The Effect of Capital Gains Taxes on Business Creation and Employment: The Case of Opportunity Zones*

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Abstract

The Tax Cuts and Jobs Act of 2017 established a new program called Opportunity Zones (OZs) that reduces or eliminates capital gains taxes on investment in a limited number of low-income Census tracts. We provide a model illustrating how a change in capital taxation affects employment in existing and new establishments. We then use establishment-level data to show that, in its first two years, the OZ designation increased employment growth relative to comparable tracts by between 3.0 and 4.5 percentage points in metropolitan areas. The job growth occurred in multiple industries and persisted into 2021 rather than quickly disappearing. However, most of the jobs created by the program were likely taken by residents that live outside of the designated tracts, consistent with only 5% of US residents working in the same Census tract as the one in which they live.

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1 Introduction

Traditional economic theory suggests that a reduction in the tax rate on capital increases worker wages, employment, or both. The intuition is as follows: At any given level of risk, investors demand a certain after-tax return. A reduction in the tax rate on capital boosts this after-tax return, all else equal, thereby attracting new capital. As long as the marginal product of labor is increasing in the quantity of capital, this inflow of new capital either increases wages, increases employment, or both. We provide a model showing that, if wages are fixed, a decrease in capital taxes can increase employment through increasing the number of establishments operating, by increasing employment at existing establishments, or both.

Estimating the impact of capital taxation on wages and employment is challenging. In an ideal environment, exogenous variation in capital taxation across households or firms allows researchers to derive estimates of the effect of capital taxes. However, capital is taxed at the federal level, and the federal tax code applies to everyone. Identifying the impact of taxes on employment using aggregate time-series data is difficult, as changes to federal tax policy with respect to capital are infrequent and may be correlated with other policy changes. States and localities differ with respect to their taxation of capital, but tax policy is not set in a vacuum. Local effective tax rates on capital are likely correlated with other taxes and benefits that reflect local economic and political circumstances. This correlation obfuscates the impact of capital taxes on employment and wages.

Occasionally, policy changes in one locality while remaining fixed in an otherwise identical locality, often nearby or adjacent. When this occurs, a comparison of outcomes between the two localities provides a clean estimate of the impact of the policy change. This Difference-in-Difference (DiD) estimation method of using data from treatment and control groups that are spatially proximate has been key to credible estimates of the impact of changes in the minimum wage on employment.[1]

We use a similar technique to estimate the impact of the large change in capital taxation as part of the 2017 Tax Cuts and Jobs Act (TCJA) on employment and business creation. The TCJA created a designation called an Opportunity Zone (OZ) "to spur economic growth and job creation in low-income communities while providing tax benefits to investors."[2] The TCJA specified that households pay zero capital gains taxes on investments in new businesses located in a Census tract designated as an OZ as long as households hold those investments for at least 10 years. While the 10-year exclusion provision is likely to be the most significant tax benefit of the OZ program (Coyne and Johnson, 2023), the OZ program also allows investors to defer paying taxes on existing capital gains by investing them in a Qualified Opportunity Fund (QOF) and reduce the taxable basis on those gains even if the original capital gains were earned outside of an OZ.

For a tract to be eligible to be designated as an OZ, it had to meet low-income and high-poverty thresholds. On a state-by-state basis, the TCJA stipulated that only 25 percent of tracts meeting those income and poverty thresholds could be designated by state executives as an OZ. Thus, many eligible low-income and high-poverty tracts in each state were not selected to receive preferential tax treatment. We use quasi-experimental variation in the designation of OZs across locations to estimate the impact of the reduction in capital taxes on employment and establishment growth.

[1] See, for example, Card and Krueger (2000) and Jardim, Long, Plotnick, van Inwegen, Vigdor, and Wething (Forthcoming)

[2] https://www.irs.gov/credits-deductions/businesses/opportunity-zones.

We find that the elimination of capital gains taxation in OZ-designated tracts located in metropolitan areas increased employment and establishment growth in those tracts by between 3 to 4.5 percentage points between January 1, 2018 and December 31, 2019 relative to similar tracts that were not designated as an OZ. Some of the jobs that were created by the elimination of taxation of capital gains were likely filled by lower-skilled workers. The construction industry experienced the greatest job growth, but the OZ designation also generated job growth in trade and service industries.

Our DiD strategy estimates how the change in tax policy changed employment in treated (OZ-designated) tracts relative to geographically proximate and similar, untreated tracts. Thus, we identify the extent to which this place-based tax policy changed hiring and employment outcomes within a metro area. Our estimation strategy does not identify the effect of the change in tax policy on aggregate or metro-area employment. That said, we find no evidence that the program shifted employment from nearby tracts not receiving preferential tax treatment to the OZ-designated tracts. Instead, we estimate the opposite: nearby tracts not receiving preferential tax treatment experienced increased employment and establishment growth.

Our results provide context for current policy debates on the appropriate level of taxation of capital and contribute to the literature examining the effects of capital gains taxes on investor behavior. Higher taxes on capital gains have been found to decrease the value of equity (Huizinga, Voget, and Wagner, 2018), lower funding for startups (Edwards and Todtenhaupt, 2020), and alter corporate governance decisions of mutual funds (Dimmock, Gerken, Ivković, and Weisbenner, 2018). In response to capital gains taxes, many investors optimally increase their holding period as argued by Dammon and Spatt (1996), Dammon, Spatt, and Zhang (2001), Ivković, Poterba, and Weisbenner (2005) for equity markets and by Shan (2011), Heuson and Painter (2014), Agarwal, Li, Qin, Wu, and Yan (2020) for housing markets. We instead study the effect of capital gains taxes on employment and establishment growth.

We also contribute to the broader literature on the effect of place-based policies on employment, 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. Ours is the first paper looking at the effects of a nationwide place-based policy on job growth in businesses at the tract-level.[3]

Perhaps the closest papers to ours is Freedman, Khanna, and Neumark (2023). Instead of studying creation of employment and businesses inside OZs, Freedman et al. (2023) study outcomes of households living in OZs and find limited to no improvement in residents' earnings, employment, or poverty rates. We study the employment in establishments and see a significant increase in job growth among businesses in OZ tracts. As our main dataset is an establishment-level census, we cannot identify the location of the residents that take the newly created jobs. However, in supplementary analysis using data from the LEHD Origin-Destination Employment Statistics (LODES) we find that most of the jobs created were taken by people living outside of the designated tracts.[4]

In complementary work, Kennedy and Wheeler (2022) use deidentified IRS data

[3]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. See Neumark and Kolko (2010), Billings (2009), Busso, Gregory, and Kline (2013), and Harger and Ross (2016).

[4]Other research has estimated the impact of the TCJA on outcomes unrelated to employment or business creation. Chen, Glaeser, and Wessel (2023) argue that the OZ program increased the growth of single-family house prices in OZs by 0 to 0.5 percentage points. Sage, Langen, and Van de Minne (2019) show that prices rose for redevelopment properties and vacant sites in OZs, but the price of existing commercial properties did not change.

to show that OZs attracted $41.5 billion in investment with the majority (63%) of this investment financing real estate or construction firms. Similarly, we find the largest effects in the construction industry. Rather than analyzing the returns to investors as Kennedy and Wheeler (2022) do, our study identifies the effects on employment.

In contrast to our findings, Atkins, Hernández-Lagos, Jara-Figueroa, and Seamans (forthcoming) and Corinth and Feldman (2022) do not find that the OZ program had positive effects on employment using different methodologies from ours. Atkins et al. (forthcoming) 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 Census tract, which exactly aligns with the geography in the OZ program legislation. Instead of a DiD methodology, Corinth and Feldman (2022) use a regression discontinuity design (RDD) based on Census tract boundaries. Because only a small fraction of eligible tracts were selected, the RDD design is unlikely to find positive effects because it dilutes the effect of the program with the 80% of eligible tracts that were not selected. Further, positive spillovers from the selected tracts to geographically proximate ineligible tracts contaminate the identification scheme.

The next section of the paper provides a stylized, partial equilibrium model illustrating how a decrease in capital gains is likely to affect employment. Section 3 describes the OZ program in detail along with our data and the empirical methodology we use to identify its effects on employment. We present our results in Section 4. Section 5 provides concluding remarks including some analysis of the costs relative to the approximate number of jobs created.

2 Model

We illustrate how a decrease in the tax rate on capital gains can increase both employment at any given establishment and the total number of establishments. Consider an establishment i facing a fixed, worldwide after-tax rate of return on capital that we denote as $\bar{r}$. The relationship between the after-tax return and the pre-tax return r is given by $\bar{r} = (1-\tau)\,r$, where τ is the tax rate on capital income.[5]

An open establishment i can produce output y_i using the decreasing returns to scale production function $y_i = z_i k_i^{\theta\alpha} l_i^{\theta(1-\alpha)}$ where k_i and l_i are capital and labor and z_i is establishment-specific productivity. $\theta \in (0,1)$ determines the degree of returns to scale in capital and labor, and $\alpha \in (0,1)$ determines the capital share of output.

The establishment takes the pre-tax rate of return on its capital r and wage rate of labor w as given such that its profits are

$$\pi_i = z_i k_i^{\theta\alpha} l_i^{\theta(1-\alpha)} \;-\; r k_i \;-\; w l_i \;-\; \kappa,$$

where κ is a fixed cost to the establishment of operating.

The establishment chooses the quantity of capital and labor to maximize profits. The first-order conditions imply that the establishment will spend α and $(1-\alpha)$ shares of the variable costs, θy_i, on capital and labor, correspondingly, i.e.,

$$r\hat{k}_i \;=\; \alpha\theta\hat{y}_i \,, \tag{1}$$

$$w\hat{l}_i \;=\; (1-\alpha)\,\theta\hat{y}_i \,, \tag{2}$$

where $\hat{k}_i$ and $\hat{l}_i$ are the optimal quantities of capital and labor, and $\hat{y}_i$ is output at $\hat{k}_i$

[5] The tax on capital gains reduces the value of future appreciation of a firm's equity, thereby reducing the return on capital, see equations (1), (5), and (38) in Gourio and Miao (2010).

and $\hat{l}_i$. Plugging in $\hat{k}_i$ and $\hat{l}_i$ into the production function yields output as

$$\hat{y}_i = \left[\theta\alpha^{\alpha}(1-\alpha)^{(1-\alpha)}\right]^{\frac{\theta}{1-\theta}} z_i^{\frac{1}{1-\theta}} r^{-\frac{\theta\alpha}{1-\theta}} w^{-\frac{\theta(1-\alpha)}{1-\theta}}. \tag{3}$$

2.1 Extensive Margin

Equations (1) and (2) imply that the profits of establishment i at $\hat{k}_i$ and $\hat{l}_i$, $\hat{\pi}_i$, can be written as $\hat{\pi}_i = (1-\theta)\,\hat{y}_i - \kappa$. An establishment with the productivity draw z_i enters if its profit is non-negative, i.e., when z_i exceeds a threshold level of z_i given by $\mathcal{Z}$

$$\mathcal{Z} = \left(\frac{\kappa}{1-\theta}\right)^{1-\theta}\left[\theta\alpha^{\alpha}(1-\alpha)^{(1-\alpha)}\right]^{-\theta}\left(\frac{\bar{r}}{1-\tau}\right)^{\theta\alpha} w^{\theta(1-\alpha)}.$$

This expression shows that the minimal level of productivity required for establishments to operate profitably declines as the tax rate on capital income declines. To see this, take the logarithms of both sides and then take the derivative with respect to τ to derive $\Delta\mathcal{Z}/\mathcal{Z} = \alpha\theta\Delta\tau/(1-\tau)$. That is, if the tax rate on capital falls, the minimum level of productivity required to operate profitably also falls. This implies that more establishments can operate profitably. Each new establishment hires workers, creating an increase in overall employment.

2.2 Intensive Margin

Each establishment that operates also responds to a cut in the tax rate on capital by hiring more workers. Using Hotelling's lemma, the labor demand of establishment i is

$$\ln \hat{l}_i = \log(-\frac{\partial \hat{\pi}_i}{\partial w}) = \zeta \ + \ \left(\frac{1}{1-\theta}\right) \ln z_i \ - \ \left(\frac{\theta\alpha}{1-\theta}\right) \ln r \ - \ \left(\frac{1-\theta\alpha}{1-\theta}\right) \ln w,$$

where $\zeta = (\ln\theta + (1-\alpha\theta)\ln(1-\alpha) + \alpha\theta\ln\alpha)/(1-\theta)$ is a constant. Since $\theta < 1$ and $\alpha < 1$, employment is (a) increasing in productivity, (b) decreasing in the local wage, and (c) decreasing in the pre-tax rate of return to capital.

