What is the Impact of Opportunity Zones on Employment?

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January 26, 2021

Abstract

We use a large database on online job vacancies to study the effect of Opportunity Zones (OZs) on employment outcomes. The 2017 U.S. OZ program is a federal initiative that grants tax breaks to capital invested in selected census tracts with the goal of promoting economic development in distressed communities. We use propensity score matching to match zip codes with OZs to similar control zip codes with no OZs but with eligible census tracts. The matching procedure addresses potential bias stemming from designation based on the economic potential of the area. Using a difference-in-differences approach over the matched sample, we find no evidence of an increase in job vacancies. Posted salaries in OZs increased by about 1.5%, but this effect is not robust to specification. We find evidence of increased vacancies during the COVID-19 pandemic and heterogeneous effects by state. Taken together, our findings indicate that OZ designation has so far had limited effects overall, although it might have helped certain areas endure the negative economic effects of the COVID-19 pandemic.

Keywords— Opportunity Zones; Employment; Place-based policies; Tax policies; Covid-19.

JEL Codes— J23, J31, J38, H25, R12

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Hernández-Lagos and Seamans thank the NYUAD Center for Interacting Urban Networks (CITIES), funded by Tamkeen under the NYUAD Research Institute Award CG001 and by the Swiss Re Institute under the Quantum Cities$^{\text{TM}}$ initiative. Cristian Jara-Figueroa thanks the Media Lab Consortia for their financial support and thanks Kent Larson, Luis Alonso, and Markus ElKatsha for their early stage feedback and Linh Vo for their early data work. Declarations of interest: none.

1 Introduction

The 2017 Tax Cuts and Jobs Act established Opportunity Zones (OZ) as a way of stimulating employment and economic growth in distressed communities in the United States. The OZ program provides tax breaks to investors in census tracts designated as OZs. Approximately 10% of the U.S. population resides in one of the more than 8,000 OZs. While supporters of the program claim that investment has flowed into areas otherwise overlooked by investors, critics argue that the tax break has funded luxury apartments and hotels in neighborhoods on the upswing, benefiting mostly middle-class renters.[1] Despite the controversies, investors have already committed resources to some of these areas. According to the White House Council of Economic Advisers, as much as $75 billion may already have been invested in OZs, although the exact amount is unknown according to the U.S. Government Accountability Office (GAO (2020), page 10).

In this paper we use a large dataset covering the near universe of online job postings to evaluate the effects of the OZ program on two employment outcomes: job vacancies and posted wages. While the OZ program's full effect on employment may take more time to realize, examining its impact after two years can help identify areas in which the program improves employment outcomes and inform policymakers about what is working and what is not. One challenge in estimating the effects of the OZ program is identifying an appropriate counterfactual for the tracts that were designated as OZs. Governors of each state nominated OZs based on a variety of observable factors. We use propensity score matching to match zip codes with tracts that were ultimately designated as OZs to zip codes in the same state with similar looking tracts that were not designated as OZs. We then use data from Burning Glass Technologies (BG) to compare the number of job vacancies and their minimum posted salaries 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 OZ) using a difference-in-differences approach over the matched sample.

We find no significant difference in job vacancies between treated and control zip codes in the period between January 2019 and March 2020. However, we find that treated zip codes saw more job vacancies than their matched counterparts (approximately 15% more) during the COVID-19 crisis (i.e., after March 2020), although the effect is not robust to error clustering. In addition, treated zip codes experienced a slight increase in posted wages (approximately 1.5%), but that effect is not robust either. These same patterns hold in communities with above median Black population. Finally, we report evidence of heterogeneous effects on vacancies and wages across states. We interpret our findings to suggest that the OZ program has so far had little effect on employment outcomes in low-income communities overall, but that it might have prepared those areas to better endure the COVID-19 crisis.

Our paper contributes to a nascent research stream on the economic effects of OZs. This research includes ongoing qualitative and quantitative work by the Economic Innovation Group on residential property prices (Chen et al., 2019) and by the Urban Institute on improvements to the OZ program (Theodos et al., 2020). The paper closest to ours, Arefeva et al. (2020), uses establishment data from YTS, a private data provider, aggregated to the census tract level and finds that the OZ program increased employment growth by 2-4 percentage points. We complement Arefeva and co-authors' work with our focus on vacancies and wages, while they examine employment and establishment growth. In addition, we focus on low income census tracts and exclude the adjacent non low-income communities that were designated as part of the OZ program.

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

Our paper also contributes to recent research on the effects of COVID-19. Although OZs tax breaks were not designed to ease the burdens of the pandemic, they lift the constraints on capital investments often associated with economic downturns (Howell et al., 2020; Townsend, 2015; Nanda and Rhodes-Kropf, 2017). Tax breaks on capital are meant to spur investment, increasing labor demand in a situation in which labor supply has increased due to the pandemic (Bartik et al., 2020; Forsythe et al., 2020). By studying job vacancies and wages, we shed light on whether a positive impact of OZs on wages counters the negative effect of the pandemic on employment. Such an effect would be particularly important in low-income areas (Coibion et al., 2020; Cortes and Forsythe, 2020; Adams-Prassl et al., 2020). In sum, our paper contributes to both of these strands of literature by (1) studying the process underlying Opportunity Zones designation, (2) estimating the effects of OZ designation on real-time labor employment outcomes such as job vacancies and wages and (3) studying the extent to which the OZ program might have ameliorated the effect of the COVID-19 pandemic in the United States.

2 Opportunity Zones

While the 2017 Tax Cuts and Jobs Act established Opportunity Zones (OZs) as a federal policy, OZ were first described in a 2015 white paper by Jared Bernstein and Kevin Hassett published by the Economic Innovation Group (Bernstein and Hassett, 2015). Their idea was to address shortcomings in prior federal place-based economic development programs that targeted low income neighborhoods. The authors recommended providing incentives to investors to reinvest capital gains in specially designated areas, via dedicated funds, called Opportunity Funds. The Opportunity Funds invest in projects in one or more of the 8,700 OZs throughout the U.S. 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.

