JUE Insight: What is the Impact of Opportunity Zones on Job Postings?

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Abstract

We study the effect of Opportunity Zones (OZs) on job postings using data comprising the near-universe of U.S. online job postings. The OZs program grants tax breaks for investment in designated distressed communities, nominated by each state's governor based on multiple factors. We use propensity score matching to account for those factors and a difference-in-differences model over the matched sample to estimate the effect of the program. We find limited evidence of any effect of OZs on job postings on average, although we do find small positive effects in urban areas, in areas with above median Black population, and in some states.

Keywords— Opportunity Zones; Employment; Place-based policies; Tax policies; Job postings.

JEL Codes— J23, J31, J38, H25, R12

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1 Introduction

The 2017 Tax Cuts and Jobs Act established Opportunity Zones (OZs) as a way of stimulating employment and economic growth in distressed communities in the United States. The OZs program designates a set of distressed communities and subsidizes investment deployed in them through tax breaks. Approximately 10% of the U.S. population resides in one of the more than 8,700 OZs. While program supporters argue that investment has reached communities otherwise overlooked by investors, critics maintain that the OZs program promotes investment in areas that would have attracted investors anyway.[1] The argument is that these tax breaks will be used to fund luxury apartments or hotels, especially in the relatively few non-low-income areas that were also designated as OZs. Despite the controversies, in August 2020 the White House Council of Economic Advisers reported as much as $75 billion have been invested in OZs, and information from electronically-filed tax records indicates investments of about $18.9 billion in tax year 2019 (Kennedy and Wheeler (2021)).[2] The divergence of opinions about the program and the amount of resources already committed to the program call for an early assessment of its effects on job creation in distressed communities. But measuring job creation is challenging not only because communities designated as OZs might have attracted investment even without the program (the selection problem) but also because the OZ program was not in effect for very long before the economy was disrupted by the COVID-19 pandemic, making assessment of the program difficult. In this paper, we use high frequency data on job postings as a forward looking measure that captures a firm's plan to hire (Gutiérrez et al., 2020) to at least partially address these issues and contribute to the debate on the efficacy of the OZ program.

We use a large dataset covering the near universe of U.S. online job postings between January 2016 and March 2020, and a propensity score matching together with a difference-in-differences research design to evaluate the local effects of the OZs program on job postings. We use high-frequency data on job postings from Burning Glass Technologies (BG). BG data afford us comprehensive time and spatial coverage at the zip code level. The main econometric challenge in estimating the effects of the OZs program is to identify an appropriate counterfactual for the areas (census tracts) that were designated as OZs. Since the governors of each state nominated OZs based on a variety of factors, estimating differences in job postings between OZs and non-OZs could be biased due to OZs' selection criteria. We address this issue by using propensity scores to match zip codes containing OZ-designated tracts to observationally similar zip codes containing tracts that qualified for the OZ program but were not selected. Our approach compares the number of job postings before and after OZ designation across treated zip codes (those with one or more designated OZ) and their matched control zip codes (those with one or more eligible census tracts that were not designated as an OZ).

We find limited evidence of any effect of OZs on job postings on average. In addition, we find no evidence of an increase in job postings in OZs with confirmed investments. Neither do we find an effect on job postings in construction and real estate industries, the industries in which we should expect a large number of projects taking advantage of the program. When exploring heterogeneous effects, we observe positive effects in urban areas, areas with above median Black population and some states. However, all those effects translate into small increases in job postings. Taken together, we interpret these results as suggesting that, on average, there is little evidence that OZ designation led to an increase in job postings, but that the null average treatment

[1] See for example The New York Times (August 31, 2019) or Politico (September 29, 2020).

[2] The $75 billion come from "The Impact of Opportunity Zones: An Initial Assessment," by the Council of Economic Advisers, August 2020, page 1.

effect of the OZ designation hides some heterogeneous effects based on population density and demographic composition of the treated areas.

Our paper contributes to the literature on the effects of place-based policies (Ham et al., 2011; Neumark and Kolko, 2010; Glaeser and Gottlieb, 2008; Bartik, 2002; Boarnet and Bogart, 1996; Papke, 1994). Specifically, we contribute to the nascent research on the drivers of OZs designation and the broad economic impact of this program. The OZs program follows the tradition of most place-based programs by listing local job creation as one of the main goals. Yet, evidence on the effectiveness of place-based programs at fostering local job creation is mixed (Neumark and Simpson, 2015), with some scholars documenting positive employment effects (Busso et al., 2013; Criscuolo et al., 2012; O'Keefe, 2004) while others highlight null results (Elvery, 2009; Neumark and Kolko, 2010), equity efficiency trade offs (Kline and Moretti, 2014), or spillover effects which make these programs less beneficial to existing residents (Freedman, 2012). Early evidence of OZs selection suggests that the designation of some census tracts and not others was driven by a variety of considerations other than improving local economic conditions (Eldar and Garber, 2020; Frank et al., 2020). Frank et al. (2020) find that governors are more likely to designate a census tract as an OZ when its local state representative is a member of the governor's political party. Despite how designation was executed, research on the economic effects of OZs is still being conducted, both using qualitative and quantitative methods, mostly finding little evidence of a positive impact (Corinth and Feldman, 2021; Alm et al., 2021; Newman and Snidal, 2021; Kurban et al., 2021; Bekkerman et al., 2021; Theodos et al., 2020; Sage et al., 2019; Chen et al., 2022). Chen et al. (2022) use data at the zip code level to study the effect of OZ designation on property values, finding no significant effect. Regarding employment outcomes, Freedman et al. (2021) use restricted-access data from the American Community Survey 2013-2019 to study whether the OZ program produced positive effects on employment, earnings, and poverty of residents of OZ designated census tracts. Using a matching procedure that balances treatment and control areas on pre-treatment outcomes, they find little evidence of an effect. In contrast, Arefeva et al. (2020) use annual establishment data from Your-economy Time Series (YTS) 2015-2019, a private data provider, and a standard difference-in-differences approach to document that the OZs program led to 2-4% higher employment growth.

Our work follows Freedman et al. (2021) and Chen et al. (2022) in that we perform a matching procedure to account for non-random treatment assignment and difference-in-differences estimation over the matched sample. We complement Freedman et al. (2021) by focusing on monthly data instead of yearly data from January 2016 through March 2020. Our window of analysis ends after March 2020 to avoid the potentially confounding effects of COVID-19. Our work also complements Freedman et al. (2021) and Arefeva et al. (2020) by considering job postings instead of employment. Job postings differ from employment in that they provide a forward looking measure that captures a firm's plans to hire. As such, job postings are correlated with future employment (Gutiérrez et al., 2020). We believe job postings are especially useful in this case as they allow us to study how firms in zip codes with OZs responded to OZ incentives right up to the onset of the COVID-19 disruption starting in March 2020. Despite their conceptual and practical importance, job postings have received relatively little attention in empirical work until recently (Davis et al., 2013).

