Do Opportunity Zones Spur Entrepreneurship? Evidence from County-Level Establishment Counts

HAL WESLEY SNARR*
Department of Economics
Willie A. Deese College of Business and Economics
North Carolina A&T State University
1601 East Market Street
Greensboro, NC 27411
United States
Email: hwsnarr@ncat.edu

CEPHAS B. NAANWAAB
Department of Economics
Willie A. Deese College of Business and Economics
North Carolina A&T State University
1601 East Market Street
Greensboro, NC 27411
United States
Email: cbnaanwaab@ncat.edu

* Corresponding author

Abstract
Because manufacturing and service-related industries account for a large share of employment and business activity in distressed communities, we use County Business Patterns data to examine the relationship between Opportunity Zone (OZ) designation and micro (1-9 employees), small (10-19), and medium-sized (20-49) establishments. Baseline difference-in-differences estimates suggest broad post-designation growth in establishment counts, but these effects diminish substantially after controlling for establishment persistence, state and county characteristics, and county and year fixed effects. Results from an alternative treatment-intensity measure based on the number of OZs within a county are similar. Relative-outcome specifications indicate that OZ designation is associated with higher shares of micro, small, and medium-sized manufacturing establishments of 5.9, 3.8, and 3.5 percent, respectively, and higher shares of service establishments of 1.8, 6.8, and 8.8 percent, respectively.

Keywords
Entrepreneurship; employment; regional economic development; opportunity zones

JEL Classification
M13; O12; R11; J38

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1 Introduction

A recent discussion of AI led by NVIDIA CEO Jensen Huang highlights competing views of entrepreneurship by contrasting leaders with limited imagination and those with greater vision.[1] Leaders with limited imagination use disruptive technologies, whether AI today or the steam engine and personal computer in earlier eras, to do the same with less by automating tasks, reducing labor inputs, and maintaining roughly the same level of output. In contrast, visionary leaders use those same technologies to do more with more by expanding productive capacity, creating new opportunities, and generating additional economic activity. He characterizes the former as a failure of leadership and the latter as the essence of entrepreneurship. His distinction reflects a broader historical pattern. Lane (1943) observed that workers often viewed new technologies as existential threats, citing resistance by craftsmen whose livelihoods depended on older production methods to the adoption of steam-powered machinery. Yet technological progress ultimately generated new industries, occupations, and opportunities that increased productivity and improved living standards. Rather than permanently displacing labor, new technologies frequently complement workers and shift them toward higher-value activities.

Whether this process should be guided by government policy or allowed to develop organically remains an open question. Either way, it is a process of discovery, adaptation, and disruption that begins when individuals, whether startup founders or managers within established firms, recognize opportunities signaled by above-normal margins, inefficient production methods, underutilized resources, or unmet consumer demand. Motivated by profit and disciplined by the risk of loss, entrepreneurs undertake uncertain ventures, introduce new technologies, adopt more productive methods, and challenge incumbents constrained by complacency or groupthink. In doing so, they disrupt existing market arrangements, reallocate resources toward higher-valued uses, and erode profits through innovation and imitation. Growth emerges not from preserving existing patterns of production but from experimentation, adaptation, and creative disruption.

The extent to which entrepreneurship requires government guidance remains a subject of debate. Market-oriented perspectives emphasize entrepreneurial discovery and competition, while more interventionist perspectives argue that information asymmetries, human-capital constraints, capital-market imperfections, and infrastructure deficiencies may discourage socially beneficial investment in distressed communities. From this view, government intervention can foster entrepreneurship by reducing barriers to business formation and expansion. Place-based development programs reflect this logic by seeking to improve the institutional and financial conditions facing distressed communities. Examples include Enterprise Zones (EZs), Opportunity Zones (OZs), Main Street, and START-UP NY,[2] which use geographically targeted incentives to stimulate economic development.

Whether such interventions actually expand entrepreneurial activity remains an open empirical question. Existing evaluations have often focused on aggregate outcomes. However, because these

[1] See Mike Funderburk, "Jensen Huang Says CEOs 'Out of Imagination' for Culling Workers Because of AI," Yahoo Finance (March 21, 2026), available at: finance.yahoo.com/sectors/technology/articles/jensen-huang-says-ceos-imagination-183000742.html.

[2] EZ legislation was first proposed in Congress in 1980, while the Omnibus Budget Reconciliation Act of 1993 established federal Empowerment Zones offering tax incentives to encourage investment and job creation. Other examples include the Main Street program, which focuses on downtown revitalization, and START-UP NY, which established tax-free zones within SUNY and CUNY campuses to promote business formation.

policies are intended to encourage business formation and expansion directly, establishment counts may provide a more appropriate measure of their effectiveness. This paper examines whether OZ designation is associated with entrepreneurial activity, as measured by county-level establishment counts from the Census Bureau's County Business Patterns (CBP). We use two-way fixed-effects models with socioeconomic, demographic, and geographic controls to estimate the effect of OZ designation on establishment growth. By focusing on establishment counts rather than broader economic indicators, this study provides a more direct assessment of whether place-based tax incentives expand entrepreneurial activity in low-income communities.

2 Literature Review

The effectiveness of place-based policies in stimulating entrepreneurship depends partly on how entrepreneurial opportunities emerge. Alvarez and Barney (2007) distinguish between discovery theory, in which entrepreneurs identify preexisting opportunities, and creation theory, in which opportunities arise through experimentation, learning, and action under uncertainty. The latter perspective provides a rationale for policies that seek to shape institutional and financial environments that encourage entrepreneurship. Consistent with this view, research emphasizes the role of entrepreneurial ecosystems, adaptive institutions, and local networks in fostering innovation and resilience (Acs et al., 2016; Dushnitsky et al., 2020; Goel and Karri, 2020). Empirical evaluations of place-based policies, however, have focused primarily on aggregate outcomes such as unemployment, employment, earnings, and poverty (Arefeva et al., 2025; Bondonio and Engberg, 2000; Couch et al., 2005; Freedman et al., 2023; Snarr et al., 2018). Because these programs are intended to encourage business formation and expansion directly, establishment counts and related measures of business activity may provide a more informative assessment of policy effectiveness (Couch et al., 2005; Snarr and Friesner, 2022; Xu et al., 2019).

Evidence from EZs supports both perspectives. Couch et al. (2005) find that Mississippi EZs increased job creation associated with new and expanding manufacturing establishments, suggesting that targeted incentives can stimulate entrepreneurial activity. In contrast, Bondonio and Engberg (2000) find no statistically significant employment effects from several state EZ programs after accounting for non-random placement of zones. Evidence from China reaches a similarly mixed conclusion. Xu et al. (2019) find that cluster-zone policies facilitated the transition of micro-enterprises into small firms but produced little evidence of comparable transitions into medium or large firms. Together, these findings suggest that place-based policies may influence the composition of entrepreneurial activity even when aggregate measures reveal little overall change. As a result, aggregate outcomes may obscure important shifts across establishment-size categories, where gains in some categories offset losses or stagnation in others.