The zones were nominated by state governors, each of whom may run a different process for nominating zones, and then certified by the U.S. Treasury. 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%.[2]

In addition, the IRS code specifies that a non-LIC tract is eligible for designation as a QOZ if the "tract is contiguous with an LIC that is designated as a QOZ." 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. 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 New Jersey included high-income tracts contiguous to low-income tracts.

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. There are additional incentives for holding investments

[2] A community could also be labeled 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.

in Opportunity Funds for longer periods of time. According to the Economic Innovation Group, every $100 invested in an Opportunity Fund would see over a 10-year period a gain of $44 above the return from investing in a traditional stock portfolio.[3] 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).[4]

OZs were initially championed by a bipartisan coalition including Senators Tim Scott (R) and Cory Booker (D) and Representatives Pat Tiberi (R) and Ron Kind (D). They were included in President Trump's 2017 Tax Cuts and Jobs Act. 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. Language in the Tax Cut and Jobs Act was clear regarding eligible real estate investments, but many investors were unsure 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.

After the initial bill was passed in 2017, the Joint Committee on Taxation estimated an average annual cost for the Opportunity Zone Program of approximately $1.5 billion.[5] 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.[6] President Trump signed an executive order in 2018 creating the White House Opportunity and Revitalization Council in order to "better coordinate Federal economic development resources in Opportunity Zones and other distressed communities."[7] This body identified more than 200 federal grants and programs that will explicitly encourage or prioritize projects located in OZs.[8]

3 Data and Methods

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

Data on job vacancies from January 2015 to September 2020 come from Burning Glass Technologies (BG). We aggregate job vacancies at the monthly and zip code levels using the address of the company associated with each job posting.[9] The Burning Glass dataset has the near universe of jobs that were posted online from 2010 through the present and has been used in other academic research, including Azar et al. (2020); Burke et al. (2019); Goldfarb et al. (2020); and Kahn et al. (2020). In section A.2 of the appendix we show that BG vacancy data are highly correlated with employment data from other sources such as the Quarterly Census of

[3]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

[4]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

[5]Estimated Budget Effects Of The Conference Agreement For H.R.1, The "Tax Cuts And Jobs Act."

[6]Estimates Of Federal Tax Expenditures For Fiscal Years https://www.jct.gov/ publications.html?func=startdown&id=5238

[7]https://opportunityzones.hud.gov/thecouncil

[8]The White House Opportunity and Revitalization Council completed program targeting actions: https://www.hud.gov/sites/dfiles/documents/OppZone_Agency_Completed_Actions_2019_0808.pdf

[9]If a company has multiple addresses, Burning Glass infers the zip code of the job posting by looking at the city listed in the posting.

Employment and Wages(QCEW) and the ZIP Codes Business Patterns (ZBP). Our dependent variables are the number of job vacancies by zip code and the median of the minimum posted wage.

In addition, we use the HUD USPS zip code crosswalk files to map from census tracts to zip codes. 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 2015-2017 period and that overlapped with at least one low-income community.

Designation of OZs happened largely at the state level. OZs were proposed by state governments and certified by the federal government, with most of the proposed tracts being certified as OZs. 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. This means that the probability of treatment assignment was the same across large states and much higher in smaller states. States were likely primed to use the ACS as their core source of information because this survey was used to define eligibility and there was very little time between the final publication of the guidelines and the date by which the nominated tracts had to be submitted to the U.S. Treasury for approval. This observation motivates our matching strategy that relies largely on the ACS.

Eligibility was based on the 2015 and 2016 ACS data. 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.[10] Eligible tracts had to qualify as low-income in either the 2015 or 2016 ACS. Designation based on poverty or median income leaves room for state authorities to nominate communities that are thriving otherwise (for example urban communities with high population or income growth). If high-potential communities were nominated, then the effect of the program could be overestimated because OZ designation would be a consequence of high-potential. We argue that the matching procedure described in this section can deal with this.

Before we describe our estimation method, however, it is important to note that not all OZs were in 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 tract's median income. States could propose up to 25% of their eligible tracts for the program. If a state had fewer than 100 eligible tracts, it could propose up to 25 of them. We focus only on low-income OZs for two reasons. First, the program's aim is to improve low-income areas. Second, low-income areas are well defined, so we can match them to similar eligible tracts that were not nominated as OZs.

It is also important to note that the total number of OZs is consistent with states proposing the maximum number of allowed tracts and the federal government approving almost all of the 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. This is important because in large states (those with over 100 eligible tracts) tracts were equally likely to be designated. But if one looks at all states together, tracts in smaller states were more likely to be included in the program. This fact has implications for calculating the standard errors. When estimating the average treatment effect on all states, errors should be clustered at the state level because the OZ assignment was clustered (Abadie et al., 2017). When estimating the average treatment effect on large states only, however, errors should be clustered at the zip code level.

The U.S. federal government designated OZs by state, according to each state's proposal of eligible low-

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

income tracts based on household income or poverty rate. As such, we expect different states to prioritize different characteristics of the communities nominated, with the possibility that a nomination could be influenced by economic potential that is not captured by household income or poverty rate. To address this source of bias of treatment effects due to the nomination process, we use propensity score matching at the state level. Our identification assumption is that, conditional on a set of demographic characteristics (population, poverty rate,income, race, education, population and income growth rate, and whether the zone is urban or rural), nomination for the program is as good as random. This is justified to some extent by the fact that there was little time between the publication of the final set of guidelines and the deadline to nominate tracts. As a result, 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 that is the case. We do not have evidence that state authorities nominated low-income tracts based on unobservables such as existing yet secret investment contracts. We also find it plausible that authorities did not have time to invite investors or companies to invest in the low-income communities before official nomination of tracts because 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). Based on all this, we believe that matching on several ACS variables accounts for the potential designation bias in the estimate of the OZ program's impact on vacancies and salaries.

