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 investments in businesses or real estate in a limited number of low-income Census tracts. We use a census of establishment-level data on employment to identify the effect of lower capital gains taxes on business and job creation. We show that in metropolitan areas, the OZ designation increased employment growth relative to comparable tracts by between 3.0 and 4.5 percentage points. The job growth occurred in multiple industries and skill levels.

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1 Introduction

Traditional economic theory suggests that a reduction in the tax rate on capital will increase 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.

Getting estimates of the impact of capital taxation on wages and employment has proven to be 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. Of course, exogenous variation in the tax rate on capital is hard to come by. To start, capital income 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 income are infrequent and may be correlated with other policy changes. Additionally, in the aggregate, capital takes time to accumulate such that the impact on labor markets may occur long after the change in policy. States and localities differ with respect to their taxation of capital income, 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 using spatial variation in policy.

Occasionally, policy will change 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.

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.

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 over the 2017-2019 period 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.

[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`.

The construction industry experienced the greatest job growth, but the OZ designation also generated job growth in trade and service industries.

Our difference-in-difference 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 (relative to faraway tracts not receiving preferential tax treatment), a result that is consistent with the well-documented presence of agglomeration economies.

Our results are robust to a variety of specifications including a placebo test in which we use a counterfactual date for the designation. Naturally, it is possible that state executives chose the tracts receiving preferential tax treatment to achieve a political objective or to reward donors with business interests in those tracts. While we show that the process of designating OZ tracts does look somewhat political, our headline results are unchanged when we control for the extent to which a tract was chosen based on partisan considerations.

Our results provide context for current policy debates on the appropriate level of taxation of capital income and contribute to the literature examining the effects of capital gains taxes on investor behavior. The majority of this literature uses data from publicly-traded equities. 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, forthcoming), 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 are Atkins, Hernández-Lagos, Jara-Figueroa, and Seamans (2020) and Freedman, Khanna, and Neumark (2021). Atkins, Hernández-Lagos, Jara-Figueroa, and Seamans (2020) 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 geogra-

[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. Some prior research has failed to find significant effects of place-based policies, for example, Neumark and Kolko (2010), who analyze California's EC program. More recent studies, such as Billings (2009) and Busso, Gregory, and Kline (2013) of Empowerment Zones (EZ), Ham, Swenson, İmrohoroğlu, and Song (2011) of EZs and state and federal ECs, and Freedman (2012) and Harger and Ross (2016) of New Market Tax Credits (NMTCs), find a significant positive impact on local employment. See also Bartik (2019) and Slattery and Zidar (2020).

phy in the OZ program legislation. Instead of studying creation of employment and businesses inside OZs, Freedman, Khanna, and Neumark (2021) study outcomes of households living in OZs and find limited to no improvement in residents' earnings or poverty rates. We study the employment in establishments and see a significant increase in job growth among businesses in OZ tracts, but we do not know the location of workers filling any newly created jobs.[4]

2 Data and Methodology

2.1 Background

The concept of tax-advantaged Opportunity Zones had bipartisan support and backing, as the legislation was conceived and sponsored by Democratic Senator Corey Booker and Republican Senator Tim Scott (Booker, 2019). The 2017 Tax Cut and Jobs Act (TCJA), signed into law by President Trump on December 22, 2017, included the OZ legislation with provisions of the law to apply to the 2018 tax year. The TCJA allowed state executives to designate up to 25% of eligible tracts as OZs. Eligible tracts were low-income tracts and some tracts contiguous with low-income tracts.[5] 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.

[4]Other research has estimated the impact of the TCJA on outcomes unrelated to employment or business creation. Chen, Glaeser, and Wessel (2019) 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.

[5]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).

Department of the Treasury, 2018). Aggregate OZ investment in 2019 exceeded 18.9 billion dollars (Kennedy and Wheeler, 2021).

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

At least 95% of tracts designated to receive favorable OZ tax treatment had to be 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 seven years, at which point the taxable basis of the capital gains is reduced by 15% and the tax becomes payable. Investors can either invest directly in an OZ or in a Qualified Opportunity Fund (QOF). A QOF must invest at least 90% of its assets into existing or new businesses or newly constructed real estate in an OZs. Because of this transfer of capital gains on old assets into a QOF, investors sometimes refer to the OZ program as the "1031 exchange program on steroids". Second, and perhaps most importantly, capital gains on any new investments 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. In particular, Republicans in the Senate asserted the rationale for OZs as follows:

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

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

Jobs Act" and a provision that would stimulate job creation (U.S. Department of the Treasury, 2018).

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

2.2 Methodology

Similar to the approach Chen, Glaeser, and Wessel (2019) use to identify the effect of the OZ program on house prices, we use a 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 2.1 but were not chosen. We refer to the eligible-but-not-chosen 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

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

where $Y_{i,t}$ is two-year growth in an economic variable of interest in the tract, P_t is a dummy variable equal to 1 for the post-2017 period, 0 otherwise, D_i is a dummy variable that takes a value of 1 if the tract was Designated and 0 otherwise, and $\mathbf{X_i}$ 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 2017 to 2019, $Y_{i,2019} = (E_{i,2019} - E_{i,2017})/E_{i,2017}$, with the growth from 2015 to 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.[6] Our post-legislation sample covers more than 1.5 years, from the last possible date for designation in June 2018 to the end of 2019. 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.

