The Impacts of Opportunity Zones on Zone Residents

Matthew Freedman, Shantanu Khanna, and David Neumark*

University of California, Irvine

February 2021

Abstract

Created by the Tax Cuts and Jobs Act in 2017, the Opportunity Zone program was designed to encourage investment in distressed communities across the U.S. We examine the early impacts of the Opportunity Zone program on residents of targeted areas. We leverage restricted-access microdata from the American Community Survey and employ difference-in-differences and matching approaches to estimate causal reduced-form effects of the program. Our results point to modest, if any, positive effects of the Opportunity Zone program on the employment, earnings, or poverty of zone residents.

* Freedman: matthew.freedman@uci.edu; Khanna: shantanu@uci.edu; Neumark: dneumark@uci.edu.
This paper uses restricted-access data from the U.S. Census Bureau. Any views expressed are those of the authors and not those of the U.S. Census Bureau. The Census Bureau's Disclosure Review Board and Disclosure Avoidance Officers have reviewed this information product for unauthorized disclosure of confidential information and have approved the disclosure avoidance practices applied to this release. This research was performed at the UC Irvine Federal Statistical Research Data Center under FSRDC Project Number 2146 (CBDRB-FY21-P2146-R8858).

1. Introduction

There is a lack of clear evidence that the most prominent place-based policy – enterprise zones – have created jobs and raised incomes for the least-advantaged people in neighborhoods with high concentrations of low-income residents (see the review in Neumark and Simpson, (2015). Nonetheless, with strong encouragement from the Trump Administration, the Tax Cuts and Jobs Act of 2017 created a new and related place-based policy: "Opportunity Zones."[1] Opportunity Zones are targeted at disadvantaged census tracts and are intended to spur job creation. Opportunity Zone incentives are directed at investors in property, allowing deferral or avoidance of federal taxes on capital gains in investments in these zones.

In this paper, we provide early evidence on the impacts of Opportunity Zone designation on residents of zones, focusing in particular on employment, earnings, and poverty. We take advantage of restricted-access microdata from the American Community Survey (ACS) for 2013-2019 to explore the program's impacts at a geographically granular level. Similar to other recent papers (e.g., Chen et al., 2019; Arefeva et al., 2020), we compare outcomes in tracts designated as Opportunity Zones to those eligible but not designated. We also estimate effects for Opportunity Zones using a control group of tracts matched on the basis of trends in outcomes prior to the program's introduction.

Overall, we find limited evidence of any positive effects on the economic conditions of residents of targeted neighborhoods. Based on a simple difference-in-differences approach comparing changes in outcomes in tracts designated as Opportunity Zones to those in tracts eligible but not designated, we find some indication of modest increases in resident employment levels and employment rates as well as reductions in poverty. However, these positive effects

[1] See https://home.treasury.gov/news/press-releases/sm0341.

appear to be driven at least in part by differential trends in outcomes across designated and non-designated tracts, and largely vanish when we estimate effects based on control tracts matched to zones on the basis of pre-treatment trends in outcomes. Our preferred estimates based on our matching approach point to effects of Opportunity Zone designation that are statistically indistinguishable from zero and economically small. Specifically, we estimate that following Opportunity Zone designation, employment rates of residents increase a statistically insignificant 0.4 percentage point (0.8%). We can rule out effects on employment rates larger than 1.2 percentage points with 95% confidence. Estimated effects on average earnings of employed residents of designated tracts are also small and statistically indistinguishable from zero. We additionally find a statistically insignificant impact of zone designation on poverty rates of −0.7 percentage point (2.5%), and we can rule out reductions in the poverty rate larger than 1.6 percentage points with 95% confidence.

Given that Opportunity Zone designations were first announced in 2018, we are at the beginning of research on the impacts of Opportunity Zones. Earlier work on the federal New Markets Tax Credit, which is the most similar prior policy, found a positive impact on investment, mainly via real-estate investment, coupled with a modest and costly poverty reduction effect (Freedman, 2012).[2] In recent work, Arefeva et al. (2020) leverage establishment-level data (the Your-economy Time Series) and find that Opportunity Zone designation increased employment growth relative to comparable (eligible, but not chosen) tracts substantially (3.0 to 4.5 percentage points), with the growth spread across industries. Using a similar identification strategy, Chen et al. (2019) find little effect of Opportunity Zone designation on residential

[2] Lester et al. (2018) discuss the similarities and differences between the New Markets Tax Credit and Opportunity Zones.

property prices, but Sage et al. (2019) document significant positive impacts on prices of some types of commercial properties. Frank et al. (2020) find positive effects of Opportunity Zone designations on commercial real estate transactions, building permits, and construction employment. Atkins et al. (2020) study Opportunity Zone effects on job postings (from Burning Glass) by zip code, distinguished by whether the zip code contains at least one Opportunity Zone tract or not, and find a small negative effect on job postings and a small positive effect on posted salaries.

The main contribution of our paper is that we identify the impacts of the Opportunity Zone program on zone residents as opposed to businesses, workers, or property values. To the extent that a major motivation for the Opportunity Zone program was improving outcomes for residents of distressed communities – as evidenced by the criteria for designating Opportunity Zones based largely on low incomes of residents – the impacts of the program on residents is of paramount importance. We know from past work on place-based policies that even those programs that are effective at creating jobs may not deliver benefits to residents of targeted places (Busso et al., 2013; Freedman, 2015; Reynolds and Rohlin, 2015). The institutional structure of the Opportunity Zone program raises concerns that any job creation or investment spurred by the program may have limited benefits for local residents (Eastman and Kaeding, 2019). Our data on the economic circumstances of those living in Opportunity Zones allow us to speak directly to the program's benefits for residents. By examining impacts on residents, we also provide evidence comparable to that for enterprise zones and other place-based policies (e.g., Busso et al., 2013; Neumark and Young, 2019).

An additional contribution of our evaluation is the use of rich, granular demographic and economic information available in the confidential ACS together with alternative empirical approaches based on selecting suitable sets of comparison groups. Following Chen et al. (2019) and Arefeva et al. (2020), we begin with difference-in-differences strategies comparing changes in outcomes between tracts designated as Opportunity Zones and tracts eligible but not designated. We then further refine our comparison group, using control tracts matched on having a similar evolution of outcomes prior to Opportunity Zone designation. To the extent that this addresses differences in underlying trends in outcomes across designated and non-designated (but eligible) tracts – which are suggested by the data – our matching approach delivers more credible estimates of the program's effects on residents of targeted areas.