We follow a two-step approach to build a matched control group for the set of all zip codes with at least one OZ. The main challenge to building a matched-control group is that designation into the program happened at the census tract level, while we aggregate our data at the zip code level. To address this, our matching procedure computes the probability of being designated at the zip code level from the probability of being designated at the census tract level. The first step estimates a propensity score for each census tract based on ACS data. 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.1. The second step uses this propensity score in combination with the pre-treatment job vacancies recorded by Burning Glass in each zip code to estimate the final propensity score for each zip code in our set. We then match zip codes with OZs to similar zip codes without OZs 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.

We compare treated and matched-control zip codes using the following difference in differences specification:

$$Y_{jt} = \alpha_0 + \sigma_j + \tau_t + \theta T_j \times I[t > 2018] + \gamma T_j \times I[t \geq 2020March] + \epsilon_{jt}, \tag{1}$$

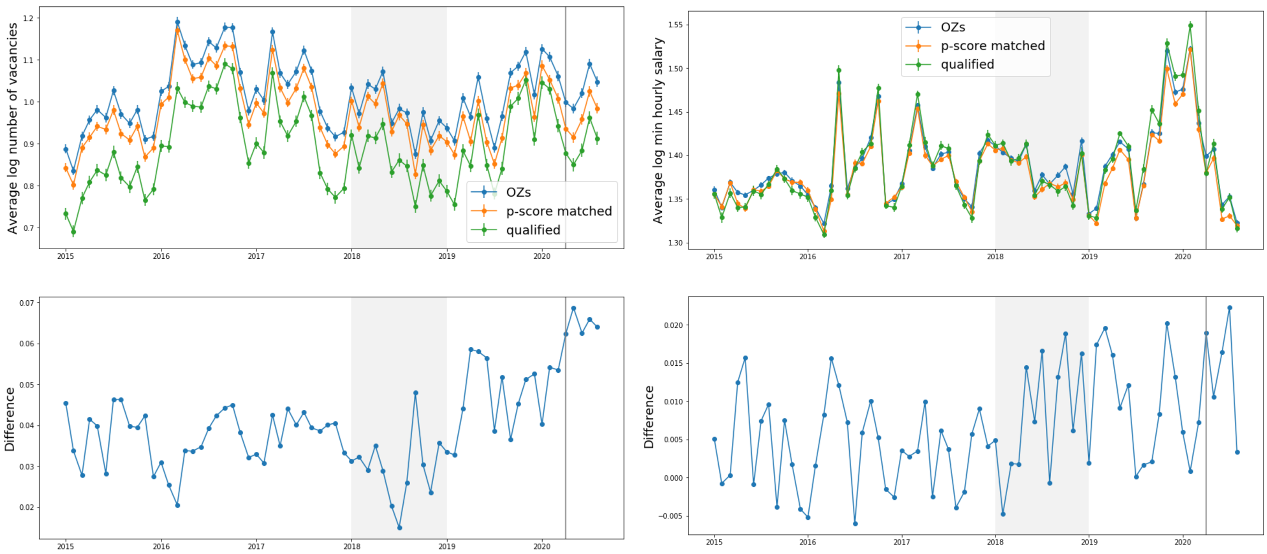
where Y_{jt} is either job vacancies or wage posted. The parameter of interest is θ, which represents the effect of OZs on employment outcomes pre-COVID-19. The parameter γ captures the differential effect of the COVID-19 pandemic on OZs' employment outcomes. We consider the OZ treatment to have taken place 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 guidance for designating OZs, while in October 2018 the Treasury released the first set of regulations. We consider the COVID-19 shock to have started in March 2020. 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 vacancies are count data, we estimate the effect of OZs on vacancies using a Poisson regression model with robust 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. This is our main specification. However, we also report estimates of the errors clustered by zip code when computing the average treatment effect for the whole sample and for large states only.

Another issue with estimating the effect of OZs on wages is that not all job vacancies report wages. As a result of this lack of information on wages, we would have to drop between 30% and 40% of zip codes in our analysis. To address this issue, we model salaries using an OLS regression corrected by a Heckman selection model.

4 Results

We start by documenting that OZs systematically differ from other eligible low-income census tracts. Table A.3 in the appendix shows that eligible tracts that qualified through the poverty route were 5% more likely to be selected as OZs than those that qualified through the income route (a tract can qualify through either route). The estimates from the second model in Table A.3 show that urban tracts were 9% less likely to be selected into the program, after controlling for demographic characteristics. Population and poverty increase the chance of being selected: a 1% increase in population translates into a 2% increase in the chance of being selected, after controlling for other demographic characteristics. The racial composition also has an influence on designation. Higher percent Black increases the probability of designation (percent White is the excluded category), whereas percent Asian and percent Hispanic decrease the probability of designation. Education negatively correlates with OZ designation. Tracts with higher year-over-year income growth were also more likely to be designated as OZs.



(a) Job vacancies.

(b) Average hourly salary of job vacancies.

Figure 1: Job vacancies and posted salaries for 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, and the gray vertical line to the start of the COVID-19 crisis.

Figure 1a shows the average number of job vacancies for zip codes with OZs (blue), for the matched control group (orange), and for all zip codes with an eligible census tract (green). The large gap between OZs and eligible zip codes, which have far fewer job vacancies, illustrates the need for a matched control group. The lower panel of figure 1a shows that the difference between OZs and control remained relatively stable and close to zero before 2018. This difference drops right before the official list of OZs was published in June 2018, and increased soon after. The shaded area covers all of 2018 and indicates that we remove 2018 from our analysis. We do this to provide a sharper contrast between pre- and post-treatment periods. In the appendix Table A.9 we show the estimates from including all the 2018 observations; the results remain qualitatively the same.

Figure 1b shows the average minimum hourly wage offered in vacancies recorded in zip codes with OZs, in matched zip codes, and in zip codes with eligible tracts. Data are noisy and differences before and after 2018 are not visually salient.

