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Place-Based Policy for Entrepreneurship: Evidence from Opportunity Zones

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March 2026

Abstract

The Opportunity Zone program is currently the largest place-based program in the United States. Leveraging restricted-access data for the universe of new firms, we examine the OZ program's impacts on startup activity at the local level. We use a matching approach that compares entrepreneurship outcomes in designated zones to similar eligible but not designated areas. We are actively working on the project in a Federal Statistical Research Data Center (FSRDC), and as such cannot yet share full results. In this extended abstract, we describe our research question, provide institutional background, outline our methods, and discuss some initial findings. Preliminary "sign-and-significance" results indicate that OZ designation reduces overall startup counts, driven primarily by a decline in non-employer firms. However, it simultaneously increases employer firm formation and accelerates the transition of non-employers into employers, suggesting a quality-quantity tradeoff that is, on net, favorable for local employment.

Keywords: Place-Based Policy, Opportunity Zones, Entrepreneurship, Startups

* We are grateful to Arnold Ventures for research funding. Any views expressed are those of the authors and not those of the U.S. Census Bureau. The Census Bureau has reviewed this data product to ensure appropriate access, use, and disclosure avoidance protection of the confidential source data used to produce this product. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 3148. (CBDRB-FY26-P3148-R12983)

I. Introduction

Rates of new business formation vary dramatically across geography, with a small number of locations accounting for a disproportionate share of innovation and high-growth entrepreneurship. In the U.S., start-up rates per capita vary by as much as a factor of five across census tracts (Dinlersoz et al., 2023). In response, policymakers have increasingly turned to place-based policies designed to stimulate local economic dynamism.

This paper examines the effect of Opportunity Zones (OZs) on local entrepreneurship. OZs were established under the Tax Cuts and Jobs Act of 2017 and took effect in 2018. The program designated 8,764 census tracts in which investors receive preferential capital gains tax treatment, including reductions or full deferral and exclusion for qualifying investments. The OZ program is not only among the most recent place-based policies in the U.S., but it also far larger in scale than prior comparable initiatives. Available estimates indicate that the program has attracted more than $100 billion in investment to low-income communities between 2018 and 2024 (Theodos and Meixell, 2025). The Joint Committee on Taxation estimated that the OZ program entailed approximately $8.2 billion in tax expenditures between 2020 and 2024 alone, and these expenditures are only expected to grow going forward (Theodos et al., 2023). While the original tax incentives were scheduled to expire in 2026, the program has recently been renewed, with modifications that phase out certain existing designations and introduce new ones.

To examine the effects of the OZ program on entrepreneurship, we take advantage information on the universe of U.S. firms and establishments from the Longitudinal Business Database (LBD) and Integrated Longitudinal Business Database (ILBD). These restricted-access data allow us to identify new business formation at fine geographic detail. These data have also been shown to accurately and comprehensively capture startups, including among employer and

non-employer businesses (Fairlie et al., 2019; Dinlersoz et al., 2023; Fairlie et al., 2026). Using a matching approach that compares outcomes in designated OZs to similar eligible but not designated tracts, we examine the impacts of OZ designation on entrepreneurship.

We are actively working on this project in a Federal Statistical Research Data Center (FSRDC). Therefore, we cannot yet discuss a full set of results. However, for this extended abstract, we obtained a sign-and-significance release for key entrepreneurship outcomes.[1] Naïve difference-in-differences estimates point to a positive and statistically significant effect on overall startup counts, including both employer and non-employer firms. However, this is driven by differential trends in OZs relative to OZ-eligible but not designated tracts. Once we account for these differential trends based on propensity-score matching and doubly-robust approaches, we find that OZ designation has a negative and statistically significant impact on total startup counts. However, this overall impact masks important heterogeneity in effects across non-employer and employer firms. While OZ designation reduces non-employer startups, it increases employer startups. Further, non-employer firms in OZs are more likely to transition to employers. Taken together, the results are suggestive of a quality-quantity trade-off that is beneficial for local employment. The OZ program was originally conceived as a mechanism to provide capital for startups in low-income communities (Eldar and Garber, 2022), and there are several reasons to believe that the program could play an important role in encouraging entrepreneurial activity.[2] First, the program's core tax incentives – which include capital gains tax deferral, a basis step-up, and permanent exclusion of gains after a 10-year hold – effectively lower the cost of capital for startups. This makes equity investment in riskier, underserved areas

