepreneurship.[5] Since the Opportunity Zone program's implementation, studies have found the policy has either a positive or null effect on the aggregated local economy.[6] My paper contributes to this literature by showing that place-based tax incentives could have negative real economic impact on local entrepreneurship and employment through increased local investments.

3 Institutional Background

The Opportunity Zone policy was introduced under the Tax Cuts and Jobs Act (TCJA) and signed into law on December 22, 2017. This policy mainly aimed to provide tax incentives to potential investors to reinvest capital gains in economically distressed communities and boost local economic development in these communities. More than 8,700 census tracts were designated in the United States. Figure 1 shows the geographical distribution of the Opportunity Zones.

[Insert Figure 1 about here]

The Opportunity Zone policy differs from previous place-based policies introduced in the United States because the government plays a much smaller role. Previous place-based policies

[5] Tian and Xu (2022) study China's national high-tech zones, which provide subsidies to in-zone high-tech firms, and find that the policy positively affected local innovation and entrepreneurship.

[6] They find the policy has no significant impact on local housing prices (Chen et al., 2022), null impact on investments in commercial property investments and consumer spending (Corinth and Feldman, 2021), and positive impact on the growth rate of establishments and employment (Arefeva et al., 2024; Freedman et al., 2021).

usually involved heavy government efforts and interventions, such as selecting firms for grants or tax benefits and monitoring their use. Although place-based policies have cost about $60 billion annually (Bartik, 2020), studies have shown mixed findings regarding the policies' impact on local investment, employment, and economic growth (Busso et al., 2013; LaPoint and Sakabe, 2021; Neumark and Simpson, 2015).[7] The Opportunity Zone program is differentiated from most previous place-based policies by a more "market-based" approach as it has "no cap on participation and require[s] no government approval" (Council of Economic Advisers, 2021).

The Opportunity Zone concept was first proposed in 2015 by the Economic Innovation Group, a bipartisan public policy organization. In April 2016, the bill to create Opportunity Zones was introduced in the U.S. Senate and House and reintroduced in February 2017, but it did not get much attention. The passage of the TCJA at the end of 2017 finally created Opportunity Zones, after which the U.S. Department of the Treasury identified 42,160 eligible census tracts among the 74,134 census tracts in the United States. For a census tract to qualify for the designation, it had to be a "low-income community" (LIC) with either a poverty rate greater than 20% or a median household income less than 80% of the local median household income statewide, or the tract had to be contiguous to a LIC tract and have a relatively low household income. Governors could nominate up to 25% of a state's LIC census tracts for Opportunity Zone designation by March 21, 2018 (later extended to April 20, 2018). Despite showing political favoritism, as shown in Frank et al. (2022) and Eldar and Garber (2023), governors have generally chosen tracts that align with the policy objective, characterized by lower income

[7]The most comparable precedents are the federal Empowerment Zones (EZ) and the New Markets Tax Credit (NMTC), which offer certain tax incentives to companies located in a specific area. Compared to the Opportunity Zone program, the EZ tax incentives target investments, employment, and property development. The tax incentives are much smaller than in the Opportunity Zone program. NMTC also targets economically distressed communities, but the tax incentives provided are capped and offered to companies on a competitive basis rather than to investors.

and higher poverty levels, as shown in Table A9. Up to 5% of the total tracts nominated could be non-LIC, but they had to be contiguous to a nominated LIC tract. By June 2018, the U.S. Treasury Department had designated 8,762 census tracts as Opportunity Zones, of which 8,534 were LICs.

The Opportunity Zone program benefits those who invest capital gains received from out-of-zone businesses in designated census tracts by allowing them to defer taxes on the initial capital gain until 2026 or until the asset is sold. In addition, if the capital gain is invested for at least seven years (five years), Opportunity Zone investors can receive a reduction of 15% (10%) in the amount of prior capital gains tax. Finally, for investments held for more than ten years, investors will receive an increase in the tax basis that equals the fair market value upon sale, effectively eliminating the taxes due from new capital gains. To obtain the tax benefits, investors must invest capital gains in Qualified Opportunity Zone (QOZ) businesses through Qualified Opportunity Funds (QOF). QOZ businesses must have at least 50% of their gross income earnings from trade, businesses, or services conducted in an Opportunity Zone, and QOFs need to invest at least 90% of their assets in QOZ businesses. There are no other requirements for receiving tax benefits except for the investment period and geographical location. The first year for investments to qualify for participating in the Opportunity Zone program was 2018. The last year to invest to qualify for the 15% tax deduction was 2019, seven years before 2026. Investors who want to enjoy the capital gain tax exemption have to invest soon after the policy is in effect in 2018 to qualify for the ten-year requirement. The short eligible investment window explains why the effects shown later happened soon after the policy's implementation.

Ideally, anyone with capital gains may invest in Opportunity Zones. In practice, however, most QOFs have filed for an exemption with the Securities and Exchange Commission (SEC)

under Regulation D, Rule (b), and Rule (c), limiting their offerings mainly to accredited investors. In the sense that they obtain their funding mainly from accredited investors, QOFs are similar to other financial intermediaries in the private market, such as angel groups, venture capital (VC) funds, and private equity funds. On the other hand, there are several differences between QOFs and the above financial intermediaries. The first is the restriction on the geographical location: investments must be mainly (at least 90%) in firms located in economically distressed areas designated as Opportunity Zones, while other intermediaries can freely invest in companies all over the United States. The second difference is that investors need to invest their capital gains into a QOF within 180 days after their capital gains are triggered; QOFs are subject to the same restriction to invest their money into QOZ businesses within 180 days.[8] Other private funds, such as VC funds, do not have a specific deadline for finishing investment choices. Sorenson and Stuart (2001) show that VC firms begin investing one year after closing a fund and invest 80% of their committed capital within the first three years.

The Opportunity Zone program has attracted much attention from investors, and the dollar amount involved has been sizable. Congress's Joint Committee on Taxation estimated that the loss of federal revenue created by the Opportunity Zone program over ten years to be at least $1.6 billion annually.[9] As of April 2025, more than 2000 QOFs have been created with more than $40 billion equity raised since the passage of the law.[10]

[8]The Internal Revenue Service (IRS) relaxed the 180-day rules after the COVID outbreak in 2020; that was beyond the time scope of this study, which uses a sample period from 2015 to 2019, so there should be no effect on the results of this study.

[9]Joint Committee on Taxation, "Estimated Budget Effects of the Conference Agreement for H.R. 1, the 'Tax Cuts and Jobs Act,'" JCX-67-17, https://www.jct.gov/publications.html?func=startdown&id=5053.

[10]According to statistics from Novogradac, https://shorturl.at/OewzW.

4 Data

In this section, I describe the data sets used and variables constructed for the empirical analyses in the paper.

4.1 Data Sources and Variable Construction

4.1.1 Data on Private Investments

To confirm there is first-stage impact of the Opportunity Zone policy on local private investments, I collect data on private investments from SEC Form D filings. Historically, information on private investment has been hard to observe, and researchers have recently started using Form D filings to analyze private investments (Xu, 2023). Federal securities laws require firms that raise capital through private placements to file a Form D, a notice of exemption for security offerings, with the SEC. Form D filings track information such as the name, location, industry, incorporation year of the filing firm, and the date and total offering amount of each filing. Firms are required to file Form D within 15 days after the first sale of securities in the offering. Failure to file a Form D may incur consequences such as being prohibited from future private investments and may constitute a felony.[11] Both the invested firms and Opportunity Zone funds are incentivized to file a Form D with the SEC to demonstrate their qualification for tax incentives

[11] See more details from the guidance issued by SEC on Regulation D, Rule 506 and the case of *Hamby v. Clearwater Consulting Concepts*.

(Atkinson, 2019). Internet Appendix D shows a sample Form D.[12]

Using information from Form D filings, I construct two variables measuring local private investments in a census tract and a given year. I first geocode the company addresses disclosed in Form D to find the census tract where the company is located. Then, I aggregate the number of private investment deals and the dollar amount in each census tract in each year to construct the two variables: the number of private investment deals (*Num_Inv*) and the dollar amount of private investment deals (*Amount_Inv*). To further break down the impact of the policy on local private investments across sectors, I categorize all the investment deals into three sectors, finance, real estate, and business, based on the industry information provided in Form D.[13]

4.1.2 Data on Business Registration

New business formation is the main outcome of interest in this paper and is measured by using official business registration filings with state governments. Using official registration to measure entrepreneurship is more comprehensive and timely compared to using establishment counts provided by other commercial databases (Engelberg et al., 2026; Guzman and Stern, 2015). The business registration data are collected by *OpenCorporates* and contain information including

[12]Other contemporaneous papers have analyzed the impact of the Opportunity Zone policy related to private investments. Corinth and Feldman (2021) use data from Real Capital Analytics and show the policy has null effects on local commercial real estate investment. Kennedy and Wheeler (2021) and Coyne and Johnson (2022) use electronic business tax filings in 2019 (and 2020) and find that OZ investments are concentrated spatially and in the real estate sector. Instead, my paper uses SEC Form D filings, the largest publicly available data set tracking investment records in all industries across the US, and covers years before the implementation of the policy to facilitate a difference-in-difference analysis. I have also used a data set that compiles additional data on private investments from VentureXpert and Crunchbase; I find similar results on the policy's impact on local private investments (and the results are available upon request). This is also consistent with the finding of Eldar and Garber (2022) that there is no significant change in startup-related investments using VentureXpert data. I report results only using data from Form D filings in the paper for consistency and simplicity.

[13]Item 4 in Form D provides the issuer's industry group. I label firms in the "Banking and Financial Services" industry group as the "Finance" sector, "Real Estate" as another sector, and all other industry groups as the "Business" sector. In Table A5, I show that all types of investments increased in Opportunity Zones compared to other eligible census tracts after the policy implementation, addressing the "double-counting" concern related to investment management companies.

the company name, address, type, dates of incorporation, and dissolution (if applicable). Recent studies have used data from *OpenCorporates* to obtain information for private firms, such as the incorporation date and active status (Ewens and Farre-Mensa, 2020). Following Engelberg et al. (2026), I focus on for-profit firms as they consist of more than 95% new businesses formed and are more economically relevant.

I first geocode the company addresses using the Census Geocoder API to obtain the census tract code.[14] I then aggregate the number of new for-profit companies incorporated in each census tract by their incorporation year to construct the variable, *Num_NewFirm*. *OpenCorporates* does not provide business registration records in Delaware, Illinois, or Puerto Rico due to limited accessibility of those state governments' websites; therefore, the census tracts for these states are excluded from the analysis of local business formation.

To analyze which sectors were most affected by the decline in new business formation, I classify industries into four categories—tradable, non-tradable, construction, and other—based on their reliance on local conditions, consistent with the methodology of Mian and Sufi (2014). A data limitation is that the business registration record data does not include standard industry codes (e.g., NAICS or SIC). To overcome this, I use a machine learning model to classify firms into these sectors. The full technical details of this approach are presented in the Internet Appendix Section C.

[14] When more than one address is provided for a company, I use the business address instead of the mailing address because Opportunity Zone policy requires that real businesses operate physically within a census tract. For the same reason, I exclude a firm from the sample when it has only one address and the address is associated with a post office box.

4.1.3 Data on Establishment-Level Employment and Sales

To study the real economic impact, I use the establishment-level employment and sales data from the National Establishment Time-Series (NETS).[15] Like the U.S. Census Bureau's Longitudinal Business Database (LBD), the NETS database, built by *Duns and Bradstreet* and *Walls & Associates*, aims to track the entire universe of firm establishments in the U.S. Several studies have used the NETS database to examine firm performance, especially for private firms and startups (Farre-Mensa et al., 2020; Neumark et al., 2011). From NETS, I obtain the information of an establishment's name, industry code, address, first year and last year of operation, annual employment and sales, and business ownership type (whether it is a subsidiary of another company or a standalone firm).

To examine the policy's real impact on the local economy, I aggregate the total employment and sales in a census tract in a year using the NETS data. To study local market competition, I construct local market concentration measures, including the concentration ratios of the top three or top five players (*CR3* and *CR5*) and the Herfindahl–Hirschman index (*HHI*), based on the employment and sales by firms in a census tract in a year.

Note that, like most databases provided by the Census Bureau, imputations are used in the missing employment and sales data for a fraction of businesses in the NETS database.[16] Barnatchez et al. (2017) do not find systematic location-specific patterns for these imputations, which might have led to biases in the estimation. Neumark et al. (2007) validate the NETS employment data against the Census's microdata and find high correlations between NETS,

[15] Note that the main outcome variable, new business formation, is constructed using official business registration records and does not rely on the NETS data.

[16] Imputation is a common practice used in most census data sets as well, including the widely-used Current Employment Survey (CES), Quarterly Census of Employment and Wages (QCEW), and the Quarterly Workforce Indicators based on the Longitudinal Employer-Household Dynamics (LEHD) Program (Eckert et al., 2020).

CES, and QCEW at the county-by-industry level (0.99 and 0.95, respectively). Nevertheless, to alleviate the concern, I rerun the tests only using the non-imputed employment data following Denes et al. (2023) and obtain similar results as shown in Table A14 in the Internet Appendix.

4.1.4 Control Variables and Other Measures for Local Economic Conditions

To control for changes in local demographic and economic conditions, I include the lagged-one-year natural logarithm of the population (*Population*), the natural logarithm of median income (*Median_Income*), the natural logarithm of median age (*Median_Age*), the percentage of white people alone (*%White*), the percentage of black people alone (*%Black*),[17] the poverty rate (*Poverty_Rate*), unemployment rate (*Unemp_Rate*), and the percentage of people without a high school diploma (*%NoHighSchool*). The outcome variables for each census tract are in year *t*, while the control variables are lagged one year as *t*-1. The data for the control variables are from the American Community Survey (ACS).