2. The Opportunity Zone Program

The Opportunity Zone program was introduced as part of the 2017 Tax Cut and Jobs Act (TCJA), which was signed into law on December 22, 2017. The Opportunity Zone program provides preferable tax treatment for capital gains from investments in certain designated census tracts. To receive these tax benefits, investors can invest directly in Opportunity Zones or invest in Qualified Opportunity Funds (QOFs); QOFs are required to invest at least 90% of their assets into Opportunity Zone businesses or real estate. The tax benefits of investing in zones take three primary forms. First, capital gains on new investments in Opportunity Zones (often made through QOFs) are not taxed, conditional on the investment being held for ten years or more. Second, there a basis step-up for realized capital gains that are reinvested in Opportunity Zones; the basis on the original investment is increased by 10% for capital gains invested for at least five years, and the basis on the original investment is increased by 15% for capital gains invested for at least seven years. Finally, the program temporarily allows investors with realized capital gains on assets to defer paying taxes on those gains by investing those gains into businesses or real estate in Opportunity Zones. Those gains are not taxed until the end of 2026 or when the investor disposes of the asset (Theodos et al., 2018; IRS, 2020; Arefeva et al., 2020).

The TCJA legislation had provisions for the designation of Opportunity Zones in the 2018 tax year, with the tax benefits beginning in that tax year. In particular, the legislation allowed state governors to designate as Opportunity Zones up to 25% of census tracts in their state that qualify as so-called "low-income communities" (LICs), as well as some tracts contiguous with LICs (a maximum of 5% of the total).[3] States were required to choose which LICs and non-LIC contiguous tracts were to be designated Opportunity Zones in early 2018; all states made their designations by June 2018 (Lester et al., 2018, Theodos et al., 2018, U.S. Department of Treasury, 2018).

The definition of LICs is based on Section 45D of the U.S. tax code, and is the same as that used by the New Markets Tax Credit program (Freedman, 2012). An LIC must have a poverty rate of at least 20% or have median family income less than or equal to 80% of the greater of metropolitan area or statewide median family income (just statewide for rural tracts).[4] A tract is also an LIC if it is within a federal Empowerment Zone, has a population less than 2,000 people, and is contiguous to one or more LICs. The vast majority of tracts that qualify as LICs qualify on the poverty rate or median family income criteria (Freedman, 2012).

According to the Opportunity Zone legislation, 95% of the tracts designated as Opportunity Zones by governors had to be an LIC. Governors were permitted to choose some additional tracts to designate as Opportunity Zones so long as those tracts were contiguous with an LIC and had

---

[3] See, for example, https://www.enterprisecommunity.org/blog/understanding-opportunity-zones-eligibility.
[4] The criterion is 85% for "high migration rural counties" that, during the 20 years since the most recent census, had outmigration of at least 10%. See, for example, https://ded.mo.gov/content/opportunity-zones-application-information.

median income less than 125% of the median income of the LIC with which it was contiguous.

In total, 42,176 tracts were eligible to be Opportunity Zones, including 31,864 LICs and 10,312 non-LIC contiguous tracts. Nationwide, governors selected a total of 8,762 tracts as Opportunity Zones; 97% of those selected (8,532) were LICs, while only 3% (230) were non-LIC contiguous tracts (Theodos et al., 2018). Several papers and reports have studied the selection process for Opportunity Zones across states. Theodos et al. (2018) analyze governors' selections and find that tracts selected as Opportunity Zones were more economically distressed than other eligible tracts, but their analysis points to only a limited amount of targeting toward more disadvantaged neighborhoods with lower access to capital. Alm et al. (2020), Frank et al. (2020), and Duarte et al. (2020) similarly find that designated tracts are on average poorer than other eligible tracts. There is some evidence to suggest that political favoritism may have influenced governors' selections (Alm et al., 2020; Frank et al., 2020; Eldar and Garber, 2020), but there is also evidence that indicates that governors largely rubber-stamped recommendations for zone designations that came from mayors (Duarte et al., 2020). Several papers have also highlighted that, at least along some dimensions, tracts that were designated as Opportunity Zones were on different trajectories than tracts eligible but not designated (Chen et al., 2019; Eldar and Garber, 2020).

3. Data

Our data on tracts eligible and designated as Opportunity Zones come from the Community Development Financial Institutions (CDFI) Fund at the U.S. Department of Treasury.[5] To construct outcomes, we take advantage of restricted-access American Community Survey (ACS)

[5] See https://www.cdfifund.gov/Pages/Opportunity-Zones.aspx.

data for 2013-2019, which we accessed in a Federal Statistical Research Data Center (FSRDC). The advantage of the restricted-access ACS data is that we can measure outcomes at the tract-level on an annual basis; the public-use data only provide tract-level information averaged over five years. However, due to sample sizes and confidentiality restrictions, we are limited in the extent to which we can drill down to look at outcomes measured for sub-geographies (e.g., individual states) or examine heterogeneity in effects across areas with different initial conditions or other characteristics.

We focus on the effects of Opportunity Zones on residents of designated areas. We construct four main outcome measures: overall employment among residents, the employment-to-population ratio for residents, average earnings of employed residents, and the poverty rate for residents. We aggregate the individual microdata to the tract-by-year level, using the person weights in the ACS. We only keep tracts that have complete information for all our outcomes of interest.[6] We additionally restrict attention to designated and eligible tracts that are LICs; while non-LIC contiguous tracts represent over one-fifth of tracts technically eligible, limits on how many of these tracts could be chosen as Opportunity Zones as well as a seeming preference for designating more distressed tracts led to only 230 non-LIC contiguous tracts being designated (less than 3% of the total). Including the complete set of non-LIC contiguous tracts in the sample would, at least for the difference-in-differences analysis, necessitate using a disproportionate number of higher-income tracts as controls – controls that are less comparable to the final set of designated tracts. Taken together, these restrictions reduce our sample of designated Opportunity

[6] We also exclude from the analysis Puerto Rico, where all eligible LICs were designated as Opportunity Zones.

Zones to (a rounded) 7,600 tracts, and our sample of eligible but not designated tracts to (a rounded) 23,000.[7]

We conduct our main analyses using this sample of tracts for the 2013-2019 period. Basic descriptive statistics for the sample, broken out by year and overall, appear in Table 1. On average, the adult population of tracts in the sample is around 3,200. Consistent with earlier findings, tracts designated as Opportunity Zones have lower employment rates, lower average earnings, and higher poverty rates than tracts eligible but not designated as Opportunity Zones (Arefeva et al., 2020).

