Can Capital Inflows Stifle Local Entrepreneurship? *

Jiajie Xu †

Abstract

This paper examines the impact of capital inflows on local entrepreneurship. Using the Opportunity Zone policy as a quasi-natural experiment, the paper shows that capital inflows to economically distressed areas led to a decline in local entrepreneurship, particularly in the non-tradable sector with local competition. Motivated by a simple model, I show that new investments favored existing firms, enhancing their market positions and survivals relative to newcomers, hence discouraging business formation. Additionally, the policy negatively affected local employment. The paper suggests that future policy designs should consider potential distributional effects between incumbents and new firms when offering market-based tax incentives.

Keywords: Entrepreneurship, Capital Inflow, Tax Credits, Opportunity Zones, Local Competition, Access to Finance

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†The University of Iowa, Tippie College of Business, Email: jiajie-xu@uiowa.edu.

1 Introduction

Financial constraints are frequently cited as barriers to potential entry into entrepreneurship, a key driver of economic growth, job creation, and wealth equality (Decker et al., 2014; Herranz et al., 2015).[1] Consequently, policymakers have implemented various subsidies for prospective entrepreneurs and their investors, operating under the assumption that increased investments could lead to a surge in entrepreneurial activities (Lerner, 2020).[2] However, do capital inflows invariably stimulate entrepreneurship, or is there a risk that heightened private investments could result in crowding out of new entrants?

Studying the above question is important given the recent adoption of "market-based" tax incentives to attract private investments and foster local economic growth. This "market-based" approach avoids heavy government interventions, focusing solely on facilitating capital inflows. However, little is known about the distributional effect between existing and new firms when local private investments surge. Motivated by a simple model, this paper uses a market-based capital gain tax policy in the US, the Opportunity Zone program of 2017, to empirically study the question.

To understand why capital inflows could stifle local entrepreneurship and motivate the empirical analyses, I start with a simple model by studying the decision of an investor considering investing in an area. The investor has the option to invest in existing firms and new firms. I show that in sectors where firms compete locally and where there are few high-quality new firms, the investment in existing firms increases while it decreases in new firms. Furthermore, in cases where potential new businesses' ability to generate jobs is sufficiently high compared to existing businesses, capital inflows could negatively impact

[1] See theoretical discussion on the relationship between financial constraints and entrepreneurship by Cagetti and De Nardi (2006); Evans and Jovanovic (1989); Kihlstrom and Laffont (1979); Knight (1921) and empirical evidence by Bellon et al. (2021); Chen and Ewens (2021); Holtz-Eakin et al. (1994); Kerr et al. (2014); Kerr and Nanda (2009); Krishnan et al. (2015); Schmalz et al. (2017).

[2] Previous literature, in general, finds that policy-induced increases in private investments have positive impact on entrepreneurial activities (Edwards and Todtenhaupt, 2020; Guenther and Willenborg, 1999; Hall and Van Reenen, 2000) except Denes et al. (2023) who find null effects in the context of angel tax credits in the US.

local employment.

Next, I use the Opportunity Zone policy as a quasi-natural experiment to show empirical evidence. The Opportunity Zone program was introduced under the Tax Cut and Jobs Act (TCJA) of 2017 in the US. The policy aims to draw long-term private investments, which would not otherwise occur, to economically distressed census tracts. Investors who reinvest their capital gains from out-of-zone businesses into in-zone businesses for a qualified period can enjoy tax delays and deductions, regardless of the future performances of their invested firms.[3] The policy directly targets local private investments in economically distressed areas without imposing additional restrictions on the types of eligible firms. The eligibility of census tracts was based purely on poverty and income, which made the treated and control tracts have similar economic conditions: Out of 42,160 eligible census tracts, 8,762 were designated as Opportunity Zones. Furthermore, the enactment of the policy was not driven by local levels of new business formation, mitigating concerns related to reverse causality.

As a starting point for the empirical analyses, I verify whether the treated census tracts indeed experienced capital inflows driven by the policy. 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. Using the Form D disclosure data released by the Securities and Exchange Commission (SEC), I find that the policy indeed introduced capital inflows to economically distressed areas. A battery of additional tests shows the robustness of the above results.

After confirming that the treated areas experienced a rise in capital inflows, I next demonstrate a decline in local entrepreneurship in these areas by examining over eight million business registration filings sourced from state government registrars. I show that the loss in new business formation was not trivial by disaggregating the reduction based on the ex-post survival period and the legal type of these new firms. Notably, the contraction in business

[3]The policy offers three main tax benefits. First, capital gain taxes are deferred until 2026 or the asset sale. Second, Opportunity Zone investors get a 15% (10%) step-up of their basis of prior capital gain after seven years (five years), reducing their tax burdens. Third, the tax basis increases to the fair market value beyond ten years, eliminating taxes on new capital gains.

formation was more substantial and statistically significant for new firms with a longer ex-post survival period and new firms registered as a Corporation rather than LLC. In addition, the decrease in local entrepreneurship was greater in Opportunity Zones with positive private investments during the sample period. Using the ZIP Codes Business Pattern data, I confirm that there was a decline in the net creation of establishments and find that the decline was mainly in firms with a smaller employment size.

What accounts for the dichotomy that private investments heightened in economically distressed areas while local entrepreneurship declined? As illustrated by the model, when there is local competition and a scarcity of high-quality startup projects, investors may prefer firms with a low risk of failure. When limited information is available for businesses in these low-income zones, investors may place particular emphasis on firm age, given that the risk of business failure tends to decrease with the company's maturity (Dunne et al., 1989). Consequently, if investors direct a greater proportion of their investments toward existing and older firms in the treated areas, as opposed to newly-formed enterprises, the additional financial resources may help the incumbents build their competitive advantage over entrants and discourage potential entrepreneurs from starting new businesses.

This explanation is supported by first showing that private investments toward older firms indeed increased significantly but decreased in new companies in the treated areas after the policy's implementation, suggesting that capital inflows induced by the policy favored incumbents over new firms. The second piece of evidence focuses on the breakdown of local entrepreneurship. By decomposing local business formation in different sectors following Mian and Sufi (2014), I find that local entrepreneurship decreased significantly in the non-tradable sector, which faces a higher level of local competition. On the contrary, there was no significant impact on the formation of businesses in the tradable or construction sectors.

Further, market concentration, as measured by the concentration ratios and the Herfindahl–Hirschman Index (HHI), in the non-tradable sector increased significantly after the policy's implementation. In comparison, the tradable sector did not experience significant

changes in local market concentration. In addition, the labor share and sales share by older subsidiaries significantly increased while those by younger standalone firms decreased. Furthermore, the likelihood of dissolution of older firms in the treated areas decreased compared to the control areas after the policy's implementation. These findings support the explanation that the additional financial resources helped incumbents enhance their market position and increase their ability to survive in the local market, thus deterring potential entrepreneurs from entering.

Some may question why focusing on the decline in local entrepreneurship if the better-financed incumbents have positively impacted the local economy through job creation, attracting talent, or mitigating regional inequality. Consistent with the prediction from the conceptual framework, there were significant declines in local firms' aggregated employment after the policy's implementation. The decline was greater in firms with higher risk profiles, such as those that were younger and standalone. This result aligns with the model that when new firms' ability to generate jobs is high enough, the widened funding gap between existing and newly formed firms could lead to a decline in local employment. No significant changes were found in the number of people of various education or poverty levels moving into the treated areas and the policy did not reduce local inequality as measured by the Gini coefficient. The above results indicate that the Opportunity Zone policy had limited, if not negative, real economic impact on local employment, geographical mobility, or inequality.

Finally, the analysis of the policy's spillover effects shows that control tracts closer to a treated tract experienced larger increases in new business formation, total employment, and total sales than those located further away from an Opportunity Zone after the policy's implementation. The results suggest that part of the policy's negative impact can be explained by the reshuffling of economic activities to nearby untreated neighborhoods.

This paper provides important policy implications. Future policymakers need to consider the potential distributional effects across businesses with various levels of riskiness when offering market-based tax incentives. One possible way to improve these investor tax

credit policies is to differentiate the level of tax incentives based on the riskiness of firms. For example, a program could offer higher incentives to new businesses or non-real-estate firms that would create more long-term local jobs locally.

This paper contributes to several strands of literature. First, it builds on the literature surrounding the role of financial constraints as barriers to business formation (Hurst and Lusardi, 2004; Nanda, 2011; Robb and Robinson, 2014). While previous empirical studies have underscored the importance of personal wealth and external financing for entrepreneurs, a critical gap exists in understanding the reactions of potential entrepreneurs and incumbents to an overall capital inflow. This paper addresses this gap by proposing an economic channel and offering empirical evidence that capital inflows may favor existing businesses and discourage entrepreneurship under certain circumstances.

Second, this paper builds on literature regarding government initiatives aimed at fostering entrepreneurship and innovation (Akcigit et al., 2022; Bayar et al., 2019; Hall and Lerner, 2010; Howell, 2022; Lerner, 2020). Many government programs have been created to address market failures associated with entrepreneurial finance (Hall, 2002).[4] Regarding the effect of tax policies on entrepreneurial activity, the literature generally finds that lowering corporate or personal income taxes is positively related to entering into entrepreneurship (Cullen and Gordon, 2007; Curtis and Decker, 2018). Particular to capital gain taxes, existing studies have found that providing these credits increases investment in startups (Edwards and Todtenhaupt, 2020) and reduces IPO underpricing (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 capital gain tax credits can negatively affect local entrepreneurship through unevenly distributed investment inflows between incumbents and newly formed firms.

[4]These programs include government awards and grants (Howell, 2017; Lerner, 2000), government-sponsored venture capitalists (Brander et al., 2015), provision of loans to small businesses (Brown and Earle, 2017; Denes et al., 2019).

Finally, this paper is related to the literature on the effects of place-based policies, given that the Opportunity Zone program targets economically distressed areas in the US. Existing studies on such place-based policies have yielded mixed findings regarding their impact on local economic growth and employment (see Neumark and Simpson (2015) and Austin et al. (2018) for a summary of the studies) but largely overlooked the impact on local entrepreneurship.[5] Since introducing the Opportunity Zone program, 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 studying the impact of place-based policies on entrepreneurship as well as the interaction between established firms and new entrants, illustrating how place-based policies can negatively impact business formation.

2 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 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 usually involved heavy government efforts and interventions, such as selecting firms for grants or tax benefits and monitoring their uses. Although place-based policies have cost

[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), positive effects on commercial property transactions (Sage et al., 2023) and null impact on investments in commercial property investments and consumer spending (Corinth and Feldman, 2021), and positive impact on the growth rate of employment, establishments, and wage (Arefeva et al., 2023; Atkins et al., 2020; Freedman et al., 2021).

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 Kolko, 2010).[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 median0 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 exhibiting political favoritism (Eldar and Garber, 2023; Frank et al., 2022), governors, on average, have mostly chosen tracts that align with the policy objective, characterized by lower income and higher poverty levels, as demonstrated in Table A8. 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

[7]The most comparable precedents are the federal Empowerment Zones (EZ) and the New Markets Tax Credit (NMTC), which offer certain tax credits 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 credits provided are capped and offered to companies on a competitive basis rather than to investors.