To examine whether the policy has led to changes in geographical mobility, I use the total number of people who move into census tracts in a year from the ACS. I group the immigrants in a census tract by their education level and poverty status to further break down the mobility composition. I collect the tract-level and county-level Gini coefficient from the ACS to measure local income inequality.

4.2 Summary Statistics

Summary statistics are reported in Table 1. In the sample, there are 42,171 census tracts eligible for the Opportunity Zone designation; among these, 31,859 are low-income communities (LICs),

[17] ACS categorizes population into six races: White, Black or African American, American Indian or Alaska Native, Asian, and Native Hawaiian or Other Pacific Islander, and Some Other Race.

while the rest are contiguous, non-LIC tracts. The sample includes 8,761 census tracts designated as Opportunity Zones, of which 8,531 are LICs.[18] For most analyses in the paper, I include only the LIC tracts to make the treated and control groups more comparable. The main findings are all robust if non-LIC contiguous tracts are included. The sample period is from 2015 through 2019. All tract-level amount variables are winsorized at the 1st and 99th percentiles to avoid data errors involving extreme values that may drive the results.

[Insert Table 1 about here]

As shown in Table 1, 24.7% of the tracts are Opportunity Zones, while the remaining tracts are eligible but non-designated. On average, each year, a census tract has 0.12 private investments that average $2.732 million. In Table A in the Internet Appendix, I report more details on the age and geographical distributions of private investment deals. An average census tract in the sample has about 11 new firms incorporated each year and has 4,046 people with a median income of $39,156, a poverty rate of 22%, a population that is 61% white and 23% black, an unemployment rate of 11%, and 21% without a high school diploma.

5 Effects on Local Private Investments and Entrepreneurship

Using the Opportunity Zone policy as a quasi-natural experiment, I run a difference-in-differences (DiD) approach to identify the policy's impact on local private investments and entrepreneurship. In the baseline regressions, I estimate the following equation:

$$Y_{i,t} = \alpha + \beta OZ_i * Post_t + Controls_{i,t-1} + \delta_t + \eta_i + \epsilon_{i,t}. \quad (1)$$

[18] Due to data availability for business registration records, as mentioned in Section 4.1.2, the number of treated tracts with business formation information decreases to 8,179.

where *i* is a census tract and *t* represents a year. *OZ* is an indicator that takes a value of one if the tract was designated as an Opportunity Zone and zero if it was eligible but not designated. *Post* is a dummy that equals zero before 2018 and one afterward. To control for local demographic and economic characteristics, I include the natural logarithm of the population, the natural logarithm of the median income, the natural logarithm of the median age, the poverty rate, the percentage of white or black people, the percentage of the population without a high school diploma, and the unemployment rate of census tract *i* in year *t-1*. To account for unobservable location-specific characteristics and time-specific trends, the DiD model includes census-tract fixed and year fixed effects.[19] I cluster standard errors by census tract.[20]

5.1 Impact on Local Private Investments

I start the empirical analysis by examining the impact of the Opportunity Zone policy on local private investments. Columns (1) to (4) in Table 2 report the ordinary least squares (OLS) regressions. The dependent variables are the natural logarithm of one plus the number of private investment deals in census tract *i* and year *t*, *Ln(Num_Inv+1)* and the natural logarithm of one plus the dollar amount of private investments, *Ln(Amount_Inv+1)*. The coefficient estimates on $OZ * Post$ are all positive and significant at the 1% level, either with or without control variables. The magnitude of the coefficient estimates suggests that the effects are also economically sizable: after the policy shock, the number and amount of private investments that flowed into treated tracts (the Opportunity Zones) increased by 10.5% and 16.1%, respectively, compared to tracts

[19] Table A2 in the Internet Appendix shows that the results are robust under alternative fixed effects, county and year fixed effects, or state-by-year fixed effects to control for state-level time-specific shocks.

[20] The main findings are robust when I use the alternative clustering options as shown in Table A3 in the Internet Appendix.

that were eligible but not designated.[21] Following Cohn et al. (2022), I report the results of Poisson estimation in Table A4 in the Internet Appendix and find positive and statistically significant coefficient estimates on $OZ * Post$. When breaking down the impact on local private investments into three categories—finance, real estate, and business—based on the "industry" information in the Form D filings as shown in Table A5, I find significant and positive impact on private investments across all three sectors, with the largest effect on the business sector.[22]

[Insert Table 2 about here]

To further analyze the impact on local investments, I examine how firm age influences investment patterns. As outlined in the model (Section 6.3), Opportunity Zone investors are likely to favor existing firms relative to new firms, likely due to their lower risk and more information available. To test this hypothesis, I categorize firms based on whether they have been operational for at least one year and compare the local private investments received by each group. The one-year threshold captures the "up or out" dynamic among new firms (Wiens and Jackson, 2015), as evidenced by Bureau of Labor Statistics data showing that roughly 25% of new firms fail within their first year, with exit rates declining thereafter.

The results are shown in Table 3 where the dependent variables are the number of investment deals (*Ln(Num+1)*) and the dollar amount of deals (*Ln(Amount+1)*) with other empirical specification similar to Equation (1). The first two columns in Table 3 suggest that the Opportunity Zone policy has invited more private investment deals for older firms than newly-formed

[21] The economic magnitude of the number of deals is calculated as follows, given that the mean of *Num_Inv* is 0.118 and the coefficient estimate is 0.011 (both close to zero): 10.5% = $\frac{0.118+1}{0.118} * (e^{0.011} - 1)$. The magnitude of the dollar amount of deals, *Amount_Inv*, is 16.1% = $\frac{2.732*10^6+1}{2.732*10^6} * (e^{0.149} - 1)$.

[22] This by-sector analysis also addresses the "double counting" concern that some of the investment management companies located in Opportunity Zones might have received investments from outside investors and then made investments to other firms located inside the Zones, even though these investments would not be qualified for OZ tax benefits as the policy requires that capital gains must come from out-of-zone businesses.

firms. The difference between these two types of firms is statistically significant at the 1% level (*z-stat* being 3.578). Similarly, the dollar amount of investment increased for both older and newly formed firms, but the increase was significantly larger for the older ones in the Opportunity Zones than for the newly formed ones. It is worth noting that the increase in investment amount is greater than that in deal count, suggesting investors were deploying large sums into fewer firms. This pattern could be driven by the tight timeline to meet the qualification requirements and is consistent with the interpretation that investment flows favored existing firms.

Table 3 presents the results, where the dependent variables are the number of investment deals (*Ln(Num+1)*) and the total dollar amount (*Ln(Amount+1)*), using specifications consistent with Equation (1). Columns (1) and (2) indicate that the Opportunity Zone policy attracted significantly more private investment deals to older firms compared to newly formed ones and the difference is statistically significant at the 1% level (*z-stat* = 3.578). A similar divergence appears in investment amount, while both groups experienced an increase, the capital injection into older incumbents was substantially larger than that into new firms. Notably, the response in investment amount is much stronger than that in the investment count, suggesting that investors are investing more money into fewer firms. This pattern is consistent with the program's tight qualification timeline, which likely incentivized investors to prioritize "shovel-ready" projects in established firms over new ventures that require longer lead times.

[Insert Table 3 about here]

The above findings show that the policy had a distributional effect on private investments between incumbents and newly formed firms. Despite bringing more private investments into economically distressed communities, incumbents benefited significantly more than newly formed firms.

5.2 Impact on Local Entrepreneurship

After verifying that the Opportunity Zone policy has indeed increased private investments in the treated census tracts, I next examine its impact on local entrepreneurship, measured by new business formation. Table 2 Columns (5) and (6) report the results where the dependent variable is the total number of new businesses registered in census tract *i* and year *t* (*Ln(Num_NewFirm+1)*), without and with controls, respectively. The coefficient estimate on $OZ * Post$ is negative and statistically significant at the 1% significance level. This suggests that, after the introduction of the policy, new business formation declined an average of 2.1% more in census tracts designated as Opportunity Zones, compared to other eligible but non-designated tracts.[23] Poisson regression also reports a significantly negative estimate, as shown in Internet Appendix Table A4 Column (3).

Next, I test whether there is a decline in local new business formation in sectors with greater local competition. The intuition is that as investors allocate more capital to existing, older firms in the treated areas, the potentially widened gap in financial resources would discourage a potential entrepreneur from entering the market in sectors that compete mostly locally.

[Insert Table 4 about here]

Using the industry classification from Mian and Sufi (2014), I divide sectors into non-tradable (e.g., restaurants, grocery stores, and retail services), tradable (e.g., manufacturing and wholesale), construction, and other. Intuitively, firms in the non-tradable sector mostly compete locally. Table 4 presents the effect of the policy on the formation of new businesses in these sectors. I observe that the coefficient estimate on $OZ * Post$ is negative and statistically

[23] The magnitude of new firms is calculated as follows, given that the mean of *Num_NewFirm* is 10.158 and the coefficient estimate is 0.019: 2.1% = $\frac{10.158+1}{10.158} * (e^{0.019} - 1)$.

significant for the non-tradable and other sectors, while it is insignificant for the tradable and construction sectors. This finding suggests that the Opportunity Zone policy could help incumbent firms gain financial resources and maintain their competitive advantages—such as store renovations, as shown by Sage et al. (2023), who find that commercial property prices rose significantly in treated areas where upgrades were needed. Potential entrepreneurs, particularly in non-tradable sectors, may avoid starting businesses if they anticipate being unable to match the financial capacity of incumbent firms.[24] Consistent with this, I later show that local employment decreased in Opportunity Zones, suggesting that private investments were directed toward non-labor expenditures like renovations rather than job creation.

Also, one should expect the decline in new business formation to be greater in Opportunity Zones that have received private investments than those that have not, compared to non-treated census tracts. Table A7 confirms the above conjecture that the magnitude of the coefficient on $OZ * Post$ in a sample restricted to only positive-investments Opportunity Zones is about twice that in the baseline regressions.

Finally, I validate the entrepreneurship finding using data from the ZIP Codes Business Patterns (ZBP) provided by the Census Bureau. Instead of counting the number of firms registered, the ZBP database provides statistics on the total number of establishments in a zip code. I calculate the changes in the number of establishments as a proxy for local net creation of businesses. As shown in Column (1) in Table A6 in the Internet Appendix, the Opportunity Zone policy had a negative net effect on the creation of local businesses, confirming the previous finding using *OpenCorporates* data. The ZBP data also provides the number of employees of

[24] Additionally, the specific design of the policy likely also imposed a supply-side constraint on new entry. The tight timeline for investors to qualify for tax benefits limited the window of opportunity for potential entrepreneurs to organize and register new qualifying businesses in time to attract this capital.

established businesses. The decrease in the net creation of local establishments was mainly among smaller firms (with fewer than ten employees or with 10 to 50 employees) but not in large ones (with more than 50 employees).

5.3 Identification Assumptions and Robustness Tests

The DiD approach compares outcome variables before and after the policy between designated census tracts and eligible but non-designated tracts. This identification strategy relies on two main assumptions. First, it assumes that changes in private investments and entrepreneurship would have been the same across the treated and control areas, absent the Opportunity Zone policy change (i.e., the parallel trend assumption). Second, it assumes that the Opportunity Zone policy was not determined based on the level and growth of local private investments and new business formation before the policy.

I take several steps to verify these assumptions and address potential concerns. One concern is that the selection of Opportunity Zones was not random and that outcome variables such as private investments may have evolved differently between designated and non-designated tracts in the absence of the policy. To address this concern, I plot the coefficient estimates around the introduction of the Opportunity Zone policy at the end of 2017. As shown in Figure 2, local private investments and entrepreneurship did not diverge before the policy's introduction, as the 95% confidence intervals all cover zero.[25] The difference between the treated and control tracts started to enlarge significantly only after the policy was introduced. The figure provides support for the parallel trend assumption required by the DiD approach.

[Insert Figure 2 about here]

[25]To facilitate the interpretation of the coefficient estimates, I use semi-annual frequency in the plotting instead of the annual frequency. Annual plots do not show a pre-trend in the outcomes either.

Another concern is related to the differences in characteristics between the designated and non-designated tracts. Even though the DiD approach requires only pre-trends to be similar instead of the levels, some may still worry that non-balanced covariates may threaten the parallel trend assumption. To address this concern, I include tract-level lagged-one-year control variables, including population, median income, median age, percentage of white or black population, poverty rate, and unemployment rate in the baseline regressions.

I also perform a propensity score matching (PSM) on pre-treatment characteristics and keep the control tract with the highest propensity score within the same county as the treated tract (an Opportunity Zone) as its matched control. I add state dummies to control the political affiliation of state governors and their potential impact on the selection of tracts. To further address the concern that the variation in political ideology drove the difference in private investments between treated and control, Table A8 shows the sub-sample tests of splitting the sample census tracts by whether the Democratic or Republican party won their located states in the 2016 presidential election, and no significantly different impact of the policy were found between the two groups of tracts.