To see how capital tax affects the optimal quantity of labor, replace r with $\bar{r}/\left(1-\tau\right)$ in the above to get

$$\ln \hat{l}_i \ = \ \ln\zeta \ + \ \left(\frac{1}{1-\theta}\right) \ln z_i \ - \ \left(\frac{1-\theta\alpha}{1-\theta}\right) \ln w \ - \ \left(\frac{\theta\alpha}{1-\theta}\right) \ln \bar{r} \ + \ \left(\frac{\theta\alpha}{1-\theta}\right) \ln\left(1-\tau\right).$$

As the tax rate on capital falls, optimal employment rises. Consider a change in the capital tax of $\Delta\tau$. With the above impact, the predicted impact on the growth rate of employment at the establishment is

$$\frac{\Delta \hat{l}_i}{\hat{l}_i} \ \approx \ -\alpha \left(\frac{\theta}{1-\theta}\right) \left(\frac{\Delta\tau}{1-\tau}\right). \tag{4}$$

To summarize, as the tax rate of capital falls, existing establishments increase scale and hire more labor.

2.3 Total Impact

The impact of a change in the capital tax rate on employment is the sum of two effects: (1) the extensive margin, which is employment at new establishments that enter the market, all of which have lower productivity than establishments already in the market; (2) the intensive margin, which is the increased employment at existing establishments. There is no reason for the intensive and extensive margins to be of the same magnitude, because different parameters determine the intensive and extensive employment elasticities. As shown by equation (4), the size of the impact of a change in the capital-income tax rate on employment of existing establishments is determined by θ, the returns to scale of any given establishment. If the returns to scale at the level of the establishment are low, existing establishments will not hire many new workers in response to a change in the tax rate on capital.[6] In contrast, the size of the impact of a change in the capital tax rate on employment generated by newly entering establishments is determined by how many new establishments enter once the marginally profitable level of productivity changes in response to the change in the tax rate. If there are many possible establishments bunched just below the old level of $\mathcal{Z}$, then a change in the capital tax rate could induce many of these establishments to enter, creating a large impact on employment. On the other hand, if there are only a few establishments with productivity just below the old level of $\mathcal{Z}$, then a change in the capital tax rate will not induce a large inflow of new establishments. Because less productive establishments do not find it profitable to operate before the tax cut, the distribution of productivity of these establishments is not observable. Our estimates of the extensive margin of the change in capital tax on the creation of establishments shed light on this distribution.

[6]Basu and Fernald (1997) use industrial data and estimate θ of between 0.95 and 1. More recently, Ruzic and Ho (2021) estimate $\theta = 0.96$ using restricted Census microdata from the manufacturing sector. That said, returns to scale are likely to be much lower at the level of the establishment. For example, sales of an existing coffee shop are not likely to double and may not increase at all if the coffee shop doubles its square footage and employees.

3 Data and Methodology

3.1 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 eligible tracts as OZs. Eligible tracts were low-income tracts and some tracts contiguous with low-income tracts.[7] The governors of each state had to submit their nominations of designated tracts from among those eligible by March 21, 2018 deadline, unless they requested a 30-day extension. Most states completed designations in early 2018 and all states - 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), which requires that the tract meet at least one of the following criteria: (1) the tract has 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, or (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.

[7] 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).

At least 95% of tracts designated to receive favorable OZ tax treatment had to be a LIC as defined above. Additionally, the median income of any designated tract contiguous to an LIC must be less than 125% 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 estate in designated OZ tracts. Taxes on the realized capital gains from prior investments can be deferred for five (seven) years, at which point the taxable basis of the capital gains is reduced by 10% (15%) and the tax becomes payable. To be eligible for the tax benefits, investors must invest in a 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 in an OZ are 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. 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 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.

3.2 Methodology

Similar to the approach Chen et al. (2023) use to identify the effect of the OZ program on house prices, we use a 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 address the concern.

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 3.1 but were not chosen. We refer to the eligible-but-not-chosen tracts as “Other” tracts. 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 similar results to our benchmark specification.

All of our DiD analyses use the following regression specification

$$Y_{i,t} = \alpha_0 + \alpha_1 D_i + \alpha_2 Post_{2019} + \alpha_3 Post_{2021} + \alpha_4 D_i Post_{2019} + \alpha_5 D_i Post_{2021} + \mathbf{X_i}\alpha_X + \epsilon_{i,t} \quad (5)$$

where $Y_{i,t}$ is two-year growth in an economic variable of interest in the tract, $Post_{2019}$ is a dummy variable equal to 1 for the January 1, 2018 - December 31, 2019 period, 0 otherwise, $Post_{2021}$ is a dummy variable equal to 1 for the January 1, 2020 - December 31, 2021 period, D_i is a dummy variable that takes a value of 1 if the tract was Designated and 0 otherwise, and $\mathbf{X_i}$ is a vector of characteristics of the tract that do not vary over the observation periods. Our initial regressions compare employment and establishment growth from January 1, 2018 to December 31, 2019, $Y_{i,2019} = (E_{i,2019} - E_{i,2017})/E_{i,2017}$ and from January 1, 2020 to December 31, 2021, $Y_{i,2021} = (E_{i,2021} - E_{i,2019})/E_{i,2019}$, with the growth from January 1, 2016 to December 31, 2017, $Y_{i,2017} = (E_{i,2017} - E_{i,2015})/E_{i,2015}$, in all tracts eligible to receive preferential tax treatment from the OZ legislation.[8] Our post-legislation sample covers more than 3.5 years, from the last possible date for designation in June 2018 to the end of 2021.

[8]The list of all eligible tracts and those ultimately designated is available at https://edit.urban.org/sites/default/files/urbaninstitute_tractleveloz analysis_update1242018.xlsx.

Because our data is a census taken throughout the year, rather than at a particular point within the year, we work with two-year periods to avoid any contamination from within the year of 2018. We 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.

3.3 Data

Our main dataset is establishment-level employment data from Your-economy Time Series (YTS) and covers 2013-2021. The data is a census conducted throughout the calendar year rather than measured at a point in time. Infogroup provides the licensed database used to create the Your-economy Time Series (YTS).

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) 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/.

To generate our two variables of interest, employment and the number of establishments at the tract-level, we sum over establishments in the YTS data in each eligible tract. We then calculate two-year growth of each of these outcomes when estimating equation (5). For the regression covariates X_i, we use tract-level data from the 2013-2017 American Community Survey (ACS) 5-year estimates.[9] We include the share of the population that is white, the share with higher education, the share

[9]Source: https://www.census.gov/data/developers/data-sets/acs-5year/2017.html. Appendix Table A1 lists the full set of ACS control variables we include in our regressions; we use the same set of ACS control variables as Chen et al. (2023).

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 average daily commute time, and the share of the population that is employed, and the share of households receiving supplemental income.[10]

Our analysis sample consists of 26,032 Census tracts. Starting from 42,176 Census tracts that are eligible for OZ, we exclude 959 tracts in US territories, 10 tracts not present in the ACS, and 56 tracts without valid employment or establishment records in either 2015 or 2017. We further exclude 15,119 Census tracts with missing ACS covariates, 14,808 out of which are dropped because of missing median commuting time. In Appendix Table A2, we confirm that our main results remain the same when we use larger sample without controlling for the median commuting time. Our benchmark analysis does not restrict the sample to having an observation in each of the three two-year growth periods we use to estimate equation (5). However, we find very similar results, reported in Appendix Table A11, when we restrict the sample to be a balanced panel.

Table 1 summarizes outcome variables and covariates for Designated and Other tracts. Consistent with the presumption that state executives used the OZ program to benefit the maximum number of people, employment and the number of establishments are substantially higher in Designated tracts than in Other tracts. Other tracts had an average of 1,870 employees, while Designated tracts had an average of 3,040 employees. Designated tracts also have a higher poverty rate (24% vs. 17%), lower median household income, lower median earnings, less education, and a higher per-

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

Table 1: 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	
Panel A: YTS					
Employment	1870	3040	3429	4571	-20.01
Employment Growth (%)	4.31	1.63	30.38	19.01	5.86
Number of Establishments	183.01	263.16	241.42	302.96	-19.79
Growth in the Number of Establishments (%)	4.19	3.50	16.55	17.86	2.60
Number of Entered Establishments	44.79	57.69	67.06	76.94	-11.73
Number of Exited Establishments	39.74	53.67	58.74	72.85	-14.18
Percent of Entered Establishments (%)	27.76	25.46	20.47	20.42	7.03
Percent of Exited Establishments (%)	23.57	21.98	9.30	8.39	10.96
Panel B: ACS 5-year Estimates					
Population	4217.52	4101.16	1966.20	1968.75	3.71
Total Housing (thousands)	1.55	1.48	0.69	0.68	6.11
% Poverty	17.37	24.07	9.33	10.77	-43.71
% Employed	30.52	27.05	7.44	7.58	29.19
% White	68.40	58.19	27.18	29.41	23.18
% Higher ed	19.72	16.02	10.48	8.99	22.72
% Renters	42.21	54.49	21.95	22.29	-34.99
% Native-born with Health Insurance	89.58	88.21	5.71	6.13	14.86
% Supplemental Income	8.50	11.76	5.92	7.18	-33.13
Median Annual Earnings	28342	24678	7902	7530	29.33
Median Annual Household Income	46980	37303	15600	13345	39.91
Median Monthly Gross Rent	915	836	318	280	15.83
Avgerage Daily Commuting Time (min)	36.63	37.02	12.67	14.50	-1.88
Observation	21203	4829			

Note: Growth in employment and the number of establishments is measured over the two-year January 1, 2016-December 31, 2017 period.

centage 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.

4 Results

4.1 All eligible tracts

Table 2 presents DiD results for employment (panel A) and establishment growth (panel B). In columns (1) and (3), we include all observations in the sample.[11] In column (1) we include no controls, while column (3) includes employment growth from January 1, 2014 to December 31, 2015 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 $Post_{t=2019}$ is 0.025 in column (1) and 0.029 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 in its first two years by 2.1-2.0 percentage points, shown in columns (1) and (3). These estimates are statistically significant.

Our data contain 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 and establishment growth by about 2 percentage points in its first two years.

[11] We have 78,076 observations rather than $41,174 \times 3 = 123,522$ because of missing data for some tract-periods. We use observations for the same sample of 78,076 tract-periods for all specifications.