4. Empirical Approach

We begin by estimating difference-in-differences models to identify the impact of Opportunity Zone designation on outcomes for residents of targeted areas. Our approach in this case is similar to that of other recent papers on the program, including Chen et al. (2019) and Arefeva et al. (2020). Our basic difference-in-differences model is

$$y_{i,t} = \beta OZ_i \times Post_t + \gamma_i + \eta_t + \varepsilon_{i,t} \tag{1}$$

In equation (1), $y_{i,t}$ is the outcome of interest for tract *i* in year *t*. OZ_i is a dummy that takes a value of 1 if tract *i* is designated as an Opportunity Zone and 0 if it is eligible but not designated; recall that the sample is restricted to designated and eligible but not designated LICs. $Post_t$ is a dummy that equals 1 for 2019 and 0 for years prior to 2019. We additionally run regressions where $Post_t$ equals 1 for both 2019 and 2018, as 2018 is a partially treated year, with designations taking place in the first half of the year. Our difference-in-differences regressions

[7] These counts of tracts are rounded for confidentiality reasons. While in principle we could estimate effects for LIC and non-LIC designated tracts separately, doing so would pose potential disclosure problems in light of the small number of non-LIC tracts that were selected as zones.

include tract fixed effects (γ_i) and year fixed effects (η_t); these fixed effects subsume the main effects for OZ_i and $Post_t$. In some specifications, we additionally include state-by-year fixed effects, Public-Use Microdata Area (PUMA)-by-year fixed effects, or county-by-year fixed effects to account for potentially differential trends in outcomes across geographies at a higher level of aggregation than census tracts, which could confound our estimates of the effects of Opportunity Zone designation. These more saturated models effectively narrow the set of control tracts for any given treatment tracts to those more geographically proximate. While this limits the scope for potential unobservable time-varying factors to bias our estimates, it may amplify bias attributable to spillovers of Opportunity Zone effects across nearby tracts.

We extend our difference-in-differences model to an event study design in which we estimate treatment effects by year. The event study design not only allows us to trace out the time pattern impacts of the policy on targeted tracts post-2017, but also permits us to assess the validity of the parallel trends assumption by estimating differences between designated tracts and eligible but non-designated tracts in each year prior to Opportunity Zone designation. As discussed below, these event study results suggest that for many of our outcomes, the full sample of eligible but not designated tracts may not represent a suitable control group for designated tracts. In particular, pre-treatment trends in some outcomes are systematically different across treated LICs and the full sample of non-treated (but eligible) LICs in a way that could generate spurious effects in our baseline difference-in-differences estimates using equation (1).

To address concerns that our baseline difference-in-differences estimates might be biased as a result of differential trends, we adopt a second, data-driven approach to selecting controls LIC tracts, in which we match treated tracts to control tracts on the basis of pre-Opportunity Zone trends in each of the outcomes. In particular, we adopt a propensity score matching approach similar to that in Bondonio and Engberg (2000) and Neumark and Young (2019). For this approach, in order to facilitate more direct comparison with our basic difference-in-differences results, we construct the dependent variable for our matching results as a difference-in-difference for each outcome *y*:[8]

$$(y_{i,2019} - y_{i,2017}) - (y_{i,2017} - y_{i,2013}) \quad (2)$$

Note that in this case, we capture the effect of zone designation on the change in the outcome for the entire 2017 to 2019 period. Across all outcomes, we use a common list of controls to predict Opportunity Zone designation in order to construct the propensity score using a logit model. These include employment levels, average wages, and poverty rates measured annually over 2013-2017, the entire pre-treatment period (excluding 2018, which is partially treated). Based on the propensity scores, we identify a nearest control tract neighbor (nearest on the basis of the estimated propensity score) for each treated tract; this nearest neighbor minimizes the difference between treatment and control tracts in terms of the evolution of pre-treatment outcomes. For this sample, we can more credibly attribute differential changes in outcomes after Opportunity Zone designation to the program itself as opposed to continuations of pre-existing trends.

5. Results

5.1. Baseline Difference-in-Differences Estimates

Tables 2-5 present our baseline difference-in-differences results for resident employment levels, employment rates, average earnings, and poverty rates. In each table, we show models in which we alternatively set our $Post_t$ dummy in equation (1) equal to 1 for just 2019 and then for

[8] We show in Appendix Table A1 that regressing this transformation of the dependent variable on a dummy for Opportunity Zone designation for our sample of tracts yields estimates nearly identical to our panel-based difference-in-differences estimates.

both 2018 and 2019 (given 2018 is a partially treated year). All specifications include tract and year fixed effects. In the specifications in columns (2) and (6), we also include state-by-year fixed effects. In the specifications in columns (3) and (7), we include instead PUMA-by-year fixed effects. In the specifications in columns (4) and (8), we include instead county-by-year fixed effects. Standard errors (in parentheses) are clustered at the tract level throughout.

In Table 2, we show results for the number of employed residents. In our basic difference-in-differences model, we see statistically significant but modest impacts on employment levels in Opportunity Zones. The point estimate in column (1) of 16.59 implies a 1.0% increase in employed residents in targeted tracts; based on this specification, we can rule out with 95% confidence an effect on resident employment larger than 33. We find the largest effect in our specification with county-by-year fixed effects (column (4)); in that case, we estimate an increase in employment due to zone designation of 26 (1.5%), and can rule out with 95% confidence an increase larger than 43. That the estimates get larger in general as we add more detailed geographic controls, and in particular when we include PUMA- or county-by-year fixed effects, could be the result of a reduction in bias owing to a better accounting for differential trends in employment growth across geographies, but also could reflect an amplification of bias due to local spillovers (e.g., business-stealing effects) associated with zone designation. Note also that the estimates are smaller in columns (5)-(8), which could be because 2018 was only partially treated.

Table 3 shows results for the resident employment-to-population ratio. Echoing results for employment levels, we see evidence of statistically significant but modest positive effects on employment rates; in particular, the results point to a 0.5 to 0.7 percentage point increase in resident employment rates (on a base of 53% in Opportunity Zones). Table 4 shows results for average earnings among working tract residents. The estimated effects on average earnings are more variable depending on the exact specification, and almost always statistically insignificant. Across all specifications, the implied effects on average earnings are economically small.

Finally, in Table 5, we show results for the poverty rate. The estimated effects of zone designation on poverty are statistically significant and more economically meaningful than those for other outcomes. Specifically, our results suggest that, regardless of the specification, zone designation reduces poverty rates by approximately one percentage point (or about 4%).

Thus, to this point it appears that Opportunity Zones boost employment and reduce poverty. However, as we discuss in our next set of results, some of the estimated effects on resident outcomes in our baseline difference-in-differences results could be contaminated by differential trajectories among designated and non-designated tracts.

5.2. Event Study Estimates

In Figures 1-4, we show event study estimates for each of the four main outcomes discussed above. In each figure, we show results both for our baseline difference-in-differences controls of just tract and year fixed effects (corresponding to columns (1) and (5) in Tables 2-5), as well as for our most saturated controls (county-by-year fixed effects, corresponding to columns (4) and (8) in Tables 2-5). The graphs show point estimates and 95% confidence intervals. In each case, 2017 is the reference year; given that the figures report the interactions of OZ_i and the year dummy variables, the interaction with the dummy variable for 2017 is omitted.

Event study results for employment of residents appear in Figure 1. In this case, we see little evidence of any significant pre-trend. Consistent with the previous results, there is a slight but insignificant uptick in resident employment in 2018, but a more pronounced impact on resident employment in 2019. Again, however, the size of the impacts is not large. The additional 20 employed residents in 2019 represents about a 1.2% increase over the average level in 2017.