[1] Obtaining only a sign-and-significance release now avoids constraints on releasing subsequent analyses that might use somewhat different samples.
[2] As Eldar and Garber (2022) also highlight, however, the ultimate terms of the OZ legislation and the program's tax incentives may tilt investment toward real estate projects.

more attractive to private investors compared to traditional portfolios. Second, Qualified Opportunity Funds (QOFs) are required to hold 90% of their assets in OZ property or businesses. This creates a dedicated stream of capital for zone businesses, which could be critical for early-stage startups that do not qualify for traditional bank loans.[3] Third, startups located in OZs may benefit from more flexible lending terms, for example through the SBA's 504 Loan Program.[4] Finally, to the extent that communities use OZ designations to coordinate with anchor institutions like hospitals and universities, these institutions could support new technologies and incubators.

Recent legislation regarding the OZ program could amplify some of these effects, particularly in rural areas. Beginning in 2026, rural OZs receive a 30% basis step-up (triple the standard rate) and a reduced "substantial improvement" threshold of 50% (down from 100%). These changes could make adaptive reuse of older buildings for new business ventures more financially viable in small towns.

As one indication of the importance of the OZ program to entrepreneurship, the startup community advocated strongly for the program when it was at risk of lapsing in 2026. This advocacy emphasized that the OZ program allowed entrepreneurs to harness significant capital and build and sustain technology and other high-growth enterprises in otherwise challenging economic environments (Prystai, 2025). These arguments echo earlier discussion at the time of the program's inception that zone incentives could help spur startup activity, including in AI and other technologies, in low-income communities across the U.S. (White-Klososky, 2019).

[3] A QOF is an investment vehicle organized as a corporation or partnership for the purpose of investing in OZ property. QOFs allow investors to reinvest capital gains into economically distressed communities in exchange for tax benefits.

[4] Through the Small Business Administration's 504 Loan Program, businesses in OZs are subject to a relaxed job creation requirement, making it easier to qualify for financing for fixed assets such as real estate and equipment. See https://www.hud.gov/opportunity-zones/entrepreneurs.

II. Prior Work and Our Contribution

Our paper contributes to several strands of literature. First, our results contribute to the growing body of work on OZs. To date, this literature has focused primarily on outcomes for zone residents, property values, and overall business activity. While the evidence suggests that OZs have had limited benefits for zone residents and changed property values little (Chen et al., 2023; Freedman et al., 2023, Freedman et al., 2025), studies have pointed to positive effects on zone business growth and employment (Arefeva et al., 2025). However, much of the business growth appears to be displaced from other, nearby low-income areas (Freedman et al., 2026).

Despite the original intent of the OZ program and the unique benefits it might confer for startups, there has been comparatively little research on its direct impacts on entrepreneurial activity. This is a key question given the importance of entrepreneurship for both local and national economic growth (Aghion and Howitt, 1992; Acs et al., 2004; Lee, 2016). One notable exception is Eldar and Garber (2022), who find no significant increase in venture capital in designated tracts in the first five quarters after the OZ program's introduction. [5] Based on these results and a legal analysis of the program's structure and provisions, Eldar and Garber (2022) argue that OZs have acted more as a tax incentive for developers than a driver of innovation. Relative to their work, we consider a much longer time frame over which new startup investment might occur,[6] use a refined empirical methodology, and study different and broader measures of

[5] Eldar and Garber (2022) use data sourced from VentureXpert, which tracks investments by private equity and venture capital firms to specific companies. They take April 2018 as the date of program implementation and estimate difference-in-differences effects on venture capital out five quarters (i.e., through mid- to late-2019). While not focused specifically on startups, Nagpal (2022) also finds little evidence that OZ designation affected small business lending in Chicago using data through 2020, Snidal and Li (2024) show that small business loan origination in OZs changed little using data through 2019, and Shen (2024) finds no changes in the number of small businesses in NYC OZs using data through 2023.