Table 2: Employment and Establishment Growth Regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	OLS	LAV	OLS	LAV	OLS	OLS	GLS	FE	FE	OLS
					Winsorized at			Winsorized at 1%		
					0.5%	1%	Weighted	Tract FE	CBSA FE	SEs Clustered
ACS Controls	No	No	Yes	Yes	Yes	Yes	Yes	No	Yes	Yes
No. of CBSAs									928	
Panel A: Employment Growth										
D_i	-0.027*	-0.015***	-0.024*	-0.011***	-0.017***	-0.017***	-0.013***		-0.016***	-0.017***
	(0.014)	(0.003)	(0.014)	(0.003)	(0.005)	(0.005)	(0.004)		(0.005)	(0.004)
$Post_{t=2019}$	0.001	-0.072***	-0.003	-0.073***	-0.040***	-0.048***	-0.078***	-0.048***	-0.049***	-0.048***
	(0.009)	(0.002)	(0.008)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.013)
$Post_{t=2021}$	0.049***	0.003*	0.049***	0.003*	0.026***	0.019***	0.075***	0.019***	0.018***	0.019***
	(0.009)	(0.002)	(0.008)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.005)
$D_iPost_{t=2019}$	0.025	0.021***	0.029	0.020***	0.039***	0.037***	0.018***	0.037***	0.037***	0.037***
	(0.020)	(0.004)	(0.019)	(0.004)	(0.007)	(0.006)	(0.005)	(0.007)	(0.006)	(0.008)
$D_iPost_{t=2021}$	0.037*	0.018***	0.037*	0.018***	0.035***	0.033***	0.026***	0.034***	0.033***	0.033***
	(0.020)	(0.004)	(0.019)	(0.004)	(0.007)	(0.006)	(0.005)	(0.007)	(0.006)	(0.005)
$Emp.Growth_{2013-2015}$			0.096***	-0.004	0.020***	0.012***	0.005*		0.009**	0.012*
			(0.013)	(0.003)	(0.005)	(0.004)	(0.003)		(0.004)	(0.007)
Observations	78,090	78,090	78,076	78,076	78,076	78,076	78,076	78,076	78,076	78,076
R^2	0.001		0.003		0.010	0.012	0.065	0.013	0.012	0.012
Panel B: Establishment Growth										
D_i	-0.007	-0.005**	-0.009	-0.006**	-0.010***	-0.009***	-0.013***		-0.009***	-0.009**
	(0.007)	(0.003)	(0.007)	(0.003)	(0.003)	(0.003)	(0.002)		(0.003)	(0.004)
$Post_{t=2019}$	-0.097***	-0.091***	-0.098***	-0.093***	-0.109***	-0.108***	-0.104***	-0.108***	-0.110***	-0.108***
	(0.004)	(0.002)	(0.004)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.016)
$Post_{t=2021}$	0.105***	0.094***	0.105***	0.092***	0.090***	0.086***	0.107***	0.086***	0.086***	0.086***
	(0.004)	(0.002)	(0.004)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.007)
$D_iPost_{t=2019}$	0.021**	0.020***	0.022**	0.019***	0.030***	0.030***	0.018***	0.030***	0.030***	0.030***
	(0.010)	(0.004)	(0.010)	(0.004)	(0.005)	(0.004)	(0.003)	(0.005)	(0.004)	(0.009)
$D_iPost_{t=2021}$	0.006	0.004	0.006	0.005	0.013***	0.013***	0.009***	0.013***	0.013***	0.013***
	(0.010)	(0.004)	(0.010)	(0.004)	(0.005)	(0.004)	(0.003)	(0.005)	(0.004)	(0.004)
$Emp.Growth_{2013-2015}$			0.100***	0.019***	0.025***	0.023***	0.005**		0.016***	0.023***
			(0.007)	(0.002)	(0.003)	(0.003)	(0.002)		(0.003)	(0.005)
Observations	78,090	78,090	78,076	78,076	78,076	78,076	78,076	78,076	78,076	78,076
R^2	0.032		0.038		0.134	0.149	0.224	0.178	0.149	0.149

Notes: 1) Columns (2) and (4) report results for quantile regression to the median or Least Absolute Value (LAV). 2) Weight for column (7) 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) $Post_{t=year}$ is a dummy variable equal to 1 if the observation is from the year, 0 otherwise, D_i is a dummy variable that takes a value of 1 if the tract was designated an OZ and 0 otherwise.

Columns (5) and (6) present the OLS results when we winsorize the dependent variable at the 0.5% and 1% levels and include all ACS controls. The results are broadly similar regardless of the level at which we winsorize. The estimates indicate that the program increased employment by approximately 3.8 percentage points and the number of establishments by approximately 3 percentage points in its first two years. For both dependent variables and for all three levels of winsorization, the coefficient on the interaction between D_i and $Post_{2019}$ is statistically significant at the 1% level. In the remainder of our analyses, we winsorize the dependent variable at the 1% level for OLS regressions.

In column (7), 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 3.7 percentage points in our benchmark specification (column (6)), suggesting that the program disproportionately affected less populous (urban) tracts. We show in the next section that the effect is significant only in urban tracts. Hence, the largest effects of the policy are in less populous urban tracts.

In column (8), we include tract fixed effects and find similar results to the specification without tract fixed effects in column (6). In column (9), we include core-based statistical area (CBSA) fixed effects, 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% in 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.

4.2 Metropolitan versus non-metropolitan areas

Columns (1) and (2) of Table 3 show our benchmark specifications for the sample of eligible tracts located in metropolitan areas. The estimated effects for the two-year period ending on December 31, 2019 on employment and establishment growth are 2.9 - 4.5 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 for the two-year period ending on December 31, 2019. This latter result is our only significant and economically meaningful negative finding of the OZ program on growth. However, the negative effect on establishment growth in rural areas is not a robust finding as we show in the Appendix Table A12.

We are mostly concerned about employment growth. Because the program did not affect employment growth in non-metropolitan tracts, we drop them in the analyses that follow unless otherwise noted. 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.

4.3 Robustness

In the appendix, we conduct a number of robustness exercises on our benchmark specifications. First, we exclude tracts that are not low-income communities. Second, we reduce our control group to eligible but not designated tracts that are contiguous to a designated tract. Third, we use a propensity-matching style of estimator, Double Robust DiD, where we match a designated tract to an eligible but not designated tract based on their pre-policy characteristics. We then change the dependent variable from two-year employment and establishment growth to one-year employment and establishment growth. We verify that governors' political affiliation or private information on tracts that will perform well does not change our results. We also perform a placebo test that shows no positive effects of the policy before the start of the policy. Finally, we show that our results are robust to prior gentrification trends or a tract being near a college.

4.4 Does Employment Increase for Tract *Residents*?

In an important, carefully executed paper, Freedman et al. (2023) show that there was no increase in the employment of residents in the designated Census tracts. At first glance, it may appear that our results are inconsistent with their findings. However, as Table 4 shows, the vast majority of Americans do not live in the same tract as they work in. Table 4 presents data from the LEHD Origin-Destination Employment Statistics (LODES) that can be used to identify where people that commute into the tract live. In every year between 2011 and 2019, only about 5% of people who commuted into the Census tract for work commuted from a residence within that tract. Given these findings, it is unlikely that any job created in a designated tract would be taken by a resident who lives in that tract.

Indeed, we can use the LODES data to assess whether, using the same methodology as Freedman et al. (2023), we see an increase in employment in designated tracts relative to Other tracts. We first reestimate equation (5) using all commuters in the

Table 3: Employment and Establishment Growth Within and Outside of Metro Areas

	(1)	(2)	(3)	(4)
	LAV	OLS	LAV	OLS
	Metropolitan Area		Non-Metropolitan Area	
ACS Controls	Yes	Yes	Yes	Yes
Panel A: Employment Growth				
D_i	-0.016***	-0.022***	0.005	0.005
	(0.003)	(0.005)	(0.006)	(0.011)
$Post_{t=2019}$	-0.090***	-0.076***	-0.015***	0.049***
	(0.002)	(0.003)	(0.004)	(0.006)
$Post_{t=2021}$	-0.003*	0.012***	0.028***	0.045***
	(0.002)	(0.003)	(0.004)	(0.007)
$D_iPost_{t=2019}$	0.029***	0.045***	-0.011	0.003
	(0.004)	(0.007)	(0.008)	(0.015)
$D_iPost_{t=2021}$	0.021***	0.035***	0.007	0.027*
	(0.004)	(0.007)	(0.009)	(0.016)
$Emp.Growth_{2013-2015}$	-0.003	0.007	-0.003	0.025***
	(0.003)	(0.005)	(0.005)	(0.009)
Observations	61,761	61,761	16,315	16,315
R^2		0.020		0.010
Panel B: Establishment Growth				
D_i	-0.012***	-0.016***	0.015***	0.017**
	(0.003)	(0.003)	(0.005)	(0.007)
$Post_{t=2019}$	-0.117***	-0.140***	-0.014***	0.007*
	(0.002)	(0.002)	(0.003)	(0.004)
$Post_{t=2021}$	0.084***	0.077***	0.119***	0.119***
	(0.002)	(0.002)	(0.003)	(0.004)
$D_iPost_{t=2019}$	0.032***	0.044***	-0.023***	-0.024***
	(0.004)	(0.005)	(0.007)	(0.009)
$D_iPost_{t=2021}$	0.008**	0.015***	-0.008	0.002
	(0.004)	(0.005)	(0.007)	(0.010)
$Emp.Growth_{2013-2015}$	0.019***	0.022***	0.017***	0.023***
	(0.003)	(0.003)	(0.004)	(0.005)
Observations	61,761	61,761	16,315	16,315
R^2		0.184		0.083

Notes: 1) Columns (1) and (3) report results for quantile regressions to the median or Least Absolute Value (LAV). 2) Dependent variable is winsorized at the 1% level in Columnns (2) and (4). 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) $Post_{t=year}$ is a dummy variable equal to 1 if the observation is from the year, 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 4: Number of Commuters into Census Tracts by Residency Status in LODES, Selected Years

Year	Non-Resident	Resident	Total Commuters	% Resident
2011	122,691,406	6,516,715	129,208,121	5.04%
2015	130,678,772	7,307,742	137,986,514	5.30%
2019	136,246,658	7,178,733	143,425,391	5.01%

Notes: 1) Data from LEHD Origin-Destination Employment Statistics. 2) Table entries show total commuters into a tract by whether they are residents or non-residents of the tracts they commute into for work.

tracts and only using data through 2019 since 2021 data is not yet available. Panel A of Table 5 contains the results. While the magnitude is smaller than when we use the YTS data, we continue to find that the program increased employment in the tracts, as measured by the number of commuters. We also estimate our DRDiD specification using all commuters in the tract (Panel B of Table 5), which yields positive, although statistically insignificant effects.

Table 6 estimates the same specifications but splits commuters into those who commute from outside the tract and those who commute from inside the tract. For the sample of commuters that live within the tracts, the coefficient is small and positive but far from statistically significant, consistent with the data illustrating that most people who work in a tract commute from outside that tract.

Table 5: Estimates Using All Commuters Working in Tracts as Reported in LODES

	(1) All	(2) Metro	(3) LIC	(4) LIC + Metro
Panel A: Differences-in-Differences				
D_i	-0.010**	-0.011**	-0.012***	-0.012***
	(0.004)	(0.004)	(0.004)	(0.005)
$Post_{t=2019}$	-0.015***	-0.019***	-0.017***	-0.020***
	(0.002)	(0.003)	(0.003)	(0.003)
$D_i Post_{t=2019}$	0.008	0.011*	0.010*	0.013**
	(0.005)	(0.006)	(0.006)	(0.006)
Observations	51,280	42,084	38,193	32,170
R^2	0.064	0.063	0.061	0.059
Panel B: DRDiD				
$\hat{\tau}_{2019}$	0.004	0.008	0.005	0.009
	(0.005)	(0.006)	(0.005)	(0.006)
Observations	50,596	41,604	37,724	31,832

Notes: 1) Data from LEHD Origin-Destination Employment Statistics (LODES). 2) Standard errors in parentheses. 3) ***, **, and * denote significance at the 1%, 5%, and 10% levels. 4) $Post_{t=year}$ is a dummy variable equal to 1 if the observation is from the year, 0 otherwise, D_i is a dummy variable that takes a value of 1 if the tract was designated an OZ and 0 otherwise. 5) We control for two-year employment growth rate in 2011-2013, 2013-2015, and 2015-2017 in Panel A and match Census tracts on the same variables in Panel B following the methodology of Freedman et al. (2023).

Table 6: Non-Resident and Resident Commuters in LODES

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Non-Residents				Residents			
	All	Metro	LIC	LIC + Metro	All	Metro	LIC	LIC + Metro
Panel A: Differences-in-Differences								
D_i	-0.010**	-0.012***	-0.012***	-0.013***	-0.009	-0.008	-0.016**	-0.014*
	(0.004)	(0.005)	(0.004)	(0.005)	(0.006)	(0.008)	(0.007)	(0.008)
$Post_{t=2019}$	-0.017***	-0.020***	-0.020***	-0.022***	-0.008**	-0.014***	-0.011**	-0.019***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.005)	(0.005)	(0.006)
$D_i Post_{t=2019}$	0.006	0.010	0.009	0.012*	0.001	0.002	0.003	0.005
	(0.006)	(0.006)	(0.006)	(0.007)	(0.009)	(0.011)	(0.010)	(0.012)
Observations	51,269	42,073	38,182	32,159	49,894	40,753	36,898	30,920
R^2	0.054	0.068	0.052	0.064	0.116	0.114	0.115	0.112
Panel B: DRDiD								
$\hat{\tau}_{2019}$	0.001	0.006	0.003	0.007	-0.002	0.004	-0.009	-0.002
	(0.005)	(0.006)	(0.006)	(0.006)	(0.009)	(0.010)	(0.009)	(0.011)
Observations	50,584	41,592	37,712	31,820	49,168	40,232	36,390	30,544

Notes: 1) Data from LEHD Origin-Destination Employment Statistics (LODES). 2) Standard errors in parentheses. 3) ***, **, and * denote significance at the 1%, 5%, and 10% levels. 4) $Post_{t=year}$ is a dummy variable equal to 1 if the observation is from the year, 0 otherwise, D_i is a dummy variable that takes a value of 1 if the tract was designated an OZ and 0 otherwise. 5) We control for two-year employment growth rate in 2011-2013, 2013-2015, and 2015-2017 in Panel A and match Census tracts on the same variables in Panel B following the methodology of Freedman et al. (2023).

