A Longer-Run Evaluation of the Employment Effects of Opportunity Zones

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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 United States. Very early research found no evidence of impacts of the program on employment, earnings, or poverty of zone residents, but some evidence of positive effects on employment among businesses in zones. Using the latest survey-based as well as administrative data, we adopt a longer-run and more holistic perspective on the intended and unintended labor market impacts of OZs. We find that OZ designation increases job creation among businesses within zones. However, newly created jobs in zones are largely taken by residents of higher-income census tracts rather than residents of the OZ or residents of other non-OZ low-income tracts. Correspondingly, we find limited impacts on zone resident employment rates, earnings, or poverty rates. To the extent that there are job gains among OZ residents, those jobs are concentrated in other, non-LIC tracts. Moreover, newly created jobs in zones are offset nearly one-to-one by declines in nearby low-income tracts.

Keywords: Place-Based Policies, Opportunity Zones, Tax Incentives, Employment, Poverty

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I. Introduction

Opportunity Zones (OZs) were created in the Tax Cuts and Jobs Act of 2017 and became effective in 2018. As a result of the creation of OZs, 8,764 Census tracts in the United States offer investors substantial tax advantages in the form of capital gains tax reductions or eliminations for investments in the zones. Although data are sparse, estimates suggest that the tax expenditures on the OZ program are large – on the order of $8.2 billion for 2020-2024 and likely to grow going forward.[1] As such, not only are OZs one of the newest place-based policies in the United States, but their scale far surpasses that of prior comparable policies.[2] The original OZ tax benefits were slated to end in 2026, but the program was recently renewed, with some changes including sunsetting of existing OZs and the designation of new ones.[3]

In this paper, we extend and enrich prior work on the OZ program's impacts on targeted areas. We take advantage of multiple data sources, including both survey-based data (the American Community Survey, or ACS) and administrative data (the LEHD Origin-Destination Employment Statistics, or LODES), and adopt a longer-run and more holistic perspective than previous papers. Using an inverse probability weighting (IPW) approach that leverages institutional rules for tract eligibility, we find that the program increased job creation among businesses in targeted areas. However, newly created jobs in targeted tracts are largely taken by residents of higher-income census tracts rather than residents of the tract where the jobs were created, or residents of other non-OZ low-income tracts. Correspondingly, we find limited impacts on zone resident employment rates, earnings, or poverty rates. To the extent that there

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[1] See https://www.urban.org/urban-wire/what-we-do-and-dont-know-about-opportunity-zones.
[2] For example, spending on Empowerment Zones and Enterprise Communities between 1994 and 2004 is estimated at about $1 billion (https://crsreports.congress.gov/product/pdf/R/R41639/5).
[3] For a discussion of changes to and extensions of the OZ program in the new tax legislation, see Wessel (2025).

are job gains among OZ residents, those jobs are concentrated in other, non-LIC tracts. Moreover, newly created jobs in zones are offset nearly one-to-one by declines in nearby low-income tracts. Overall, while the OZ program may have increased the number of jobs located in designated zones, its impacts on employment on aggregate, and on employment specifically among individuals with low incomes, have been comparatively small.

There has been renewed interest in place-based policies in recent years, spurred at least in part by research on the critical role place plays in determining lifetime economic outcomes (Chetty et al., 2014) as well as on how place-based programs can complement other policies to aid in redistribution and create positive externalities by improving neighborhoods (Gaubert et al., 2021). This impetus for place-based policies has been further amplified by recent work pointing to decreases in geographic mobility that, in the past, may have previously led people and families to move to regions with greater job opportunities (Austin et al., 2018; Zabek, 2024), although this work is at a more aggregate geographic level than most place-based policies. Moreover, there is some evidence that policymakers have adapted place-based programs based on lessons learned from research highlighting limitations of prior place-based policies and the potential ways in which the poor design of those policies limited their benefits (Freedman and Neumark, 2024).[4]

While there may be some cause for optimism, there are reasons to be skeptical of the OZ program's potential benefits for targeted areas. First, place-based policies, in general, have not proven very effective. Neumark and Simpson (2015) provide an extensive review of the evidence on place-based programs pre-dating OZs and highlight the many factors that have impeded

[4] As perhaps the best example, research on the California Competes Tax Credit (CCTC) – a prominent job creation incentive program in California, which has a place-based flavor, and which replaced the state's ineffective enterprise zone program – points to substantial positive effects on jobs (Freedman et al., 2023a; Hyman et al., 2023).

programs' effectiveness. As Freedman and Neumark (2024) discuss, it is unclear why many of those factors would not be equally problematic for OZs.

Second, OZs do not directly incentivize hiring, but instead incentivize investment, and there is evidence that much of this investment may be going into real estate, often for housing that does not benefit the intended beneficiaries – like housing for college students who, because of their low incomes, make some tracts appear quite poor (Wessel, 2021). The lessons from other place-based policies that focus more on real estate and other investments are also not positive. Most notably, Freedman (2012, 2015) studied the New Markets Tax Credit (NMTC), viewed by some as the closest precursor to OZs, and found only limited evidence of positive impacts of NMTC-subsidized investment on neighborhood poverty and income levels.[5]

Third, like many past enterprise zone (EZ) programs, OZs create "by-right" eligibility for tax incentives. That is, they establish eligibility based on geographic location, but firms or other agents meeting these criteria can claim the tax benefits if they invest, and there is no role for program administrators to exercise discretion as to which investments are eligible for incentives.[6] This setting and past evidence suggest that windfalls might be pervasive in the OZ program, as, for example, real estate investors already planning to invest in an OZ can earn tax incentives even when the policy induces little or no change in their behavior. Indeed, as Corinth and Feldman (2024) describe, the structure of the OZ program is such that tax benefits are largest for investment that would have happened in the absence of the program.

Fourth, OZs may merely shift the locations of planned investments. The geographic

[5] Lester et al. (2018) and Corinth et al. (2025) discuss the similarities and differences between the New Markets Tax Credit and Opportunity Zones.

[6] As a notable contrast, not only does the CCTC program in California directly incentivize hiring, but it also provides program administrators discretion in awarding tax credits to businesses. These features, along with the recapture of credits that can occur when awardees fail to meet pre-specified investment and hiring milestones, likely contributed to the CCTC's relative effectiveness.

granularity at which OZs are defined (census tracts) may increase the scope for relocation. Such displacement might lead to reduced hiring and investments in proximate areas, which, given the high degree of spatial correlation in poverty, could be similarly low-income neighborhoods. Negative spillovers owing to business displacement have been documented in the context of federal Empowerment Zones (Hanson and Rohlin, 2013) among other programs (Freedman and Neumark, 2024). However, to the extent that the OZ program successfully induces investment in targeted neighborhoods, it is possible that there could be agglomeration effects that positively impact nearby communities.[7]

The a priori negative assessment of OZs' efficacy was largely substantiated in the earliest research evaluating the program.[8] The critical limitation of this earlier research, however, was just that – it was early. OZ advocates have argued, possibly justifiably, that the existing research simply does not cover a long enough period to accurately gauge the effects of OZs.[9] Early research also tended to focus on only a single dimension of the program's effects – for example, its effects on job creation by businesses, or its effects on employment among zone residents – without more comprehensively considering its myriad potential impacts.

While the OZ program's design does not appear conducive to generating large positive impacts on targeted communities, it remains an open question whether a longer-term perspective points to more beneficial effects of OZs. On the one hand, there are reasons to believe that short-run effects on employment and wages would be larger than the long-run effects. OZs might generate some immediate job growth from luring construction or other investment to an area,

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[7] Using different data and a shorter time horizon than us, Arefeva et al. (2025) find that OZs had significant positive spillovers on employment and establishment growth in immediately adjacent tracts, but that any agglomeration effects decay quickly with distance.
[8] This evidence is discussed in Section III.
[9] For example, see https://eig.org/wp-content/uploads/2023/03/Examining-the-Latest-Multi-Year-Evidence-on-Opportunity-Zones-Investment.pdf.

whereas in the longer run, the tax benefits might be capitalized into land values, increasing property prices and driving employment rates and real wages back toward their equilibrium levels. However, these latter forces might be mediated by agglomeration and multiple equilibria (Glaeser and Gottlieb, 2008; Moretti, 2010; Bartik, 2020). Indeed, some evidence indicates that one-time increases in local job opportunities can have persistent impacts on communities; for example, Garin and Rothbaum (2025) find lasting effects of WWII-era investments on population and benefits to lower-income households.

Moreover, there may have been more meaningful changes in zone economic conditions as more OZ capital was deployed in communities in years following enactment. With the effects of the pandemic subsiding and larger OZ projects underway, it is possible that the positive effects of the program have only emerged more recently.

This brings us to the motivation for this paper. With data available now, we can provide a comprehensive assessment of the effects of OZs on employment, covering a period extending well beyond the pandemic and thus providing more definitive evidence. Using a combination of ACS and LODES data, we provide new visibility into the longer-run impacts of the OZ program on neighborhoods. We also shed new light on the extent to which investments subsidized by the program have had positive or negative spillovers in nearby tracts, as well as whether it yielded benefits for residents of low-income communities as opposed to more affluent areas. Our results provide important insights into the effects – intended or otherwise – of the OZ program, and more broadly speak to the efficacy of such programs in improving economic opportunities in disadvantaged communities.