[6] 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.

2.3 Data

Our main dataset is establishment-level employment data from Your-economy Time Series (YTS) and covers 2013-2019. Infogroup provides the licensed database used to create the Your-economy Time Series (YTS). We sum over establishments in each eligible tract to generate two variables of interest at the tract-level: employment and number of establishments. We then calculate two-year growth of each of these outcomes when estimating equation (1).

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

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

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

For the regression covariates X_i, we use tract-level data from the 2013-2017 American Community Survey (ACS) 5-year estimates.[7] We include the share of the population that is white, the share with higher education, the share that rent, the share living in poverty, the share covered by health insurance among native-born individuals, the log of median annual earnings, the log of median annual household income, the log of median monthly gross rent, the share of households receiving supplemental income,[8] the average daily commute time, and the share of the population that is employed.

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

Table 2 reports these same variables separately for Designated and Other tracts. Consistent with the presumption that state executives used the OZ program to benefit the maximum number of people, employment and the number of establishments are substantially higher in Designated than in Other tracts. Other tracts had an average of 1,912 employees, while Designated tracts had 3,156 employees. Designated tracts

[7]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, Glaeser, and Wessel (2019).

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

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

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

3 Results

3.1 All eligible tracts

Table 3 presents DiD results for employment (panel A) and establishment growth (panel B). In columns (1) and (3), we include all observations in the sample.[9] In column (1) we include no controls, while column (3) includes lagged growth (i.e. growth from 2013-2015) of the dependent variable as well as the full set of tract-level controls from the ACS. For employment growth in panel A, the coefficient on the interaction between D_i and P_t is 0.025 in column 1 and 0.028 in column 3, indicating the OZ program boosted employment growth by about 2.5 percentage points in Designated tracts, although the point estimates are not statistically significant as the standard errors are large. Panel B shows the estimates of the OZ program on establishment growth. The program increased establishment growth by 2.1 - 2.2 percentage points, shown in columns 1 and 3; these estimates are statistically significant.

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

[9] We have 52,060 tracts rather than $41,174 \times 2 = 82,348$ because of missing data for some observations.

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

In column (8), we weight the observations by the total employment in the tract in 2015. Weighting by employment reduces the magnitude of the effect on employment to 1.8 percentage points from 3.6 percentage points in our benchmark specification (column (6)), suggesting that the program disproportionately affected less populous tracts.[10] 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.

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

3.2 Metropolitan versus non-metropolitan areas

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

3.3 Robustness

3.3.1 LICs

A tract is eligible to be designated if it is an LIC or if it is contiguous to an LIC (non-LIC). We identify whether the effect of the program differs for LIC and non-LIC tracts by running the DiD regression (1) separately for the LIC and non-LIC tracts. Columns (1) and (2) of Table 5 show the results for tracts eligible by the LIC criteria. LIC tracts experienced similar growth in employment and establishments as the overall sample of all tracts in metropolitan areas, between 3.3 - 5.0 percentage points. Columns (3) and (4) repeat this analysis for tracts eligible by the contiguity criteria (non-LIC). Our point estimates suggest these tracts experienced faster employment growth, 12.4 - 13.3 percentage points, and faster establishment growth, 8.2 - 8.8 percentage points. However, the standard errors on these estimates are also higher.[11]

3.3.2 Nearby tracts

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

3.3.3 Placebo test

We check the robustness of our results by running a placebo test in which we pretend that legislation for the OZ program occurred in 2015. In implementing the DiD, we compare employment and establishment growth from 2015-2017 with 2013-2015 for

[11]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 5, 9,510, is equal to two times 4,910 less 310 observations from 155 tracts where we do not have information on commuting time.

Designated tracts relative to Other tracts in metropolitan areas. Columns (9) and (10) of Table 5 report the results. The point estimates of the coefficient on the interaction term $D_i P_t$ are nearly zero and negative for employment growth and nearly zero and positive for establishment growth, and only the small negative coefficient on employment growth in the median regression (column 9) is statistically significant at a 5% level. We conclude the results of this placebo test reinforce the validity of our findings of a positive impact of the OZ designation on employment and establishment growth in tracts in metropolitan areas.

3.3.4 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). 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). Table 6 shows the DRDiD estimates of the impact of the policy, 5.4 and 4.4 percentage points for employment and establishment growth, respectively. These estimates are on the higher end of our baseline specification and are statistically significant.[12]

3.3.5 Political tract selection

Perhaps not surprisingly, Frank, Hoopes, and Lester (2020) find that the process for selecting specific tracts to receive preferential tax treatment arising from the OZ legislation is somewhat political. To estimate whether this aspect of tract selection af-

[12] We thank Jiafeng Chen, Edward Glaeser, David Wessel for sharing their code for Chen, Glaeser, and Wessel (2019) to perform the DRDiD estimation.

fects our results, we collect data on the party of the state Governor and lower house state legislators in 2018. We assign legislators to tracts using the lower chamber State Legislative District Block Equivalent File. As in Frank, Hoopes, and Lester (2020), we define a tract to be politically affiliated with the governor if the tract's lower house representative and the governor belong to the same party.

Many tracts belong to one electoral district, which sends one representative to the lower house. In this case, 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 contained 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.[13] 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.