As a final reconciliation of our results with those of Freedman et al. (2023), we reestimate the effect of OZs following their methodology. First, we calculate inverse probability weighted (IPW) estimates and multi-period doubly robust estimates (Callaway and Sant'Anna, 2021), where we calculate the propensity score using the employment growth rate in 2013, 2015, and 2017, as well as our baseline ACS controls. We also restrict our sample to Low-income Community eligible tracts and include tract fixed effects as in Freedman et al. (2023) and ensure that there is no positive pre-trend growth prior to the policy for matched tracts, see Figure 1. Table 7 presents these results which are similar to those in Table A4 which use the YTS data. As this is the specification that most closely matches that of Freedman et al. (2023), we conclude that the difference between their finding that the program had no effect on the employment of residents that live in the designated tracts and our finding a positive effect on total employment in the tract is most likely due to where the employees that work in the tract live.

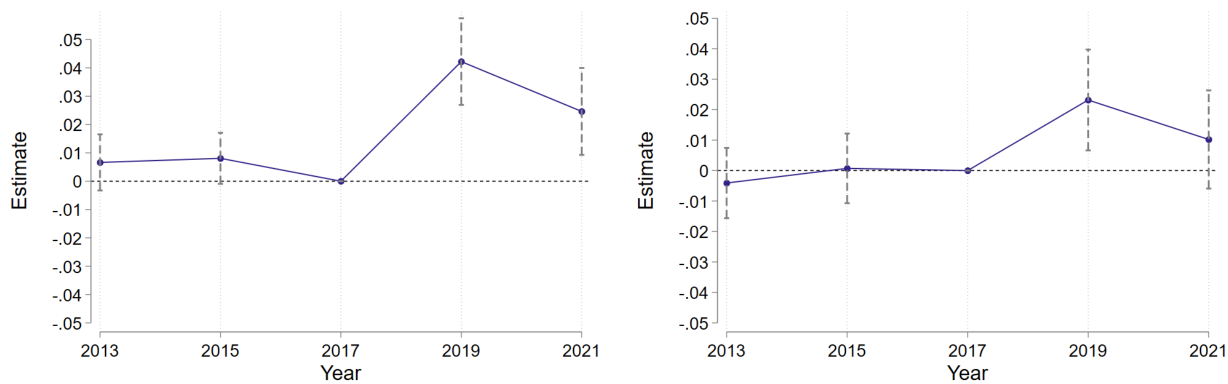
4.5 Heterogeneity

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.

4.5.1 New or old establishments?

The regression results reported in Table 3 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

Figure 1: Event Study Graphs



(a) ACS Controls

(b) Prior Growth Rates and ACS Controls

Notes: 1) Sample is LIC tracts in both metropolitan and non-metropolitan areas to match Freedman et al. (2023)'s sample. 2) Dependent variable is the two-year employment growth rate. 3) OZ-designated tracts are matched to eligible non-designated tracts based on propensity score. 4) Propensity scores are calculated using ACS controls in Figure 1a and two year employment growth rate in 2011-13, 2013-15, and 2015-17 and ACS controls in Figure 1b. 5) 95% confidence intervals are based on robust standard errors.

Table 7: Results Following the Methodology of Freedman et al. (2023)

	(1) IPW	(2) DRDiD	(3) IPW	(4) DRDiD
	All		Metropolitan Area	
Panel A: Employment Growth				
$\hat{\tau}_{2019}$	0.027***	0.025***	0.023**	0.022***
	(0.008)	(0.006)	(0.009)	(0.007)
$\hat{\tau}_{2021}$	0.013*	0.011**	0.012	0.011*
	(0.008)	(0.005)	(0.009)	(0.006)
Observations	58,108	58,103	47,380	46,939
Panel B: Establishment Growth				
$\hat{\tau}_{2019}$	0.021***	0.021***	0.024***	0.022***
	(0.005)	(0.005)	(0.006)	(0.005)
$\hat{\tau}_{2021}$	0.003	0.006	0.003	0.007*
	(0.005)	(0.004)	(0.005)	(0.005)
Observations	58,108	58,103	47,380	46,939

Notes: 1) Table includes only Low-income Community eligible tracts. Columns (3)-(4) further include only tracts in metropolitan area. 2) Dependent variable is two-year establishment growth rate winsorized at the 1% level. 3) Columns (1) and (3) report reports inverse probability weighted (IPW) estimates of the treatment effect on the treated. Columns (2) and (4) report multi-period doubly robust estimates (Callaway and Sant'Anna, 2021). Census tracts are matched based on the propensity score, constructed using ACS controls and two-year employment growth rate in 2011-2013, 2013-2015, and 2015-2017 following the methodology of Freedman et al. (2023). 4) Standard errors in parentheses. 5) ***, **, and * denote significance at the 1%, 5%, and 10% levels.

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.

4.5.2 Intensive or extensive margin?

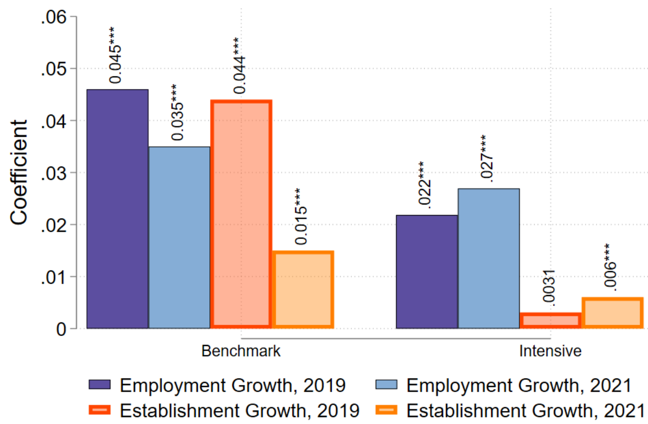
We now study whether the OZ policy induced employment growth by encouraging the growth of existing establishments (intensive margin) or new establishments (ex-

Table 8: Establishment Birth and Death Regressions

	(1)	(2)	(3)	(4)
	Percent of Entered Establishment		Percent of Exiting Establishment	
	LAV	OLS	LAV	OLS
ACS Controls	Yes	Yes	Yes	Yes
D_i	-0.023***	-0.031***	-0.014***	-0.012***
	(0.002)	(0.002)	(0.001)	(0.001)
$Post_{t=2019}$	-0.055***	-0.088***	-0.013***	-0.008***
	(0.001)	(0.001)	(0.001)	(0.001)
$Post_{t=2021}$	-0.062***	-0.095***	-0.226***	-0.244***
	(0.001)	(0.001)	(0.001)	(0.001)
$D_i Post_{t=2019}$	0.031***	0.040***	-0.004**	-0.009***
	(0.003)	(0.003)	(0.002)	(0.002)
$D_i Post_{t=2021}$	0.018***	0.026***	0.014***	0.015***
	(0.003)	(0.003)	(0.002)	(0.002)
$Emp.Growth_{2013-2015}$	0.054***	0.075***	-0.000	0.074***
	(0.002)	(0.002)	(0.001)	(0.001)
Observations	61,761	61,761	61,761	61,761
R^2		0.157		0.696

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) The dependent variable is winsorized at the 1% level in columns (2) and (4). 4) Standard errors in parentheses. ***, **, 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. 5) $Post_{t=year}$ is a dummy variable equal to 1 if the observation is from the year, 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 within Existing Establishments



Notes: 1) Sample of tracts in metropolitan areas. 2) ***, **, and * denote significance at the 1%, 5%, and 10% levels. 3) The benchmark results are from column (2) of Table 3, OLS Winsorized at 1%. 4) The intensive results are estimates from the sample of the existing establishments only, see text.

tensive margin). To address this question, we define an existing establishment as an establishment that existed in the data in both 2017 and 2019 and remained in the same tract in both years. Figure 2 presents the results. The darker blue bars show the effect of the OZ policy on employment growth in existing establishments is positive but smaller than our baseline estimates. Thus, the creation of new establishments is a driving force of the positive effect of the OZ program on employment growth.

4.5.3 Which industries are affected?

We use the 4-digit NAICS code classification of Mian and Sufi (2014) to study heterogeneity in the effects of OZs across 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 3a 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 in percentage terms 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 3a suggests the OZ program may have largely created only construction jobs. To investigate this possibility, we rerun our benchmark DiD specification excluding establishments in Construction industries. The estimates from this restricted sample decline to 4.4 (2019) and 3.1 (2021) percentage points for employment growth and 4.3 (2019) and 1.2 (2021) percentage points for establishment growth but remain statistically significant (not shown).

We also look at tract employment and establishment growth by 1-digit NAICS sectors. In Table A13 of the Appendix, 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. We then estimate the impact on employment and establishment growth for each 1-digit NAICS sector. Figure 3b shows OLS 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 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. The response of employment and establishment growth is insignificant for agriculture and manufacturing, NAICS sectors 1 and 3.[12]

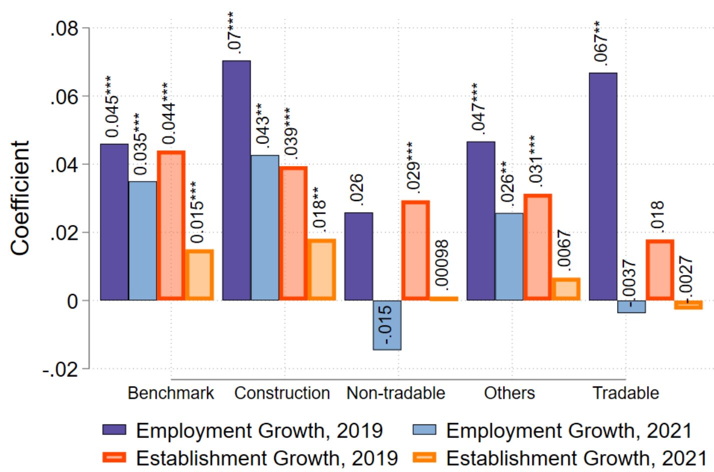
While the construction sector may have seen the largest percent increase in jobs, given its small overall share of employment, most of the jobs created were in fact in other sectors as Figure 4 illustrates. Figure 4 shows the overall share of jobs created using our estimates of the effect on job growth in the first four years of the program and the pre-OZ program number of jobs in each 1-digit NAICS sector. Only about 10% of jobs created were in the construction sector. The largest category of jobs created were in services. Our employment results are in line with the investment data reported by Coyne and Johnson (2023). Table 13 of Coyne and Johnson (2023) reports that the greatest investment was in Management of Companies followed by Finance and Insurance with Real Estate occupying third place.

Given our finding in Section 4.4 that most of the jobs created are unlikely to be taken by residents, it is possible that the jobs in the construction sector in the designated tracts provide the most lasting welfare benefit of the program. Because low-income households spend a very large share of their incomes on rent, any increase in housing supply that keeps rents down is likely to positively benefit residents.[13] Indeed, Chen et al. (2023) and Wheeler (2022) provide evidence that the program stimulated housing supply.

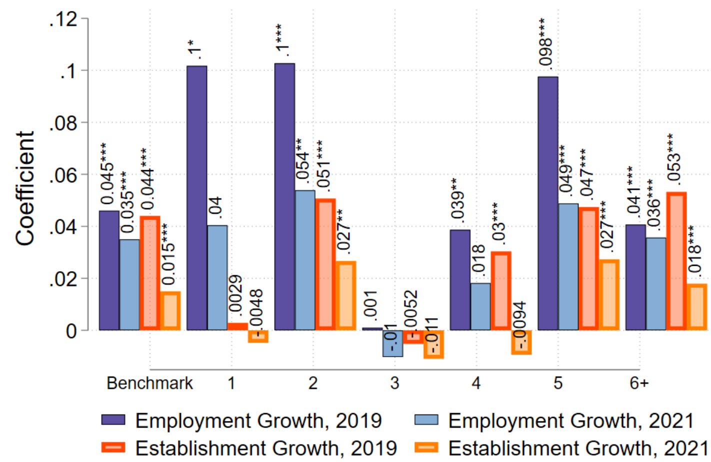
[12]While it may at first seem surprising that there is an effect on agriculture, given that we found the program only had an impact in metropolitan areas, many agricultural establishments are meatpacking facilities that are often located on the outskirts of metropolitan areas.