2 Opportunity Zones

Opportunity Zones (OZs) were first described in a 2015 white paper by Jared Bernstein and Kevin Hassett published by the Economic Innovation Group (Bernstein and Hassett, 2015). In 2017, the OZ program was included in the Trump Administration's 2017 Tax Cuts and Jobs Act. According to the Council of Economic Advisers, OZs are a supply-side policy "designed to spur investment and drive up labor demand, and thus directly help the disadvantaged achieve self-sufficiency through increased economic activity" (CEA 2020, p. 72).[3] The OZ program provides incentives for investors to reinvest capital gains in specially designated areas via dedicated funds called Opportunity Funds. The incentives granted to Opportunity Fund investors include a temporary tax deferral and a step-up in basis for capital gains invested in an Opportunity Fund. These incentives get larger if investors hold their investments in Opportunity Funds for longer periods of time. According to the Economic Innovation Group, every $100 invested in an Opportunity Fund would see a gain of $44 above the return from investing in a traditional stock portfolio over a 10-year period.[4]

The OZs program was passed into law in 2017, but several important updates to the program occurred throughout 2018. Perhaps the most important of these updates was the definition of the areas that could qualify as Opportunity Zones. According to the February 2018 IRS guidance for nominating OZs, a "population census tract is eligible for designation as a Qualified Opportunity Zone (QOZ) if it satisfies the definition of "low-income community" (LIC) in S45D(e) of the [Internal Revenue] Code." LICs are census tracts with median incomes below 80% of the area median income according to the American Community Survey (ACS) 5-year estimates (2011-2015 or 2012-2016) or with a poverty rate of at least 20%.[5] In addition, the IRS code specifies that a non-LIC tract is eligible for designation as an OZ if the "tract is contiguous with an LIC that is designated as an [OZ]." These non-LIC tracts that are contiguous to LIC tracts can only comprise up to 5% of the total number of census tracts that a state nominates for the OZ program.

Ultimately, each state's governor ran a different process for nominating zones, which the U.S. Treasury later certified as OZs. According to Wallwork and Schakel (2018), some states such as Georgia, Texas, and Wisconsin only nominated low-income tracts; other states such as California and North Carolina included high-income tracts contiguous to low-income tracts. Moreover, evidence suggests that some state governments sought to pair existing initiatives with the OZ program to maximize capital investment. Illinois, for example, expanded eligibility for its "Solar for All" clean energy investment program to include all of the state's OZs (Theodos et al., 2020). Other examples include Maryland, Virginia and the District of Columbia, which implemented web-based platforms to help investors in OZs to take advantage of state or local incentives such as funding streams for workforce development and affordable housing, among others. The government of South Carolina gave preferential status to OZ projects in its Qualified Allocation Plan for low-income housing tax credits, Colorado created the OZ technical support grants and New Jersey pledged $500K to communities with the best OZ plans. In an attempt to ameliorate the potential confounding role of these additional incentives on the effect

[3]From the February 2020 "Economic Report to the President," available at https://www.whitehouse.gov/wp-content/uploads/2020/02/2020-Economic-Report-of-the-President-WHCEA.pdf

[4]The calculations assume an annual investment appreciation of 7%, and a long-term capital gains tax rate of 23.8% (federal capital gains tax of 20% and net investment income tax of 3.8%). Details in "Opportunity Zones: A New Incentive For Investing in LIC" available at https://eig.org/wp-content/uploads/2019/10/Opportunity-Zones-Fact-Sheet.pdf

[5]A community could also be a LIC if its median household income is at or below the threshold designated as low-income by HCD's State Income Limits. Both definitions of LICs (determined through ACS or HCD's State Income Limits) are used and can be combined according to the documentation in https://ww2.arb.ca.gov, retrieved in May 2021.

of OZs, we match on observable characteristics and pre-treatment outcome variables and account for time and space fixed effects using difference-in-differences techniques, as we describe in the Data and Methods section.

The Opportunity Funds invest in projects in one or more of the 8,700 OZs throughout the U.S. While this investment is expected to increase labor demand, the program does not include hiring requirements for eligible projects. Projects that qualify for Opportunity Fund investment include real estate development of property located within an Opportunity Zone and stock ownership of, or partnership interest in, qualified businesses that operate entirely or primarily within an Opportunity Zone. Language in the Tax Cut and Jobs Act was clear regarding eligible real estate investments, but many investors were unsure of which direct business investments would qualify. Thus, most early Opportunity Funds favored investments in real estate while investors waited for new regulations to be finalized. The Treasury Department offered further clarification on the types of investments that qualify for tax incentives through a series of regulations that were not finalized until 2019.

President Trump signed an executive order in 2018, creating the White House Opportunity and Revitalization Council to "better coordinate Federal economic development resources in Opportunity Zones and other distressed communities."[6] This body identified more than 200 federal grants and programs to explicitly encourage or prioritize projects located in OZs.[7] After the Tax Cut and Jobs Act was passed in 2017, the Joint Committee on Taxation estimated an average annual cost for the Opportunity Zone Program of approximately $1.5 billion.[8] In 2019, after the final regulations were released, the Joint Committee estimated that the program would cost approximately $3.5 billion per year in lost tax revenue.[9]

3 Data and Methods

We combine data from two primary sources: the American Community Survey 5-year estimates (ACS) and Burning Glass Technologies (BG). The ACS is a yearly survey conducted by the U.S. Census Bureau that reports demographic variables at the census tract level. The 2015 and 2016 versions of the ACS were the latest available versions when the federal government proposed which census tracts could participate in the OZ program. We use data from these two versions.

Data on job postings from January 2016 through March 2020 come from Burning Glass Technologies (BG). The BG dataset has the near universe of jobs that were posted online from 2010 through the present. We start our time series in January 2016 and stop our time series in March 2020 because of COVID-related disruptions. The BG dataset includes the location of the firm advertising the opening. Anyone can see the advertisement in any location. BG does not provide data at the census tract level but it does provide data at the zip code level. Our dependent variable is the monthly total number of new job postings by zip code.

Job posting data, from BG and other providers, have become increasingly important sources of job market information tracked by academics and firms alike (e.g., the S&P Dow Jones has created an index that measures job openings at the firms in the S&P 500). A number of academic studies have used job postings data from BG. Many of these papers validate job posting data by correlating it with other measures of job openings, such as the Bureau of Labor Statistics Job Openings and Labor Turnover Survey (JOLTS) data, and showing that job posting data also correlate with measures of other employment outcomes. Hershbein and Kahn (2018),

[6] The White House Opportunity and Revitalization Council.
[7] The White House Opportunity and Revitalization Council completed program targeting actions, see www.hud.gov.
[8] Estimated Budget Effects Of The Conference Agreement For H.R.1, The "Tax Cuts And Jobs Act."
[9] Estimates Of Federal Tax Expenditures For Fiscal Years 2019-2023.

for example, compare BG job posting data to JOLTS data, as do Chetty et al. (2020). These papers show a strong correlation between BG job postings and job openings in JOLTS, across industries. Hershbein and Kahn (2018) also compare BG data to Current Population Survey (CPS) data and find that the representativeness of the BG data is generally stable over time. Deming and Kahn (2018) show that skill requirements listed in job postings are predictive of wage patterns. Forsythe et al. (2020) show a high correlation between BG job postings and BLS employment data. In a similar spirit, we compare the number of job postings reported in BG with data from the Quarterly Census of Employment and Wages (QCEW), available at the county-quarter unit of analysis, and the ZIP Codes Business Patterns (ZBP), available at the zipcode-year unit of analysis. These comparisons are reported in Table A.1 of the Appendix. Other researchers have used the Burning Glass data at several levels of analysis including establishment (Acemoglu et al., 2022), zip code (Burke et al., 2020), county (Azar et al., 2020), state (Forsythe et al., 2020), and national (Goldfarb et al., 2020) levels. Other papers have explored the link between employment and job posting data from sources other than BG. For example, using a detailed dataset from Denmark, Bagger et al. (2022) show that posting a vacancy is associated with a 70% increase in hiring over the baseline of no-vacancy posting hiring within four months of a job posting.

One benefit of the BG job posting data is its high frequency. This allows us to use job postings as a monthly measure that captures a firm's hiring plans. The benefit of such a measure is that we can track its changes from after OZ designation right up until the onset of COVID-19 disruptions in March 2020. One limitation of the BG data is that the unit of analysis is the zip code instead of the census tract (BG data does not provide a field for "census tract"). This is an important limitation because the OZ designation occurs at the census tract level, not zip code level. However, a robustness test using only zip codes that overlap at least 90% in terms of population or area with census tracts suggests that the results should not be sensitive to whether the unit of analysis is zip code or census tract. We report the results in the Appendix.