	(1) N. of Vacancies	(2) N. of Vacancies	(3) N. of Vacancies	(4) N. of Vacancies	(5) Avg. Hourly Wage	(6) Avg. Hourly Wage	(7) Avg. Hourly Wage	(8) Avg. Hourly Wage
OZ × Post 2018	0.0314	0.0314	-0.00401	-0.00401	0.0145***	0.0145*	0.0140*	0.0140
	(0.0343)	(0.0719)	(0.0325)	(0.0566)	(0.00286)	(0.00696)	(0.00545)	(0.00773)
OZ × Post-COVID			0.150***	0.150			0.00185	0.00185
			(0.0348)	(0.0925)			(0.00639)	(0.00834)
Year-month fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Zipcode fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Errors clustering	Zipcode	State	Zipcode	State	Zipcode	State	Zipcode	State
Estimation method	Poisson	Poisson	Poisson	Poisson	OLS-Heckman	OLS-Heckman	OLS-Heckman	OLS-Heckman
N	632464	632464	632464	632464	364282	364282	364282	364282
adj. R^2					0.030	0.060	0.060	0.060
AIC	12511069.9	12511051.9	12467515.2	12467495.2	371644.4	371626.4	371644.2	371626.2

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

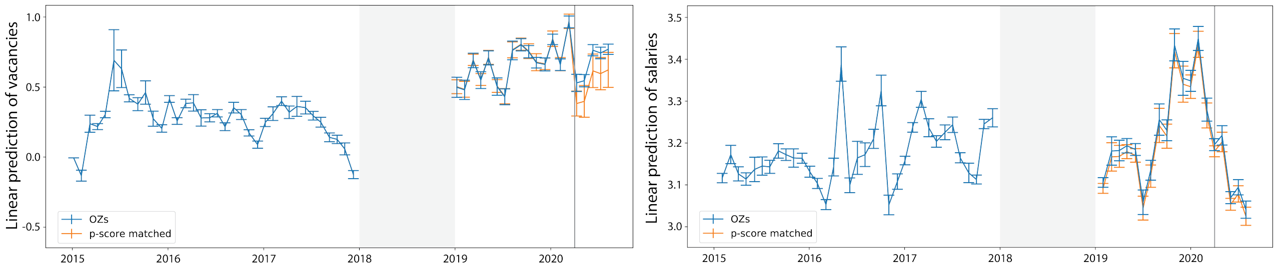
Table 1: Difference-in-differences estimate of the effect of OZs on job vacancies and wages. All regressions exclude 2018 because it was the year when the policy was being implemented. Effect on job vacancies is estimated using a Poisson regression and effect on salaries is estimated using an OLS model on log-wages with a Heckman correction to account for attrition (see Table A.10).

Table 1 reports our difference in differences estimates from Equation 1. The coefficients of OZ post 2018 in Models (columns) 1-2 show that job vacancies modestly increased in OZs by 3.1%, but the coefficients are small compared to their standard errors. Models 3-4 add an interaction between OZ zip codes and a Post-COVID indicator. The results there suggest that OZs saw a negligible drop in vacancies after 2018 and an increase in monthly vacancies during the COVID crisis of about 15%. This last result, however, is not robust to clustering the standard errors by state.

Models 5-8 in Table 1 report the estimates for the effect of the OZ program on average hourly wage of job vacancies. Although there is a small 1.5% increase after the policy was implemented, this evidence is also not very robust to clustering of the errors.

Figure 2 panels A and B present the time series linear prediction of the results of Models 4 and 8 in Table 1, respectively. For vacancies, by construction of the model the predicted values overlap before the treatment in 2018 and then diverge after 2019. The divergence becomes more pronounced after March 2020, but is still within the margin of error. The divergence in wages in panel B is less salient and also within the margin of error after 2018.

According to Abadie et al. (2017), the clustering level for standard errors should respond to clustering treatment, which is arguably the case for the whole sample of states. In appendix Tables A.7 and A.8 we show the results from estimating vacancies and wages only for those states in which there is no clustering in treatment assignment (states that have more than 100 eligible tracts). The results remain qualitatively unchanged from those in Table 1 when we account for clustering in treatment assignment this way.



(a) Job vacancies (b) Average hourly wages

Figure 2: Linear prediction of job vacancies using Poisson regression, excluding fixed effect, with errors clustered by state (left) and linear prediction of log wages using OLS with Heckman correction, excluding fixed effects, with errors clustered by state.

The results presented so far gloss over the fact that there is variation in the fraction of the population in each zip code that was treated by the program. In other words, not all treated zip codes are treated the same way. Our treatment variable is binary (zip code with at least one overlapping OZ), even though the intensity of treatment may vary depending on the fraction of people in the zip code that live in a designated census tract. We re-estimate our models after restricting our sample to those zip codes in which more than 80% of the population lives in an OZ designated tract. These results are presented in Table A.6 in the appendix. In each specification (Models 1-4), the number of job vacancies is higher in OZs after 2018. However, none of these estimates are significant. When looking at wages (Models 5-8), we see evidence of a small, yet marginally significant, effect even when estimating standard errors to account for state level clustering. Opportunity Zones do not differ significantly from the control group in either vacancies or wages after the beginning of the COVID-19 crisis. We interpret these results as suggesting that the intensity of the treatment is not driving the null results in Table 1 at least for job postings. The effect of OZs on wages is more precisely estimated though still noisy.

These results may average out heterogeneous effects. One important source of heterogeneity could be race. Table 2 shows the results for zip codes with above-median percent Black population. Although the estimates for job vacancies are slightly larger, the overall results are similar to those using the whole population. Job vacancies increase, but this increase is not robust to clustering the errors at the state level. The results for wages are not significant in any specification. In other words, we find no evidence that OZs have already worked by benefiting black communities.