Figure 2 shows event study results for the employment rate. In this case, there is stronger evidence of a differential pre-treatment trend in the outcome for those areas designated as Opportunity Zones relative to those areas eligible but not designated. In particular, the employment rate in designated areas was trending upward prior to 2017, and the higher employment rate after 2017 may be merely the continuation of that trend. This seeming violation of the parallel trends assumption calls into question our ability to interpret the difference-in-differences estimates as representing the casual impact of Opportunity Zone designation on the employment rate.

In Figure 3, we see limited evidence of any Opportunity Zone effects on resident average earnings, but also less indication of a strong pre-trend in resident average earnings. Consistent with the difference-in-differences estimates in Table 4, we see little indication in Figure 3 of any positive impacts of zone designation on resident average earnings.

Figure 4 indicates a very strong pre-treatment trend in resident poverty rates. The results suggest that poverty rates of tracts that were designated as Opportunity Zones were already declining prior to designation and that the post-treatment changes more likely reflect the continuation of the prior trend than the causal effect of Opportunity Zone designation.

To purge the estimates of bias due to these differential trends, we implement a matching approach that balances treatment and control tracts on the pre-designation evolution of outcomes. We turn to these results in the next section.

5.3 Matching Estimates

In this section, we present results from matching Opportunity Zone tracts to eligible but not designated tracts based on pre-treatment trends in outcomes. The baseline estimates of the effects of treatment on the treated for our nearest-neighbor matched sample of tracts appear in Table 6. The estimated effects on resident employment levels are similar to the estimated effects in our baseline difference-in-differences results, which is not surprising given the lack of significant divergence in trends prior to treatment. In part due to the smaller sample size, however, the estimated effects on employment are no longer statistically distinguishable from zero. The results for employment point to an increase in resident employment of 24, or about 1.4%, following zone designation. Even with the larger standard errors with our matching approach, we can rule out with 95% confidence an increase in resident employment larger than 59.

The estimates from our matching approach for the employment rate, for which there was a more pronounced pre-treatment trend, are 23-46% smaller than those from our difference-in-differences approach (depending on the difference-in-differences specification). The estimated effect is also no longer statistically significant. The point estimate implies a 0.4 percentage point increase in the employment rate. We can rule out with 95% confidence an effect size for the employment rate larger than 1.2 percentage points.

Our results for the effects of Opportunity Zone designation for average earnings are small and insignificant, similar to our baseline difference-in-differences estimates. The point estimate of $434.90 represents 1.3% of the 2017 mean of average earnings in designated tracts.

Turning to the effects on poverty, consistent with the matching procedure reducing bias due to pre-existing trends, the estimated effect on the poverty rates is smaller than in our difference-in-differences results. It is also not statistically different from zero. The point estimate implies a 0.65 percentage point (2.5%) decline in the poverty rate due to Opportunity Zone designation.

The lower bound of the 95% confidence interval for the poverty rate effect of zone designation is a 1.58 percentage point decline.

In Table 7 and Figures 5-8, we present event study estimates using just our matched sample of tracts. These regressions mimic the baseline event study models for which we show estimates in Figures 1-4, except that the sample is now limited to designated tracts and propensity score-matched control tracts. It is clear from these event study estimates that the matching succeeded in eliminating the differential pre-2017 trends in outcomes that existed for designated vs. all other eligible tracts. And consistent with the previous results, we continue to see economically small and generally insignificant estimated effects of Opportunity Zone designations on resident outcomes.

6. Conclusion

We provide early evidence on the impacts of Opportunity Zone designation on residents of zones, focusing in particular on employment, earnings, and poverty. We use restricted-access microdata from the American Community Survey (ACS) for 2013-2019 to explore the program's impacts at a geographically granular level, comparing outcomes in tracts designated as Opportunity Zones to those eligible but not designated, and also estimating effects using a control group of tracts matched on the basis of trends in outcomes prior to the program's introduction.

Studying impacts on employment, earnings, and poverty, we find limited evidence of positive effects of Opportunity Zone designation on the economic conditions of residents of targeted neighborhoods. Simple difference-in-differences estimates, comparing changes in outcomes in low-income tracts designated as Opportunity Zones to those in tracts eligible but not designated, produce some evidence consistent with increases in employment levels and employment rates as well as reductions in poverty. However, these effects are substantially attenuated when we estimate effects using a group of control tracts matched to tracts designated as Opportunity Zones on the basis of pre-treatment trends in outcomes. Based on the latter approach, which generates more credible estimates, we find effects of Opportunity Zone designation that are statistically indistinguishable from zero and economically small. Specifically, we estimate that following Opportunity Zone designation, employment rates of residents increase a statistically insignificant 0.4 percentage point (0.8%), and we can rule out effects on employment rates larger than 1.2 percentage points with 95% confidence. Estimated effects on average earnings of employed residents of designated tracts are also small and not statistically different from zero. Similarly, we find a statistically insignificant impact of zone designation on poverty rates of −0.7 percentage point (2.5%), and we can rule out reductions in the poverty rate larger than 1.6 percentage points with 95% confidence.

Our analysis contributes to a growing number of studies of Opportunity Zones, which have looked at a variety of outcomes and thus far found some mixed evidence: positive impacts on employment growth in the zones (Arefeva et al., 2020); reductions in job postings but increases in salaries (Atkins et al., 2020); no impact on residential property prices (Chen et al., 2019); and positive impacts on some commercial property prices (Sage et al., 2019) and on commercial real estate activity (Frank et al., 2020). Some of these findings appear contradictory, but they may not be. For example, the effect on jobs in the zones could differ the effect on employment of zone residents; and commercial real estate prices could increase without many job gains if the tax credits are largely capitalized into real estate prices. It remains to future research to paint a fuller picture. However, one important lesson from our work is that researchers need to pay attention to prior trends in outcomes, which may be correlated with Opportunity Zone designation (consistent with the evidence in zone designation in Chen et al. (2019) and Eldar and Garber (2020)).

In future work, we plan to study the impacts of Opportunity Zone designation on additional outcomes measured in the ACS, including employment measured at place-of-work, commuting patterns, property values, and more.[9] This will allow us to provide a more comprehensive assessment of the program's effects. We also plan to estimate possibly heterogeneous effects of zone designation across tracts eligible based on different criteria. These include, for example, the effects for tracts that are directly eligible as low-income communities (LICs) vs. tracts that are eligible based on contiguity to an LIC (and other criteria), and the effects for tracts that qualify as LICs based on different criteria (e.g., median family income, poverty rates, or other criteria).[10] Information available in the confidential ACS will also permit us to explore potentially differential impacts across industries and occupations, as well as across tracts with different baseline characteristics and trajectories.