[6] A limitation of much of the early work on the OZ program was that it considered outcomes only within the first 1-3 years after program implementation; as examples, see Nagpal (2022), Atkins et al. (2023),

entrepreneurial activity using restricted-access data on all businesses in the U.S.

Our paper also contributes to the large literature on policies to promote entrepreneurship and innovation. Much of this work has focused on R&D tax credits, which aim to encourage private innovation and firm creation through tax incentives for research expenditures (Hall and Van Reenan, 2000; Bloom et al., 2002; Howell, 2017; Dechezleprêtre et al., 2023; Fairlie et al., 2026). Past research has also considered the effects of Small Business Administration (SBA) loan programs, which could relax credit constraints for entrepreneurs (Lerner, 2000; Higgins et al., 2021; Van Leuven and Schaefer, 2024), as well as state and federal venture capital initiatives, which could stimulate capital investment in certain areas (Lerner, 2009; Brander et al., 2015; Denes et al., 2023). Indeed, recent work on venture capital and startup finance highlights how the spatial distribution of financial capital can shape regional entrepreneurial activity as well as innovation and employment outcomes (Kortum and Lerner, 2000; Samila and Sorenson, 2011). A sizable literature also considers how innovation hubs and regional cluster initiatives impact entrepreneurship (Chatterji et al., 2014), and some work has studied how various regulatory and administrative reforms impact business formation (Djankov et al., 2002; Klapper et al., 2006; Van Stel et al., 2007; De Jong et al., 2020).

Meanwhile, there has been less research on how explicitly place-based policies targeted at distressed communities affect startup activity in the U.S. context. What research does exist is decidedly mixed on whether place-based policies can successfully promote entrepreneurship and new business formation. For example, Neumark and Kolko (2010) find that enterprise zones in California may have, if anything, depressed entrepreneurial activity in targeted areas. However, there is some indication that the New Markets Tax Credit, arguably the closest counterpart to the

Freedman et al. (2023), Chen et al. (2023) Sage et al. (2023), Arefeva et al. (2024), Rupasingha and Davis (2024), and Snidal and Li (2024).

OZ program, helped to attract new businesses in some sectors to low-income areas (Harger and Ross, 2014). We extend this line of research to explore if OZs, a program originally conceived as a way to deepen the pool of capital available for startups in low-income communities, thus potentially increasing effect entrepreneurship in targeted areas.

III. The Opportunity Zone Program

The Opportunity Zones (OZ) program was enacted as part of the Tax Cuts and Jobs Act (TCJA) of 2017. It provides preferential capital gains tax treatment for investments made in designated census tracts. Specifically, investors may defer taxes on realized capital gains if those gains are reinvested in qualifying businesses or real estate projects located in OZs. The program also allows for a step-up in basis on deferred gains and offers permanent exclusion of capital gains on OZ investments held for at least ten years (Theodos et al., 2018; Internal Revenue Service, 2020).

The TCJA allowed state governors to designate up to 25% of eligible census tracts in their state as OZs, selecting from areas that qualify as "low-income communities" (LICs) and certain adjacent tracts. An LIC is defined as a census tract with either a poverty rate of at least 20% or a median family income at or below 80% of the greater of the metropolitan-area or statewide median family income (with statewide thresholds applied in rural areas). LICs also include tracts located within federal Empowerment Zones, tracts with populations below 2,000, and tracts adjacent to other LICs. By statute, at least 95% of designated OZ tracts had to be LICs, though governors could also designate additional adjacent tracts as OZs if those tracts had median incomes below 125% of the median income of the neighboring LIC.

Altogether, 42,176 census tracts were eligible for Opportunity Zone designation, comprising 31,864 low-income communities (LICs) and 10,312 adjacent non-LIC tracts.