II. The Opportunity Zone Program

The OZ program was introduced as part of the 2017 Tax Cut and Jobs Act (TCJA). The OZ program offers preferential tax treatment for capital gains stemming from investments in specific designated census tracts. The tax benefits associated with investing in OZs include temporary deferment of taxes owed on realized capital gains from liquidating an asset if those gains are invested in businesses or real estate in OZs, a basis step-up for realized capital gains that are reinvested in OZs, and non-taxation of capital gains on OZ investments if those investments are held for at least ten years (Theodos et al., 2018; Internal Revenue Service, 2020).

The TCJA legislation gave authority to state governors to designate as OZs up to 25% of census tracts in their state that qualified as "low-income communities" (LICs), as well as some tracts adjacent to LICs. An LIC is a census 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 (statewide for rural tracts). Also included among LICs are tracts within a federal Empowerment Zone, tracts with population below 2,000, and tracts adjacent to one or more LICs. By law, 95% of OZ tracts were required to be LICs; state governors were allowed to select some additional tracts to designate as OZs if those tracts were adjacent to an LIC and had median income less than 125% of the median income of the LIC with which it was adjacent.

Overall, 42,176 tracts were eligible to be OZs. These included 31,864 LICs and 10,312 non-LIC adjacent tracts. Governors selected 8,762 tracts as OZs. Of those selected, 8,532 (97%) were LICs while 230 (3%) were non-LIC adjacent tracts. States announced their designations by June 2018 (Theodos et al., 2018; U.S. Department of Treasury, 2018).

Figure 1 provides a map of OZs in the contiguous United States. As the map shows, OZs are widely dispersed geographically. While past evidence suggests that place-based policies tend to be more effective when carefully targeted (Glaeser and Gottlieb, 2008; Moretti, 2010; Freedman and Neumark, 2024), the selection process for OZs was hurried and may have been influenced by political as much as economic considerations (Frank et al., 2020; Alm et al., 2021; Eldar and Garber, 2023; Corinth and Feldman, 2024).

Under the recent tax legislation, OZ tax benefits for current zones sunset in 2026, and a new set of zones will be created in 2027, with governors then slated to pick new zones every 10 years subsequently (Wessel, 2025). Even if, at this point, it appears that the original program will live on, there are still questions to answer about what the benefits are, their incidence, and more, which can inform the designation of new zones. Furthermore, continuing the OZ program or not is a choice policymakers always have, which should be informed by evidence. Answers about the efficacy of the program based on a longer-run perspective with the data now available could well differ from the earliest evidence based on the experience only a year or two after OZ benefits took effect.

III. Earlier Evidence

Early research on the OZ program yielded mixed results, but most studies pointed to relatively modest effects of the program on targeted communities. For example, an early analysis by Freedman et al. (2023b) focused on the impact of OZ designation on resident employment. Freedman et al. used restricted-access microdata from the American Community Survey (ACS) for 2013-2019 to explore the program's impacts at a geographically granular level, estimating effects for tracts designated as OZs using a control group of eligible, but not designated, tracts matched on the basis of trends in outcomes prior to the program's introduction. The available data permitted estimation of the effects of OZs up to about one-and-a-half years after enactment of the zones.

Overall, Freedman et al. (2023b) find limited evidence that OZ designation had positive effects on the economic circumstances of local residents. The preferred estimates based on an inverse probability weighting (IPW) approach point to effects of OZ designation that are economically small and generally statistically indistinguishable from zero. For example, following OZ designation, employment rates of residents did not change, with statistically insignificant yet fairly precise estimates that are very near zero; the estimates can rule out increases in employment rates larger than 0.2 percentage point with 95% confidence. Estimated effects on median earnings of employed residents of designated tracts are positive but are economically small and not consistently statistically significant. Meanwhile, they find that zone designation was associated with a slight increase in local poverty rates, although the evidence is largely consistent with no effect.

Notably, as Freedman et al. (2023b) highlight, selected and non-selected OZs were on different trajectories prior to OZ enactment. In particular, tracts that were selected tended to be improving along various economic dimensions. Therefore, a difference-in-differences approach that ignores differential pre-designation trends suggests positive effects on zone resident employment rates and reductions in poverty rates. That is, 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, because in fact zone designation was associated with already-improving economic circumstances of residents.

Several other studies of the OZ program have focused on employment-related outcomes, including some that have considered impacts on employment measured at the workplace, as opposed to employment impacts for residents. For example, Atkins et al. (2023) find limited evidence of increases in online job postings in OZs, and Shen (2024) finds no evidence of employment growth or small business formation associated with OZs in New York City. However, Arefeva et al. (2025) find evidence of increases in job growth among businesses in OZs in metropolitan areas, with large estimated impacts (3.0 to 4.5 percentage point increases in the two-year growth rate). Arefeva et al.'s main results rely on the YourEconomy Time Series data, but they also find positive, albeit smaller, effects on workplace employment when they use LODES data (which we also utilize in our analysis). Rupasingha and Davis (2024) also document positive effects of OZ designation on resident employment using the LODES for 2009-2019. However, not only do their data end in 2019, but they also face challenges in establishing parallel trends.

Other work has focused on outcomes beyond employment. Wheeler (2023), for example, finds an increase in building permits in OZs in larger cities. However, Corinth and Feldman (2023) and Sage et al. (2023) find evidence of only limited effects of OZ designation on commercial real estate markets. Snidal and Li (2024) also find no indication that OZ incentives affect home or business lending. Similarly, Nagpal (2022) finds no effects of OZ designation on small business lending in Chicago. Meanwhile, Chen et al. (2023) and Alm et al. (2024) find no evidence that OZs increased real estate prices, consistent with limited anticipated local benefits from OZ designation.

A core limitation of prior research that this paper addresses is that, as noted above, most previous studies use data that end within 2-3 years of the OZ program's introduction. Arefeva et al. (2025) use the YourEconomy Time Series through 2021. Atkins et al. (2023) use Burning Glass data through March 2020, and ACS 5-year files for 2015-19 and 2016-20. Chen et al. (2023) consider Federal Housing Finance Agency house price data for 2018-2020. Freedman et al. (2023b) study ACS data through 2019. Nagpal (2022) uses loan data in Chicago through 2020. Rupasingha and Davis (2024) employ LODES data through 2019. Sage et al. (2023) study commercial real estate transactions data through 2019. Snidal and Li (2024) use small business and residential loan origination data also through 2019. Shen (2024) deploys InfoGroup historical directories of small businesses in New York City through 2023 – the one exception with more recent data, although in a limited application. Our data extend through 2023 and cover the whole country.

IV. Data and Outcomes

Our data on tracts eligible and designated as OZs come from the Community Development Financial Institutions (CDFI) Fund at the U.S. Department of Treasury.[10] Designated tracts appear in Figure 1.

We use American Community Survey (ACS) data for 2013-2023 to examine the effects of OZs on residents of designated areas. We study four main outcome measures: the employment-to-population ratio for residents, median earnings of employed residents, the poverty rate for residents, and employment levels for residents (the last for a more direct comparison with outcomes measured in other data).

We also use the LEHD Origin-Destination Employment Statistics (LODES) for 2013-2022 for the same tract-level analysis. The LODES are derived from state unemployment insurance tax records and thus cover the near universe of workers in the United States. Moreover, the LODES permit us to conduct a year-by-year analysis. The LODES data specifically allow us to measure the number of resident jobs, workplace jobs, and commuting flows by tract and year. The commuting flows give us information (which we will also be able to corroborate later confidential ACS data) on the residential tracts of people working in OZs and comparison tracts. As described below, we use these origin-destination data to ask whether jobs created in OZs tend to go disproportionately to residents of the same tract, residents of other LICs, or residents of non-LICs (the latter being relatively more affluent areas). We use all primary jobs in the LODES data.[11]

[10] See https://opportunityzones.hud.gov/home.
[11] This corresponds to "JT01" in the LODES data. We use LODES 8, for which the latest release was October 2024. We use NHGIS correspondence files to aggregate 2020-vintage block-level data in the LODES to 2010-vintage tract level data. Data for Alaska are not available after 2016, Mississippi after 2018, and Michigan after 2021.

For our main analysis, we restrict attention to designated and eligible tracts that are LICs. Limits on how many non-LIC contiguous tracts could be chosen as OZs, as well as a tendency to designate more distressed tracts, led to only 230 non-LIC contiguous tracts being designated (3% of all OZs). Including non-LIC contiguous tracts in the sample would entail using a disproportionate number of higher-income tracts as controls. These tracts are less comparable to the final set of designated tracts. Overall, our sample restrictions result in a sample of about 7,500 designated OZ tracts and 23,000 non-designated LICs.[12]

We leverage the most recent data available for our analysis. The current ACS data extend through 2023, and the current LODES data extend through 2022. Descriptive statistics for the (unweighted) sample of non-OZ LICs and OZ LICs appear in the first four columns of Table 1. Panel A shows means (and standard deviations) for pre- and post-treatment outcomes measured in the ACS, while Panel B shows the same for pre- and post-treatment outcomes measured in the LODES. The pre-treatment period is 2013-2017 in both datasets, but the post treatment period is slightly shorter in the LODES than the ACS (2019-2022 vs. 2019-2023).

