Job Growth from Opportunity Zones*

Alina Arefeva
University of Wisconsin-Madison
Wisconsin School of Business

Morris A. Davis
Rutgers University
Rutgers Business School

Andra C. Ghent
University of North Carolina - Chapel Hill
Kenan-Flagler Business School

Minseon Park
University of Wisconsin-Madison

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Abstract

The Tax Cuts and Jobs Act of 2017 established a new program called Opportunity Zones (OZs) that created tax advantages for investing in businesses or real estate in a limited number of low-income Census tracts. We use a census of establishment-level data on employment to identify the effect of the program on job creation. We show that in metropolitan areas, the OZ designation increased employment growth relative to comparable tracts by between 3.0 and 4.5 percentage points and new jobs were created across many different industries and education levels. The OZ designation did not create jobs in rural areas.

Keywords: Placed-Based Policies, Opportunity Zones, Tax Incentives.
JEL codes: H73, R30, R38, G11, G18.

*Arefeva: arefeva@wisc.edu; Davis: mdavis@business.rutgers.edu; Ghent: andra_ghent@kenan-flagler.unc.edu.; Park: mpark88@wisc.edu. We thank Philip McDaniel, the UNC GIS librarian, for his assistance, Dan Hartley for sharing his data on Census tract centroids and adjacent tracts, and Steve Malpezzi for helpful conversations. The paper has benefited from the feedback of workshop participants at the AREUEA 2020 Virtual Seminar Series, Peking University Guanghua School of Business & Jinan University IESR, Rutgers University, the University of California - Santa Cruz, the University of North Carolina at Chapel Hill, the University of Wisconsin-Madison, and the Urban Economics Association 2020 Annual Meeting. We are grateful for the financial support of an award from the University of Wisconsin's Fall Research Competition. We have been authorized to use YTS through the Business Dynamics Research Consortium (BDRC) by the University of Wisconsins Institute for Business and Entrepreneurship. The contents of this paper are solely the responsibility of the authors.

1 Introduction

The economic well-being of Americans varies dramatically across places, even within the same Metropolitan Statistical Area (MSA). Persistent, localized pockets of poverty persist and the convergence in economic fortunes that historically characterized the US economy has ceased in the last few decades (see, for example, Ganong and Shoag (2017)). Mobility costs remain high, especially for low-income households that tend to live in low-income communities, and mobility has decreased in recent years.[1]

Against this backdrop, there is renewed interest in policies to promote employment in low-income places. Even researchers recently skeptical of place-based policies now appear more open to potential benefits of these policies; compare, for example, Austin, Glaeser, and Summers (2018) with Glaeser (2012). The Tax Cuts and Jobs Act (TCJA) of 2017 introduced a new national place-based policy in the form of capital gains tax relief to investments operating in particular low-income communities called Opportunity Zones (OZs). We study the impact of the OZ program on employment in the small geographic areas (Census tracts) directly receiving the tax benefits as well as surrounding areas.

While the OZ program has the potential to create jobs in distressed communities, there are a number of reasons to believe the program may not have any effects, or perhaps even detrimental effects on employment. For example, depending on the complementarity of capital and various types of labor, a decrease in capital gains taxes has the potential to reduce labor demand. Additionally, given the US Federal

[1] Topel (1986), Henderson and Ioannides (1989), Bound and Holzer (2000), and Hedman (2013), among others, provide evidence that low-income households face higher mobility costs. Molloy, Smith, and Wozniak (2011) review potential reasons for the recent decline in household mobility in the United States.

tax code already allows real estate investors to defer capital gains taxes via the 1031 exchange program, the OZ program may only reduce the taxes paid by inframarginal investors rather than increasing the flow of capital to low-income communities. A quantitative analysis is needed to estimate how many jobs the OZ program created and to compare the program's cost-effectiveness with other job-creation programs.

We study the effect of the OZ program on job growth at the Census tract-level using a difference-in-difference strategy that compares growth in tracts designated as OZs with growth in tracts that were eligible but not designated as OZs. We show the OZ program increased employment and establishment growth in tracts in metropolitan areas receiving tax benefits by between 3 to 4.5 percentage points over the 2017-2019 period relative to similar, eligible tracts that were not chosen to receive benefits. In contrast, we find tracts designated as OZs in non-metropolitan (rural) areas experienced no such job growth. While we find that state governors' choice of tracts was somewhat political, our results are unchanged when we control for the extent to which a tract was chosen as part of a political process. Our results are robust to a placebo test in which we use a counterfactual date for the designation.

We show at least some of the jobs that were created were likely filled by lower-skilled workers based on the average skill-level used in the industry. The construction industry experienced the greatest job growth but jobs were also generated in trade and service industries. Finally, we find that the program had significant positive spillovers to employment and establishment growth in nearby tracts.

A number of recent papers have studied the impact of the OZ program on various outcomes of interest not related to employment, the focus of our paper. Chen, Glaeser, and Wessel (2019) argue that the program did not significantly affect the growth of single-family house prices. Sage, Langen, and Van de Minne (2019) show prices increased for redevelopment properties and vacant sites, but the price of existing commercial properties did not rise. Perhaps the closest paper to ours is Atkins, Hernández-Lagos, Jara-Figueroa, and Seamans (2020) who study job postings. These authors find that the number of job postings linked to ZIP codes that include at least one tract designated to receive tax benefits from the OZ program were lower than the number of postings associated with ZIP codes that include no such tracts. Our measurement of outcomes is employment, not postings, and our level of geography is the tract which exactly aligns with the geography in the OZ program legislation.

We contribute to the literature on place-based policies, reviewed in Neumark and Simpson (2015), by evaluating the impact of one of the biggest federal place-based policies on local employment and establishment growth. To our knowledge, ours is the first paper looking at the effects of a nationwide place-based policy on job growth at the tract-level. Earlier national place-based programs in the US, Enterprise Communities (ECs) and Renewal Communities (RCs), targeted a smaller number of tracts and focused on providing wage credits, higher depreciation expense allowances, and tax-exempt funding. Some prior research has failed to find significant effects of place-based policies, for example, Neumark and Kolko (2010) who analyze California's EC program. More recent studies, such as Billings (2009) and Busso, Gregory, and Kline (2013) of Empowerment Zones (EZ), Ham, Swenson, İmrohoroğlu, and Song (2011) of EZs and state and federal ECs, and Freedman (2012) and Harger and Ross (2016) of New Market Tax Credits (NMTCs), find a significant positive impact on local employment.

Our paper is also related to a literature studying the effect of capital gains taxes on investor behavior. The majority of this literature uses data from publicly-traded equities. Research topics include the effect of capital gains taxes on investor holding periods (Ivković, Poterba, and Weisbenner, 2005; Dammon and Spatt, 2015; Dammon, Spatt, and Zhang, 2015), stock prices (Dai, Maydew, Shackelford, and Zhang, 2008; Lang and Shackelford, 2000; Starks, Yong, and Zheng, 2006), and corporate governance (Dimmock, Gerken, Ivković, and Weisbenner, 2018). Outside of publicly-traded equities, Shan (2011), Heuson and Painter (2014), and Agarwal, Li, Qin, Wu, and Yan (2020) find that capital gains taxes meaningfully affect individuals' housing decisions while Edwards and Todtenhaupt (forthcoming) show that the reduction of capital gains taxes in the US in 2010 increased funding for start-up firms. Poterba (2002) hypothesizes that the effect of capital gains taxes will be smaller for investments like commercial real estate than for publicly traded equities. We instead show that the capital gains tax relief of the OZ program meaningfully increased construction employment in the targeted areas, suggesting that capital gains taxes can influence the investment choices of investors in commercial real estate.

2 Identifying the Effect of the OZ Program

2.1 Background

The concept of tax-advantaged Opportunity Zones had bipartisan support and backing, as the legislation was conceived and sponsored by Democratic Senator Corey Booker and Republican Senator Tim Scott (Booker, 2019). The 2017 Tax Cut and Jobs Act (TCJA), signed into law by President Trump on December 22, 2017, included the OZ legislation with provisions of the law to apply to the 2018 tax year. The TCJA allowed state executives to designate up to 25% of low-income tracts and some tracts contiguous with low-income tracts as OZs.[2] The governors of each state designated tracts from among those eligible in early 2018 and all states completed their designations by June 2018 (U.S. Department of the Treasury, 2018).

For the purposes of the OZ legislation, the definition of a low-income community (LIC) is from section 45D(e) of the U.S. tax code (Internal Revenue Service, 2010) and requires that the tract meet at least one of the following criteria:

1. A poverty rate of at least 20%,
2. The tract is not in a metropolitan area and median family income does not exceed 80% of statewide median family income,
3. The tract is in a metropolitan area and median family income is less than or equal to 80% of the greater of metropolitan area or statewide family income,
4. The tract has a population of less than 2,000 people, it is within an empowerment zone, and it is contiguous to one or more LIC.

At least 95% of tracts designated to receive favorable OZ tax treatment had to be an LIC as defined above. Additionally, the median income of any designated tract contiguous to an LIC must be less than 125% that of the median income of the LIC with which the tract is contiguous (US House of Representatives, 2017).

The OZ program includes two different types of tax relief for capital gains. First, investors with realized capital gains on existing assets can defer paying tax on the gains by investing them into existing or new businesses or newly constructed real

[2] If the number of low-income tracts in a state is less than 100, a total of 25 tracts may be designated (US House of Representatives, 2017).

estate in designated OZ tracts. Abstracting from some details, taxes on the realized capital gains from the prior investments can be deferred for seven years at which point the taxable basis of the capital gains is reduced by 15% and the tax becomes payable. Investors can either invest directly in an OZ or in a Qualified Opportunity Fund (QOF). A QOF must invest at least 90% of its assets into existing or new businesses or newly constructed real estate in an OZs. Because of this transfer of capital gains on old assets into a QOF, investors sometimes refer to the OZ program as the "1031 exchange program on steroids". Second, and perhaps most importantly, capital gains on any new investments are totally tax-free as long as the new investment is held for at least ten years. For additional details, see Internal Revenue Service (2020) and US House of Representatives (2017).