Column (1) of Table A9 presents the results of running the Logit regression in the full sample to produce the propensity scores. The results show that the policy was successful in general at targeting areas in need: census tracts with lower income, higher poverty, and a greater percentage of black people were more likely to become Opportunity Zones, alleviating the concern that the increase in private investments was driven by governors' favoritism toward areas that had already been gentrified.[26] Column (2) shows the Logit regression results using

[26]The higher percentage of white people residing in census tracts, compared to the base group consisting mostly of Asian residents, predicts the higher likelihood of a tract being selected as an OZ; this can be explained by the inclusion of poverty areas, such as the rust belt, where there are more white people with low incomes.

the matched sample, and one sees that the observation numbers decreased to 15,210.[27] The independent variables lost significance in the matched sample, and the pseudo R^2 decreased from 0.0558 to 0.0011, suggesting that the pre-shock characteristics are comparable in the matched sample and not likely to explain the selection of the Opportunity Zone in the matched sample. Table A10 shows the DiD results with a sub-sample of matched pairs from PSM, and the main findings are robust in the matched sample.

Some critics of the policy argue that certain census tracts designated as Opportunity Zones are close to colleges and universities where students are considered low-income residents. Thus, I conduct a robustness test by excluding census tracts with colleges and universities. The campus address and population information are collected from Homeland Infrastructure Foundation-Level Data. The results in Table A11 in the Internet Appendix show that the previous results are not driven purely by treated census tracts that have or are close to a campus of colleges and universities, thereby alleviating the concern that the economic characteristics of college towns and students might affect the level of investment in Opportunity Zones.

Concerns may arise regarding whether private investments were influenced by other tax reform measures enacted simultaneously with the TCJA, particularly the tax deductions for multinational corporations (MNCs). However, it is unlikely that other provisions of the TCJA would confound the results presented in this paper. The primary benefit of Opportunity Zones stems from capital gains tax relief for investors, whereas other TCJA provisions target corporate expenses and profits, which are less likely to directly affect distressed areas like Opportunity Zones. Moreover, as demonstrated by Albertus et al. (2025), there was no significant response

[27] As mentioned in Section 4.1.2, the information on business registration is not available for a few states, and that explains the smaller number of tracts in the PSM analysis than in Table 2 Columns 1 to 4.

in US firms' investment and employment due to increased access to inexpensive capital, further mitigating concerns about confounding effects in this context. To alleviate the specific concern about tax deductions for MNCs, I conduct a robustness test by excluding census tracts containing headquarters or at least twenty subsidiaries of any MNC. A firm is classified as multinational in a given year if its pretax foreign income (PIFO) or foreign tax expenses (TXFO) are non-missing and greater than zero, following the approach in Lampenius et al. (2021). The coefficient estimates on *OZ*Post* in Table A12 remain statistically significant, suggesting that the original findings were not substantially impacted by tax reform provisions of the TCJA specifically benefiting MNCs.

Finally, I examine whether the distance from a control tract to a treated tract affects the results. The analysis aims to determine whether the policy had any spillover effects on local investments and entrepreneurship in nearby control tracts, which could potentially bias the estimation. I split the control tracts, eligible but non-designated as OZs, based on whether there is an Opportunity Zone within 3 kilometers and re-run the baseline regressions. Table A13 shows that the baseline results are robust regardless of whether benchmarking against control groups closer to a treated census tract or not. This suggests little spill-over effect on local private investments or entrepreneurship.

The above analyses confirm that the Opportunity Zone policy is a plausible quasi-natural shock to local private investments: The policy was effective in introducing private investments to treated areas that otherwise would not have happened. Some may question why the effect showed up quickly after the policy implementation and the importance of the effect if it was only short-term. Two points worth discussing when interpreting this result. First, the immediate decrease in new firms in 2018 should not be surprising because the law requires investors to

reinvest their capital gains within 180 days after realization. Second, the negative effect, which was greater in 2018 compared to 2019, may be due to the timeline of the tax benefits offered: the latest time to max out all the tax benefits provided by the policy is 2018. Without the pandemic in 2020, the negative impact may have faded over the years, given that investors would have had more time to do due diligence on their investment targets. In this case, their preference for older businesses, as discussed in the following section, might diminish. Although the policy's negative effect on local new business formation might be relatively short-term, decreases in local business formation could significantly impact local market competition and employment, as I will show in the following sections of the paper.

6 Real Economic Impact: Employment, Sales, and Market Competition

Thus far, the analyses have shown that despite increased private investments in the treated areas, there was a decline in the formation of new businesses. However, it raises the question: if the policy has potentially bolstered the local economy in other, arguably more pivotal economic dimensions, should readers care about the decline in new business formation? In this section, I extend the examination to assess the policy's impact on broader economic indicators. I show that declines in local entrepreneurship are associated with decreased employment levels and market competition.

6.1 Reduced Employment and Sales

Table 5 presents the result of examining how the Opportunity Zone policy affects local employment. Panel A Column (1) shows that local employment decreased by 1.7% in the treated areas after the policy and the effect is statistically significant at the 1% level. When breaking down by

firm age (i.e., newly-formed or more than one year) and ownership type (i.e., whether it is a standalone firm or a subsidiary of the parent company), the total employment by *Age≤1&Standalone* firms decreased significantly by 4.1% and the other type of firms also decreased significantly by 1.6%. However, the total employment for *Age<1&Subsidiary* firms that have the most information available to investors and the lowest risk did not change significantly. When replacing the dependent variable with sales generated by local firms, I observe similar findings that the total sales also declined significantly. The only types of firms that survived this decrease were those older than one year and subsidiaries.[28]

[Insert Table 5 about here]

To validate that the decline in new business formation is the main driver of the observed decrease in aggregate employment, I first examine whether there is indeed a large gap between new firms and existing firms in job creation in Opportunity Zones. New entrants create, on average, 11.827 jobs per firm, whereas incumbents create only 0.048 jobs per firm. This confirms that new firms are the primary engine of local job growth in these distressed neighborhoods. Using these statistics, I then perform a back-of-the-envelope calculation to test the mechanism. Given that new firms constitute approximately 8% of total local establishments, and the policy reduced new business formation by 1.8% (based on the coefficient of -0.018 in Table 2), the predicted impact on aggregate employment driven by this decline in new firms is approximately $0.08 \times (-0.018) \times 11.827 \approx -1.703\%$. This calculated magnitude matches the actual aggregate employment decline of 1.7% observed in Panel A of Table 5 (the coefficient on $OZ * Post$ being -0.017). This result supports the hypothesis that the aggregate employment loss was driven by

[28]Tables 5 and 7 use data from the NETS database. To alleviate the concerns about the data imputations in the NETS database as discussed in Section 4.1.3, I repeat these analyses using only non-imputed employment data in Table A14. The estimation results using non-imputed data provide similar conclusions.

the "missing generation" of high-growth new firms, which incumbents failed to replace.

To explain why incumbents failed to offset this job loss, I draw on studies showing that these established firms directed tax-advantaged capital toward non-employment-related assets and property renovations rather than expanding their workforce. Specifically, Freedman et al. (2021) find that the policy's impact on the average earnings of local residents is statistically indistinguishable from zero, while Sage et al. (2023) document an increase in local commercial property prices for assets requiring renovation in the treated areas. Combining these findings with the negative employment results from my analysis suggests that the tax-induced capital inflows were likely spent toward physical assets rather than labor.[29]

To further validate that the decline in entrepreneurship and employment is driven by incumbent crowding-out in locally competitive markets, I examine whether the policy's impact varies based on the region's industrial composition. If the negative real effects indeed stem from incumbents edging out new firms through local competition, one would expect these effects to be mitigated in regions dominated by tradable sectors, where demand is not constrained by the local market and firms do not compete locally.

[Insert Table 6 about here]

Table 6 reports the results of estimating the baseline difference-in-differences model in two subsamples: census tracts where the tradable sector is the largest employer sector and accounts

[29]My finding of the Opportunity Zone policy's negative effect on the level of local employment contributes to the ongoing discussion evaluating the policy's impact on job creation. Other papers examining the impact of the policy have shown either null effects or positive effects. Atkins et al. (2023) and Freedman et al. (2021) find little or no effect on employment using resident-level data (place of residence). Combining their findings with my results, which rely on establishment-level data (place of work), suggests that the decline in local employment was likely concentrated among workers who commute into the Opportunity Zones from surrounding areas, rather than among the residents of the zones themselves. Arefeva et al. (2024) shows that the growth rate in employment increases as a result of the policy. This could be driven by the fact that the impact on local employment follows a concave pattern: while the overall level decreases, the growth rate actually increases.

for more than 20% of the workforce ("Tradable-Dominant"), and those where the tradable sector is not the dominant sector. As shown in Columns (1) and (3), in tradable-dominant tracts, the coefficients for both new business formation and employment are statistically indistinguishable from zero. On the contrary, the negative and statistically significant impact on both new business formation and total employment is concentrated mainly in tracts where the tradable sector is not dominant. This analysis confirms that the crowding-out of entrepreneurship and employment is specific to markets where firms compete locally.

6.2 Decreased Market Competition and Exits for Incumbents

Next, I examine whether and how market competition and firm exits changed after the policy's implementation.

First, I study the Opportunity Zone policy's impact on local market competition. In Table 7 Panel A, the dependent variables are the concentration ratios of the top three and top five employers in a census tract in a year (*CR3* and *CR5*) as well as the Herfindahl-Hirschman Index (HHI) based on employment and use them as dependent variables in different sectors.[30] One would expect to see that market competition would decrease significantly in the non-tradable sector, where most of the decline in business formation is observed, and see little impact in the tradable sector. This is the case shown in Table 7: The coefficient estimates on $OZ * Post$ are all positive and significant at a 1% significance level for the non-tradable sector, and I do not observe significance in the tradable sector. In Panel B, I use sales instead of employment to construct the above three measures. The results are similar to those in Panel A.

[Insert Table 7 about here]

[30] Here, the measures are based on a tract-sector level, as using a more detailed classification like the three-digit NAICS code could result in insufficient data due to a limited number of businesses.

Second, I examine whether the policy affected incumbents' dissolution (i.e., firm exit). The logic is that increased private investments in incumbents may help their survival. In Table 8, the dependent variable is the natural logarithm of the number of firms that dissolve in a census tract each year. In column (1), the coefficient on *OZ*Post* is negative and insignificant, suggesting that the total number of companies going out of business did not change significantly. However, as shown in Columns (2) and (3), there was a significant decrease in the number of firms that were at least one or two years old when they dissolved. The results suggest that older incumbents in the treated areas were more likely to survive (less likely to go out of business) after implementing the policy. The finding is consistent with the hypothesis that the additional financing resources, brought in by the tax incentives, helped incumbents build up their competitive advantage over potential newcomers and reduced local competition.

[Insert Table 8 about here]

Overall, the results in this section show that the Opportunity Zone policy has negatively impacted local employment and market competition. The negative economic effects highlight the importance of the previous findings in the paper that the Opportunity Zone policy negatively impacted the formation of local businesses.[31]

6.3 Stylized Model: Place-Based Tax Incentives, Business Formation, and Employment

To understand why tax-induced investments negatively affect local business formation and employment, I present a stylized framework. Rather than attempting to capture every institutional detail of the Opportunity Zone program, the model isolates a specific economic channel, the tension between funding established incumbents versus riskier new ventures. While this framework

[31] One caveat when interpreting the results is that the potential "positive" effect of the policy may be yet to be shown, given that the sample period ends two years after the policy's implementation. However, the results at least suggest that the policy was not very successful in improving local economic conditions in the short run.

provides useful intuition for the observed displacement effects, I acknowledge it may not be the only mechanism at work. Other institutional factors, such as the program's short qualification window, likely operated alongside this channel to discourage firm entry. The full model and proofs are in the Internet Appendix B. A brief summary of the model predictions is discussed below.[32]

The model explores how an investor (i.e., a Qualified Opportunity Fund (QOF)) allocates capital between existing firms and new firms within an Opportunity Zone. Most of the firms located in these low-income and high-poverty neighborhoods are private companies with severe information asymmetry between owners and investors (Leland and Pyle, 1977) due to limited availability of information and uncertainty (Ivković and Weisbenner, 2005; Seasholes and Zhu, 2010). One important observable factor related to the risk of failure that investors would consider is the company's age (Dunne et al., 1989). The investor faces a trade-off between the safety of investing in established companies, which guarantees a fixed return, and the higher but riskier potential returns from investing in new firms conditional on success. The model incorporates two key features to mirror the conditions in Opportunity Zones. First, the expected profitability of new ventures being successful is constrained by a scarcity of high-quality entrepreneurial projects. As more capital is invested in the distressed tracts, the marginal probability of success diminishes, reflecting a limited supply of productive local opportunities. Second, local competition from better-financed existing firms further reduces the likelihood of success for new firms, with the effect being particularly pronounced in non-tradable sectors, where local competition is more intense.

[32] An analogy of this model in consumer theory is the classic concept of "inferior goods," where the increase in a consumer's income decreases their consumption of the inferior good. In my context, the increase in an investor's fund size decreases its investment in local entrepreneurship for a similar mechanism, as I explain below.

The model shows that the investor's optimal investment in new firms depends on the size of the available fund (K). When the fund size is small, the investor directs almost all capital toward new firms because the marginal success probability remains high. However, as the fund size grows, competition between firms intensifies, and the optimal investment in new firms decreases. If the fund becomes too large, the investor eventually allocates all the capital to existing firms, as the competitive pressure makes investments in new firms unprofitable. The model predicts that in sectors with higher local competition, such as restaurants and grocery stores in the non-tradable sector, the Opportunity Zone policy can have a negative impact on new business formation.

The model also suggests that employment outcomes are tied to the allocation of capital between existing and new firms. New firms are generally more effective in creating jobs than established firms and if the gap in job creation potential between new and old firms is large enough, an increase in the fund size could lead to a decrease in total employment. This occurs because the investor increasingly favors established firms, which contribute less to employment, as the fund grows. Thus, the model predicts that the Opportunity Zone policy could reduce employment if new firms' job creation advantage is sufficiently large.

