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JUE INSIGHT: THE IMPACTS OF OPPORTUNITY ZONES ON ZONE RESIDENTS

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ABSTRACT

The Opportunity Zone program, created by the Tax Cuts and Jobs Act in 2017, was designed to encourage investment in distressed communities across the U.S. We examine the early impacts (up to one-and-a-half years after enactment) of the Opportunity Zone program on residents of targeted areas. We leverage restricted-access microdata from the American Community Survey and employ a matching approach to estimate causal reduced-form effects of the program. Our results point to little or no evidence of positive effects of the Opportunity Zone program on the employment, earnings, or poverty of zone residents.

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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 place-based policy: "Opportunity Zones." Opportunity Zones are targeted at disadvantaged census tracts and are intended to spur economic development. Opportunity Zone incentives are directed at investors in property, allowing deferral or avoidance of federal taxes on capital gains from 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. We estimate effects for tracts designated as Opportunity Zones using a control group of eligible, but not designated tracts matched on the basis of trends in outcomes prior to the program's introduction. With our data, we can study effects up to about one-and-a-half years after enactment of the zones.

Overall, we find limited evidence that Opportunity Zone designation has positive effects on the economic conditions of local residents. Our preferred estimates based on an inverse probability weighting (IPW) approach point to effects of Opportunity Zone designation that are economically small and generally statistically indistinguishable from zero. Specifically, we estimate that following Opportunity Zone designation, employment rates of residents do not change, with statistically insignificant yet fairly precise estimates that are very near zero. We can rule out increases in employment rates larger than 0.2 percentage point with 95% confidence. Estimated effects on average earnings of employed residents of designated tracts are positive, but are economically small and not consistently statistically significant. We additionally find that, if anything, zone designation is associated with a slight increase in local poverty rates, although the evidence is largely consistent with no effect.

Notably, a difference-in-differences approach that ignores differential pre-designation trends implies positive effects on zone resident employment rates and reductions in poverty rates, with effects that are both statistically significant and economically meaningful. The problem of differential pre-designation trends is also apparent from an event-study analysis. Hence, an approach that assumes that zone selection was orthogonal to tracts' economic trajectories gives the misleading impression of substantial positive effects of zone designation on residents.

Given that Opportunity Zone designations were first announced in 2018, we are at the beginning of research on the impacts of the program.[1] In recent work, Arefeva et al. (2021) 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 significantly (by 3.0 to 4.5 percentage points), with the growth spread across industries. Atkins et al. (2021) 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. They find only limited evidence of any effects of zone designation on job postings or posted salaries.

Other recent work has studied the effects of Opportunity Zone designations on investment

[1] Earlier work on the federal New Markets Tax Credit, which is the most similar prior place-based policy, found a positive impact on investment, mainly via real-estate investment, coupled with a modest and costly poverty reduction effect (Freedman, 2012). Lester et al. (2018) discuss the similarities and differences between the New Markets Tax Credit and Opportunity Zones.

and real estate markets. While Corinth and Feldman (2021) find no impacts of zone designation on commercial investments, Sage et al. (2021) document significant positive effects on the prices of some types of commercial properties. Frank et al. (2020) also find positive effects of Opportunity Zone designation on commercial real estate transactions, building permits, and construction employment. However, Chen et al. (2019) find little effect of Opportunity Zone designation on residential property prices.

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 being based largely on the economic circumstances of residents – the impacts of the program on residents is of paramount importance. Past work on place-based policies suggests 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 (Gelfond and Looney, 2018; Eastman and Kaeding, 2019). Our data on the economic conditions 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., Freedman, 2012; 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. We first leverage our detailed data to establish that tracts designated as Opportunity Zones were on different trajectories, particularly with respect to employment and poverty rates, than tracts eligible but not designated. These differential trends undermine a simple difference-in-differences approach to estimating the causal effects of Opportunity Zone designation using eligible but not designated tracts as a control group. We therefore use a refined comparison group, assigning weights to control tracts to better match the evolution of outcomes for treated tracts prior to Opportunity Zone designation. By addressing differences in underlying trends in outcomes across designated and non-designated (but eligible) tracts, our matching approaches deliver 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). The Opportunity Zone program provides preferential tax treatment for capital gains from investments in certain designated census tracts. There are a number of tax benefits associated with investing in Opportunity Zones: temporary deferment of taxes owed on realized capital gains from liquidating an asset if those gains are invested in businesses or real estate in Opportunity Zones, a basis step-up for realized capital gains that are reinvested in Opportunity Zones, and non-taxation of capital gains on Opportunity Zone investments if those investments are held for ten years or more (Theodos et al., 2018; Internal Revenue Service, 2020).

The TCJA legislation provided for designation of Opportunity Zones in the 2018 tax year. It allowed governors to designate as Opportunity Zones up to 25% of census tracts in their state that qualified as "low-income communities" (LICs), as well as some tracts contiguous with LICs.