Governors ultimately designated 8,762 tracts as OZs, of which 8,532 (97%) were LICs and 230 (3%) were adjacent non-LIC tracts. States completed and announced their selections by June 2018 (U.S. Department of the Treasury, 2018).

Figure 1 maps OZs across the contiguous U.S., illustrating their broad geographic dispersion. Notably, the OZ designation process was conducted rapidly and may have reflected political as well as economic considerations (Alm et al., 2021; Frank et al., 2022; Eldar and Garber, 2023; Corinth and Feldman, 2024).

Under recent tax legislation (OBBB), the tax benefits for existing OZs are slated to sunset in 2026. A new set of zones will be established in 2027, with governors then selecting new zones every ten years thereafter (Wessel, 2025). The OBBB also expanded tax incentives for OZ investments in rural areas relative to urban areas and relaxed the eligibility criteria for qualifying rural investments.

IV. Data and Outcomes

We obtained data regarding tract OZ eligibility and designation from the U.S. Department of the Treasury's Community Development Financial Institutions (CDFI) Fund.[7] Designated tracts are depicted in Figure 1.

Our outcome data are derived from the Comprehensive Startup Panel (Fairlie et al. 2019; Fairlie et al., 2023), which is assembled from the Longitudinal Business Database (LBD) and the Integrated Longitudinal Business Database (ILBD). Both the LBD and ILBD are restricted-use researcher datasets developed by the Census Bureau's Center for Economic Studies (CES). The LBD combines administrative records and survey data for the universe of all U.S. employer establishments (those with paid employees). The ILBD is an expansion of the LBD that

[7] See https://opportunityzones.hud.gov/home.

additionally records the attributes of non-employer firms (such as sole proprietorships with no paid workers). By drawing on both of these datasets, the Comprehensive Startup Panel covers both employer startups and non-employer startups. As such, it is uniquely well suited to studying business formation and startup activity (Fairlie et al., 2019).

We updated the startup panel through 2023, and supplemented the data with detailed geographic information on establishments from the Census's Business Register, allowing us to map businesses to census tracts and thereby determine whether they were potentially impacted by OZ designation.[8] Both the LBD and ILBD data are annual and are currently available through 2023. In this extended abstract, we focus on counts and attributes of businesses started between 2013 and 2022, and use one-year ahead data to define outcomes like firm survival and transition rates. In future versions of this paper, we plan to broaden the set of entrepreneurial outcomes we consider as well as study longer-term outcomes and potentially important dimensions of heterogeneity in effects.

For our primary analysis, we focus exclusively on designated and eligible tracts that qualify as low-income communities (LICs).[9] Restrictions on the number of non-LIC contiguous tracts that could be selected as Opportunity Zones (OZs), along with a general preference for designating more economically distressed areas, resulted in only 230 non-LIC contiguous tracts being designated. Including non-LIC contiguous tracts in the sample would require incorporating a substantial number of higher-income tracts as controls, which are less comparable to the ultimately designated tracts.

We are actively working on our analysis in a Federal Statistical Research Data Center

[8]Despite geocoding efforts by the CES, some establishments in the LBD lack geographic identifiers finer than zip codes (Brooks et al., 2025). For these establishments, we use zip codes to assign tracts probabilistically, with weights from HUD-USPS commercial address counts crosswalks (HUD, 2024).
[9] We exclude Puerto Rico, where all eligible LICs were designated as OZs.

(FSRDC) and cannot yet release descriptive statistics. However, Table 1 is a shell for the descriptive statistics we intend to release. It will show means (and standard deviations) for pre- and post-treatment outcomes as measured in the Comprehensive Startup Panel.