	(1) N. of Vacancies	(2) N. of Vacancies	(3) N. of Vacancies	(4) N. of Vacancies	(5) Avg. Hourly Wage	(6) Avg. Hourly Wage	(7) Avg. Hourly Wage	(8) Avg. Hourly Wage
OZ × Post 2018	0.108**	0.108	0.0626	0.0626	0.00620	0.00620	0.00778	0.00778
	(0.0390)	(0.0682)	(0.0372)	(0.0528)	(0.00403)	(0.00994)	(0.00802)	(0.0116)
OZ × Post-COVID			0.198***	0.198			-0.00606	-0.00606
			(0.0421)	(0.108)			(0.00903)	(0.0106)
Year-month fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Zipcode fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Errors clustering	Zipcode	State	Zipcode	State	Zipcode	State	Zipcode	State
Estimation method	Poisson	Poisson	Poisson.	Poisson	OLS-Heckman	OLS-Heckman	OLS-Heckman	OLS-Heckman
N	297696	297696	297696	297696	171822	171822	171822	171822
adj. R^2					0.019	0.049	0.049	0.049
AIC	6955551.4	6955521.4	6907399.1	6907367.1	162302.0	162272.0	162301.3	162271.3

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

Table 2: Difference-in-differences estimates of the effect of Opportunity Zones on job vacancies and wages, for zip codes with above-median percent of Black population.

Another important source of heterogeneity is differences across states. Figure 3 shows the post-OZ designation treatment and the post-COVID treatment estimates for each state in our sample. The table with the coefficients and their corresponding standard errors can be found in the appendix, Tables A.4 and A.5. We reject the simple hypothesis that 5% (95% significance) of the 45 states in our sample should turn a false positive by chance. Each of the four panels in Figure 3 shows more than 3 states with significant effects on vacancies and wages. Although these estimates should be carefully interpreted because we are testing multiple hypotheses simultaneously, they suggest that the type of investment and work being done in OZs may have helped to buffer the economic downturn.

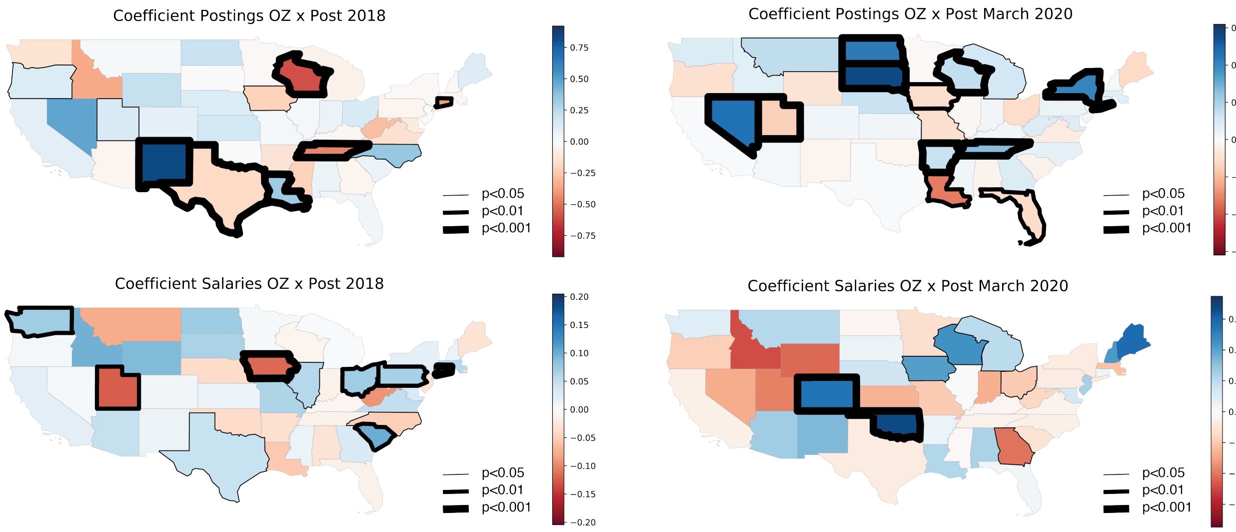


Figure 3: Effect of OZs on vacancies and wages by state.

5 Conclusion

We study the effect of the Opportunity Zone program on employment outcomes. 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 vacancies and posted wages between these two groups over time. Zip codes with OZs have more job vacancies and higher wages than comparable non-OZ zip codes, but these results are small in magnitude and not statistically significant in most cases. We also assess how the COVID-19 pandemic has affected zip codes with OZs. Although there is no statistically significant effect, the evidence suggests that economic harms resulting from the crisis were buffered somewhat in OZs. OZ designation may have ameliorated the damage of the crisis on employment outcomes.

While much work remains to evaluate the efficacy of the Opportunity Zone program and its effects on economic outcomes, we believe the results thus far point to the need for at least two policies. First, even though 8700 census tracts have been designated as OZs, it appears from analysis of EIG data that only a few hundred OZs have received investments. A policy to subsidize capital investments may help reduce the economic damage of the COVID-19 crisis, and therefore policymakers should consider ways to direct future investments to those areas most in need.[11] Second, the federal government should do more to track which types of projects are being implemented in OZs, and should release more up-to-date and timely data about OZ investments. Researchers and policymakers could use the data to assess the efficacy of the program. This recommendation is echoed in a recent report by the U.S. Government Accountability Office (GAO, 2020), which in addition proposes agencies to oversee the OZ program.

[11] Several organizations raised these and related concerns. For example, the Urban Institute suggests several ideas here: urban.org.

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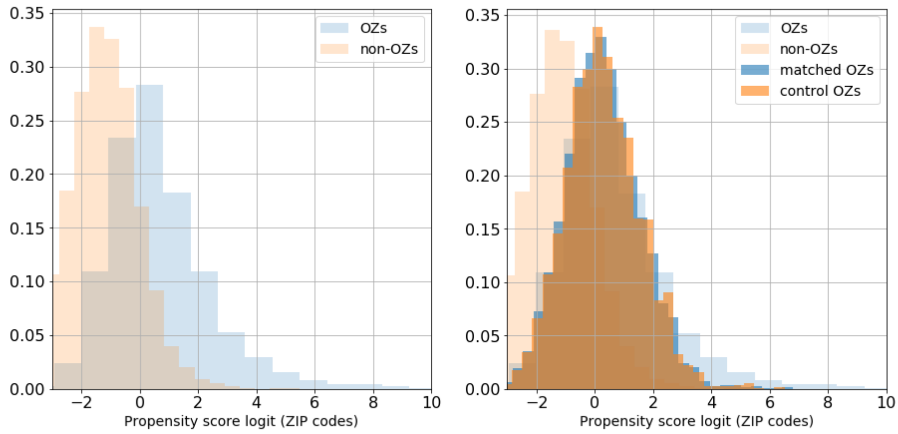

Figure A.1: Propensity scores for each zip code. Left panel shows the estimated propensity score for zip codes with and without opportunity zones. While there is overlap, there is also a considerable distributional difference between the two groups. The right panel highlights the result of the matching.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

A Appendix

A.1 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 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.[12] We also add two dummies on whether the qualification was within the ACS measurement error for each variable. These variables were chosen because they were the key variables used for the eligibility criteria, or they were recognized by policymakers as being important.[13] We restrict our observations to only census tracts that qualified for the program through the poverty or the income route.[14]

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 j by regressing the treatment of j (whether there is at least one

[12]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.