An LIC is a tract with a poverty rate of at least 20% or 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). Tracts within a federal Empowerment Zone, with population below 2,000, and contiguous with one or more LICs are also LICs. By law, 95% of Opportunity Zone tracts had to be LICs; governors were permitted to choose some additional tracts to designate as Opportunity Zones if the tracts were contiguous with an LIC and had 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 – 31,864 LICs and 10,312 non-LIC contiguous tracts. Nationwide, governors selected 8,762 tracts as Opportunity Zones; 97% (8,532) were LICs while only 3% (230) were non-LIC contiguous tracts. States made their designations by June 2018 (Theodos et al., 2018; U.S. Department of Treasury, 2018).

Figure 1 maps Opportunity Zones across the United States. The map suggests that Opportunity Zones are diffusely distributed geographically, in contrast to the more focused approach taken in, for example, the federal Empowerment Zone program (Busso et al. 2013). Prior work suggests that place-based policies can be more effective if they are carefully targeted at distressed areas where there is the potential to generate agglomeration externalities (Glaeser and Gottlieb, 2008; Moretti, 2010). However, evidence on the selection process for Opportunity Zones points to only limited attention to such strategic considerations. Theodos et al. (2018) find that tracts selected as Opportunity Zones were more economically distressed than other eligible tracts, but that there was only a limited amount of targeting toward more disadvantaged neighborhoods with lower access to capital. Similarly, Alm et al. (2021) find that selection processes for Opportunity Zones were generally not sophisticated, and may have in fact favored areas that were already better positioned to grow in the future. Some evidence points to political favoritism in governors' zone selections (Alm et al., 2021; Frank et al., 2020; Eldar and Garber, 2020), but there is also evidence indicating 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 (Frank et al., 2020; Atkins et al., 2021) – consistent with our evidence.

According to the Internal Revenue Service, Opportunity Zones represent a tool "to spur economic development and job creation in distressed communities" (Internal Revenue Service, 2020). In principle, the program could affect employment in designated tracts, even in the short run. The funds flowing into qualified areas under the program can finance a wide variety of projects, including infrastructure, commercial or industrial real estate, and new or existing businesses. Some of these projects might be associated with new, albeit transitory, construction employment. Others may be associated with more durable jobs in different industries. Still others may be labor neutral or even labor displacing (Patrick, 2016; Brachert et al., 2019; Criscuolo et al., 2019).

Given that our data cover a relatively short period after the creation of Opportunity Zones, it is important to consider the possible dynamics of the effects of the policy. It is conceivable that there will be more meaningful changes in zone economic conditions as more Opportunity Zone capital is deployed in the future. Conceptually, however, we would expect the short-run effects on employment and wages to be larger than the long-run effects. Opportunity Zones could generate some immediate job growth from luring investment to an area. In the longer run we would expect the tax benefits to be capitalized into land values, increasing property prices and driving employment rates and real wages back toward their equilibrium levels, although this can be mediated by agglomeration and multiple equilibria (Glaeser and Gottlieb, 2008; Moretti, 2010; Bartik, 2020). Evidence indicates that long-run effects of one-time increases in local job opportunities do in fact tend to be smaller than short-run effects (Freedman, 2017; Garin, 2019). Recent research on U.S. as well as French enterprise zones also finds no evidence of growing effects many years after zone incentives are created (Neumark and Kolko, 2010; Gobillon et al., 2012; Givord et al., 2018; Neumark and Young, 2021).

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.[2] To construct outcomes, we take advantage of restricted-access American Community Survey (ACS) 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 three main outcome measures: the employment-to-population ratio for residents, average earnings of employed residents, and the poverty rate for residents. We aggregate the individual

[2] See https://www.cdfifund.gov/opportunity-zones.

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.[3]

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 nationwide (3% of the total). Including the complete set of non-LIC contiguous tracts in the sample would, at least for the event-study and difference-in-differences analyses, 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 Zones to (a rounded) 7,600 tracts, and our sample of eligible but not designated tracts to (a rounded) 23,000.[4]

We conduct our main analyses using this sample of tracts for the 2013-2019 period. Descriptive statistics for our sample, broken out by year and for designated Opportunity Zone tracts as well as eligible but not designated tracts, 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 (e.g., Theodos, 2018; Frank et al., 2020).

[3] We also exclude from the analysis Puerto Rico, where all eligible LICs were designated as Opportunity Zones.
[4] 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.

4. Empirical Approach

We begin by using an event-study framework to assess the comparability of designated Opportunity Zones and eligible but not designated tracts in terms of pre-existing trends in employment, earnings, and poverty. Our basic model is

$$y_{it} = \sum_{j=2013}^{2016}\{\beta_j^{pre} \times OZ_i \times 1[j = t]\} + \sum_{k=2018}^{2019}\{\beta_k^{post} \times OZ_i \times 1[k = t]\} + \gamma_i + \eta_t + \varepsilon_{it} \qquad (1)$$

In equation (1), y_{it} 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 Opportunity Zones and eligible but not designated LICs. The tract fixed effects in the model (γ_i) control for time invariant tract characteristics that could be correlated with Opportunity Zone designation and also independently affect outcomes.[5] The year fixed effects in the model (η_t) control for factors changing each year that are common to all tracts in the sample. Finally, β_j^{pre} and β_k^{post} are the estimated "effects" of Opportunity Zones for each pre- and post-treatment year.[6] These are measured relative to 2017.