Policy makers' stated motivation for creating OZs was to spur job growth in areas left behind by the economic expansion. In particular, Republicans in the Senate asserted the rationale for OZs as follows:

> Although the post-recession U.S. economy has entered its 10th year of expansion, job and wage growth has been geographically uneven. Approximately 50 million Americans live in communities where the decline of industries like mining, manufacturing, and textiles has led to stubbornly high rates of unemployment and poverty.
>
> One significant handicap for these communities has been the lack of access to loans, grants, and venture capital needed to start or expand a small business. Opportunity zones were devised to address this gap. *US Senate Republican Policy Committee (2019)*

Similarly, the Internal Revenue Service (2020) asserts that "[O]pportunity zones are an economic development tool - that is, they are designed to spur economic development and job creation in distressed communities." Treasury Secretary Steven Mnuchin called the creation of OZs "one of the most significant provisions of the Tax Cut and Jobs Act" and a provision that would stimulate job creation (U.S. Department of the Treasury, 2018).

While policy makers did not clarify why they believed the market distribution of economic activity across space was inefficient or inequitable, economists propose several arguments for place-based policies; see Neumark and Simpson (2015) for an overview. Perhaps the most compelling efficiency-based reason is that multiple equilibria may arise in models with agglomeration economies and a particular location may be stuck in a bad equilibrium; see Kline (2010) for an illustration. Under this rationale, a successful place-based policy would at a minimum increase employment. Equity-based rationales for place-based policies similarly would suggest a minimum requirement for a policy to be successful is for it to generate an increase in labor demand, and the most frequently mentioned rationale for the policy by policy makers is job creation (Internal Revenue Service, 2020; U.S. Department of the Treasury, 2018). We thus assess the extent to which the OZ legislation achieved its stated goals.

2.2 Methodology

Similar to the approach Chen, Glaeser, and Wessel (2019) use to identify the effect of the OZ program on house prices, we use a difference-in-difference (DiD) strategy to identify the effect of the program on tract-level employment and establishment growth. This method exploits the discretion left to state Governors to designate particular tracts for preferential tax treatment of the OZ program. While governors may have chosen tracts at least partially based on political considerations, such that designated tracts may differ systematically from those left undesignated, we include many controls for fixed characteristics of tracts and perform a variety of analyses to show that selected tracts and eligible-but-not-chosen tracts did not differ in unobserved ways.

We compare two-year employment growth in tracts that were designated, tracts we refer to as "Designated," with tracts that were eligible to receive benefits based on the criteria described in Section 2.1 but not chosen. We refer to the eligible-but-not-chosen as "Other" tracts throughout. While all eligible tracts including those ultimately designated satisfy the eligibility criteria, we capture systematic differences in outcomes between Designated and Other tracts that are not absorbed by our control variables by using a fixed effect for Designated. We also consider a specification in which we include tract fixed effects and find extremely similar results to our benchmark specification.

All of our DiD analyses use the following regression specification

$$Y_{i,t} = \alpha_0 + \alpha_1 P_t + \alpha_2 D_i + \alpha_3 D_i P_t + \mathbf{X_i} \alpha_X + \epsilon_{i,t} \tag{1}$$

where $Y_{i,t}$ is two-year growth in an economic variable of interest in the tract, P_t is a dummy variable equal to 1 for the post-2017 period, 0 otherwise, D_i is a dummy variable that takes a value of 1 if the tract was Designated and 0 otherwise, and $\mathrm{X_i}$ is a vector of characteristics of the tract that do not vary over the observation periods. In our initial regressions, we include observations from 2015-2017 and 2017-2019 and all tracts eligible to receive preferential tax treatment from the OZ legislation.[3] We

[3]The list of all eligible tracts and those ultimately designated is available at

vary the sample dates, the set of tracts in the sample, and $Y_{i,t}$ and X_i to investigate details and perform a variety of robustness tests.

2.3 Data

Our main dataset is establishment-level employment data from Your-economy Time Series (YTS) and covers 2015-2019. Infogroup is the provider of the licensed database used to create the Your-economy Time Series (YTS). We sum over establishments in each eligible tract to generate two variables of interest at the tract-level: employment and number of establishments. We then calculate two-year growth of each of these outcomes when estimating equation (1).

The YTS data begins in 1997 and covers all US public and private establishments. YTS aggregates data from the Infogroup Business Data historical files. Kunkle (2018) details Infogroup's methodology to gather the data underlying YTS:

> To develop its datasets, Infogroup operates a 225-seat call center that makes contact with over 55,000 businesses each and every day in order to record and qualify company information. During a typical month, 15% of the entire Infogroup business dataset is re-verified. On average, 150,000 new businesses are added while 100,000 businesses are removed each month, capturing the dynamic business churn happening in the economy. Infogroups team also identifies new companies through U.S. Yellow Pages, county-level public sources on new business registrations, industry directories, and press releases.

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https://edit.urban.org/sites/default/files/urbaninstitute_tractleveloz analysis_update1242018.xlsx.

Kunkle (2018) compares the YTS data with employment data from the US Bureau of Labor Statistics (BLS). He finds that the YTS data is as encompassing as the data in the Current Population Survey (CPS). Additional information on the YTS data are available at `https://wisconsinbdrc.org/data/`.

For the regression covariates X_i, we use tract-level data from the 2013-2017 5-year American Community Survey (ACS).[4] We include the share of the population that is white, the share with higher education, the share that rent, the share living in poverty, the share covered by health insurance among native-born individuals, the log of median annual earnings, the log of median annual household income, the log of median monthly gross rent, the share of households receiving supplemental income, the average daily commute time, and the share of the population that is employed.[5]

Table 1 summarizes the data in our preliminary, unrestricted sample of all eligible tracts. We highlight here a few points from the table. First, the average population was 4,172 people and the average poverty rate was 19%. Second, establishments located in tracts in this sample employed 2,148 workers, on average, although only 29.7% of the resident population of these tracts was employed. Finally, the rightmost column of Table 1 highlights the presence of outlier tracts with extreme values for employment and establishment growth. We adopt various strategies that we discuss later to address the impact of outliers on results.

Table 2 reports these same variables separately for Designated and Other tracts. Consistent with the presumption that state executives used the OZ program to benefit

[4]Source: https://www.census.gov/data/developers/data-sets/acs-5year/2017.html. Table 12 in the Appendix lists the full set of ACS control variables we include in our regressions; we use the same set of ACS control variables as Chen, Glaeser, and Wessel (2019).

[5]Supplemental income includes food stamps/Supplemental Nutrition Assistance Program (SNAP), public assistance income, or Supplemental Security Income (SSIP).

the maximum number of people, employment and the number of establishments are substantially higher in Designated than in Other tracts. Other tracts had an average of 1,912 employees while Designated tracts had average employment of 3,156 people. Designated tracts also have a higher poverty rate (25% vs. 18%), lower median household income, lower median earnings, less education, and a higher percentage of non-white residents than Other tracts. While Designated tracts are larger in terms of employment and the number of establishments than the Other tracts, they experienced lower growth in employment and the number of establishments in the two years prior to the passage of the TCJA.

As a precursor to our formal DiD analysis, Figure 1 shows average growth rates for employment and the number of establishments in Designated and Other tracts. The top two graphs display the raw data; the bottom two graphs show the data after winsorizing at the 1% level to address the influence of outliers. All graphs show four growth rates: 2011-2013, 2013-2015, 2015-2017 and 2017-2019 labeled, respectively, as 2013, 2015, 2017 and 2019. All graphs show that growth rates of Designated and Other tracts had similar but not identical trends prior to the TCJA, with Other tracts having higher rates of growth for both employment and establishments up through 2017. The positive effect of the TCJA on growth in Designated tracts from 2017-2019 is quite visible in the bottom two figures, a finding we confirm in our regression analyses.

Table 1: 2017 Characteristics of Eligible Tracts

Variable	Obs.	Mean	SD	Median	Minimum	Maximum
Designated	41,174	0.190	0.392	0	0	1
Number of entered establishments	41,174	48.7	70.6	30	0	2,456
Number of exited establishments	41,161	43.4	67.9	28	0	4,709
Percent of entered establishments	41,161	0.279	0.215	0.224	0	9
Percent of exited establishments	41,161	0.235	0.094	0.216	0	1
Number of establishments	41,174	202	266	130	1	12793
Growth in the number of establishments	41,161	0.044	0.180	0.016	-0.864	9
Employment	41,174	2,148	4,013	1,137	1	235,158
Employment growth	41,161	0.039	0.349	0.014	-0.985	41.1
Total housing	41,164	1,534	707	1,446	0	12,768
% White	41,146	0.659	0.280	0.735	0	1
% Higher ed	41,146	0.191	0.104	0.173	0	1
% Renters	41,107	0.455	0.227	0.426	0	1
% Native-born with health insurance	41,139	0.891	0.060	0.901	0	1
% Poverty	41,146	0.190	0.103	0.170	0	1
% Supplemental income	41,146	0.092	0.064	0.079	0	0.528
% Employed	41,146	0.297	0.078	0.3	0	1
Median annual earnings	41,058	$27,384	$7,929	$26,772	$2,499	$116,354
Median annual household income	41,029	$44,553	$15,531	$43,077	$2,499	$177,824
Median monthly gross rent	40,917	$899	$308	$832	$99	$3,501
Population	41,164	4,172	1,994	3,905	0	40,402
Average daily commuting time (min)	26,190	36.9	14.8	34.9	3.3	632.5

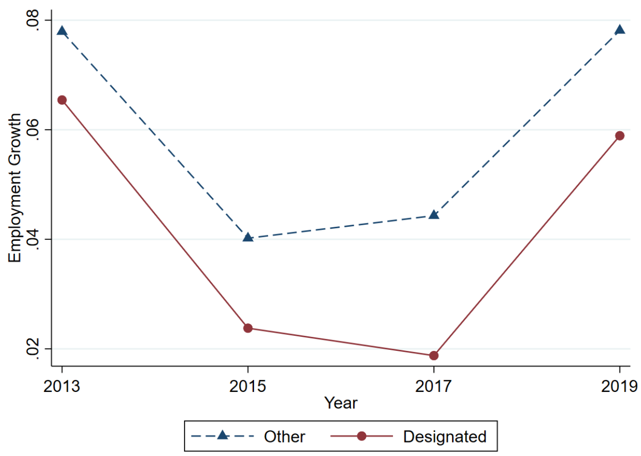
Notes: 1) Growth in employment and the number of establishments is measured over the two-year 2015-2017 period.