Evidence from START-UP NY underscores the importance of establishment counts as a measure of place-based policy effectiveness. Using establishment-level data, Snarr and Friesner (2022) find that the program increased manufacturing and technology establishment counts, particularly among micro and small establishments. Yet Snarr et al. (2018) find no corresponding reduction in county-level unemployment. One explanation is that labor-market measures are based on where workers reside rather than where they work. Consequently, even if a place-based policy successfully increases business activity within a designated area, its labor-market effects may be distributed across multiple counties as workers commute from surrounding communities. Employment gains generated in a zone may therefore reduce unemployment both within it and in neighboring counties where workers reside, attenuating measured effects on local labor-market outcomes. These findings suggest that place-based policies may influence business formation even when their effects are not readily apparent in conventional labor-market indicators, reinforcing the value of establishment counts as a more direct measure of policy effectiveness.

Introduced under the 2017 Tax Cuts and Jobs Act, the OZ program represents one of the most ambitious recent attempts to encourage entrepreneurship through place-based incentives. By offering preferential capital gains tax treatment for investments in low-income communities, OZs seek to attract capital to areas that might otherwise struggle to compete for investment. Whether those capital inflows translate into entrepreneurial activity remains uncertain. Freedman et al. (2023) find little evidence that OZ designation improved employment, earnings, or poverty outcomes, while Brazil and Portier (2021) note that overlapping eligibility with other development programs complicates identification of OZ effects. Using establishment-level data, Arefeva et al. (2025) find stronger employment growth in metropolitan OZs than in comparable non-OZ tracts during the program's early years. In contrast, Xu (forthcoming) reports increased private investment but fewer new business registrations following OZ designation, while Eldar and Garber (2022) find no evidence that OZs increased startup investment in low-income communities. Collectively, these findings suggest that attracting capital and fostering entrepreneurship are related but distinct outcomes, and that increased investment may not translate into new business formation.

3 Data and empirical methods

Because OZs are intended to encourage business formation within low-income communities, and because manufacturing (MFG) and service-related industries (SRI) tend to comprise a substantial share of economic activity in many low- and moderate-income communities that rely on low- and semi-skilled workers,[3] we focus on establishment counts in these sectors. To account for potentially heterogeneous policy effects, these counts are categorized by sector and further divided into micro (1-9 employees), small (10-19), and medium-sized (20-49) establishments. For brevity, MFG establishment counts are hereafter referred to as MFG counts, and similarly for SRI counts.

Like Snarr and Friesner (2022), our empirical approach employs a difference-in-differences (DiD) framework that compares county-level establishment counts in treated and untreated areas. Whereas they define treatment based on the presence of a START-UP NY zone and estimate year-specific treatment effects, we define treatment based on whether a county contains one or more OZ and estimate a single average post-treatment effect. Because OZ designation occurs at the census-tract level while CBP establishment counts are available only at the county level,[4] counties serve as the unit of analysis. Consequently, the estimated effects should be interpreted as county-level associations between OZ exposure and establishment counts rather than tract-level treatment effects. In addition, because CBP reports establishment counts rather than firm births, deaths, or transitions across employment-size categories, the analysis evaluates changes in establishment

[3] MFG and SRI account for a substantial share of economic activity in many low- and moderate-income communities (Chicago Federal Reserve Bank, 2019; see www.chicagofed.org/publications/profitwise-news-and-views/2019/small-business-performance-in-industries-in-lmi-neighborhoods-after-the-great-recession), while low- and semi-skilled workers are disproportionately concentrated in urban low-income communities and communities of color (see econofact.org/concentrated-poverty-and-the-disconnect-between-jobs-and-workers).

[4] Available at: www2.census.gov/programs-surveys/cbp/datasets.

counts rather than direct measures of firm formation or firm growth. A total of 8,764 OZs were designated across the 50 states, DC, and US territories. Because these designations have remained fixed since 2018, they provide a stable basis for evaluating changes in entrepreneurial activity over time.[5]

The following model estimates the effect of OZ designation on establishment counts. The coefficient on the interaction between the OZ indicator (*treat* = 1 for counties containing one or more OZs, 0 otherwise) and the post-designation indicator (*post* = 1 for 2018 and later, 0 otherwise) identifies the DiD estimate for each dependent variable, defined as the logarithm of MFG or SRI counts within one of the three establishment-size categories:

$$y_{kit} = \boldsymbol{\varepsilon}_k'\mathbf{y}_{it\text{-}1} + \theta_k{\cdot}post + \tau_k{\cdot}treat + \delta_k{\cdot}treat{\times}post + \boldsymbol{\gamma}_k'\mathbf{X}_{it} + \zeta_{ki} + \phi_{kt} + \xi_{kit}$$

where k indexes the six equation categories; i is the county index; t is the year index; y_{kit} is the establishment count of category k in county i at time t; $\mathbf{y}_{it\text{-}1}$ represents lagged establishment counts for the six categories for county i; $\boldsymbol{\varepsilon}_k$ is the set of establishment-count parameters, $\mathbf{X}_{it}$ is a common set of county-level policies, economic, demographic, and geographic controls for county i at time t; ζ_k denotes county level fixed effects for equation k, ϕ_k denotes year-fixed effects for equation k; and ξ_k is the set of white-noise errors for equation k. Regarding the coefficients of interest, θ_k is the change in establishment counts during the post-designation period relative to the pre-designation period, τ_k captures differences in establishment counts between treatment and control counties, and δ_k is the DiD estimate for equation k.

The model controls for persistence in establishment counts, state-level policies, and county-level economic, demographic, and geographic characteristics that may influence establishment growth. County-level demographic and educational variables from the American Community Survey (ACS) include the percentage of high school dropouts (*hsdo*), the share of adults with a bachelor's degree or higher (bs), and the shares of the population identifying as White only (*wht*), Black only (*blk*), and Hispanic or Latino of any race (*hsp*). Economic controls from the Federal Reserve Economic Data (FRED) database include real per capita personal income (*pcpi*), the poverty rate (*pov*), and the unemployment rate (*unemp*). We also include the Economic Freedom Summary Index (*efsi*) from the Fraser Institute's Economic Freedom of North America database to account for differences in state policy environments that may affect entrepreneurial activity.

Our econometric strategy is designed to generate reliable and interpretable estimates of the relationship between OZ designation and establishment counts. We employ a two-way fixed-effects framework that accounts for unobserved heterogeneity across more than 3,000 counties, with county fixed effects controlling for time-invariant county characteristics and year fixed effects capturing factors common to all counties in a given year. Following Bertrand et al. (2004), clustered-robust standard errors address potential heteroskedasticity and within-panel serial correlation. All continuous variables are expressed in natural logarithms, with one added to each value prior to transformation to accommodate observations with zero values and facilitate elasticity-based interpretations of the estimated coefficients. Explanatory variables are lagged one period to reflect the possibility that entrepreneurial activity responds to prior rather than contemporaneous economic conditions. Because the same model is estimated for six dependent

[5] Designated OZ census tracts are available from the Community Development Financial Institutions Fund at www.cdfifund.gov/system/files/documents/designated-qozs.12.14.18.xlsx.

variables using an identical set of explanatory variables, each equation is estimated separately rather than as part of a system. Results from the baseline specification, which includes only the treatment and post-designation indicators and their interaction, are reported in Table 4, whereas results from the fully specified models are reported in Table 5.

As robustness checks, we explored several alternative specifications. We attempted to estimate Poisson and negative binomial models to account for potential overdispersion in county-level establishment counts, but these models failed to converge given the large number of counties and fixed effects included in the analysis. To address variation in OZ exposure across counties, we also estimate the six models with the binary OZ designation indicator replaced by OZ intensity, measured as the number of OZs within each county (*ozn*). These estimates, reported in Table 6, provide a robustness check on the binary treatment specification while allowing the estimated relationship to vary with the degree of OZ exposure.