In some of the event-study specifications, we additionally include county-by-year fixed effects in an effort to account for potentially differential trends in outcomes across geographies at a higher level of aggregation than census tracts. This more saturated model effectively narrows 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.

[5] The tract fixed effect also subsumes the main effect for OZ_i.
[6] We put effects in quotes because in the pre-treatment period, at a minimum, we do not think of the estimates as capturing causal effects.

As a point of comparison with our preferred estimates based on matching methods described below, we also run the simple difference-in-differences version of equation (1), in which β_j^{pre} is constrained to be zero and we estimate a single β_k^{post} parameter (where the post-period is defined as 2018-2019 or, in a robustness check, as only 2019). For the event-study and difference-in-differences analyses, we cluster standard errors at the tract level, which allows for arbitrary patterns of heteroskedasticity across tracts and serial correlation within tracts.

Motivated by the results of our event-study analyses, which point to violations of the parallel trends assumption necessary to interpret the difference-in-differences estimates as causal, we proceed to construct a control group using a data-driven approach to weight potential comparison tracts. Our preferred results use inverse probability weighting (IPW) as well as the doubly-robust inverse probability weighted regression adjustment method. For these approaches, we construct the dependent variable as simply the average of the outcome for each tract over the post-designation period (defined as 2018-2019 or, in a robustness check, as only 2019) minus the average over the pre-designation period (defined as 2013-2017 or, for the robustness check, as 2013-2018).[7] We want to compare this outcome for treated (Opportunity Zone) and control (eligible but not designated) tracts. With IPW, an estimate of the unobserved counterfactual of the average outcome for the treated tracts, if Opportunity Zone designation had not occurred, is constructed as a weighted average across non-treated tracts, where the weights are the inverse of the probability that the tract was not treated, adjusted for the probability of treatment.[8] These

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[7] By construction, regressing this differenced dependent variable on a dummy for Opportunity Zone designation for our tract-level sample of LICs delivers identical point estimates as a standard difference-in-differences regression run on a tract-by-year sample with tract and year fixed effects (see columns (1), (3), and (5) of Appendix Table A3).

[8] The expression for the weights for the non-treated tracts is $\frac{\hat{p}}{1-\hat{p}}$, where $\hat{p}$ are the predicted probabilities from Equation (2) – described below.

weights are estimated from a logit model, for which the underlying linear model for the latent variable is:

$$OZ_i^* = \alpha + \sum_{\tau=2013}^{2017}\{\rho_\tau emp_{i\tau} + \varphi_\tau earn_{i\tau} + \omega_\tau poverty_{i\tau}\} + v_i \qquad (2)$$

That is, we predict Opportunity Zone designation for all tracts in our sample of LICs based on each tract's employment rate (*emp*), average earnings (*earn*), and poverty rate (*poverty*) in each year between 2013 and 2017 (i.e., over the entire pre-treatment period). Thus, the most weight is put on the non-treated tracts with the highest estimated probability of being treated based on the observables. The assumption is that the expected value of the weighted average of the outcome for the non-treated tracts equals the expected value of the outcome for the treated tracts if they were not treated, which is more plausible because we are using as controls tracts on trajectories more comparable to those of the treated tracts.

While the IPW method above models the treatment, regression adjustment methods allow us to model the outcome to account for non-random treatment assignment. Regression adjustment methods construct counterfactuals by fitting linear regression models separately for the treated group and the control group and using the predicted values of the outcome for a given covariate vector as estimates of the potential outcomes. Averaging the covariate-specific treatment effect across treated tracts using these fitted values yields the ATT estimate. The regression-adjusted IPW method uses the IPW weights in the estimation of the regression adjustment models to estimate corrected regression coefficients, and therefore combines these two approaches. This estimator has the virtue of being "doubly robust." That is, it provides a consistent estimate as long as *either* the inverse probability weighting *or* the regression adjustment removes the confoundedness of treatment with unobservables – although both approaches, as this implies, use selection on observables.[9] In our application of regression-adjusted IPW, we model both the outcome and the treatment using the same set of covariates.[10]

As we illustrate below, using the IPW and regression-adjusted IPW approaches, 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. Event Study Estimates

In Figure 2, we show event-study estimates for the three main outcomes discussed above. We show results based on models with just tract and year fixed effects as well as models including tract and county-by-year fixed effects. The graphs show point estimates and 95% confidence intervals. In each case, 2017 is the reference year; the figures report the interactions of OZ_i and the year dummy variables, with the interaction with the dummy variable for 2017 omitted, as in Equation (1).