V. Empirical Approach

A. Basic Event Study

Our empirical analysis will begin with an event study designed to estimate the effects of OZ designation, using eligible but non-designated tracts as the comparison group. The baseline specification is as follows:

$$y_{it}=\sum_{j=2013}^{2016}\{\beta_j^{pre}\times OZ_i\times 1[j=t]\}+\sum_{k=2018}^{T}\{\beta_k^{post}\times OZ_i\times 1[k=t]\}+\gamma_i+\eta_t+\varepsilon_{it}$$

In this equation, y_{it} is an outcome of interest, like the log count of new firms, for tract *i* in year *t*. OZ_i is a dummy that equals 1 if tract *i* is designated as an OZ and 0 if it is eligible but not designated. We include in the model tract fixed effects (γ_i), which control for time invariant tract characteristics that could be correlated with OZ designation and independently affect outcomes. The year fixed effects (η_t), meanwhile, control for factors changing each year that are common to all tracts in the sample. The coefficients β_j^{pre} and β_k^{post} capture pre-and post-treatment differences in outcomes between OZs and comparison tracts each year; these are measured relative to 2017.[10] Throughout the analysis, we cluster standard errors at the tract level; this allows for arbitrary patterns of heteroskedasticity across tracts and serial correlation within tracts.

[10] The OZ program took effect in 2018. Therefore, 2018 might be considered a "partially treated" year, while 2019 is the first "fully treated" year. In the event study, one can simply interpret each year's estimate through this lens.

B. Selection and Parallel Trends

Prior research on the OZ program that relied on eligible tracts as controls has documented violations of the parallel trends assumption in the pre-treatment period. In particular, OZ designation has been shown to be correlated with prior economic improvements in designated tracts (Brazil et al., 2021; Eldar and Garber, 2023; Freedman et al., 2023). We therefore build a control group using a data-driven approach that reweights potential comparison tracts. Following Freedman et al. (2023), we implement inverse probability weighting (IPW) as well as a doubly robust inverse probability weighted regression adjustment estimator. For the doubly robust approach, we draw on the methods developed by Sant'Anna and Zhao (2020) and extended by Callaway and Sant'Anna (2021).

Our objective is to account for counterfactual changes in employment in both OZ tracts and control (eligible but non-designated) tracts. Using IPW, we estimate the unobserved counterfactual mean outcome for treated tracts – i.e., what would have occurred in the absence of OZ designation – by constructing a weighted average of outcomes among non-treated tracts. The weights are given by the inverse of the probability of not being treated, scaled by the probability of treatment. We obtain these probabilities from a logit model, where the underlying latent variable specification (OZ*) is given by:

$$OZ_i^* = \alpha + \sum_{t=2013}^{2017} y_{it} + v_i$$

Specifically, we predict OZ designation for all LIC tracts in our sample using each tract's outcomes over the full pre-treatment period (2013 to 2017). Greater weight is assigned to non-treated tracts with a higher estimated probability of treatment based on their pre-treatment trajectories. In practice, this approach selects control tracts whose trends more closely resemble

those of the treated tracts, thereby strengthening the plausibility that the weighted average outcome among non-treated (eligible but non-designated) tracts approximates the counterfactual outcome that treated (designated OZ) tracts would have experienced in the absence of designation. We construct a distinct set of weights for each outcome variable analyzed.

The IPW approach models the treatment assignment process, while regression adjustment methods additionally model the outcome to address potential non-random selection into treatment. Regression adjustment constructs counterfactual outcomes by estimating separate linear regressions for treated and control tracts and using the resulting predicted values to approximate potential outcomes for given covariates. Averaging the covariate-specific treatment effects across treated tracts yields an estimate of the average treatment effect on the treated (ATT).

The regression-adjusted IPW estimator integrates the IPW weights into the outcome regression to obtain bias-corrected coefficient estimates, effectively combining both strategies. This approach is "doubly robust," in the sense that it delivers consistent estimates if either the treatment model (IPW) or the outcome model (regression adjustment) is correctly specified. Nonetheless, both methods rely on the assumption of selection on observables (Tan, 2010). In our implementation, we model both treatment and outcomes using the same set of covariates and apply the extension of doubly robust methods to multiple time periods developed by Callaway and Sant'Anna (2021). By employing both IPW and regression-adjusted IPW estimators, we strengthen our ability to attribute post-designation changes in outcomes to the OZ program itself rather than to the continuation of pre-existing trends.