Table 2: 2017 Characteristics of Eligible Tracts by Designation

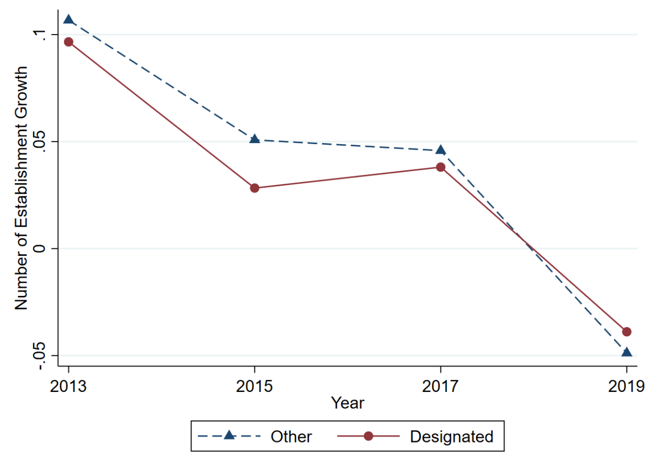
Variable	Mean		SE		t-value for
	Other	Designated	Other	Designated	diff. in means
Designated	0	1	0	0	
Number of entered establishments	46.2	59.4	66.8	84.0	-15.0
Number of exited establishments	40.7	55.2	63.9	81.7	-17.1
Percent of entered establishments	0.284	0.259	0.210	0.234	9.11
Percent of exited establishments	0.238	0.221	0.095	0.085	14.4
Number of establishments	186	269	244	334	-25.1
Growth in the number of establishments	0.046	0.038	0.171	0.213	3.41
Employment	1912	3156	3589	5349	-24.8
Employment growth	0.044	0.019	0.362	0.285	5.825
Total housing units	1550	1464	711	687	9.73
% White	0.680	0.574	0.271	0.299	30.4
% Higher ed	0.198	0.160	0.106	0.090	29.3
% Renters	0.432	0.552	0.222	0.224	-42.9
% Native-born with health insurance	0.894	0.879	0.058	0.064	20.0
% Poverty	0.177	0.246	0.097	0.110	-55.3
% Supplemental income	0.086	0.119	0.060	0.072	-41.8
% Employed	0.303	0.268	0.077	0.077	36.8
Median annual earnings	$28,087	$24,386	$7899	$7335	37.7
Median annual household income	$46,435	$36,538	$15,444	$13,167	52.3
Median monthly gross rent	$915	$826	$314	$271	23.1
Population	4208	4022	1997	1973	7.5
Average daily commuting time (min)	36.8	14.7	37.1	15.2	-1.11

Notes: 1) Growth in employment and the number of establishments is measured over the two-year 2015-2017 period.

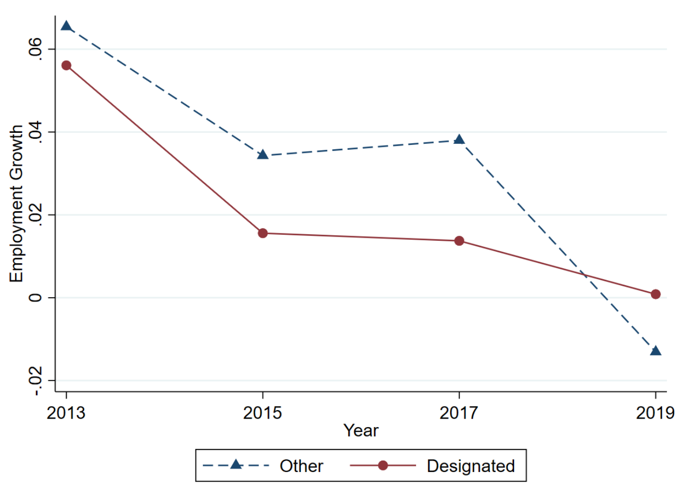
Figure 1: Biennial Tract Growth Rates for Eligible Tracts



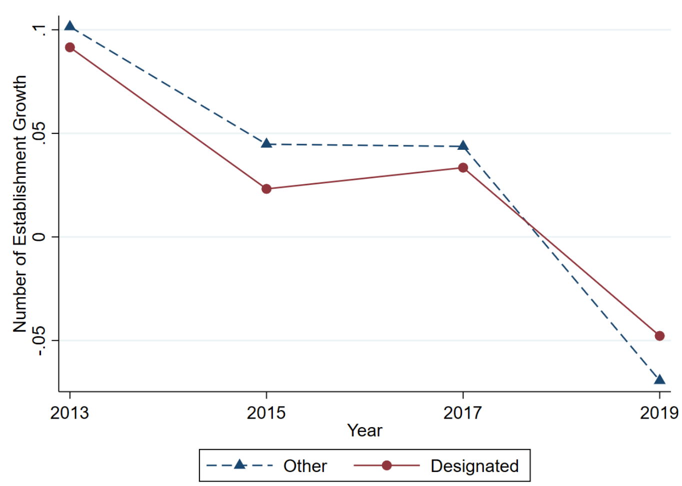
(a) Employment Growth, Raw Data



(b) Establishment Growth, Raw Data



(c) Employment Growth, Winsorized at 1%



(d) Establishment Growth, Winsorized at 1%

3 Results

3.1 All eligible tracts

Table 3 presents DiD results for employment (panel A) and establishment growth (panel B). In columns (1) and (3), we include all observations in the sample. In column (1) we include no controls while column (3) includes lagged growth (i.e. growth from 2013-2015) of the dependent variable as well as the full set of tract-level controls from the ACS. For employment growth in panel A, the coefficient on the interaction between D_i and P_t is 0.025 in column 1 and 0.028 in column 3, indicating the OZ program boosted employment growth by about 2.5 percentage points in Designated tracts, although the point estimates are not statistically significant as the standard errors are large. Panel B shows the estimates of the OZ program on establishment growth. The program increased establishment growth by 2.1 - 2.2 percentage points, shown in columns 1 and 3; these estimates are statistically significant.

Table 3: Employment and Establishment Growth Regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	OLS	LAV	OLS	LAV	OLS	OLS	OLS	GLS	FE	OLS
						Winsorized at			Winsorized at 1%	
					0.5%	1%	2.5%	Weighted	CBSA FE	SEs Clustered
ACS Controls	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of CBSAs									928	
Panel A: Employment Growth										
$Designated_i$	-0.027*	-0.015***	-0.018	-0.009***	-0.012**	-0.012***	-0.012***	-0.006*	-0.012***	-0.012***
	(0.015)	(0.003)	(0.015)	(0.003)	(0.005)	(0.005)	(0.004)	(0.003)	(0.005)	(0.004)
$Post_t$	0.001	-0.072***	-0.003	-0.074***	-0.043***	-0.050***	-0.059***	-0.079***	-0.053***	-0.050***
	(0.009)	(0.002)	(0.009)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)	(0.012)
$Designated_i Post_t$	0.025	0.021***	0.028	0.021***	0.038***	0.036***	0.033***	0.018***	0.036***	0.036***
	(0.022)	(0.004)	(0.021)	(0.004)	(0.007)	(0.006)	(0.005)	(0.005)	(0.006)	(0.008)
$Emp.Growth_{2013-2015}$			0.098***	-0.003	0.016***	0.009*	0.003	-0.058***	0.005	0.009
			(0.017)	(0.004)	(0.006)	(0.005)	(0.004)	(0.003)	(0.005)	(0.010)
Observations	52,060	52,060	52,053	52,053	52,053	52,053	52,053	52,053	52,053	52,053
R^2	0.000		0.002		0.008	0.010	0.018	0.035	0.010	0.010
Panel B: Establishment Growth										
$Designated_i$	-0.007	-0.005*	-0.007	-0.006**	-0.009***	-0.008***	-0.008***	-0.010***	-0.008***	-0.008*
	(0.007)	(0.003)	(0.007)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.005)
$Post_t$	-0.097***	-0.091***	-0.098***	-0.093***	-0.110***	-0.109***	-0.103***	-0.104***	-0.112***	-0.109***
	(0.004)	(0.002)	(0.004)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.016)
$Designated_i Post_t$	0.021**	0.020***	0.022**	0.018***	0.030***	0.030***	0.026***	0.019***	0.029***	0.030***
	(0.010)	(0.004)	(0.010)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)	(0.009)
$Emp.Growth_{2013-2015}$			0.127***	0.016***	0.024***	0.021***	0.018***	0.012***	0.015***	0.021***
			(0.008)	(0.003)	(0.004)	(0.003)	(0.003)	(0.002)	(0.003)	(0.006)
Observations	52,060	52,060	52,053	52,053	52,053	52,053	52,053	52,053	52,053	52,053
R^2	0.011		0.018		0.071	0.080	0.091	0.117	0.081	0.080

Notes: 1) Columns (2) and (4) report results for quantile regression to the median or Least Absolute Value (LAV). 2) Weight for column (8) is 2015 Census tract employment. 3) In column (10), standard errors are clustered by CBSA. 4) Standard errors in parentheses. 5) ***, **, and * denote significance at the 1%, 5%, and 10% levels. 6) $Emp.Growth_{2013-2015}$ is the growth in tract employment from 2013 to 2015. 7) P_t is a dummy variable equal to 1 for the post-2017 period, 0 otherwise, D_i is a dummy variable that takes a value of 1 if the tract was designated an OZ and 0 otherwise.