Event-study results for the employment rate of residents appear in Panel A of Figure 2. There is clear evidence of a differential pre-treatment trend in employment rates for those areas designated as Opportunity Zones relative to those areas eligible but not designated. In particular, relative to employment rates in other eligible tracts, employment rates in designated areas were trending upward prior to 2017, and the higher relative employment rates after 2017 appear to reflect merely the continuation of that trend.

[9] Tan (2010) provides a detailed explanation of these estimators.
[10] With our matching approaches, the unit of observation is the census tract. We calculate heteroskedasticity robust standard errors following Wooldridge (2010).

In Panel B of Figure 2, we see limited evidence of any Opportunity Zone effects on resident average earnings, although there is also less indication of a strong differential pre-trend in resident average earnings. However, Panel C of Figure 2 indicates a strong relative pre-treatment trend in resident poverty rates. The results suggest that, compared to poverty rates in other eligible areas, poverty rates in tracts that were designated as Opportunity Zones were already declining prior to designation, and that the post-treatment changes may reflect the continuation of the prior trend rather than the causal effect of Opportunity Zone designation. The clear violation of parallel trends, particularly for employment and poverty rates, would call into question any causal interpretation of results from a standard difference-in-differences approach that compares changes in outcomes for tracts designated Opportunity Zones to those eligible but not designated Opportunity Zones.[11]

In light of the differential pre-treatment trends in outcomes and the resulting bias that would arise in a difference-in-differences framework that ignored these trends, we implement a matching strategy that balances treatment and control tracts on the pre-designation evolution of outcomes. We turn to our matching-based results in the next section.

5.2 Estimates using IPW

In this section, we present results from matching Opportunity Zone tracts to eligible but not designated tracts based on pre-treatment trends in outcomes. We begin by illustrating in Figure 3 that our IPW approach (without further regression adjustment) eliminates the differential pre-2017 trends in outcomes that existed for designated vs. all other eligible tracts. By design, our IPW approach ensures parallel trajectories in outcomes for designated Opportunity Zones and

[11] For this reason, we present unadjusted difference-in-differences estimates only in the Appendix (see Appendix Table A3).

our (weighted) group of non-designated but eligible LIC tracts prior to 2017.[12] The contrast with the event study estimates from Figure 2 (reproduced in Figure 3 for comparison) is clear.

Estimates of the effects of treatment on the treated using our IPW and regression-adjusted IPW approaches appear in Panels A and B of Table 2. For these results, the Opportunity Zone indicator takes a value of one for designated tracts in 2018 and 2019, and zero otherwise. We report estimates in which we define the post-treatment period as only 2019 in Appendix Table A2; the results in that case are very similar, with some exceptions regarding statistical significance noted below.

Our IPW estimates in Table 2 indicate that Opportunity Zone designation has no meaningful effect on the employment rates of residents on average (column (1)). The point estimates from both our IPW and regression-adjusted IPW approaches imply miniscule (negative) effects on employment rates. Based on these estimates, we can rule out with 95% confidence an effect size for employment rates larger than 0.2 percentage point.

Our estimates of the effects of Opportunity Zone designation on average annual earnings, reported in column (2) of Table 2, are also economically small, but are positive and statistically significant. The IPW estimates of around $350 represent 1% of the 2017 mean of average earnings in designated tracts (see Table 1). When we define the post-treatment period as 2019 only, the point estimates are smaller and not statistically significant (see Appendix Table A2).

Finally, we find that Opportunity Zone designation does not reduce poverty rates (and may even increase them slightly). In Table 2, the IPW and regression-adjusted IPW estimates of the treatment on the treated imply that designation is associated with a 0.4-0.6 percentage point (approximately 2%) increase in resident poverty rates. While the effect sizes are small, their

[12] Descriptive statistics on the weights are reported in Appendix Table A1. The large positive skewness indicates a long right tail, indicating that the weight is concentrated in a smaller number of control tracts.

significance implies that we can statistically rule out with 95% confidence any reduction in poverty due to Opportunity Zone designation. When we define the post-treatment period as only 2019, the IPW estimates for poverty rates remain positive but are less precise; in that case, the regression-adjusted IPW estimate is not statistically significant. Overall, then, a true effect of on poverty of zero or very close to zero is fully consistent with the estimates.

By way of comparison, Appendix Table A3 reports standard difference-in-differences estimates for the effects of zone designation on employment rates, average earnings, and poverty rates. For employment, the naïve difference-in-differences results suggest substantial gains from Opportunity Zone designation, with estimates indicating that zone designation is associated with a statistically significant 0.5 percentage point increase in resident employment rates. But as we saw from Figure 2, these estimates are misleading, as they reflect differential pre-existing trends between designated zones and eligible but not designated tracts; the IPW results in Table 2 indicate a much different conclusion, with a quite precise estimate that rules out material employment gains from Opportunity Zones. The estimated earnings effects in Appendix Table A3 are more similar to those we obtain using IPW, which is not surprising given the lack of a pronounced pre-treatment trend in this outcome apparent in Figure 2. For poverty rates, like employment rates, the difference-in-differences estimates generate spurious evidence of beneficial effects from Opportunity Zone designation; specifically, they indicate statistically significant reductions in poverty rates of about 1 percentage point (4%). The pre-trends in Figure 2, however, imply that the IPW estimates showing no effect or, if anything, a slight increase in poverty rates following zone designation are more credible.