We apply our weighting methods to examine all outcomes from the Comprehensive Startup Panel. The final two columns of Table 1 will show the effects of the IPW-based

reweighting on our effective control group of non-OZ LICs. The goal of the reweighting is to match pre-treatment trends in outcomes, but these descriptive statistics will shed light on the extent to which it renders the control group more similar in levels to the treatment group as well.

VI. Results

Preliminary sign-and-significance results for key outcomes appear in Table 2. We estimate the effects of OZ designation on seven indicators of business formation and longevity. We first examine the count of total new businesses (including employer and non-employer businesses) and their one year-survival rates; the one-year survival rate is defined as the share of startups that are still active one year after they are founded. We then look separately at the count of new non-employer businesses and their one-year transition rate; the one-year transition rate is defined as the share of non-employer businesses that become employers within one year. We then estimate the effect of OZs on the count of employer startups, total startup employment, and total startup payroll; these are defined only for firms that begin their life as employers. Startup employment and payroll are totaled across firms in their first year of business.

The first panel in Table 2 reports naïve difference-in-differences results that compare changes in each outcome in OZs to changes in eligible but not designated census tracts. These models do not correct for non-parallel trends between treatment and control tracts. The results point to a relative increase in the number startups in OZs, with statistically significant positive effects on the counts of both new non-employer and employer firms (see columns (1), (3), and (5) of Panel A). However, as shown in column (2), the naïve results are also suggestive of a statistically significant decline in the total number of startups surviving their first year. As shown in column (4), there is a positive but insignificant effect on the rate at which non-employer firms transition to being employer firms. We also find no statistically significant effects of OZ

designation on total startup employment or payroll when not correcting for diverging trends. Looking only at the naïve estimates, then, one would get the impression that OZs experienced an increase in the count but possibly a decrease in the quality (measured by longevity and size) of new firms.

However, as in previous studies of the effects of OZs, we find evidence of differential pre-treatment trends between our treatment and control tracts. In Table 3, we report the signs and significance of coefficients from an event study specification estimating the effect of OZs on total new firm counts. Column (1) reports the naïve case (i.e., not reweighting to correct for different pre-treatment trends). Coefficients on years prior to treatment are negative and statistically significant, indicating that designated tracts were on a rough upward trajectory ahead of treatment.

We correct for these pre-treatment trends using the previously discussed IPW and doubly-robust methods. The event study estimates using these methods are reported in columns (2) and (3) of Table 3. By design, our matching approaches largely eliminate estimated pre-treatment differences in outcomes. Post-treatment, we observe a negative effect of OZ designation on the total number of startups, which becomes statistically significant in later years. In other words, OZ designation reduces the total volume of local startups on average.

However, this aggregate effects masks important differences across types of startups. Specifically, as shown in Panels B and C of Table 2, we find that the overall reduction in startup is driven by a decrease in new non-employer firms in OZs. Meanwhile, the number of employer startups increases in OZs. Correspondingly, OZs increase total local startup employment and payroll. Furthermore, while we do not find a statistically significant effect of OZ designation on new business survival rates, we find that OZ designation increases the fraction of non-employers

that transition to being employers. This suggests that OZs may facilitate business scaling. While preliminary, these results are consistent with OZs helping some new firms grow, while at the same time suppressing the formation of weaker firms.

VII. Provisional Conclusion, and Next Steps

The Opportunity Zone program represents the largest place-based economic development initiative in the United States, yet rigorous evidence on its effects on local entrepreneurial activity remains limited. We help fill this gap by using restricted-access Census Bureau microdata covering the universe of new firms to examine how OZ designation affected startup activity at the local level. Our empirical strategy employs a matching approach that compares entrepreneurship outcomes in designated zones to eligible but non-designated census tracts with similar pre-OZ program trends.