To map from census tracts to zip codes, we use the HUD USPS zip code crosswalk files.[10] The median zip code contains three census tracts. We consider all zip codes in the continental United States, except Washington DC, that had at least one job posting during the pre-treatment period (2016 - 2017) and overlapped with at least one low-income community. We consider a zip code to be treated if at least one of the census tracts within the zip code is designated as an OZ.

The designation of OZs happened at the state level during the first half of 2018. State governments proposed OZs and the federal government proceeded to certify them. Most of the proposed tracts were certified as OZs. Given that the final publication of the guidelines for nomination and the date by which the nominated tracts had to be submitted to the Treasury for approval were very close to each other, state governments had very little time to come up with a list of tracts to propose to the Treasury. Because the federal government used the ACS to establish OZ eligibility, it is reasonable to believe that state governments used the ACS as their core source of information to select tracts. This observation motivates our matching strategy, which relies on the ACS.

The 2015 and 2016 ACS data provided the basis for eligibility. Low-income census tracts were eligible to be designated as OZs if their poverty rate was at least 20% or their median income was less than 80% of area median income.[11] Eligible tracts had to qualify as low-income in either the 2015 or 2016 ACS. Designation based on

[10] Available from HUD USPS.

[11] Area meaning state for rural tracts and CBSA for urban tracts, according to the urban/rural definition of the 2010 census.

poverty or median income left room for state authorities to nominate communities that were thriving otherwise, such as urban communities with high population growth or income growth. If high-potential communities were nominated, then any effect of the OZ program on job postings could be biased upwards because OZ designation would be a consequence of the area's already existing growth potential. Motivated by the possibility of non-random OZ designation and the event-study results in Freedman et al. (2021), we include pre-treatment BG job posting growth and other growth related variables in the matching procedure to address the potential bias in the effect.

Beyond demographic characteristics captured in the ACS data, politics seems to have played a role. Frank et al. (2020) shows that tracts were more likely to be selected when the local state representative for that tract was of the same political party as the state governor. This means that even if conditioned on ACS data, the assignment was not random. We include political variables in our propensity score matching to address potential bias due to arbitrary nomination.

Before we describe our estimation method, it is important to note that not all OZs correspond to low-income communities. The program allowed for 5% of designated tracts to be contiguous with an eligible tract, if they did not exceed 125% of the eligible tracts' median income. We focus only on low-income OZs for two reasons. First, the program's stated goal is to improve low-income areas in several dimensions. Our focus is on job postings in low-income areas, but other studies consider other outcomes such as employment of residents (Freedman et al., 2021) or real estate prices (Chen et al., 2022). Including non-low-income census tracts makes it harder to assess the program's efficacy in meeting its goal. Second, low-income tracts are plenty and well defined, so we can match zip codes encompassing low-income tracts to zip codes encompassing similar eligible tracts that were not nominated as OZs. In contrast, non-low-income census tracts are few, and their selection responded to the idiosyncrasies of each state; some states did not even allow non-low-income tracts to be selected. Moreover, designated non-low-income tracts do not possess sufficient eligible but not nominated (control) areas for matching (Freedman et al., 2021).

Our identification assumption is that, conditional on a set of demographic and political characteristics (population, poverty rate, income, race, education, population and income growth rate, whether the zone is urban or rural, and political affiliation), as well as pre-treatment outcomes (as we describe below) nomination for the program is as good as random. This is justified by the assumption that state governments relied on data from the ACS due to the little time they had to nominate tracts. We expect the propensity score to capture the part of the policymakers' designation criteria that is due to relevant information from the American Community Survey other than median household income or poverty rate. Our interviews with stakeholders suggest that our assumptions are valid. In particular, most stakeholders support the idea that the process was fairly "rushed." The IRS published guidance for designating OZs in February 2018 and the deadline for governors to nominate tracts was just one month later (GAO (2020), Figure 1).

More precisely, we build a matched control group for the set of all zip codes with at least one OZ. The matching and the propensity score estimation are done for each state independently. The main challenge to building a matched-control group of zip codes is that designation into the program happened at the census tract level, while our job postings data is aggregated at the zip code level. We tackle this issue by estimating the propensity score using a two-step process. First, we estimate a propensity score for each census tract based on ACS data and based on the political party of the local representatives. Then we use this score at the census tract level to calculate a raw propensity score for each zip code, as described in Appendix section A.2. Second, we use this zip code level propensity score in combination with the pre-treatment job postings recorded by BG in 2016, pre-treatment job posting growth in 2016 and 2015, the zip code's population, the zip code's share of area that falls within an eligible tract, and the zip code's share of population that falls within an eligible tract to estimate the final propensity score for each zip code in our dataset. This last estimation uses a logistic regression model to predict whether each zip code will have at least one designated tract. We then match zip codes with OZs to similar zip codes without OZs that are located in the same state using this final propensity score. We repeat this procedure independently for each state, to account for state-level differences across states in how OZs were designated.

Finally, we compare the treated and the matched-control zip codes before and after the OZ designation using the following difference-in-differences specification:

$$Y_{jt} = \alpha_0 + \sigma_j + \tau_t + \theta T_j \times I[t \geq 2019] + \epsilon_{jt}, \tag{1}$$

where Y_{jt} is job postings in zip code j in month t. The parameter θ represents the average treatment effect: the effect of OZs on job postings between January 2019 and March 2020. Our sample includes the first quarter of 2020 but excludes data after March 2020 to avoid the potentially confounding effect of the COVID-19 pandemic (according to Forsythe et al. (2020) job postings fell by 44% between March 15 and April 26 2020). We consider the OZ treatment to have occurred throughout 2018 because there is no date within 2018 that we can unambiguously label as the treatment event. For example, in February 2018 the IRS first published the guidance for designating OZs, while in October 2018 the Treasury released the first set of regulations. We therefore exclude all of 2018 (though as we show in the Appendix, our results are robust to including 2018). The variable I in Equation 1 denotes the indicator function. The parameters σ_j and τ_t represent zip code fixed effects and year-month fixed effects, respectively.

Given that job postings are count data, we estimate the effect of OZs on postings using a Poisson regression model with robust standard errors. State governments could select up to 25% of their eligible tracts, or 25 tracts if there were fewer than 100 eligible tracts in the state. The data on OZs designation indicate that states nominated the maximum number of allowed tracts and that the federal government approved almost all proposed tracts. In states with more than 100 eligible tracts, the average share of eligible tracts designated as OZs was 22.5%, with a standard deviation of less than 1% across states (25% was the maximum). In contrast, tracts in smaller states were more likely to be included in the program because the cap on OZs was a larger fraction of the number of eligible tracts. This fact has implications for calculating standard errors. Following Abadie et al. (2017) and given that the designation probability was the same for all large states but different for small states, we take the view that treatment assignment is clustered at the state level when estimating the average treatment effect for all states. Because clustering at the state level can produce larger standard errors, we also report results clustering at zip code level for all states and for large states only. The reason to conduct the latter exercise is that for large states the probability of treatment was the same so we can cluster the errors at the zip code level.