6. Conclusion

We provide early evidence on the impacts of Opportunity Zone designation on residents of zones, estimating effects 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. We show that credible evidence on the effects of zone designation requires using a control group of tracts matched on the basis of trends in outcomes prior to the program's introduction.

Using an inverse probability weighting approach, we find little if any evidence of positive effects of Opportunity Zone designation on the economic conditions of residents of targeted neighborhoods. Our estimates of the effects of Opportunity Zone designation on employment rates are statistically indistinguishable from zero and sufficiently precise to rule out material increases. We find at best modest positive effects on the average earnings of zone residents. And we find no evidence that Opportunity Zones reduce local poverty rates.

Other recent studies have found mixed evidence on the effects of Opportunity Zones on labor markets. While Atkins et al. (2021) document limited effects of zone designation on job postings, Arefeva et al. (2021) find that zone designation is associated with significant job growth. Evidence of job growth in Opportunity Zones does not necessarily contradict our results. As previously shown in the context of Empowerment Zones (Busso et al., 2013) and the New Markets Tax Credit (Freedman, 2015), the effects of place-based policies could be different for jobs in the zones vs. employment of zone residents.

Institutional features of the Opportunity Zone program may be militating against meaningful positive impacts of the program on residents of targeted communities (Gelfond and Looney, 2020). The program lacks several features of place-based policies that have been found to be associated with stronger job creation effects, including directly incentivizing hiring, focusing the incentives on new hires rather than windfalls, and facilitating the recapture of tax benefits if job creation or investment goals are not met.[13] In addition, the Opportunity Zone selection process did not ensure that communities most in need were the ones ultimately eligible to receive tax-advantaged investments. Indeed, as our results suggest, many of the communities designated as Opportunity Zones likely would have experienced improvements in economic conditions even in the absence of zone designation.

The Joint Committee on Taxation (2019) estimated that through 2023, the Opportunity Zone Program would cost approximately $3.5 billion each year in foregone tax revenues. The White House Council of Economic Advisors (2020) estimated that Qualified Opportunity Funds had raised as much as $75 billion in private capital by the end of 2019, although how much has been invested in Opportunity Zones is currently unclear (U.S. Government Accountability Office, 2020). We find limited evidence of any impacts of zone investment to date on zone residents.

An important limitation of our study is that our estimates are "early," in the sense of extending only one-and-a-half years since Opportunity Zones were officially designated. It is possible that there will be more meaningful changes in zone economic conditions as more Opportunity Zone capital is deployed in the future. However, prior evidence from other place-based programs suggests that the long-run effects of the program are unlikely to be larger than the short-run effects (Neumark and Kolko, 2010; Gobillon et al., 2012; Givord et al., 2018; Neumark and Young, 2021). Additionally, given that the 2020 data will include a year strongly affected by the COVID-19 pandemic, with effects also extending into 2021, the data through

[13] See Neumark and Grijalva (2017) for evidence on features of hiring credits that more likely lead to net job creation, and Freedman et al. (2021) for evidence of positive job creation effects from an economic development policy that includes these features.

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.