We are conducting our analysis within a Federal Statistical Research Data Center (FSRDC), and full results are not yet available for public release. Based on preliminary sign-and significance results, however, we reach several qualitative conclusions. Accounting for selection in OZ designation, we find that OZ designation has a negative and statistically significant impact on total startup counts. However, this overall impact masks important heterogeneity in effects across non-employer and employer firms. While OZ designation reduces non-employer startups, it increases employer startups. Further, non-employer firms in OZs are more likely to transition to employers. Taken together, the results are suggestive of a quality-quantity trade-off that is beneficial for local employment, at least in the short term. This trade-off could potentially reflect the impact of capital investment increases owing to OZ incentives. These could both make it more likely that firms begin with employees, and that non-employer firms more quickly transition to employer firms.

In future versions of this paper, we hope to extend our analysis in several directions. First, we will explore heterogeneity in effects by broad industry to help better understand the implications on the composition of startups. Second, we will examine whether effects differ by tract characteristics and initial conditions, which will shed light on what types of communities might be differentially impacted by the program; this extension is particularly policy relevant given the recently strengthened incentives to invest in rural areas. Third, we will examine the degree to which any new startup activity in OZs is truly new vs. displaced from other nearby areas. Fourth, as the ILBD and LBD are updated within the FSRDC, we hope to incorporate an additional year of data, which will further increase power as well as potentially accommodate studying longer-term outcomes (e.g., survival rates over different time frames and whether non-employer to employer transitions translate into sustained employment and payroll gains). Fifth, we may be able to incorporate additional external data to speak to other entrepreneurship- and innovation-related outcomes – for example, small business lending or patenting data.

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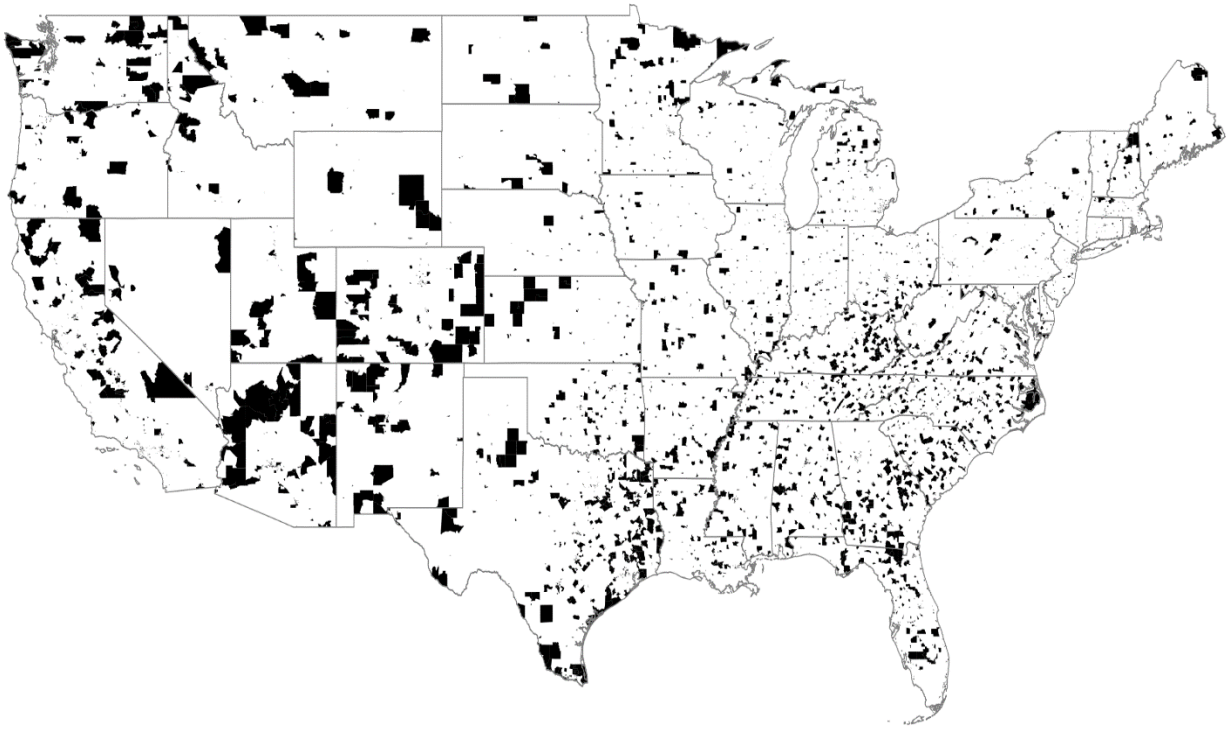
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Figure 1. Opportunity Zones



Notes: Shaded areas on the map are census tracts designated as Opportunity Zones in 2018. Information on Opportunity Zones are from the Community Development Financial Institutions Fund at the U.S. Department of the Treasury.