4 Results

We start by documenting that OZs differ from other eligible low-income census tracts. Table 1 model (1) shows that eligible tracts that qualified through the poverty route were more likely to be designated as OZs than those that qualified through the income route (a tract can qualify through either route). The estimates from model (2) in Table 1 show that urban tracts were 8% less likely to be designated into the program, after controlling for demographic characteristics. Population and poverty are associated with higher chances of being designated: 1% higher population is associated with 2% higher chance of being designated as an OZ, after controlling for other demographic characteristics. Relatively higher income OZs were less likely to be selected. Racial composition also influences designation. Higher percent Black is associated with higher probability of designation (percent White is the excluded category), whereas percent Asian and percent Hispanic are associated with a lower probability of designation. Model (3) adds income growth and population growth to the specification. Tracts with higher year-over-year income growth were more likely to be designated as OZs. Our last specification, model (4), shows estimates in line with the results reported by Frank et al. (2020). Census tracts with a representative of the same party as the state governor were more likely to be designated into the program. Interestingly, this correlation is strongest when the same political party is Republican. Note that the results in Table 1 are computed at the census tract level. They do not include pre-treatment job posting growth because that variable is only available at the zip code level. Thus, we include pre-treatment job-posting growth when we estimate the propensity score at the zip code level as explained in Appendix section A.2. In Tables A.2 and A.3 of the Appendix, we compare characteristics between treated and non-treated by eligible and between treated and matched controls, respectively, to illustrate the importance of matching. Before identifying a proper control group through matching, eligible zip codes are different from treated zip codes in every characteristic used in the computation of the propensity score. After matching, treatment and control zip codes differ nominally in four characteristics.

The top panel in Figure 1 shows the average natural logarithm of the number of new monthly job postings for zip codes with OZs (blue dots), for the matched control group (orange triangles), and for all zip codes with an eligible census tract (green squares). The gap between OZs and eligible zip codes and the correlations reported in Table 1 suggest that OZ designation was not random and illustrate the importance of a matched control group. The bottom panel of Figure 1 shows that the difference between OZs and matched controls (blue dots) is rather flat and hovers slightly below zero before 2018. Differences between matched and control did not systematically increase or decrease before the OZs treatment, suggesting pre-treatment parallel trends. From 2019 on, the difference did not seem to shift trend, anticipating a small effect of OZs, if any. We also plot the difference between OZs and zip codes with qualified tracts (orange squares) to further illustrate the role of matching in identifying an effect. One could argue the existence of a visually decreasing trend, suggesting that zip codes with OZs were becoming more similar to zip codes with not-selected eligible tracts even before treatment. The shaded gray area corresponds to 2018, which is the period when the program guidelines rolled out. We remove the 2018 observations from our main analysis to provide a sharper contrast between pre- and post-treatment periods.

Table 2 reports our difference-in-differences estimates from Equation 1 over the matched sample. The coefficient of OZ $\times$ post 2019 in model (1) indicates that job postings increased in zip codes with OZs by about 5% faster than in matched zip codes. A 5% effect may seem high, but compared to the mean of about 15

	Selection into the program (census tract level)			
	(1)	(2)	(3)	(4)
income-qualified	0.102***			
	(0.00769)			
poverty-qualified	0.159***			
	(0.00545)			
is-urban		-0.0809***	-0.0828***	-0.0800***
		(0.00626)	(0.00628)	(0.00635)
population		0.0228***	0.0234***	0.0226***
		(0.00492)	(0.00508)	(0.00510)
poverty		0.726***	0.730***	0.736***
		(0.0247)	(0.0252)	(0.0253)
income-ratio		-0.159***	-0.162***	-0.161***
		(0.0173)	(0.0177)	(0.0177)
percent-black		0.0743***	0.0716***	0.0885***
		(0.0115)	(0.0115)	(0.0124)
percent-asian		-0.200***	-0.199***	-0.203***
		(0.0319)	(0.0320)	(0.0324)
percent-other-non-white		0.0595*	0.0590*	0.0519*
		(0.0245)	(0.0245)	(0.0247)
percent-hispanic		-0.100***	-0.101***	-0.0887***
		(0.0151)	(0.0151)	(0.0157)
bachelors or higher		-0.0117	-0.0110	-0.00537
		(0.0240)	(0.0243)	(0.0246)
income growth (YoY)			0.0797***	0.0765***
			(0.0211)	(0.0212)
pop growth (YoY)			0.0692	0.0696
			(0.0472)	(0.0473)
same-party (dem)				0.0209*
				(0.00926)
same-party (rep)				0.0366***
				(0.00704)
State fe	Yes	Yes	Yes	Yes
N	32994	32803	32680	32501
adj. R^2	0.028	0.070	0.071	0.072
AIC	35736.8	34045.4	33878.3	33665.0

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1: Linear probability model of selection into the program. All models include state fixed effects and are at the census tract level. The variables income-qualified and poverty-qualified are one when the tract was qualified through the income route and the poverty route, respectively. The variable income-ratio refers to the ratio between the tract's median family income and the area median family income. This variable accounts for differences in purchasing power and was the variable used to qualify tracts through the income route. Finally, we add a dummy variable for when at least one of the state representatives of the census tract belonged to the same party as the state governor. Note: The probability model presented does not include pre-treatment job posting growth because that pre-treatment variable enters in the aggregation of probability from census tract to zip code as described in Appendix A.2. We remove Nebraska as its legislature only has a senate.

new job postings per month (see, e.g., Figure 1 where the average of the natural logarithm of monthly new job postings is 2.7), it is a small effect of about 1 new posting per month. This coefficient, however, is not

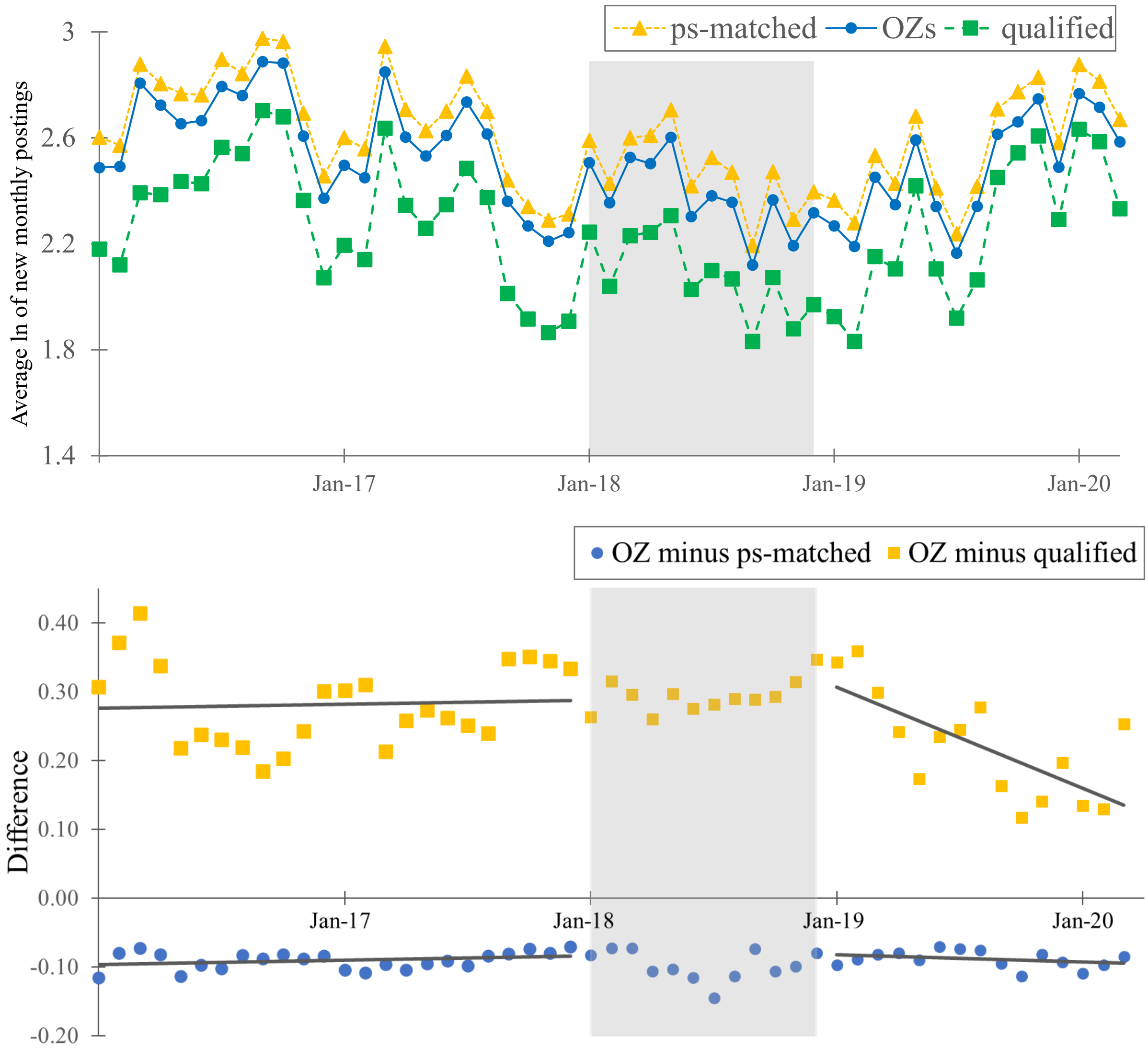

Figure 1: Job postings from January 2016 to March 2020 comparing OZs, control group using propensity score matching (p-score matched), and non-OZ low-income communities (all non-OZ LIC). Shaded gray area corresponds to the policy implementation period. The bottom sub-figure shows linear trend-lines before 2018 and after 2018.