Finally, we replace the dependent variables (logarithm of sector-specific establishment counts) with the logarithm of their ratio to total establishments within the same employee-size category; these results are reported in Table 7. Because the ratio specification uses total establishment counts within the same size category as an internal benchmark, the coefficient on the post-designation indicator captures changes in sector-specific establishments relative to overall establishment activity. Whereas year fixed effects control for business-cycle conditions, long-run growth patterns, spatial spillovers, pandemic-related disruptions, and other factors at the national level, the ratio specification helps account for analogous effects at the county level.[6]

4 Results

The data are summarized in Tables 1-3 and Figure 1. Table 1 reports descriptive statistics for a panel of approximately 43,000 county-year observations. Of these observations, 42 percent occur during the post-designation period, 66 percent of counties contain at least one OZ, and the number of OZs per county ranges from 0 to 274. Establishment counts are highly right-skewed, with mean values substantially below their corresponding maximums. For example, the average number of small SRI establishments is 277.28, compared with a maximum of 28,110. Real per capita personal income averages \$53,143 and ranges from \$20,860 to more than \$500,000, while county populations range from fewer than 400 residents to more than 10 million. Poverty rates, educational attainment, racial and ethnic composition, labor-market conditions, and economic freedom also exhibit substantial variation across counties, with the economic freedom index ranging from 1.5 to 8.1.

[6] Formally, if $R_{it} = n_{it}/N_{it}$, where n denotes sector-specific establishment counts and *N* denotes total establishment counts within the same size-sector category, then $\ln R_{it} = \ln n_{it} - \ln N_{it}$. Consequently, the coefficient on the j^{th} control variable in the ratio specification equals $\beta_{jn} - \beta_{jN}$. An insignificant coefficient therefore implies that the variable affects sector-specific and total establishment counts similarly within a size category, causing its effect to cancel in the ratio.

Table 1. Descriptive statistics

Variable	N	Min	Mean	Max	Std Dev
micro.m	43115	0.0	40.80	7543.0	155.71
small.m	43115	0.0	14.03	2044.0	50.44
med.m	43115	0.0	13.72	1902.0	48.40
micro.s	43115	3.0	1576.77	217072.0	5831.63
small.s	43115	0.0	277.28	28110.0	903.57
med.s	43115	0.0	191.72	19703.0	659.41
treat	43115	0.0	0.66	1.0	0.47
post	43115	0.0	0.42	1.0	0.49
ozn	43115	0.0	1.08	274.0	5.86
pov	43170	2.7	15.11	55.9	5.68
wht	43170	3.1	82.46	100.0	16.88
blk	43170	0.0	9.05	87.8	14.48
hsp	43170	0.0	8.98	99.2	13.54
bs	42514	3.2	21.05	77.1	9.28
hsdo	42514	0.0	14.06	100.0	6.91
unemp	42441	1.1	5.87	29.0	2.81
pcpi.r	42568	20860.3	53143.08	500556.0	15396.76
pop	43170	348.0	103315.46	10105722.0	327142.93
efsi	40152	1.5	6.43	8.1	0.96

Tables 2 and 3 report mean values for treated and control counties before and after OZ designation. Table 2 summarizes the control variables and their percentage changes between the pre- and post-designation periods. Consistent with the targeting of economically distressed areas, treatment counties exhibited higher poverty and unemployment rates, lower real per capita personal income, and lower economic freedom scores than control counties prior to designation. Treatment counties also had substantially larger populations and higher shares of Black and Hispanic residents. Over time, both groups experienced declines in poverty, unemployment, and high-school dropout rates, along with increases in real per capita personal income, educational attainment, and economic freedom. However, poverty rates declined somewhat more rapidly and real per capita personal income increased somewhat more rapidly in treatment counties. These patterns suggest that treatment and control counties followed somewhat different economic trajectories during the sample period, providing motivation for including lagged economic and demographic controls in the regression analysis.

Table 2. Descriptive statistics pre- and post-OZ

	Before (post = 0)		After (post = 1)		Before to After	
	treat=0	treat=1	treat=0	treat=1	treat=0	treat=1
variable	**mean**	**mean**	**Mean**	**mean**	**% change**	**% change**
pov	14.4	16.8	12.8	14.5	-11.6	-13.8
wht	88.8	81.6	86.8	79.4	-2.2	-2.7
blk	5.5	10.5	5.7	10.5	3.8	0.1
hsp	7.6	9.0	8.6	10.0	13.5	11.5
bs	18.6	20.4	21.0	23.0	12.7	12.9
hsdo	14.6	15.7	12.1	12.8	-17.4	-18.3
unemp	5.9	7.4	4.1	5.0	-30.5	-32.5
pcpi.r	52835.8	49163.3	58228.5	55348.3	10.2	12.6
pop	20553.1	140320.7	22662.3	147535.9	10.3	5.1
efsi	6.5	6.1	6.9	6.6	6.9	8.0

Table 3 reports mean establishment counts by establishment size and sector for treatment and control counties before and after OZ designation, along with the corresponding DiD estimates. Prior to designation, treatment counties exhibited substantially higher MFG and SRI counts across all establishment-size categories than control counties. For example, treatment counties averaged 68.72 micro MFGs compared with 11.06 in control counties. The large pre-designation differences reflect the substantially larger populations and broader economic bases of treatment counties. While these level differences are informative, the more relevant question is whether the gap between treatment and control counties widened or narrowed following OZ designation. The simple DiD estimates indicate markedly different patterns across sectors. For MFG establishments, the treatment-control gap narrowed, with the largest estimated decline occurring among micro establishments (-22.88), while the estimated declines for small and medium-sized MFGs were comparatively modest. In contrast, the treatment-control gap widened for all three SRI categories, with the largest increase occurring among micro SRIs (127.61), followed by medium-sized (24.00) and small (15.03) SRIs.

Table 3. Mean Establishment Counts and DiD Estimates

	Before OZ (*post* = 0)			**After OZ** (*post* = 1)			
means	**control**	**treatment**	**diff**	**control**	**Treatment**	**diff**	**DiD**
micro.m	11.06	68.72	57.67	6.19	40.98	34.78	-22.88
small.m	3.01	20.06	17.05	2.79	19.11	16.32	-0.73
med.m	3.04	19.48	16.44	2.91	18.73	15.81	-0.63
micro.s	289.12	2157.26	1868.15	316.74	2312.49	1995.75	127.61
small.s	47.51	386.58	339.07	48.65	402.75	354.10	15.03
med.s	27.85	263.33	235.48	29.70	289.19	259.49	24.00

Comparing Tables 2 and 3 highlights an important feature of the data. Treatment counties entered the sample with substantially larger populations and establishment bases than control counties. Prior to OZ designation, treatment counties averaged 140,321 residents compared with 20,553 in control counties and exhibited markedly higher establishment counts across all manufacturing and service-related industry categories. These differences persisted throughout the sample period. As a result, changes in establishment counts can appear quite different depending on whether they are evaluated in absolute or relative terms. For example, treatment counties experienced larger absolute increases in service-related establishments, particularly among micro establishments, despite beginning with much larger establishment counts. Conversely, manufacturing establishment counts declined in both groups, with larger absolute declines occurring in treatment counties. These patterns underscore the importance of accounting for differences in county characteristics and estimating logarithmic specifications, which facilitate comparisons across counties, sectors, and establishment-size categories with substantially different scales by expressing changes in percentage rather than level terms.