Table 7 presents the estimates of a Linear Probability Model in which we check to see if tract political affiliation is predictive of a tract's Designation as an OZ, conditional on the tract being eligible. Columns (1) and (2) show results with the entire sample (inclusive of non-metropolitan tracts) with state fixed effects but no ACS controls for the two definitions of political affiliation. As in Frank, Hoopes, and Lester (2020), tract political affiliation and designation as an OZ is negatively correlated without controlling for tract observable characteristics. Columns (3) and (4) add ACS

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

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 8 show that the point estimates of the impact of OZ designation on employment and establishment growth in Section 3.2 are robust to controlling for the political affiliation of the tract, the $sameparty$ variable. In columns (3) and (4), we include an interaction of the $sameparty$ variable with the P_t and D_i to see if the measured effect of the OZ program depends on the political affiliation of the tract. The estimate on 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.

3.4 Heterogeneity

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

3.4.1 New or old establishments?

The regression results reported in Table 4 considered the net change in establishments. Here, we consider establishment births and deaths. Table 9 shows that, relative to Other tracts, Designated tracts experienced a reduction in the number of failing establishments, columns (3) and (4), and an increase in new establishments, columns (1) and (2). The table shows that the effect of the OZ program on establishment births is four to six times larger than the effect on establishment deaths.

3.4.2 Intensive or extensive margin?

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

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

3.4.3 Which industries are affected?

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

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

Figure 3 suggests the OZ program 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 2.8 - 4.5 percentage points for employment growth and 3.3 - 4.3 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 10, we aggregate 2-digit NAICS sectors into six broad sectors that represent (1) agriculture, (2) construction, (3) manufacturing, (4) trade, (5) information, FIRE (finance, insurance and real estate) and management, and (6) services. Then we estimate the impact on employment and establishment growth for each 1-digit NAICS sector. Figure 4 shows 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.

3.4.4 Creation of new industries

In this section, we ask if the OZ legislation encouraged job creation in industries that had no prior establishments or employment in any given Designated tract. To answer this question, we create a dummy variable that takes a value of 1 if at least one new establishment was created in a two-year period in a "new", i.e., previously unrepresented, industry for the tract and 0 otherwise. Columns (1) and (3) of Table 11 present results based on the 2-digit and 4-digit NAIC classifications when this dummy variable is the dependent variable in the DiD. Even though the estimates in these columns are not statistically significant, for completeness we run a placebo test of designation on the number of new industries created in 2013-2015 (pre-period) and in 2015-2017 (post-period) and also find insignificant estimates, shown in columns (2) and (4). We thus conclude that the policy did not create jobs in industries that had no prior establishments in Designated tracts.

3.4.5 Who gets hired?

Policymakers might be concerned that new jobs created by the OZ program are predominantly being filled by high-wage workers who have no immediate connection to the low-income tracts targeted by the OZ program. We thus explore growth in employment created by the OZ program by the skill level of the industry. We measure the skill level of the industry using the average educational level of an industry from the 2004 ACS, which ranges from 1 for "some high school" to 5 for "graduate school". We use the 4-digit NAICS code to classify industries into education quantiles based on the intensity of skilled occupations in each industry. We take the skill-level of each 4-digit NAICS code from Oldenski (2012).[14]

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

3.4.6 Heterogeneity by tract characteristics

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

[14] We are grateful to Lindsay Oldenski for sharing her data with us.

3.5 Displacement of employment

We now investigate the extent to which the program simply shifted employment from nearby tracts to Designated tracts, or whether the presence of an OZ in an adjacent tract increased employment through agglomeration 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.[15] 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:

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

$D_i = 1$ if tract i is k-step contiguous to an OZ for any $k = 0, ..., 4$. Similarly, $D_i = 0$ if tract i is k-step contiguous to a non-designated eligible tract for any $k = 0, ...4$.

[15] 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.

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

Column (1) of Table 12 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 12, 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 (forthcoming) show that vertical agglomeration economies within a building are strongest on the same floor and are largely gone by a distance of three floors. Rosenthal and Strange (2020) review the evidence on the scale of agglomeration economies and conclude that the strongest agglomeration forces are likely at the neighborhood level.

4 Conclusion

The OZ program created quasi-experiemental 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. Given the fact that the OZ program is new, we leave to future research the longevity of the employment gains as well dynamic impacts related to agglomeration.

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Table 1: 2017 Characteristics of Eligible Tracts

Variable	Obs.	Mean	SD	Median	Minimum	Maximum
Designated	41,174	0.190	0.392	0	0	1
Employment	41,174	2,148	4,013	1,137	1	235,158
Employment growth	41,161	3.9%	34.9%	1.4%	-98.5%	41,100%
Number of establishments	41,174	202	266	130	1	12,793
Growth in the number of establishments	41,161	4.4%	18.0%	1.6%	-86.4%	900%
Number of entered establishments	41,174	48.7	70.6	30	0	2,456
Number of exited establishments	41,161	43.4	67.9	28	0	4,709
Percent of entered establishments	41,161	27.9%	21.5%	22.4%	0%	900%
Percent of exited establishments	41,161	23.5%	9.4%	21.6%	0%	100%
Population	41,164	4,172	1,994	3,905	0	40,402
Total housing	41,164	1,534	707	1,446	0	12,768
% Poverty	41,146	19%	10.3%	17%	0%	100%
% Employed	41,146	29.7%	7.8%	30%	0%	100%
% White	41,146	65.9%	28%	73.5%	0%	100%
% Higher ed	41,146	19.1%	10.4%	17.3%	0%	100%
% Renters	41,107	45.5%	22.7%	42.6%	0%	100%
% Native-born with health insurance	41,139	89.1%	6%	90.1%	0%	100%
% Supplemental income	41,146	9.2%	6.4%	7.9%	0%	52.8%
Median annual earnings	41,058	$27,384	$7,929	$26,772	$2,499	$116,354
Median annual household income	41,029	$44,553	$15,531	$43,077	$2,499	$177,824
Median monthly gross rent	40,917	$899	$308	$832	$99	$3,501
Average daily commuting time (min)	26,190	36.9	14.8	34.9	3.3	632.5