significantly different from zero at conventional levels. Recall that standard errors are clustered at the state level to account for the fact that treatment was decided at the state level, and that there were differences in the chance of being treated between large and small states. Thus, we can rule out effects larger than 2 new job postings per month with 95% confidence.[12] In model (2) we cluster errors at the zip code level. Clustering errors at the zip code level increases the precision of the estimate, but standard errors remain high. The effect is non-significant at conventional levels. Model (3) focuses on large states, defined as states that have more than 100 eligible census tracts. Across these states, the fraction of selected tracts was fairly even, so we cluster standard errors at the zip code level per Abadie et al. (2017). The magnitude of the coefficient is about the same and the standard error is smaller, but not small enough to become significant at conventional levels. The

[12]We compute the upper bound of the 95% confident interval for the coefficient given the data from the table. That is about 14% (4.7% + 1.96*4.6%). Then we take 14% of the mean number of new job postings per month per zip code (15 new postings), which is about 2 postings.

	Number of new monthly job postings per zip code					
	(1)	(2)	(3)	(4)	(5)	(6)
OZ × post 2019	0.0472	0.0472	0.0459			
	(0.0457)	(0.0311)	(0.0312)			
OZ × 2019-Q1				0.0349	0.0343	-0.0116
				(0.0462)	(0.0599)	(0.0889)
OZ × 2019-Q2				0.0693	0.0761	0.0206
				(0.0514)	(0.0713)	(0.112)
OZ × 2019-Q3				0.0217	0.00740	-0.160
				(0.0474)	(0.0631)	(0.125)
OZ × 2019-Q4				0.0565	0.0250	-0.0450
				(0.0476)	(0.0623)	(0.142)
OZ × 2020-Q1				0.0521	0.0418	-0.0942
				(0.0479)	(0.0666)	(0.124)
Year-month fe	Yes	Yes	Yes	Yes	Yes	Yes
Zipcode fe	Yes	Yes	Yes	Yes	Yes	Yes
Error clustering	State	Zip code	Zip code	State	State	State
Sample desc.	All	All	Large states	All	Const. & R.E.	With EIG Inv.
N	570588	570588	554982	570588	410499	6834
AIC	9502753.4	9502763.4	9226449.3	9501053.8	1070818.2	153888.1

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2: Difference-in-differences estimate of the effect of OZs on monthly job postings. All regressions exclude 2018 and include year-month and zip code fixed effects. Model (1) considers all states and clusters errors at state level. Model (2) considers all states and clusters standard errors at zip code level. Model (3) focuses on large states and clustering at zip code level. Model (4) replicates the specification in model (1) but presents effects by year-quarter. Model (5) also presents effects by year-quarter and standard errors clustered at state level but considers only the Construction and Real Estate industry. Model (6) replicates the specification in model (5) but over the sample of confirmed investments according to the Economic Innovation Group (EIG). We follow Abadie et al. (2017) for error clustering criteria.

small and not significant results in models (1)-(3) are consistent with the minuscule and non-significant effect of OZs on employment rates reported in Freedman et al. (2021) Table 2 model (1).

Given these null results, we expand Equation 1 to include quarter-year dummies, instead of one post-treatment dummy. If the effect exists but it has taken some time to show up, it may be obscured by the post-2019 dummy. Model (4) shows no evidence of the effect increasing as time goes by. The largest coefficients correspond to Q2 2019 and Q4 2019, 6.9% and 5.7% respectively. However, these estimates fail to reject the null of zero effect even at a five percent confidence level. Perhaps averaging across industries hides the effect of OZs on the industry that was supposed to be the most sensitive to the program in the short run: Construction and Real Estate. We therefore next present results for Construction and Real Estate as we should expect that early effects, if any, would appear in those industries. Model (5) suggests that this is not the case. Despite featuring coefficients about 7.6% in Q2 2019 and 4.2% in Q1 2020, the standard errors are large. In addition, there is no evidence that the coefficients increase in value over time. Finally, in model (6) we present results for zip codes with confirmed investments according to the Economic Innovation Group (EIG).[13] We do not find

[13]We obtain the data on confirmed investments by screen scraping the data from EIG's website eig.org. According to their website, EIG collects and reports data on investments in OZs that were financed using Qualified Opportunity Funds. They use information from news reports, press releases and other sources to ensure that the projects are actively

evidence of an effect of OZ designation on job postings in those zip codes either.

Table 2 provides no evidence of a significant average treatment effect of OZs on job postings. In Appendix Table A.4 we present a variety of additional results: using different calipers in the matching procedure, disregarding matching at all, including the year 2018 while using Q3 2018 as the start of the post-treatment period, as well as considering only zip codes that house more than 90% of their population in a treated census tract, considering only zip codes in which a treated census tract covers more than 90% of the land area. Each estimate in these robustness exercises remains non-significant. We also include a specification over the larger sample that adds 2018, but now with quarter-by-quarter dummies until Q1 2020. We fail to reject the null that each coefficient is equal to zero at conventional levels.

Table 3 presents results split by demographic characteristics. Model (1) shows results on rural zip codes and model (2) on urban zip codes. In model (1), the estimate is negative and statistically significant at conventional levels. In model (2) the coefficient is positive and significant.[14] Taking these results at face value, rural zip codes with OZs see about 26% less job postings after the OZs program implementation than before, and urban zip codes with OZs, see about 17% more job postings. These numbers may appear large in magnitude, but to put them in perspective, recall the mean number of new postings per zip code is about 15. A -26% coefficient translates into about 4 fewer new postings after the program than before and a 17% percent translates into only about 3 additional new postings per month after the program on average. Model (3) shows the results of estimating the treatment effect on zip codes with below median Black population and model (4) on zip codes with above median Black population. The coefficient in model (3) is negative and non-significant, while the coefficient in model (4) is positive and barely significant at conventional levels. But even in this case, we can discard effects over 3 new job postings per month per zip code. Model (5) replicates the main specification but now on zip codes with below median Hispanic population and model (6) on zip codes with above median Hispanic population. In model (5), the coefficient is negative but not significant. In model (6), the coefficient is about 9% but not significant. We can rule out effects over 3 new job postings per month with 95% confidence here as well. In Table A.5 in the Appendix we provide additional groupings for rural/urban and Black populations. Model (1) and (2) feature rural and urban zip codes, respectively, only considering Construction and Real Estate industry firms (C&RE). The effect in rural C&RE communities is negative and significant at one percent level. Job postings increase for urban C&RE firms and the magnitude of such an increase is about the same as the one for all urban firms in Table 3. These results provide further evidence that if there is a positive effect of the OZ program, it occurs in urban areas. But even in those areas, the economic magnitude of the OZs effect on the number of job postings is small. The remaining groupings, below-median and above-median Black in C&RE firms and below-median and above-median Black in OZs tracts that cover more than 90% of the zip code area, yield non-significant results.