Panel A of Table 4 reproduces the simple DiD estimates reported in Table 3 using OLS, where the coefficients on the interaction terms correspond to the DiD estimates. The estimated constants equal the pre-OZ means for control counties, the treat coefficients capture the differences between treatment and control county means during the pre-OZ period, and the *post* coefficients capture the differences between the pre- and post-OZ means for control counties. Panels B and C extend the analysis by incorporating county and year fixed effects, which absorb time-invariant county characteristics and common shocks affecting all counties. Panel B reports results using establishment counts in levels, whereas Panel C reports results using logarithmic establishment counts. This distinction is important because treatment counties, as noted above, begin the sample with substantially larger bases than control counties. As such, a given absolute change in establishments represents a much smaller proportional change in treatment counties than in control counties. By transforming establishment counts into logarithms, the estimated DiD effects are interpreted as percentage changes rather than level changes, thereby facilitating comparisons across counties with vastly different bases. Consistent with this interpretation, highly significant negative DiD estimates for MFGs reported in Panels A and B remain highly significant but become positive in Panel C's logarithmic specifications.

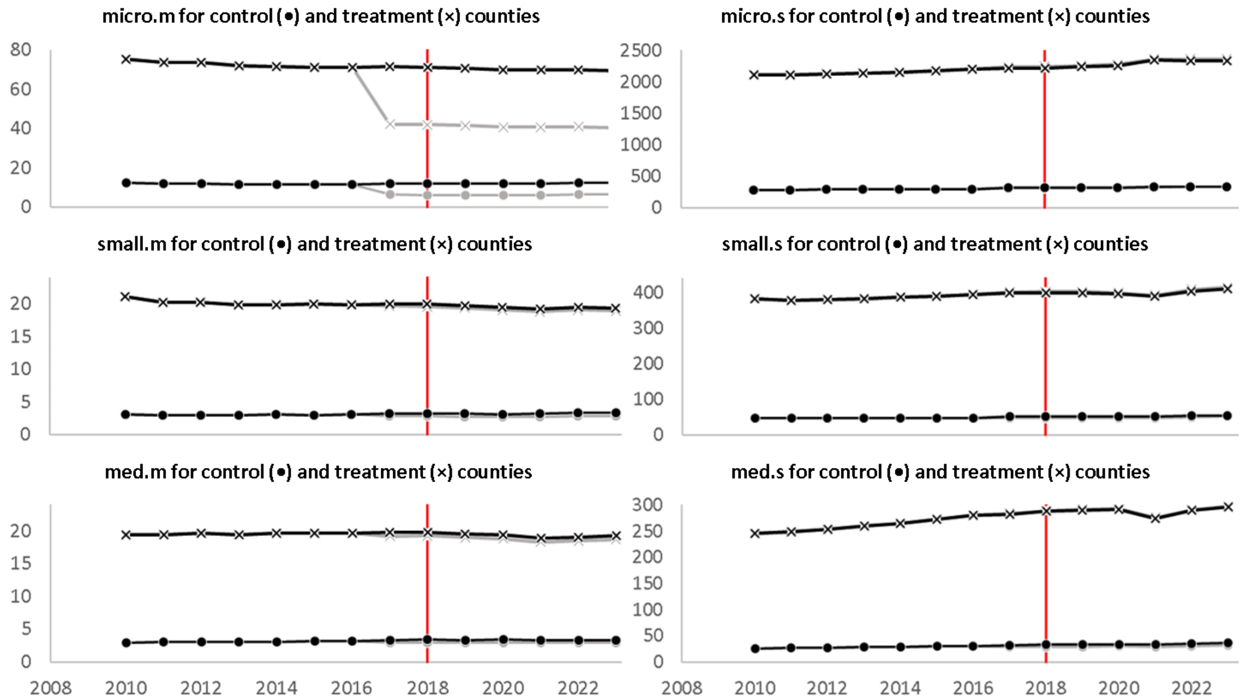
Table 4. Baseline and Alternative DiD Specifications

	(1)	(2)	(3)	(4)	(5)	(6)
	micro.m	small.m	med.m	micro.s	small.s	med.s
Panel A: DiD Estimates (OLS, Levels)						
treat	57.67***	17.05***	16.44***	1868.15***	339.07***	235.48***
	(4.93)	(1.42)	(1.36)	(148.57)	(23.54)	(16.92)
post	-4.87***	-0.22***	-0.12**	27.62***	1.14*	1.85***
	(0.21)	(0.05)	(0.05)	(4.21)	(0.69)	(0.63)
treat×post	-22.88***	-0.73***	-0.63***	127.61***	15.03***	24.00***
	(2.27)	(0.21)	(0.23)	(22.98)	(2.50)	(2.25)
constant	11.06***	3.01***	3.04***	289.12***	47.51***	27.85***
	(0.51)	(0.16)	(0.17)	(14.52)	(2.54)	(1.79)
Panel B: DiD Estimates with County and Year Fixed Effects (Levels)						
post	-10.06***	-1.07***	-0.17	87.86***	10.95***	19.67***
	(0.47)	(0.12)	(0.10)	(10.53)	(1.36)	(1.47)
treat×post	-22.47***	-0.60***	-0.50**	136.08***	16.55***	24.81***
	(2.29)	(0.21)	(0.23)	(21.27)	(2.15)	(2.03)
constant	54.08***	15.05***	13.91***	1503.79***	270.46***	172.07***
	(0.94)	(0.11)	(0.11)	(10.77)	(1.15)	(1.41)
Panel C: DiD Estimates with County and Year Fixed Effects (Logs)						
post	-0.93***	-0.29***	-0.24***	-0.08***	-0.38***	-0.45***
	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)
treat×post	0.18***	0.10***	0.09***	0.07***	0.27***	0.37***
	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
constant	2.84***	1.71***	1.63***	6.02***	4.25***	3.60***
	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)
Observations	43115	43115	43115	43115	43115	43115
RMSE (OLS, Levels)	153.652	49.820	47.796	5760.279	888.712	649.044
RMSE (FE, Levels)	47.341	4.766	4.907	447.573	49.412	49.090
RMSE (FE, Logs)	0.355	0.355	0.332	0.085	0.265	0.329

Notes: Robust standard errors clustered at the county (GEOID) level are reported in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

With Panel C presenting the preferred baseline specification, OZ designation is positively associated with establishment counts in all six size-sector categories and statistically significant in each case. The estimated effects imply approximately 18, 10, and 9 percent increases in micro, small, and medium-sized MFG establishments, respectively, and 7, 27, and 37 percent increases in micro, small, and medium-sized SRI establishments. The causal interpretation of these estimates depends on the parallel-trends assumption underlying the DiD framework (Bertrand et al., 2004). As an initial assessment of this assumption, Figure 1 presents establishment-count trends for the six size-sector categories in treatment and control counties. The vertical lines at 2018 denote the beginning of the OZ program, while the x-marked and dot-marked lines represent treatment- and control-county means, respectively, over time.