A potential limitation is that our estimates are "early" in the sense of extending only one full year past the year in which Opportunity Zones were designated and the tax incentives kicked in, hence covering about one-and-a-half years since inception. However, given that the 2020 data will include a year largely affected by the COVID-19 pandemic, with effects also extending into 2021, the data through 2019 may provide the most definitive evidence we can obtain for many years, barring future policy changes such as creating new Opportunity Zones or eliminating existing ones.

---

[9] The ACS micro-data provide visibility into respondents' tract of work and tract of residence.

[10] See https://www.enterprisecommunity.org/blog/understanding-opportunity-zones-eligibility It is possible that some of these analyses for will be constrained by Census confidentiality rules.

References

Alm, James, Trey Dronyk-Trosper, and Sean Larkin. 2020. "In the Land of OZ: Designating Opportunity Zones." *Public Choice*, forthcoming.

Arefeva, Alina, Morris Davis, Andra Ghent, and Minseon Park. 2020. "Who Benefits from Place-Based Policies? Job Growth from Opportunity Zones." SSRN Working Paper 3645507.

Atkins, Rachel M. B., Pablo Hernandez-Lagos, Cristian Jara-Figueroa, and Robert Seamans. 2020. "What is the Impact of Opportunity Zones on Employment Outcomes?" NYU Stern School of Business.

Bondonio, Daniele, and John Engberg. 2000. "Enterprise Zones and Local Employment: Evidence from the States' Programs." *Regional Science and Urban Economics* 30, 519-549.

Busso, Matias, Jesse Gregory, and Patrick Kline. 2013. "Assessing the Incidence and Efficiency of a Prominent Place Based Policy." *American Economic Review* 103(2), 897-947.

Chen, Jiafeng, Edward Glaeser, and David Wessel. 2019. "The (Non-) Effect of Opportunity Zones on Housing Prices." NBER Working Paper No. 26587.

Eastman, Scott, and Nicole Kaeding. 2019. "Opportunity Zones: What We Know and What We Don't." Tax Foundation Fiscal Fact, 630.

Eldar, Ofer, and Chelsea Garber. 2020. "Does Government Play Favorites? Evidence from Opportunity Zones." Duke Law School Public Law & Legal Theory Series No. 2020-28.

Frank, Mary Margaret, Jeffrey Hoopes, and Rebecca Lester. 2020. "What Determines Where Opportunity Knocks? Political Affiliation and Early Effects of Opportunity Zones." SSRN Working Paper 3534451.

Freedman, Matthew. 2012. "Teaching New Markets Old Tricks: The Effects of Subsidized Investment on Low-Income Neighborhoods." *Journal of Public Economics* 96(11-12), 1000-14.

Freedman, Matthew. 2015. "Place-Based Programs and the Geographic Dispersion of Employment." *Regional Science and Urban Economics* 53, 1-19.

Internal Revenue Service. 2020. "Opportunity Zone Frequently Asked Questions." Technical Report.

Lester, Rebecca, Cody Evans, and Hanna Tian. 2018. "Opportunity Zones: An Analysis of the Policy's Implications." *State Tax Notes* 90(3): 221-235.

Neumark, David, and Helen Simpson. 2015. "Place-Based Policies." In G. Duranton, V. Henderson, and W. Strange, Eds., <u>Handbook of Regional and Urban Economics, Vol. 5</u>. Amsterdam: Elsevier, 1197-287.

Neumark, David, and Timothy Young. 2019. "Enterprise Zones, Poverty, and Labor Market Outcomes: Resolving Conflicting Evidence." *Regional Science and Urban Economics* 78, 103462.

Reynolds, C. Lockwood, and Shawn Rohlin. 2015. "The Effects of Location-Based Tax Policies on the Distribution of Household Income: Evidence from the Federal Empowerment Zone

Program." *Journal of Urban Economics* 88, 1-15.

Sage, Alan, Mike Langen, and Alex Van de Minne. 2019. "Where Is the Opportunity in Opportunity Zones? Early Indicators of the Opportunity Zone Program's Impact on Commercial Property Prices." SSRN Working Paper 3385502.

Theodos, Brett, Brady Meixell, and Carl Hedman. 2018. "Did States Maximize Their Opportunity Zone Selections? Analysis of the Opportunity Zone Designations." Urban Institute Brief, May 21.

U.S. Department of Treasury. 2018. "Treasury, IRS Announce Final Round of Opportunity Zone Designations." U.S. Department of Treasury, June 14. https://home.treasury.gov/news/press-releases/sm0414.

Table 1. Descriptive Statistics for Sample Tracts, 2013-2019

	Treated Tracts (Opportunity Zone Tracts)							
	2013	2014	2015	2016	2017	2018	2019	All Years
Adult Population	3115	3151	3165	3195	3193	3207	3219	3178
	(1587)	(1583)	(1636)	(1652)	(1680)	(1680)	(1747)	(1653)
Resident Employment	1584	1631	1664	1715	1729	1758	1795	1697
	(946.9)	(970)	(1001)	(1053)	(1073)	(1076)	(1134)	(1040)
Resident Employment Rate	0.501	0.5103	0.5185	0.5267	0.5312	0.5391	0.5471	0.5248
	(0.1379)	(0.1353)	(0.1362)	(0.1397)	(0.1424)	(0.1436)	(0.1475)	(0.1412)
Resident Poverty Rate	0.3144	0.3066	0.2919	0.2752	0.2614	0.2544	0.2415	0.2779
	(0.1735)	(0.1744)	(0.1692)	(0.1718)	(0.1738)	(0.172)	(0.1701)	(0.174)
Resident Average Earnings	28340	28860	30230	31470	32700	34270	35770	31660
	(15730)	(10880)	(11570)	(12750)	(13140)	(14120)	(14670)	(13600)
N	7600	7600	7600	7600	7600	7600	7600	
	Control Tracts (Other Low Income Communities)							
	2013	2014	2015	2016	2017	2018	2019	All Years
Adult Population	3173	3203	3232	3249	3269	3281	3283	3241
	(1542)	(1555)	(1597)	(1617)	(1649)	(1663)	(1699)	(1619)
Resident Employment	1720	1762	1801	1835	1864	1886	1910	1825
	(975.8)	(999.6)	(1033)	(1059)	(1097)	(1105)	(1147)	(1063)
Resident Employment Rate	0.5357	0.543	0.5494	0.5568	0.5613	0.5663	0.573	0.5551
	(0.133)	(0.1325)	(0.133)	(0.1362)	(0.1385)	(0.1396)	(0.1413)	(0.1369)
Resident Poverty Rate	0.2551	0.2525	0.241	0.2226	0.2152	0.2093	0.1993	0.2279
	(0.1599)	(0.1564)	(0.1554)	(0.156)	(0.156)	(0.1554)	(0.1567)	(0.1578)
Resident Average Earnings	30480	31000	32240	33630	34820	36120	37900	33740
	(10930)	(10890)	(11850)	(13060)	(13050)	(13750)	(14850)	(12950)
N	23000	23000	23000	23000	23000	23000	23000	

Notes: Data derived from 2013-2019 American Community Survey. Standard deviations in parentheses.