We also explore heterogeneous effects by state. Figure 2 shows the post-OZ designation treatment effects for each state in our sample. The table with the coefficients and their corresponding standard errors is in

underway, rather than just in planning stages, and to confirm that they are funded using OZ capital. EIG reports the location of commercial, residential, and mixed use real estate developments that are either under construction or actively operating as well as actively operating businesses. While EIG does not claim that their list is exhaustive, they do claim to provide a source of confirmed investments in opportunity zones.

[14] Note that there are almost twice as many rural zip codes than urban zip codes in our sample. This is because we use zip codes instead of census tracts. Census tracts are primarily determined by population size, whereas zip codes are primarily determined by mail delivery efficiency. As a result, urban zip codes tend to have more tracts inside them than do rural zip codes. We do the split sample based on whether the zip code is designated as 'urban' or 'rural'. We consider a zip code to be urban if most of its population is in urban census tracts, and rural otherwise.

	Number of monthly new job postings per zip code					
	(1)	(2)	(3)	(4)	(5)	(6)
OZ x post 2019	-0.255***	0.170***	-0.0623	0.108*	-0.0523	0.0877
	(0.0527)	(0.0394)	(0.0702)	(0.0467)	(0.0706)	(0.0530)
Year-month fe	Yes	Yes	Yes	Yes	Yes	Yes
Zipcode fe	Yes	Yes	Yes	Yes	Yes	Yes
Errors cluster	State	State	State	State	State	State
Sample desc.	rural	urban	low-Black	high-Black	low-Hisp.	high-Hisp.
N	395148	175440	305592	264078	298962	270708
AIC	4967281.6	4337587.9	4417375.4	5012362.8	4161406.5	5239249.3

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, ***, $p < 0.001$

Table 3: Difference-in-differences estimates of the effect of Opportunity Zones on job postings, for different zip code groupings: rural, urban, below median percent of Black population, above median percent of Black population, below median percent of Hispanic population, and above median percent of Hispanic population. All regressions exclude 2018, include year-month and zip code fixed effects, and cluster standard errors at the state level.

the Appendix, Table A.6, using the Bonferroni correction to account for false positives while testing multiple hypotheses simultaneously. Although these estimates should be carefully interpreted, they suggest that the OZs program may have resulted in job posting increases in some states, such as Nevada, Louisiana, and North Carolina among others, whereas other states suggest that the OZs program may have resulted in job posting decreases. But even in those states in which we find large percent effects, the increase in the number of new job postings is not remarkable. Take Nevada, for example. The coefficient estimate is 88% (the average number of new job postings per zip code per month in Nevada is about 14) and the standard error estimate is very small, yet we can discard an effect above 12 new postings per month per zip code with 95% confidence.

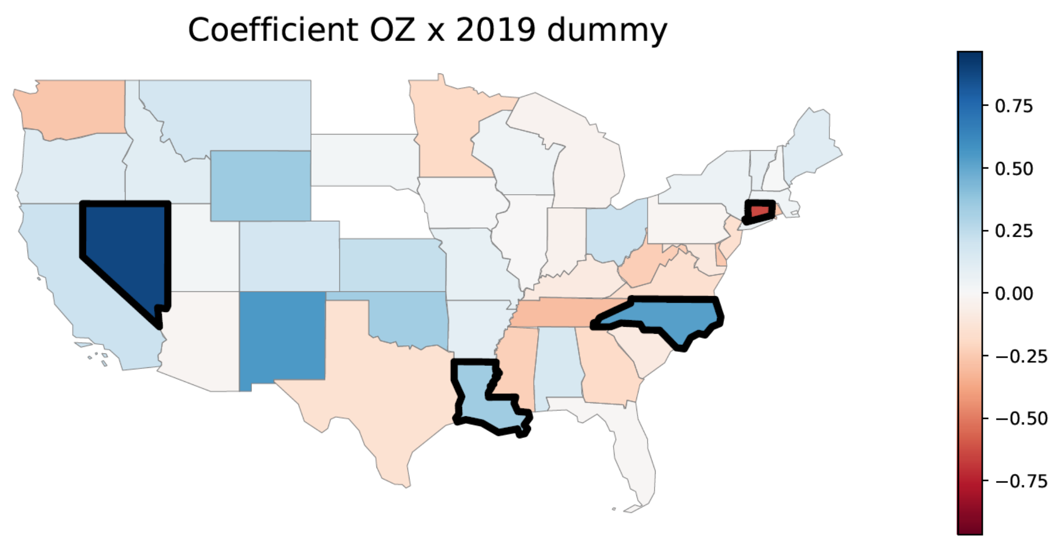

Figure 2: Effect of OZs on job postings by state. Line thickness indicates significance level calculated using Bonferroni correction.

5 Conclusion

The 2017 Tax Cuts and Jobs Act Opportunity Zones (OZs) are a new place-based program designed to bolster economic growth in distressed communities by stimulating investment. We study the effect of the OZ program on job postings, a high frequency, forward looking measure that captures a firm's plans to hire (Forsythe et al. 2020; Gutiérrez et al. 2020). Given that the OZs program implementation began only in 2018, the high-frequency, forward-looking nature of job postings provides us an opportunity to identify an effect of OZs before the onset of COVID-19. We match zip codes with low-income OZs to a control group of similar zip codes that have no OZs and compare changes in job postings between these two groups before and after the policy treatment. We do not find evidence that zip codes with OZs have more job postings than comparable non-OZ zip codes over the whole sample. Neither do we find evidence of an effect in areas with confirmed investment nor in construction or real estate industries. We do find, however, some evidence of heterogeneous effects of OZs on job postings by population density as well as across states. But the economic magnitude of those effects is small. Given the mixed set of results, we believe a prudent interpretation is that the OZs program has so far had a limited effect on job postings.

We want to highlight some limitations and opportunities for future research. First, even though job postings data are forward-looking, the COVID-19 disruption precludes us from ruling out that the OZs program would have larger effects if studied over a longer time frame. Second, we focus on low-income census tracts and exclude the small number of adjacent non-low-income communities also designated as part of the OZ program. We do so to assess the efficacy of the program's stated goal: to uplift distressed communities. But, a holistic study of the OZs program may also consider effects in these non-low-income communities. Third, we rely on the accuracy of job postings data. The Burning Glass (BG) dataset is relatively new. While the data have been used in a number of papers, and while these papers often include some validation of the data by comparing it to JOLTS or other measure of job openings, the most comprehensive of which appears to be Hershbein and Kahn (2018), we are not aware of any research paper that is devoted to validation of the BG dataset. Such validation would include describing the benefits of the data and also its limitations (for example, see the paper by Neumark et al. (2007) which validates the NETS dataset). In addition, further validation would address the accuracy of the BG data at different levels of geography.[15] There is room for more testing and verification of job postings, which is a task we leave for future research. Fourth, more research is needed to understand the link between job postings and other employment outcomes. For example, as reported in the appendix (Table A.1), in most cases we find a positive correlation between job postings and employment as measured by the QCEW and ZBP datasets, but in one case we find a negative correlation between job postings and annual changes in ZBP employment. We conjecture this may be due to employment changes being the result of new hires and separations, whereas job postings measure anticipated hiring, not separations, and so the relationship between job postings and employment changes may break down over longer periods of time, though further research is warranted. Fifth, another avenue for future research can be to study comprehensive data on state policies that could complement the OZs program. The potential for combined or crowding-out effects is an interesting area beyond the scope of our paper.

[15] BG data do not provide a field for "census tract." Instead, it provides fields for "latitude-longitude of canonicalized location," zip code, and higher levels of geography. These are based off of the data from the underlying job posting. While in principle the latitude-longitude data could be used to identify the census tract, it is unclear how BG determines the latitude-longitude. We have not seen any other paper that uses BG data at this level of granularity.