[13]For evidence that low-income households spend a larger fraction of income on housing than higher income households, see, for example, Rosen (1979), Green and Malpezzi (2003), Glaeser, Kahn, and Rappaport (2008), and Rosenthal (2014).

Figure 3: Estimates by Industry



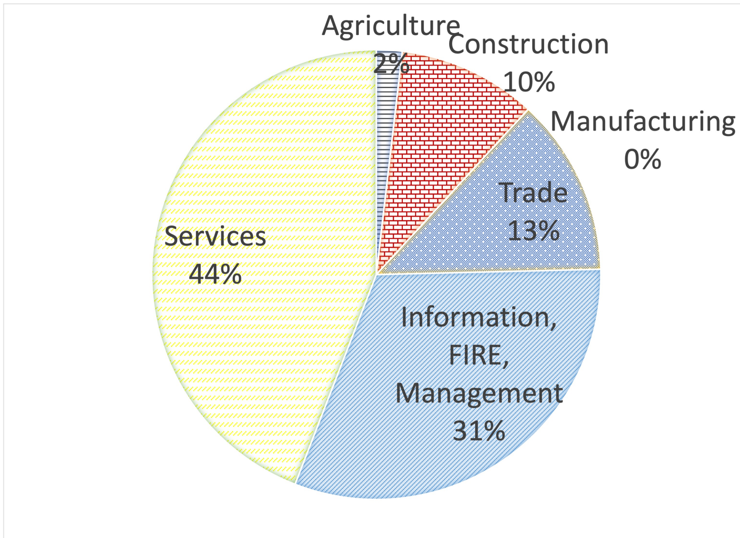
(a) Estimates by Industry Type



(b) Estimates by 1-digit NAICS industry

Notes: 1) Sample of tracts in metropolitan areas. 2) Benchmark estimate is from Table 3, 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 A13.

Figure 4: Number of Jobs Created by 1-digit NAICS industry

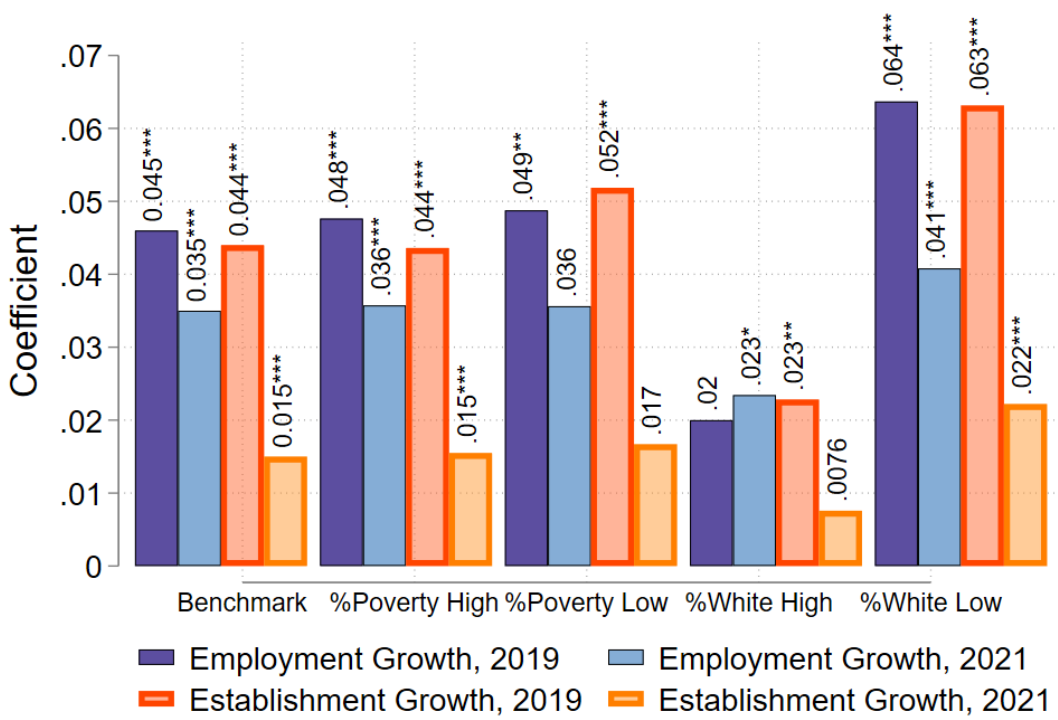


Notes: 1) Figure uses estimates from Figure 3b and baseline employment levels in each sector to construct overall job growth by sector. 2) Broad 1-digit NAICS sectors: (1) agriculture, (2) construction, (3) manufacturing, (4) trade, (5) information, FIRE (finance, insurance and real estate) and management, and (6) services, see Appendix Table A13.

4.5.4 Heterogeneity by tract characteristics

Figure 5 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 and establishment growth is roughly similar for the two groups of tracts. 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 shows that the program had much larger effects in tracts with a lower share of white households.

Figure 5: Estimates by Tract Characteristics



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

4.6 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 or related effects. Previous analyses of place-based policies have found that the direct effects of these policies are sometimes offset, at least in part, by reductions nearby.[14] 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 this 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 for $k = 1, 2, 3, 4$

$$Y_{i,t} = \alpha_0 + \alpha_{0,k}G_{i,k} + (\alpha_1 + \alpha_{1,k}G_{i,k})D_i + (\alpha_2 + \alpha_{2,k}G_{i,k})Post_{t=2019} + (\alpha_3 + \alpha_{3,k}G_{i,k})Post_{t=2021} +$$
$$+ (\alpha_4 + \alpha_{4,k}G_{i,k})D_iPost_{t=2019} + (\alpha_5 + \alpha_{5,k}G_{i,k})D_iPost_{t=2021} + \mathbf{X}_{\mathrm{i}}\alpha_X + \epsilon_{i,t}, \quad (6)$$

where $D_i = 1$ 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 $k = 1, 2, 3, 4$. The 0-

[14]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.

Table 9: Estimates of Spillover Effects on Neighboring Tracts

	(1)	(2) Net Effect
D_i	-0.020***	
	(0.003)	
$Post_{t=2019}$	-0.078***	
	(0.003)	
$Post_{t=2021}$	0.010***	
	(0.003)	
$D_i Post_{t=2019}$	0.045***	
	(0.007)	
$D_i Post_{t=2021}$	0.034***	
	(0.006)	
$G_{i,1} D_i Post_{t=2019}$	-0.025***	0.020***
	(0.009)	p=0.0007
$G_{i,1} D_i Post_{t=2021}$	-0.002	0.032***
	(0.008)	p=0.0000
$G_{i,2} D_i Post_{t=2019}$	-0.026**	0.019**
	(0.011)	p=0.0173
$G_{i,2} D_i Post_{t=2021}$	0.004	0.034***
	(0.009)	p = 0.000
$G_{i,3} D_i Post_{t=2019}$	-0.032**	0.013
	(0.015)	p=0.3171
$G_{i,3} D_i Post_{t=2021}$	-0.043***	-0.009
	(0.013)	p=0.4217
$G_{i,4} D_i Post_{t=2019}$	-0.042	0.003
	(0.027)	p=0.8996
$G_{i,4} D_i Post_{t=2021}$	-0.035	0.010
	(0.024)	p=0.9674
$Emp.Growth_{2013-2015}$	0.005	
	(0.003)	
Observations	191,593	
R^2	0.020	

Notes: 1) Results of estimating equation (6) 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) $Post_{t=year}$ is a dummy variable equal to 1 if the observation is from the year, 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.

step contiguous group ($G_{i,0} = 1$) is the baseline category, α_4 and α_5 represent the effect of being designated as an OZ, and $\alpha_{4,k}$ and $\alpha_{5,k}$ capture 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 on $Y_{i,t}$ from 2017 to 2019 is $\alpha_4 + \alpha_{4,1}$ and on $Y_{i,t}$ in from 2019 to 2021 is $\alpha_4 + \alpha_{4,1}$. Similarly, the estimated effects of designation on a tract 2-steps contiguous are $\alpha_4 + \alpha_{4,2}$ and $\alpha_5 + \alpha_{5,2}$.

Column (1) of Table 9 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. From the results of Table 9, we conclude that the OZ program created positive employment spillovers to neighboring tracts rather than poaching employment from these tracts.

While we are not able to identify specific agglomeration forces generating positive spillovers to 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 (2020) 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.

5 Conclusion

The OZ program created quasi-experimental variation in the capital gains tax rate across similar geographies. We exploit this variation to estimate the impact of capital gains taxes on employment. We find that the OZ program led to significantly higher employment and establishment growth in tracts receiving the beneficial tax treatment. However, we find that most of the jobs created by the program were taken by non-residents of the targeted tracts.

All qualified investment into the Opportunity Zones is in QOFs. QOF investment was $66 billion according to Coyne and Johnson (2023). If we apply 0.15 to the total investment to account for the exclusion of the deferred gain from income, the total cost of the program is $9.9 billion dollars. We can use these numbers to estimate a lower bound for the cost per job. Total employment in all designated tracts was 27,659,184 in 2017. We estimate that the program increased employment in designated tracts by approximately 0.018 and 0.026 percentage points from 2017 to 2019 and from 2019 to 2021, correspondingly. Thus, the program created 1,217,004 new jobs. Using the $9.9 billion estimates 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 $8,135. However, this cost per job does not account for the biggest tax advantage of the program which is eliminating capital gains taxation on new investments with holding periods over 10 years. The amount of this tax benefit is impossible to calculate without realization of those capital gains.

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 suggest that programs that subsidize capital rather than employment may be effective in creating employment. Given the findings of Neumark and Kolko (2010) that a wage subsidy to hire low-income workers was ineffective in California, place-based policies may need to incentivize hiring of workers of diverse skill levels to directly 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 create more jobs for low-skill workers.

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A Not for Publication Appendix

Table A1: 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). (3) The ACS controls are all variables with names starting with "acs".

Table A2: Robustness: Not Controlling for Median Commuting Time

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	OLS	LAV	OLS	LAV	OLS	OLS	GLS	FE	FE	OLS
					Winsorized at			Winsorized at 1%		
					0.5%	1%	Weighted	Tract FE	CBSA FE	SEs Clustered
ACS Controls	No	No	Yes	Yes	Yes	Yes	Yes	No	Yes	Yes
No. of CBSAs									928	
Panel A: Employment Growth										
D_i	-0.025	-0.016***	-0.032**	-0.011***	-0.020***	-0.019***	-0.019***		-0.019***	-0.019***
	(0.020)	(0.002)	(0.017)	(0.002)	(0.004)	(0.004)	(0.003)		(0.004)	(0.003)
$Post_{t=2019}$	0.022*	-0.074***	0.009	-0.075***	-0.042***	-0.050***	-0.074***	-0.050***	-0.051***	-0.050***
	(0.012)	(0.001)	(0.010)	(0.001)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.012)
$Post_{t=2021}$	0.059***	0.002	0.060***	0.002	0.026***	0.018***	0.072***	0.018***	0.018***	0.018***
	(0.012)	(0.001)	(0.010)	(0.001)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.005)
$D_i Post_{t=2019}$	0.006	0.023***	0.019	0.023***	0.040***	0.038***	0.019***	0.038***	0.038***	0.038***
	(0.028)	(0.003)	(0.023)	(0.003)	(0.006)	(0.005)	(0.004)	(0.005)	(0.005)	(0.007)
$D_i Post_{t=2021}$	0.029	0.018***	0.030	0.018***	0.032***	0.030***	0.031***	0.030***	0.030***	0.030***
	(0.028)	(0.003)	(0.023)	(0.003)	(0.006)	(0.005)	(0.004)	(0.005)	(0.005)	(0.004)
$Emp.Growth_{2013-2015}$			0.100***	0.000	0.021***	0.012***	-0.014***		0.011***	0.012***
			(0.011)	(0.001)	(0.003)	(0.002)	(0.002)		(0.002)	(0.004)
Observations	122,513	122,513	122,473	122,473	122,473	122,473	122,473	122,473	122,473	122,473
R^2	0.000		0.002		0.010	0.012	0.058	0.013	0.012	0.012
Panel B: Establishment Growth										
D_i	-0.008	-0.009***	-0.014	-0.007***	-0.011***	-0.011***	-0.014***		-0.010***	-0.011**
	(0.009)	(0.002)	(0.009)	(0.002)	(0.003)	(0.002)	(0.002)		(0.002)	(0.004)
$Post_{t=2019}$	-0.096***	-0.097***	-0.100***	-0.098***	-0.113***	-0.113***	-0.106***	-0.113***	-0.114***	-0.113***
	(0.006)	(0.001)	(0.005)	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)	(0.016)
$Post_{t=2021}$	0.105***	0.091***	0.105***	0.089***	0.088***	0.084***	0.105***	0.084***	0.084***	0.084***
	(0.006)	(0.001)	(0.005)	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)	(0.007)
$D_i Post_{t=2019}$	0.019	0.025***	0.022*	0.023***	0.032***	0.032***	0.020***	0.032***	0.032***	0.032***
	(0.013)	(0.003)	(0.012)	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)	(0.003)	(0.009)
$D_i Post_{t=2021}$	0.000	0.006**	0.000	0.005*	0.010***	0.010***	0.006**	0.011***	0.010***	0.010***
	(0.013)	(0.003)	(0.012)	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)
$Emp.Growth_{2013-2015}$			0.076***	0.019***	0.021***	0.017***	0.007***		0.014***	0.017***
			(0.006)	(0.001)	(0.002)	(0.002)	(0.001)		(0.002)	(0.003)
Observations	122,513	122,513	122,473	122,473	122,473	122,473	122,473	122,473	122,473	122,473
R^2	0.012		0.017		0.136	0.151	0.219	0.181	0.150	0.151