As the summary statistics in Table 1 illustrate, our data contains extreme outliers in some tracts and these may disproportionately affect standard errors. In columns (2) (no controls) and (4) (full set of ACS controls) we run Least Absolute Variation regressions i.e., regressions to the median, to mitigate the influence of outliers. According to these specifications, the effect of the OZ program on employment and establishments is positive and highly statistically significant. The point estimates in both columns (2) and (4) indicate that the program raised employment growth by 2.1 percentage points and increased the growth in the number of establishments by 1.8 - 2.0 percentage points.

Columns (5) through (7) present the OLS results when we winsorize the dependent variable at the 0.5%, 1%, and 2.5% levels and include all ACS controls. The results are broadly similar regardless of the level at which we winsorize: these estimates suggest the program increased employment by approximately 3.6 percentage points and the number of establishments by approximately 3 percentage points. For both dependent variables and for all three levels of winsorization, the coefficient on the interaction between D_i and P_i is statistically significant at the 1% level. In the remainder of our analyses, we winsorize the dependent variable at the 1% level whenever we run OLS regressions.

In column (8), we weight the observations by the total employment in the tract in 2015. Weighting by employment reduces the magnitude of the effect on employment to 1.8 percentage points from 3.6 percentage points in our benchmark specification (column (6)), suggesting that the program disproportionately affected less populous tracts.[6] In column (9), we include core-based statistical area (CBSA) fixed effects

[6]Indeed, in binned regression analyses (not reported), we find larger effects for less populous tracts. Similarly, when we weight by tract population instead of employment, also not reported, the effect of

while column (10) clusters the standard errors by CBSA. The estimates are similar (with slightly larger standard errors in the case of clustering by CBSA) to the specification when we simply winsorize at 1%, column (6).

Our preferred regression specifications correspond to columns (4) and (6), LAV and OLS with winsorizing at 1%. For the rest of the analysis, we will focus on these two specifications.

3.2 Metropolitan versus non-metropolitan areas

Columns (1) and (2) of Table 4 show our benchmark specifications for the sample of eligible tracts located in metropolitan areas. The estimated effects on employment and establishment growth are 2.9 - 4.6 percentage points, higher than the estimates for all eligible tracts reported earlier. Columns (3) and (4) report the results for the sample of eligible tracts outside of metropolitan areas. For tracts in non-metropolitan areas, the results are different: The estimate of the OZ program on employment growth is essentially zero and the estimate on establishment growth is negative. This latter result is our only negative finding of the OZ program on growth. Since we are mostly concerned about employment growth, we conclude that the OZ program had little to no impact on growth in tracts that are not located in metropolitan areas and we drop these tracts from our sample in all analyses that follow. We refer to the metropolitan-area sample of tracts and specifications in columns (1) and (2) as our "benchmark specifications" for the remainder of the paper.

the program on the number of establishments declines.

Table 4: Employment and Establishment Growth Within and Outside of Metro Areas

	(1)	(2)	(3)	(4)
	LAV	OLS	LAV	OLS
		Winsorized at 1%		Winsorized at 1%
ACS Controls	Yes	Yes	Yes	Yes
	Metropolitan Area		Non-Metropolitan Area	
Panel A: Employment Growth				
D_i	-0.014***	-0.019***	0.008	0.015
	(0.003)	(0.005)	(0.007)	(0.012)
P_t	-0.091***	-0.077***	-0.016***	0.044***
	(0.002)	(0.003)	(0.004)	(0.007)
D_iP_t	0.029***	0.046***	-0.012	-0.000
	(0.005)	(0.007)	(0.010)	(0.015)
$Emp.Growth_{2013-2015}$	-0.005	-0.005	0.021***	0.048***
	(0.004)	(0.006)	(0.007)	(0.011)
Observations	40,944	40,944	11,109	11,109
R^2		0.020		0.017
Panel B: Establishment Growth				
D_i	-0.014***	-0.016***	0.016***	0.024***
	(0.003)	(0.003)	(0.006)	(0.007)
P_t	-0.117***	-0.140***	-0.015***	0.003
	(0.002)	(0.002)	(0.003)	(0.004)
D_iP_t	0.032***	0.043***	-0.022***	-0.023**
	(0.004)	(0.005)	(0.007)	(0.009)
$Emp.Growth_{2013-2015}$	0.010***	0.015***	0.045***	0.039***
	(0.004)	(0.004)	(0.005)	(0.007)
Observations	40,944	40,944	11,109	11,109
R^2		0.125		0.011

Notes: 1) Columns (1) and (3) report results for quantile regression to the median or Least Absolute Value (LAV). 2) Standard errors in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels. 4) $Emp.Growth_{2013-2015}$ is the growth in tract employment from 2013 to 2015. 5) P_t is a dummy variable equal to 1 for the post-2017 period, 0 otherwise, D_i is a dummy variable that takes a value of 1 if the tract was designated an OZ and 0 otherwise.

3.3 Robustness

3.3.1 LICs

A tract is eligible to be designated if it is an LIC or if it is contiguous to an LIC (non-LIC). We identify whether the effect of the program differs for LIC and non-LIC tracts by running the DiD regression (1) separately for the LIC and non-LIC tracts. Columns (1) and (2) of Table 5 show the results for tracts eligible by the LIC criteria. LIC tracts experienced similar growth in employment and establishments as the overall sample of all tracts in metropolitan areas, between 3.3 - 5.0 percentage points. Columns (3) and (4) repeat this analysis for tracts eligible by the contiguity criteria (non-LIC). Our point estimates suggest these tracts experienced much faster employment growth, 12.4 - 13.3 percentage points, and much faster establishment growth, 8.2 - 8.8 percentage points. However, the standard errors on these estimates are also higher.[7]

[7]Recall that no more than 5% of the Designated tracts could be designated by the contiguity criteria, which reduced the non-LIC sample size to around 4,910 tracts out of which around 89 were designated. The number of observations in column (3) of table 5, 9,510, is equal to two times 4,910 less 310 observations from 155 tracts where we do not have information on commuting time.

Table 5: Robustness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	LIC		Non-LIC		3-mile Ring		LIC + 3-mile Ring		Placebo	
	LAV	OLS Winsorized at 1%	LAV	OLS Winsorized at 1%	LAV	OLS Winsorized at 1%	LAV	OLS Winsorized at 1%	LAV	OLS Winsorized at 1%
Panel A: Employment Growth										
D_i	-0.015***	-0.023***	-0.005	0.004	-0.015***	-0.023***	-0.016***	-0.020***	-0.008***	-0.012***
	(0.004)	(0.005)	(0.022)	(0.029)	(0.004)	(0.005)	(0.004)	(0.004)	(0.002)	(0.003)
P_t	-0.094***	-0.084***	-0.077***	-0.058***	-0.102***	-0.098***	-0.129***	-0.155***	0.007***	0.007***
	(0.002)	(0.003)	(0.004)	(0.006)	(0.003)	(0.004)	(0.003)	(0.003)	(0.001)	(0.002)
D_iP_t	0.033***	0.050***	0.133***	0.124***	0.040***	0.064***	0.041***	0.055***	-0.006**	-0.007
	(0.005)	(0.007)	(0.032)	(0.041)	(0.005)	(0.007)	(0.005)	(0.005)	(0.003)	(0.004)
$Emp.Growth_{2013-2015}$	-0.006	-0.008	-0.003	0.001	-0.014***	-0.022***	0.003	0.003	-0.013***	-0.024***
	(0.005)	(0.007)	(0.010)	(0.012)	(0.005)	(0.007)	(0.005)	(0.005)	(0.002)	(0.003)
Observations	31,434	31,434	9,510	9,510	27,543	27,543	27,543	27,543	41,926	41,926
R^2		0.021		0.016		0.027		0.141		0.029
Panel B: Establishment Growth										
D_i	-0.014***	-0.017***	-0.010	-0.016	-0.015***	-0.023***	-0.015***	-0.019***	-0.011***	-0.016***
	(0.003)	(0.003)	(0.020)	(0.019)	(0.004)	(0.005)	(0.004)	(0.004)	(0.002)	(0.003)
P_t	-0.119***	-0.143***	-0.110***	-0.133***	-0.103***	-0.098***	-0.128***	-0.153***	0.003*	0.004**
	(0.002)	(0.002)	(0.004)	(0.004)	(0.003)	(0.004)	(0.003)	(0.003)	(0.001)	(0.002)
D_iP_t	0.033***	0.045***	0.082***	0.088***	0.040***	0.062***	0.040***	0.053***	0.006*	0.007*
	(0.005)	(0.005)	(0.028)	(0.027)	(0.005)	(0.008)	(0.005)	(0.005)	(0.003)	(0.004)
$Emp.Growth_{2013-2015}$	0.009**	0.014***	0.029***	0.018**	-0.008	-0.021***	0.003	0.004	0.005**	0.015***
	(0.004)	(0.005)	(0.008)	(0.008)	(0.005)	(0.008)	(0.005)	(0.005)	(0.002)	(0.002)
Observations	31,434	31,434	9,510	9,510	23,580	23,580	23,580	23,580	41,926	41,926
R^2		0.125		0.127		0.026		0.136		0.072

Notes: 1) Sample of tracts in metropolitan areas. 2) Columns (1), (3), (5), (7), (9) report results for quantile regression to the median or Least Absolute Value (LAV). 3) Standard errors in parentheses. 4) ***, **, and * denote significance at the 1%, 5%, and 10% levels. 5) $Emp.Growth_{2013-2015}$ is the growth in tract employment from 2013 to 2015. 6) D_i is a dummy variable that takes a value of 1 if the tract was designated an OZ and 0 otherwise. 7) In columns (1)-(8), P_t is a dummy variable equal to 1 for the post-2017 period, 0 otherwise, In columns (9) and (10), P_t is equal to 1 for the 2015-2017 period, 0 otherwise.