7 Policy Implications

The findings of this paper offer important insights for the design of future place-based interventions, particularly as large-scale programs such as the CHIPS and Science Act of 2022 continue to deploy capital into targeted regions. The policy implications of the article are as follows.

First, policymakers could consider differentiating tax incentives based on firm age and local industrial composition to mitigate the distributional distortions observed in this study. Because

investors naturally favor safer, information-rich incumbents, simply inviting more capital inflow could widen the funding gap between existing firms and newcomers, particularly in industries that are sensitive to local competition like the non-tradable sector. Future policies could consider offering higher tax credits for investments in newly formed firms, ensuring that capital fosters new market entry rather than reinforcing the position of established businesses.

Second, the government needs to carefully choose the policy goal for place-based incentives that would promote economic activities like entrepreneurship and innovation while also ensuring long-term employment gains. The Opportunity Zone program aimed to attract capital inflows to economically distressed areas but imposed no employment requirements. As a result, capital flowed mostly toward physical assets and renovations that benefited incumbents but did not translate into job creation. To ensure broader economic gains, future tax credits could be made conditional on net job creation, ensuring that the subsidized capital complements rather than substitutes for labor.

Third, policymakers should be aware that the design of implementation timelines could significantly impact capital allocation efficiency. The Opportunity Zone program imposed tight deadlines for realizing capital gains and deploying funds, which likely exacerbated investors' preference toward incumbents rather than new businesses. Future policies should provide longer qualification windows to allow investors sufficient time to screen and identify high-potential new firms, thereby reducing the bias toward "shovel-ready" incumbents.

8 Conclusion

This paper studies how place-based tax incentives affect local investments and new business formation. Using the recent place-based tax credit policy, the Opportunity Zone program, as a

quasi-natural experiment, I show that the policy effectively attracted more private investments to the economically distressed neighborhoods that the policy targeted. However, the number of new business formations declined significantly in the local area. I further show that the inflow of tax-advantaged capital disproportionately benefited older, incumbent firms. As these better-financed incumbents compete with potential entrants for finite local resources and market share, the policy ultimately stifled local entrepreneurship, especially in the non-tradable sector. Furthermore, the program's tight qualification timeline likely exacerbated this dynamic by incentivizing investors to fund "shovel-ready" incumbents rather than new ventures.

Furthermore, the policy had negative real effects on the local economy. Despite the increase in investment, treated areas experienced declines in aggregate local employment and sales, and a reduction in market competition. These findings highlight the limitations of employing purely market-based tax incentives in distressed communities. While successful at attracting capital, such policies may inadvertently stifle entrepreneurship and job creation that they aim to foster. Future policy designs must therefore look beyond aggregate investment volumes to consider the distributional impact of capital allocation. To ensure broad economic gains from these incentive programs, policymakers should consider differentiating credits based on firm age and local industry composition, mandating job creation, and extending qualification windows.

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Appendix: Variable Definitions

Variable Name	Definition
OZ	An indicator that takes a value of one if the tract was designated as an Opportunity Zone and zero if it was eligible but not designated.
Post	is a dummy that equals zero prior to 2018 and one afterward.
Num_Inv	Number of investment deals filed in a census tract. Obtained from the SEC Form D filings.
Amount_Inv	Dollar amount of investments in $milions in a census tract. Obtained from the SEC Form D filings.
Num_NewFirms	Number of for-profit new firms registered in a tract. Obtained from official business registration records collected by Opencorporates.
Num_Dissolved (Age>1 or 2 Yrs)	Number of firms that dissolved in a census tract (when they were older than one or two years at the time of dissolution). Obtained from official business registration records collected by Opencorporates.
Population	The population of a census tract in thousands. Obtained from the ACS by Census.
Median_Income	The median income of a census tract in thousand dollars. Obtained from the ACS by Census.
Median Age	The median age of a census tract. Obtained from the ACS by Census.
Poverty Rate	The poverty rate of a census tract. Obtained from the ACS by Census.
%White	The percentage of people in a census tract who are white. Obtained from the ACS by Census.
%Black	The percentage of people in a census tract who are black. Obtained from the ACS by Census.
Unemp_Rate	The unemployment rate of a census tract. Obtained from the ACS by Census.
%NoHighSchool	The percentage of people in a census tract who do not have a high school degree. Obtained from the ACS by Census.
Employment	Total employment of firms located in a census tract. Obtained from NETS.
Sales	Total sales by firms located in a census tract. Obtained from NETS.
CR3 and CR5	Concentration ratios of the top-three and top-five employers or sales producers in a census tract.
HHI	The Herfindahl-Hirschman Index calculated based on a census' employment or sales.

Figures and Tables

Figure 1. Geographical Distribution of Opportunity Zones

This figure displays the geographical distribution of Opportunity Zones in the United States. Regions highlighted in red represent census tracts designated as Opportunity Zones. Regions marked in yellow were eligible for designation but were not finally selected as Opportunity Zones. The remaining areas show census tracts ineligible for designation within the US.

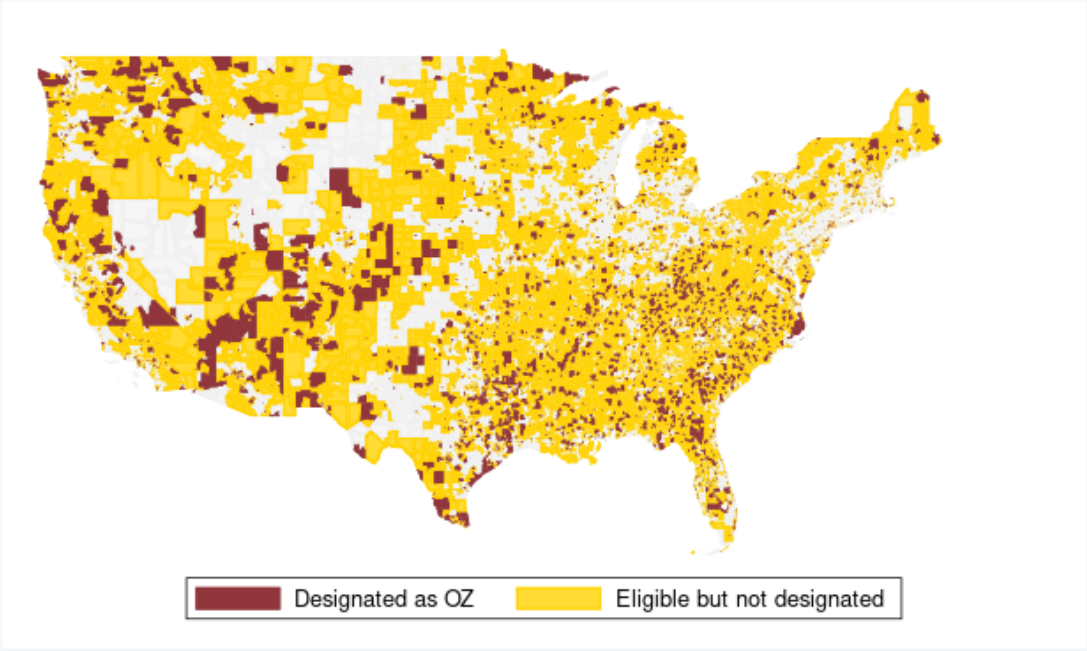
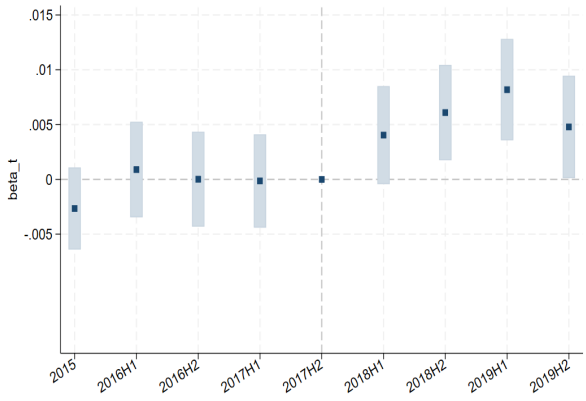


Figure 2. Semi-annual Coefficient Estimates Plotting

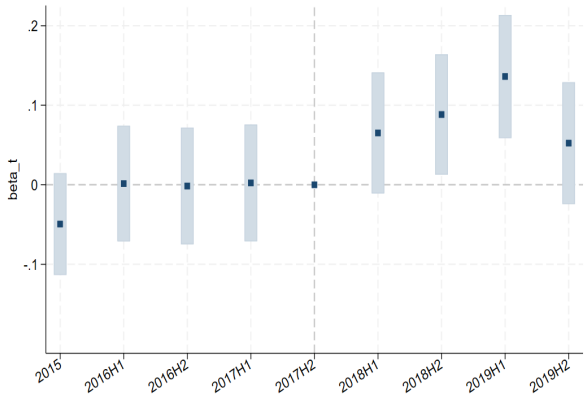
The figures show the coefficient plot around the Opportunity Zone policy by estimating the following model:

$$Y_{i,t} = \alpha + \sum_{t=2015, t\neq 2017H2}^{2019} \beta_t OZ_i * Semester_t + OZ_i + Controls_i + \delta_t + \eta_c + \epsilon_{i,t},$$

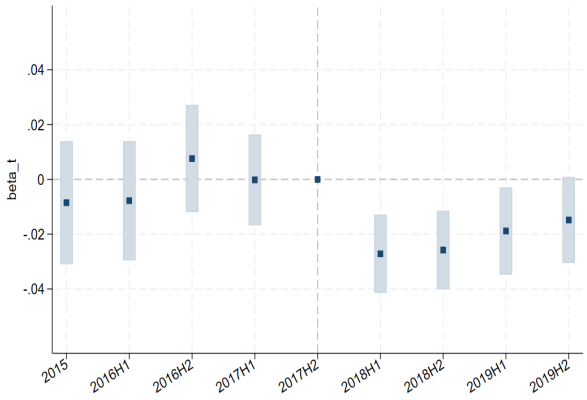
where $Semester_t$ is a set of indicator variables that equals one in semester t. The benchmark group comprises observations from the second semester of 2017 (2017H2) when the Opportunity Zone policy was signed into law. OZ_i is a dummy that equals one if census tract *i* was designated as an Opportunity Zone and equals zero if the tract was eligible but not selected. Panel (a) shows the plot of coefficient estimates of β_t when the outcome variable is the natural logarithm of one plus the number of private investments. The outcome variable in Panel (b) is the natural logarithm of one plus the amount of private investments. The outcome variable in Panel (c) is the natural logarithm of one plus the number of new for-profit firms. The center points show the point estimates of β_t and the vertical lines denote the 95% confidence intervals of β_t estimates. Control variables are specified as in the baseline regressions in Table 2. Census tract fixed effects and semester fixed effects are included. Standard errors are clustered by census tracts.



((a)) Number of Private Investments



((b)) Amount of Private Investments



((c)) Number of New Firms

Table 1. Summary Statistics

This table displays the summary statistics for the data used in this study. Most of the variables are constructed at the census-tract-year level. Panel A reports the statistics of the outcome variables related to private investment deals and business formation. Panel B displays the statistics of control variables. Panel C reports the statistics of variables constructed using data on establishment-level employment and sales information. Panel D shows the statistics of variables measuring other local economic outcomes. Variable construction and data sources are introduced in Section 4.

	N	Mean	SD	Min	Median	Max
	Panel A: Private Investments and Entrepreneurship					
Opportunity Zone Dummy (*OZ*)	154,563	0.247	0.431	0.000	0.000	1.000
Number of Investment Deals (*Num_Inv*)	154,563	0.118	2.257	0.000	0.000	641.000
Dollar Amount of Investments (*Amount_Inv*, $milions)	154,563	2.732	92.635	0.000	0.000	15,422.180
Number of New Firms (*Num_NewFirm*)	147,638	10.158	46.537	0.000	3.000	4,628.000
	Panel B: Control Variables					
Population (thousands)	154,492	4.046	1.892	0.011	3.801	40.616
Median Income ($thousands)	154,492	39.156	12.987	2.499	38.141	181.125
Median Age	154,492	35.860	7.501	21.000	35.100	80.400
Poverty Rate (%, *Poverty_Rate*)	154,492	22.181	9.976	3.356	20.619	51.164
White Alone (*%White*)	154,492	61.206	28.727	1.061	67.339	99.339
Black Alone (*%Black*)	154,492	23.000	27.929	0.000	10.426	100.000
Unemployment Rate (%, *Unemp_Rate*)	154,492	10.827	6.116	1.190	9.603	30.999
Without High School Degree Rate (*%NoHighSchool*)	154,492	20.867	11.883	0.000	18.630	100.000
	Panel C: Employment and Sales Related					