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

Table 2: 2017 Characteristics of Eligible Tracts by Designation

Variable	Mean		SE		t-value for
	Other	Designated	Other	Designated	diff. in means
Designated	0	1	0	0	
Employment	1912	3156	3589	5349	-24.8
Employment growth	4.4%	1.9%	36.2%	28.5%	5.825
Number of establishments	186	269	244	334	-25.1
Growth in the number of establishments	4.6%	3.8%	17.1%	21.3%	3.41
Number of entered establishments	46.2	59.4	66.8	84.0	-15.0
Number of exited establishments	40.7	55.2	63.9	81.7	-17.1
Percent of entered establishments	28.4%	25.9%	21.0%	23.4%	9.11
Percent of exited establishments	23.8%	22.1%	9.5%	8.5%	14.4
Population	4208	4022	1997	1973	7.5
Total housing units	1550	1464	711	687	9.73
% Poverty	17.7%	24.6%	9.7%	11%	-55.3
% Employed	30.3%	26.8%	7.7%	7.7%	36.8
% White	68.0%	57.4%	27.1%	29.9%	30.4
% Higher ed	19.8%	16%	10.6%	9.0%	29.3
% Renters	43.2%	55.2%	22.2%	22.4%	-42.9
% Native-born with health insurance	89.4%	87.9%	5.8%	6.4%	20.0
% Supplemental income	8.6%	11.9%	6.0%	7.2%	-41.8
Median annual earnings	$28,087	$24,386	$7899	$7335	37.7
Median annual household income	$46,435	$36,538	$15,444	$13,167	52.3
Median monthly gross rent	$915	$826	$314	$271	23.1
Average daily commuting time (min)	36.8	14.7	37.1	15.2	-1.11

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

Table 3: Employment and Establishment Growth Regressions

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

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

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

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

Notes: 1) Columns (1) and (3) report results for quantile 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) P_t is a dummy variable equal to 1 for the post-2017 period, 0 otherwise, D_i is a dummy variable that takes a value of 1 if the tract was designated an OZ and 0 otherwise.

Table 5: Robustness

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

Notes: 1) Sample of tracts in metropolitan areas. 2) Columns (1), (3), (5), (7), (9) report results for quantile regression to the median or Least Absolute Value (LAV). 3) 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), P_t is a dummy variable equal to 1 for the post-2017 period, 0 otherwise, In columns (9) and (10), P_t is equal to 1 for the 2015-2017 period, 0 otherwise.

Table 6: DRDiD Results

	All		Metropolitan Area	
	(1)	(2)	(3)	(4)
	Raw	Winsorized at 1%	Raw	Winsorized at 1%
Panel A: Employment Growth				
$\hat{\tau}$	0.033**	0.042***	0.053***	0.054***
	(0.016)	(0.007)	(0.007)	(0.020)
t-value	2.028	6.037	7.529	2.672
Panel B: Establishment Growth				
$\hat{\tau}$	0.023**	0.032***	0.046***	0.044***
	(0.009)	(0.004)	(0.005)	(0.007)
t-value	2.544	8.050	9.681	6.397

Notes: 1) $\hat{\tau}$ is the average treatment effect on the treated (ATT) estimated following Sant'Anna and Zhao (2020). 2) Standard errors in parentheses. 3) ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Table 7: 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 8: Employment and Establishment Growth with Political Consideration

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

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

Table 9: 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	-0.025***	-0.031***	-0.012***	-0.011***
	(0.003)	(0.003)	(0.002)	(0.002)
P_t	-0.056***	-0.089***	-0.014***	-0.008***
	(0.002)	(0.002)	(0.001)	(0.001)
D_iP_t	0.031***	0.040***	-0.005*	-0.009***
	(0.004)	(0.004)	(0.003)	(0.002)
$Emp.Growth_{2013-2015}$	0.083***	0.104***	0.150***	0.112***
	(0.003)	(0.003)	(0.002)	(0.002)
Observations	40,944	40,944	40,944	40,944
R^2		0.177		0.211

Notes: 1) Sample of tracts in metropolitan areas. 2) Columns (2) and (4) report results for quantile regression to the median or Least Absolute Value (LAV). 3) 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) P_t is a dummy variable equal to 1 for the post-2017 period, 0 otherwise, D_i is a dummy variable that takes a value of 1 if the tract was designated an OZ and 0 otherwise.