Notes: 1) Columns (2) and (4) report results for quantile regression to the median or Least Absolute Value (LAV). 2) Weight for column (7) 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) $P_{t=year}$ is a dummy variable equal to 1 if the observation is from the year, 0 otherwise, D_i is a dummy variable that takes a value of 1 if the tract was designated an OZ and 0 otherwise. 8) There are substantially more observations in Table A2 than in our main regressions because we control for commute time from the ACS in our main regression specifications which is missing for many tracts.

A.1 Robustness

In the exercises in this section, we include only tracts in metropolitan areas given that we find no employment effect for tracts in non-metropolitan areas.

A.1.1 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 (5) separately for the LIC and non-LIC tracts. Columns (1) and (2) of Table A3 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 3.2 - 5.0 percentage points in the first two years of the program. Columns (3) and (4) repeat this analysis for tracts eligible by the contiguity criteria (non-LIC). Our point estimates suggest these tracts experienced faster employment growth, 12.6 - 14.2 percentage points, and faster establishment growth, 7.4 - 8.8 percentage points. However, the standard errors on these estimates are also higher.[15] Using the OLS results, the effect of the OZ program on employment growth is not significantly different in LIC and non-LIC tracts (p-value = 0.123), but the effect on establishment growth is statistically higher in non-LIC tracts (p-value = 0.021).

[15]Recall that states could select no more than 5% of the Designated tracts using the contiguity criteria. This 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 A3, 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 A3: Robustness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	LIC		Non-LIC		Contiguous		LIC + Contiguous		Placebo	
	LAV	OLS	LAV	OLS	LAV	OLS	LAV	OLS	LAV	OLS
Panel A: Employment Growth										
D_i	-0.016***	-0.024***	-0.002	0.002	-0.011***	-0.014**	-0.007**	-0.010**	-0.008***	-0.012***
	(0.003)	(0.005)	(0.020)	(0.029)	(0.004)	(0.006)	(0.003)	(0.004)	(0.002)	(0.003)
$Post_{t=2019}$	-0.094***	-0.082***	-0.078***	-0.057***	-0.081***	-0.061***	-0.110***	-0.130***	0.007***	0.007***
	(0.002)	(0.004)	(0.004)	(0.006)	(0.003)	(0.005)	(0.003)	(0.003)	(0.001)	(0.002)
$Post_{t=2021}$	-0.004	0.012***	-0.003	0.011**	0.000	0.021***	0.085***	0.083***		
	(0.002)	(0.004)	(0.004)	(0.006)	(0.003)	(0.005)	(0.003)	(0.003)		
$D_iPost_{t=2019}$	0.032***	0.050***	0.142***	0.126***	0.019***	0.028***	0.021***	0.031***	-0.006**	-0.007
	(0.005)	(0.007)	(0.029)	(0.042)	(0.005)	(0.008)	(0.004)	(0.005)	(0.003)	(0.005)
$D_iPost_{t=2021}$	0.022***	0.036***	-0.010	-0.009	0.013***	0.023***	0.003	0.007		
	(0.005)	(0.007)	(0.028)	(0.041)	(0.005)	(0.008)	(0.004)	(0.005)		
$Emp.Growth_{2013-2015}$	-0.003	0.003	-0.001	0.019*	-0.004	0.006	0.023***	0.021***	-0.013***	-0.024***
	(0.004)	(0.006)	(0.007)	(0.010)	(0.004)	(0.007)	(0.004)	(0.005)	(0.002)	(0.003)
Observations	47,383	47,383	14,378	14,378	34,464	34,464	34,464	34,464	41,774	41,774
R^2		0.022		0.014		0.017		0.174		0.028
Panel B: Establishment Growth										
D_i	-0.012***	-0.017***	-0.009	-0.016	-0.012***	-0.015**	-0.008**	-0.011***	-0.011***	-0.016***
	(0.003)	(0.004)	(0.018)	(0.019)	(0.004)	(0.006)	(0.003)	(0.004)	(0.002)	(0.003)
$Post_{t=2019}$	-0.119***	-0.142***	-0.111***	-0.133***	-0.085***	-0.067***	-0.113***	-0.133***	0.003*	0.004**
	(0.002)	(0.002)	(0.003)	(0.004)	(0.003)	(0.005)	(0.003)	(0.003)	(0.001)	(0.002)
$Post_{t=2021}$	0.084***	0.076***	0.087***	0.082***	-0.000	0.021***	0.084***	0.081***		
	(0.002)	(0.002)	(0.003)	(0.004)	(0.003)	(0.005)	(0.003)	(0.003)		
$D_iPost_{t=2019}$	0.032***	0.045***	0.074***	0.088***	0.021***	0.031***	0.024***	0.033***	0.005	0.007*
	(0.004)	(0.005)	(0.025)	(0.028)	(0.005)	(0.008)	(0.005)	(0.006)	(0.003)	(0.004)
$D_iPost_{t=2021}$	0.009**	0.017***	0.016	0.009	0.015***	0.024***	0.004	0.009		
	(0.004)	(0.005)	(0.025)	(0.027)	(0.005)	(0.008)	(0.005)	(0.006)		
$Emp.Growth_{2013-2015}$	0.013***	0.020***	0.034***	0.029***	-0.003	0.009	0.017***	0.020***	0.005**	0.017***
	(0.003)	(0.004)	(0.006)	(0.007)	(0.005)	(0.008)	(0.004)	(0.005)	(0.002)	(0.002)
Observations	47,383	47,383	14,378	14,378	28,849	28,849	28,849	28,849	41,774	41,774
R^2		0.180		0.193		0.018		0.172		0.071

Notes: 1) Table includes only tracts in metropolitan area such that the total sample corresponds to that analyzed in columns (1) and (2) of Table 3. 2) Columns (1), (3), (5), (7), (9) report results for quantile regression to the median or Least Absolute Value (LAV). 3) The dependent variable is winsorized at the 1% level in Columns (2), (4), (6), (8), and (10). 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) D_i is a dummy variable that takes a value of 1 if the tract was designated an OZ and 0 otherwise. 8) In columns (1)-(8), $Post_{t=year}$ is a dummy variable equal to 1 if the observation is from the year, 0 otherwise. In columns (9) and (10), $Post_{t=year}$ is equal to 1 for the 2015-2017 period, 0 otherwise.

A.1.2 Nearby tracts

In this section we restrict the control group to non-selected eligible tracts bordering Designated OZ tracts. 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 A3 show estimates from this restricted sample. The point estimates are a bit lower than the results shown in Section 4.2 due to the positive spillover documented in Table 9, as they suggest employment and establishment growth increased by 1.9-2.8 and 2.1-3.1 percentage points, respectively. These estimates are robust to further restricting the sample to LIC tracts, as can be seen in columns (7) and (8).

A.1.3 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 A3 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 for establishment growth. 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.

A.1.4 Doubly Robust Difference-in-Difference estimator

We verify the robustness of our results by using an alternative estimator that matches on the propensity score, called Doubly Robust Difference-in-Difference or DRDiD (Sant'Anna and Zhao, 2020).[16] The advantage of the DRDiD estimator is that it is consistent even if either the propensity score function or the regression model for the outcome is not correctly specified (but not both). We use our ACS covariates to propensity score match following Chen et al. (2023). Table A4 shows the DRDiD estimates of the impact of the policy, 4.6 and 3.4 percentage points for employment and establishment growth, respectively. These estimates are on the higher end of our baseline specification and are statistically significant.[17]

[16]Since our analysis includes three years of data, we use multi-period (three or more) doubly robust DiD (Callaway and Sant'Anna, 2021). We use a Stata package *csdid*.

[17]We thank Jiafeng Chen, Edward Glaeser, David Wessel for sharing their code for Chen et al. (2023) to perform the DRDiD estimation.

Table A4: DRDiD Results

	(1) Raw	(2) Winsorized at 1%
Panel A: Employment Growth		
$\hat{\tau}_{2019}$	0.057***	0.046***
	(0.017)	(0.008)
$\hat{\tau}_{2021}$	0.030**	0.030***
	((0.014)	(0.007)
Observations	61,117	61,117
Panel B: Establishment Growth		
$\hat{\tau}_{2019}$	0.033***	0.034***
	(0.008)	(0.006)
$\hat{\tau}_{2021}$	0.003	0.016***
	(0.008)	(0.005)
Observations	61,117	61,117

Notes: 1) Includes only tracts in metropolitan areas. 2) Standard errors in parentheses. 3) ***, **, and * denote significance at the 1%, 5%, and 10% levels. 4) We construct the propensity score used to match observations using our ACS controls detailed in Section 3.3.

A.1.5 One-year employment growth

Our main analysis in Section 4.1 studies the effect of OZs on two-year growth in employment and establishment growth from 2017 to 2019 to avoid 2018 during which legislation was finalized. In this section, we check the robustness of our findings by using one-year growth in employment and establishment growth to include the growth from 2017 to 2018 and from 2018 to 2019. Table A5 summarizes our findings. The effect of the policy on employment growth is significant and positive both in 2018 and 2019 in most specifications that we found an effect when we grouped the two years of data together.

Table A5: One-year Employment and Establishment Growth Regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	LAV	Tract FE	LAV	OLS	LAV	OLS	IPW	DrDiD	IPW	DrDiD
		Winsorized at 1%	Metro Area		Non-Metro Area		All		Metro Area	
ACS Controls	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel A: Employment Growth										
$D_i Post_{t=2018}$	0.007***	0.015***	0.008***	0.016***	-0.005	0.016	0.026***	0.025***	0.022***	0.021***
	(0.002)	(0.005)	(0.003)	(0.005)	(0.005)	(0.012)	(0.006)	(0.006)	(0.007)	(0.007)
$D_i Post_{t=2019}$	0.004	0.010**	0.005**	0.011**	-0.002	0.005	0.010***	0.011***	0.009***	0.013***
	(0.002)	(0.005)	(0.003)	(0.005)	(0.005)	(0.012)	(0.003)	(0.003)	(0.003)	(0.003)
Observations	78,077	78,077	63,874	63,874	14,203	14,203	58109	58104	48798	48626
R^2		0.020		0.027		0.012	0.022		0.036	
Panel B: Establishment Growth										
$D_i Post_{t=2018}$	0.016***	0.024***	0.008***	0.016***	-0.005	0.016	0.023***	0.022***	0.022***	0.022***
	(0.002)	(0.003)	(0.003)	(0.005)	(0.005)	(0.012)	(0.004)	(0.004)	(0.004)	(0.004)
$D_i Post_{t=2019}$	0.000	0.001	0.005**	0.011**	-0.002	0.005	0.000	0.002	0.001	0.003*
	(0.002)	(0.003)	(0.003)	(0.005)	(0.005)	(0.012)	(0.002)	(0.002)	(0.002)	(0.002)
Observations	78,077	78,077	63,874	63,874	14,203	14,203	58109	58104	48798	48626
R^2		0.114		0.027		0.012				

Notes: 1) The dependent variable is a one-year growth rate. It is winsorized at the 1% level in Columns (2), (4), and (6)-(10). 2) $Post_{t=year}$ is a dummy variable equal to 1 if the observation is from year t, 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) Columns (1), (3), and (5) report results for quantile regression to the median or Least Absolute Value (LAV). 4) Columns (7) and (9) report inverse probability weighted (IPW) estimates of the treatment effect on the treated. Columns (8) and (10) report multi-period doubly robust estimates (Callaway and Sant'Anna, 2021). Census tracts are matched based on the propensity score, constructed using ACS controls and two-year employment growth rate in 2011-2013, 2013-2015, and 2015-2017 following Freedman et al. (2023)'s methodology. 5) Columns (3)-(4) include only tracts in metropolitan areas, and columns (5)-(6) only tracts in non-metropolitan areas. 6) Standard errors in parentheses. 7) ***, **, and * denote significance at the 1%, 5%, and 10% levels.