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 into Qualified Opportunity Zone (QOZ) businesses through Qualified Opportunity Funds (QOF). QOZ businesses need to 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 into QOZ businesses. There are no other requirements for receiving tax benefits except the investment period and the 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. As of July 2021, more than 300 QOFs, with more than $64 billion in investment capacity, have been created since the passage of the law.[9] 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.[10]

3 Conceptual Framework

How could capital inflows discourage local entrepreneurship? This section presents a simple model that seeks to capture the crucial economic mechanism in this paper.

3.1 Setup

Consider an investor operating a fund of size K.[11] The investor distributes funds between two types of firms with different risk levels and returns. Here, we focus on the split of

[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] As of July 6, 2021, there were 303 QOFs, according to the OpportunityDB website (ttps://opportunitydb.com/funds/).

[10] 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.

[11] In the context of the Opportunity Zone program, K is the capital gain that the investor realized from out-of-zone businesses. The main benefit for the investor by investing in a firm 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.

investment between existing firms and new firms.[12] The investor'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 $0 \leq \lambda_1 < \lambda_2 < K$. Notice that $\lambda_2 > 0$ captures the idea that good entrepreneurial projects are scarce,[13] leading to the decrease 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, we let μ_1 and μ_2 denote the proportion of investment spent in employment for existing and new firms, respectively.

3.2 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, we 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*

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

[12] The reason we focus on the split between old and new is that most of the firms located in these low-income and high-poverty neighborhoods are private companies with limited information; one important factor related to the risk of failure that investors would consider is the company's age (Dunne et al., 1989).

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

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, we 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 $\lambda_1 > 0$.

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, we get the following prediction.

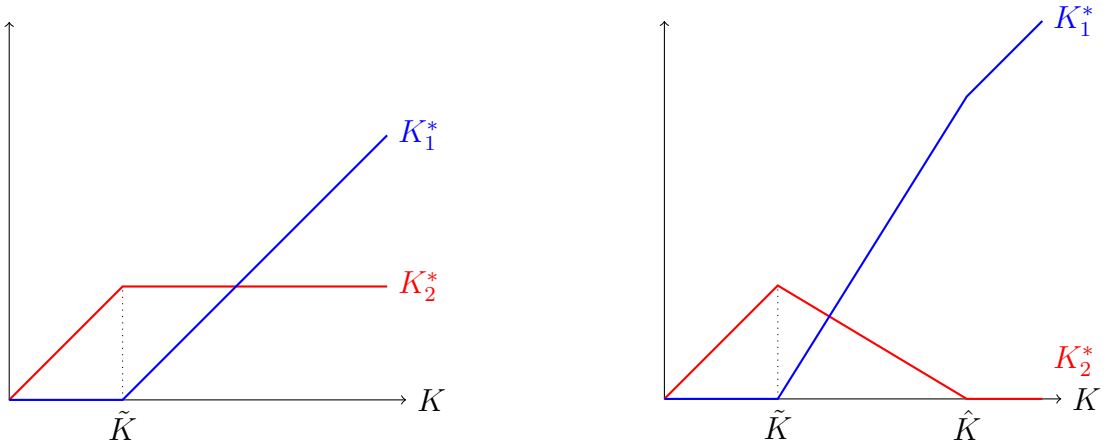
Prediction 1.

(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.*

Figure. Illustration of Optimal Investment between Old and New Firms

The figures depict the optimal investment of investors' objective to maximize their profit as discussed in section 3. 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) $\lambda_1 > 0$

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 $K \in [\tilde{K}, \hat{K}]$, we 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), the 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.*

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 capital inflows, 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 (Denes et al., 2023; Ewens and Malenko, 2020; 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.[14] Internet Appendix B shows a sample Form D.[15]

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.[16]

4.1.2 Data on Business Registration

Local entrepreneurship is the main outcome of interest in this paper and is measured by using official business registration filings to the 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., 2022; Guzman and Stern, 2015). The business registration data are collected by *OpenCorporates* and con-

[14]See more details from the guidance issued by SEC on Regulation D, Rule 507 and the case of *Hamby v. Clearwater Consulting Concepts*.

[15]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) use electronic business tax filings for 2019 and find that OZ investments appear 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). I report results only using data from Form D filings in the paper for consistency and simplicity.

[16]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 A2, 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.

tain information including 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. (2022), I focus on for-profit firms as they consist of more than 95% of new business formation and are more economically relevant.

I first geocode the company addresses using the Census Geocoder API to obtain the census tract code.[17] 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*. To evaluate entrepreneurship quality, I look into the new firm's survival length after its registration and group firms by whether they have maintained an active status for one year after the registration.[18] Based on the legal structure under which the companies are incorporated, I group new firms established in a given tract in a given year as corporations, limited liability companies, or "other." *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.

4.1.3 Data on Establishment-Level Employment and Sales

To gauge the economic mechanism and the real economic impact, I use the establishment-level employment and sales data from the 2021 National Establishment Time-Series (NETS).[19] 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

[17] 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.

[18] I chose one year as the threshold, given that firms' survival rate in the first year after formation is significantly lower compared to subsequent years, according to data from the Bureau of Labor Statistics's "Table of Survival of Private Sector Establishments by Opening Year."

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

firm establishments in the U.S. Several studies in economics and finance have used the NETS database to examine firm performance, especially for private firms and startups (Bias and Ljungqvist, 2022; Borisov et al., 2021; 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).

Using the NETS data, I construct local market concentration measures, including the concentration ratios of the top three or top five players (*CR3* and *CR5*) as well as the Herfindahl–Hirschman index (*HHI*) based on the employment or sales by firms in a census tract in a year. To study local business composition by different types of firms based on their age and ownership, I calculate the percentage of employment or sales generated by each type of firm in the local census tract in a year (*Employment_Share* and *Sales_Share*). 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.

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.[20] 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 fail to find any systematic bias in how NETS imputes missing data. Nevertheless, to alleviate the concern, in Table A15 in the Internet Appendix, I rerun the tests only using the non-imputed employment data, and the results are similar.

[20]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 (Abowd et al., 2009; Eckert et al., 2020).

4.1.4 Using Machine Learning to Assign Business Sector

To examine which sector experienced the greatest decline in entrepreneurship, 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.[21] 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 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

[21] 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.

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.[22]

4.1.5 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*),[23] 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 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), 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. For most analyses in the

[22] 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.

[23] 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.

paper, I include only the LIC tracts to make the treated and control groups more comparable. The main findings are all robust if the non-LIC contiguous tracts are included. The sample period is from 2015 through 2019. All the 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 A1 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 Empirical Analysis and Results

5.1 Main Specification

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 entrepreneurial activity. In the baseline regressions, I estimate the following equation:

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

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.[24] $Post$ is a dummy that equals zero before 2018 and one afterward. To

[24]To ensure the comparability of designated and non-designated census tracts, I conduct the analyses using a sample that contains only tracts that are considered low-income communities (i.e., contiguous tracts that did not qualify as low-income were omitted). Results stay robust using all eligible census tracts (see Table A3).

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.[25] I cluster standard errors by tract.[26]

5.2 Impact on Local Private Investments

I start the empirical analysis by validating that the Opportunity Zone policy indeed induced capital inflows to the treated census tracts.

[Insert Table 2 about here]

Table 2 presents the results. Columns (1) to (4) report the ordinary least squares (OLS) regressions where the dependent variable is 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 that were eligible but not designated.[27] Following Cohn et al. (2022), I report the results of Poisson estimation in Table A6 in the Appendix and find positive and statistically significant coefficient estimates on $OZ * Post$.

[25] Table A4 in 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.

[26] The main findings are robust when I use the alternative clustering options. Results are shown in Table A5 in the Internet Appendix.

[27] 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 $16.1\% = \frac{2.732*10^6+1}{2.732*10^6} * (e^{0.149} - 1)$.

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, I find signicant and positive impact on private investments across all three sectors, with the largest effect on the business sector.[28]

5.3 Identification Assumptions and Challenges

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 would have been the same across the treated and control areas, absent the TCJA policy change (i.e., the parallel trend assumption). Second, it assumes that the Opportunity Zone policy was not determined based on the level of local private investments and new business formation before the policy.

I take several steps to substantiate 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 absent of the policy. I address this concern by plotting the coefficient estimates around the TCJA's passage 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. The difference between the treated and control tracts started to enlarge significantly only after the policy was introduced.[29] The figure provides support for the parallel trend assumption required by the DiD approach.

[Insert Figure 2 about here]

Another concern is related to the differences in characteristics between the designated

[28]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.

[29]The annual coefficient plot is shown in Figure A1 and there is no significant pre-trend in private investments or entrepreneurship either.

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 its potential impact on the selection of tracts. To further address the concern that the variation in political ideology drove the difference in capital inflows between treated and control, Table A7 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 A8 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.[30]

Column (2) shows the Logit regression results using the matched sample. One can observe that the independent variables lost significance in the matched sample, and the pseudo R^2 decreased from 0.0558 to 0.0011, suggesting 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 A9 shows the DiD results with a sub-sample of matched

[30]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.

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 A10 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.

Others may have concerns about whether private investments were driven by other tax reform policies enacted for corporations at the same time under the TCJA, especially tax deductions for multinational companies (MNCs). By logic, it is unlikely that other provisions in the TCJA could contaminate the results found in this paper: The primary benefit of Opportunity Zones lies in capital gains tax relief for investors while other provisions in the TCJA for firms, however, relate to corporate expenses and profits, may not directly impact distressed areas within Opportunity Zones. Additionally, in Albertus et al. (2023), they show that there were no significant responses to the increased access to cheap capital in firms' investment and employment in the US, thus alleviating the above concern in my setting. Nevertheless, to address the concern on tax deductions for MNCs, I conduct a robustness test by excluding census tracts with headquarters or at least five branches of any MNCs. Firms are identified 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, following Lampenius et al. (2021). All the coefficient estimates on *OZ*Post* in Table A11 are statistically significant, indicating that the original findings were not influenced by areas that would substantially benefit from other tax reform provisions of the TCJA.

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 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 (or 5 kilometers) and re-run the baseline regressions. Table A12 shows that the policy had positive impact on local private investments regardless of whether benchmarking against control groups closer to a treated census tract or not. This suggests a limited spill-over effect on local private investments.

The above analyses confirm that the Opportunity Zone policy is a plausible quasi-natural shock to local capital inflows: The policy was effective in introducing private investments to treated areas that otherwise would not happen.

5.4 Impact on Local Entrepreneurship

After verifying that the Opportunity Zone policy has indeed incurred capital inflows to the treated census tracts, I next examine its impact on local entrepreneurship. As predicted in the conceptual framework, entrepreneurship may decline following capital inflows into local areas, particularly when facing local competition.

[Insert Table 3 about here]

Table 3 reports 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 in Columns (1) and (2), 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, local entrepreneurship declined an average of 2.1% more in census tracts designated as Opportunity Zones compared to other eligible but non-designated tracts.[31] Poisson regression also provides a significantly negative estimate, as reported in Table A6 Column (3).

Some argue that the decrease in business formation could be seen as a positive outcome

[31] The magnitude of new firms is calculated as follows, given that the mean of *Num_NewFirm* is 10.227 and the coefficient estimate is 0.019: $2.1\% = \frac{10.227+1}{10.227} * (e^{0.019} - 1)$.

of the policy, as it facilitated the elimination of low-quality firms that were likely to fail shortly after their establishment anyway. To show that the loss of entrepreneurship caused by the policy was economically meaningful, I perform two heterogeneity tests. First, I categorize the new firms based on their post-establishment survival durations. In Table 4, I group all the new for-profit firms formed by whether they have existed for more than one year after their firm birth, i.e., whether they shut down soon after opening.[32] I find that the decrease was mainly among firms that could operate longer, which can be considered as firms of higher quality.