Table 10: One digit NAICS industries

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

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

Table 11: Creation of New Industries

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

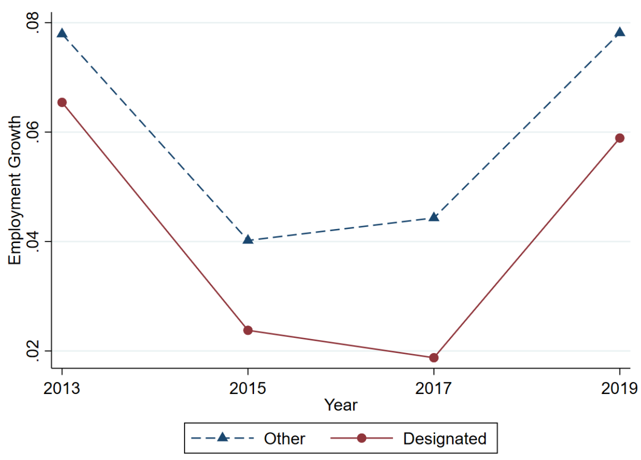
Notes: 1) Sample of tracts in metropolitan areas. 2) Robust standard errors are in parentheses. 3) ***, **, and * denote significance at the 1%, 5%, and 10% levels. 4) D_i is a dummy variable that takes a value of 1 if the tract was designated an OZ and 0 otherwise. 5) In columns (1) and (3), P_t is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

Table 12: Estimates of Spillover Effects on Neighboring Tracts

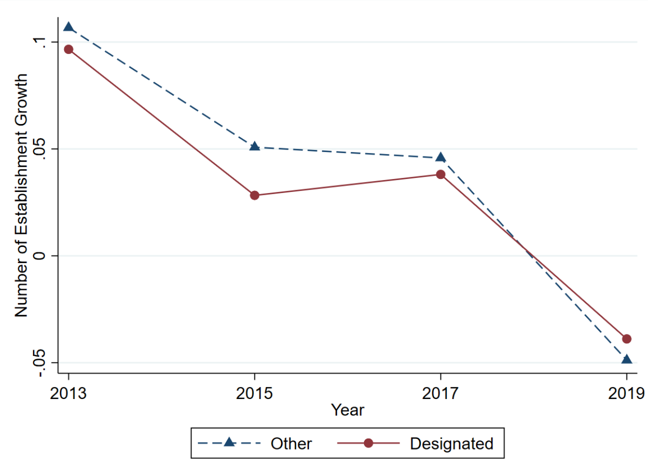
	(1)	(2) test of net effect
D_i	-0.018***	
	(0.005)	
P_t	-0.080***	
	(0.003)	
D_iP_t	**0.045*** **	
	(0.007)	
$G_{i,1}D_iP_t$	**-0.026*** **	**0.019**
	(0.009)	**p=0.0006**
$G_{i,2}D_iP_t$	**-0.027** **	**0.018**
	(0.010)	**p=0.0134**
$G_{i,3}D_iP_t$	**-0.030** **	**0.015**
	(0.015)	**p=0.2493**
$G_{i,4}D_iP_t$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

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

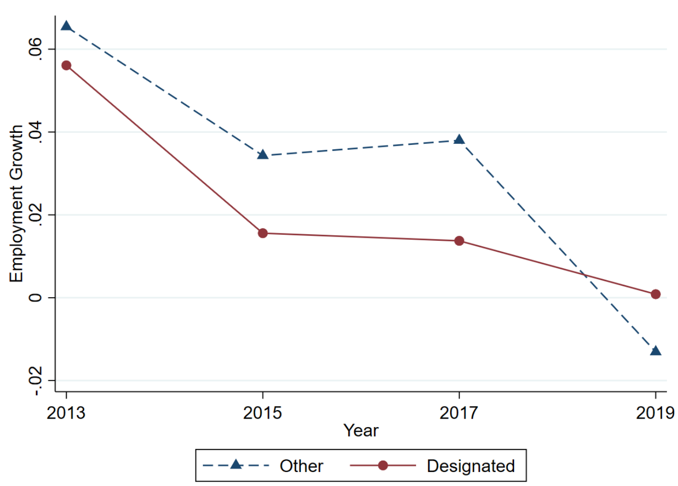
Figure 1: Biennial Tract Growth Rates for Eligible Tracts