Table 2. Difference-in-Differences Estimates for Resident Employment Levels

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
=1 if OZ in 2019	16.59**	17.36**	25.91***	26.02***				
	(8.25)	(8.219)	(8.68)	(8.833)				
=1 if OZ in 2018-19					10.93*	11.57*	20.34***	21.21***
					(6.224)	(6.171)	(6.48)	(6.616)
Constant	1793***	1793***	1793***	1793***	1793***	1793***	1792***	1792***
	(0.2901)	(0.2891)	(0.3053)	(0.3107)	(0.4378)	(0.4341)	(0.4558)	(0.4654)
Tract FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	No	No	No	Yes	No	No	No
State-Year FE	No	Yes	No	No	No	Yes	No	No
PUMA-Year FE	No	No	Yes	No	No	No	Yes	No
County-Year FE	No	No	No	Yes	No	No	No	Yes
N	214200	214200	214200	214200	214200	214200	214200	214200
Tracts	30600	30600	30600	30600	30600	30600	30600	30600

Notes: Data derived from 2013-2019 American Community Survey. Standard errors (in parentheses) clustered at the tract level. * p<10%, ** p<5%, ***p<1%.

Table 3. Difference-in-Differences Estimates for Resident Employment Rates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
=1 if OZ in 2019	0.005056***	0.005192***	0.005702***	.006889***				
	(0.001537)	(0.001535)	(0.001649)	(0.001646)				
=1 if OZ in 2018-19					0.005151***	0.005328***	0.005996***	0.007128***
					(0.001152)	(0.001147)	(0.00123)	(0.001232)
Constant	0.5474***	0.5474***	0.5474***	0.5474***	0.5473***	0.5473***	0.5472***	0.5471***
	(0.00005)	(0.00005)	(0.00006)	(0.00006)	(0.00008)	(0.00008)	(0.00009)	(0.00009)
Tract FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	No	No	No	Yes	No	No	No
State-Year FE	No	Yes	No	No	No	Yes	No	No
PUMA-Year FE	No	No	Yes	No	No	No	Yes	No
County-Year FE	No	No	No	Yes	No	No	No	Yes
N	214200	214200	214200	214200	214200	214200	214200	214200
Tracts	30600	30600	30600	30600	30600	30600	30600	30600

Notes: Data derived from 2013-2019 American Community Survey. Standard errors (in parentheses) clustered at the tract level. * p<10%, ** p<5%, *** p<1%.

Table 4. Difference-in-Differences Estimates for Resident Average Earnings

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
=1 if OZ in 2019	-61.32	-49.7	-31.24	69.58				
	(159)	(158.4)	(168.3)	(170.8)				
=1 if OZ in 2018-19					129.3	129.6	107.3	227.0*
					(121.8)	(121.1)	(127.9)	(129.7)
Constant	33230***	33230***	33230***	33230***	33220***	33220***	33220***	33210***
	(5.591)	(5.569)	(5.92)	(6.006)	(8.569)	(8.521)	(8.994)	(9.125)
Tract FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	No	No	No	Yes	No	No	No
State-Year FE	No	Yes	No	No	No	Yes	No	No
PUMA-Year FE	No	No	Yes	No	No	No	Yes	No
County-Year FE	No	No	No	Yes	No	No	No	Yes
N	214200	214200	214200	214200	214200	214200	214200	214200
Tracts	30600	30600	30600	30600	30600	30600	30600	30600

Notes: Data derived from 2013-2019 American Community Survey. Standard errors (in parentheses) clustered at the tract level. * p<10%, ** p<5%, *** p<1%.

Table 5. Difference-in-Differences Estimates for Resident Poverty Rates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
=1 if OZ in 2019	-0.00915***	-0.00924***	-0.00965***	-0.01205***				
	(0.001929)	(0.001926)	(0.002082)	(0.002088)				
=1 if OZ in 2018-19					-0.00900***	-0.00911***	-0.00941***	-0.01129***
					(0.001464)	(0.001458)	(0.001569)	(0.00158)
Constant	0.2405***	0.2405***	0.2405***	0.2406***	0.2408***	0.2408***	0.2408***	0.2410***
	(0.00006784)	(0.00006775)	(0.00007321)	(0.00007341)	(0.000103)	(0.0001026)	(0.0001103)	(0.0001111)
Tract FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	No	No	No	Yes	No	No	No
State-Year FE	No	Yes	No	No	No	Yes	No	No
PUMA-Year FE	No	No	Yes	No	No	No	Yes	No
County-Year FE	No	No	No	Yes	No	No	No	Yes
N	214200	214200	214200	214200	214200	214200	214200	214200
Tracts	30600	30600	30600	30600	30600	30600	30600	30600

Notes: Data derived from 2013-2019 American Community Survey. Standard errors (in parentheses) clustered at the tract level. * p<10%, ** p<5%, *** p<1%.

Table 6. Propensity Score Matching Treatment on the Treated Estimates

	(1)	(2)	(3)	(4)
	Employment	Employment Rate	Avg. Earnings	Poverty Rate
OZ	23.56	0.00387	434.9	-0.00654
	(17.83)	(0.004219)	(353.2)	(0.004721)
Observations (Tracts)	15200	15200	15200	15200

Notes: Data derived from 2013-2019 American Community Survey. Standard errors (in parentheses) clustered at the tract level. * p<10%, ** p<5%, *** p<1%.

Table 7. Event Study Estimates, Propensity Score Matched Sample

	(1)	(2)	(3)	(4)
	Employment	Employment Rate	Avg. Earnings	Poverty Rate
2013	3.158	0.00159	32.15	-0.005102
	(13.03)	(0.00262)	(275.1)	(0.003526)
2014	-10.25	-0.001927	144.7	-0.0009826
	(13.03)	(0.002604)	(242.9)	(0.003523)
2015	-6.562	0.001165	-4.099	-0.001793
	(12.89)	(0.002601)	(256.8)	(0.003482)
2016	2.938	0.001568	337.8	-0.00411
	(13.11)	(0.002596)	(260.9)	(0.003476)
2017	-	-	-	-
2018	-5.574	0.000517	599.1**	0.003838
	(13.6)	(0.002634)	(268.9)	(0.003492)
2019	20.4	0.00228	402.8	-0.001438
	(13.8)	(0.002719)	(277.7)	(0.003554)
Constant	1697***	0.5294***	31450***	0.2779***
	(4.341)	(0.0008579)	(87.9)	(0.001136)
Tract FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
N	106400	106400	106400	106400
Tracts	15200	15200	15200	15200

Notes: Data derived from 2013-2019 American Community Survey. Standard errors (in parentheses) clustered at the tract level. * p<10%, ** p<5%, *** p<1%.