[Insert Table 4 about here]

Second, I examine the policy's impact by the legal form of firms incorporated, as corporations are much more likely to grow than non-corporations (Guzman and Stern, 2015). I show that the decrease in local entrepreneurship appears in all types of businesses registered: corporations, limited liability companies (LLCs), or other types such as partnerships. Table 5 shows that corporations decreased the most by 3.1%, LLCs decreased by 1.8%, and others decreased by 2.8%.[33]

[Insert Table 5 about here]

I validate the finding based on the business registration data from *OpenCorporates* by cross-referencing with 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.[34] As shown in Column (1) in Table A13 in the Internet Appendix, the Opportunity Zone policy had a negative net effect on the creation of local businesses, confirming the previous

[32]This information is also collected from *OpenCorporate*, which contains a firm's incorporation date and dissolution date. Starting with Table 4, dependent variables related to entrepreneurship refer to the establishment of for-profit firms if not otherwise specified.

[33]$3.1\% = \frac{2.747+1}{2.747} * e^{0.023} - 1$, given that the mean of the number of new firms registered as corporations is 2.747 and the coefficient estimate on $OZ * Post$ equals 0.023.

[34]Many studies in the economics and finance literature have used this data set to measure local entrepreneurship (see **?** as an example).

finding using *OpenCorporates* data. The ZBP data also provides the number of employees of 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). This finding based on employment size is consistent with the market concentration results later shown in Table 9.

Finally, one should expect the decline in entrepreneurship to be more substantial in Opportunity Zones that have received private investments than those that have not, compared to non-treated census tracts. Table A14 confirms the above conjecture that the magnitude of the coefficient on $OZ * Post$ in a sample restricting to only positive-investments Opportunity Zones is about two times that in the baseline regressions where all Opportunity Zones are included.

The above results confirm Prediction 1 that local entrepreneurship could decline following capital inflows. However, 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 entrepreneurship 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 entrepreneurship might be relatively short-term, decreases in local new business formation could significantly impact local market competition and employment, as I will show in the following sections of the paper.

6 Mechanism

The previous findings show that heightened private investments, brought by the Opportunity Zone policy, discouraged local entrepreneurship. This section provides empirical evidence supporting the prediction outlined in Section 3 that the capital injection prompted by the policy has skewed investor preferences towards established, older ventures, deterring potential entry. I start by showing that private investments increased in established firms while newly formed ones experienced a decrease. Next, I show that the decrease in entrepreneurship was mainly in the non-tradable sector, which faces a higher level of local competition. Finally, I present the evidence that market concentration measures increased significantly in the non-tradable sector, which helped reduce firm dissolution for older companies.

6.1 Increased Private Investments Went to Existing Firms

As illustrated in Section 3, Opportunity Zone investors would prefer firms with existing firms as they present lower risks compared to new firms. Below, I test the impact on the capital flows to firms by their age group by running the following triple-difference regression:

$$Y_{i,g,t} = \alpha + \beta_1 Old_{i,g} * OZ_i * Post_t + \beta_2 OZ_i * Post_t + \beta_3 Old_{i,g} + Controls_{i,t-1} + \delta_t + \eta_i + \epsilon_{i,t}. \tag{2}$$

where the private investments in each census tract i year t are grouped by their invested firms' age at the investment. The dummy variable, $Old_{i,g}$, equals one if the investments were made in firms that have been established for at least one year and zero if less than one year.[35] The dependent variables are the number of investment deals (*Ln(Num_Inv + 1)*) and the dollar amount of deals (*Ln(Amount_Inv + 1)*) as in Table 2. The other empirical specification is similar to Equation (1). β_1 captures the additional private investments the policy brings to firms at least one-year-old in the treated areas, compared to the newly formed ones. β_2

[35]The one-year-old definition helps to distinguish the "up or out" pattern among new firms (Wiens and Jackson, 2015). According to the Bureau of Labor Statistics, about one-fourth of the new firms die in the first year and the exit rate slows down after one year.

captures the impact on private investments for those newly formed ones in the treated areas compared to those in the control areas after the policy' enactment. $\beta_1 + \beta_2 + \beta_3$ captures that impact for the existing firms in the treated areas. As predicted by the model in Section 3, we expect $\beta_1 + \beta_2 + \beta_3$ to be positive and β_2 to be negative.

[Insert Table 6 about here]

Table 6 presents the results of estimating Equation (2). In Column (1) Table 6, the coefficient estimate of β_1 is 0.022, β_2 is -0.005, and β_3 is 0.03, all statistically significant at 1% level, and $\beta_1 + \beta_2 + \beta_3$ is positive, suggesting that newly formed firms in Opportunity Zones received significantly fewer number of investments after the policy's implementation, but older firms experienced a significant increase in the number of investment deals. Column (2) shows that dollar-amount investments follow a similar pattern: the incumbents in treated areas received significantly more capital while newly formed firms experienced a decrease.

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 while newly formed firms suffered.

6.2 Declining Entrepreneurship in Sectors Facing Local Competition

Next, I test whether there is a decline in decline in local entrepreneurship in sectors with greater local competition as shown in Prediction 1. The intuition is that as investors allocate more capital to existing, older firms in the treated areas, these firms can leverage the additional resources to build their competitive advantage over newly formed firms. This would discourage a potential entrepreneur from entering the market and competing with existing firms locally.

[Insert Table 7 about here]

Following the industry categorization in Mian and Sufi (2014) based on the sensitivity to local conditions, I group firms into non-tradable, tradable, construction, and other sectors.

Table 7 shows the results of testing the policy's impact on local entrepreneurship by sector. The coefficient estimate on $OZ * Post$ is negative and statistically significant for the non-tradable and other sectors while insignificant for the tradable and construction sectors. The results suggest that the decline in local entrepreneurship is concentrated mainly in sectors more sensitive to local demand and supply. Existing firms can use the additional financial resources brought by the policy to gain competitive advantages locally, such as renovating stores as documented in Sage et al. (2023) that prices of local commercial properties that needed renovation increased significantly in the treated areas. Potential entrepreneurs, especially those in the non-tradable sector, anticipate (or observe) that they would not have the same financial resources to compete with existing firms in the neighborhood and may, therefore, choose not to start a business. Later in the paper, I show that local employment decreased in the Opportunity Zones, and this suggests that the capital inflows were likely spent toward non-labor expenditures such as construction or renovation of existing places.

6.3 Enhanced Market Position and Reduced Exits for Existing Firms

Then, I examine whether the increased financial resources provided to the existing businesses bolstered their market dominance, potentially acting as a deterrent. Specifically, I examine whether and how market concentration, business composition, and firm exits changed after the policy's implementation.

[Insert Table 8 about here]

First, I analyze the policy's impact on concentration measures in the local market and present the results in Table 8. In 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.[36] One would expect to see that market concentration would increase significantly in the non-tradable sector, where most of the decline in

[36]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.

entrepreneurship is observed, and the tradable sector should see little impact. This is the case shown in Table 8: 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.[37]

Second, I examine the policy's impact on the local economy's business composition. If the policy indeed influenced investors' preferences and subsidized businesses with lower risks, we would expect to see a significant increase in employment and sales from older businesses or non-independent entities (such as chain stores and subsidiaries) within treated areas compared to untreated areas. Specifically, I calculate the percentage of employment and sales by three types of firms in a census tract year: 1) older than one year and a subsidiary of a big company; 2) younger than one year and a standalone firm; 3) others (older than one year and standalone or younger than one year and a subsidiary). I hypothesize that type-1 companies, *Older & Subsidiary*, are mostly preferred by Opportunity Zone investors and therefore would gain market position in the local market, while type-2 firms, *Younger & Standalone*, are the riskiest type of business and would see a decrease in local market position, and type-3 firms, *Older & Standalone*, or, *Younger & Subsidiary* would experience an impact that is in between type-1 and type-2 companies.

[Insert Table 9 about here]

Table 9 shows the results of how the contribution by firm age and ownership to local employment or sales is affected. In column (1), where the dependent variable is the percentage of employment by type-1 firms, *Older & Subsidiary*, the coefficient estimate on $OZ * Post$ is indeed positive and statistically significant at the 1% level, suggesting that the older and non-standalone firms have played a greater role in the local employment market. In Column (2), the dependent variable is replaced by the fraction of employment by type-2

[37] In this paper, Tables 8, 9, and 11 use data from the NETS database to examine the economic channel and the real economic impact. 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 following Denes et al. (2023) in Table A15. The estimation results using non-imputed data provide similar conclusions.

firms, *Younger & Standalone*, and I find a significant and negative coefficient estimate on $OZ * Post$, suggesting that their market position measured by employment shrank significantly after the policy's implementation. Column (3) shows that type-3 firms captured the rest of the employment shares lost by the type-2 firms, given their riskiness is in between type-1 and type-2 companies.

[Insert Table 10 about here]

Third, I examine whether the policy affected incumbents' dissolution (i.e., firm exit). The logic is that increased capital inflows to incumbents may help their survival. In Table 10, the dependent variable is the natural logarithm of the number of firms that dissolved in a census tract each year. In Column (1) in Table 10, the coefficient on *OZ*Post* is negative and insignificant, suggesting that the total number of firms 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, thereby deterring the entry of new local firms.

In summary, the findings in this section suggest that the additional capital flowed to the treated areas strengthened the market positions and subsidized the survival of these incumbents, thereby discouraging potential entrepreneurs from creating new firms. This effect was particularly notable in the non-tradable sector, which plays a crucial role in providing long-term employment opportunities within targeted low-income neighborhoods.

7 Impact on Local Employment and Other Economic Outcomes

Thus far, the analyses have shown that despite heightened private investments in the treated areas, there was a decline in the formation of new businesses. However, it raises the ques-

tion: if the policy has potentially bolstered the local economy in other, arguably more pivotal economic dimensions, shall we emphasize the impact on entrepreneurship? Alternatively, if there is a negative impact on other local economic measures, is there any correlation with the decline in entrepreneurship within the local areas? In this section, I extend the examination to assess the policy's impact on broader economic indicators, such as aggregated employment levels and other economic outcomes, including migration patterns and inequality. Moreover, I explore the spillover effects of the policy on adjacent areas, thereby providing a comprehensive assessment of its overall impact.

[Insert Table 11 about here]

According to Prediction 2 in Section 3, when new firms' ability to generate jobs is sufficiently high compared to existing firms, inflows of capital could negatively impact local employment. Table 11 presents the result of examining the above prediction. 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 and ownership type as in Table 9, the total employment by *Younger&Standalone* firms decreased significantly by 4.1% and the other type of firms also decreased significantly by 1.6%. However, the total employment for the lowest risk type of firms, *Older&Subsidiary*, did not change significantly. When replacing the dependent variable with sales generated by local firms, we observe similar findings that the total sales also declined significantly. The only type of firms that survived this decrease were the ones older than one year and subsidiaries.

Next, I test whether the policy attracted people of various backgrounds to move to the treated areas. Table A16 uses data from the American Community Survey to show the policy's impact on the total number of people who moved into a census tract each year by these migrants' poverty level or education level. No significant effects of the policy are found on the total inflows of people or the inflows of a specific group of people by education or poverty status. Additionally, when analyzing the policy's impact on local inequality, as

shown in Table A17, I do not observe any significant changes in the Gini Index in Opportunity Zones compared to other eligible tracts after the policy change.