While much work remains to evaluate the efficacy of the Opportunity Zones program and its effects on economic outcomes, one implication of the results thus far is that despite the overall null effect, it seems that some distressed communities and some states benefited from the program. This suggests that the OZ program might be better understood as a complement to other local development efforts instead of a homogeneous treatment across the board. An important question for future research is whether that benefit is cost effective. A fuller understanding of the trade-offs of the OZs program will be important in assessing the efficacy of supply-side oriented place-based policies. Efforts by the federal government are in place to track the volume of investment dollars committed to Qualified Opportunity Funds (the financial vehicles through which investors can take advantage of the OZ program) and which types of projects are being implemented in OZs. Releasing up-to-date data about OZs investments will allow researchers and policymakers to assess the efficacy of the program. This recommendation is echoed in a recent report by the U.S. Government Accountability Office (GAO, 2020).

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A Appendix

A.1 Comparing Burning Glass with QCEW and ZBP

In this section we compare the number of job postings reported in Burning Glass with employment level reported in the Quarterly Census of Employment and Wages (QCEW) and with yearly employment level reported in ZIP Codes Business Patterns (ZBP). The QCEW reports quarterly data at the county level, which allows us to compare the quarterly Burning Glass data we use in the main text with official quarterly data. However, QCEW only reports county-level data. On the other hand, ZBP reports yearly data at the Zip Code Tabulation Area level (ZCTA) and is only available until 2018.

	QCEW (unit: county-quarter)			ZBP (unit: zip code-year)		
			I. Employment			
	(1)	(2)	(3)	(4)	(5)	(6)
Number of job postings	47.03***	47.09***	3.568***	0.328***	0.328***	0.0131***
	(0.0799)	(0.0798)	(0.0227)	(0.00220)	(0.00221)	(0.000523)
Quarter fe	No	Yes	Yes			
County fe	No	No	Yes			
Year fe				No	Yes	Yes
Zip code fe				No	No	Yes
N	70425	70425	70425	357587	357587	357587
R^2	0.83	0.83	0.99	0.058	0.059	0.048
			II. Forward Employment			
	(1)	(2)	(3)	(4)	(5)	(6)
Number of job postings	48.24***	48.31***	3.485***	0.323***	0.323***	0.0141***
	(0.0832)	(0.0830)	(0.0224)	(0.00225)	(0.00225)	(0.000518)
Quarter fe	No	Yes	Yes			
County fe	No	No	Yes			
Year fe				No	Yes	Yes
Zip code fe				No	No	Yes
N	67222	67222	67222	324691	324691	324691
R^2	0.83	0.83	0.99	0.060	0.060	0.046
			III. Employment Changes			
	(1)	(2)	(3)	(4)	(5)	(6)
Number of job postings	0.284***	0.285***	0.155***	0.00424***	0.00428***	-0.00260***
	(0.00335)	(0.00327)	(0.0121)	(0.000246)	(0.000246)	(0.000422)
Quarter fe	No	Yes	Yes			
County fe	No	No	Yes			
Year fe				No	Yes	Yes
Zip code fe				No	No	Yes
N	67222	67222	67222	324691	324691	324691
R^2	0.097	0.139	0.119	0.001	0.002	0.002

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.1: Comparison between: I. Employment and job postings, II. Forward employment and job postings, and III. Changes in employment and job postings. Models (1)-(3) compare quarterly employment (levels in subtables I. and II., changes in subtable III.) in each county (QCEW) with quarterly job postings in each county (BG). Models (4)-(6) compare yearly employment (levels in subtables I. and II., changes in subtable III.) at zip code level with yearly job postings in each zip code. All models are panel data linear regressions.

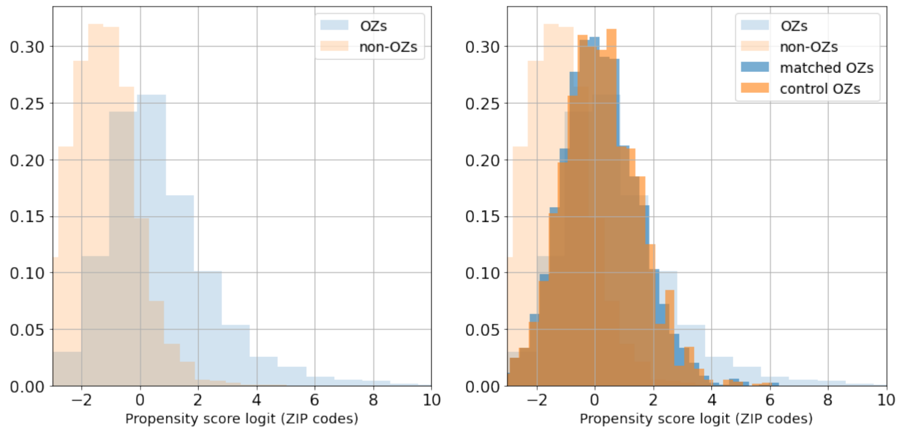

Figure A.1: Propensity scores for each zip code. Left panel shows the estimated propensity score for zip codes with and without opportunity zones. While there is overlap, there is also a considerable distributional difference between the two groups. The right panel highlights the result of the matching. All propensity score values are logit-transformed $\left(\frac{R_j}{1-R_j}\right)$

A.2 Estimation propensity score

We consider the following variables from the American Community Survey to estimate propensity scores: population and population squared (B01003), poverty rate (B17001), median family income (B19113), population year over year growth rate, median family income year over year growth rate, percent Black and percent White (B02001), percent with high-school degree or higher and percent with bachelors degree or higher (S1501), and dummies for urban tracts, for partisan identity of the governor, and for whether the tract qualified through the income route (income less than 80% of the area income), through the poverty route (poverty rate of 20% or higher), or both.[16] We also add two dummies on whether the qualification was within the ACS measurement error for each variable. These are the key variables used to assess eligibility, or they were recognized by policy-makers as being important.[17] We restrict our observations to only census tracts that qualified for the program through the poverty or the income route.[18]

We define treated zip codes as those zip codes with at least one census tract designated as an OZ. With the treatment defined this way, we use the propensity score of each eligible tract inside each zip code to calculate the propensity that the zip code gets at least one OZ. If r_i is the propensity score of census tract i (probability of i being selected into the program), then the implied propensity score R_j of zip code j that contains i is:

$$\tilde{r}_j = 1 - \prod_{i \in j}(1 - r_i). \tag{2}$$

Finally, we estimate a propensity score for zip code j by regressing the treatment of j (whether there is at least one OZ in j) on $\tilde{r}_j$, on the number of job postings recorded in j for 2016, and on the growth in job

[16]We also include the ratio between the median family income of the Census Tract and the median family income of the area (metropolitan statistical area for urban tracts and county for rural tracts), as this ratio was one of the selection criteria.

[17]For example, the governor of Texas declared that race was an important consideration when proposing Opportunity Zones to the federal government.

[18]Some census tracts qualified as opportunity zones because they were contiguous to a low-income tract (less than 3% of OZs qualified through this route). We do not consider these tracts because they usually feature better demographic characteristics such as higher incomes.

postings between 2015 and 2016. We run each regression by state.[19] We use this final propensity score R_j to build the matched control group. Figure A.1 shows the distribution of the propensity score for zip codes with and without OZs. We observe the desirable property that the empirical distributions of treatment (matched) and control OZs overlap.

For each treated unit j, we restrict the matched unit to be inside the same state and find the untreated unit j' such that the absolute difference between their propensity score is the smallest possible, restricted to a minimum difference of 0.3.[20] This leaves us with a total of 6,627 matched pairs.

[19]We use the logit transformed $\frac{\tilde{r}_j}{1-\tilde{r}_j}$.