3.3.2 Nearby tracts

In this section we restrict the control group to non-selected eligible tracts located within 3 miles of designated OZ tracts. We measure the distance between the centroids of two tracts using the Haversine formula with radius 6,371. The treatment group consists of Designated tracts, as before. By restricting tracts in the control group to be geographically near non-selected eligible tracts, we hope to control for any unobserved local economic forces. Columns (5) and (6) of Table 5 show estimates from this restricted sample. The point estimates are a bit higher than the results shown in Section 3.2, as they suggest employment and establishment growth increased by 4.0 - 6.4 and 4.0 - 6.2 percentage points, respectively. These estimates are robust to further restricting the sample to LIC tracts, as can be seen in columns (7) and (8).

3.3.3 Placebo test

We check the robustness of our results by running a placebo test in which we pretend that legislation for the OZ program occurred in 2015. In implementing the DiD, we compare employment and establishment growth from 2015-2017 with 2013-2015 for Designated tracts relative to Other tracts in metropolitan areas. Columns (9) and (10) of Table 5 report the results. The point estimates of the coefficient on the interaction term $D_i P_t$ are nearly zero and negative for employment growth and nearly zero and positive but small for establishment growth, and only the small negative coefficient on employment growth in the median regression (column 9) is statistically significant at a 5% level. We conclude the results of this placebo test reinforce the validity of our findings of a positive impact of the OZ designation on employment and establishment growth in tracts in metropolitan areas.

3.3.4 Political tract selection

Perhaps not surprisingly, Frank, Hoopes, and Lester (2020) find that the process for selecting specific tracts to receive preferential tax treatment arising from the OZ legislation is somewhat political. To estimate whether this aspect of tract selection affects our results, we collect data on the party of the state Governor and lower house state legislators in 2018. We assign legislators to tracts using the lower chamber State Legislative District Block Equivalent File. As in Frank, Hoopes, and Lester (2020), we define a tract to be politically affiliated with the governor if the tract's lower house representative and the governor belong to the same party.

Many tracts belong to one electoral district, which sends one representative to the lower house. In this case, a tract is represented by one lower house representative. However, some tracts belong to several electoral districts. Moreover, in some states, each electoral district sends two or more representatives to the lower house. 10 U.S. states had multi-member districts as of 2018. In these cases, a tract may have multiple lower house representatives. To capture these cases, we use the share of the tract's lower house representatives that belong to the same party as the governor, %sameparty, to measure political affiliation of the tract. Out of 41,055 tracts included in the analysis, 12,094 (29%) are matched with more than two legislators. As an alternative specification, we also construct the variable $Nsameparty$ that simply counts the number of legislators of the same party as the governor.

Table 6 presents the estimates of a Linear Probability Model in which we check to see if tract political affiliation is predictive of a tract's Designation as an OZ, conditional on the tract being eligible. Columns 1 and 2 show results with the entire sample (inclusive of non-metropolitan tracts) with state fixed effects but no ACS controls for the two definitions of political affiliation. As in Frank, Hoopes, and Lester (2020), tract political affiliation and designation as an OZ is negatively correlated without controlling for tract observable characteristics. Columns 3 and 4 add ACS controls to columns 1 and 2; these columns show that political affiliation and OZ designation are significantly positively correlated once we control for observable tract attributes. Finally, columns 5 and 6 are the same as 3 and 4, but with all non-metropolitan tracts removed from the sample. With this sample restriction, the point estimates fall slightly from those in columns 3 and 4, and the coefficient on $Nsameparty$ is no longer statistically significant at the 5% level.

Column (1) and (2) of Table 7 show that the point estimates in Section 3.2 are robust to controlling for the political affiliation of the tract, the $sameparty$ variable. In columns (3) and (4), we include an interaction of the $sameparty$ variable with the P_t and D_i to see if the measured effect of the OZ program depends on the political affiliation of the tract. The estimate on this triple interaction term is negative and significant for employment growth; the estimate on the triple interaction term is small and insignificant for the establishment growth.

3.4 Heterogeneity

Having demonstrated that the OZ program significantly and positively affected employment and establishment growth in designated tracts, we turn now to understanding what type of employment and establishments the program created.

Table 6: OZ selection and Political Consideration

	(1)	(2)	(3)	(4)	(5)	(6)
ACS Controls	No	No	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
					Metropolitan Area	
$Nsameparty$	-0.009***		0.009**		0.007*	
	(0.003)		(0.004)		(0.004)	
$\%sameparty$		-0.011***		0.017***		0.012**
		(0.004)		(0.005)		(0.006)
Observations	41,055	41,055	25,920	25,920	20,890	20,890
R^2	0.003	0.003	0.099	0.099	0.101	0.101

3.4.1 New or old establishments?

The regression results reported in Table 4 considered the net change in establishments. Here, we consider establishment births and deaths. Table 8 shows that, relative to Other tracts, Designated tracts experienced a reduction in the number of failing establishments, columns (3) and (4), and an increase in new establishments, columns (1) and (2). The table shows that the effect of the OZ program on establishment births is four to six times larger than the effect on establishment deaths.

3.4.2 Intensive or extensive margin?

We now study whether the OZ policy induced employment growth by encouraging the growth of existing establishments (intensive margin) or new establishments (extensive margin). To address this question, we employ three definitions of "existing" establishments. Group 1 includes establishments that existed in all years of the sample, i.e., 2013, 2015, 2017, and 2019. Group 2 includes establishments that existed in 2015, 2017, and 2019. Finally, Group 3 includes all establishments that existed in

Table 7: Employment and Establishment Growth with Political Consideration

	(1)	(2)	(3)	(4)
	LAV	OLS	LAV	OLS
		Winsorized at 1%		Winsorized at 1%
ACS Controls	Yes	Yes	Yes	Yes
Panel A: Employment Growth				
D_i	-0.014***	-0.019***	-0.014***	-0.019***
	(0.004)	(0.005)	(0.004)	(0.005)
P_t	-0.093***	-0.077***	-0.093***	-0.077***
	(0.002)	(0.003)	(0.002)	(0.003)
$D_i P_t$	0.031***	0.046***	0.037***	0.058***
	(0.005)	(0.007)	(0.006)	(0.009)
$\%sameparty$	0.001	0.004	0.002	0.006*
	(0.002)	(0.003)	(0.002)	(0.003)
$D_i P_t \%sameparty$			-0.011	-0.024**
			(0.007)	(0.010)
$Emp.Growth_{2013-2015}$	-0.014***	-0.010*	-0.013***	-0.010*
	(0.004)	(0.006)	(0.004)	(0.006)
Observations	40,716	40,716	40,716	40,716
R^2		0.023		0.024
Panel B: Establishment Growth				
D_i	-0.011***	-0.016***	-0.011***	-0.016***
	(0.003)	(0.003)	(0.003)	(0.003)
P_t	-0.119***	-0.141***	-0.119***	-0.141***
	(0.002)	(0.002)	(0.002)	(0.002)
$D_i P_t$	0.032***	0.043***	0.031***	0.040***
	(0.004)	(0.005)	(0.006)	(0.006)
$\%sameparty$	0.001	0.002	0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.002)
$D_i P_t \%sameparty$			0.002	0.005
			(0.006)	(0.006)
$Emp.Growth_{2013-2015}$	0.000	0.007*	-0.000	0.007*
	(0.004)	(0.004)	(0.004)	(0.004)
Observations	40,716	40,716	40,716	40,716
R^2		0.140		0.140

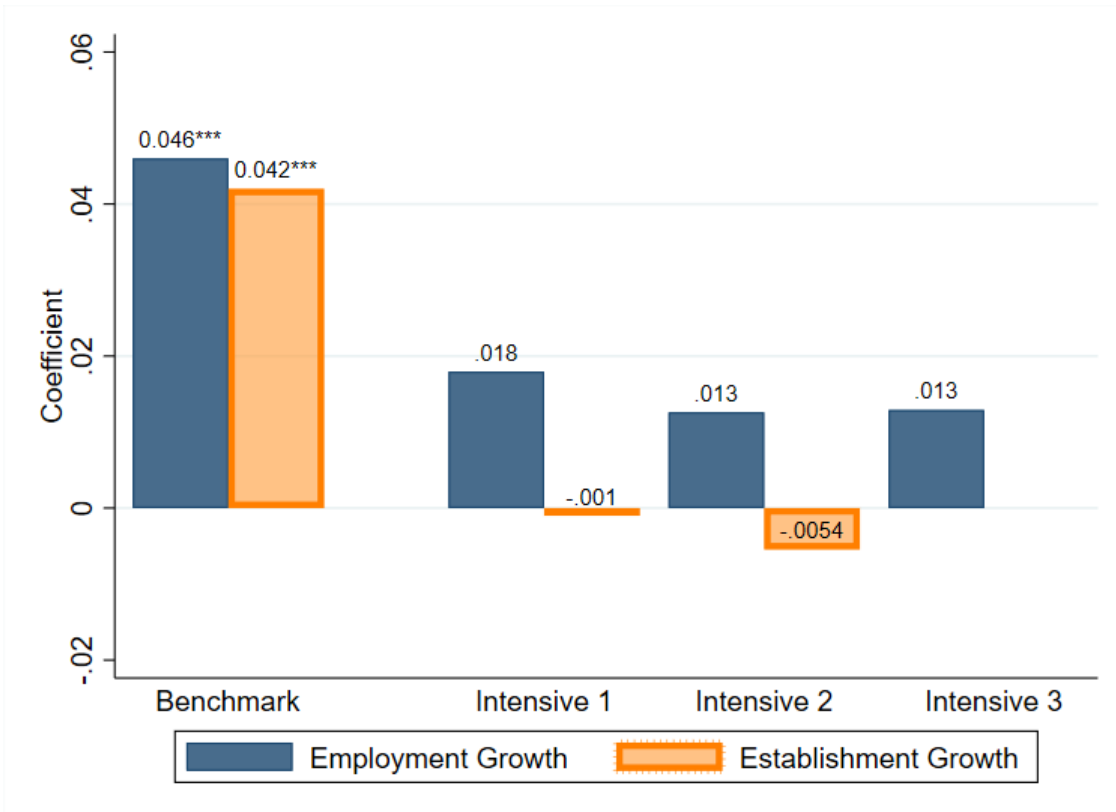
Notes: 1) Sample of tracts in metropolitan areas. 2) Columns (1), (3) report results for quantile regression to the median or Least Absolute Value (LAV). 3) Standard errors in parentheses. 4) ***, **, and * denote significance at the 1%, 5%, and 10% levels. 5) $Emp.Growth_{2013-2015}$ is the growth in tract employment from 2013 to 2015. 6) P_t is a dummy variable equal to 1 for the post-2017 period, 0 otherwise, D_i is a dummy variable that takes a value of 1 if the tract was designated an OZ and 0 otherwise.