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

[14]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.

OZ in j) on $\tilde{r}_j$ and the number of job postings recorded in j for 2016, by state.[15] 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.[16] This leaves us with a total of 6,627 matched pairs.

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

[16]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.2 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 other hand, ZBP reports yearly data at the ZipCode Tabulation Area level (ZCTA) and is only available until 2018.

	(1)	(2)	(3)	(4)	(5)	(6)
	log quart. emp.	log quart. emp.	log quart. emp.	quart. emp.	quart. emp.	quart. emp.
number of job postings (log)	0.956***	0.970***	0.0455***	0.902***	0.913***	0.00255***
	(0.00187)	(0.00183)	(0.000992)	(0.00000623)	(0.00000637)	(0.0000201)
1-quarter lagged n.o. job postings (log)			0.949***			0.997***
			(0.000906)			(0.0000220)
Year-quarter fe	No	Yes	Yes	No	Yes	Yes
Estimation method	OLS	OLS	OLS	Poisson	Poisson	Poisson
N	70425	70425	67223	70425	70425	67223
adj. R^2	0.788	0.799	0.988			
AIC	191219.6	187315.5	-12712.7	1.87902e+09	1.73719e+09	10261765.1

Standard errors in parentheses

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

Table A.1: Cross section comparison between quarterly employment level in each county (QCEW) with quarterly job postings in each county (BG). Model (1) shows that job postings explain 70% of the variance in employment and that a 1% increase in job postings translates into a 0.95% increase in employment. Model (2) highlights that this is not due to any observable time trend. Model (3) shows that job postings are still significant at explaining employment even after controlling for previous-quarter employment level. Models 4-6 highlight the same results using a Poisson regression model.

	(1)	(2)	(3)	(4)	(5)	(6)
	log yearly emp.	log yearly emp.	log yearly emp.	yearly emp.	yearly emp.	yearly emp.
number of job postings (log)	0.646***	0.651***	0.0720***	0.256***	0.260***	0.000709***
	(0.00212)	(0.00218)	(0.00117)	(0.0000124)	(0.0000125)	(0.0000107)
1-year lagged n.o. job postings (log)			0.874***			0.968***
			(0.000859)			(0.0000295)
Year fe	No	Yes	Yes	No	Yes	Yes
Estimation method	OLS	OLS	OLS	Poisson	Poisson	Poisson
N	288121	288121	255019	288121	288121	255019
adj. R^2	0.243	0.254	0.851			
AIC	1389495.4	1385318.3	812599.7	2.10495e+09	2.09944e+09	72629713.7

Standard errors in parentheses

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

Table A.2: Comparison between yearly job postings in BG and yearly employment level in ZBP. Model (1) shows that the number of job postings in Burning Glass explains 25% of the variance in yearly employment, with a 1% increase in job postings translating into a 0.65% increase in employment level. Model (2) adds yearly fixed effects, highlighting that this relationship is not due to to general time trends. Model (3) shows that this relationship persists even after controlling for last-year job postings. Models 4-6 repeat the exercise using a Poisson regression model.

A.3 Additional Tables

	(1)	(2)	(3)
	OZ_num	OZ_num	OZ_num
income-qualified	0.0955***		
	(0.00733)		
poverty-qualified	0.158***		
	(0.00537)		
is-urban		-0.0762***	-0.0772***
		(0.00837)	(0.00837)
population		0.0260***	0.0272***
		(0.00481)	(0.00497)
poverty		0.704***	0.710***
		(0.0242)	(0.0247)
income-ratio		-0.103***	-0.103***
		(0.0164)	(0.0168)
percent-black		0.0573***	0.0540***
		(0.0107)	(0.0107)
percent-asian		-0.247***	-0.246***
		(0.0313)	(0.0313)
percent-other-non-whie		0.0551*	0.0554*
		(0.0236)	(0.0237)
percent-hispanic		-0.122***	-0.124***
		(0.0144)	(0.0144)
bachelors or higher		-0.0664**	-0.0679**
		(0.0229)	(0.0232)
income growth (YoY)			0.0764***
			(0.0207)
pop growth (YoY)			0.0443
			(0.0459)
State fe	Yes	Yes	Yes
N	33935	33738	33611
adj. R^2	0.102	0.136	0.137
AIC	35922.8	34372.3	34210.6

Standard errors in parentheses

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

Table A.3: 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.