Finally, I analyze the policy's spillover effects on nearby areas by focusing on eligible-but-non-OZ tracts that are in the same county as the Opportunity Zone tracts and estimating the following equation:

$$Y_{i,t} = \alpha + \beta CloseToOZ_i * Post_t + Controls_{i,t-1} + \delta_{c,t} + \eta_i + \epsilon_{i,t}. \tag{3}$$

where $CloseToOZ_i$ is defined as the natural logarithm of the reciprocal of an eligible tract i's distance to the closest Opportunity Zone tract within the same county, if there is any. $Y_{i,t}$ includes variables related to private investments, new business formation, as well as local employment and sales. Control variables are the same as in the baseline regressions. I include tract fixed effects to control for time-invariant characteristics of census tracts as well as county-by-year fixed effects to control for economic shocks that are specific to a county.[38] β in Equation (4) measures the spillover effects: A positive β suggests that the closer an eligible tract is located to an Opportunity Zone, the higher the outcome is.

[Insert Table 12 about here]

Table 12 presents the results. In Columns (1) and (2), the dependent variables are the number and dollar amount of private investments. The estimate of β is insignificant at the 10% significance level, confirming the finding in Table A12 that the Opportunity Zone policy has no spillover effect on private investments in the nearby eligible tracts. When examining the business formation in Column (3), the estimate of β is positive and significant at 1% level, suggesting that if an eligible tract is located closer to an Opportunity Zone, it experiences an increase in the number of new businesses compared to those further ones in the same county after the policy's implementation. As shown in Columns (4) and (5), the estimates of β are also positive and statistically significant at 1% level. This indicates that nearby

[38]Results and conclusions are similar if controlling for tract fixed effects and year fixed effects as in the baseline regressions.

eligible tracts also experienced increases in employment and sales after the implementation of the Opportunity Zone policy.

Overall, the results in this section show that the Opportunity Zone policy has negatively impacted local employment and generated little economic impact on geographical mobility and regional inequality. The limited, if not negative, economic effects could be due to the decrease in the formation of new businesses and reduced firm dynamics shown earlier in the paper.[39] The spillover results indicate that at least a portion of these declines can be attributed to the relocation of economic activities induced by the policy among nearby census tracts. This finding holds significant academic and policy relevance, as the Opportunity Zone policy was initially designed as a place-based program to address economic underperformance in specific areas. Paradoxically, it generated an unintended consequence of diminishing economic dynamism in the targeted regions and benefiting nearby non-targeted areas, simultaneously exacerbating economic inequalities.

8 Conclusion and Policy Implications

This paper examines the relationship between capital inflows and their impact on local entrepreneurship. Using a recent investor capital gain tax credit policy, the Opportunity Zone program under the Tax Cuts and Jobs Act of 2017, as a quasi-natural experiment to increase local private investments, I first show that the policy effectively achieved its immediate goal of drawing more private investments to poor neighborhoods. However, local new business registration declined significantly, and the loss of entrepreneurship was not trivial in terms of firm quality.

Motivated by a simple model, the empirical analyses suggest that tax incentives shift investors' preference toward incumbent firms with a lower risk profile, such as those with a longer operating history. The additional capital helped the existing firms build their com-

[39] 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.

petitive advantage and gain market position over new entrants, thus discouraging potential entrepreneurs. This explanation is supported by the findings that local private investments increased in existing firms but decreased in newly formed firms; the decrease in entrepreneurship was mainly in the non-tradable sector with greater local competition, but not significant in the tradable or the construction sectors; local concentration, measured by concentration ratios and the HHI, increased significantly in the non-tradable sector; and firm dissolution decreased significantly for older companies.

Further, the observed decline in local entrepreneurship is not offset by significant gains in job creation or reductions in regional inequality. Despite the injection of capital, local employment and sales declined in the treated areas. The reductions were larger in firms with higher risk profiles, such as those newly formed, standalone companies. A spillover effect to neighboring tracts could partially explain the negative impact. No significant changes are found in other economic outcomes, such as inflows of talent or local inequality.

The broader policy implications of the study emphasize the need for policymakers to navigate potential distributional effects associated with market-based tax incentives. While policies may be successful in drawing investments to distressed areas, there exists a risk of favoring the financing of existing businesses and stifling entrepreneurship—a key driver of local job creation and economic growth, particularly in the targeted low-income, high-poverty areas. Moving forward, implementing more precise targeting strategies, such as generating long-term local employment rather than simply inviting more investments, could enhance the effectiveness of market-driven investor tax credits.

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Figures and Tables

Figure 1. Geographical Distribution of Opportunity Zones

This figure displays the geographical distribution of Opportunity Zones established under the Tax Cuts and Jobs Act (TCJA), enacted on December 22, 2017. Regions highlighted in red represent census tracts designated as Opportunity Zones. Those marked in yellow are eligible for designation but were not selected as Opportunity Zones. The remaining areas depict census tracts ineligible for designation within the US.

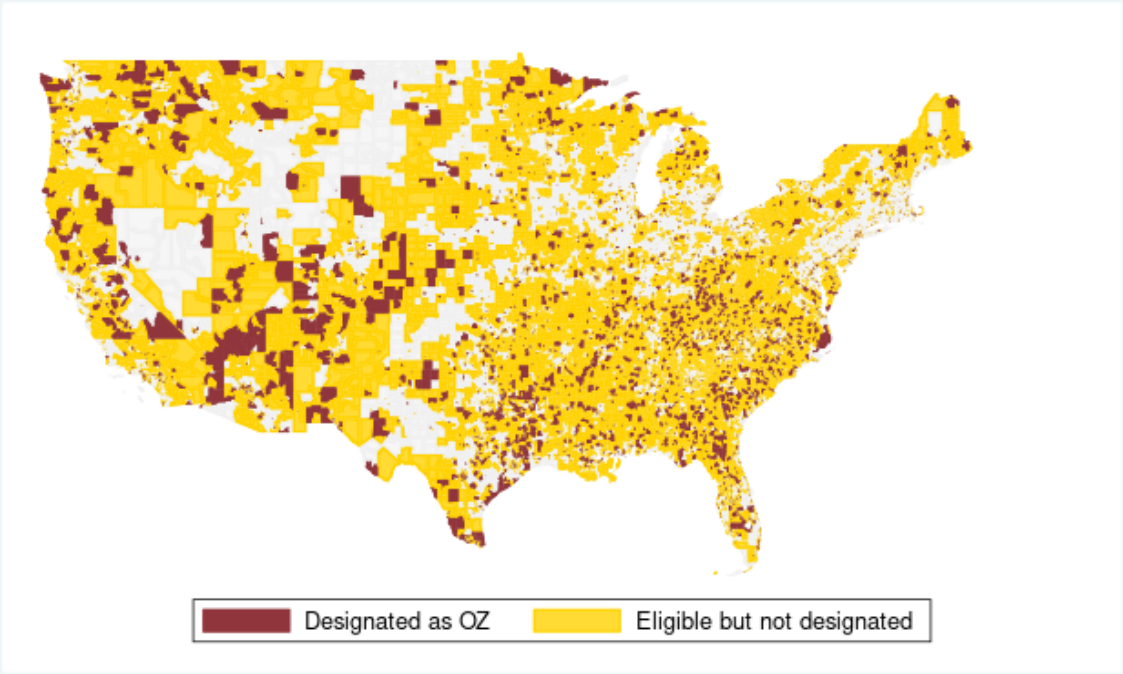
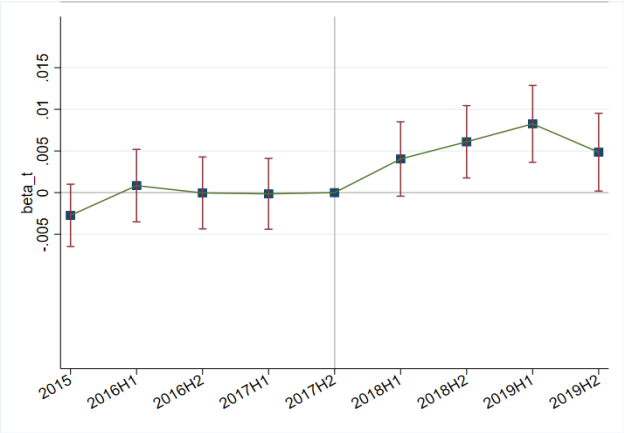


Figure 2. Semi-annual Coefficient Estimates Plotting

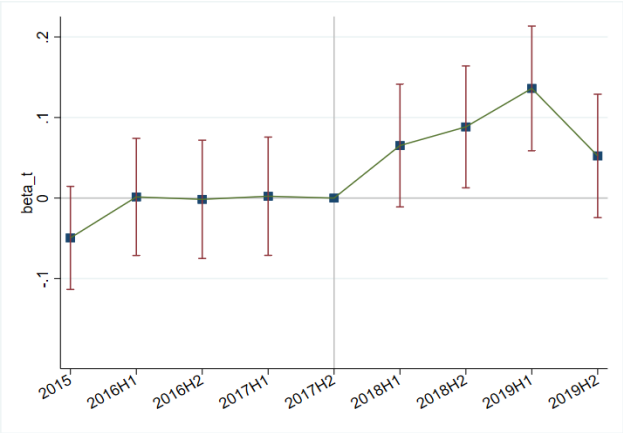
The figures show the coefficient plot around the Opportunity Zone policy by estimating the following model:

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

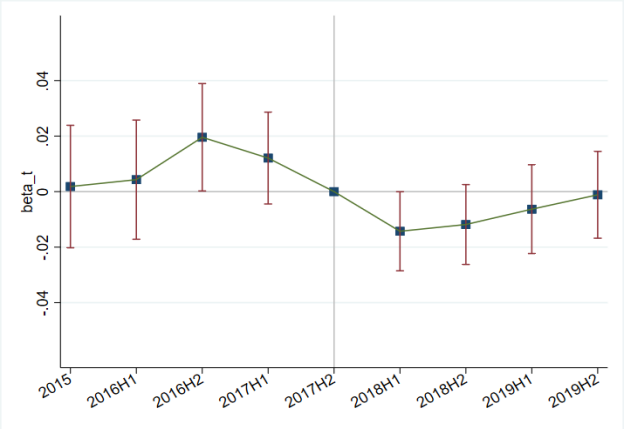
where $Semester_t$ is a set of indicator variables that equals one in semester t. The benchmark group comprises observations from the second semester of 2017 (2017H2) when the Opportunity Zone policy was signed into law. OZ_i is a dummy that equals one if census tract i was designated as an Opportunity 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*, $milions)	154,563	2.732	92.635	0.000	0.000	15,422.180
Number of New Firms (*Num_NewFirm*)	147,565	10.227	46.688	0.000	3.000	4,628.000
Number of New Firms Registered as Corporations	147,565	2.747	14.393	0.000	1.000	1,579.000
Number of New Firms Registered as LLCs	147,565	7.497	33.184	0.000	2.000	3,510.000
Number of New Firms Registered as Other Types	147,565	0.547	2.976	0.000	0.000	279.000
	Panel B: Control Variables					
Population (thousands)	154,492	4.046	1.892	0.011	3.801	40.616
Median Income ($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					
Emp. Share by *Age>1 & Subsidiary*	154,476	0.393	0.197	0.000	0.388	1.000

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Table 1 – Continued from previous page