Figure 1. Trends in Establishment Count Means



(a) **(b)**

Grey lines represent raw establishment counts, while black lines represent counts adjusted to reflect a scenario in which the 2017 County Business Patterns (CBP) reporting revision had not occurred; micro.m, small.m, and med.m denote MFGs with 1-9, 10-19, and 20-49 employees, respectively; and micro.s, small.s, and med.s denote SRIs with 1-9, 10-19, and 20-49 employees, respectively.

An important feature of the data is a sharp decline in reported establishment counts among many smaller counties between 2016 and 2017, most evident in the grey line representing mean micro MFG counts. From 2017 onward, establishment counts generally resumed their pre-2017 trajectories, albeit from lower levels. To facilitate comparison, Figure 1 reports the establishment-count means (grey lines) and what they might have been in the absence of the sharp 2016-2017 breaks (black lines). Further investigation of these abrupt declines indicates that, beginning in 2017, the Census Bureau revised its disclosure-avoidance procedures by suppressing county-industry cells containing fewer than three establishments.[7] As a result, some counties experienced substantial breaks in reported establishment counts. The resulting declines are most evident for micro MFG counts, whereas the SRI count means displayed in Figure 1b remain comparatively unaffected throughout the period. Thus, the reporting revision introduced an artificial discontinuity into the establishment-count series. To account for this discontinuity, we define an indicator variable, *rrev*, equal to 1 for 2017 and later and 0 otherwise, and include it in the fully specified regression models.

[7] See the Bureau's 2017 CBP reporting revision: www.census.gov/data/datasets/2017/econ/cbp/2017-cbp.html.

While Figure 1 provides a useful visual summary of pre-treatment trends, the parallel-trends assumption should also be evaluated using formal statistical tests. The event-study results provide mixed evidence.[8] Although the pre-treatment coefficients are jointly significant in all six specifications, indicating persistent differences between treated and untreated counties prior to OZ designation, the large sample size of approximately 43,000 county-year observations yields highly precise estimates, making even modest deviations statistically detectable. More importantly, tests of whether the pre-treatment coefficients are equal over time fail to reject the null in four of the six specifications. Only the micro- and medium-sized SRI specifications reject the null of equal pre-treatment coefficients. Moreover, the estimated pre-treatment coefficients are generally stable over time and do not exhibit the systematic upward or downward movement that would be expected under substantial violations of parallel trends. Taken together, these results suggest that the DiD estimates are more appropriately interpreted as capturing associations between OZ designation and establishment outcomes than as causal effects of OZ designation.

Although Panel C of Table 4 reports positive associations between establishment counts and OZ designation, those specifications include only the treatment and post-designation indicators, their interaction, and county and year fixed effects. Table 5 extends the analysis by incorporating lagged establishment measures, state- and county-level controls, and the reporting revision indicator (*rrev*). The lagged establishment measures capture persistence and cross-category dynamics within counties, while lagged demographic, economic, and policy controls account for changes in local conditions that may be correlated with both OZ designation and establishment growth. We also estimated specifications incorporating state-specific time trends, but because these models produced results that were qualitatively similar to those reported in Table 5 while increasing model complexity, we report only the more parsimonious specification. The similarity of the findings suggests that the lagged establishment measures capture much of the gradual adjustment and persistence that state-specific time trends are intended to address.

[8] The p-values for the joint tests that the pre-treatment coefficients equal zero are 0.000, 0.000, 0.000, 0.000, 0.000, and 0.000 for micro, small, and medium MFGs and micro, small, and medium SRIs, respectively. The corresponding p-values for the tests that the pre-treatment coefficients are equal are 0.351, 0.293, 0.565, 0.004, 0.702, and 0.024, respectively.

Table 5. FE Panel Estimates (Log DVs with All Lagged Log DVs Included)

	(1)	(2)	(3)	(4)	(5)	(6)
	ln.micro.m	ln.small.m	ln.med.m	ln.micro.s	ln.small.s	ln.med.s
post	0.387***	0.122***	0.124***	0.049***	0.096***	0.211***
	(0.018)	(0.017)	(0.016)	(0.003)	(0.013)	(0.016)
treat×post	0.035***	0.012	0.009	-0.006***	0.043***	0.063***
	(0.010)	(0.011)	(0.010)	(0.002)	(0.007)	(0.009)
rrev	-0.813***	-0.223***	-0.234***	-0.059***	-0.259***	-0.441***
	(0.016)	(0.014)	(0.014)	(0.004)	(0.013)	(0.015)
L.ln.micro.m	0.489***	0.053***	0.026***	-0.002*	-0.000	0.023***
	(0.009)	(0.007)	(0.006)	(0.001)	(0.005)	(0.006)
L.ln.small.m	0.026***	0.341***	0.064***	0.001*	-0.002	0.015***
	(0.007)	(0.009)	(0.007)	(0.001)	(0.004)	(0.005)
L.ln.med.m	0.049***	0.063***	0.440***	0.003***	-0.002	0.020***
	(0.007)	(0.008)	(0.009)	(0.001)	(0.004)	(0.005)
L.ln.micro.s	0.011	0.062**	0.073***	0.706***	0.550***	0.412***
	(0.027)	(0.025)	(0.022)	(0.009)	(0.031)	(0.035)
L.ln.small.s	0.023***	0.027***	0.011*	0.031***	0.422***	0.148***
	(0.008)	(0.007)	(0.007)	(0.002)	(0.013)	(0.010)
L.ln.med.s	0.054***	0.039***	0.027***	0.012***	0.100***	0.483***
	(0.007)	(0.007)	(0.006)	(0.001)	(0.007)	(0.011)
L.ln.pov	-0.070***	-0.078***	-0.024	-0.032***	-0.061***	-0.113***
	(0.023)	(0.025)	(0.021)	(0.004)	(0.017)	(0.018)
L.ln.wht	-0.130**	-0.141**	-0.175***	-0.017	-0.019	-0.139**
	(0.058)	(0.059)	(0.052)	(0.015)	(0.048)	(0.058)
L.ln.blk	-0.001	0.024*	-0.008	0.002	0.019	0.003
	(0.015)	(0.014)	(0.014)	(0.003)	(0.013)	(0.014)
L.ln.hsp	-0.048***	-0.014	-0.004	-0.007	-0.025	-0.028*
	(0.017)	(0.015)	(0.013)	(0.004)	(0.019)	(0.017)
L.ln.bs	0.045	-0.014	-0.001	0.011	-0.019	0.000
	(0.028)	(0.028)	(0.024)	(0.007)	(0.026)	(0.028)
L.ln.hsdo	0.021	0.017	0.058***	0.011**	0.027	-0.007
	(0.023)	(0.022)	(0.020)	(0.004)	(0.021)	(0.025)
L.ln.unemp	-0.068***	-0.011	-0.039**	-0.020***	-0.084***	-0.129***
	(0.018)	(0.017)	(0.016)	(0.005)	(0.014)	(0.015)
L.ln.pcpi.r	-0.040	0.100***	0.025	0.057***	0.127***	0.155***
	(0.037)	(0.035)	(0.033)	(0.010)	(0.037)	(0.037)
L.ln.efsi	0.277***	0.120	0.208***	0.046***	0.200***	0.218***
	(0.070)	(0.073)	(0.065)	(0.012)	(0.046)	(0.057)
Observations	38980	38980	38980	38980	38980	38980
Constant	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.66	0.21	0.27	0.63	0.43	0.49
RMSE	0.31	0.33	0.30	0.05	0.22	0.27

Notes: Robust standard errors clustered at the county (GEOID) level are reported in parentheses. Year and county fixed effects are included in all specifications. Mean VIF equals 5.91. Elevated VIFs are concentrated primarily among lagged SRI counts, which are retained to maintain a common specification across equations.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The estimates reported in Table 5 generally conform to economic intuition. Establishment counts exhibit substantial persistence. Higher poverty and unemployment rates are typically associated with fewer establishments. Greater economic freedom is associated with more establishments in most specifications. The 2017 reporting revision coefficients are uniformly negative and highly significant, suggesting that the indicator successfully captures the 2017 reporting revision. Relative to the baseline DiD estimates in Panel C of Table 4, the corresponding coefficients in Table 5 are generally smaller and less likely to be statistically significant. Collectively, these results suggest that the larger coefficients observed in the baseline specification partly reflect omitted persistence and local economic conditions, indicating that simpler specifications may overstate the association between OZ designation and establishment activity.