Full results by state

NAME	OZ EFFECT	OZ p-value	COVID EFFECT	COVID p-value
Alabama	0.07060	0.506	-0.03590	0.662
Arizona	-0.04720	0.602	0.01980	0.813
Arkansas	-0.12800	0.146	0.14100	0.000
California	0.09600	0.180	0.00716	0.885
Colorado	0.09300	0.404	0.03360	0.673
Connecticut	-0.38800	0.007	0.06440	0.550
Florida	0.03830	0.313	-0.12000	0.003
Georgia	-0.04650	0.559	0.08960	0.090
Idaho	-0.35100	0.140	0.06960	0.304
Illinois	0.03200	0.640	-0.01800	0.759
Indiana	0.07070	0.474	0.02540	0.634
Iowa	-0.21000	0.043	-0.11100	0.008
Kansas	0.17500	0.281	0.03180	0.592
Kentucky	-0.02180	0.898	0.02840	0.546
Louisiana	0.33700	0.000	-0.32100	0.003
Maine	0.11900	0.311	-0.12300	0.203
Maryland	-0.08720	0.103	0.07830	0.183
Massachusetts	-0.02680	0.563	0.07230	0.188
Michigan	-0.05790	0.228	0.11500	0.029
Minnesota	-0.02060	0.864	-0.00993	0.824
Mississippi	-0.21000	0.069	0.03340	0.542
Missouri	0.03180	0.798	-0.10600	0.024
Montana	0.01590	0.885	0.15700	0.012
Nebraska	0.12500	0.679	0.13600	0.282
Nevada	0.48400	0.067	0.46500	0.000
New Hampshire	-0.02100	0.711	0.01390	0.778
New Jersey	-0.01170	0.888	0.01880	0.803
New Mexico	0.83700	0.000	-0.03340	0.458
New York	-0.01810	0.759	0.41600	0.000
North Carolina	0.35400	0.015	0.05630	0.472
North Dakota	0.20400	0.080	0.44500	0.000
Ohio	0.14600	0.410	-0.11200	0.315
Oklahoma	-0.00285	0.985	-0.03120	0.431
Oregon	0.12600	0.035	-0.09820	0.307
Pennsylvania	-0.04700	0.608	0.04660	0.490
South Carolina	0.06200	0.547	-0.04240	0.388
South Dakota	-0.00609	0.958	0.56300	0.001
Tennessee	-0.44800	0.000	0.26400	0.000
Texas	-0.18700	0.000	0.00484	0.945
Utah	0.14300	0.028	-0.14600	0.001
Virginia	-0.14800	0.308	-0.05620	0.123
Washington	-0.14100	0.090	0.08410	0.057
West Virginia	-0.27400	0.082	0.08200	0.239
Wisconsin	-0.59500	0.000	0.15400	0.000
Wyoming	0.22000	0.238	-0.08270	0.739

Table A.4: 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.

NAME	OZ EFFECT	OZ p-value	COVID EFFECT	COVID p-value
Alabama	-0.02870	0.308	0.058500	0.147
Arizona	0.04800	0.185	0.064300	0.105
Arkansas	-0.03290	0.247	0.012200	0.838
California	0.02210	0.143	-0.013400	0.479
Colorado	0.00910	0.715	0.136000	0.000
Connecticut	0.18600	0.000	0.010600	0.809
Florida	-0.01280	0.617	0.007390	0.767
Georgia	0.03190	0.321	-0.101000	0.016
Idaho	0.09870	0.072	-0.122000	0.077
Illinois	0.05820	0.016	0.000862	0.975
Indiana	-0.01320	0.647	-0.067700	0.069
Iowa	-0.11800	0.001	0.103000	0.018
Kansas	0.01390	0.740	-0.065400	0.372
Kentucky	-0.00659	0.769	-0.006820	0.852
Louisiana	-0.05470	0.180	0.056900	0.192
Maine	-0.02960	0.684	0.147000	0.063
Maryland	0.03470	0.359	0.033300	0.293
Massachusetts	0.05730	0.077	-0.054300	0.103
Michigan	0.00202	0.937	0.050400	0.042
Minnesota	0.00478	0.882	-0.031800	0.414
Mississippi	0.01740	0.678	-0.003990	0.949
Missouri	0.06310	0.134	-0.048300	0.363
Montana	-0.07530	0.233	0.052900	0.518
Nebraska	-0.03890	0.678	0.038200	0.695
Nevada	0.00705	0.915	-0.069700	0.259
New Hampshire	0.01710	0.800	0.108000	0.180
New Jersey	-0.03070	0.362	0.061200	0.062
New Mexico	0.01570	0.773	0.083700	0.426
New York	0.01990	0.519	-0.017600	0.528
North Carolina	-0.04990	0.043	-0.014600	0.582
North Dakota	0.07410	0.192	-0.006430	0.933
Ohio	0.07220	0.002	-0.049100	0.047
Oklahoma	-0.03850	0.233	0.170000	0.001
Oregon	0.00587	0.861	-0.046200	0.228
Pennsylvania	0.06840	0.007	-0.016400	0.593
South Carolina	0.10100	0.001	-0.028000	0.505
South Dakota	0.06450	0.522	0.014000	0.915
Tennessee	-0.01360	0.576	-0.012100	0.724
Texas	0.04650	0.010	-0.018400	0.394
Utah	-0.12400	0.006	-0.094500	0.127
Virginia	0.05050	0.052	-0.011500	0.717
Washington	0.07230	0.002	0.022500	0.494
West Virginia	-0.09180	0.069	-0.038800	0.621
Wisconsin	-0.01060	0.796	0.113000	0.024
Wyoming	0.09090	0.270	-0.108000	0.270

Table A.5: Summary of the difference in differences estimator for salaries 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.

Estimation for strong treatment

	(1) N. of Vacancies	(2) N. of Vacancies	(3) N. of Vacancies	(4) N. of Vacancies	(5) Avg. Hourly Wage	(6) Avg. Hourly Wage	(7) Avg. Hourly Wage	(8) Avg. Hourly Wage
OZ × Post 2018	0.0918	0.0918	0.114	0.114	0.0442***	0.0442**	0.0397**	0.0397*
	(0.0655)	(0.0653)	(0.0680)	(0.0731)	(0.00766)	(0.0154)	(0.0134)	(0.0176)
OZ × Post-COVID			-0.0909*	-0.0909			0.0189	0.0189
			(0.0430)	(0.0572)			(0.0171)	(0.0207)
Year-month fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Zipcode fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Errors clustering	Zipcode	State	Zipcode	State	Zipcode	State	Zipcode	State
Estimation method	Poisson	Poisson	Poisson	Poisson	OLS-Heckman	OLS-Heckman	OLS-Heckman	OLS-Heckman
N	133616	133616	133616	133616	63462	63462	63462	63462
adj. R^2					0.056	0.091	0.091	0.091
AIC	2075616.9	2075596.9	2073838.4	2073816.4	77073.0	77053.0	77071.1	77051.1

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

Table A.6: Results for zipcodes identified as strongly treated, defined as zipcodes in which more than 80% of their population lives in a OZ designated track.