Emp. Share by *Age≤1 & Standalone*	154,476	0.055	0.044	0.000	0.044	0.851
Emp. Share by *Others*	154,476	0.551	0.178	0.000	0.557	1.000
Sales Share by *Age>1 & Subsidiary*	154,476	0.451	0.234	0.000	0.454	1.000
Sales Share by *Age≤1 & Standalone*	154,476	0.051	0.075	0.000	0.029	1.000
Sales Share by *Others*	154,476	0.498	0.216	0.000	0.493	1.000
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
Total Employment (Thousands)	154,476	2.497	4.253	0.000	1.440	198.553
Employment by *Age>1 & Subsidiary*	154,476	1.242	2.793	0.000	0.530	128.057
Employment by *Age≤1 & Standalone*	154,476	0.099	0.141	0.000	0.065	10.122
Employment by *Others*	154,476	1.155	1.626	0.000	0.763	70.089
Total Sales ($Million)	154,476	400.210	3,939.252	0.000	160.574	815,350.900
Sales by *Age>1 & Subsidiary*	154,476	47.229	3,693.916	0.000	66.494	772,814.500
Sales by *Age≤1 & Standalone*	154,476	10.582	40.808	0.000	4.664	10,983.710
Sales by *Others*	154,476	142.399	389.569	0.000	70.613	41,402.800

Panel D: Other Economic Outcomes

Continued on next page

Table 1 – Continued from previous page

Number of Firms Dissolved (*Num_Dissolved*)	147,565	0.431	3.078	0.000	0.000	436.000
Number of Firms Dissolved, Age>1 Year	147,565	0.390	2.864	0.000	0.000	407.000
Number of Firms Dissolved, Age>2 Years	147,565	0.172	1.353	0.000	0.000	193.000
Number of All In-Migrants (*Num_migrants*)	154,486	685.838	581.916	0.000	543.000	15277.000
Gini Index (*Gini*)	154,278	0.443	0.063	0.001	0.436	0.903

Table 2. Impact of Opportunity Zones on Private Investment

This table shows the impact of the Opportunity Zone policy on local private investments. Specifically, I show the results of the DiD analysis by estimating the following model:

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

where i is a census tract, t represents a year. In Columns (1) to (4), 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 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 alone (*%White*), the percentage of black people alone (*%Black*), poverty rate (*Poverty_Rate*), unemployment rate (*Unemp_Rate*), and the percentage of people without a high-school degree (*%NoHighSchool*) 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)
	Ln(Num_Inv+1)		*Ln(Amount_Inv+1)*	
*OZ*Post*	0.011***	0.011***	0.151***	0.149***
	(0.002)	(0.002)	(0.032)	(0.032)
Population		0.017**		0.251**
		(0.008)		(0.120)
Median_Income		0.010		0.101
		(0.006)		(0.087)
Median_Age		0.003		0.106
		(0.008)		(0.120)
%White		0.000*		0.005***
		(0.000)		(0.002)
%Black		-0.000		0.001
		(0.000)		(0.003)
Poverty_Rate		0.000*		0.005**
		(0.000)		(0.002)
Unemp_Rate		-0.000		-0.001
		(0.000)		(0.002)
%NoHighSchool		0.000		0.000
		(0.000)		(0.002)
Constant	0.042***	-0.228**	0.589***	-3.347**
	(0.000)	(0.099)	(0.003)	(1.463)
Observations	154,563	154,492	154,563	154,492
R-squared	0.733	0.733	0.565	0.565
Tract FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Table 3. Impact of Opportunity Zones on New Business Formation

This table shows the impact of the Opportunity Zone policy on local new business formation. The dependent variable is the natural logarithm of the one plus the number of total new for-profit firms registered in census i and year t (*Ln(Num_NewFirm+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. 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)
	Ln(Num_NewFirm+1)	
*OZ*Post*	-0.018*	-0.019**
	(0.010)	(0.010)
Population		-0.040
		(0.039)
Median_Income		0.085***
		(0.025)
Median_Age		0.151***
		(0.044)
%White		-0.001*
		(0.001)
%Black		-0.005***
		(0.001)
Poverty_Rate		0.003***
		(0.001)
Unemp_Rate		0.001
		(0.001)
%NoHighSchool		-0.004***
		(0.001)
Constant	1.445***	0.525
	(0.001)	(0.463)
Observations	147,638	147,567
R-squared	0.849	0.849
Tract FE	YES	YES
Year FE	YES	YES

Table 4. Heterogeneous Impact on Local Entrepreneurship By Ex-Post Survival Period

This table shows the impact of the Opportunity Zone policy on local new business formation categorized by the length of time a new firm has lived before it died or by the end of the sample period. The dependent variables are the natural logarithm of the one plus the number of total new for-profit firms that survived for more than one year and those that did not. *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)
	Ln(Num_NewFirm+1)	
	Survive >1 Years	*Survive ≤1 Year*
*OZ*Post*	-0.023**	-0.002
	(0.010)	(0.002)
Population	-0.050	0.019**
	(0.040)	(0.008)
Median_Income	0.090***	-0.001
	(0.026)	(0.005)
Median_Age	0.150***	0.021**
	(0.044)	(0.008)
%White	-0.001*	-0.000
	(0.001)	(0.000)
%Black	-0.005***	-0.000*
	(0.001)	(0.000)
Poverty_Rate	0.003***	0.000
	(0.001)	(0.000)
Unemp_Rate	0.001*	0.000
	(0.001)	(0.000)
%NoHighSchool	-0.004***	0.000
	(0.001)	(0.000)
Constant	0.593	-0.168*
	(0.466)	(0.096)
Observations	147,565	147,565
R-squared	0.851	0.441
Tract FE	YES	YES
Year FE	YES	YES

Table 5. Heterogeneous Impact on Local Entrepreneurship By Business Structure

This table shows the impact of the Opportunity Zone policy on local new business formation by the types of firms incorporated. The dependent variables are the natural logarithm of one plus the number of total new for-profit firms registered in census i and year t as a corporation, the number of firms registered as a limited liability company, and other types of firms such as a sole proprietorship or partnership. *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)
	Ln(Num_NewFirm+1)		
	Corporation	*Limited Liability Company*	*Other*
*OZ*Post*	-0.023***	-0.016*	-0.010***
	(0.006)	(0.010)	(0.003)
Population	-0.037	-0.033	-0.016
	(0.025)	(0.039)	(0.014)
Median_Income	0.051***	0.079***	0.001
	(0.018)	(0.025)	(0.010)
Median_Age	0.095***	0.134***	0.001
	(0.029)	(0.043)	(0.015)
%White	-0.001**	-0.001	0.000
	(0.000)	(0.001)	(0.000)
%Black	-0.004***	-0.004***	0.000
	(0.001)	(0.001)	(0.000)
Poverty_Rate	0.002***	0.003***	-0.000
	(0.000)	(0.001)	(0.000)
Unemp_Rate	0.001**	0.001	-0.000*
	(0.001)	(0.001)	(0.000)
%NoHighSchool	-0.002***	-0.003***	-0.000
	(0.001)	(0.001)	(0.000)
Constant	0.315	0.358	0.327*
	(0.307)	(0.463)	(0.168)
Observations	147,565	147,565	147,565
R-squared	0.809	0.828	0.772
Tract FE	YES	YES	YES
Year FE	YES	YES	YES

Table 6. 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)*) and the natural logarithm of one plus the total amount of private investments (*Ln(Amount_Inv+1)*) received by firms in a census tract i and year t, respectively. *Old* is a dummy which equals one for investments received by firms who are at least one year old. *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)
	Ln(Num_Inv+1)	*Ln(Amount_Inv+1)*
*OZ*Post*Old*	0.022***	0.333***
	(0.002)	(0.033)
*OZ*Post*	-0.005***	-0.079***
	(0.001)	(0.019)
Old	0.030***	0.428***
	(0.001)	(0.010)
Population	0.011**	0.171**
	(0.005)	(0.071)
Median_Income	0.007*	0.079
	(0.003)	(0.050)
Median_Age	0.002	0.072
	(0.004)	(0.069)
%White	0.000	0.003**
	(0.000)	(0.001)
%Black	-0.000	-0.000
	(0.000)	(0.002)
Poverty_Rate	0.000*	0.003**
	(0.000)	(0.001)
Unemp_Rate	-0.000	-0.001
	(0.000)	(0.001)
%NoHighSchool	-0.000	0.000
	(0.000)	(0.001)
Constant	-0.169***	-2.561***
	(0.057)	(0.862)
Observations	308,994	308,994
R-squared	0.520	0.393
Tract FE	YES	YES
Year FE	YES	YES

Table 7. Heterogeneous 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_NewFirm+1)			
	Non-tradable	*Tradable*	*Construction*	*Other*
*OZ*Post*	-0.016***	-0.003	-0.005	-0.021**
	(0.005)	(0.002)	(0.006)	(0.009)
Population	0.014	-0.007	-0.021	-0.053
	(0.018)	(0.007)	(0.023)	(0.038)
Median_Income	-0.001	0.008	0.025	0.084***
	(0.012)	(0.005)	(0.016)	(0.025)
Median_Age	-0.000	0.004	0.068***	0.140***
	(0.020)	(0.008)	(0.026)	(0.042)
%White	-0.000	-0.000	-0.000	-0.001*
	(0.000)	(0.000)	(0.000)	(0.001)
%Black	-0.001**	-0.000	-0.002***	-0.005***
	(0.000)	(0.000)	(0.001)	(0.001)
Poverty_Rate	0.000	0.000*	0.000	0.003***
	(0.000)	(0.000)	(0.000)	(0.001)
Unemp_Rate	0.000	-0.000	-0.000	0.002**
	(0.000)	(0.000)	(0.000)	(0.001)
%NoHighSchool	-0.001	0.000	-0.002***	-0.003***
	(0.000)	(0.000)	(0.000)	(0.001)
Constant	0.184	0.000	0.252	0.575
	(0.216)	(0.090)	(0.282)	(0.452)
Observations	147,565	147,565	147,565	147,565
R-squared	0.568	0.388	0.722	0.847
Tract FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Table 8. Impact on Local Competition in Employment and Sales

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 9. Share of Employment and Sales by Firm Age and Ownership

This table shows the impact of the Opportunity Zone policy on the share of employment and sales generated 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 fraction of jobs supported by a specific type of firm to the total number of jobs in a census tract in a year (*Employment_Share*). In Panel B, the dependent variable is the fraction of sales generated by a specific type of firm to the total sales in a census tract in a year (*Sales_Share*). 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: *Employment_Share*		
	(1)	(2)	(3)
	Age>1 & Subsidiary	Age≤1 & Standalone	Others
*OZ*Post*	0.003***	-0.005***	0.002***
	(0.000)	(0.000)	(0.001)
Observations	154,476	154,476	154,476
R-squared	0.973	0.650	0.955
Controls	YES	YES	YES
Tract FE	YES	YES	YES
Year FE	YES	YES	YES

	Panel B: *Sales_Share*		
	(1)	(2)	(3)
	Age>1 & Subsidiary	Age≤1 & Standalone	Others
*OZ*Post*	0.005***	-0.004***	-0.001
	(0.001)	(0.001)	(0.001)
Observations	154,476	154,476	154,476
R-squared	0.949	0.382	0.897
Controls	YES	YES	YES
Tract FE	YES	YES	YES
Year FE	YES	YES	YES