In level terms, prior to OZ implementation (i.e., over 2013-2017), LICs that were designated OZs exhibited greater disadvantage than LICs that were not designated; for example, OZs had lower employment rates, lower median earnings, and higher poverty rates. They also tended to be in more urban areas, as indicated by the relatively high workplace employment in OZs relative to non-OZs (as evidenced in the LODES data). These patterns are consistent with findings in past studies (e.g., Theodos et al. 2018). While worse off in levels, however, Freedman et al. (2023b) show that OZs were on stronger economic trajectories, which we confirm below in the LODES data.

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

Much of our analysis will eventually rely on restricted-access ACS data for 2013-2023 and the Longitudinal Business Database (LBD) for 2013-2022 (through likely 2024 and 2023 respectively by the final version of this paper), which we are accessing in the UCI Federal Statistical Research Data Center (FSRDC). As discussed in Freedman et al. (2023b), the advantage of the restricted-access ACS data is that we can measure outcomes at the tract-level on an annual basis. However, because of disclosure requirements that preclude repeated releases as the analysis evolves, *for now* we are reporting results based only on public-use data.

V. Empirical Approach

A. Core Approach

The starting point of our empirical analysis is an event-study framework to estimate the impacts of OZ designation, relying on comparisons to tracts eligible but not designated as OZs. When using the LODES data, for which we have annual data, the basic model is:

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

In this equation, 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 OZ and 0 if it is eligible but not; recall that the sample is restricted to designated OZs 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 OZ designation and independently affect outcomes.[13] 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 OZs for each pre- and post-treatment year.[14] These

[13] The tract fixed effect also subsumes the main effect for OZ_i.
[14] "Effects" is in quotes because we do not think of the estimates as capturing causal effects in the pre-treatment period.

are measured relative to 2017. We cluster standard errors at the tract level, which allows for arbitrary patterns of heteroskedasticity across tracts and serial correlation within tracts.

For the ACS analyses for which we have only the 5-year averages (based on public-use data), we cannot do a yearly event study. We instead estimate a simple difference-in-differences model with one 5-year pre-treatment and one 5-year post-treatment observation for each treated and control tract. We also report estimates of this model for the LODES, partly for comparability, and partly because with fewer impact parameters there is an efficiency gain (relative to the LODES estimates using annual observations). In this case, defining $POST_i$ as a dummy variable equal to one after the OZ program is enacted, the model simplifies to

$$y_{it} = \beta^{post} \times OZ_i \times POST_t + \gamma_i + \eta_t + \epsilon_{it}$$

When we estimate this model, we use the ACS 5-year files from 2013-2017 and 2019-2023, to incorporate the most recent data possible. We hence omit 2018, the year OZ designations were announced and when many policy details remained unclear. We do the same when we estimate this model using the LODES, to be comparable.[15]

B. Selection and Parallel Trends

Prior work (e.g., Freedman et al., 2023b) pointed to violations of the parallel trends assumption in the pre-treatment period, with OZ designation being associated with prior economic improvements in tracts.[16] We thus construct a control group using a data-driven approach to weight potential comparison tracts. Following Freedman et al. (2023b), we use inverse probability weighting (IPW) as well as the doubly robust inverse probability weighted

[15] Because the program took effect in 2018, one might view that year as "partially treated." For the event study using annual data one can simply interpret the estimates for 2018 via this lens (indeed the evidence reported below sometimes indicates smaller effects in 2018), while 2019 and after are "fully treated." For the two-period models, we want to exclude 2018 from the "post" period, and hence simply omit it.
[16] As described below, we also present evidence from the ACS data using the 5-year average for 2008-2012, to provide some evidence on pre-trends despite using 5-year averages.

regression adjustment method. When estimating the doubly robust inverse probability weighted regression adjustment method, we rely on the methods developed in Sant'Ana and Zhao (2020) and generalized in Callaway and Sant'Anna (2021).

We want to control for counterfactual changes in employment in treated (OZ) and control (eligible but not designated) tracts. With IPW, we construct an estimate of the unobserved counterfactual of the average outcome for the treated tracts, if OZ designation had not occurred, as a weighted average across non-treated tracts. The weights are the inverse of the probability that the tract was not treated, adjusted for the probability of treatment.[17] We estimate these weights from a logit model, for which the underlying linear model for the latent variable (OZ^*) is:

$$OZ_i^* = \alpha + \sum_{t=2013}^{2017} y_{it} + v_i$$

That is, we predict OZ designation for all tracts in our sample of LICs based on each tract's outcomes between 2013 and 2017 (i.e., over the entire pre-treatment period). The most weight will be put on the non-treated tracts with the highest estimated probability of being treated based on the path of the pre-treatment observable. In effect, we use as controls tracts that are on trajectories more comparable to those of the treated tracts, making it more plausible that the expected value of the weighted average of each outcome for the non-treated (eligible but not designated) tracts equals the expected value of that outcome for the treated (designated OZ) tracts if they were not treated. Note that we construct a separate set of weights for each outcome for which we estimate the model.

[17] 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 the OZ selection equation described just below.

This description of our approach is completely accurate for the analysis of the LODES data, which are annual. The LODES data feature more prominently in this paper than the ACS data, not only because of their higher frequency but also because they allow us to examine both workplace and resident employment at the tract level. For the ACS data, we simply use data for the 2013-2017 and 2019-2023 periods in our main analysis. As a consequence, we can match on 2013-2017 levels, but not changes. In a supplemental analysis, we confirm past work pointing to differential pre-treatment trends in ACS-measured outcomes by incorporating an earlier wave of the ACS data.[18]

In one robustness test, we additionally include state-by-year fixed effects in our models to absorb differential changes over time in outcomes across geographies at a higher level of aggregation than census tracts, due, perhaps, to state-level policy changes, impacts of the pandemic, etc.[19] Note that the addition of state-by-year fixed effects is limited to the outcome model. The cross-sectional treatment model used to calculate the propensity weights is not affected. In another robustness check, we winsorize the propensity weights, excluding control tracts in the top and bottom 5 percentiles of treatment propensity. The purpose of this exercise is to confirm that IPW results are not being driven by extreme weighting on a few influential observations.

The IPW method models the treatment. Regression adjustment methods further allow us to model the outcome in order to account for non-random treatment assignment. Regression adjustment methods construct counterfactuals by fitting separate linear regression models for the

[18] See Appendix Table A2, discussed further below. Following Freedman et al. (2023b), we will be able to match on the yearly evolution of these outcomes using the confidential ACS data in subsequent versions of this paper.

[19] We could further saturate the model with city-by-year or county-by-year fixed effects. While these richer sets of fixed effects would limit the scope for potential unmeasured or unobservable time-varying factors to bias our estimates, they may amplify bias attributable to spillovers of OZ effects across nearby tracts, especially with county-by-year fixed effects. As we show later, this is an issue.

treated and the control groups. The predicted values of the outcome for a given set of covariates are used as estimates of the potential outcomes. By averaging the covariate-specific treatment effect across treated tracts using these predicted values, we obtain the ATT estimate. The regression-adjusted IPW method incorporates the IPW weights to estimate corrected regression coefficients, effectively combining both approaches. This estimator is considered "doubly robust," meaning that it provides consistent estimates as long as either the inverse probability weighting or the regression adjustment eliminates bias due to unobservables. Both methods, however, rely on selection based on observables.[20] In our application of regression-adjusted IPW, we model both the outcome and the treatment using the same set of covariates. We rely on Callaway and Sant'Anna's (2021) generalization of doubly robust methods to multiple time period settings. By using the IPW and regression-adjusted IPW methods, we can more confidently attribute changes in outcomes after OZ designation to the program itself, rather than to continuations of pre-existing trends.

We apply our weighting methods to examine all outcomes from both the ACS and the LODES. The final two columns of Table 1 show the effects of the IPW-based reweighting on our effective control group of non-OZ LICs.[21] While the goal of the reweighting is to match pre-treatment trends in outcomes, it also leads to a sample that, prior to OZ implementation, is much more similar in levels to the treated sample as well. That is, our matching procedure largely eliminates discrepancies in pre-treatment characteristics between treated tracts and control tracts.

C. Outcomes on different tracts, workers, and residents

We present estimates for a number of different outcomes, varying by both where the

[20] Tan (2010) provides a detailed explanation of these estimators.

[21] We show summary statistics for the inverse probability weights assigned to the control tracts in Appendix Table A1.

effects occur, and for whom. We begin with effects on the designated OZ tracts, estimating effects on employment of residents ("resident employment") and employment among businesses in the tract ("workplace employment"), as well as the employment rate, median earnings, and poverty rate of tract residents. We then study effects of OZ designation on employment in the tract, but characterizing tract workers based on where they live: in the tract, in other LIC tracts, or in non-LIC tracts. Next, we flip this around, studying effects on employment of residents of OZ tracts, but characterizing their jobs based on location: in the tract, in other LIC tracts, and in non-LIC tracts. Finally, we turn to evidence on spillovers, estimating effects of OZ designation on jobs in the OZ tract, in other LIC tracts adjacent to OZ tracts, and on all tracts adjacent to OZ tracts.