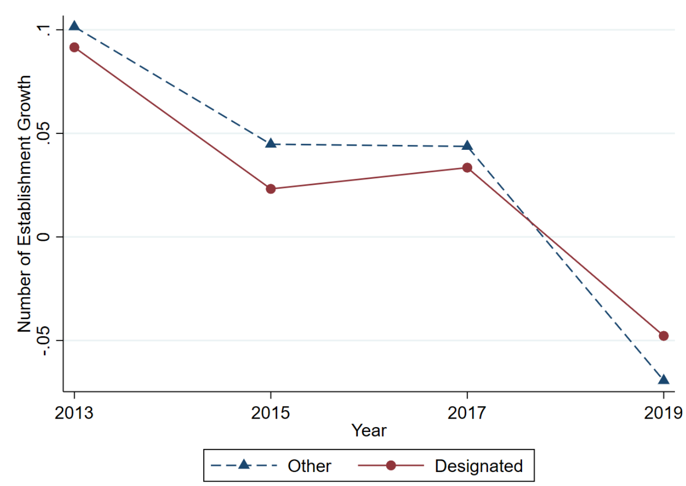
(a) Employment Growth, Raw Data



(b) Establishment Growth, Raw Data

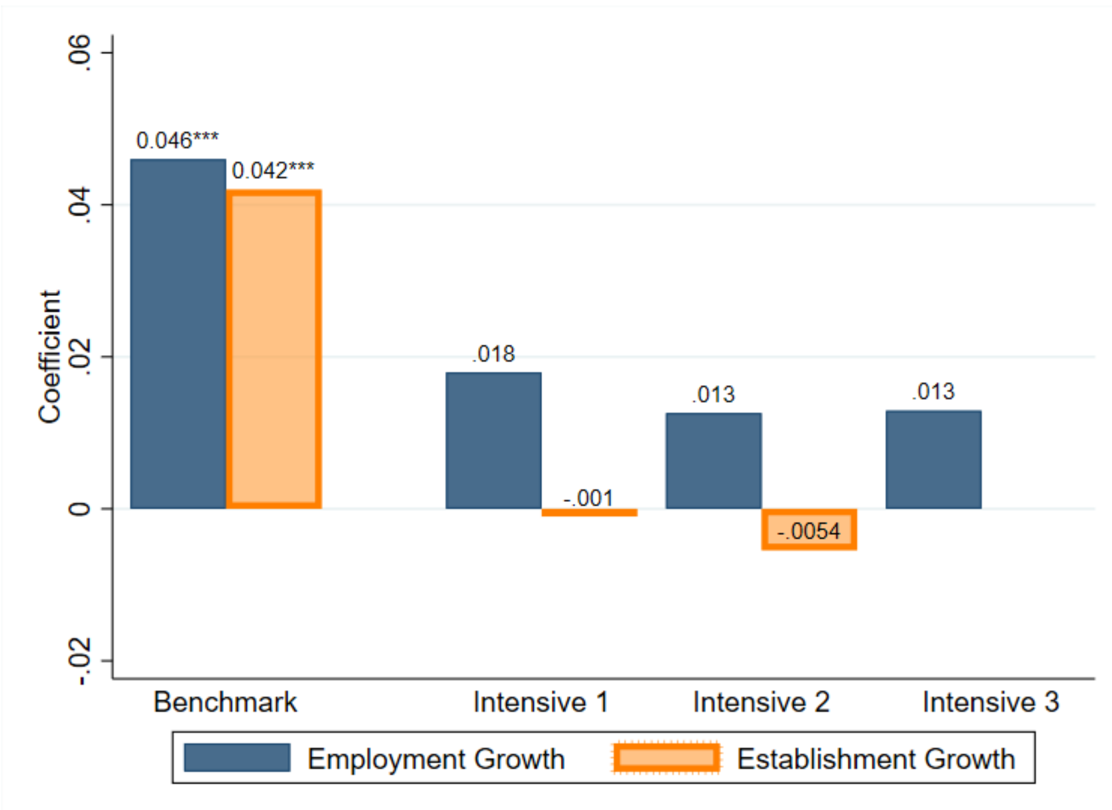


(c) Employment Growth, Winsorized at 1%



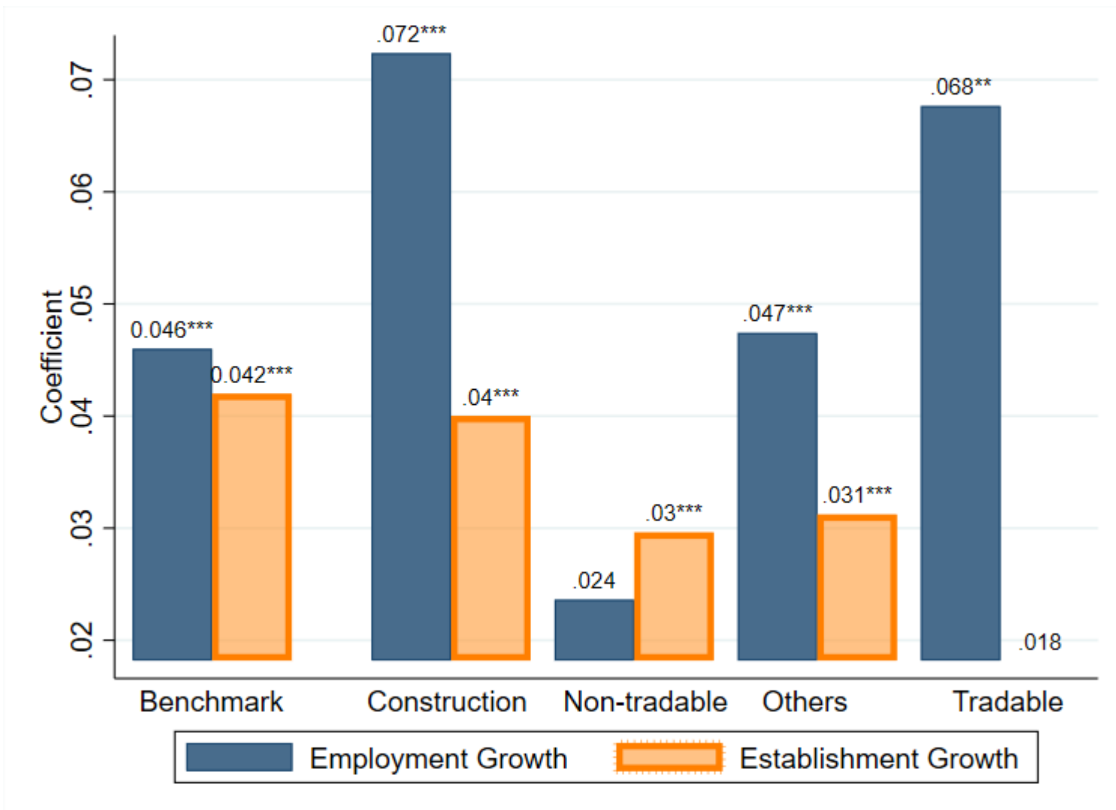
(d) Establishment Growth, Winsorized at 1%