Continued on next page

Table 1 – Continued from previous page

Total Employment (Thousands)	154,476	2.497	4.253	0.000	1.440	198.553
Total Sales ($Million)	154,476	400.210	3,939.252	0.000	160.574	815,350.900
Emp. Top-3 Concentration Ratio (%,*CR3*): Non-Tradable	153,703	53.156	20.233	2.538	50.549	100.000
Emp. Top-5 Concentration Ratio (%,*CR5*): Non-Tradable	153,703	65.464	19.757	4.230	64.407	100.000
Emp. HHI (*HHI*): Non-Tradable	153,709	1,758.177	1,562.017	0.000	1,269.934	10,000.000
Emp. Top-3 Concentration Ratio (%,*CR3*): Tradable	140,231	85.224	17.656	11.324	92.135	100.000
Emp. Top-5 Concentration Ratio (%,*CR5*): Tradable	140,231	92.938	12.219	16.541	100.000	100.000
Emp. HHI (*HHI*): Tradable	140,314	4,747.626	2,849.086	0.000	4,012.216	10,000.000
Sales. Top-3 Concentration Ratio (%,*CR3*): Non-Tradable	153,703	68.713	19.118	4.081	69.439	100.000
Sales. Top-5 Concentration Ratio (%,*CR5*): Non-Tradable	153,703	79.247	15.989	6.316	81.714	100.000
Sales. HHI (*HHI*): Non-Tradable	153,709	2,741.045	1,683.877	472.524	2,244.765	5,993.340
Sales. Top-3 Concentration Ratio (%,*CR3*): Tradable	140,231	89.304	14.437	13.507	96.273	100.000
Sales. Top-5 Concentration Ratio (%,*CR5*): Tradable	140,231	95.304	9.184	19.284	100.000	100.000
Sales. HHI (*HHI*): Tradable	140,314	5,399.009	2,832.087	0.000	4,955.247	10,000.000
	Panel D: Other Economic Outcomes					
Number of Firms Dissolved (*Num_Dissolved*)	147,638	0.431	3.078	0.000	0.000	436.000
Number of Firms Dissolved, Age>1 Year	147,638	0.390	2.864	0.000	0.000	407.000
Number of Firms Dissolved, Age>2 Years	147,638	0.172	1.353	0.000	0.000	193.000

Table 2. Impact of OZs on Private Investment and Entrepreneurship

This table shows the impact of the Opportunity Zone policy on local private investments and entrepreneurship. The dependent variables are the natural logarithm of the one plus the number of private investment deals invested in census *i* and year *t* (*Ln(Num_Inv+1)*), the amount of private investment deals invested (*Ln(Amount_Inv+1)*), and the number of new firms registered (*Ln(Num_NewFirms+1)*). *OZ* is an indicator that takes a value of one if the tract was designated as an Opportunity Zone and zero if it was eligible but not designated. *Post* is a dummy that equals zero prior to 2018 and one afterward. Control variables include the natural logarithm of the population (*Population*), the natural logarithm of median income (*Median_Income*), the natural logarithm of median age (*Median_Age*), the percentage of white people (*%White*), the percentage of black people (*%Black*), poverty rate (*Poverty_Rate*), unemployment rate (*Unemp_Rate*), and the percentage of people without a high-school degree (*%NoHighSchool*). I also control for year and tract fixed effects. Standard errors are clustered at the census tract level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(Num_Inv+1)		*Ln(Amount_Inv+1)*		*Ln(Num_NewFirms+1)*	
*OZ*Post*	0.011***	0.011***	0.151***	0.149***	-0.018*	-0.019**
	(0.002)	(0.002)	(0.032)	(0.032)		(0.010)
Population		0.017**		0.251**		-0.040
		(0.008)		(0.120)		(0.039)
Median_Income		0.010		0.101		0.085***
		(0.006)		(0.087)		(0.025)
Median_Age		0.003		0.106		0.151***
		(0.008)		(0.120)		(0.044)
%White		0.000*		0.005***		-0.001*
		(0.000)		(0.002)		(0.001)
%Black		-0.000		0.001		-0.005***
		(0.000)		(0.003)		(0.001)
Poverty_Rate		0.000*		0.005**		0.003***
		(0.000)		(0.002)		(0.001)
Unemp_Rate		-0.000		-0.001		0.001
		(0.000)		(0.002)		(0.001)
%NoHighSchool		0.000		0.000		-0.004***
		(0.000)		(0.002)		(0.001)
Constant	0.042***	-0.228**	0.589***	-3.347**	1.445***	0.525
	(0.000)	(0.099)	(0.003)	(1.463)	(0.001)	(0.463)
Observations	154,563	154,492	154,563	154,492	147,638	147,565
R-squared	0.733	0.733	0.565	0.565	0.849	0.849
Tract FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES

Table 3. Impact on Private Investments by Firm Age

This table shows the impact of the Opportunity Zone policy on local private investments grouped by the age of firms when receiving the investments. The dependent variable in Columns (1) and (2) is the natural logarithm of one plus the total number of private investments (*Ln(Num_Inv+1)*) received by firms that are at least one year old or less than one-year old census tract *i* and year *t*, respectively. The dependent variable in Columns (3) and (4) is the natural logarithm of one plus the total amount of private investments (*Ln(Amount_Inv+1)*) received by firms that are at least one year old or less than one-year old census tract *i* and year *t*, respectively. *OZ* is an indicator that takes a value of one if the tract was designated as an Opportunity Zone and zero if it was eligible but not designated. *Post* is a dummy that equals zero prior to 2018 and one afterward. Control variables are the same as in Table 2. I control for year and tract fixed effects. Standard errors are clustered at the census tract level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	Ln(Num_Inv+1)		*Ln(Amount_Inv+1)*	
	One Year and Above	*Less Than 1 Year*	*One Year and Above*	*Less Than 1 Year*
*OZ*Post*	0.010***	0.002**	0.140***	0.035**
	(0.002)	(0.001)	(0.031)	(0.016)
Observations	154,492	154,492	154,492	154,492
R-squared	0.727	0.579	0.576	0.450
Controls	YES	YES	YES	YES
Tract FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Difference	0.008		0.105	
z-statistics	3.578***		3.010***	

Table 4. Impact on Local Entrepreneurship By Sector

This table shows the impact of the Opportunity Zone policy on local new business formation by categorizing firms into sectors following Mian and Sufi (2014). The dependent variables in Columns (1), (2), and (3) are the natural logarithm of the one plus the number of total new for-profit firms in the non-tradable, tradable, construction, and other sectors registered in census *i* and year *t*, respectively. *OZ* is an indicator that takes a value of one if the tract was designated as an Opportunity Zone and zero if it was eligible but not designated. *Post* is a dummy that equals zero prior to 2018 and one afterward. This analysis does not include census tracts in Delaware, Illinois, and Puerto Rico due to data coverage. Control variables are the same as in Table 2. I also control for year and tract fixed effects. Standard errors are clustered at the census tract level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	Ln(Num_NewFirms+1)			
	Non-tradable	*Tradable*	*Construction*	*Other*
*OZ*Post*	-0.016***	-0.003	-0.005	-0.021**
	(0.005)	(0.002)	(0.006)	(0.009)
Observations	147,565	147,565	147,565	147,565
R-squared	0.568	0.388	0.722	0.847
Controls	YES	YES	YES	YES
Tract FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Table 5. Impact on Local Employment and Sales

This table shows the impact of the Opportunity Zone policy on the aggregated employment and sales and those by different types of firms based on their age (older than one year old or not) and ownership type (a subsidiary of another company or a standalone business). In Panel A, the dependent variable is the total jobs supported by a specific type of firm in a census tract in a year (*Ln(Employment+1)*). In Panel B, the dependent variable is the total sales generated by a specific type of firm in a census tract in a year (*Ln(Sales+1)*). Establishment-level employment and sales data are from the NETS database and aggregated to census tract and year level based on different firm types. *OZ* is an indicator that takes a value of one if the tract was designated as an Opportunity Zone and zero if it was eligible but not designated. *Post* is a dummy that equals zero prior to 2018 and one afterward. Control variables are the same as in Table 2. I control for year and tract fixed effects. Standard errors are clustered at the census tract level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively..

	Panel A: *Ln(Employment+1)*			
	(1)	(2)	(3)	(4)
	Total	Age>1 & Subsidiary	Age≤1 & Standalone	Others
*OZ*Post*	-0.017***	-0.004	-0.041***	-0.016***
	(0.001)	(0.003)	(0.006)	(0.002)
Observations	154,476	154,476	154,476	154,476
R-squared	0.995	0.991	0.827	0.987
Controls	YES	YES	YES	YES
Tract FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

	Panel B: *Ln(Sales+1)*			
	(1)	(2)	(3)	(4)
	Total	Age>1 & Subsidiary	Age≤1 & Standalone	Others
*OZ*Post*	-0.028***	-0.008	-0.066***	-0.031***
	(0.003)	(0.007)	(0.012)	(0.004)
Observations	154,476	154,476	154,476	154,476
R-squared	0.983	0.969	0.670	0.966
Controls	YES	YES	YES	YES
Tract FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Table 6. Heterogeneous Effects by Tradable Sector Dominance

This table reports the heterogeneous impact of the Opportunity Zone policy on local business formation and employment, based on the dominance of the tradable sector in the census tract. The sample is split into tracts where the tradable sector is the largest employment sector and accounts for more than 20% of the workforce (Columns 1 and 3), and those where it is not (Columns 2 and 4). The dependent variable in Columns (1) and (2) is the natural logarithm of the number of new firms (*Ln(Num_NewFirms+1)*). In Columns (3) and (4), the dependent variable is the natural logarithm of total employment (*Ln(Employment+1)*). *OZ* is a dummy variable equal to one if the tract was designated as an Opportunity Zone and zero if it was eligible but not designated. *Post* is an indicator variable that equals one for years 2018 and later, and zero otherwise. All specifications include tract and year fixed effects, as well as the same set of time-varying tract-level controls used in Table 2. Standard errors are clustered at the census tract level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	Ln(Num_NewFirms+1)		*Ln(Employment+1)*	
Tradable-Dominant Tracts	Yes	No	Yes	No
*OZ*Post*	0.012	-0.020**	-0.006	-0.017***
	(0.032)	(0.010)	(0.004)	(0.001)
Observations	10,818	136,749	11,493	142,972
R-squared	0.847	0.849	0.995	0.995
Tract FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Table 7. Impact on Local Competition

This table shows the impact of the Opportunity Zone policy on local market competition. Three measures of local market competition are used: Concentration ratios of the top-three and top-five employers or sales producers (*CR3* and *CR5*) and the Herfindahl-Hirschman Index (*HHI*) calculated based on employment or sales. Panel A shows market competition dynamics in employment and Panel B shows sales. The geographical unit is a census tract. Establishment-level employment and sales data are from the NETS database and aggregated to census tract and year level. *OZ* is an indicator that takes a value of one if the tract was designated as an Opportunity Zone and zero if it was eligible but not designated. *Post* is a dummy that equals zero prior to 2018 and one afterward. Control variables are the same as in Table 2. I control for year and tract fixed effects. Standard errors are clustered at the census tract level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Panel A: *Local Market Competition in Employment*					
	Non-Tradable			Tradable		
	(1)	(2)	(3)	(4)	(5)	(6)
	CR3	*CR5*	*HHI*	*CR3*	*CR5*	*HHI*
*OZ*Post*	0.378***	0.289***	30.957***	0.053	0.008	17.060
	(0.096)	(0.084)	(8.423)	(0.087)	(0.054)	(17.059)
Observations	153,703	153,703	153,709	140,231	140,231	140,314
R-squared	0.936	0.950	0.908	0.938	0.954	0.890
Controls	YES	YES	YES	YES	YES	YES
Tract FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES

	Panel B: *Local Market Competition in Sales*					
	Non-Tradable			Tradable		
	(1)	(2)	(3)	(4)	(5)	(6)
	CR3	*CR5*	*HHI*	*CR3*	*CR5*	*HHI*
*OZ*Post*	0.262**	0.250**	23.415*	-0.055	-0.016	11.782
	(0.128)	(0.098)	(12.585)	(0.076)	(0.044)	(17.783)
Observations	153,703	153,703	153,709	140,231	140,231	140,314
R-squared	0.876	0.902	0.835	0.927	0.945	0.880
Controls	YES	YES	YES	YES	YES	YES
Tract FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES

Table 8. Impact on Existing Firms' Dissolution by Firm Age

This table shows the impact of the Opportunity Zone policy on local firm dissolution by the age of firms. The dependent variable is the natural logarithm of one plus the number of firms dissolved (*Ln(Num_Dissolved+1)*) in a census tract in a year in Column (1). In Columns (2) and (3), the dependent variable is the number of firms dissolved that are at least one-year-old or two-year-old, respectively. *OZ* is an indicator that takes a value of one if the tract was designated as an Opportunity Zone and zero if it was eligible but not designated. *Post* is a dummy that equals zero prior to 2018 and one afterward. Control variables are the same as in Table 2. I also control for year and tract fixed effects. Standard errors are clustered at the census tract level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)
	Ln(Num_Dissolved+1)		
	All Dissolved	Firm Age>1 Year	Firm Age>2 Years
*OZ*Post*	-0.006	-0.007*	-0.014***
	(0.004)	(0.004)	(0.003)
Observations	147,565	147,565	147,565
R-squared	0.510	0.483	0.437
Controls	YES	YES	YES
Tract FE	YES	YES	YES
Year FE	YES	YES	YES

Internet Appendix

The Effect of Tax Incentives on Local Private Investments and Entrepreneurship: Evidence from the Tax Cuts and Jobs Act of 2017

A Additional Tests

Table A1. Distribution of Private Investment Deals in Eligible Census Tracts by Age Group and by State

This table shows the distribution of investments by firms' age group and by state. Panel A shows the distribution of firm age groups: established for less than one year, from one to five years, and above five years. Panel B displays the geographical distribution of firms receiving investments with the top 20 states listed and the rest states shown jointly as "other states." Panel C displays the distribution across sectors (business, real estate, and finance) as well as the industries under each sector. The first column shows the age group, state name, or sector. The second column shows the number of investment deals. The third column shows the percentage.