VI. Results

A. Effects on Employment, Poverty, and Earnings of OZ Residents, and on Tract ("Workplace") Employment

We begin by estimating simple difference-in-differences models for employment, poverty, and other outcomes in OZ tracts, measured over a longer time frame than previous work (through 2023 for ACS variables, and 2022 for LODES variables). These models compare changes in each outcome pre- vs. post-2018, for designated OZ tracts and non-designated LICs, not taking into account potential differences in trajectories prior to treatment.

The naïve regression estimates, reported in Table 2, suggest that OZ designation leads to an increase in the resident employment rate as well as reductions in the resident poverty rate, but no discernible impact on median earnings of tract residents – all based on ACS data. The magnitudes and statistical significance of these estimates closely align with those in Freedman et al. (2023b), who only considered effects through 2019. We extend their results by looking at resident and workplace employment levels. In column (vi) of the table, we find a statistically significant and economically meaningful 2.4% increase in employment in OZs relative to other LICs, based on the ACS data. In the LODES, however, we find a smaller 1.1% increase in resident employment, and no economically or statistically significant impact on workplace employment (i.e., jobs in the tract, regardless of whether held by residents or not) – indeed, the point estimate is negative.

However, prior work suggests that OZs were on different trajectories than non-OZ LICs (Eldar and Garber, 2023). This could lead to violations of parallel trends that would bias naïve difference-in-differences estimates. Using annual data from the restricted-access ACS, Freedman et al. (2023b) showed evidence of these differential trends for employment rates and poverty.[22] We validate the general pattern of differential pre-trends using a sample that also includes an earlier wave of the ACS.[23]

In Figure 2, we show event study estimates for the LODES data, which are annual.[24] Panel A shows results for log resident employment, while Panel B shows results for log workplace employment. The blue dots in each figure correspond to the naïve unweighted estimates, while the red dots correspond to the IPW-adjusted estimates and the green dots

[22] See Appendix Figure A1, which replicates Figure 3 from Freedman et al. (2023b). It shows the estimated program effects in an event-study framework using the raw data, and then using the IPW approach to match designated OZs to control tracts with similar prior trends (without further regression adjustment, which has a negligible impact). The raw data suggest sizable increases in employment and declines in poverty after OZs are designated, but also show that these apparent "effects" are just the continuation of prior trends. In contrast, the IPW approach ensures parallel trajectories in outcomes for designated OZs and the (weighted) group of non-designated but eligible LIC tracts prior to 2017.
[23] See Appendix Table A2, which shows results from a sample that incorporates an earlier wave of the ACS (for the 5-year period 2008-2012). Consistent with the differential pre-treatment trends documented in Freedman et al. (2023b), we find that employment rates rose more between 2008-2012 and 2013-2017 in OZs than in non-OZ LICs, and that poverty rates fell more between 2008-2012 and 2013-2017 in OZs than in non-OZ LICs (although the latter difference is not statistically significant).
[24] Note that the panel numbering corresponds to the column numbering in Table 2 (and Table 3, discussed next), for ease of reference.

correspond to "doubly robust" regression adjustment with IPW estimates.[25] Focusing first on the blue dots, we see distinct patterns for resident and workplace employment prior to OZ designation. Prior to designation, residential employment appears to be low but trending upwards, while workplace employment is higher in the eventually treated than the control tracts. After designation, resident employment appears to increase modestly whereas workplace employment decreases (in line with the estimates in Table 2).

Reweighting the estimates (red dots) better balances treatment and control groups on pre-treatment trends in both resident and workplace employment. In the IPW-adjusted results, we continue to see an increase in resident employment, but also simultaneously see an increase in workplace employment. The doubly robust estimates are very similar.

The two alternative sets of estimates of average treatment effect on treated tracts for these outcomes, along with the adjusted estimates for the ACS outcomes, appear in Table 3. Consistent with Freedman et al. (2023b), in the adjusted estimates (using either approach) we find no evidence of increases in the employment rate or earnings and, if anything, increases in the poverty rate of residents of OZ-designated tracts. However, the estimated positive impact on resident employment measured in the ACS persists, although it is smaller than in Table 2 (an effect of approximately 1.8%). The estimated effects in the LODES data are now consistently positive for both resident and worksite employment, with the effect being larger for worksite employment (1.2% vs. 0.7%). The larger residential employment estimate with the ACS data may well reflect the prior trends documented in Freedman et al. (2023b) and in Appendix Table A2, for which we cannot control as well with the 5-year ACS averages.[26] We thus regard the

[25] Note that because the additional regression adjustment matches on all values of prior outcomes, the green dots are mechanically on the axis at zero.

[26] Freedman et al. did not present evidence on log residential employment, but we add that to Appendix Table A2 and find the same evidence of prior trends.

LODES estimate as more reliable. The estimated effects on workplace employment are qualitatively consistent with Arefeva et al.'s (2025) results using the YourEconomy Time Series data, although our LODES estimates are roughly half the size.

The results are very similar with state-by-year controls added, which can better control for the influences of Covid (and associated policy responses) by state, as well as other state policy changes. These results are reported in Appendix Table A3, Panel A. Similarly, winsorized results, displayed in Appendix Table A3, Panel B, are statistically indistinguishable from the main IPW results. This suggests the difference between the naive and IPW results is not driven by a small number of extreme-weighted observations.[27]

One possible explanation for the evidence from the ACS data of a positive impact on resident employment (echoed in the LODES), but no impact on the employment rate, is that there is population growth in OZs, and in particular in-migration of individuals with similar socioeconomic characteristics as existing residents. This could drive increased resident employment levels, but translate into little to no effect on employment rates, median earnings, or poverty rates. There is some evidence to support this, but it is not statistically strong. In particular, estimated effects on the size of the adult civilian population are positive (about 0.4% in the adjusted estimates), but not statistically significant.[28]

B. Employment Effects Based on Tract of Residence

The evidence from the LODES suggesting stronger effects on workplace than on resident employment is consistent with some newly created jobs not going to OZ residents. To shed more light on the connection between workplace jobs and residential location, we leverage the richness

[27] This is confirmed further in Appendix Figure A2, which shows the distribution of each set of weights used. Extreme values are not apparent.
[28] See Appendix Table A4.

of the LODES origin-destination information to examine the extent to which the growth in workplace employment is driven by jobs filled by residents of the same tract, residents of other non-OZ LICs, or residents of non-LICs. The results appear in Figure 3 and Table 4. We find that the increase in workplace employment is predominantly driven by increases in jobs held by residents of non-LICs – i.e., more affluent tracts. Based on our doubly robust IPW estimates (Panel C of Table 4), for example, we find that OZ designation leads to a 1.9% increase in workplace jobs held by residents of non-LICs, compared to a 0.6% increase for residents of other LICs, and a 0.6% decrease for residents of the tract itself (the latter two estimates are not statistically significant). The evidence that the employment gains associated with OZ designation primarily benefit individuals who are not living in OZs echoes Freedman (2015), who finds that employment growth spurred by NMTC investment predominantly benefits higher-income, more-educated residents of tracts that are relatively distant from those targeted by the program.

Together with the larger estimate for resident employment in Table 3 (0.7%), these results also suggest that some of the growth in OZ resident employment occurs outside the OZ.[29] This is confirmed in Figure 4 and Table 5, which show that employment gains for OZ residents consist of jobs outside of the designated tracts, and outside LICs more generally; indeed, there are significant positive effects for employment of OZ residents in non-LIC (i.e., more affluent) tracts.

C. Spillovers

Some of the workplace employment growth that is occurring within zones could come at the expense of surrounding communities.[30] OZs target compact areas within broader labor

[29] Busso et al. (2013) report similar evidence for federal Empowerment Zones, with a large but insignificant 12.3 log point increase in non-zone jobs held by zone residents.
[30] Such displacement has been documented for other place-based policies (Einiö and Overman, 2020; Hanson and Rohlin, 2013).

markets, and employers may simply relocate investments or employment in order to take advantage of zone incentives. We explore this possibility in Figure 5 and Table 6. First, with potential spillovers in mind, we repeat the main analysis excluding LIC control tracts that border OZs (since if there are spillovers, these are the most likely to experience effects of the treatment). As shown in column (i), we still detect job gains in OZs, though the magnitude of those gains is smaller than in our main results (0.9%, in Panel C, vs. 1.2% in Table 3, Panel C). The smaller impact when we exclude adjacent LICs from the controls suggests that nearby LIC control tracts may be subject to negative spillovers.

Second, we directly investigate spillover employment effects on low-income communities and other tracts near OZs. Keeping the control group the same as in column (i), we estimate treatment effects for the LICs adjacent to OZ tracts. IPW and doubly robust methods point to an approximately 0.8% reduction in jobs in non-OZ LICs when an adjacent tract is designated as an OZ. With such tracts hosting roughly the same number of jobs as OZs (prior to treatment, in 2017, as shown at the bottom of the table), this implies a nearly one-to-one correspondence between job gains in OZs and job losses in nearby LICs.

In the final column of Table 6 (and panel (iii) of Figure 5), we expand the treatment group to include all tracts adjacent to OZs (including LICs and non-LICs), and similarly expand the control group to include all tracts adjacent to LICs that are not themselves OZs. The logic of the identification strategy carries over, but there is more heterogeneity within both the treatment and control groups. We find an even stronger negative effect – a similar-sized impact applied to a larger base. This would imply that job loss from displacement across all tracts exceeds job creation in OZs, which is in principle possible if the OZ incentives shift investment to areas where it is less productive or leads to weaker agglomeration externalities.