Table 1. Descriptive Statistics for Opportunity Zones and Control Tracts (Shell)

	Unweighted				ATT Weights	
	Treated (OZs)		Untreated (non-OZ LICs)		Untreated (non-OZ LICs)	
	(i)	(ii)	(iii)	(iv)	(v)	(vi)
	2013-2017	2018-2022	2013-2017	2018-2022	2013-2017	2018-2022
Comprehensive Startup Panel Data						
All New Firms						
log(Startup Count)						
1-year Survival Rate						
New Non-Employer Firms						
log(Non-Emp. Startup Count)						
1-year Transition Rate						
New Employer Firms						
log(Emp. Startup Count)						
log(Emp. Startup Count)						
log(Emp Count)						
log(Payroll)						
Tracts						

Notes: Variables are derived from annual LBD and ILBD for the years specified in each column. Standard deviations in parentheses. The year ranges in the column headings refer to the startup cohort year.

Table 2: Sign and Significance of Preliminary Estimates

	log(Startup Count)	1-year Survival Rate	log(Non-Emp. Startup Count)	1-year Transition Rate	log(Emp. Startup Count)	log(Emp. Count)	log(Payroll)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A: Naïve Difference-in-Differences						
Post × OZ	+***	-**	+***	+	+***	+	+
	Panel B: Inverse Propensity Weighting						
Post × OZ	-***	-	-	+***	+***	+***	+***
	Panel C: Doubly-Robust Method						
Post × OZ	-***	-	-	+***	+***	+***	+***
Tract FE	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y
Sample	All Startups		Non-Employer Startups		Employer Startups		
Approx. Obs. (Tract by Years)	274,000	274,000	261,000	261,000	159,000	159,000	159,000
Approx. Count of Firms	15,050,000	15,050,000	13,610,000	13,610,000	1,051,000	1,051,000	1,051,000

Notes: The "All Startups" sample is estimated based on positive counts of startups in low-income communities eligible for designation in opportunity zones. To maintain consistent samples across the three panels, the sample is restricted to low-income communities in which the startup count is positive in every pre-period. The "Non-Employer Startups" sample and "Employer Startups" sample are both defined analogously, but with the positive count in the pre-period restriction applying to counts of non-employer and employer startups respectively. Significance calculated using standard errors clustered by tract. Year and tract fixed effects are included in the model. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Sign and Significance of Event Study for Startup Count

	log(Startup Count)		
	(1) Naïve	(2) IPW	(3) Doubly-Robust
2013 × OZ	-**	-	-
2014 × OZ	-**	-	-
2015 × OZ	-***	-	-
2016 × OZ	-	-	-
2017 × OZ	Excluded	Excluded	Excluded
2018 × OZ	+***	-	-
2019 × OZ	+***	-	-
2020 × OZ	+***	-***	-***
2021 × OZ	+***	-**	-**
2022 × OZ	+***	-***	-***
Tract FE	Y	Y	Y
Year FE	Y	Y	Y
Sample		All Startups	
Approx. Obs. (Tract-Years)	274,000	274,000	274,000
Approx. Count of Firms	15,050,000	15,050,000	15,050,000

Notes: The "All Startups" sample is estimated based on positive counts of startups in low-income communities eligible for designation in Opportunity Zones. To maintain consistent samples across the three panels, the sample is restricted to low-income communities in which the startup count is positive in every pre-period. Significance calculated using standard errors clustered by tract. Year and tract fixed effects are included in the model. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$