Panel A: Age Distribution

Age Group	Freq.	Percent
Less than 1	4,333	15.43
From 1 to 5	18,810	66.98
More than 5	4,940	17.59
Total	28,083	100.00

Panel B: Geographical Distribution

Continued on next page

Table A1 – Continued from previous page

	Freq.	Percent
State	Freq.	Percent
California	6,423	22.87
Washington	2,204	7.85
Colorado	1,777	6.33
Texas	1,731	6.16
New York	1,517	5.40
Massachusetts	1,442	5.13
Florida	1,304	4.64
Illinois	1,002	3.57
Pennsylvania	938	3.34
North Carolina	828	2.95
Ohio	706	2.51
Oregon	618	2.20
Virginia	556	1.98
Georgia	467	1.66
Tennessee	450	1.60
Maryland	407	1.45

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Table A1 – Continued from previous page

	Freq.	Percent
Minnesota	406	1.45
Delaware	402	1.43
Connecticut	389	1.39
Arizona	371	1.32
Other States	4,145	14.77
Total	28,083	100.00

Panel C: Sector and Industry Distribution

Sector	Freq.	Percent
- *Industry*		
Business	15,165	54.00
- *Biotechnology*	975	3.47
- *Manufacturing*	576	2.05
- *Oil and Gas*	459	1.63
- *Retailing*	438	1.56
- *Restaurants*	313	1.11
- *Pharmaceuticals*	289	1.03

Continued on next page

Table A1 – Continued from previous page

	Freq.	Percent
- *Business Services*	237	0.84
- *Agriculture*	223	0.79
- *Computers*	207	0.74
- *Other Energy*	254	0.90
- *Other Technology*	5,437	19.36
- *Other Health Care*	1,159	4.13
- *Other Business*	4,195	16.37
Real Estate	4,747	16.90
- *Commercial*	1,755	36.97
- *Other Real Estate*	1,144	24.10
- *Residential*	1,133	23.87
- *REITS and Finance*	651	13.71
- *Construction*	64	1.35
Finance	8,171	29.10
- *Pooled Investment Fund*	6,963	24.79
- *Investing*	507	1.81
- *Other Banking and Financial Serv*	403	1.44

Continued on next page

Table A1 – Continued from previous page

	Freq.	Percent
- *Insurance*	125	0.45
- *Investment Banking*	20	0.07
Total	28,083	100.00

Table A2. Robustness Test: Alternative Options of Fixed Effects

This table shows the results of a robustness test using alternative options of fixed effects. In Columns (1), (4), and (7), county fixed effects and fixed effects are included. In Columns (2), (5), and (8), I include county fixed effects as well as the state-year fixed effects. In Columns (3), (6), and (9), I include tract fixed effects as well as state-year fixed effects. The dependent variables are the natural logarithm of the one plus the number of private investment deals invested in census *i* and year *t* (*Ln(Num_Inv+1)*), the amount of private investment deals invested (*Ln(Amount_Inv+1)*), and the number of new firms registered (*Ln(Num_NewFirms+1)*). *OZ* is an indicator that takes a value of one if the tract was designated as an Opportunity Zone and zero if it was eligible but not designated. *Post* is a dummy that equals zero prior to 2018 and one afterward. Control variables are the same as in Table 2. Standard errors are clustered at the census tract level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Ln(Num_Inv+1)			*Ln(Amount_Inv+1)*			*Ln(Num_NewFirms+1)*		
*OZ*Post*	0.010***	0.009***	0.011***	0.134***	0.129***	0.147***	-0.023**	-0.024***	-0.019***
	(0.002)	(0.002)	(0.002)	(0.032)	(0.032)	(0.032)	(0.010)	(0.005)	(0.005)
OZ	0.030***	0.030***		0.427***	0.428***		0.216***	0.156***	
	(0.003)	(0.003)		(0.036)	(0.036)		(0.015)	(0.009)	
Observations	154,492	154,492	154,492	154,492	154,492	154,492	147,572	147,572	147,567
R-squared	0.037	0.046	0.733	0.035	0.044	0.566	0.271	0.756	0.934
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	-	-	YES	-	-	YES	-	-
County FE	YES	YES	-	YES	YES	-	YES	YES	-
Tract FE	-	-	YES	-	-	YES	-	-	YES
State*Year FE	-	YES	YES	-	YES	YES	-	YES	YES

Table A3. Robustness Test: Alternative Options of Clustering the Standard Errors

This table shows the results of a robustness test using alternative options of clustering the standard errors. The dependent variables are the natural logarithm of the one plus the number of private investment deals invested in census *i* and year *t* (*Ln(Num_Inv+1)*), the amount of private investment deals invested (*Ln(Amount_Inv+1)*), and the number of new firms registered (*Ln(Num_NewFirms+1)*). *OZ* is an indicator that takes a value of one if the tract was designated as an Opportunity Zone (OZ) and zero if it was eligible but not designated. *Post* is a dummy that equals zero prior to 2018 and one afterward. Control variables are the same as in Table 2. I also control for year and tract fixed effects. Standard errors are clustered at the county level, county and year level, or state and year level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Ln(Num_Inv+1)			*Ln(Amount_Inv+1)*			*Ln(Num_NewFirms+1)*		
*OZ*Post*	0.011***	0.011***	0.011**	0.158***	0.158***	0.158***	-0.019*	-0.019*	-0.019**
	(0.002)	(0.002)	(0.002)	(0.029)	(0.030)	(0.029)	(0.010)	(0.008)	(0.007)
Observations	154,492	154,492	154,492	154,492	154,492	154,492	147,567	147,567	147,567
R-squared	0.733	0.733	0.733	0.579	0.579	0.579	0.849	0.849	0.849
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES
Tract FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Cluster of SE	County	County*Year	State*Year	County	County*Year	State*Year		County*Year	State*Year

Table A4. Impact of Opportunity Zones on Private Investment and Entrepreneurship: Poisson Regressions

This table shows the results of Poisson estimation of how the Opportunity Zone policy affects local private investments and entrepreneurship. The dependent variables are the number and dollar amount of private investment deals (*Num_Inv* and *Amount_Inv*) and the number of new firms registered in census *i* and year *t* (*Num_NewFirm*). *OZ* is an indicator that takes a value of one if the tract was designated as an Opportunity Zone and zero if it was eligible but not designated. *Post* is a dummy that equals zero prior to 2018 and one afterward. Control variables are the same as in Table 2. I control for year and tract fixed effects. Standard errors are clustered at the census tract level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)
	Num_Inv	*Amount_Inv*	*Num_NewFirm*
*OZ*Post*	0.125***	0.162***	-0.032***
	(0.040)	(0.053)	(0.011)
Observations	154,492	154,492	147,565
Controls	YES	YES	YES
Tract FE	YES	YES	YES
Year FE	YES	YES	YES

Table A5. Impact of Opportunity Zones on Private Investment by Sector

This table shows the impact of the Opportunity Zone policy on local private investments in three sectors: business, real estate, and finance. In Columns (1)-(3), the dependent variable is the natural logarithm of one plus the number of private investment deals in a specific sector invested in census i and year t (*Ln(Num_Inv+1)*). In Columns (4)-(6), the dependent variable is the natural logarithm of one plus the amount of private investment deals in a specific sector invested (*Ln(Amount_Inv+1)*). *OZ* is an indicator that takes a value of one if the tract was designated as an Opportunity Zone and zero if it was eligible but not designated. *Post* is a dummy that equals zero prior to 2018 and one afterward. Control variables are the same as in Table 2, including the natural logarithm of population, median income, and median age as well as the poverty rate, percentage of white or black people, unemployment rate, and percentage of the population without a high-school degree of a census tract in a given year. I also control for year and tract fixed effects. Standard errors are clustered at the census tract level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(Num_Inv+1)			*Ln(Amount_Inv+1)*		
	Business	*Real Estate*	*Finance*	*Business*	*Real Estate*	*Finance*
*OZ*Post*	0.007***	0.004***	0.003**	0.122***	0.070***	0.019
	(0.002)	(0.001)	(0.001)	(0.028)	(0.018)	(0.017)
Observations	154,492	154,492	154,492	154,492	154,492	154,492
R-squared	0.695	0.592	0.716	0.564	0.485	0.563
Controls	YES	YES	YES	YES	YES	YES
Tract FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES

Table A6. Net Creation of Local Businesses by Employment Size

This table shows the impact of the Opportunity Zone policy on the net creation of businesses by zip code. I collect the number of establishments by zip code from the Zip Codes Business Patterns from the Census Bureau. The dependent variables are the natural logarithm of the absolute value of the changes in the number of establishments in zip code *i* and year *t* (*sign(Chg_Estab)*Ln(*$|Chg\_Estab.|$*)* by the size of employment. The dependent variable in Column (1) is the number of establishments of all sizes while it is the number of establishments when the size of employment is less than 10, from 10 to 49, from 50 to 99, and equal to or more than 100 in Columns (2) to (5), respectively. *OZ%* is a continuous variable that equals the percentage of the population in a zip code that resides in Opportunity Zones. *Post* is a dummy that equals zero prior to 2018 and one afterward. Control variables include the population, median income, percentage of white or black people, poverty rate, unemployment rate, and percentage of the population without a high school degree of a census tract in a given year. I also control for year and county fixed effects. Standard errors are clustered at the county level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
	All Emp. Sizes	*Emp.<10*	*10≤Emp.<50*	*50≤Emp.<100*	*Emp.≥100*
*OZ%*Post*	-0.080***	-0.074***	-0.077***	0.006	0.008
	(0.023)	(0.023)	(0.018)	(0.010)	(0.010)
OZ%	0.033	0.019	0.046***	-0.038***	-0.018***
	(0.021)	(0.018)	(0.012)	(0.007)	(0.006)
Observations	122,989	122,989	122,989	122,989	122,989
R-squared	0.151	0.101	0.056	0.038	0.069
County FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES

Table A7. Impact of Opportunity Zones on New Business Formation: Restricting OZs That Have Received Investments

This table shows the impact of the Opportunity Zone (OZ) policy on local new business formation by restricting the treated census tracts to OZs that have received at least one deal of private investment during the sample period. Column (1) shows the OLS regression results where the dependent variable is natural logarithm of one plus the number of total new firms registered in census *i* and year *t* (*Ln(Num_NewFirm+1)*). Column (2) shows the Poisson regression result. *OZ* is an indicator that takes a value of one if the tract was designated as an Opportunity Zone and zero if it was eligible but not designated. *Post* is a dummy that equals zero prior to 2018 and one afterward. This analysis does not include census tracts in Delaware, Illinois, and Puerto Rico due to data coverage. Control variables are the same as in Table 2. I also control for year and tract fixed effects. Standard errors are clustered at the census tract level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)
	OLS	Poisson
	Ln(Num_NewFirm+1)	*Num_NewFirm*
*OZ*Post*	-0.044*	-0.043**
	(0.023)	(0.017)
Observations	116,482	116,482
R-squared	0.854	-
Controls	YES	YES
Tract FE	YES	YES
Year FE	YES	YES

Table A8. Impact of Opportunity Zones on Local Private Investment: By the 2016 Presidential Election Winning Party

This table shows the impact of the Opportunity Zone policy on local private investments by the winning party of the 2016 presidential election in the state that a census tract is located in. In Columns (1) and (3), I include sample census tracts located in states won by the Democratic Party in the 2016 election. In Columns (2) and (4), sample census tracts located in states won by the Republican party are included. The dependent variables are the natural logarithm of the one plus the number of private investment deals invested in census *i* and year *t* (*Ln(Num_Inv+1)*) and the amount of private investment deals invested (*Ln(Amount_Inv+1)*). *OZ* is an indicator that takes a value of one if the tract was designated as an Opportunity Zone and zero if it was eligible but not designated. *Post* is a dummy that equals zero prior to 2018 and one afterward. Control variables are the same as in Table 2. I control for year and tract fixed effects. Standard errors are clustered at the census tract level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	Ln(Num_Inv+1)		*Ln(Amount_Inv+1)*	
*OZ*Post*	0.012***	0.010***	0.125**	0.165***
	(0.004)	(0.002)	(0.053)	(0.039)
Observations	64,241	90,251	64,241	90,251
R-squared	0.775	0.673	0.590	0.538
2016 Election	Blue	Red	Blue	Red
Controls	YES	YES	YES	YES
Tract FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Blue-Red Diff.	0.002		-0.040	
z-statistics	0.447		-0.608	

Table A9. Logit Regressions for Propensity Score Matching

This table shows the logit regressions when the dependent variable is the indicator variable for Opportunity Zones (*OZ*) before and after the propensity score matching procedure. Independent variables include the logarithm of population, median income, and median age as well as the poverty rate, percentage of white or black people, and unemployment rate, percentage of the population without a high-school degree of a census tract at the end of 2017. I also include the level and the past two-year growth of private investments and new firm registrations. I include a set of dummy variables for each state. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1) Pre-PSM *OZ*	(2) Post-PSM *OZ*
Population	0.091***	-0.053
	(0.033)	(0.039)
Median_Income	-1.106***	-0.075
	(0.069)	(0.080)
Median_Age	0.240***	0.016
	(0.078)	(0.092)
%White	0.007***	0.000
	(0.001)	(0.001)
%Black	0.007***	0.001
	(0.001)	(0.001)
Poverty_Rate	0.012***	0.000
	(0.002)	(0.003)
Unemp_Rate	0.030***	0.005
	(0.003)	(0.004)
%NoHighSchool	0.006***	0.002
	(0.002)	(0.002)
lnnum	-0.186	-0.109
	(0.119)	(0.127)
lnamount	0.042***	0.011
	(0.009)	(0.010)
lnnewfirm	0.269***	0.028
	(0.022)	(0.026)
Num_Inv_Growth	0.095	-0.118
	(0.240)	(0.264)
Amnt_Inv_Growth	-0.404*	0.119
	(0.231)	(0.251)
New_Firm_Growth	-0.030*	0.011
	(0.016)	(0.018)
Constant	7.144***	0.976
	(0.883)	(1.031)
Observations	30,904	15,210
Pseudo R2	0.0558	0.0011
State Dummies	YES	YES