Figure 2: Estimates with Existing Establishments



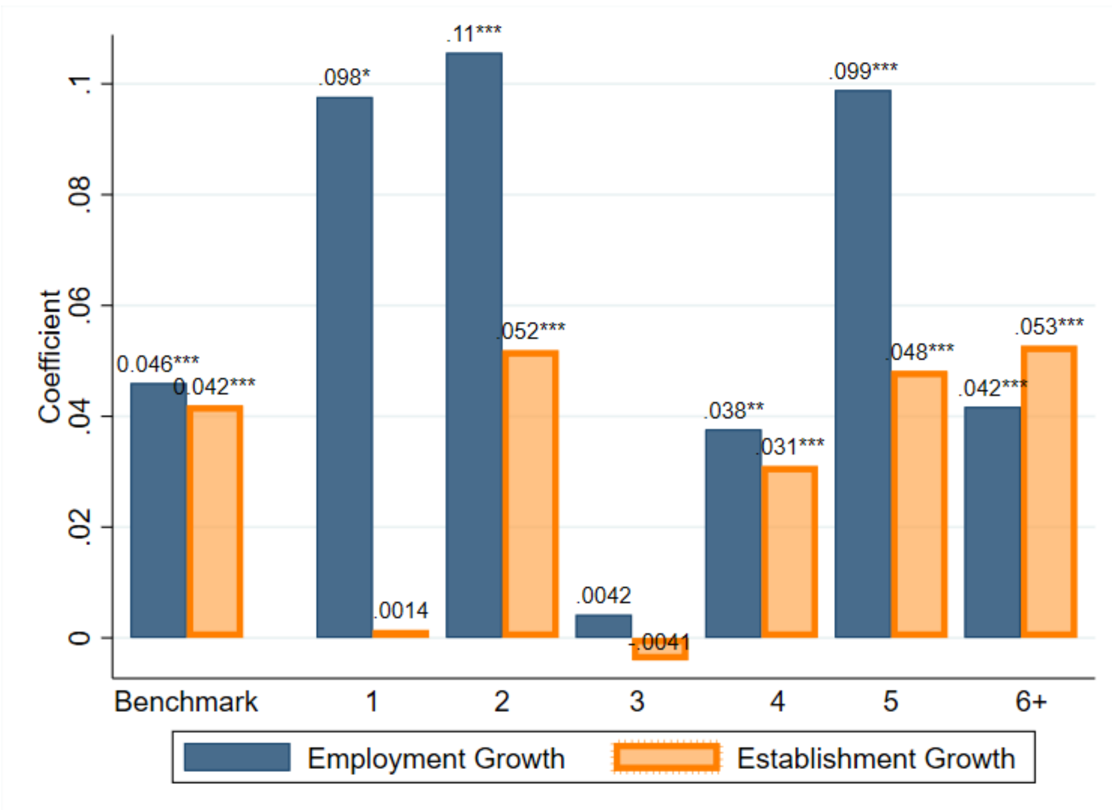
Notes: 1) Sample of tracts in metropolitan areas. 2) ***, **, and * denote significance at the 1%, 5%, and 10% levels. 3) See definitions of Intensive 1, 2, 3 in the text. 4) The benchmark results are from column (2) of Table 4, OLS Winsorized at 1%.