Figure 1. Event Study Estimates for Resident Employment Levels

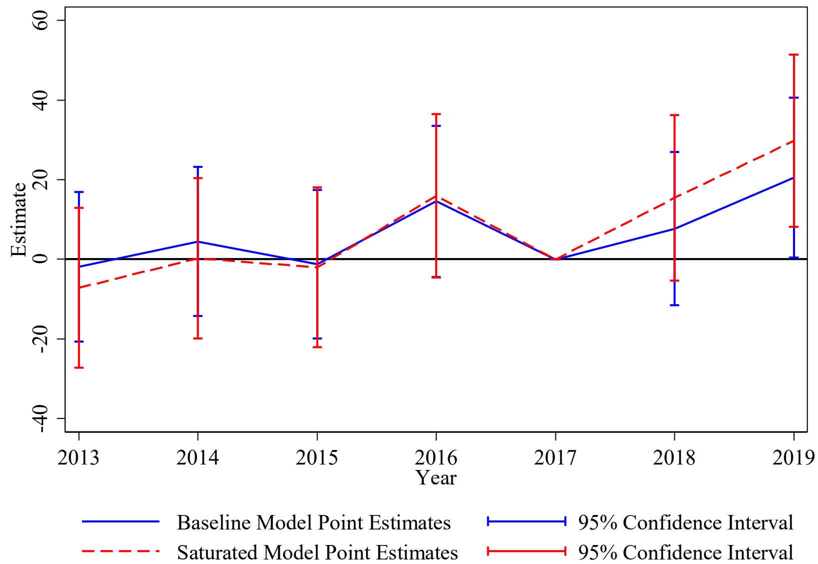


Figure 2. Event Study Estimates for Resident Employment Rates

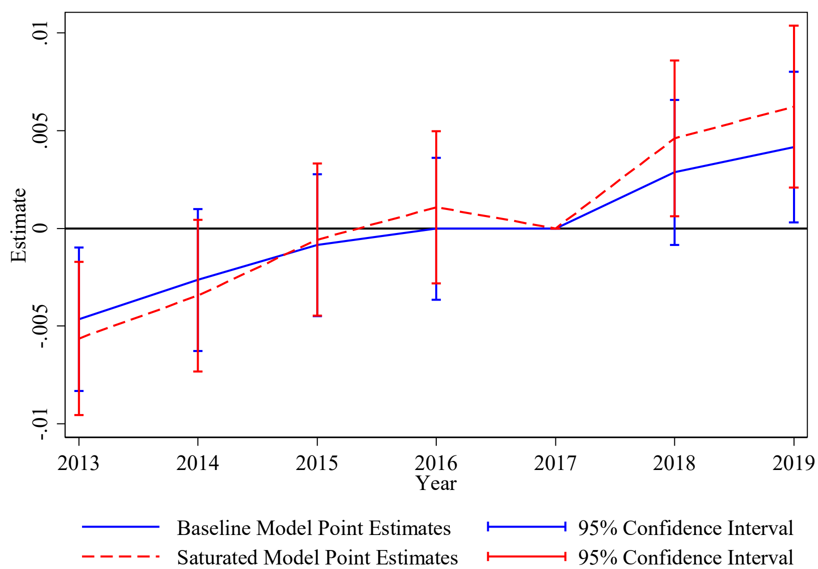


Figure 3. Event Study Estimates for Resident Average Earnings

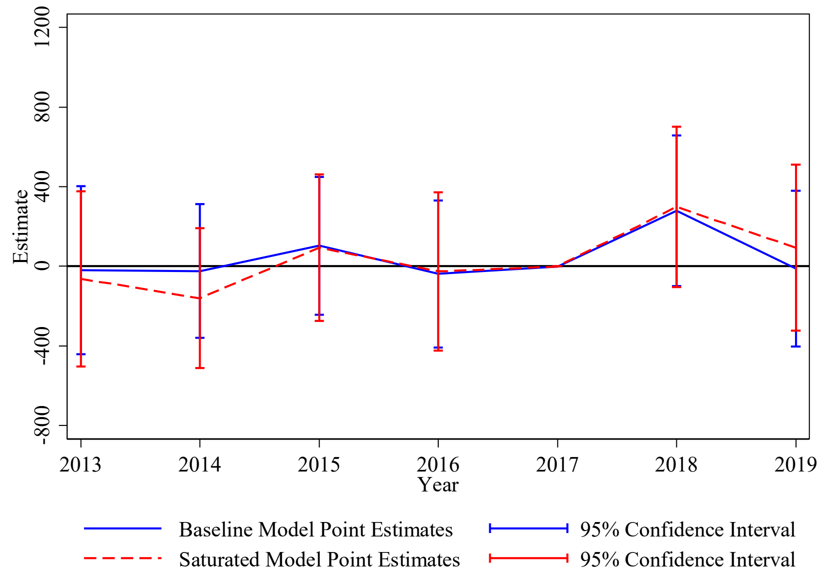


Figure 4. Event Study Estimates for Resident Poverty Rates

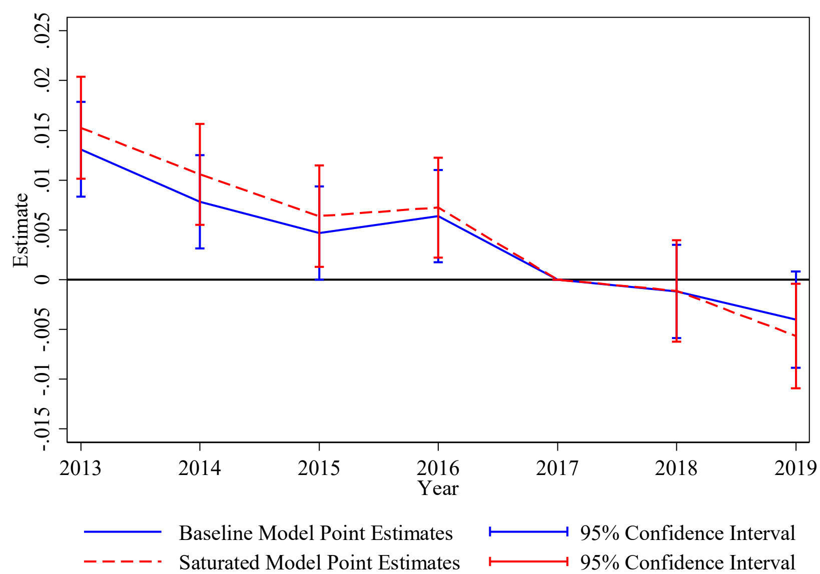


Figure 5. Matching-Based Event Study Estimates for Resident Employment

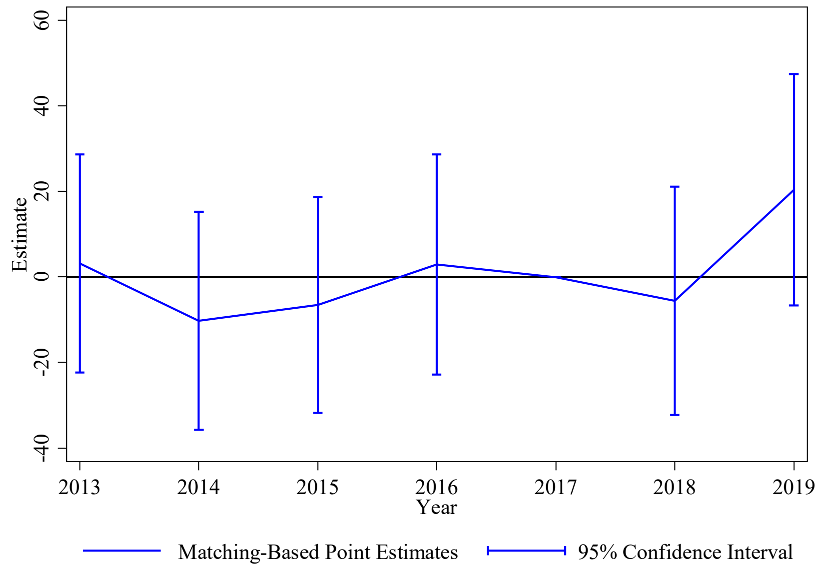


Figure 6. Matching-Based Event Study Estimates for Resident Employment Rates

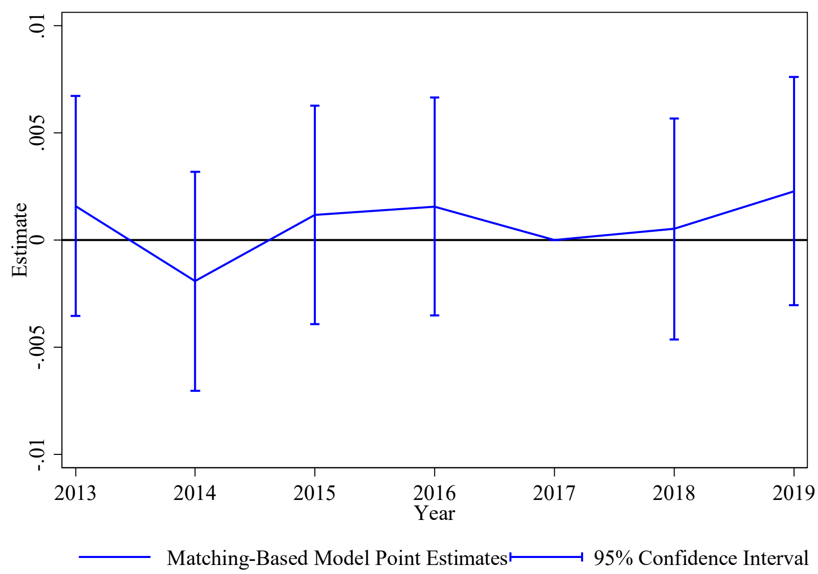


Figure 7. Matching-Based Event Study Estimates for Resident Average Earnings