VII. Conclusion

The OZ program, created by the Tax Cuts and Jobs Act in 2017, was designed to encourage investment in distressed communities across the United States. We extend and enrich the existing literature on the program by studying the OZ program's longer-run effects on both resident outcomes and job growth in the designated OZ tracts. We also characterize job changes in OZs based on where workers live, and job changes for zone residents based on where they work. Finally, we look at spillovers to nearby LICs that were not designated as OZs.

We find evidence of positive effects of OZ designation on job creation by businesses within zones. However, newly created jobs in zones are largely taken by residents of higher-income census tracts rather than residents of the targeted tract or residents of other non-OZ low-income tracts. Correspondingly, we find limited impacts on zone resident employment rates, earnings, or poverty rates. To the extent that there are job gains among OZ residents, they are concentrated in other, non-LIC tracts. Moreover, newly created jobs in zones are offset nearly one-to-one by declines in nearby low-income tracts.

Our results not only provide a longer-run perspective on the OZ program's impacts but also help reconcile previous findings on the program's effects. Earlier work pointed to limited effects of the program on residents of designated areas, but other studies suggested positive impacts on some outcomes measured at the workplace. Our results indicate that both may be true to some extent, but that many of the jobs created in OZs may be going to residents of not only other neighborhoods, but primarily other more advantaged neighborhoods. This effectively undoes some of the redistributive benefits of the program.

In future versions of this paper, we plan to incorporate restricted-access ACS data to further expand our analysis of resident outcomes. We will use our estimated employment effects together with evidence on tax expenditures and other inputs to estimate the marginal value of public funds spent on the OZ program.

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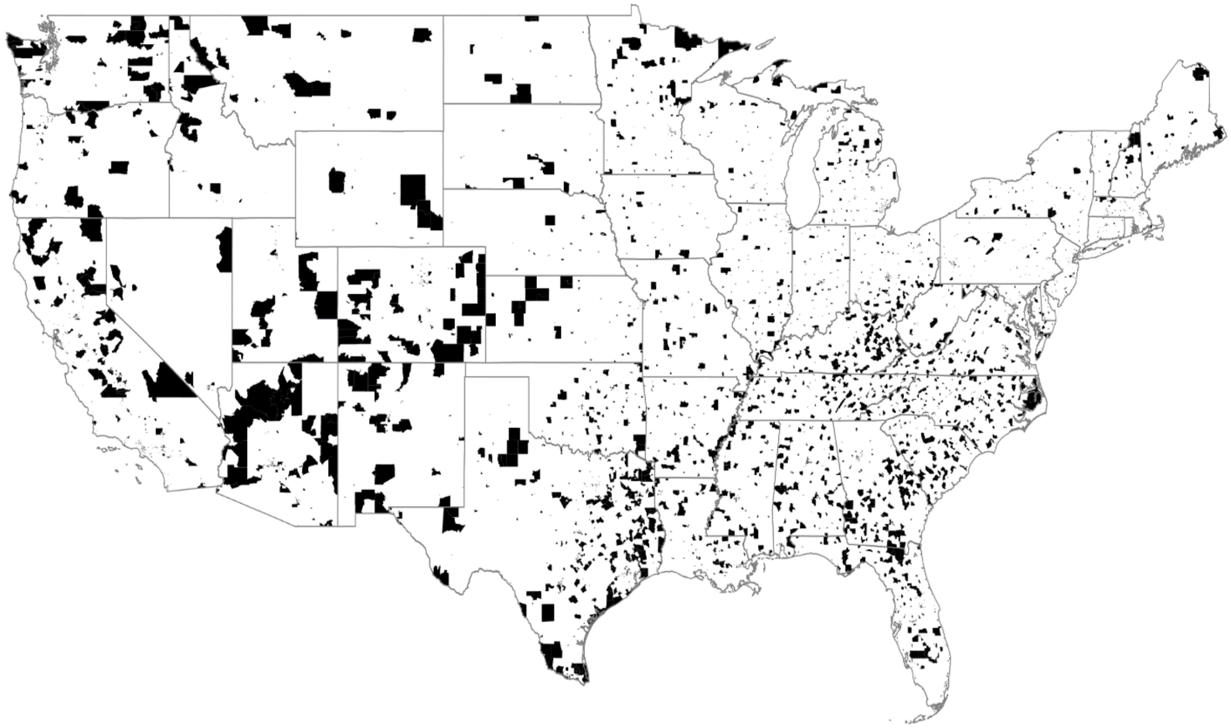
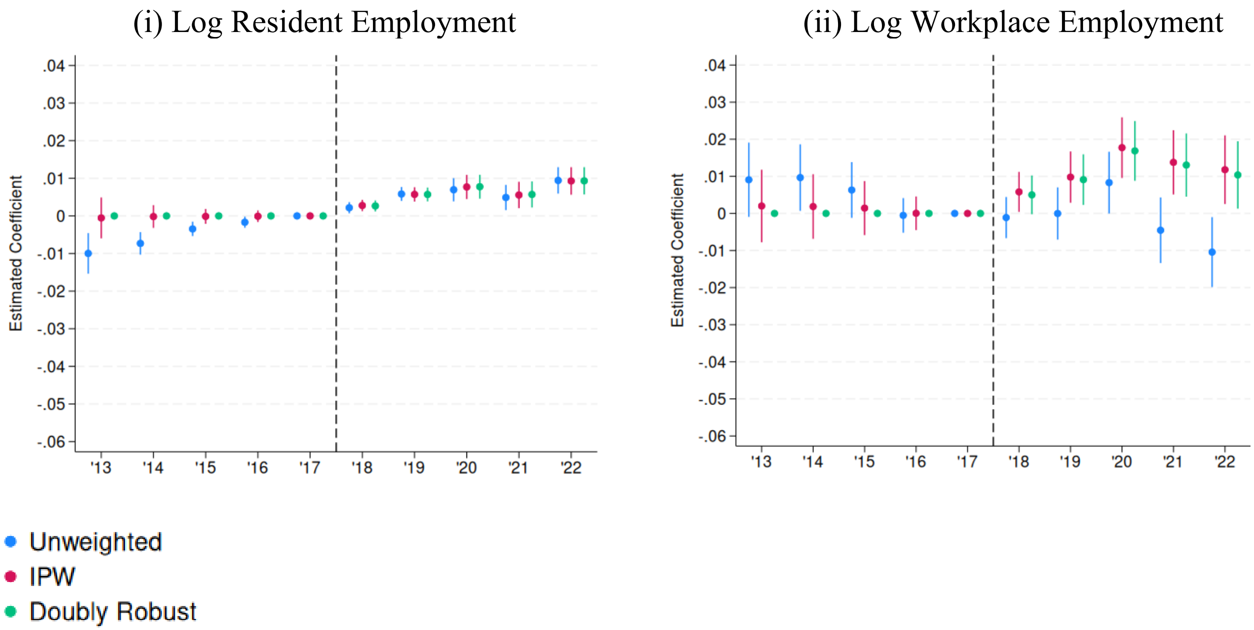
Figure 1. Opportunity Zones



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.

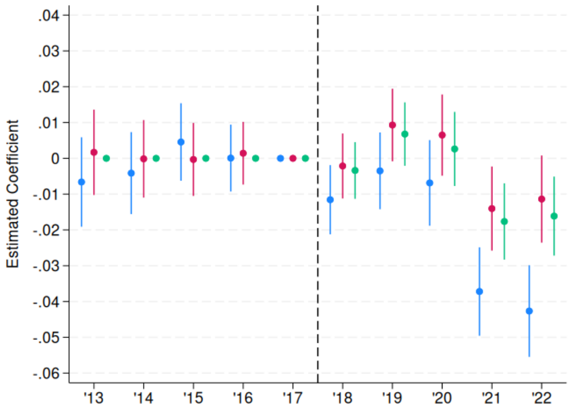
Figure 2. Event Studies for LODES Employment



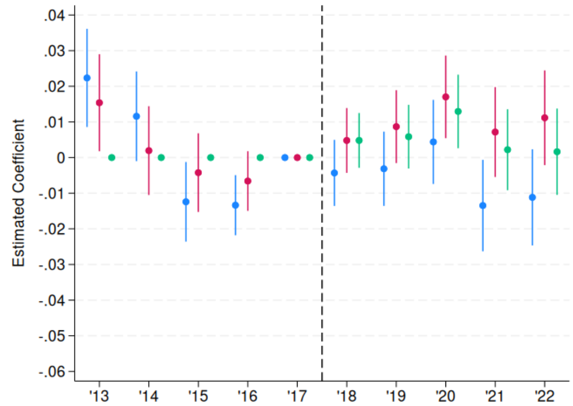
Notes: Data derived from the LODES 8. The panels show point estimates from event studies using as controls all eligible but not designated LICs. Naive Difference in difference results are shown in blue. In red, controls (eligible tracts) are weighted based on the estimated propensity to be treated based on the pre-treatment values of the outcome specific to each model. Doubly robust methods are shown in green, where pre-treatment values of the outcome variable are used for both first step linear regressions and in generating inverse propensity weights.