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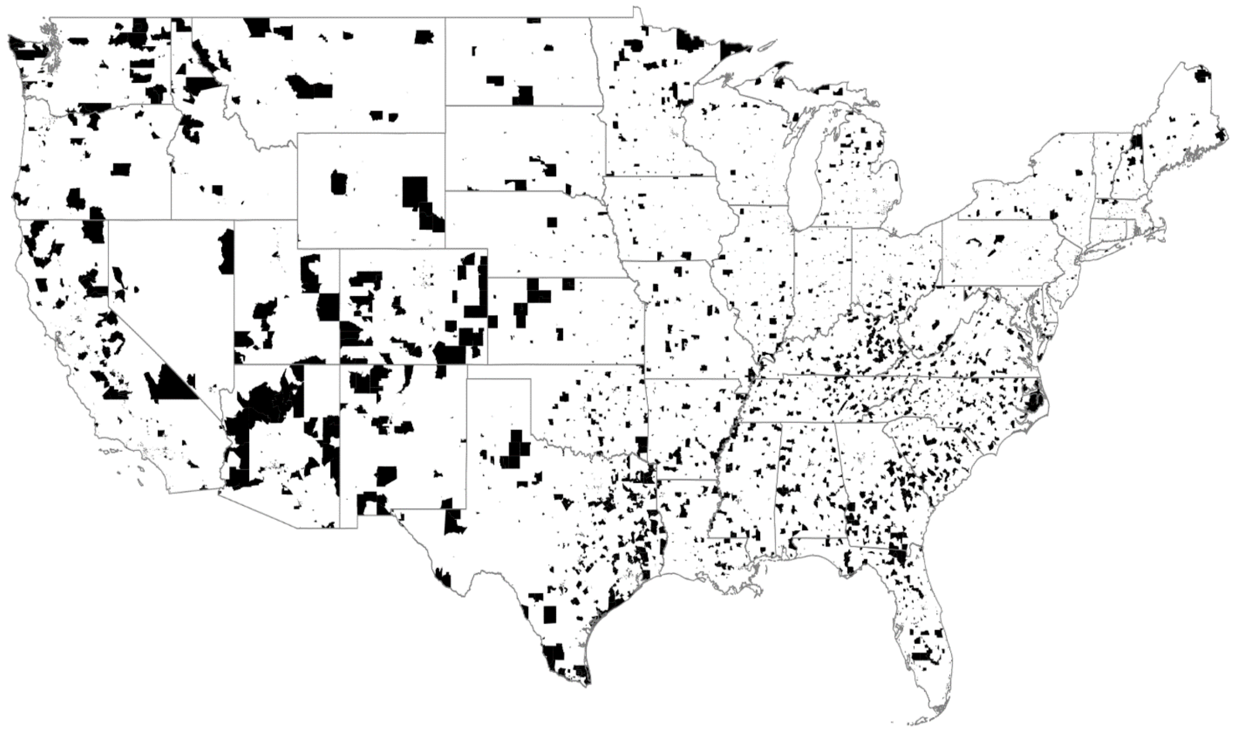
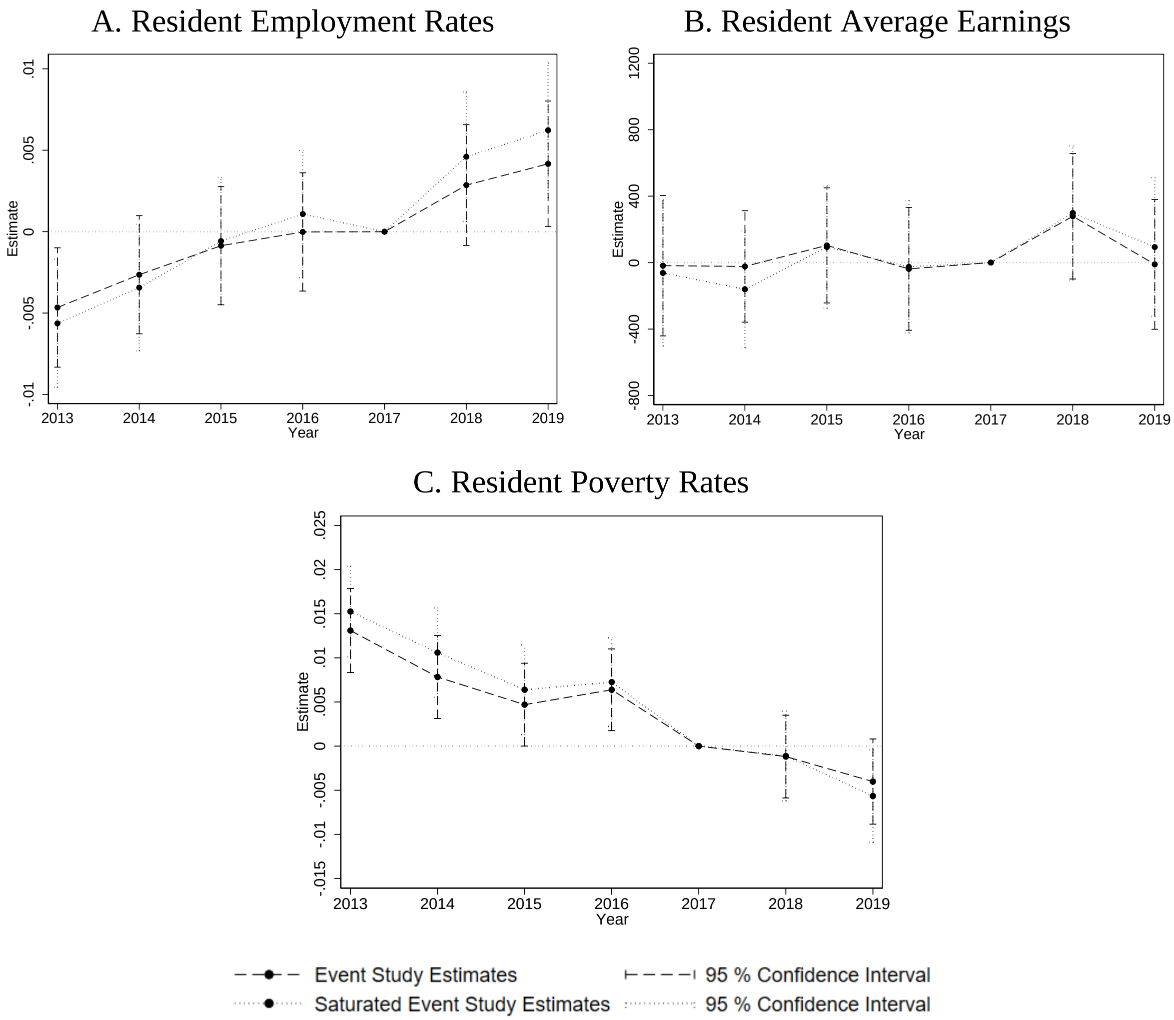
Figure 1. Opportunity Zones



Notes: Shaded areas are census tracts designated as Opportunity Zones. Information on Opportunity Zones are from the Community Development Financial Institutions (CDFI) Fund at the U.S. Department of the Treasury.