Because treatment counties differ systematically from control counties in their economic, demographic, and entrepreneurial characteristics (Renski, 2008; see also Tables 1-3), Table 6 evaluates the sensitivity of the results to the measure of OZ exposure by replacing the binary treatment indicator with the logarithm of the number of OZs in each county (*ln.ozn*). This treatment-intensity specification allows the estimated relationship to vary with the extent of OZ exposure while maintaining the broader empirical framework used in Table 5.

The results in Table 6 are similar to those reported in Table 5, suggesting that the findings are not sensitive to the measure of OZ exposure. The coefficients on the lagged establishment measures continue to indicate substantial persistence, many of the control variables retain signs consistent with economic intuition, and the 2017 reporting revision coefficients remain uniformly negative and highly significant. Relative to Table 5, the treatment-intensity specification yields qualitatively similar results, although some coefficients differ modestly in magnitude and statistical significance. Overall, the similarity of the results in Tables 5 and 6 indicates that the estimated relationships are robust to alternative measures of OZ exposure and do not depend on whether OZ exposure is modeled as a treatment indicator or the number of OZs within a county.

Because the specifications in Tables 5 and 6 use sector-specific establishment counts as the dependent variables, estimated effects may partly reflect broader changes in overall business activity rather than shifts in the relative prevalence of MFGs and SRIs. To address this issue, Table 7 reports estimates from a specification that uses total establishments within the same county and establishment-size category as an internal comparison group. Specifically, the dependent variables are constructed as the difference between the logarithm of sector-specific establishment counts and the logarithm of total establishments, which is equivalent to the logarithm of the sector's share of all establishments within the same size category (e.g., ln.r.micro.m denotes the natural logarithm of the ratio of micro MFGs to total micro establishments in a county). Although not a matched-sample procedure, this approach is analogous in spirit to matched-sample estimators (Heckman et al., 1997, 1998; Smith and Todd, 2005; Caliendo and Kopeinig, 2008) because it compares sector-specific activity with overall establishment activity within the same local market.

Table 6. FE Panel Estimates (Log DVs with All Lagged Log DVs Included)

	(1)	(2)	(3)	(4)	(5)	(6)
	ln.micro.m	ln.small.m	ln.med.m	ln.micro.s	ln.small.s	ln.med.s
post	0.388***	0.115***	0.116***	0.045***	0.107***	0.218***
	(0.016)	(0.016)	(0.015)	(0.003)	(0.012)	(0.014)
ln.ozn×post	0.026***	0.016***	0.015***	0.000	0.022***	0.040***
	(0.005)	(0.005)	(0.005)	(0.001)	(0.003)	(0.004)
rrev	-0.816***	-0.224***	-0.235***	-0.058***	-0.262***	-0.446***
	(0.016)	(0.014)	(0.014)	(0.004)	(0.013)	(0.015)
L.ln.micro.m	0.488***	0.052***	0.025***	-0.002*	-0.000	0.023***
	(0.009)	(0.007)	(0.006)	(0.001)	(0.005)	(0.006)
L.ln.small.m	0.025***	0.341***	0.063***	0.001	-0.003	0.014***
	(0.007)	(0.009)	(0.007)	(0.001)	(0.004)	(0.005)
L.ln.med.m	0.048***	0.062***	0.439***	0.003***	-0.003	0.017***
	(0.007)	(0.008)	(0.009)	(0.001)	(0.004)	(0.005)
L.ln.micro.s	0.003	0.055**	0.067***	0.705***	0.545***	0.401***
	(0.027)	(0.025)	(0.022)	(0.009)	(0.031)	(0.034)
L.ln.small.s	0.023***	0.025***	0.009	0.030***	0.423***	0.148***
	(0.008)	(0.007)	(0.007)	(0.002)	(0.013)	(0.010)
L.ln.med.s	0.053***	0.037***	0.025***	0.011***	0.101***	0.482***
	(0.007)	(0.007)	(0.006)	(0.001)	(0.007)	(0.011)
L.ln.pov	-0.067***	-0.076***	-0.021	-0.031***	-0.058***	-0.108***
	(0.023)	(0.025)	(0.021)	(0.004)	(0.017)	(0.018)
L.ln.wht	-0.103*	-0.123**	-0.159***	-0.016	0.004	-0.098*
	(0.058)	(0.060)	(0.053)	(0.015)	(0.048)	(0.059)
L.ln.blk	0.001	0.026*	-0.006	0.003	0.020	0.005
	(0.015)	(0.014)	(0.014)	(0.003)	(0.013)	(0.014)
L.ln.hsp	-0.048***	-0.012	-0.003	-0.006	-0.025	-0.028
	(0.017)	(0.015)	(0.013)	(0.004)	(0.019)	(0.017)
L.ln.bs	0.046	-0.013	-0.000	0.011*	-0.019	0.002
	(0.028)	(0.028)	(0.024)	(0.007)	(0.026)	(0.028)
L.ln.hsdo	0.018	0.015	0.056***	0.011**	0.025	-0.012
	(0.023)	(0.022)	(0.020)	(0.004)	(0.021)	(0.026)
L.ln.unemp	-0.071***	-0.012	-0.040**	-0.020***	-0.088***	-0.135***
	(0.018)	(0.017)	(0.016)	(0.005)	(0.014)	(0.015)
L.ln.pcpi.r	-0.042	0.099***	0.024	0.056***	0.126***	0.153***
	(0.037)	(0.036)	(0.033)	(0.010)	(0.037)	(0.037)
Observations	38980	38980	38980	38980	38980	38980
Constant	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.66	0.21	0.27	0.63	0.43	0.49
RMSE	0.31	0.33	0.30	0.05	0.22	0.27

Notes: Robust standard errors clustered at the county (GEOID) level are reported in parentheses. Year and county fixed effects are included in all specifications. Mean VIF equals 5.91. Elevated VIFs are concentrated primarily among lagged SRI counts, which are retained to maintain a common specification across equations.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7. FE Panel Estimates (Log DVs with All Lagged Log DVs Included)