Table A10. Robustness Test: Propensity-Score-Matched Sample

This table shows the impact of the Opportunity Zone policy on local private investments using a propensity-score-matched sample. The dependent variables are the natural logarithm of the one plus the number of private investment deals invested in census *i* and year *t* (*Ln(Num_Inv+1)*) and the amount of private investment deals invested (*Ln(Amount_Inv+1)*). *OZ* is an indicator that takes a value of one if the tract was designated as an Opportunity Zone and zero if it was eligible but not designated. *Post* is a dummy that equals zero prior to 2018 and one afterward. Control variables are the same as in Table 2. I also control for year and census tract fixed effects. Standard errors are clustered at the census tract level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(Num_Inv+1)		*Ln(Amount_Inv+1)*		*Ln(NewFirms+1)*	
*OZ*Post*	0.013***	0.013***	0.210***	0.210***	-0.018**	-0.019**
	(0.003)	(0.003)	(0.039)	(0.039)	(0.009)	(0.009)
Observations	76,043	76,030	76,043	76,030	72,536	72,525
R-squared	0.737	0.737	0.593	0.594	0.852	0.852
Controls	NO	YES	NO	YES	NO	YES
Tract FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
PSM	YES	YES	YES	YES	YES	YES
# of Tracts	15210	15210	15210	15210	14508	14508

Table A11. Robustness Test: Excluding Neighborhoods Close to Colleges and Universities

This table shows the impact of the Opportunity Zone policy on local private investments on a sample excluding the neighborhoods with colleges and universities. In Columns (1) to (3), I drop census tracts that are located in counties with at least one university that has a population of over 10,000. In Columns (4) to (6), I drop census tracts that have a college or university campus. The information on the campus address and population is collected from Homeland Infrastructure Foundation-Level Data (HIFLD). The dependent variables are the natural logarithm of the one plus the number of private investment deals invested in census *i* and year *t* (*Ln(Num_Inv+1)*), the amount of private investment deals invested (*Ln(Amount_Inv+1)*), and the number of new firms registered (*Ln(NewFirms+1)*). *OZ* is an indicator that takes a value of one if the tract was designated as an Opportunity Zone and zero if it was eligible but not designated. *Post* is a dummy that equals zero prior to 2018 and one afterward. Control variables are the same as in Table 2. I also control for year and census tract fixed effects. Standard errors are clustered at the census tract level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Sample:	Drop Counties w/ Colleges of 10K Pop			Drop Census Tracts w/ Colleges		
Dependent Variable:	*Ln(Num_Inv+1)*	*Ln(Amount_Inv+1)*	*Ln(NewFirm+1)*	*Ln(Num_Inv+1)*	*Ln(Amount_Inv+1)*	*Ln(NewFirms+1)*
*OZ*Post*	0.005**	0.097***	-0.019*	0.010***	0.148***	-0.020**
	(0.002)	(0.037)	(0.010)	(0.002)	(0.032)	(0.010)
Observations	68,935	68,935	66,180	150,049	150,049	143,324
R-squared	0.573	0.484	0.850	0.703	0.559	0.850
Controls	YES	YES	YES	YES	YES	YES
Tract FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES

Table A12. Robustness Test: Excluding Neighborhoods with Multinational Company Headquarters or Subsidiaries

This table shows the impact of the Opportunity Zone policy on local private investments in a sample excluding the neighborhoods with branches of multinational companies (MNCs). I drop census tracts that have at least twenty branches of MNCs or have any MNC headquarters. The dependent variables are the natural logarithm of the one plus the number of private investment deals invested in census *i* and year *t* (*Ln(Num_Inv+1)*), the amount of private investment deals invested (*Ln(Amount_Inv+1)*), and the number of new firms registered (*Ln(Num_NewFirms+1)*). I identify firms as multinational in a given year if the absolute value of their pretax foreign income (PIFO) or foreign tax expenses (TXFO) are non-missing and greater than zero in Compustat. I then match branch address information from the NETS database to MNCs by their headquarters name and state and geocode the branch address to find its census tract. *OZ* is an indicator that takes a value of one if the tract was designated as an Opportunity Zone and zero if it was eligible but not designated. *Post* is a dummy that equals zero before 2018 and one afterward. Control variables are the same as in Table 2. I also control for year and census tract fixed effects. Standard errors are clustered at the census tract level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)
	Ln(Num_Inv+1)	*Ln(Amount_Inv+1)*	*Ln(Num_NewFirms+1)*
*OZ*Post*	0.007***	0.117***	-0.015**
	(0.002)	(0.030)	(0.007)
Observations	143,145	143,145	136,565
R-squared	0.585	0.490	0.842
Controls	YES	YES	YES
Tract FE	YES	YES	YES
Year FE	YES	YES	YES

Table A13. Robustness Test: Splitting Nearby and Non-nearby Control Tracts

This table shows the results of the sub-sample test choosing different census tracts as the control group based on their distance to an Opportunity Zone. In Columns (1), (3), and (5), I only include eligible but non-designated census tracts that are located within 3 kilometers of an Opportunity Zone. In Columns (2), (4), and (6), I include control census tracts that are located at least 3 kilometers away from an Opportunity Zone. The dependent variables are the natural logarithm of the one plus the number of private investment deals invested in census *i* and year *t* (*Ln(Num_Inv+1)*), the amount of private investment deals invested (*Ln(Amount_Inv+1)*), and the number of new firms registered (*Ln(NewFirms+1)*). *OZ* is an indicator that takes a value of one if the tract was designated as an Opportunity Zone and zero if it was eligible but not designated. *Post* is a dummy that equals zero prior to 2018 and one afterward. Control variables are the same as in Table 2. I also control for year and census tract fixed effects. Standard errors are clustered at the census tract level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(Num_Inv+1)		*Ln(Amount_Inv+1)*		*Ln(NewFirms+1)*	
*OZ*Post*	0.010***	0.012***	0.165***	0.149***	-0.024**	-0.018*
	(0.002)	(0.002)	(0.035)	(0.033)	(0.010)	(0.011)
Observations	136,864	98,360	136,864	98,360	130,364	94,275
R-squared	0.736	0.737	0.583	0.594	0.850	0.857
Controls	YES	YES	YES	YES	YES	YES
Tract FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Distance of Control Tract to an OZ	<3 km	≥3 km	<3 km	≥3 km	<3 km	≥3 km

Table A14. Using Non-Imputed NETS Data

This table shows the results of replicating Tables 5 and 7 using non-imputed employment data from the NETS database. Establishment-level employment data are from the NETS database and aggregated to census tract and year level. *OZ* is an indicator that takes a value of one if the tract was designated as an Opportunity Zone and zero if it was eligible but not designated. *Post* is a dummy that equals zero prior to 2018 and one afterward. Control variables are the same as in Table 2. I control for year and tract fixed effects. Standard errors are clustered at the census tract level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Panel A (replicating Table 5): *Ln(Employment+1)*			
	(1)	(2)	(3)	(4)
	Total	Age>1 & Subsidiary	Age≤1 & Standalone	Others
*OZ*Post*	-0.028***	-0.001	-0.051***	-0.028***
	(0.001)	(0.003)	(0.006)	(0.002)
Observations	154,465	154,465	154,465	154,465
R-squared	0.994	0.989	0.819	0.988
Controls	YES	YES	YES	YES
Tract FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

	Panel B (replicating Table 7): *Local Market Competition in Employment*					
	Non-Tradable			Tradable		
	(1)	(2)	(3)	(4)	(5)	(6)
	CR3	*CR5*	*HHI*	*CR3*	*CR5*	*HHI*
*OZ*Post*	0.563***	0.466***	36.866***	0.176	0.090	21.319
	(0.097)	(0.084)	(9.049)	(0.189)	(0.089)	(18.621)
Observations	153,563	153,563	153,712	139,552	139,552	140,583
R-squared	0.934	0.949	0.892	0.932	0.949	0.865
Controls	YES	YES	YES	YES	YES	YES
Tract FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES

B Model Details

How could increased private investments in an area discourage local entrepreneurship? This section presents a simple model that seeks to capture the crucial economic mechanism of how tax-induced investments could stifle local business formation and employment.

B.1 Setup

Consider an investor operating a fund of size K. In the context of the Opportunity Zone program, the fund is the Qualified Opportunity Fund (QOF), and K is all the capital gain that the fund's investors realized from out-of-zone businesses.[A2] The fund distributes the investments between two types of firms with different risk levels and returns. Here, I focus on the split of investment between existing firms and new firms because most of the firms located in these low-income and high-poverty neighborhoods are private companies with limited information, and one important factor related to the risk of failure that investors would consider is the company's age (Dunne et al., 1989). The fund's investment amount in existing firms is denoted by K_1, and that in new firms is K_2. For simplicity, I assume that the investment in existing firms always succeeds with a constant rate of return r. On the contrary, investment in new firms has a higher rate of return upon success, $R > r$, but success is not guaranteed. The average success probability across the investment in new firms is defined as $1 - \lambda_1 K_1 - \lambda_2 K_2$, where $0 \leq \lambda_1 < \lambda_2 < K$.[A3] Notice that $\lambda_2 > 0$ captures the idea that good entrepreneurial projects are scarce,[A4] leading to the decrease

[A2] The main benefit for the investor by investing in firms located in an Opportunity Zone and keeping the money for at least five (seven) years is that she can receive a 10% (15%) deduction on the capital gain taxes she has to pay, along with tax delays until 2026 and elimination of tax on the profits from this investments if invest for ten years.

[A3] If new firms were strictly dominated by incumbents ($R < r$), the optimal K_2^*, investment in new firms, would be zero for any K. This can be viewed as an extreme case of the model where investors exclusively favor established firms.

[A4] This is likely the case in Opportunity Zones where economic conditions are challenging.

of the average success probability as more new firms are invested; the assumption of $\lambda_1 > 0$ indicates that better-financed existing firms lessen the new firms' success probability because of market competition; also, I assume $\lambda_2 > \lambda_1$ because the new firms' profit prospects should be more sensitive to their own production compared to the competitive firms' production. I will refer to λ_1 as the *local competition parameter* and allow it to differ by industry. For instance, λ_1 is higher in the non-tradable sector than in the tradable sector because firms selling non-tradable goods compete mainly locally. Also, I let μ_1 and μ_2 denote the proportion of investment spent in employment for existing and new firms, respectively.

B.2 Model Predictions

The investor's objective is to maximize $\pi = rK_1 + R(1 - \lambda_2 K_2 - \lambda_1 K_1)K_2$ subject to $K_1 + K_2 = K$. Substitute the budget constraint into the objective function, I infer that the investor's total return as a function of K_2 is $\pi(K_2) = r(K - K_2) + R(1 - \lambda_2 K_2 - \lambda_1(K - K_2))K_2$.

Proposition 1. *The optimal investment in new firms is*

$$K_2^* = f(K) := \begin{cases} K & \text{if } K < \tilde{K}, \\ \frac{\frac{R-r}{R} - \lambda_1 K}{2(\lambda_2 - \lambda_1)} & \text{if } K \in [\tilde{K}, \hat{K}], \\ 0 & \text{if } K > \hat{K}, \end{cases}$$

where $\tilde{K} := \frac{R-r}{(2\lambda_2 - \lambda_1)R}$ *and* $\hat{K} := \frac{R-r}{\lambda_1 R}$.

Proof. I first consider the interior solution by using the first-order condition $\frac{\partial \pi(K_2)}{\partial K_2} = 0$, which is equivalent to $r = R(1 - \lambda_2 K_2 - \lambda_1(K - K_2) + K_2(\lambda_1 - \lambda_2)$. Solving this equation, the optimal interior solution of K_2 is $\frac{\frac{R-r}{R} - \lambda_1 K}{2(\lambda_2 - \lambda_1)}$.

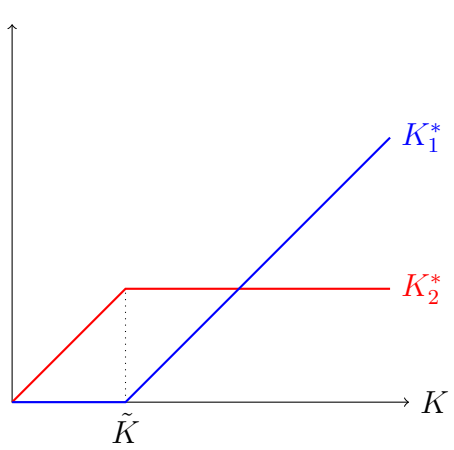
The interior solution is effective if and only if $0 \leq \frac{\frac{R-r}{R}-\lambda_1 K}{2(\lambda_2-\lambda_1)} \leq K$, which is equivalent to $K \in [\tilde{K}, \hat{K}]$; otherwise, I need to consider the corner solution. When $K < \tilde{K}$, the optimal corner solution is $K_2^* = K$ — that is, the investor will invest all the money in new firms when his fund size is very small because the marginal success probability of the new firms is still high.