Figure 2. Event Study Estimates for Resident Outcomes



Notes: Data derived from the 2013-2019 American Community Surveys. The panels show point estimates and 95% confidence intervals from event studies comparing outcomes in Opportunity Zones to outcomes in eligible but not designated LICs. The dashed lines show results from an event study with only tract and year fixed effects. The dotted lines show results from an event study that additionally includes county-by-year fixed effects. 2017 is the omitted year. The confidence intervals are based on standard errors clustered at the tract level.

Figure 3. Event Study Estimates with Alternative Weighting Schemes

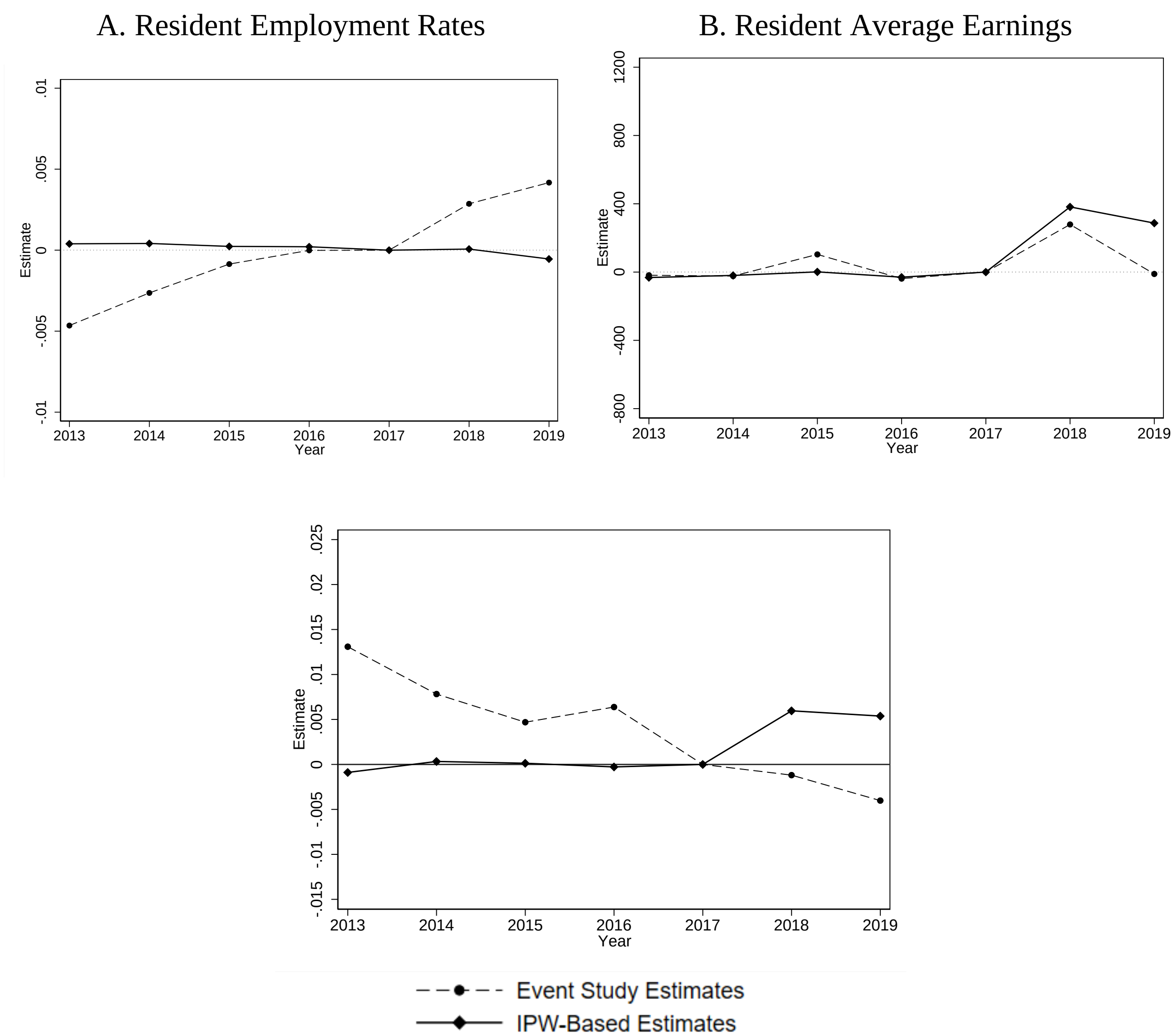


Table 1. Descriptive Statistics for Opportunity Zones and Other Low-Income Communities, 2013-2019