A.1.6 Political tract selection

Perhaps not surprisingly, Frank, Hoopes, and Lester (2022) 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 (2022), 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, one lower house representative represents a tract and we set the variable defining whether the political affiliation of the tract is the same as the governor, $\%sameparty$, equal to 1 if the lower house representative and the governor are in the same party, 0 otherwise. However, some tracts belong to several electoral districts. Ten U.S. states contain districts sending two or more representatives to the lower house. To capture these cases, we set $\%sameparty$ equal to the share of the tract's lower house representatives that belong to the same party as the governor to measure political affiliation of the tract.[18] As an alternative specification, we construct the variable $Nsameparty$, which counts the number of legislators representing that tract of the same party as the governor.

[18] Out of the 41,055 tracts we include in the analysis, 12,094 (29%) are matched with more than two legislators.

Table A6: 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

Notes: 1) The outcome variable is an indicator if the tract is selected as OZ. 2) $Nsameparty$ ($\%sameparty$) is the number (share) of legislators representing that tract of the same party as the governor. 3) Standard errors in parentheses. 4) ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Table A7: Employment and Establishment Growth with Political Consideration

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	LAV	OLS	LAV	OLS	LAV	OLS	LAV	OLS
ACS Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
	Employment Growth				Establishment Growth			
D_i	-0.016***	-0.022***	-0.015***	-0.024***	-0.010***	-0.016***	-0.008*	-0.013**
	(0.003)	(0.005)	(0.005)	(0.007)	(0.003)	(0.003)	(0.004)	(0.005)
$Post_{t=2019}$	-0.091***	-0.076***	-0.089***	-0.075***	-0.118***	-0.141***	-0.112***	-0.133***
	(0.002)	(0.003)	(0.003)	(0.005)	(0.002)	(0.002)	(0.003)	(0.003)
$Post_{t=2021}$	-0.004**	0.012***	-0.004	0.012***	0.083***	0.078***	0.087***	0.082***
	(0.002)	(0.003)	(0.003)	(0.005)	(0.002)	(0.002)	(0.003)	(0.003)
$D_iPost_{t=2019}$	0.030***	0.045***	0.033***	0.061***	0.032***	0.043***	0.026***	0.034***
	(0.004)	(0.007)	(0.007)	(0.011)	(0.004)	(0.005)	(0.006)	(0.007)
$D_iPost_{t=2021}$	0.021***	0.034***	0.021***	0.031***	0.010**	0.015***	0.004	0.007
	(0.004)	(0.007)	(0.007)	(0.010)	(0.004)	(0.005)	(0.006)	(0.007)
$\%sameparty$	0.004**	0.005*	0.004	0.006	0.001	0.002	0.005*	0.008**
	(0.002)	(0.003)	(0.003)	(0.005)	(0.002)	(0.002)	(0.003)	(0.003)
$D_i\%sameparty$			-0.001	0.004			-0.002	-0.006
			(0.006)	(0.010)			(0.006)	(0.007)
$Post_{t=2019}\%sameparty$			-0.003	-0.000			-0.010***	-0.013***
			(0.004)	(0.006)			(0.004)	(0.004)
$Post_{t=2021}\%sameparty$			-0.000	-0.001			-0.007**	-0.008*
			(0.004)	(0.006)			(0.004)	(0.004)
$D_iPost_{t=2019}\%sameparty$			-0.006	-0.029**			0.009	0.017*
			(0.009)	(0.015)			(0.009)	(0.010)
$D_iPost_{t=2021}\%sameparty$			0.001	0.006			0.011	0.013
			(0.009)	(0.014)			(0.009)	(0.010)
$Emp.Growth_{2013-2015}$	-0.008**	0.005	-0.008**	0.005	0.008**	0.015***	0.008**	0.015***
	(0.003)	(0.005)	(0.003)	(0.005)	(0.003)	(0.003)	(0.003)	(0.003)
Observations	61,418	61,418	61,418	61,418	61,418	61,418	61,418	61,418
R^2		0.023		0.023		0.191		0.191

Notes: 1) Table includes only tracts in metropolitan areas such that the total sample corresponds to that analyzed in columns (1) and (2) of Table 3. 2) Columns (1), (3), (5), and (7) report results for quantile regression to the median or Least Absolute Value (LAV). 3) The dependent variable is winsorized at the 1% level in columns (2), (4),(6),and (8). 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) $Post_{t=year}$ is a dummy variable equal to 1 if the observation is from the year, 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 A6 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 (2022), 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.

Columns (1) and (2) of Table A7 show that the point estimates of the impact of OZ designation on employment and establishment growth in Section 4.2 are robust to controlling for the political affiliation of the tract, the $sameparty$ variable. In columns (3) and (4), we include interactions of the $sameparty$ variable with the $Post_{t=2019}$ and $Post_{t=2021}$ and D_i to see if the measured effect of the OZ program depends on the political affiliation of the tract. The estimate on the triple interaction term is negative and significant for employment growth. The estimate on the triple interaction term is small and insignificant for establishment growth.

A.1.7 Excluding top employment growth tracts

Apart from political affiliation, the governors' choices of tracts could have been driven by information on which tracts would have significant employment growth. To confound our estimates, these tracts would have to not be predictable based on past employment growth or tract characteristics from the ACS since we control for these observable variables.

To further reduce the chance our results are driven by such selection bias, we drop tracts with the highest 5% or 10% employment growth within each state (which amounts to 10 or 20 metro tracts per state), and reestimate our main specification. Table A8 shows the results together with the benchmark results from Table 3, column (1). In columns (2) and (3) of Table A8, we exclude tracts based on the employment growth measured over the two-year January 1, 2018 - December 31, 2019 period. In columns (4) and (5), we exclude tracts based on the employment growth measured over the four-year January 1, 2018 - December 31, 2021 period. The estimates of the effect of the program decrease in magnitude by design, but are still similar to our benchmark estimates.

A.1.8 Trends in demographic composition

Table A8: Excluding Top Employment Growth Tracts

% of Excluded Tracts Based on	(1) Benchmark	(2) 5% 2017-2019	(3) 10% Emp. Growth	(4) 5% 2017-2021	(5) 10 % Emp. Growth
D_i	-0.016***	-0.018***	-0.020***	-0.016***	-0.016***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
$Post_{t=2019}$	-0.090***	-0.090***	-0.090***	-0.090***	-0.090***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
$Post_{t=2021}$	-0.003*	-0.003*	-0.003*	-0.003*	-0.003*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
$D_iPost_{t=2019}$	0.029***	0.023***	0.018***	0.018***	0.006
	(0.004)	(0.005)	(0.005)	(0.004)	(0.005)
$D_iPost_{t=2021}$	0.021***	0.018***	0.017***	0.017***	0.013***
	(0.004)	(0.005)	(0.005)	(0.004)	(0.005)
$Emp.Growth_{2013-2015}$	-0.003	-0.002	-0.001	-0.003	-0.003
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Observations	61,761	60,469	59,290	61,112	60,556

Note: 1) All columns report results for quantile regression to the median or Least Absolute Value (LAV). 2) We exclude Census tract whose employment growth is the top 5% and 10% within each state. 3) In columns (2) and (3), we exclude tracts based on the growth in employment measured over the two-year January 1, 2018 - December 31, 2019 period. In columns (4) and (5), growth is measured over the four-year January 1, 2018-December 31, 2021 period. 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) $Post_{t=year}$ is a dummy variable equal to 1 if the observation is from the year, 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 A9: Controlling for Changes in Resident Characteristics and Rent

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	OLS	LAV	OLS	LAV	OLS	OLS	GLS	FE	FE	OLS
					Winsorized at			Winsorized at 1%		
					0.5%	1%	Weighted	Tract FE	CBSA FE	SEs Clustered
ACS Controls	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of CBSAs									928	
Panel A: Employment Growth										
$D_i Post_{t=2019}$	0.055***	0.029***	0.055***	0.029***	0.048***	0.045***	0.021***	0.046***	0.045***	0.045***
	(0.018)	(0.004)	(0.018)	(0.004)	(0.008)	(0.007)	(0.006)	(0.008)	(0.007)	(0.009)
$D_i Post_{t=2021}$	0.039**	0.021***	0.039**	0.021***	0.037***	0.035***	0.032***	0.035***	0.035***	0.035***
	(0.018)	(0.004)	(0.018)	(0.004)	(0.008)	(0.007)	(0.006)	(0.007)	(0.007)	(0.006)
Δlog(median income)			-0.042*	-0.004	-0.018	-0.012	0.000		-0.015	-0.012
			(0.024)	(0.006)	(0.011)	(0.009)	(0.008)		(0.010)	(0.009)
Δ% poverty			0.032	0.007	0.006	0.004	0.014		0.011	0.004
			(0.065)	(0.015)	(0.028)	(0.025)	(0.023)		(0.025)	(0.019)
Δ% higher-ed			0.118	0.048**	0.032	0.029	-0.044		0.031	0.029
			(0.091)	(0.022)	(0.040)	(0.035)	(0.029)		(0.035)	(0.031)
Δ log(median gross rent)			0.007	0.003	0.017	0.014	0.028***		0.011	0.014
			(0.026)	(0.006)	(0.011)	(0.010)	(0.010)		(0.010)	(0.009)
Observations	61,761	61,761	61,713	61,713	61,713	61,713	61,713	61,713	61,713	61,713
R^2	0.003		0.005		0.016	0.020	0.072	0.024	0.020	0.020
Panel B: Establishment Growth										
$D_i Post_{t=2019}$	0.044***	0.032***	0.043***	0.032***	0.044***	0.043***	0.022***	0.046***	0.044***	0.043***
	(0.009)	(0.004)	(0.009)	(0.004)	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)	(0.007)
$D_i Post_{t=2021}$	0.008	0.007	0.008	0.008**	0.016***	0.016***	0.010***	0.016***	0.016***	0.016***
	(0.009)	(0.004)	(0.009)	(0.004)	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)	(0.005)
Δ log(median income)			-0.015	0.012**	0.009	0.010	0.018***		0.006	0.010*
			(0.011)	(0.006)	(0.007)	(0.006)	(0.005)		(0.006)	(0.006)
Δ% poverty			0.010	0.020	0.017	0.017	0.005		0.029*	0.017
			(0.031)	(0.015)	(0.018)	(0.017)	(0.015)		(0.017)	(0.016)
Δ% higher-ed			0.056	0.047**	0.059**	0.055**	0.076***		0.040*	0.055***
			(0.043)	(0.021)	(0.026)	(0.024)	(0.019)		(0.024)	(0.021)
Δ log(median gross rent)			0.002	-0.004	-0.000	-0.001	0.003		0.002	-0.001
			(0.012)	(0.006)	(0.007)	(0.007)	(0.006)		(0.007)	(0.009)
Observations	61,761	61,761	61,713	61,713	61,713	61,713	61,713	61,713	61,713	61,713
R^2	0.070		0.073		0.166	0.182	0.232	0.220	0.183	0.182

Notes: 1) Table includes only tracts in metropolitan area such that the total sample corresponds to that analyzed in columns (1) and (2) of Table 3. 2) Columns (2) and (4) report results for quantile regression to the median or Least Absolute Value (LAV). 3) Weight for column (7) is 2015 Census tract employment. 4) In column (10), standard errors are clustered by CBSA. 5) Standard errors in parentheses. 6) ***, **, and * denote significance at the 1%, 5%, and 10% levels. 7) $Emp.Growth_{2013-2015}$ is the growth in tract employment from 2013 to 2015. 8) $Post_{t=year}$ is a dummy variable equal to 1 if the observation is from the year, 0 otherwise, D_i is a dummy variable that takes a value of 1 if the tract was designated an OZ and 0 otherwise.