Figure 3. Event Studies for LODES Workplace Employment by Residential Location

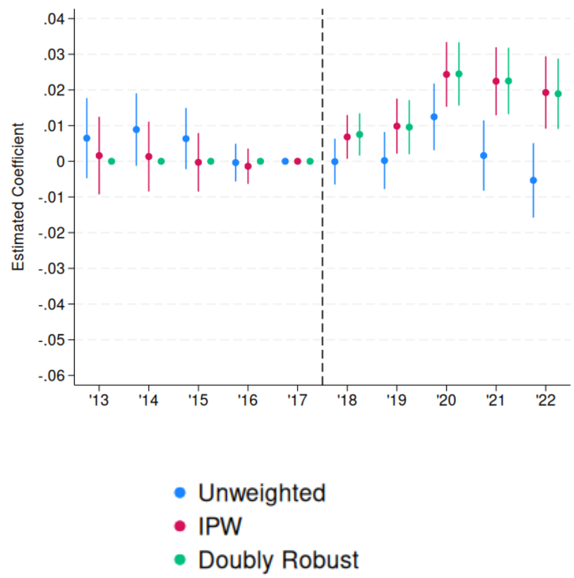
(i) Log Workplace Employment of Designated OZ Tract Residents



(ii) Log Workplace Employment of Residents of Non-OZ LIC Tracts



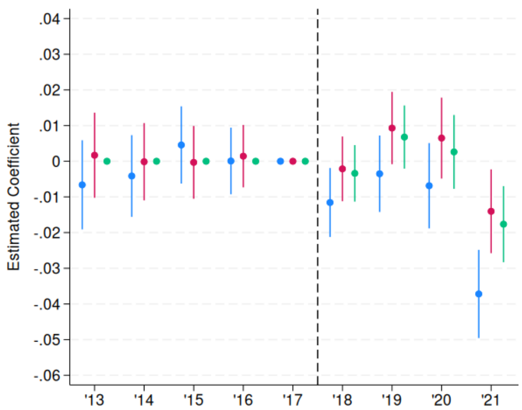
(iii) Log Workplace Employment of Residents of Non-LIC Tracts



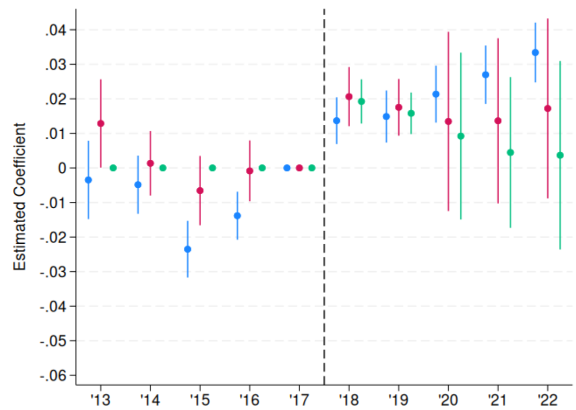
Notes: Data derived from the LODES 8. The panels show point estimates from event studies using as controls all eligible but not designated LICs . Naive Difference in difference results are shown in blue. In red, controls (eligible tracts) are weighted based on the estimated propensity to be treated based on the pre-treatment values of the outcome specific to each model. Doubly robust methods are shown in green, where pre-treatment values of the outcome variable are used for both first step linear regressions and in generating inverse propensity weights.

Figure 4. Event Studies for LODES Residential Employment by Workplace Location

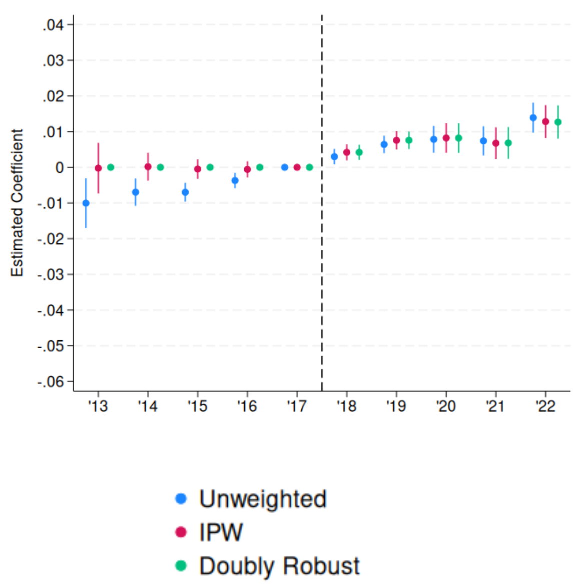
(i) Log Residential Employment in Designated OZ Tracte



(ii) Log Residential Employment at Workplaces in Non-OZ LIC Tracts



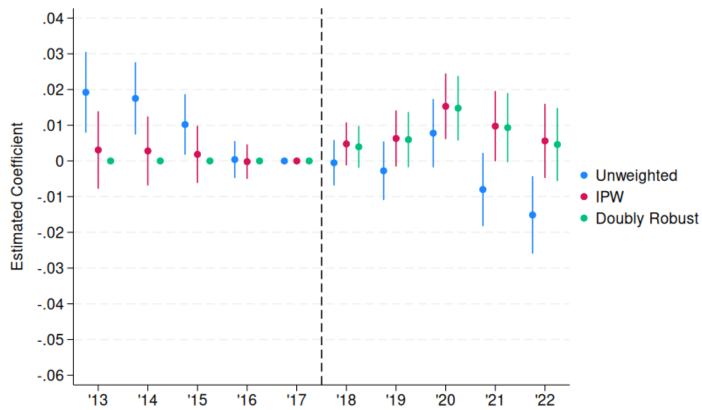
(iii) Log Residential Employment at Workplaces in Non-LIC Tracts



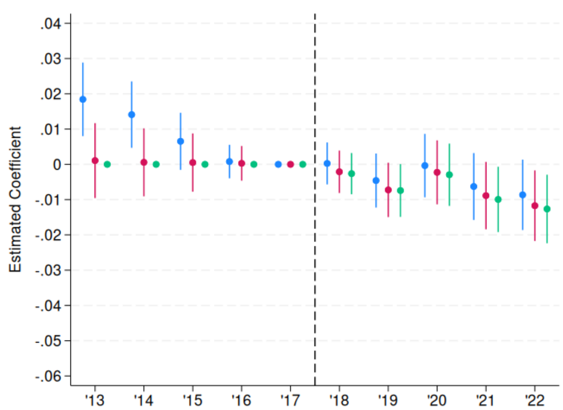
Notes: Data derived from the LODES 8. The panels show point estimates from event studies using as controls all eligible but not designated LICs. Naive Difference in difference results are shown in blue. In red, controls (eligible tracts) are weighted based on the estimated propensity to be treated based on the pre-treatment values of the outcome specific to each model. Doubly robust methods are shown in green, where pre-treatment values of the outcome variable are used for both first step linear regressions and in generating inverse propensity weights.

Figure 5. Event Studies for LODES Workplace Employment in OZs and Adjacent Tracts

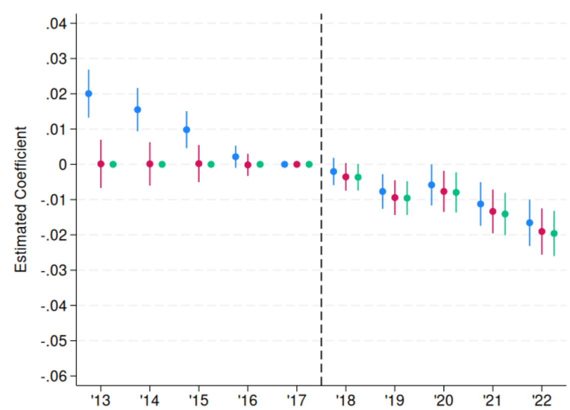
(i) Log Workplace Employment Omitting Adjacent Tracts from Controls



(ii) Log Workplace Employment in Adjacent LICs, non-adjacent LICs as controls



(iii) Log Workplace Employment in all adjacent tracts, all tracts adjacent to LIC but not OZ as controls



Notes: Data derived from the LODES 8. Panel (i) estimates the effects on designated Opportunity Zones using non-adjacent) eligible tracts as controls to account for the possibility of spillovers. Panel (ii) estimates such spillovers on adjacent eligible tracts, again using distant eligible tracts as controls. Panel (iii) estimates spillovers on all nearby tracts, using tracts which are not adjacent to a designated OZ but are adjacent to eligible LICs as controls. Naive difference-in-difference results are shown in blue. In red, controls (eligible tracts) are weighted based on the estimated propensity to be treated based on the pre-treatment values of the outcome specific to each model. Doubly robust methods are shown in green, where pre-treatment values of the outcome variable are used for both first step linear regressions and in generating inverse propensity weights.