Errors clustered at different levels

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Vacancies	Vacancies	Vacancies	Vacancies	Vacancies	Vacancies	Vacancies	Vacancies	Vacancies	Vacancies	Vacancies	Vacancies
OZ x post 2018	0.0314	0.0314	0.0314	-0.00401	-0.00401	-0.00401	0.0345	0.0345	0.0345	-0.000772	-0.000772	-0.000772
	(0.0343)	(0.0783)	(0.0719)	(0.0325)	(0.0651)	(0.0566)	(0.0345)	(0.0793)	(0.0730)	(0.0326)	(0.0659)	(0.0575)
OZ x post COVID				0.150***	0.150	0.150				0.149***	0.149	0.149
				(0.0348)	(0.0901)	(0.0925)				(0.0350)	(0.0904)	(0.0929)
Year-month fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Zipcode fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Error cluster	Zipcode	CBSA/County	State	Zipcode	CBSA/County	State	Zipcode	CBSA/County	State	Zipcode	CBSA/County	State
Est. method	Poisson	Poisson	Poisson	Poisson	Poisson	Poisson	Poisson	Poisson	Poisson	Poisson	Poisson	Poisson
Large states?	No	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
N	632464	632464	632464	632464	632464	632464	616560	616560	616560	616560	616560	616560
adj. R^2												
AIC	12511069.9	12511069.9	12511051.9	12467515.2	12467515.2	12467495.2	12264258.8	12264258.8	12264226.8	12221217.4	12221217.4	12221183.4

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

Table A.7: Models 1-6 show results at different levels of geographic clustering for all states in our sample, where our preferred level for estimating the errors is at the state level. Models 7-12 are restricted to States with more than 100 eligible tracts, where our preferred method for estimating errors is at the zip code level. Since these states were all able to select 25% of their eligible tracts, the assignment is not clustered at the state level, which means our preferred specification should not include state clustering following the guidelines in Abadie et al. (2017).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Avg. Hourly Wage	Avg. Hourly Wage	Avg. Hourly Wage	Avg. Hourly Wage	Avg. Hourly Wage	Avg. Hourly Wage	Avg. Hourly Wage	Avg. Hourly Wage	Avg. Hourly Wage	Avg. Hourly Wage	Avg. Hourly Wage	Avg. Hourly Wage
OZ x Post 2018	0.0145**	0.0145	0.0145*	0.0140*	0.0140	0.0140	0.0150**	0.0150	0.0150*	0.0146**	0.0146	0.0146
	(0.00508)	(0.00835)	(0.00696)	(0.00545)	(0.00900)	(0.00773)	(0.00513)	(0.00841)	(0.00704)	(0.00549)	(0.00903)	(0.00782)
OZ x Post COVID				0.00185	0.00185	0.00185				0.00144	0.00144	0.00144
				(0.00639)	(0.0115)	(0.00834)				(0.00645)	(0.0115)	(0.00848)
Year-month fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Zipcode fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Error cluster	Zipcode	CBSA/County	State	Zipcode	CBSA/County	State	Zipcode	CBSA/County	State	Zipcode	CBSA/County	State
Est. method	OLS/Heckman	OLS/Heckman	OLS/Heckman	OLS/Heckman	OLS/Heckman	OLS/Heckman	OLS/Heckman	OLS/Heckman	OLS/Heckman	OLS/Heckman	OLS/Heckman	OLS/Heckman
Large states?	No	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
N	364282	364282	364282	364282	364282	364282	356427	356427	356427	356427	356427	356427
adj. R^2	0.060	0.060	0.060	0.060	0.060	0.060	0.059	0.059	0.059	0.059	0.059	0.059
AIC	371642.4	371642.4	371626.4	371644.2	371644.2	371626.2	362248.4	362248.4	362218.4	362250.3	362250.3	362218.3

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

Table A.8: Models 1-6 show results at different levels of geographic clustering for all states in our sample, where our preferred level for estimating the errors is at the state level. Models 7-12 are restricted to States with more than 100 eligible tracts, where our preferred method for estimating errors is at the zip code level. Since these states were all able to select 25% of their eligible tracts, the assignment is not clustered at the state level, which means our preferred specification should not include state clustering following the guidelines in Abadie et al. (2017).

Results including 2018 observations

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	CountJobID	CountJobID	CountJobID	CountJobID	log_MinHourlySalary	log_MinHourlySalary	log_MinHourlySalary	log_MinHourlySalary
OZ × Post 2018	0.0195	0.0195	-0.00889	-0.00889	0.0120***	0.0120*	0.0114*	0.0114
	(0.0313)	(0.0609)	(0.0307)	(0.0498)	(0.00256)	(0.00593)	(0.00456)	(0.00642)
OZ × Post-COVID			0.152***	0.152			0.00351	0.00351
			(0.0373)	(0.0983)			(0.00621)	(0.00822)
Year-month fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Zipcode fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Errors clustering	Zipcode	State	Zipcode	State	Zipcode	State	Zipcode	State
Estimation method	Poisson	Poisson	Poisson	Poisson	OLS-Heckman	OLS-Heckman	OLS-Heckman	OLS-Heckman
N	767992	767992	767992	767992	447148	447148	447148	447148
adj. R^2					0.028	0.053	0.053	0.053
AIC	14769507.3	14769465.3	14721570.2	14721526.2	469063.3	469019.3	469062.7	469018.7

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

Table A.9: Effects including all 2018 observations.

First stage of Heckman sample selection model

	(1)	(2)
	has_salary	has_salary
has_salary		
L.log_Vacancies	0.662***	0.702***
	(0.00150)	(0.00161)
_cons	-1.165***	-1.512***
	(0.00346)	(0.0153)
Year-month fe	No	Yes
N	609876	609876
adj. R^2		
AIC	502785.6	477061.6

Standard errors in parentheses

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

Table A.10: First stage Heckman selection model. We predict whether or not the zip code posts vacancies that report average minimum wage based on the number of postings in the month before.