Figure 3: Estimates by Industry Type



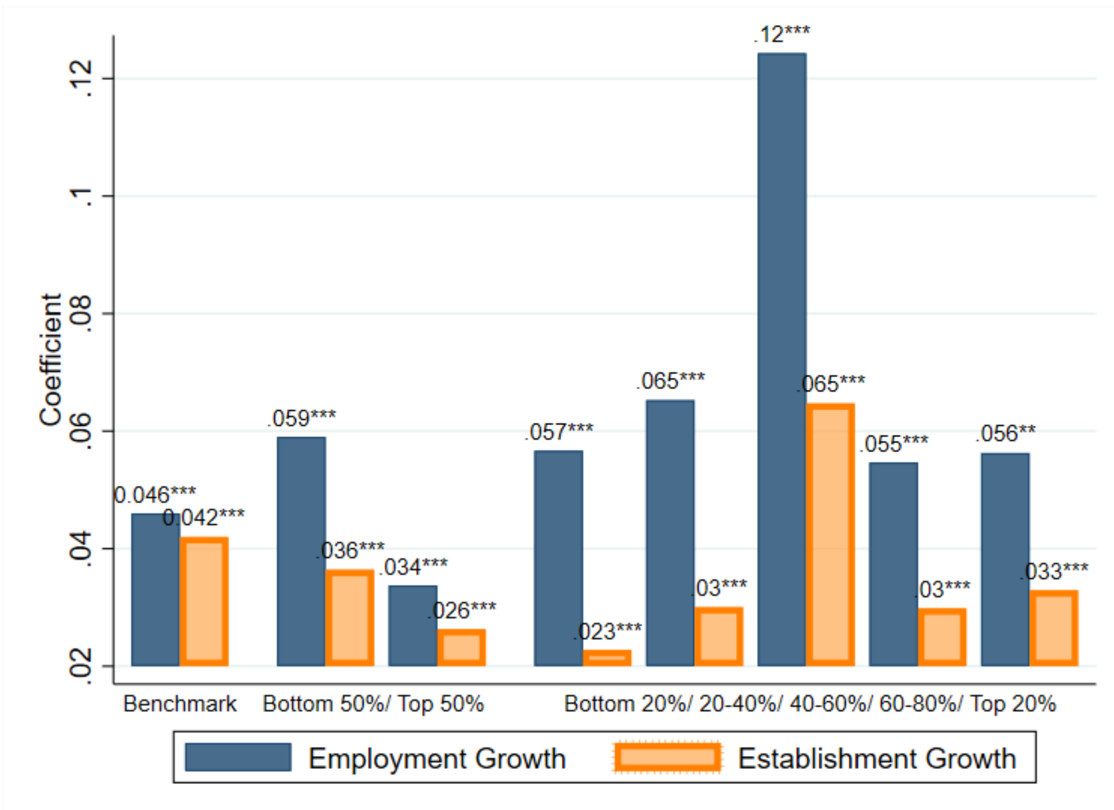
Notes: 1) Sample of tracts in metropolitan areas. 2) Benchmark estimate is from Table 4, column (2). 3) ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Figure 4: Estimates by 1-digit NAICS industry



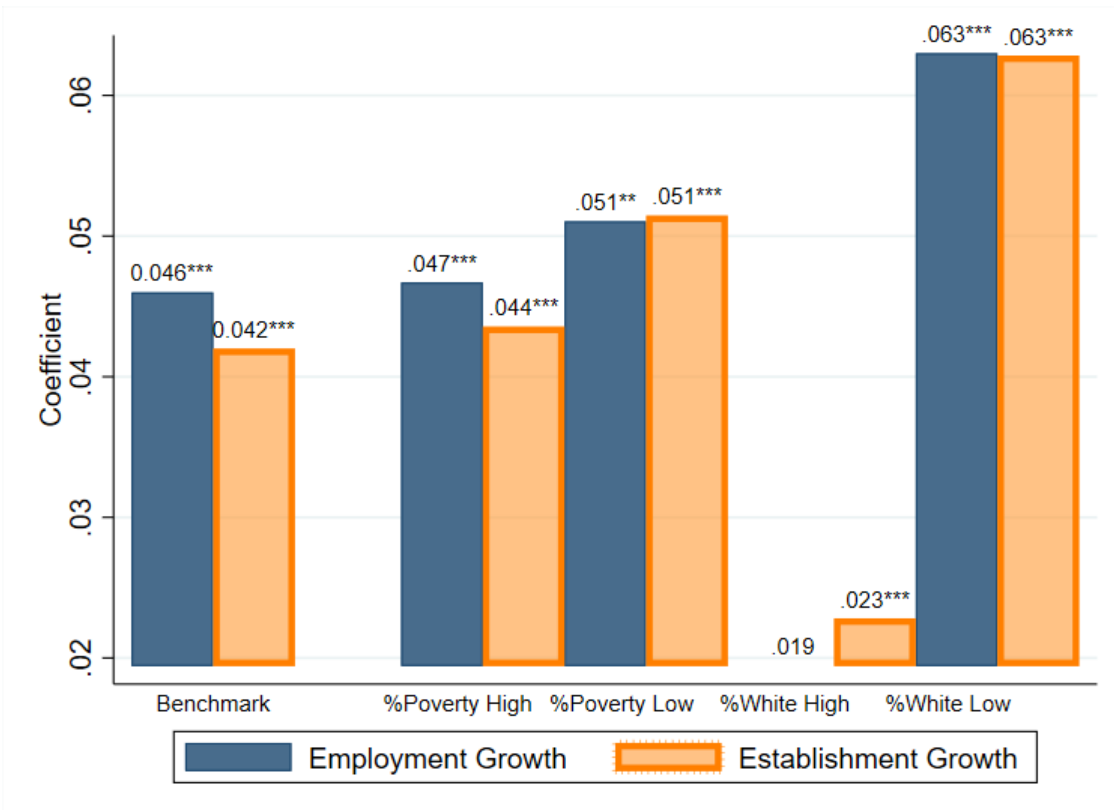
Notes: 1) Sample of tracts in metropolitan areas. 2) Benchmark estimate is from Table 4, column (2). 3) ***, **, and * denote significance at the 1%, 5%, and 10% levels. 4) Broad 1-digit NAICS sectors: (1) agriculture, (2) construction, (3) manufacturing, (4) trade, (5) information, FIRE (finance, insurance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

Figure 5: Estimates by Education of Industry



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

Figure 6: Estimates by Tract Characteristics



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

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), see `https://www.census.gov/topics/employment/labor-force/about/faq.html#par_textimage_735773790`. (3) The ACS controls are all variables with names starting with "acs".