	2013	2014	2015	2016	2017	2018	2019	All Years
A. Treated Tracts (Opportunity Zone Tracts)								
Adult Population	3115	3151	3165	3195	3193	3207	3219	3178
	(1587)	(1583)	(1636)	(1652)	(1680)	(1680)	(1747)	(1653)
Resident Employment Rate	0.5010	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)
Tracts	7600	7600	7600	7600	7600	7600	7600	
B. Potential Control Tracts (Other Low-Income Communities – Eligible but Not Designated)								
	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 Rate	0.5357	0.5430	0.5494	0.5568	0.5613	0.5663	0.5730	0.5551
	(0.1330)	(0.1325)	(0.1330)	(0.1362)	(0.1385)	(0.1396)	(0.1413)	(0.1369)
Resident Poverty Rate	0.2551	0.2525	0.2410	0.2226	0.2152	0.2093	0.1993	0.2279
	(0.1599)	(0.1564)	(0.1554)	(0.1560)	(0.1560)	(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)
Tracts	23000	23000	23000	23000	23000	23000	23000	

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

Table 2. Estimates of the Effects of Opportunity Zones on Residents

	(1)	(2)	(3)
	Employment Rate	Average Earnings	Poverty Rate
	A. IPW Treatment on the Treated Estimates		
Opportunity Zone	-0.0005	350.4***	0.0058***
	(0.0012)	(124.9)	(0.0017)
Tracts	30600	30600	30600
	B. Regression-Adj. IPW Treatment on the Treated Estimates		
Opportunity Zone	-0.0002	337.9***	0.0043***
	(0.0012)	(124.0)	(0.0016)
Tracts	30600	30600	30600

Notes: Data derived from the 2013-2019 American Community Surveys. The Opportunity Zone variable takes a value of one for designated tracts in 2018 and 2019. Panel A reports inverse probability weighted (IPW) estimates of the treatment on the treated. Panel B reports doubly robust regression-adjusted IPW estimates of the treatment on the treated. Standard errors in parentheses are heteroskedasticity robust. Statistically significant at * 10%, ** 5%, *** 1%.

Appendix Table A1. Summary Statistics for the Inverse Probability Weights Assigned to the Control Tracts

	IPW Weights
Mean	0.3294
Std. Dev.	0.1991
Skewness	5.42
Kurtosis	68.12
Control Tracts	23000

Appendix Table A2. IPW and Regression-Adjusted IPW Estimates using Opportunity Zone Post-Treatment Period Defined as 2019

	(1)	(2)	(3)
	Employment Rate	Average Earnings	Poverty Rate
	A. IPW Treatment on the Treated Estimates		
Opportunity Zone	-0.0008	236.6	0.0045**
	(0.0016)	(162.9)	(0.0022)
Tracts	30600	30600	30600
	B. Regression-Adj. IPW Treatment on the Treated Estimates		
Opportunity Zone	-0.0004	213.9	0.0030
	(0.0016)	(162.1)	(0.0021)
Tracts	30600	30600	30600

Notes: Data derived from the 2013-2019 American Community Surveys. The Opportunity Zone variable takes a value of one for designated tracts in 2019. The pre-treatment period is defined as 2013-2018. Panel A reports inverse probability weighted (IPW) estimates of the treatment on the treated. Panel B reports doubly robust regression-adjusted IPW estimates of the treatment on the treated. Standard errors in parentheses are heteroskedasticity robust. Statistically significant at * 10%, ** 5%, *** 1%.

Appendix Table A3. Unadjusted Difference-in-Differences Estimates of the Effects of Opportunity Zones on Residents

	(1)	(2)	(3)	(4)	(5)	(6)
	Employment Rate		Average Earnings		Poverty Rate	
	A. Post-Treatment Period Defined as 2018-2019					
Opportunity Zone	0.0052***	0.0071***	129.3	227*	-0.0090***	-0.0113***
	(0.0012)	(0.0012)	(121.8)	(129.7)	(0.0015)	(0.0016)
	B. Post-Treatment Period Defined as 2019					
Opportunity Zone	0.0051***	0.0069***	-61.32	69.58	-0.0092***	-0.0121***
	(0.0015)	(0.0016)	(159)	(170.8)	(0.0019)	(0.0021)
Tract Fixed Effects	Y	Y	Y	Y	Y	Y
Year Fixed Effects	Y		Y		Y	
County-by-Year Fixed Effects		Y		Y		Y
Tracts	30600	30600	30600	30600	30600	30600
Observations	214200	214200	214200	214200	214200	214200

Notes: Data derived from the 2013-2019 American Community Surveys. Panel A reports difference-in-differences estimates with the post-treatment period defined as 2018-2019. Panel B reports difference-in-differences estimates with the post-treatment period defined as only 2019. The odd-numbered columns show estimates from models with tract and year fixed effects. The even-numbered columns show results from models with tract and county-by-year fixed effects. Standard errors in parentheses are heteroskedasticity robust and clustered at the tract level. Statistically significant at * 10%, ** 5%, *** 1%.