Table 10. Impact on Local Incumbents' 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)
Population	-0.095***	-0.096***	-0.086***
	(0.018)	(0.018)	(0.013)
Median_Income	-0.024*	-0.025**	-0.028***
	(0.012)	(0.012)	(0.009)
Median_Age	-0.062***	-0.061***	-0.038**
	(0.021)	(0.021)	(0.015)
%White	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
%Black	-0.001	-0.000	-0.000
	(0.000)	(0.000)	(0.000)
Poverty_Rate	-0.001***	-0.001**	-0.000**
	(0.000)	(0.000)	(0.000)
Unemp_Rate	-0.004***	-0.003***	-0.002***
	(0.000)	(0.000)	(0.000)
%NoHighSchool	0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)
Constant	1.435***	1.436***	1.234***
	(0.225)	(0.224)	(0.164)
Observations	147,565	147,565	147,565
R-squared	0.510	0.483	0.437
Tract FE	YES	YES	YES
Year FE	YES	YES	YES

Table 11. 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 12. Spillover Effects on Nearby Eligible Tracts

This table shows the test examining the spillover effects of Opportunity Zones on nearby eligible-but-non-designated tracts in the same county by estimating the following model:

$$Y_{i,t} = \alpha + \beta CloseToOZ_i * Post_t + Controls_{i,t-1} + \delta_{c,t} + \eta_i + \epsilon_{i,t}. \quad (4)$$

where $CloseToOZ_i$ is defined as the natural logarithm of the reciprocal of an eligible tract i's distance to the closest Opportunity Zone tract within the same county if there is any. $Y_{i,t}$ includes variables related to private investments, new business formation, as well as local employment and sales that are defined in the previous tables. Control variables are the same as in the baseline regressions. Tract fixed effects and county-by-year fixed effects are included. β in Equation (4) measures the spillover effects: A positive β suggests that the closer an eligible tract is located to an Opportunity Zone, the higher the outcome is. 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)
	Ln(Num_Inv+1)	*Ln(Amount_Inv+1)*	*Ln(Num_NewFirm+1)*	*Ln(Employment+1)*	*Ln(Sales+1)*
*CloseToOZ*Post*	0.002	0.037	0.148***	0.045***	0.033***
	(0.002)	(0.038)	(0.009)	(0.004)	(0.002)
Observations	105,622	105,622	102,052	107,135	107,135
R-squared	0.612	0.570	0.850	0.980	0.994
Controls	YES	YES	YES	YES	YES
Tract FE	YES	YES	YES	YES	YES
County*Year FE	YES	YES	YES	YES	YES

Internet Appendix

Can Capital Inflows Stifle Local Entrepreneurship?

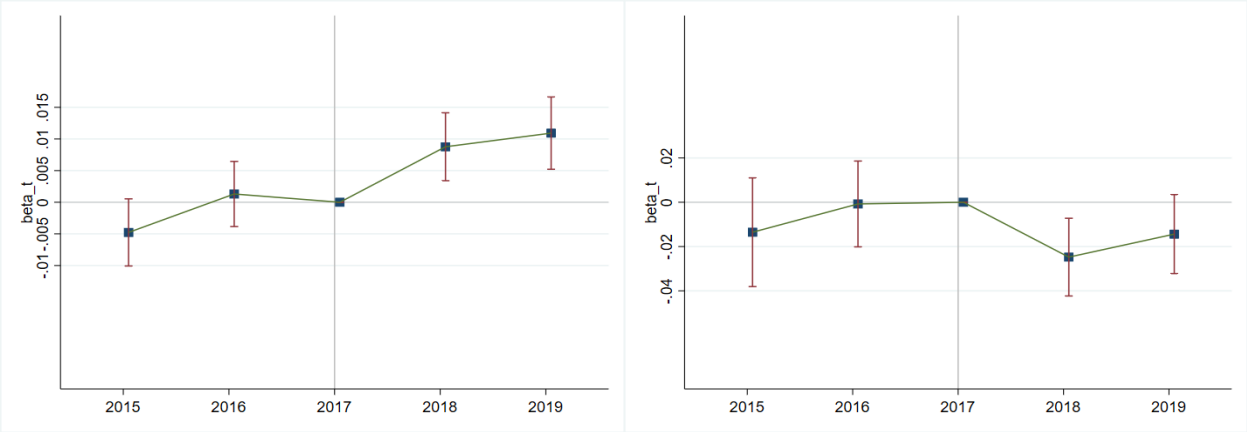
A Additional Tests

Figure A1. Annual Coefficient Estimates Plotting

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

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

where $Year_t$ is a set of year indicator variables that equals one in year t. The benchmark group comprises observations from 2017, 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 The outcome variable in Panel (b) 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 year fixed effects are included. Standard errors are clustered by census tracts.



((a)) Number of Private Investments

((b)) Number of New Firms

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

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

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Table A1 – Continued from previous page

	Freq.	Percent
Maryland	407	1.45
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
- *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

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Table A1 – Continued from previous page

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

Table A2. 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)
Population	0.010	0.005	0.011**	0.204*	0.129*	0.087
	(0.007)	(0.005)	(0.005)	(0.112)	(0.071)	(0.069)
Median_Income	0.006	0.002	0.007*	0.078	0.058	0.074*
	(0.005)	(0.003)	(0.004)	(0.078)	(0.047)	(0.045)
Median_Age	-0.000	0.005	0.004	0.092	0.103	0.042
	(0.006)	(0.004)	(0.004)	(0.104)	(0.065)	(0.059)
%White	0.000*	0.000	0.000	0.003*	0.001	0.001
	(0.000)	(0.000)	(0.000)	(0.002)	(0.001)	(0.001)
%Black	0.000	-0.000*	-0.000	0.002	-0.002	-0.000
	(0.000)	(0.000)	(0.000)	(0.003)	(0.002)	(0.001)
Poverty_Rate	0.000	0.000	0.000	0.003	0.001	0.001
	(0.000)	(0.000)	(0.000)	(0.002)	(0.001)	(0.001)
Unemp_Rate	-0.000	0.000	-0.000	-0.001	0.001	0.000
	(0.000)	(0.000)	(0.000)	(0.002)	(0.001)	(0.001)
%NoHighSchool	0.000	-0.000	0.000	-0.000	-0.001	0.002
	(0.000)	(0.000)	(0.000)	(0.002)	(0.001)	(0.001)
Constant	-0.127	-0.070	-0.169***	-2.652**	-1.965**	-1.640**
	(0.084)	(0.057)	(0.064)	(1.337)	(0.863)	(0.830)
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
Tract FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES

Table A3. Impact of Opportunity Zones on Private Investment: Include All Eligible Tracts

This table shows the impact of the Opportunity Zone policy on local private investments with all the eligible census tracts included (both low-income communities (LIC) and non-LIC but contiguous tracts). 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 (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 census tract fixed effects. Standard errors are clustered at the tract level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	Ln(Num_Inv+1)	*Ln(Num_Inv+1)*	*Ln(Amount_Inv+1)*	*Ln(Amount_Inv+1)*
*OZ*Post*	0.010***	0.010***	0.143***	0.144***
	(0.002)	(0.002)	(0.031)	(0.031)
Population		0.020***		0.257**
		(0.008)		(0.113)
Median_Income		0.011**		0.134*
		(0.005)		(0.080)
Median_Age		-0.001		0.019
		(0.007)		(0.111)
%White		0.000**		0.005***
		(0.000)		(0.002)
%Black		-0.000		0.000
		(0.000)		(0.003)
Poverty_Rate		0.000		0.003*
		(0.000)		(0.002)
Unemp_Rate		-0.000		-0.000
		(0.000)		(0.002)
%NoHighSchool		0.000		0.002
		(0.000)		(0.002)
Constant	0.043***	-0.253***	0.626***	-3.426**
	(0.000)	(0.092)	(0.002)	(1.366)
Observations	205,876	205,800	205,876	205,800
R-squared	0.739	0.739	0.580	0.580
Tract FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Table A4. Impact of Opportunity Zones on Local Private Investment: Alternative Options of Fixed Effects

This table shows the impact of the Opportunity Zone policy on local private investments under alternative options of fixed effects. In Columns (1) and (4), county fixed effects and fixed effects are included. In Columns (2) and (5), I include county fixed effects as well as the state-year fixed effects. In Columns (3) and (6), 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)*) 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. 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)*		
*OZ*Post*	0.010***	0.009***	0.011***	0.134***	0.129***	0.147***
	(0.002)	(0.002)	(0.002)	(0.032)	(0.032)	(0.032)
OZ	0.030***	0.030***		0.427***	0.428***	
	(0.003)	(0.003)		(0.036)	(0.036)	
Population	-0.007***	-0.012***	0.016*	-0.054*	-0.110***	0.226*
	(0.003)	(0.003)	(0.008)	(0.029)	(0.030)	(0.121)
Median_Income	0.049***	0.040***	0.009	0.447***	0.347***	0.092
	(0.009)	(0.009)	(0.006)	(0.081)	(0.084)	(0.087)
Median_Age	-0.039***	-0.037***	0.001	-0.597***	-0.572***	0.095
	(0.008)	(0.008)	(0.008)	(0.080)	(0.081)	(0.121)
%White	-0.001***	-0.001***	0.000*	-0.009***	-0.010***	0.005***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.002)
%Black	-0.001***	-0.001***	-0.000	-0.011***	-0.011***	0.001
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.003)
Poverty_Rate	0.002***	0.002***	0.000	0.025***	0.024***	0.004**
	(0.000)	(0.000)	(0.000)	(0.002)	(0.002)	(0.002)
Unemp_Rate	-0.002***	-0.003***	-0.000	-0.029***	-0.032***	-0.001
	(0.000)	(0.000)	(0.000)	(0.002)	(0.002)	(0.002)
%NoHighSchool	-0.002***	-0.003***	0.000	-0.030***	-0.032***	0.000
	(0.000)	(0.000)	(0.000)	(0.001)	(0.002)	(0.002)
Constant	-0.191*	-0.055	-0.204**	-0.461	1.105	-3.014**
	(0.098)	(0.101)	(0.099)	(0.956)	(0.978)	(1.469)
Observations	154,492	154,492	154,492	154,492	154,492	154,492
R-squared	0.037	0.046	0.733	0.035	0.044	0.566
Year FE	YES	-	-	YES	-	-
County FE	YES	YES	-	YES	YES	-
Tract FE	-	-	YES	-	-	YES
State*Year FE	-	YES	YES	-	YES	YES

Table A5. Impact of Opportunity Zones on Local Private Investment: Alternative Options of Clustering the Standard Errors