Table 1. Descriptive Statistics for Opportunity Zones and Control Tracts

	Unweighted				ATT Weights	
	Untreated (non-OZ LICs)		Treated (LIC OZs)		Untreated (non-OZ LICs)	
	2013-2017	2019-2023*	2013-2017	2019-2023*	2013-2017	2019-2023*
Panel A: ACS 5-Year Averages						
Resident employment rate	55.29%	57.12%	52.15%	54.75%	52.59%	55.03%
	(10.65%)	(10.85%)	(10.76%)	(11.01%)	(11.09%)	(11.15%)
Resident median earnings	$26,031	$37,792	$23,945	$35,699	$24,145	$35,840
	($7,187)	($11,149)	($6,966)	($10,691)	($6,845)	($10,459)
Adult population	3,220	3,473	3,147	3,405	3,202	3,457
	(1,478)	(1,762)	(1,518)	(1,768)	(1,439)	(1,712)
Resident poverty rate	23.99%	19.94%	29.25%	24.13%	28.98%	23.23%
	(11.66%)	(11.35%)	(12.93%)	(12.44%)	(14.70%)	(13.82%)
Resident employment	1,787	1,989	1,656	1,885	1,722	1,924
	(923)	(1,114)	(910)	(1,111)	(886)	(1068)
Panel B: Annual LODES Data						
Resident employment	1,571	1,611	1,487	1,542	1,530	1,575
	(753)	(830)	(755)	(836)	(723)	(797)
Worksite employment	1,586	1,637	2,696	2,723	2,927	3,004
	(3,534)	(3,804)	(4,963)	(5,056)	(6,631)	(6,894)
...of same-tract residents	70	70	114	111	101	100
	(106)	(104)	(169)	(163)	(137)	(134)
...of other non-OZ LIC residents	448	453	628	626	718	722
	(930)	(959)	(1,256)	(1,251)	(1,539)	(1,555)
...of non-LIC residents	845	878	1,444	1,477	1,521	1,572
	(2,151)	(2,291)	(3,075)	(3,169)	(3,759)	(3,945)
Tracts	23,211	22,629	7,580	7,369	22,156	21,621

Notes: Population, Resident Median Earnings, Resident Employment Rate, and Resident Poverty Rate are sourced from ACS 5-year averages. Resident Employment and Worksite Employment are sourced from LODES. Standard deviations in parentheses. "Worksite Employment" data is missing for Alaska after 2016, Mississippi after 2019, and Michigan after 2022. Alaska is omitted from columns 5 and 6; missing data precludes inverse propensity score calculation. Sample sizes vary slightly within the two untreated groups and the treated group because of missing values, in some cases because of tract boundary changes; and, when we weight, such cases, as well as zero values in 2013-2017 (because we use logged outcomes in the treatment model), also result in a zero weight for 2019-2023.

* Post-period limited to 2018-2022 for LODES variables.

Table 2. Naïve Difference-in-Difference Estimates

	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Data:	LODES	LODES	ACS	ACS	ACS	ACS
	Log Resident Emp.	Log Workplace Emp.	Emp. Rate	Avg. Earnings	Poverty Rate	Log Resident Emp.
Opportunity Zone	0.011***	-0.006	0.009***	34.55	-0.011***	0.024***
	(0.002)	(0.004)	(0.001)	(108.4)	(0.001)	(0.004)
Tract FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Tracts	30,973	30,975	30,875	30,847	30,870	30,870
Obs.	275,557	275,557	61,443	61,268	61,425	61,423

Notes: Heteroskedasticity robust standard errors (in parentheses) are clustered on census tract. Stars indicate p-values: * $p<0.05$ ** $p<0.01$ *** $p<0.001$

Table 3. Estimates of the Effects of Opportunity Zones on Residents

	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Data:	LODES	LODES	ACS	ACS	ACS	ACS
	Log Resident Emp.	Log Workplace Emp.	Emp. Rate	Avg. Earnings	Poverty Rate	Log Resident Emp.
	A. IPW Treatment on the Treated Estimates					
Opportunity Zone	0.007***	0.012**	0.002	138.37	0.008***	0.018***
	(0.002)	(0.004)	(0.001)	(108.74)	(0.002)	(0.004)
	B. Regression-Adj. IPW Treatment on the Treated Estimates					
Opportunity Zone	0.007***	0.012***	0.002*	136.17	0.005***	0.018***
	(0.001)	(0.004)	(0.001)	(108.72)	(0.002)	(0.004)
Tract FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Tracts	30,889	30,814	30,871	30,815	30,862	30,860
Obs.	247,097	274,426	61,441	61,236	61,417	61,413

Notes: 2018 omitted. IPW based on outcome specific to each model. Heteroskedasticity robust standard errors (in parentheses) are clustered on census tract. Stars indicate p-values: * p<0.05 ** p<0.01 *** p<0.00

Table 4. Effects on Workplace Jobs by Resident Location

	(i)	(ii)	(iii)
	Log Workplace Jobs Held by Residents of		
	...the same tract	...non-OZ LIC tracts	...non-LIC tracts
A. Naive Difference-in-Difference Estimates			
Opportunity Zone	-0.021***	-0.007	-0.002
	(0.005)	(0.005)	(0.005)
B. IPW Treatment on the Treated Estimates			
Opportunity Zone	-0.003	0.010*	0.018***
	(0.004)	(0.005)	(0.005)
C. Regression-Adj. IPW Treatment on the Treated Estimates			
Opportunity Zone	-0.006	0.006	0.019***
	(0.004)	(0.005)	(0.003)
Tracts	29,821	30,302	29,727
Observations	264,483	267,809	255,694

Notes: Data sourced from LODES. 2018 omitted. IPW based on outcome specific to each model. Heteroskedasticity robust standard errors (in parentheses) are clustered on census tract.
Stars indicate p-values: * $p<0.05$ ** $p<0.01$ *** $p<0.001$

Table 5. Effects on Resident Jobs by Workplace Location

	(i)	(ii)	(iii)
	Log Resident Jobs at Workplaces in		
	...the same tract	...non-OZ LIC tracts	...non-LIC tracts
A. Naive Difference-in-Difference Estimates			
Opportunity Zone	-0.021***	0.033***	0.014***
	(0.005)	(0.003)	(0.002)
B. IPW Treatment on the Treated Estimates			
Opportunity Zone	-0.003	0.014	0.009***
	(0.004)	(0.010)	(0.002)
C. Regression-Adj. IPW Treatment on the Treated Estimates			
Opportunity Zone	-0.006	0.008	0.009***
	(0.004)	(0.009)	(0.002)
Tracts	29,821	30,302	29,727
Observations	269,366	268,685	254,123

Notes: Data sourced from LODES. 2018 omitted. IPW based on outcome specific to each model.
Heteroskedasticity robust standard errors (in parentheses) are clustered on census tract.
Stars indicate p-values: * $p<0.05$ ** $p<0.01$ *** $p<0.001$

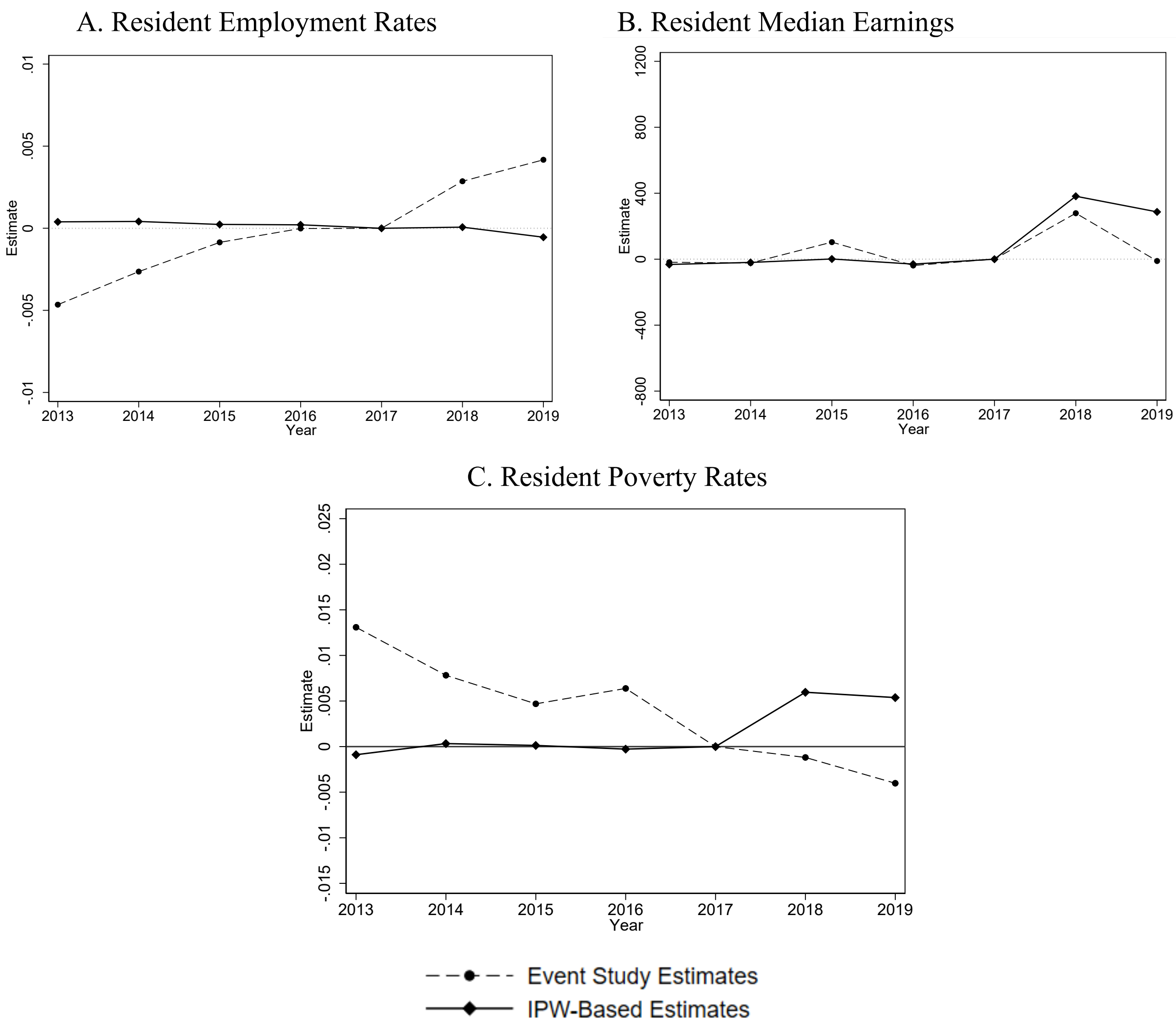
Table 6. Spillover Effects on Workplace Jobs in OZs and Adjacent Tracts