[20]In practice, we use the distance between the logit of the propensity score to account for distributional issues $\frac{R_j}{1-R_j}$.

A.3 Additional Tables

	Treatment				Qualified				t-test	diff. in
	count	mean	std	median	count	mean	std	median		means
monthly job vacancies 2016 (log)	6411	2.886	1.912	2.708	10422	1.872	1.612	1.386	36.896***	1.014
first stage propensity	6411	0.729	2.060	0.390	10422	-1.390	1.293	-1.360	82.021***	2.119
population (log)	6411	9.020	1.565	9.408	10422	8.034	1.680	7.967	37.955***	0.986
poverty rate	6388	0.203	0.102	0.187	10385	0.165	0.102	0.147	23.417***	0.038
median family income (log)	6269	10.866	0.302	10.871	9963	10.946	0.321	10.936	-15.748***	-0.080
family income YoY growth	6232	0.033	0.093	0.026	9795	0.030	0.113	0.021	1.997*	0.003
percent black	6398	0.143	0.202	0.048	10405	0.078	0.152	0.011	23.689***	0.065
percent white	6398	0.755	0.229	0.827	10405	0.845	0.191	0.924	-27.467***	-0.090
percent pop qualified	6411	0.717	0.303	0.805	10422	0.666	0.367	0.834	9.315***	0.051
percent area qualified	6411	0.721	0.299	0.812	10422	0.675	0.362	0.855	8.555***	0.046

Table A.2: Balance table comparing zip codes to OZs with zip codes with non-selected eligible tracts.

	Matched treatment				Matched control				t-test	diff. in
	count	mean	std	median	count	mean	std	median		means
monthly job vacancies 2016 (log)	5594	2.708	1.801	2.539	5594	2.795	1.880	2.615	-2.500*	-0.087
first stage propensity	5594	0.210	1.312	0.137	5594	0.204	1.303	0.135	0.255	0.006
population (log)	5594	8.861	1.564	9.191	5594	8.920	1.594	9.201	-1.987*	-0.059
poverty rate	5575	0.193	0.098	0.178	5576	0.186	0.092	0.176	3.925***	0.007
median family income (log)	5460	10.889	0.292	10.889	5467	10.915	0.296	10.903	-4.554***	-0.026
family income YoY growth	5424	0.033	0.098	0.025	5431	0.031	0.097	0.023	1.280	0.002
percent black	5585	0.124	0.185	0.039	5585	0.121	0.179	0.038	0.959	0.003
percent white	5585	0.780	0.214	0.852	5585	0.778	0.210	0.846	0.633	0.003
percent pop qualified	5594	0.702	0.313	0.786	5594	0.696	0.316	0.788	1.111	0.007
percent area qualified	5594	0.707	0.309	0.789	5594	0.700	0.313	0.796	1.302	0.008

Table A.3: Balance table comparing zip codes to OZs their matched control units. Only job postings 2016 (log), first stage propensity, and population (log) were directly used for the matching. For all other variables, we used their version at the census tract level to construct the first stage propensity.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
OZ x post 2019	0.0257	0.0429	-0.00761	0.101	0.103		
	(0.0451)	(0.0459)	(0.0276)	(0.0652)	(0.0578)		
OZ x post 2018-Q3						0.0476	
						(0.0436)	
OZ x 2018-Q3							0.0409
							(0.0454)
OZ x 2018-Q4							0.0843
							(0.0571)
OZ x 2019-Q1							0.0304
							(0.0437)
OZ x 2019-Q2							0.0649
							(0.0487)
OZ x 2019-Q3							0.0173
							(0.0447)
OZ x 2019-Q4							0.0521
							(0.0448)
OZ x 2020-Q1							0.0477
							(0.0451)
Year-month fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Zipcode fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Errors clustering	State	State	State	State	State	State	State
Sample desc.	Low caliper	High caliper	No matching	Large OZ population	Large OZ area	with 2018	with 2018
N	458388	613020	835278	124338	126174	704844	704844
AIC	6827915.2	10829614.1	11802627.6	1660178.1	1691674.1	11660949.5	11657628.4

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.4: Robustness checks. Models (1) and (2) present the results from using low and high calipers (respectively) in the matching procedure. Model (3) disregards matching at all. Model (4) considers only zip codes that house more than 90% of their population in a treated census tract and model (5) only zip codes in which a treated census tract covers more than 90% of the land area. Model (6) includes the year 2018 while using Q3 2018 as the start of the post-treatment period. Model (7) also uses the Q3 2018 starting point, but includes quarter-by-quarter dummies until Q1 2020.

	Number of new job postings					
	(1)	(2)	(3)	(4)	(5)	(6)
OZ x post 2019	-0.382***	0.195***	-0.101	0.119	-0.131	-0.0727
	(0.0697)	(0.0669)	(0.0896)	(0.0687)	(0.289)	(0.132)
Year-month fe	Yes	Yes	Yes	Yes	Yes	Yes
Zipcode fe	Yes	Yes	Yes	Yes	Yes	Yes
Errors cluster	State	State	State	State	State	State
Sample desc.	rural C&RE	urban C&RE	low-Black C&RE	high-Black C&RE	low-Black largeOZ-area	high-Black largeOZ-area
N	301767	145044	229296	216903	77928	43554
AIC	615458.1	529155.4	534736.5	615161.0	787677.4	886374.1

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, ***, $p < 0.001$

Table A.5: Additional groupings of zip codes. Models (1) and (2) present the results from using rural and urban sub-samples for the Construction and Real Estate industry (C&RE). Models (3) and (4) considers low and high percent of Black population for the C&RE industry. Models (5) and (6) also show low and high percent of Black population but consider only OZs that highly overlap with zip code area (90% overlap).

Full results by state

Name	OZ x post 2019	s.e.
Alabama	0.164*	(0.050)
Arizona	-0.0173	(0.859)
Arkansas	0.0919	(0.514)
California	0.206**	(0.003)
Colorado	0.179	(0.057)
Connecticut	-0.636***	(0.000)
Delaware	-0.261	(0.213)
Florida	-0.00852	(0.807)
Georgia	-0.183**	(0.002)
Idaho	0.113	(0.610)
Illinois	-0.00353	(0.966)
Indiana	-0.0447	(0.658)
Iowa	0.0146	(0.881)
Kansas	0.230	(0.159)
Kentucky	-0.0899	(0.566)
Louisiana	0.345***	(0.000)
Maine	0.118	(0.286)
Maryland	-0.0983	(0.069)
Massachusetts	0.0339	(0.460)
Michigan	-0.0331	(0.476)
Minnesota	-0.194	(0.080)
Mississippi	-0.228*	(0.047)
Missouri	0.0803	(0.531)
Montana	0.180*	(0.046)
Nevada	0.878***	(0.000)
New Hampshire	0.00520	(0.941)
New Jersey	-0.156	(0.078)
New Mexico	0.551*	(0.014)
New York	0.0545	(0.410)
North Carolina	0.535***	(0.000)
North Dakota	0.189	(0.095)
Ohio	0.206	(0.262)
Oklahoma	0.337	(0.058)
Oregon	0.121*	(0.042)
Pennsylvania	-0.0226	(0.773)
Rhode Island	-0.268	(0.494)
South Carolina	-0.0845	(0.386)
South Dakota	0.0235	(0.866)
Tennessee	-0.296*	(0.029)
Texas	-0.150*	(0.012)
Utah	0.0270	(0.821)
Vermont	0.0724	(0.740)
Virginia	-0.153	(0.313)
Washington	-0.266**	(0.004)
West Virginia	-0.238	(0.218)
Wisconsin	0.0428	(0.725)
Wyoming	0.349	(0.081)

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.6: Summary of the difference in differences estimator for job postings in each state. Although some estimates are significant at standard levels of confidence, these results are subject to the multiple testing problem so the significance needs to be interpreted carefully. Significance levels include Bonferroni correction.