This table shows the impact of the Opportunity Zone policy on local private investments under 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)*) 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 (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)
	Ln(Num_Inv+1)			*Ln(Amount_Inv+1)*		
*OZ*Post*	0.011***	0.011***	0.011**	0.158***	0.158***	0.158***
	(0.002)	(0.002)	(0.002)	(0.029)	(0.030)	(0.029)
Population	0.018*	0.018	0.018	0.270*	0.270	0.270*
	(0.010)	(0.010)	(0.009)	(0.150)	(0.138)	(0.122)
Median_Income	0.010*	0.010	0.010	0.112	0.112	0.112
	(0.006)	(0.006)	(0.006)	(0.093)	(0.123)	(0.114)
Median_Age	0.003	0.003	0.003	0.105	0.105	0.105
	(0.007)	(0.010)	(0.010)	(0.104)	(0.148)	(0.150)
%White	0.000*	0.000	0.000	0.005***	0.005**	0.005
	(0.000)	(0.000)	(0.000)	(0.002)	(0.002)	(0.003)
%Black	-0.000	-0.000	-0.000	0.001	0.001	0.001
	(0.000)	(0.000)	(0.000)	(0.003)	(0.002)	(0.003)
Poverty_Rate	0.000**	0.000	0.000	0.005**	0.005	0.005
	(0.000)	(0.000)	(0.000)	(0.002)	(0.003)	(0.003)
Unemp_Rate	-0.000	-0.000	-0.000	-0.001	-0.001	-0.001
	(0.000)	(0.000)	(0.000)	(0.002)	(0.003)	(0.003)
%NoHighSchool	0.000	0.000	0.000	0.001	0.001	0.001
	(0.000)	(0.000)	(0.000)	(0.002)	(0.004)	(0.005)
Constant	-0.230**	-0.230	-0.230	-3.612**	-3.612	-3.612
	(0.096)	(0.141)	(0.137)	(1.554)	(2.324)	(2.051)
Observations	154,492	154,492	154,492	154,492	154,492	154,492
R-squared	0.733	0.733	0.733	0.579	0.579	0.579
Tract FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Cluster of SE	County	County*Year	State*Year	County	County*Year	State*Year

Table A6. 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)
Population	0.008	0.150	-0.009
	(0.169)	(0.229)	(0.052)
Median_Income	0.030	0.013	0.114***
	(0.112)	(0.141)	(0.032)
Median_Age	-0.006	0.109	0.125**
	(0.257)	(0.356)	(0.056)
%White	0.006**	0.009**	-0.001
	(0.003)	(0.004)	(0.001)
%Black	-0.000	0.002	-0.004***
	(0.004)	(0.006)	(0.001)
Poverty_Rate	0.009**	0.011**	0.002**
	(0.004)	(0.005)	(0.001)
Unemp_Rate	-0.009*	-0.010	0.002
	(0.005)	(0.007)	(0.001)
%NoHighSchool	0.002	0.010*	-0.002**
	(0.004)	(0.005)	(0.001)
Constant	-1.232	12.805***	1.566**
	(2.128)	(2.901)	(0.581)
Observations	154,492	154,492	147,565
Tract FE	YES	YES	YES
Year FE	YES	YES	YES

Table A7. Impact of Opportunity Zones on Local Private Investment: By the 2016 Presidential Election Winning Party

This table 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 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)
Population	0.042***	0.002	0.492**	0.101
	(0.015)	(0.009)	(0.216)	(0.143)
Median_Income	-0.002	0.018**	-0.074	0.223*
	(0.009)	(0.008)	(0.126)	(0.119)
Median_Age	0.002	0.002	0.110	0.095
	(0.013)	(0.010)	(0.210)	(0.147)
%White	0.001***	-0.000	0.008***	0.001
	(0.000)	(0.000)	(0.003)	(0.003)
%Black	-0.000	-0.001*	0.004	-0.003
	(0.000)	(0.000)	(0.004)	(0.004)
Poverty_Rate	-0.000	0.001***	-0.002	0.008***
	(0.000)	(0.000)	(0.004)	(0.003)
Unemp_Rate	0.000	-0.000	0.002	-0.003
	(0.000)	(0.000)	(0.004)	(0.003)
%NoHighSchool	0.000	0.000	0.001	-0.000
	(0.000)	(0.000)	(0.004)	(0.003)
Constant	-0.301*	-0.162	-3.496	-3.155*
	(0.168)	(0.123)	(2.460)	(1.834)
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
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 A8. 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 A9. Impact of Opportunity Zones on Local Private Investment: 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)
	Ln(Num_Inv+1)		*Ln(Amount_Inv+1)*	
*OZ*Post*	0.013***	0.013***	0.210***	0.210***
	(0.003)	(0.003)	(0.039)	(0.039)
Population		0.019		0.348*
		(0.013)		(0.201)
Median_Income		0.014		0.244*
		(0.010)		(0.138)
Median_Age		-0.001		0.139
		(0.012)		(0.179)
%White		0.000		0.005*
		(0.000)		(0.003)
%Black		-0.000		0.001
		(0.000)		(0.004)
Poverty_Rate		0.000		0.008**
		(0.000)		(0.003)
Unemp_Rate		-0.000		-0.001
		(0.000)		(0.003)
%NoHighSchool		0.000		0.006*
		(0.000)		(0.004)
Constant	0.052***	-0.265*	0.751***	-5.780**
	(0.001)	(0.161)	(0.008)	(2.375)
Observations	76,043	76,030	76,043	76,030
R-squared	0.737	0.737	0.593	0.594
Tract FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
PSM	YES	YES	YES	YES
# of Tracts	15210	15210	15210	15210

Table A10. Impact of Opportunity Zones on Private Investment: 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) and (2), I drop census tracts that are located in counties with at least one university that has a population of over 10,000. In Columns (3) and (4), 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)*) 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)
Sample:	Drop Counties w/ Colleges of 10K Pop		Drop Census Tracts w/ Colleges	
Dependent Variable:	*Ln(Num_Inv+1)*	*Ln(Amount_Inv+1)*	*Ln(Num_Inv+1)*	*Ln(Amount_Inv+1)*
*OZ*Post*	0.005**	0.097***	0.010***	0.148***
	(0.002)	(0.037)	(0.002)	(0.032)
Population	0.029***	0.458***	0.017**	0.239**
	(0.010)	(0.163)	(0.008)	(0.120)
Median_Income	-0.005	-0.133	0.011**	0.126
	(0.006)	(0.104)	(0.006)	(0.084)
Median_Age	0.006	0.158	0.004	0.124
	(0.009)	(0.149)	(0.008)	(0.121)
%White	0.000**	0.006*	0.000*	0.005***
	(0.000)	(0.003)	(0.000)	(0.002)
%Black	0.000	0.004	-0.000	0.001
	(0.000)	(0.004)	(0.000)	(0.003)
Poverty_Rate	0.000	0.001	0.000**	0.006***
	(0.000)	(0.003)	(0.000)	(0.002)
Unemp_Rate	-0.000	-0.004	-0.000	-0.001
	(0.000)	(0.003)	(0.000)	(0.002)
%NoHighSchool	-0.000	-0.002	-0.000	0.000
	(0.000)	(0.003)	(0.000)	(0.002)
Constant	-0.223*	-3.080	-0.244**	-3.625**
	(0.114)	(1.880)	(0.096)	(1.446)
Observations	68,935	68,935	150,049	150,049
R-squared	0.573	0.484	0.703	0.559
Tract FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Table A11. Impact of Opportunity Zones on Private Investment: Excluding Neighborhoods with Multinational Company Branches

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). In Columns (1) and (2), I drop census tracts that have at least ten branches of MNCs or have any MNC headquarters. In Columns (3) and (4), I drop census tracts that have at least five branches of MNCs or have any MNC headquarters. The dependent variables are the natural logarithm of 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)*). 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)	(4)
Sample:	Drop Tracts Have ≥ 10 MNC Branches		Drop Tracts Have ≥ 5 MNC Branches	
Dependent Variable:	*Ln(Num_Inv+1)*	*Ln(Amount_Inv+1)*	*Ln(Num_Inv+1)*	*Ln(Amount_Inv+1)*
*OZ*Post*	0.007***	0.099***	0.007***	0.102***
	(0.002)	(0.030)	(0.002)	(0.031)
Population	0.009	0.169	0.008	0.169
	(0.007)	(0.110)	(0.007)	(0.106)
Median_Income	0.004	0.078	0.004	0.085
	(0.005)	(0.076)	(0.005)	(0.076)
Median_Age	0.008	0.186*	0.007	0.139
	(0.007)	(0.109)	(0.007)	(0.108)
%White	0.000	0.004**	0.000***	0.006***
	(0.000)	(0.002)	(0.000)	(0.002)
%Black	-0.000	0.001	0.000	0.004
	(0.000)	(0.003)	(0.000)	(0.003)
Poverty_Rate	0.000	0.003	0.000*	0.004*
	(0.000)	(0.002)	(0.000)	(0.002)
Unemp_Rate	-0.000	0.001	-0.000	0.000
	(0.000)	(0.002)	(0.000)	(0.002)
%NoHighSchool	-0.000	-0.000	0.000	0.002
	(0.000)	(0.002)	(0.000)	(0.002)
Constant	-0.134	-2.803**	-0.132*	-3.051**
	(0.084)	(1.316)	(0.079)	(1.298)
Observations	134,027	134,027	113,732	113,732
R-squared	0.564	0.474	0.540	0.462
Tract FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Table A12. Impact of Opportunity Zones on Private Investment: Nearby versus 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) and (5), I only include eligible but non-designated census tracts that are located within 3 kilometers of an Opportunity Zone; In Columns (2) and (6), I include control census tracts that are located at least 3 kilometers away from an Opportunity Zone. Similarly, the cutoff point is replaced as 5 kilometers instead of 3 kilometers in Columns (3), (4), (7), (8). The dependent variables are the natural logarithm of 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)	(7)	(8)
		Ln(Num_Inv+1)				*Ln(Amount_Inv+1)*		
*OZ*Post*	0.010***	0.012***	0.010***	0.012***	0.165***	0.149***	0.168***	0.151***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.035)	(0.033)	(0.034)	(0.033)
Population	0.019**	0.016	0.018**	0.017	0.312*	0.290**	0.296*	0.281**
	(0.009)	(0.010)	(0.008)	(0.011)	(0.164)	(0.131)	(0.159)	(0.129)
Median_Income	0.010	0.016**	0.010	0.015**	0.176	0.105	0.183*	0.112
	(0.007)	(0.007)	(0.006)	(0.008)	(0.111)	(0.095)	(0.108)	(0.093)
Median_Age	0.003	0.007	0.003	0.006	0.249	0.112	0.251	0.118
	(0.009)	(0.010)	(0.008)	(0.011)	(0.165)	(0.133)	(0.159)	(0.130)
%White	0.000*	0.000	0.000*	0.000	0.003	0.006***	0.003	0.005**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.003)	(0.002)	(0.003)	(0.002)
%Black	-0.000	-0.000	-0.000	-0.000	-0.001	0.001	-0.002	0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.004)	(0.003)	(0.004)	(0.003)
Poverty_Rate	0.000	0.000*	0.000	0.000*	0.006**	0.005**	0.006**	0.005**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.003)	(0.002)	(0.003)	(0.002)
Unemp_Rate	-0.000	-0.000	-0.000	-0.000	-0.002	-0.001	-0.002	-0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.003)	(0.002)	(0.003)	(0.002)
%NoHighSchool	0.000	0.000*	0.000	0.000*	0.006*	0.000	0.006*	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.003)	(0.003)	(0.003)	(0.003)
Constant	-0.235**	-0.288**	-0.236**	-0.291**	-5.060***	-3.683**	-4.988***	-3.691**
	(0.107)	(0.126)	(0.105)	(0.130)	(1.954)	(1.600)	(1.894)	(1.567)
Observations	136,864	98,360	141,124	94,100	94,100	136,864	98,360	141,124
R-squared	0.736	0.737	0.735	0.739	0.594	0.583	0.591	0.581
Tract FE	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Distance of Control Tract to an OZ	<3 km	≥3 km	<5 km	≥5 km	<3 km	≥3 km	<5 km	≥5 km