Table 8: Establishment Birth and Death Regressions

	(1)	(2)	(3)	(4)
	Percent of Entered Establishment		Percent of Exiting Establishment	
	LAV	OLS	LAV	OLS
		Winsorized at 1%		Winsorized at 1%
ACS Controls	Yes	Yes	Yes	Yes
D_i	-0.025***	-0.031***	-0.012***	-0.011***
	(0.003)	(0.003)	(0.002)	(0.002)
P_t	-0.056***	-0.089***	-0.014***	-0.008***
	(0.002)	(0.002)	(0.001)	(0.001)
D_iP_t	0.031***	0.040***	-0.005*	-0.009***
	(0.004)	(0.004)	(0.003)	(0.002)
$Emp.Growth_{2013-2015}$	0.083***	0.104***	0.150***	0.112***
	(0.003)	(0.003)	(0.002)	(0.002)
Observations	40,944	40,944	40,944	40,944
R^2		0.177		0.211

Notes: 1) Sample of tracts in metropolitan areas. 2) Columns (2) and (4) report results for quantile regression to the median or Least Absolute Value (LAV). 3) Standard errors in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels. 4) $Emp.Growth_{2013-2015}$ is the growth in tract employment from 2013 to 2015. 5) P_t is a dummy variable equal to 1 for the post-2017 period, 0 otherwise, D_i is a dummy variable that takes a value of 1 if the tract was designated an OZ and 0 otherwise.

Figure 2: Estimates with Existing Establishments



Notes: 1) Sample of tracts in metropolitan areas. 2) ***, **, and * denote significance at the 1%, 5%, and 10% levels. 3) See definitions of Intensive 1, 2, 3 in the text. 4) The benchmark results are from column (2) of Table 4, OLS Winsorized at 1%.

2015, 2017, and 2019 and remained in the same tract in all three years.

Figure 2 presents the results for each of the definitions. Each shows the coefficient estimate on D_iP_t, the key interaction term; the blue bars show growth of employment and the red bars show growth in establishments. Given that we restrict the sample to establishments that existed before 2017, any establishment growth we estimate in OZ tracts is driven by establishments that move across tracts. By definition, we cannot see the effect on establishment growth at the tract-level for the third group. Summarizing the results of Figure 2, the blue bars show the effect of the OZ policy on employment growth from existing establishments is positive, but small and insignificant while the red bars show that results for establishment growth are essentially zero. Thus, the creation of new establishments is the driving force of the positive effect of the OZ program on employment growth.

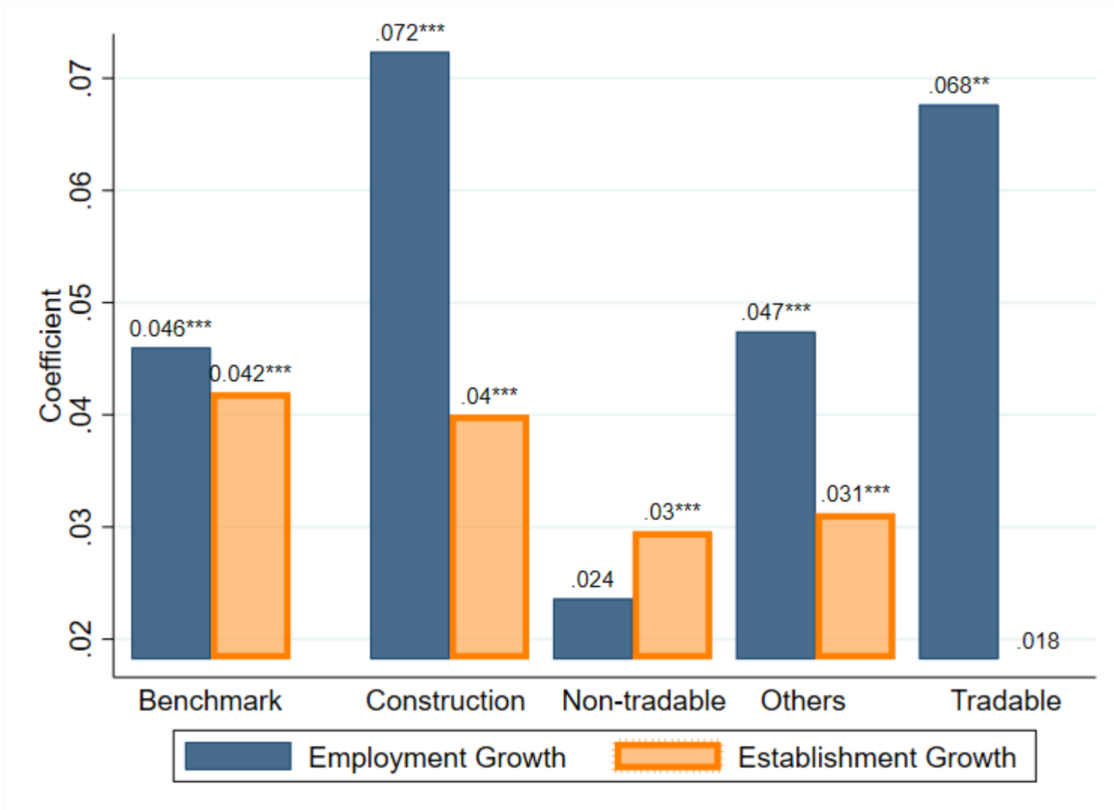
3.4.3 Which industries are affected?

We now turn to tract employment and establishment growth by industry type. We use the classification of Mian and Sufi (2014) that is based on 4-digit NAICS industries. We winsorize all dependent variables at 1% and run the DiD specifications separately for establishments in the Construction, Non-tradable, Others, and Tradable sectors. The Others category includes a variety of industries that Mian and Sufi (2014) do not classify as tradable or non-tradable.

Figure 3 shows estimates of the impact of the OZ program on each sector. Like Figure 2, the blue bars show coefficient estimates on the interaction term for employment growth and the red bars show coefficient estimates for establishment growth. This figure shows that the OZ program had the largest impact on both employment and establishment growth in the construction industry. Employment growth is lowest in Non-tradable industries and establishment growth is lowest in Tradable industries.

Figure 3 suggests the OZ program largely created construction jobs that may be temporary. To investigate this possibility, we rerun our benchmark DiD specification but exclude establishments in Construction industries. The estimates from this restricted sample decline to 2.8 - 4.5 for employment growth and 3.3 - 4.3 percentage points for establishment growth, but remain statistically significant (not shown).

Figure 3: Estimates by Industry Type



Notes: 1) Sample of tracts in metropolitan areas. 2) Benchmark estimate is from Table 4, column (2). 3) ***, **, and * denote significance at the 1%, 5%, and 10% levels.

We also look at tract employment and establishment growth by 1-digit NAICS sectors. Shown in Table 9, we aggregate 2-digit NAICS sectors into six broad sectors that represent (1) agriculture, (2) construction, (3) manufacturing, (4) trade, (5) information, FIRE (finance, insurance and real estate) and management, and (6) services. Then we estimate the impact on employment and establishment growth for each 1-digit NAICS sector. Figure 4 shows estimates with the dependent variable winsorized at the 1% level. The estimates for NAICS sectors 2 and 5, construction and information, FIRE and management, are quite a bit higher than our benchmark estimates; the response of the employment and establishment growth in NAICS sectors 4 and 6, trade and services, are close to our benchmark results; and the response of employment and establishment growth is insignificant for agriculture and manufacturing, NAICS sectors 1 and 3.

Table 9: One digit NAICS industries

2-digit NAICS Sectors	Description	1-digit NAICS Sectors
11	Agriculture, Forestry, Fishing and Hunting (not covered in economic census)	1
21	Mining, Quarrying, and Oil and Gas Extraction	
22	Utilities	2
23	Construction	
31-33	Manufacturing	3
42	Wholesale Trade	
44-45	Retail Trade	4
48-49	Transportation and Warehousing	
51	Information	
52	Finance and Insurance	
53	Real Estate and Rental and Leasing	
54	Professional, Scientific, and Technical Services	5
55	Management of Companies and Enterprises	
56	Administrative and Support and Waste Management and Remediation Services	
61	Educational Services	
62	Health Care and Social Assistance	
71	Arts, Entertainment, and Recreation	6
72	Accommodation and Food Services	
81	Other Services (except Public Administration)	
92	Public Administration (not covered in economic census)	

Source: `https://www.census.gov/programs-surveys/economic-census/guidance/understanding-naics.html`.