When $K > \hat{K}$, the optimal corner solution is $K_2^* = 0$ — the investor only invests in existing firms because the competition from the well-financed existing firms makes it unprofitable to invest in new firms. □

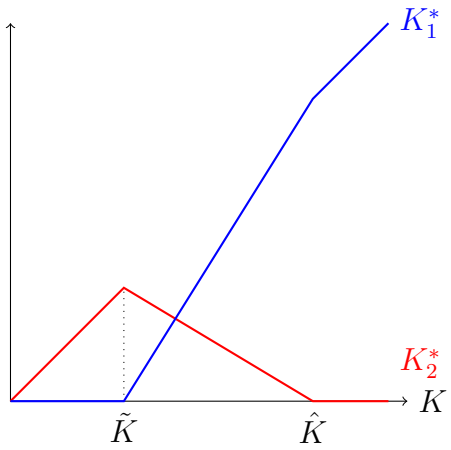
The figure below depicts the optimal investment as a function of the fund size K for both cases with $\lambda_1 = 0$ and $\lambda_1 > 0$. The most notable feature is that the optimal investment in new firms (K_2^*) is non-monotonic in the fund size (K) when $\lambda_1 > 0$.

Figure. Illustration of Optimal Investment between Old and New Firms

The figures depict the optimal investment of investors' objective to maximize their profit. Panel (a) depicts when the competition parameter $\lambda_1 = 0$ and Panel (b) shows the case when $\lambda_1 > 0$.



(a) $\lambda_1 = 0$



(b) $\lambda_1 > 0$

Based on Proposition 1, one has the following prediction regarding how the Opportunity Zone policy (i.e., an increase in the fund size, K) affects new business formation (measured by the optimal amount of investment in the new firm, K_2^*). I make a reasonable restriction that

$K \geq \tilde{K}$ — there is usually capital available for investment besides the OZ fund. Assuming that the investment in new firms positively correlates with new business formation, I get the following prediction.

Prediction 1.

(1) When the local competition parameter $\lambda_1 = 0$ (such as in the tradable sector), new business formation is not impacted by the OZ policy.

(2) When the local competition parameter $\lambda_1 > 0$ (such as in the non-tradable sector), new business formation is negatively impacted by the OZ policy.

To better interpret the connection between the above prediction and the model, consider the situation where the OZ policy increases the available fund from K' to $K'' := K' + \Delta K$ where $\tilde{K} < K' < \hat{K}$, the corresponding change in new business formation, as predicted by the model, is $f(K'') - f(K')$, which is negative when $\lambda_1 > 0$ and zero when $\lambda_1 = 0$. It is also worth emphasizing that the crucial channel for the above prediction is the competition that the old firm posts on the new firm. Notably, the slope of $f(K)$ in the interior solution is $-\frac{\lambda_1}{2(\lambda_2 - \lambda_1)}$, whose absolute value is increasing in λ_1. In other words, as the competition parameter increases, the negative impact of the OZ policy on new business formation also increases.

Proposition 2. *When $K \in [\tilde{K}, \hat{K}]$, the total expense in employment is strictly decreasing in the fund size K if and only if $\frac{\mu_1}{\mu_2} < \frac{\lambda_1}{2\lambda_2 - \lambda_1}$.*

Proof. When $K \in [\tilde{K}, \hat{K}]$, I only need to consider the interior solution. Since the total expense in employment is $L = \mu_1 K_1 + \mu_2 K_2$, I substitute the optimal investment of K_1^* and K_2^* into this expression and get $L(K) = \left[\mu_1 - (\mu_2 - \mu_1)\frac{\lambda_1}{2(\lambda_2 - \lambda_1)}\right] K + C$ where C is a constant. It

can be seen that $L'(K) < 0$ if and only if $\mu_1 - (\mu_2 - \mu_1)\frac{\lambda_1}{2(\lambda_2-\lambda_1)} < 0$, which is equivalent to $\frac{\mu_1}{\mu_2} < \frac{\lambda_1}{2\lambda_2-\lambda_1}$. □

The above proposition helps us understand the impact of the policy on employment. It is well-documented in the literature that new firms (or entrepreneurial firms) are better than old firms (or existing firms) in promoting employment (Adelino et al., 2017; Haltiwanger et al., 2013). Indeed, the above proposition suggests that if the gap between new firms and old firms in creating jobs is sufficiently large (i.e., the ratio $\frac{\mu_1}{\mu_2}$ is sufficiently small), employment will strictly decrease as the fund size increases. This leads to the following prediction when the OZ policy is introduced.

Prediction 2. *When new firms' abilities to create jobs are sufficiently higher than those of existing firms, employment is negatively impacted by the policy.*

C Using Machine Learning to Assign Business Sector in Business Registration Data

To examine which sector experienced the greatest decline in new business formation, one could follow Mian and Sufi (2014) and categorize industries into four sectors: tradable, non-tradable, construction, and other, based on each business's connection to local conditions. However, one data challenge of performing cross-sectional analysis for this study is that business formation data from *OpenCorporates* do not provide industry codes such as NAICS or SIC. I tackle this issue by taking a machine-learning approach. I first use the list of firms from the NETS database, which contains the NAICS code, to train the model, and then I use the trained model to predict a firm's sector in the business registration dataset.

In the machine learning procedure, I first prepare the independent and dependent variables for training and prediction data sets. Following Cuffe et al. (2019), I use company names and geographical information to predict a firm's sector.[A5] Using data from the NETS to form the training data, I first standardize the text data by lowering the case and removing punctuation, special characters, and stopwords. There are 2,420,466,463 unique words in the training data set before standardization and 224,911,471 unique words after. I then vectorize the words in the company name using the Count Vectorization approach. Combined with the geographical information (zip code), I obtain a vector of information to predict a firm's sector.

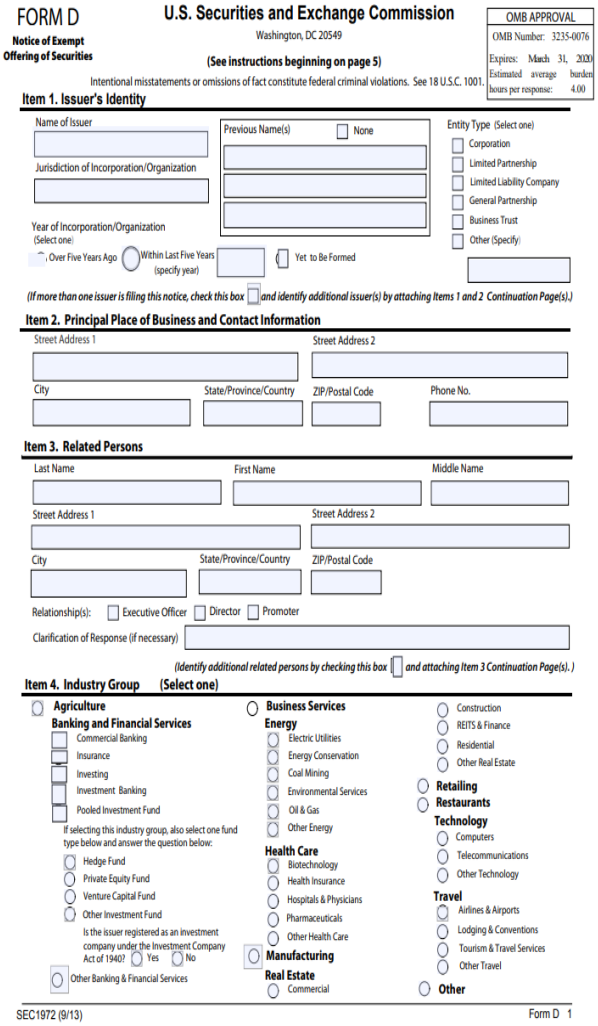
As the NETS database provides the NAICS code, I match the crosswalk list of NAICS Codes to the four sectors defined in Mian and Sufi (2014) each firm's four-digit NAICS code

[A5] Cuffe et al. (2019) use the company name and information web-scraped from Google Reviews to predict the industry code of firms. They first use the *word2vec* approach to analyze and vectorize the text information. They then adopt a *RandomForest* model to predict the industry code. Using this approach, they achieved a 59% accuracy in assigning correct NAICS sectors.

in NETS. To evaluate the model performance and select the model with the best prediction accuracy, I split the NETS data sets: 80% as the training set and 20% as the validation set. I train the model on the training set and then predict the sector in the validation set. I focus on three types of algorithms: Logistic Regression, XGBOOST, and Support Vector Machine. The prediction accuracy on the validation set is 82.7%, 80.5%, and 72.6%, respectively. Therefore, I choose the model trained with Logistic Regression and predict each firm's sector in the business registration database. I perform the same training process on the SBA loan datasets, which also provide the name, industry code, and geographical location of firms. The prediction accuracy is similar. However, there could be a potential concern that the SBA database may overweight businesses in certain industries or those that borrow funds instead of raising equity. Hence, I use the prediction from the NETS database for the main analyses in the paper.

D Form D

Form D is used to file a notice of an exempt offering of securities with the SEC. The federal securities laws require the notice to be filed by companies that have sold securities without registration under the Securities Act of 1933 in an offering made under Rule 504 or 506 of Regulation D or Section 4(a)(5) of the Securities Act.[A6] The figure below shows the first two pages of Form D that firms file for exemption of registration to the SEC.



FORM D

Notice of Exempt Offering of Securities

U.S. Securities and Exchange Commission

Washington, DC 20549

OMB APPROVAL

OMB Number: 3235-0076

Expires: March 31, 2020

Estimated average burden hours per response: 4.00

(See instructions beginning on page 5)

Intentional misstatements or omissions of fact constitute federal criminal violations. See 18 U.S.C. 1001.

Item 1. Issuer's Identity

Name of Issuer

Previous Name(s) None

Entity Type (Select one)

Corporation

Limited Partnership

Limited Liability Company

General Partnership

Business Trust

Other (Specify)

Jurisdiction of Incorporation/Organization

Year of Incorporation/Organization (Select one)

Over Five Years Ago

Within Last Five Years (specify year)

Yet to Be Formed

(If more than one issuer is filing this notice, check this box and identify additional issuer(s) by attaching Items 1 and 2 Continuation Page(s).)

Item 2. Principal Place of Business and Contact Information

Street Address 1

Street Address 2

City

State/Province/Country

ZIP/Postal Code

Phone No.

Item 3. Related Persons

Last Name

First Name

Middle Name

Street Address 1

Street Address 2

City

State/Province/Country

ZIP/Postal Code

Relationship(s): Executive Officer Director Promoter

Clarification of Response (if necessary)

(Identify additional related persons by checking this box and attaching Item 3 Continuation Page(s).)

Item 4. Industry Group (Select one)

Agriculture

Banking and Financial Services

Commercial Banking

Insurance

Investing

Investment Banking

Pooled Investment Fund

If selecting this industry group, also select one fund type below and answer the question below:

Hedge Fund

Private Equity Fund

Venture Capital Fund

Other Investment Fund

Is the issuer registered as an investment company under the Investment Company Act of 1940? Yes No

Other Banking & Financial Services

Business Services

Energy

Electric Utilities

Energy Conservation

Coal Mining

Environmental Services

Oil & Gas

Other Energy

Health Care

Biotechnology

Health Insurance

Hospitals & Physicians

Pharmaceuticals

Other Health Care

Manufacturing

Real Estate

Commercial

Construction

REITS & Finance

Residential

Other Real Estate

Retailing

Restaurants

Technology

Computers

Telecommunications

Other Technology

Travel

Airlines & Airports

Lodging & Conventions

Tourism & Travel Services

Other Travel

Other

SEC1972 (9/13)

Form D 1

FORM D

U.S. Securities and Exchange Commission

Washington, DC 20549

Item 5. Issuer Size (Select one)

Revenue Range (for issuer not specifying "hedge" or "other investment" fund in Item 4 above)

No Revenues

$1 - $1,000,000

$1,000,001 - $5,000,000

$5,000,001 - $25,000,000

$25,000,001 - $100,000,000

Over $100,000,000

Decline to Disclose

Not Applicable

OR

Aggregate Net Asset Value Range (for issuer specifying "hedge" or "other investment" fund in Item 4 above)

No Aggregate Net Asset Value

$1 - $5,000,000

$5,000,001 - $25,000,000

$25,000,001 - $50,000,000

$50,000,001 - $100,000,000

Over $100,000,000

Decline to Disclose

Not Applicable

Item 6. Federal Exemptions and Exclusions Claimed (Select all that apply)

Rule 504(b)(1) (not (i), (ii) or (iii))

Rule 504(b)(1)(i)

Rule 504(b)(1)(ii)

Rule 504(b)(1)(iii)

Rule 506(b)

Rule 506(c)

Securities Act Section 4(a)(5)

Investment Company Act Section 3(c)

Section 3(c)(1)

Section 3(c)(2)

Section 3(c)(3)

Section 3(c)(4)

Section 3(c)(5)

Section 3(c)(6)

Section 3(c)(7)

Section 3(c)(9)

Section 3(c)(10)

Section 3(c)(11)

Section 3(c)(12)

Section 3(c)(13)

Section 3(c)(14)

Item 7. Type of Filing

New Notice OR Amendment

Date of First Sale in this Offering: OR First Sale Yet to Occur

Item 8. Duration of Offering

Does the issuer intend this offering to last more than one year? Yes No

Item 9. Type(s) of Securities Offered (Select all that apply)

Equity

Debt

Option, Warrant or Other Right to Acquire Another Security

Security to be Acquired Upon Exercise of Option, Warrant or Other Right to Acquire Security

Pooled Investment Fund Interests

Tenant-in-Common Securities

Mineral Property Securities

Other (describe)

Item 10. Business Combination Transaction

Is this offering being made in connection with a business combination transaction, such as a merger, acquisition or exchange offer? Yes No

Clarification of Response (if necessary)

Form D 2

[A6] See more information on the website of the SEC: https://www.sec.gov/smallbusiness/exemptofferings/formd.