Table A13. 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)
Population	0.217***	0.160***	0.110***	-0.043***	-0.050***
	(0.015)	(0.013)	(0.005)	(0.002)	(0.002)
Med_Income	0.490***	0.423***	0.137***	0.068***	0.059***
	(0.056)	(0.051)	(0.025)	(0.015)	(0.012)
%White	-0.006***	-0.005***	-0.003***	-0.001***	-0.000**
	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)
%Black	-0.006***	-0.005***	-0.003***	-0.001***	-0.001***
	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)
Poverty_Rate	0.002	0.001	0.003***	0.000	0.000
	(0.002)	(0.001)	(0.001)	(0.001)	(0.000)
Unemp_Rate	-0.004**	-0.001	-0.004***	0.001*	-0.000
	(0.002)	(0.002)	(0.001)	(0.000)	(0.000)
NoHighSchool	-0.001	0.000	-0.003***	-0.000	0.000
	(0.002)	(0.002)	(0.001)	(0.000)	(0.000)
Constant	-6.739***	-5.744***	-2.358***	-0.351**	-0.204
	(0.622)	(0.593)	(0.274)	(0.163)	(0.129)
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 A14. Impact of Opportunity Zones (OZs) on New Business Formation: Restricting OZs That Have Received Investments

This table shows the impact of the Opportunity Zone 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) OLS *Ln(Num_NewFirm+1)*	(2) Poisson *Num_NewFirm*
*OZ*Post*	-0.044*	-0.043**
	(0.023)	(0.017)
Population	-0.063	-0.025
	(0.044)	(0.057)
Median_Income	0.116***	0.148***
	(0.029)	(0.035)
Median_Age	0.171***	0.156**
	(0.051)	(0.063)
%White	-0.001	-0.001
	(0.001)	(0.001)
%Black	-0.004***	-0.004***
	(0.001)	(0.001)
Poverty_Rate	0.003***	0.002**
	(0.001)	(0.001)
Unemp_Rate	0.002**	0.002*
	(0.001)	(0.001)
%NoHighSchool	-0.003***	-0.001
	(0.001)	(0.001)
Constant	0.308	1.247**
	(0.521)	(0.630)
Observations	116,482	116,482
R-squared	0.854	-
Tract FE	YES	YES
Year FE	YES	YES

Table A15. Using Non-Imputed NETS Data

This table shows the results of replicating Tables 8, 9, and 11 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 8): *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

Continued on next page

Table A15 - Continued from previous page

	Panel B (replicating Table 9): *Employment_Share*		
	(1)	(2)	(3)
	Age>1 & Subsidiary	Age≤1 & Standalone	Others
*OZ*Post*	0.004***	-0.005***	0.001*
	(0.001)	(0.000)	(0.001)
Observations	154,465	154,465	154,465
R-squared	0.974	0.660	0.956
Controls	YES	YES	YES
Tract FE	YES	YES	YES
Year FE	YES	YES	YES

	Panel C (replicating Table 11): *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

Table A16. Impact of Opportunity Zones on Geographical Mobility

This table shows the impact of the Opportunity Zone policy on immigrants who moved in within the last year from different backgrounds. The dependent variable in Column (1) is the natural logarithm of one plus the total number of movers into census tract i and year t within the last year (*Ln(Num_migrants+1)*). In Columns (2) to (4), the dependent variables are the number of move-ins by education: movers that do not have a high-school degree, have a high-school degree, and have a bachelor's degree or above, respectively. In Columns (5) to (7), the dependent variables are the number of move-ins by their poverty status: movers that are below 100 percent of the poverty level, 100 to 149 percent of the poverty level, at or above 150 percent of the poverty level, 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 before 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)	(4)	(5)	(6)	(7)
	Ln(Num_immigrants+1)						
		Education			Relative to Poverty Level		
	All Move-ins	Less than High School	High School	Bachelor's or Above	< 100%	[100%, 149%]	≥ 150%
*OZ*Post*	-0.007	0.001	0.004	0.009	0.009	0.005	0.009
	(0.004)	(0.012)	(0.009)	(0.012)	(0.010)	(0.015)	(0.007)
Population	0.575***	0.549***	0.620***	0.456***	0.496***	0.575***	0.545***
	(0.028)	(0.053)	(0.041)	(0.052)	(0.047)	(0.064)	(0.041)
Median_Income	0.023	0.024	-0.032	0.117***	-0.017	-0.440***	0.245***
	(0.015)	(0.035)	(0.026)	(0.034)	(0.028)	(0.043)	(0.024)
Median_Age	-0.188***	0.171***	0.124***	0.071	-0.328***	-0.474***	-0.077**
	(0.024)	(0.055)	(0.040)	(0.055)	(0.046)	(0.070)	(0.035)
%White	-0.001***	-0.002*	-0.000	-0.002**	-0.001	-0.001	-0.001
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)
%Black	-0.001**	-0.000	0.001	-0.005***	-0.001	0.001	-0.002**
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Poverty_Rate	0.002***	0.003***	0.001	-0.002**	0.023***	-0.017***	-0.004***
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Unemp_Rate	0.003***	0.002**	0.005***	0.001	0.005***	0.006***	0.001
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
%NoHighSchool	0.000	0.031***	-0.005***	-0.006***	0.000	0.005***	-0.001*
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Constant	1.976***	-2.377***	-0.890*	-1.292**	1.622***	5.568***	-1.197***
	(0.295)	(0.628)	(0.472)	(0.599)	(0.532)	(0.753)	(0.452)
Observations	154,486	154,486	154,486	154,486	154,486	154,486	154,486
R-squared	0.923	0.785	0.811	0.845	0.814	0.735	0.885
Tract FE	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES

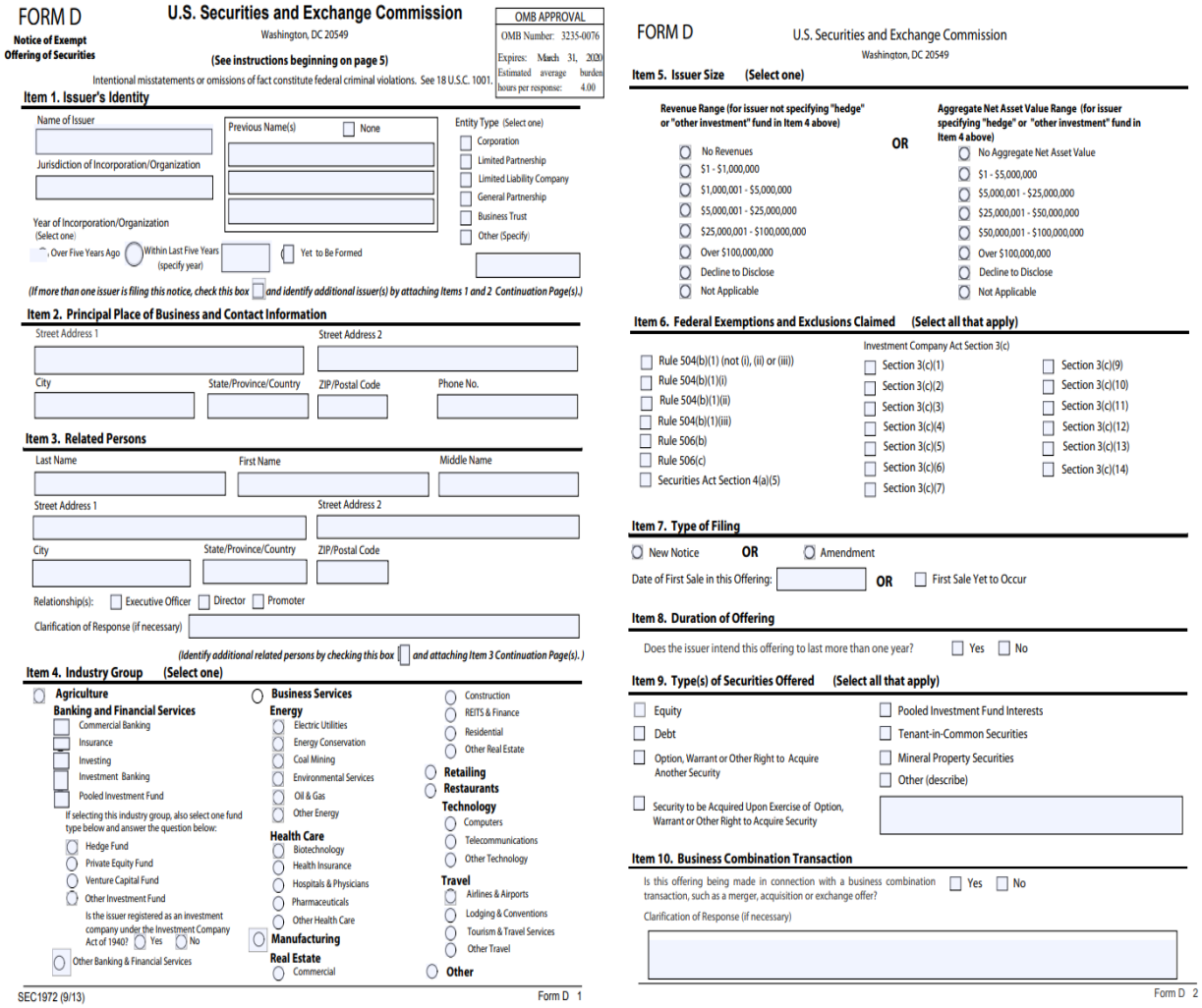
Table A17. The Impact of Opportunity Zones on Local Inequality

This table shows the impact of the Opportunity Zone policy on local inequality measured by the Gini coefficient. The dependent variables are census-tract-level and county-level Gini coefficients. The data are collected from the American Community Survey. In the tract-level regressions, *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. In the county-level regressions, *OZ%* is a continuous variable that equals the percentage of the population that resides in Opportunity Zones of the total population that resides in the eligible census tracts in the same county. *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 (or county) fixed effects. Standard errors are clustered at the tract level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(1)	(2)
	Gini coefficient			
	Tract-level		County-level	
*OZ*Post*	0.000	0.001		
	(0.001)	(0.000)		
*%OZ*Post*			0.000	-0.000
			(0.002)	(0.002)
Population		0.003		0.017
		(0.003)		(0.018)
Median_Income		-0.046***		-0.048***
		(0.002)		(0.007)
Median_Age		0.012***		0.005
		(0.002)		(0.017)
%White		0.000**		0.000
		(0.000)		(0.000)
%Black		-0.000		0.000
		(0.000)		(0.000)
Poverty_Rate		0.001***		0.001***
		(0.000)		(0.000)
Unemp_Rate		-0.000		-0.001***
		(0.000)		(0.000)
%NoHighSchool		-0.000***		-0.000
		(0.000)		(0.000)
Constant	0.443***	0.847***	0.445***	0.731***
	(0.000)	(0.034)	(0.000)	(0.206)
Observations	154,278	154,259	15,709	15,706
R-squared	0.837	0.843	0.880	0.885
Tract FE	YES	YES	-	-
County FE	-	-	YES	YES
Year FE	YES	YES	YES	YES

B 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.[40] 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

(See instructions beginning on page 5)

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

OMB APPROVAL

OMB Number: 3235-0076

Expires: March 31, 2009

Estimated average burden hours per response: 4.00

Item 1. Issuer's Identity

Name of Issuer

Previous Name(s) None

Jurisdiction of Incorporation/Organization

Year of Incorporation/Organization (Select one)

Over Five Years Ago Within Last Five Years (specify year) Yet to Be Formed

Entity Type (Select one)

Corporation
Limited Partnership
Limited Liability Company
General Partnership
Business Trust
Other (Specify)

(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

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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

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