Figure 4: Estimates by 1-digit NAICS industry

Notes: 1) Sample of tracts in metropolitan areas. 2) Benchmark estimate is from Table 4, column (2). 3) ***, **, and * denote significance at the 1%, 5%, and 10% levels. 4) Broad 1-digit NAICS sectors: (1) agriculture, (2) construction, (3) manufacturing, (4) trade, (5) information, FIRE (finance, insurance and real estate) and management, and (6) services, see Table 9.

Table 10: Creation of New Industries

	(1)	(2)	(3)	(4)
	2-digit		4-digit	
	Main	Placebo	Main	Placebo
ACS Controls	Yes	Yes	Yes	Yes
D_i	-0.058***	-0.060***	0.005***	0.005***
	(0.008)	(0.008)	(0.001)	(0.001)
P_t	0.071***	-0.010*	0.006***	-0.001
	(0.005)	(0.005)	(0.001)	(0.001)
$D_i P_t$	0.020	-0.003	-0.001	-0.003*
	(0.012)	(0.012)	(0.001)	(0.002)
Observations	44,676	45,652	44,676	45,652
R^2	0.021	0.022	0.009	0.008

Notes: 1) Sample of tracts in metropolitan areas. 2) Robust standard errors, in parentheses. 3) ***, **, and * denote significance at the 1%, 5%, and 10% levels. 4) D_i is a dummy variable that takes a value of 1 if the tract was designated an OZ and 0 otherwise. 5) In columns (1) and (3), P_t is a dummy variable equal to 1 for the post-2017 period, 0 otherwise, In columns (2) and (4), P_t is equal to 1 for the 2015-2017 period, 0 otherwise. 6) The dependent variable is the number of new industries created in a two-year period t.

3.4.4 Creation of new industries

In this section, we ask if the OZ legislation encouraged creation of jobs in industries that had no prior establishments or employment in any given Designated tract. To answer this question, we create a dummy variable that takes a value of 1 if at least one new establishment was created in a two-year period in a "new", i.e., previously un-represented, industry for the tract and 0 otherwise. Columns (1) and (3) of Table 10 present results based on the 2-digit and 4-digit NAIC classifications when this dummy variable is the dependent variable in the DiD. Despite the fact that the estimates in these columns are not statistically significant, we run a placebo test of designation on the number of new industries created in 2013-2015 (pre-period) and 2015-2017 (post-period) and also find insignificant estimates, shown in columns (2) and (4). We thus conclude that the policy did not create jobs in industries that had no prior establishments in Designated tracts.

3.4.5 Who gets hired?

Policymakers might be concerned that new jobs created by the OZ program are predominantly being filled by high-wage workers who have no immediate connection to the low-income tracts targeted by the OZ program. We thus explore growth in employment created by the OZ program by the skill level of the industry. We measure the skill level of the industry using the average educational level of an industry from the 2004 ACS, which ranges from 1 for "some high school" to 5 for "graduate school". We use the four-digit NAICS code to classify industries into education quantiles based on the intensity of skilled occupations in each industry. We take the skill-level of each four-digit NAICS code from Oldenski (2012) and are grateful to Lindsay Oldenski for sharing her data with us.

Figure 5 shows results. The first two sets of bars show our benchmark estimates. The next four bars show results for industries with the intensity of skilled occupations below the median ("Bottom 50%") and above the median ("Top 50%"). The final ten bars show results for all five quintiles of the education measure. The figure suggests growth in employment and establishments is broad-based across both skilled and unskilled industries with the greatest growth in the middle skill quintile.

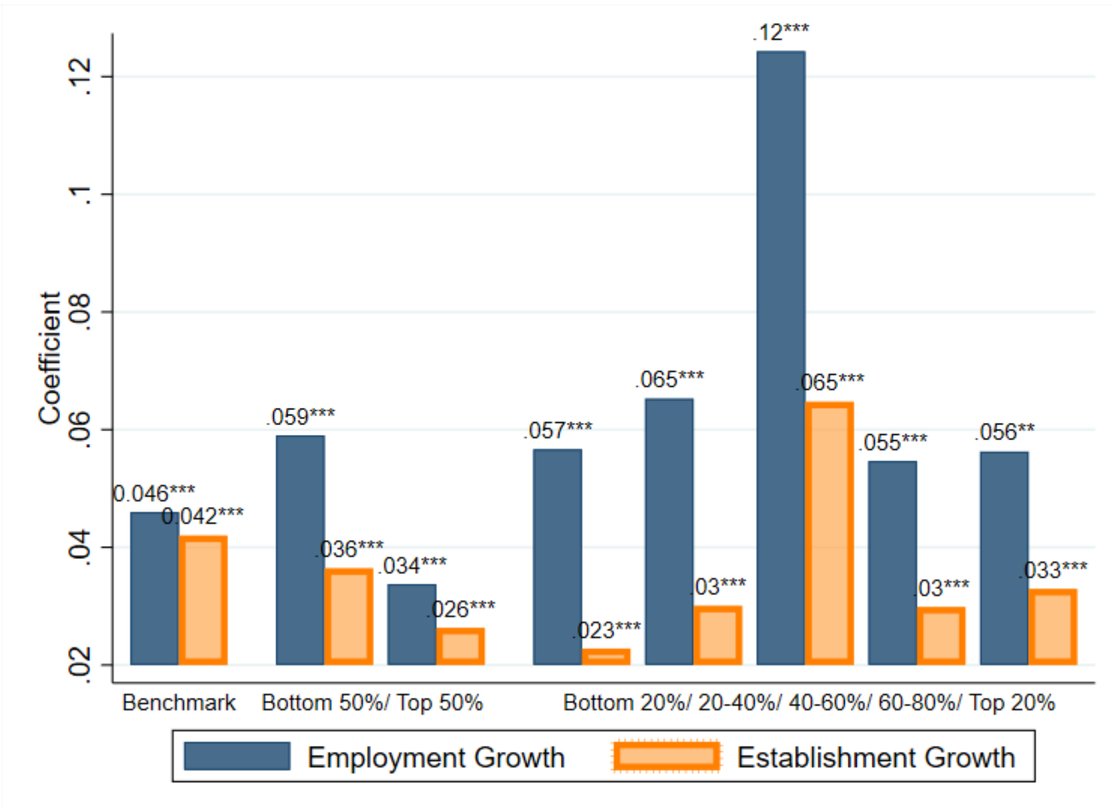
3.4.6 Heterogeneity by tract characteristics

Figure 6 presents our final two analyses studying heterogeneity of the impact of the OZ legislation on outcomes. In the first analysis, we form two groups based on whether the poverty rate in the tract is above ("High") or below ("Low") the median for eligible tracts. The effect of the program on employment is similar for the two groups of tracts, but the lower-poverty tracts experience a larger increase in the number of establishments. In the second analysis, we form two groups based on whether the population of white residents in the tract is above ("High") or below ("Low") the median for eligible tracts. The figure makes obvious that the program had much larger effects in tracts with a lower share of white households in the population.

3.5 Displacement of employment

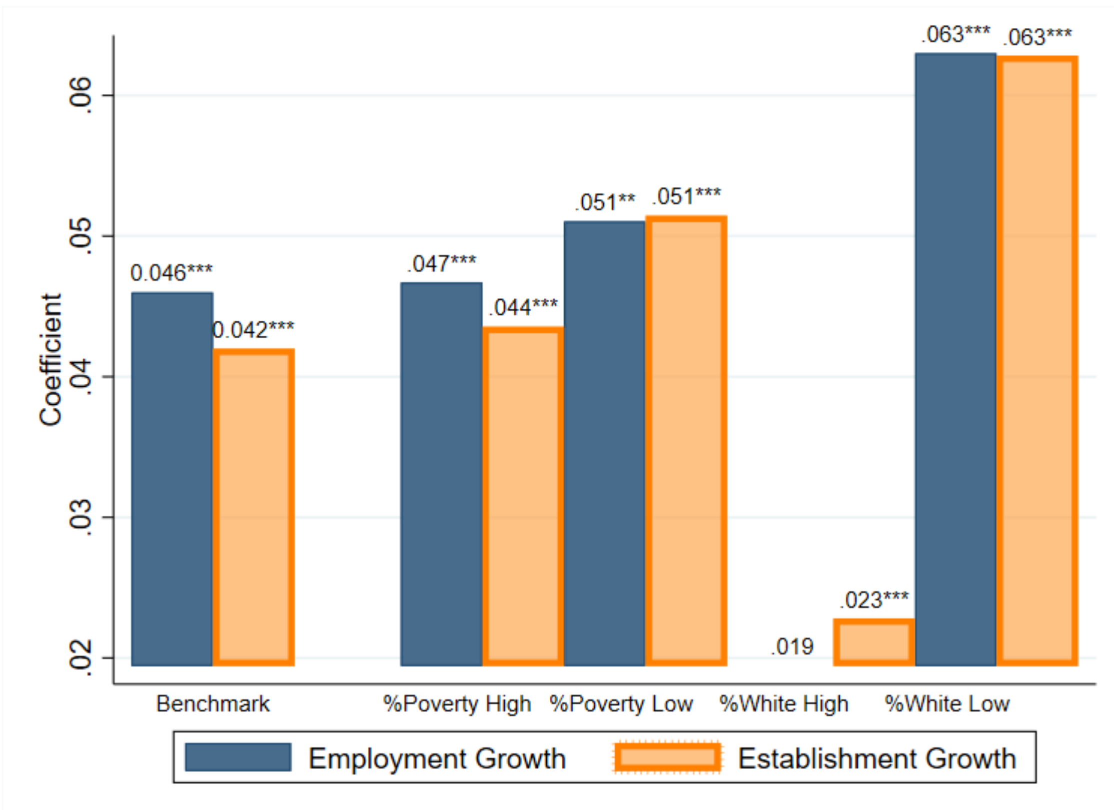
We now investigate the extent to which the program simply shifted employment from nearby tracts to Designated tracts or whether the presence of an OZ in an adjacent tract increased employment through agglomeration effects. Previous analysis of place-based policies has found that the direct effects of place-based policies are

Figure 5: Estimates by Education of Industry



Notes: 1) Sample of tracts in metropolitan areas. 2) Benchmark estimate is from Table 4, column (2). 3) ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Figure 6: Estimates by Tract Characteristics



Notes: 1) Sample of tracts in Metropolitan areas. 2) Benchmark estimate is from Table 4, column (2). 2) ***, **, and * denote significance at the 1%, 5%, and 10% levels.

sometimes offset, at least in part, by reductions nearby.[8] To address this question, we compare two-year employment growth in tracts that are contiguous to Designated tracts with tracts contiguous to Other (non-designated eligible tracts). We can take one step further by comparing tracts that are contiguous to tracts contiguous to Designated, with tracts that are contiguous to tracts contiguous to Other (referred as 2-step contiguity). In the following analysis, we include tracts that are up to 4th step contiguous to eligible tracts. Eligible tracts themselves are also included and referred as 0-step contiguous.