	(1)	(2)	(3)	(4)	(5)	(6)
	ln.r.micro.m	ln.r.small.m	ln.r.med.m	ln.r.micro.s	ln.r.small.s	ln.r.med.s
post	0.355***	0.106***	0.081***	0.017***	0.081***	0.168***
	(0.018)	(0.018)	(0.019)	(0.003)	(0.013)	(0.018)
treat×post	0.059***	0.038***	0.035***	0.018***	0.068***	0.088***
	(0.010)	(0.011)	(0.012)	(0.002)	(0.008)	(0.010)
rrev	-0.806***	-0.212***	-0.209***	-0.052***	-0.248***	-0.416***
	(0.016)	(0.015)	(0.017)	(0.004)	(0.012)	(0.016)
L.ln.micro.m	0.483***	0.061***	0.049***	-0.007***	0.008*	0.047***
	(0.009)	(0.008)	(0.007)	(0.001)	(0.004)	(0.006)
L.ln.small.m	0.020***	0.316***	0.054***	-0.004***	-0.027***	0.006
	(0.007)	(0.009)	(0.007)	(0.001)	(0.003)	(0.005)
L.ln.med.m	0.041***	0.061***	0.412***	-0.005***	-0.004	-0.008
	(0.007)	(0.008)	(0.009)	(0.001)	(0.003)	(0.005)
L.ln.micro.s	-0.397***	-0.105***	-0.230***	0.299***	0.384***	0.109**
	(0.027)	(0.037)	(0.038)	(0.017)	(0.036)	(0.043)
L.ln.small.s	0.030***	-0.087***	-0.018	0.038***	0.308***	0.118***
	(0.008)	(0.009)	(0.012)	(0.003)	(0.012)	(0.013)
L.ln.med.s	0.059***	0.050***	-0.070***	0.016***	0.110***	0.385***
	(0.007)	(0.008)	(0.008)	(0.002)	(0.007)	(0.011)
L.ln.pov	-0.019	-0.017	0.045*	0.019***	0.000	-0.044**
	(0.023)	(0.027)	(0.025)	(0.005)	(0.016)	(0.020)
L.ln.wht	-0.018	-0.107	-0.221***	0.096***	0.015	-0.186**
	(0.057)	(0.071)	(0.076)	(0.021)	(0.049)	(0.073)
L.ln.blk	0.009	0.033**	0.009	0.012***	0.028**	0.019
	(0.015)	(0.016)	(0.018)	(0.004)	(0.014)	(0.018)
L.ln.hsp	-0.047***	-0.022	0.003	-0.006	-0.034*	-0.021
	(0.017)	(0.018)	(0.023)	(0.005)	(0.020)	(0.022)
L.ln.bs	0.021	-0.044	-0.062*	-0.013	-0.049*	-0.060*
	(0.028)	(0.031)	(0.034)	(0.010)	(0.028)	(0.034)
L.ln.hsdo	0.018	0.015	0.033	0.008	0.026	-0.032
	(0.023)	(0.025)	(0.031)	(0.007)	(0.020)	(0.031)
L.ln.unemp	-0.066***	0.032*	0.094***	-0.018**	-0.040***	0.004
	(0.018)	(0.019)	(0.021)	(0.007)	(0.014)	(0.018)
L.ln.pcpi.r	-0.130***	-0.100**	-0.161***	-0.033***	-0.074**	-0.031
	(0.037)	(0.043)	(0.048)	(0.012)	(0.035)	(0.044)
L.ln.efsi	0.243***	0.082	0.143*	0.012	0.163***	0.154**
	(0.071)	(0.076)	(0.077)	(0.015)	(0.047)	(0.064)
Observations	38980	38980	38980	38980	38980	38980
Constant	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.66	0.20	0.25	0.47	0.44	0.46
RMSE	0.31	0.34	0.32	0.05	0.19	0.26

Notes: Robust standard errors clustered at the county (GEOID) level are reported in parentheses. Year and county fixed effects are included in all specifications. Mean VIF equals 5.91. Elevated VIFs are concentrated primarily among lagged SRI counts, which are retained to maintain a common specification across equations.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Aside from replacing the dependent variables with relative-outcome measures, the models are estimated with county and year fixed effects and the same controls used in Tables 5 and 6. While the year fixed effects absorb nationwide shocks affecting all counties, the log-ratio specification additionally controls for county-level shocks that influence both sector-specific and overall establishment activity within a size category. By expressing sector-specific establishments relative to total establishments, common local influences are differenced out, allowing the estimates to isolate changes in the relative prevalence of MFG and SRI establishments following OZ designation. Consequently, the estimates indicate whether these establishments became more or less prevalent relative to all establishments of the same size after OZ designation. Event-study estimates reported below provide generally supportive evidence for the parallel-trends assumption underlying these specifications.[9]

To validate the DiD estimates reported in Table 7, we conducted a series of robustness checks, the results of which are summarized in Table 8. Column 2 reproduces the specification from Table 7. As in that model, all alternative specifications include county and year fixed effects. Columns 3-5 report estimates from models that retain five cross-size-sector lagged establishment measures and state and county characteristics, but modify how persistence is modelled. In column 3, the lagged own-size-sector establishment count is replaced with the lagged count of low-wage-worker-dependent (LWD) establishments[10] in the same size class, preserving information about local business persistence and dynamic adjustment while removing the lagged outcome most directly related to the dependent variable. In column 4, persistence is modeled using state-specific time trends in place of the lagged own-size-sector establishment count. In column 5, the own-size-sector lagged establishment count is omitted altogether, allowing the remaining five lagged establishment measures to capture persistence dynamics. Column 6 reports a sparse specification containing only the treatment indicator, post-period indicator, their interaction, and the 2017 reporting revision indicator.

Table 8. Sensitivity of Table 7 DiD Estimates to Alternative Dynamic Specifications

Outcome	Table 7	Own Lag Replaced with Lagged LWD	Own Lag Replaced by State Trends	Own Lag Omitted	Sparse TWFE
ln.r.micro.m	0.059***	0.096***	0.094***	0.105***	0.178***
ln.r.small.m	0.038***	0.033**	0.028*	0.037**	0.074***
ln.r.med.m	0.035***	0.035**	0.046***	0.046***	0.035**
ln.r.micro.s	0.018***	0.016***	0.022***	0.024***	0.059***
ln.r.small.s	0.068***	0.095***	0.103***	0.117***	0.241***
ln.r.med.s	0.088***	0.129***	0.152***	0.179***	0.311***

Notes: Robust standard errors clustered at the county (GEOID) level are reported in parentheses. Year and county fixed effects are included in all specifications. * p < 0.10, ** p < 0.05, *** p < 0.01.

[9] The p-values for the joint tests that the pre-treatment coefficients equal zero are 0.000, 0.001, 0.043, 0.000, and 0.000 for micro, small, and medium MFGs and small and medium SRIs, respectively. The corresponding p-values for the tests that the pre-treatment coefficients are equal are 0.170, 0.079, 0.109, 0.463, and 0.521, respectively.

[10] Such establishments are defined as those in NAICS industries 722511 (full-service restaurants), 722513 (limited-service restaurants), 722514 (cafeterias, grills, and buffets), 722515 (snack and beverage bars), 4481 (clothing stores), 4482 (shoe stores), 4471 (gasoline stations), 44511 (supermarkets and other grocery stores), and 44512 (convenience stores).