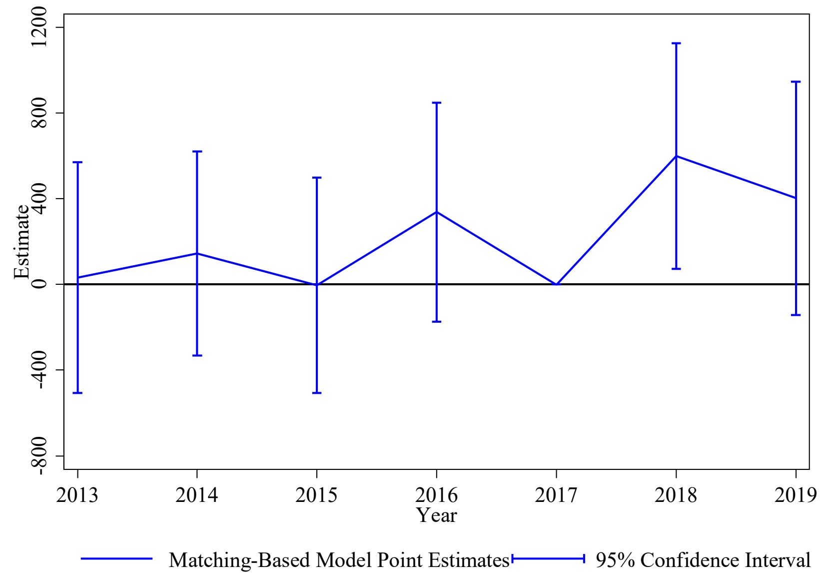
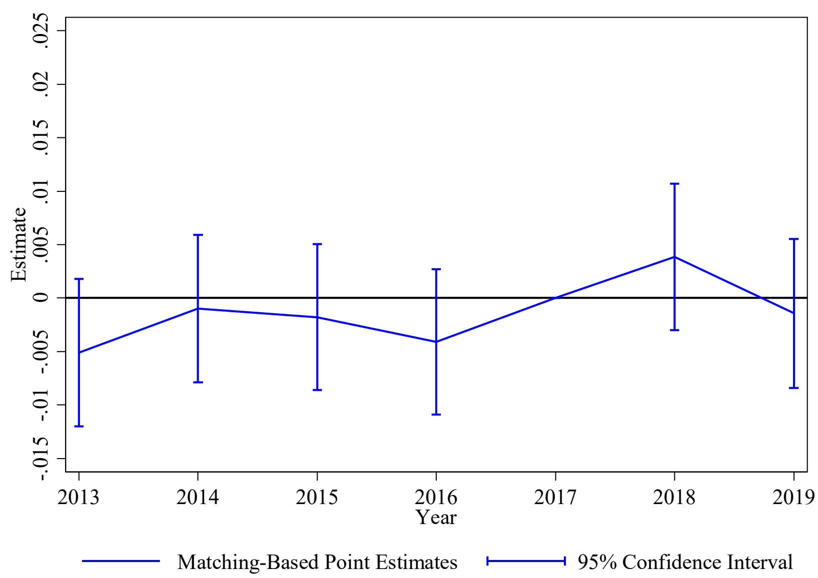


Figure 8. Matching-Based Event Study Estimates for Resident Poverty Rates



Appendix

Table A1. Difference-in-Differences Estimates Using PS Outcome Measure

	(1)	(2)	(3)	(4)
	Employment	Employment Rate	Avg. Earnings	Poverty Rate
OZ	18.71	-0.00048	-29.79	0.00907**
	(16.94)	(0.003312)	(348.6)	(0.004204)
Constant	-96.93***	-0.01392***	-1260***	0.02404***
	(8.611)	(0.001585)	(154.6)	(0.001958)
Observations (Tracts)	30600	30600	30600	30600

Notes: Data derived from 2013-2019 American Community Survey. Standard errors (in parentheses) clustered at the tract level. * p<10%, ** p<5%, ***p<1%.