Specifically, we run the following regression:

$$\begin{aligned} Y_{i,t} = \alpha_0 + \alpha_{0,k}G_{i,k} + (\alpha_1 + \alpha_{1,k}G_{i,k})P_t + (\alpha_2 + \alpha_{2,k}G_{i,k})D_i \\ + (\alpha_3 + \alpha_{3,k}G_{i,k})D_iP_t + \mathbf{X}_{\mathrm{i}}\alpha_X + \epsilon_{i,t}, \;\; k = 1, 2, 3, 4 \end{aligned} \tag{2}$$

$D_i = 1$ if tract i is k-step contiguous to an OZ for any $k = 0, ...4$. Similarly, $D_i = 0$ if tract i is k-step contiguous to a non-designated eligible tract for any $k = 0, ...4$. $G_{i,k} = 1$ if tract i is k-step contiguous to an eligible tract for $k = 1, 2, 3, 4$. 0-step contiguous group ($G_{i,0} = 1$) is used as the baseline category. α_3 represents the effect of being designated as OZ. $\alpha_{3,k}$ captures the additional effect of designation on tracts that are k-step contiguous beyond the effect of designation. For instance, the effect of designation on a tract 1-step contiguous is $\alpha_3 + \alpha_{3,1}$. Similarly, the estimated effect of

[8]For example, Sinai and Waldfogel (2005) find that an increase in government-financed low-income housing by one unit results in only one-third to one-half of a unit in a market. Baum-Snow and Marion (2009) and Eriksen and Rosenthal (2010)similarly find significant crowding out of new housing supply from the Low Income Housing Tax Credit (LIHTC). Perhaps more directly related to the OZ policy is the finding by Freedman (2012) that investment subsidized through the NMTC program had at most incomplete crowd out effects. To the extent agglomeration economies arise through employment, rather than housing supply, we anticipate less crowding out from employment-creation programs.

designation on a tract 2-steps contiguous is $\alpha_3 + \alpha_{3,2}$.

Column (1) of Table 11 reports coefficient estimates while column (2) shows estimates of the net effect for each step contiguous and the corresponding p-value of the test (where the null is no effect). Column 1 shows that the impact of the OZ designation on employment growth of the designated tract continues to be high, 4.5 percentage points, even after controlling for local spillovers. Columns 1 and 2 show statistically significant positive spillover to contiguous tracts of about 1.9 percentage points, smaller but positive and statistically significant spillovers to communities two tracts away, and no statistically significant spillover effects in tracts further away. Thus, the OZ program created positive employment spillovers to neighboring tracts rather than poaching employment from these tracts.

While we are not able to identify the type of agglomeration economy generating the positive spillovers in adjacent tracts, these results are consistent with findings that some agglomeration benefits decay rapidly with distance. For example, Arzaghi and Henderson (2008) find that agglomeration economies in the birth of new advertising firms decline within 500 meters and are no longer significant after one kilometer. Liu, Rosenthal, and Strange (forthcoming) show that vertical agglomeration economies within a building are strongest on the same floor and are largely gone by a distance of three floors. Rosenthal and Strange (2020) review the evidence on the scale of agglomeration economies and conclude that the strongest agglomeration forces are likely at the neighborhood level.

Table 11: Estimates of Spillover Effects on Neighboring Tracts

	(1)	(2) test of net effect
D_i	-0.018***	
	(0.005)	
P_t	-0.080***	
	(0.003)	
D_iP_t	**0.045*** **	
	(0.007)	
$G_{i,1}D_iP_t$	**-0.026*** **	**0.019**
	(0.009)	**p=0.0006**
$G_{i,2}D_iP_t$	**-0.027** **	**0.018**
	(0.010)	**p=0.0134**
$G_{i,3}D_iP_t$	**-0.030** **	**0.015**
	(0.015)	**p=0.2493**
$G_{i,4}D_iP_t$	**-0.041**	**0.004**
	(0.027)	**p=0.8512**
$Emp.Growth_{2013-2015}$	0.003*	
	(0.002)	
Observations	127,718	
R^2	0.025	

Notes: 1) Results of estimating equation (2) with $Emp.Growth$ as the dependent variable. 2) Standard errors in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels. 3) $Emp.Growth_{2013-2015}$ is the growth in tract employment from 2013 to 2015. 4) P_t is a dummy variable equal to 1 for the post-2017 period, 0 otherwise, D_i is a dummy variable that takes a value of 1 if the tract is itself Designated or contiguous to a Designated tract. 5) Estimation sample is all tracts that are Designated, Eligible, or four steps contiguous to such tracts. 6) Coefficients α_{0i}, α_{1i}, and α_{2i} only shown for $k = 0$; coefficients on ACS controls not shown.

4 Conclusion

We have shown that the OZ program led to significantly higher employment and establishment growth in designated tracts in metropolitan areas. Given the fact that the OZ program is new, we cannot identify the permanence of any newly created jobs. The Council of Economic Advisors (2020) estimates that the program will cost the Federal Government approximately $11 billion in foregone tax revenue. We can use this number to estimate the cost per job. The total employment in all designated tracts in metropolitan areas was 20,822,975 in 2017. We estimate that the program increased employment in designated tracts by approximately 3.75 percentage points thus creating 780,862 new jobs. Using the $11 billion estimate as the cost of the program, and ignoring any employment created in adjacent tracts via any of the spillover effects we document, this translates into a cost per job of $14,087. However, that estimate only considers the foregone tax revenue from funds invested directly in QOFs. Given that QOFs are only 20% of the total investment in OZs, the actual foregone tax revenue could be much higher, perhaps as high as $55 billion if all investments in OZs are held for the full 10 years required.

While a full cost-benefit analysis is beyond the scope of this paper, it is useful to consider the cost per job created in the context of other place-based policies and local incentives. Bartik (2019) estimates that average non-discretionary US place-based incentives cost approximately $24,000 per job. Slattery (2020) finds that, for discretionary firm-specific tax subsidies of at least $5 million, the average cost per job averaged $110,000 or$11,000 per job per year over the 2002-2017 period. Slattery and Zidar (2020) also find that the costs per job created are higher in low-income counties.

Our findings do suggest that programs that subsidize capital rather than employment may be effective in creating employment. Given Neumark and Kolko (2010)'s findings that a wage subsidy to hire low-income workers was ineffective in California, place-based policies may need to encourage hiring of workers of diverse skill levels to help boost employment of low-skill workers. Another possibility is that capital spending in particular, rather than a wage subsidy, is more likely to permanently change an area's infrastructure and thus move an area to a new equilibrium. However, more time is required to assess how successful the policy was in creating long-term jobs for workers of various skill levels.

Finally, we do not model the welfare effects of the OZ program. If the program increases residential rents, in contrast to what Chen, Glaeser, and Wessel (2019) find for home prices, there is a risk that low-income workers could be hurt by the program given the large share of their income they pay toward rent.[9] If evidence emerges that the OZ program increased rents, a model is required to estimate the welfare consequences of the OZ legislation.

[9]A large number of studies that find that a 1% increase in income results in a much less than 1% increase in housing expenditure. See, for example, Rosen (1979), Green and Malpezzi (2003), Glaeser, Kahn, and Rappaport (2008), and Rosenthal (2014).

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A Appendix

Table 12: American Community Survey control variables

ACS Name	Description
B01003_001E	population
B02001_002E	white_population
C24020_001E	employed_population
B08131_001E	minutes_commute
B09010_002E	supplemental_income
B15003_021E	associate
B15003_022E	bachelor
B15003_023E	master
B15003_024E	professional_school
B15003_025E	doctoral
B16009_002E	poverty
B18140_001E	median_earnings
B19019_001E	median_household_income
B25011_001E	acs_total_housing
B25011_026E	renter_occupied
B25031_001E	median_gross_rent
B27020_002E	native_born
B27020_003E	native_born_hc_covered
acs_pct_white	white_population / population
acs_minutes_commute	minutes_commute / employed_population
acs_pct_higher_ed	(associate + bachelor + master + professional_school + doctoral)/population
acs_pct_rent	renter_occupied / total_housing
acs_pct_native_hc_covered	native_born_hc_covered / native_born
acs_pct_poverty	poverty / population
acs_log_median_earnings	log(median_earnings)
acs_log_median_household_income	log(median_household_income)
acs_log_median_gross_rent	log(median gross rent)
acs_pct_supplemental_income	supplemental_income / population
acs_pct_employed	employed_population / population

Notes: (1) Codes in ACS Name column correspond to the code from https://api.census.gov/data/2017/acs/acs5/variables.html, (2) the employed population is defined as all people 16 years old and over who usually worked 35 hours or more per week for 50 to 52 weeks in the (reference period), see `https://www.census.gov/topics/employment/labor-force/about/faq.html#par_textimage_735773790`. (3) The ACS controls are all variables with names starting with “acs”.