A primary motivation for estimating the alternative specifications in Table 8 is the possibility of Nickell bias arising from the inclusion of lagged outcomes in a relatively short panel ($T = 13$). To evaluate the sensitivity of the results to the treatment of persistence, columns 3-5 replace the lagged own-size-sector establishment measure with alternative controls for persistence or omit it altogether, while column 6 reports a sparse two-way fixed-effects specification. Across all specifications, the estimated OZ effects remain positive and statistically significant. The estimated persistence parameters are all below 0.5, the DiD coefficients rather than the persistence parameter is the primary coefficient of interest, and the estimated OZ effects remain qualitatively similar regardless of how persistence is modeled. The sparse specification consistently produces the largest estimated treatment effects, suggesting that some portion of the estimated impact reflects omitted persistence, differential growth trajectories, and county characteristics. As progressively richer controls for persistence and local economic conditions are introduced, the estimated treatment effects generally become smaller but remain statistically significant. The Table 7 and lagged-LWD specifications produce the most conservative estimates while yielding qualitatively similar conclusions.

Collectively, these results indicate that the estimated treatment effects are not sensitive to alternative approaches for modeling persistence and are unlikely to be driven by dynamic-panel bias. Consequently, the Table 7 specification provides a reasonable basis for interpreting the relationship between OZ designation and relative establishment outcomes. As such, the estimates reported therein indicate that OZ designation is associated with modest but statistically significant shifts in the composition of establishment activity. All six estimated DiD coefficients are positive and statistically significant at the one percent level. Relative to total establishments in the corresponding size category, the shares of micro, small, and medium-sized MFG establishments increased by approximately 5.9, 3.8, and 3.5 percent, respectively, following OZ designation. Among SRIs, the shares of micro, small, and medium-sized establishments increased by approximately 1.8, 6.8, and 8.8 percent, respectively. Together, these results indicate that MFG and SRI establishments became more prevalent relative to total establishments within the same size category following OZ designation, consistent with a shift in the composition of local establishment activity rather than a uniform increase in establishment counts across sectors.

As a group, the lagged establishment-count variables are largely economically and statistically important in all six specifications. In each equation, the coefficient on the corresponding lagged establishment count is positive and highly significant, suggesting that counties with relatively large concentrations of establishments in a particular sector and size category tend to maintain those advantages over time. Within each sector, lagged establishment-count coefficients are generally larger for transitions from smaller to larger establishment-size categories than for transitions from larger to smaller categories, consistent with gradual firm growth over time. The cross-sector lagged coefficients reflect relationships in entrepreneurial composition rather than establishment growth. The three largest significant cross-sector lagged-to-contemporaneous coefficients are negative, suggesting that counties with relatively larger shares of micro SRIs tend to become less manufacturing-intensive over time. More broadly, the negative average coefficient linking lagged SRI shares to contemporaneous MFG shares indicates that, on net, counties with relatively larger service-sector shares tend to exhibit lower subsequent manufacturing shares. In contrast, the near-zero average coefficient linking lagged MFG shares to contemporaneous SRI shares suggests that, on net, manufacturing composition has little effect on subsequent service-sector composition.

Among the controls, real per capita personal income exhibits the most consistent pattern, with predominantly negative coefficients, suggesting that MFGs and SRIs comprise a smaller share of overall establishment activity in more affluent counties. Economic freedom is positively associated with the relative prevalence of several MFG and SRI categories, indicating that freer policy environments may promote these sectors at least as rapidly as overall establishment activity. Many of the remaining demographic, economic, and policy controls are statistically insignificant. Because the coefficients measure the difference between a variable's effect on sector-specific establishments and its effect on all establishments within the same size category, insignificant estimates suggest that these factors affect both similarly, causing their effects to largely cancel in the ratio specification (see Footnote 6). Overall, the results indicate that local economic conditions are associated with modest differences in the composition of entrepreneurial activity across counties.

Taken together, the estimates reported in Tables 4-7 suggest that the relationship between OZ designation and entrepreneurial activity is relatively modest and concentrated in specific sectors and establishment-size categories. The baseline estimates in Table 4 indicate positive associations between OZ designation and establishment counts across all six size-sector categories, whereas the estimates in Tables 5 and 6 show that many of these associations become smaller after accounting for establishment persistence, state and county characteristics, the 2017 reporting discontinuity, and year and county fixed effects. Table 7 provides an additional robustness check by evaluating sector-specific establishment growth relative to overall establishment growth within the same county and establishment-size category. The results indicate that OZ designation is associated with increases in the relative prevalence of both MFG and SRI establishments, with the largest effects occurring among small and medium-sized SRIs.

5 Conclusion

This study examined whether OZ designation was associated with entrepreneurial activity across U.S. counties, distinguishing between MFG and SRI establishments and across establishment-size categories. The objective of the analysis was to evaluate whether OZ designation was associated with changes in establishment counts rather than to distinguish between newly created establishments and establishments potentially relocated from neighboring areas. Accordingly, the results should be interpreted as evidence regarding the relationship between OZ designation and establishment activity rather than as direct evidence of entrepreneurial entry, survival, or scaling. On balance, the results suggest that OZ designation is associated with modest changes in establishment activity concentrated in particular sectors and establishment-size categories. In the baseline specifications, where the dependent variables are the logarithms of sector-specific establishment counts, OZ designation is associated with broad increases in establishment counts, although these associations are substantially attenuated in the fully specified models. The relative-outcome specifications, in which the dependent variables are the logarithms of sector-specific establishment counts divided by total establishments within the same county and establishment-size category, provide the strongest evidence because, together with year fixed effects, they better account for factors affecting overall business activity, including COVID-era disruptions, spatial spillovers, and other shocks likely to influence the numerator and denominator similarly. These results suggest that OZ designation is associated primarily with changes in entrepreneurial composition rather than overall establishment growth across all six size-sector categories, with the largest effects occurring among small and medium-sized SRIs. In other words, these categories expanded more rapidly than overall establishment activity within the same county and establishment-size category following designation.

These findings contribute to the literature on place-based economic development by suggesting that targeted tax incentives can influence how entrepreneurial activity is distributed within distressed communities. While OZ designation appears capable of affecting the relative growth of particular establishment categories, the evidence provides less support for the view that such programs generate substantial increases in overall entrepreneurial activity.

More broadly, the findings speak to competing perspectives on economic development. Proponents of place-based policies argue that capital-market frictions and other barriers suppress investment in distressed communities and that targeted incentives can help unlock entrepreneurial opportunities. The results provide qualified support for this view. Although OZ designation is associated with measurable changes in local establishment activity, the estimated effects are generally modest and vary across sectors and establishment-size categories. Thus, the evidence suggests that targeted tax incentives can influence the pattern of entrepreneurial activity within distressed communities, but provides less support for the view that such policies generate large or transformative increases in entrepreneurship.

Several important questions remain unresolved. Most notably, the analysis cannot determine whether the observed changes in establishment activity reflect new business formation and expansion or the relocation of existing establishments from other areas. This distinction is important because place-based policies increase national economic activity only to the extent that they generate new production rather than redistribute existing activity across locations. However, the primary objective of this study was not to determine whether OZ designation increased national economic activity or merely shifted where production occurred, but whether it was associated with changes in entrepreneurial activity within the distressed communities targeted by the program. Because manufacturing and service-related industries account for a substantial share of business activity and employment in many OZ communities, changes in establishment activity within these sectors may nevertheless provide useful evidence regarding the program's local economic effects. Although the results suggest that OZ designation is associated with changes in the composition of local establishment activity, they do not reveal whether those changes arise from establishment births, deaths, relocations, or the scaling of existing establishments. Future research should therefore examine these channels to determine whether OZs primarily accelerate the growth of existing firms, stimulate new entrepreneurial activity, or simply reallocate economic activity across locations.

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