In our benchmark specification, we control for the trend in employment growth in the tract. However, it remains possible that a different kind of trend influenced selection of the tracts. Specifically, in Table A9 we control for changes in the demographic composition of the neighborhood or what some commentators term gentrification. We focus on changes in residents' characteristics and housing cost to measure gentrification following Guerrieri, Hartley, and Hurst (2013). The results are similar to those we estimate using our benchmark specification.

A.1.9 Excluding college towns

Critics of the OZ selection process have suggested that some tracts that are not truly poor but rather just eligible by proximity to a college were selected. To understand whether the employment growth we find from the program is due solely to such tracts, we repeat our analysis excluding tracts likely to be college tracts.

We define a Census tract as 'eligible-by-being-near-a-college' if it satisfies both the following criteria: (1) the median ages of male and female residents of the tract are below 30, and (2) it is located within a county with a 4-year college and the proportion of college members to the population of the county is greater than 10%. College locations and sizes are from the National Center for Education Statistics Integrated Postsecondary Education Data System (IPEDS). These criteria detected 887 concerning Census tracts according to our criteria with 189 unique colleges such as University of Nebraska-Lincoln, SUNY Oneonta, and Cornell University. Our threshold captures many tracts that are not likely eligible solely because of adjacency to a college in the interest of being conservative in our methodology.

College tracts were more likely to be selected as OZs. 28% of these tracts were designated as OZ while 18% of other tracts were designated. As Table A10 shows, our main results are the same when we exclude College tracts, however. Taken together, this implies that some tracts were selected because they were near a college, but our results are not driven by such tracts.

Table A10: Excluding College Tracts

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	OLS	LAV	OLS	LAV	OLS	OLS	GLS	FE	FE	OLS
					Winsorized at			Winsorized at 1%		
					0.5%	1%	Weighted	Tract FE	CBSA FE	SEs Clustered
ACS Controls	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of CBSAs									928	
Panel A: Employment Growth										
D_i	-0.032**	-0.019***	-0.029**	-0.016***	-0.023***	-0.022***	-0.019***		-0.022***	-0.022***
	(0.013)	(0.003)	(0.013)	(0.003)	(0.006)	(0.005)	(0.004)		(0.005)	(0.004)
$Post_{t=2019}$	-0.053***	-0.088***	-0.053***	-0.090***	-0.071***	-0.075***	-0.084***	-0.077***	-0.075***	-0.075***
	(0.008)	(0.002)	(0.008)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.010)
$Post_{t=2021}$	0.042***	-0.003*	0.042***	-0.004*	0.018***	0.011***	0.068***	0.011***	0.011***	0.011*
	(0.008)	(0.002)	(0.008)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.006)
$D_iPost_{t=2019}$	0.056***	0.029***	0.057***	0.028***	0.048***	0.045***	0.022***	0.046***	0.045***	0.045***
	(0.019)	(0.004)	(0.019)	(0.004)	(0.008)	(0.007)	(0.006)	(0.008)	(0.007)	(0.009)
$D_iPost_{t=2021}$	0.037**	0.020***	0.037**	0.021***	0.036***	0.034***	0.032***	0.035***	0.034***	0.034***
	(0.018)	(0.004)	(0.018)	(0.004)	(0.008)	(0.007)	(0.006)	(0.008)	(0.007)	(0.006)
$Emp.Growth_{2013-2015}$			0.052***	-0.002	0.014**	0.009*	0.009***		0.005	0.009
			(0.013)	(0.003)	(0.006)	(0.005)	(0.003)		(0.005)	(0.007)
Observations	60,503	60,503	60,494	60,494	60,494	60,494	60,494	60,494	60,494	60,494
R^2	0.003		0.004		0.016	0.020	0.066	0.024	0.020	0.020
Panel B: Establishment Growth										
D_i	-0.013**	-0.011***	-0.014**	-0.011***	-0.015***	-0.015***	-0.018***		-0.016***	-0.015***
	(0.006)	(0.003)	(0.006)	(0.003)	(0.004)	(0.003)	(0.003)		(0.003)	(0.004)
$Post_{t=2019}$	-0.139***	-0.115***	-0.139***	-0.117***	-0.142***	-0.140***	-0.114***	-0.143***	-0.140***	-0.140***
	(0.004)	(0.002)	(0.004)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.015)
$Post_{t=2021}$	0.097***	0.086***	0.097***	0.085***	0.082***	0.078***	0.100***	0.077***	0.078***	0.078***
	(0.004)	(0.002)	(0.004)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.008)
$D_iPost_{t=2019}$	0.044***	0.032***	0.044***	0.032***	0.044***	0.043***	0.025***	0.045***	0.044***	0.043***
	(0.009)	(0.004)	(0.009)	(0.004)	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)	(0.007)
$D_iPost_{t=2021}$	0.007	0.006	0.007	0.007*	0.015***	0.015***	0.010***	0.015***	0.015***	0.015***
	(0.009)	(0.004)	(0.009)	(0.004)	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)	(0.005)
$Emp.Growth_{2013-2015}$			0.041***	0.020***	0.024***	0.024***	0.005***		0.015***	0.024***
			(0.006)	(0.003)	(0.004)	(0.003)	(0.002)		(0.003)	(0.005)
Observations	60,503	60,503	60,494	60,494	60,494	60,494	60,494	60,494	60,494	60,494
R^2	0.071		0.074		0.167	0.183	0.231	0.221	0.183	0.183

Notes: 1) Table includes only tracts in metropolitan area such that the total sample corresponds to that analyzed in columns (1) and (2) of Table 3. 2) Columns (2) and (4) report results for quantile regression to the median or Least Absolute Value (LAV). 3) Weight for column (7) is 2015 Census tract employment. 4) In column (10), standard errors are clustered by CBSA. 5) Standard errors in parentheses. 6) ***, **, and * denote significance at the 1%, 5%, and 10% levels. 7) $Emp.Growth_{2013-2015}$ is the growth in tract employment from 2013 to 2015. 8) $Post_{t=year}$ is a dummy variable equal to 1 if the observation is from the year, 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 A11: Robustness: Balanced Panel

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	OLS	LAV	OLS	LAV	OLS	OLS	GLS	FE	FE	OLS
					Winsorized at			Winsorized at 1%		
					0.5%	1%	Weighted	Tract FE	CBSA FE	SEs Clustered
ACS Controls	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of CBSAs									928	
Panel A: Employment Growth										
D_i	-0.027**	-0.015***	-0.024*	-0.011***	-0.018***	-0.017***	-0.013***		-0.017***	-0.017***
	(0.014)	(0.003)	(0.014)	(0.003)	(0.005)	(0.005)	(0.004)		(0.005)	(0.004)
$Post_{t=2019}$	-0.003	-0.072***	-0.003	-0.073***	-0.040***	-0.048***	-0.078***	-0.048***	-0.049***	-0.048***
	(0.008)	(0.002)	(0.008)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.013)
$Post_{t=2021}$	0.049***	0.003*	0.049***	0.003*	0.026***	0.019***	0.075***	0.019***	0.018***	0.019***
	(0.008)	(0.002)	(0.008)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.005)
$D_iPost_{t=2019}$	0.029	0.021***	0.029	0.021***	0.039***	0.037***	0.018***	0.037***	0.037***	0.037***
	(0.019)	(0.004)	(0.019)	(0.004)	(0.007)	(0.006)	(0.005)	(0.007)	(0.006)	(0.008)
$D_iPost_{t=2021}$	0.037*	0.018***	0.037*	0.018***	0.035***	0.034***	0.026***	0.034***	0.034***	0.034***
	(0.019)	(0.004)	(0.019)	(0.004)	(0.007)	(0.006)	(0.005)	(0.007)	(0.006)	(0.005)
$Emp.Growth_{2013-2015}$			0.096***	-0.004	0.020***	0.012***	0.005*		0.009**	0.012*
			(0.013)	(0.003)	(0.005)	(0.004)	(0.003)		(0.004)	(0.007)
Observations	78,072	78,072	78,069	78,069	78,069	78,069	78,069	78,069	78,069	78,069
R^2	0.001		0.003		0.010	0.012	0.065	0.013	0.012	0.012
Panel B: Establishment Growth										
D_i	-0.007	-0.005**	-0.010	-0.006**	-0.010***	-0.010***	-0.013***		-0.010***	-0.010**
	(0.007)	(0.003)	(0.007)	(0.003)	(0.003)	(0.003)	(0.002)		(0.003)	(0.004)
$Post_{t=2019}$	-0.098***	-0.091***	-0.098***	-0.093***	-0.109***	-0.108***	-0.104***	-0.108***	-0.110***	-0.108***
	(0.004)	(0.002)	(0.004)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.016)
$Post_{t=2021}$	0.105***	0.094***	0.105***	0.092***	0.090***	0.086***	0.107***	0.086***	0.086***	0.086***
	(0.004)	(0.002)	(0.004)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.007)
$D_iPost_{t=2019}$	0.022**	0.020***	0.022**	0.019***	0.030***	0.030***	0.019***	0.030***	0.030***	0.030***
	(0.010)	(0.004)	(0.010)	(0.004)	(0.005)	(0.004)	(0.003)	(0.005)	(0.004)	(0.009)
$D_iPost_{t=2021}$	0.006	0.004	0.006	0.005	0.013***	0.013***	0.009***	0.013***	0.013***	0.013***
	(0.010)	(0.004)	(0.010)	(0.004)	(0.005)	(0.004)	(0.003)	(0.005)	(0.004)	(0.004)
$Emp.Growth_{2013-2015}$			0.100***	0.019***	0.025***	0.023***	0.005**		0.016***	0.023***
			(0.007)	(0.002)	(0.003)	(0.003)	(0.002)		(0.003)	(0.005)
Observations	78,072	78,072	78,069	78,069	78,069	78,069	78,069	78,069	78,069	78,069
R^2	0.034		0.038		0.134	0.149	0.224	0.178	0.149	0.149

Notes: 1) Columns (2) and (4) report results for quantile regression to the median or Least Absolute Value (LAV). 2) Weight for column (7) 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) $P_{t=year}$ is a dummy variable equal to 1 if the observation is from the year, 0 otherwise, D_i is a dummy variable that takes a value of 1 if the tract was designated an OZ and 0 otherwise. 7) To be included in the regression shown in Table A11, the tract had to have an observation for all three periods in our sample.

Table A12: (Non)Robustness of OZ Effect on Establishment Growth in Non-Metropolitan Area

Matching on	(1) ACS Control	(2) ACS Control + Prior Year's Emp. Growth
	Panel A: IPW	
$\hat{\tau}_{2019}$	-0.004	-0.005
	(0.010)	(0.010)
$\hat{\tau}_{2021}$	-0.001	-0.001
	(0.010)	(0.010)
Observations	16,319	16,315
R-squared	0.075	0.075
	Panel B: DrDiD	
$\hat{\tau}_{2019}$	0.006	0.007
	(0.010)	(0.010)
$\hat{\tau}_{2021}$	-0.003	-0.004
	(0.010)	(0.010)
Observations	14,944	14,944

Notes: 1) Table includes only tracts in non-metropolitan area. 2) Dependent variable is two-year establishment growth rate winsorized at the 1% level. 3) Panel A report reports inverse probability weighted (IPW) estimates of the treatment effect on the treated. Panel B reports multi-period doubly robust estimates (Callaway and Sant'Anna, 2021). Census tracts are matched based on the propensity score, constructed using ACS controls in column (1), ACS controls and two-year employment growth rate in 2011-2013, 2013-2015, and 2015-2017 in column (2). 4) Standard errors in parentheses. 5) ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Table A13: 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/year/2022/guidance/understanding-naics.html.