	(i)	(ii)	(iii)
		Log Workplace Jobs	
		A. Naive Difference-in-Difference Estimates	
Opportunity Zone	-0.014**		
	(0.005)		
Near OZ		-0.013**	-0.020***
		(0.005)	(0.002)
		B. IPW Treatment on the Treated Estimates	
Opportunity Zone	0.008		
	(0.005)		
Near OZ		-0.008	-0.012***
		(0.004)	(0.003)
		C. Regression-Adj. IPW Treatment on the Treated Estimates	
Opportunity Zone	0.009*		
	(0.004)		
Near OZ		-0.008*	-0.013***
		(0.004)	(0.003)
Treated tracts (N)	OZs (7,656)	LICs adjacent to OZs (12,228)	All tracts adjacent to OZ (22,492)
Control tracts (N)	LICs not adjacent to OZs (11,095)	LICs not adjacent to OZs (11,095)	Tracts adjacent to an LIC but not an OZ (26,029)
Total 2017 workplace jobs in treated tracts	21,005,705	19,496,515	40,985,928
Observations	225,082	207,144	430,389

Notes: Data sourced from LODES. 2018 omitted. IPW based on outcome specific to each model. Heteroskedasticity robust standard errors (in parentheses) are clustered on census tract. Column (1) estimates the effects on designated Opportunity Zones using distant (non-adjacent) LICs as controls to account for the possibility of spillovers. Column (2) directly estimates spillovers on adjacent LICs, again using distant LICs as controls. Column (3) estimates spillovers on all nearby tracts. Since some of the nearby tracts will not be LICs, controls are broadened to any tract not adjacent to designated opportunity zones but adjacent to an LIC.
Stars indicate p-values: * p<0.05 ** p<0.01 *** p<0.001

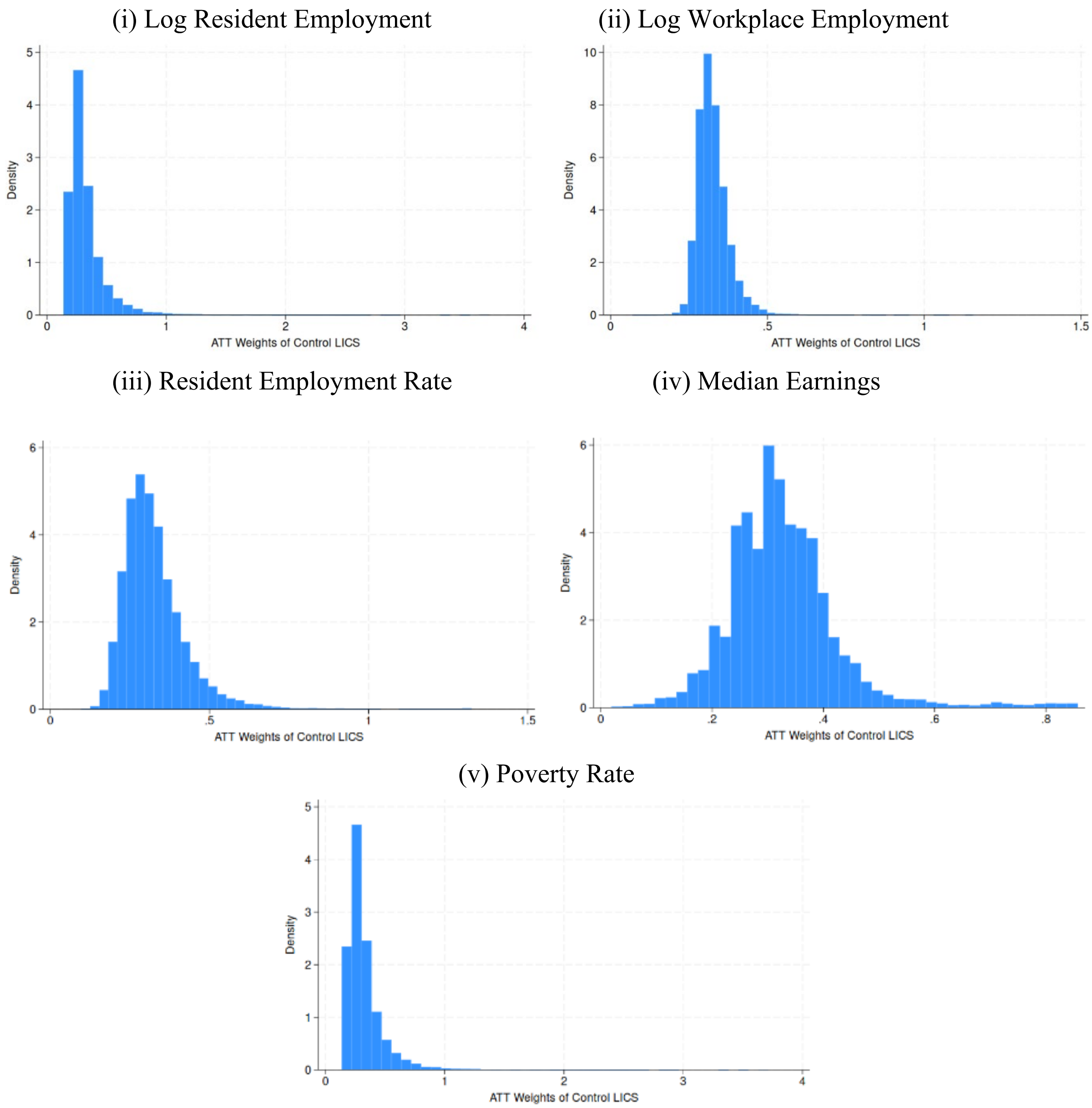
Appendix Figures and Tables

Figure A1. Event Study Estimates of Effects of Opportunity Zones with Alternative Weighting Schemes



Source: Figure 3 (Freedman et al., 2023b).
Notes: Data derived from the 2013-2019 American Community Surveys. The panels show point estimates from event studies using as controls all eligible but not designated LICs (reproducing the estimates with tract and year fixed effects in Figure 2), as well as using as controls eligible tracts weighted based on the estimated propensity to be treated (the IPW approach).

Figure A2. ATT Control Weights



Each panel shows the distribution of inverse propensity weights estimated for each of our five main outcomes. Panels (i) and (ii) show weights for outcomes measured in LODES. Panels (iii)-(v) show weights for outcomes measured in the ACS. Note that weights for treated units are set to one, and not included in the above histograms.

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

	Mean	Std. Dev.	Skewness	Kurtosis	No. of Control Tracts
Panel A: Variables Measured in ACS 5-year Averages					
Resident employment rate	0.233	0.327	2.53	17.41	23,271
Resident median earnings	0.327	0.101	1.49	8.33	23,222
Log resident poverty rate	0.330	0.196	6.39	79.05	23,262
Log resident employment	0.327	0.056	4.93	65.60	23,260
Panel B: Variables Measured in LODES					
Log residential employment	0.326	0.051	2.35	21.52	23,215
Log workplace employment	0.326	0.154	1.69	9.54	23,215
...of same-tract residents	0.329	0.102	0.816	3.60	22,238
...of non-OZ LIC residents	0.289	0.108	1.14	6.22	23,146
...of non-LIC residents	0.326	0.136	1.29	6.60	23,144

Notes: The number of control tracts varies slightly because of missing/non-reported data.

Table A2. Naïve Event Study Estimates for ACS Variables

	(1)	(2)	(3)	(4)
	Employment Rate	Median Earnings	Poverty Rate	Log Employment
Opportunity Zone × 2008-2012	-0.002***	-74.14	0.001	-0.007*
	(0.001)	(108.5)	(0.001)	(0.003)
Opportunity Zone × 2019-2023	0.008***	29.42	-0.011**	0.024***
	(0.001)	(108.5)	(0.001)	(0.004)
Tract fixed effects	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y
Tracts	30,889	30,869	30,874	30,874
Observations	92,313	92,123	92,281	92,275

Notes: ACS 20013-2017 period excluded. Heteroskedasticity robust standard errors (in parentheses) are clustered on census tract.
Stars indicate p-values: * p<0.05 ** p<0.01 *** p<0.001

Table A3. Estimates of the Effects of Opportunity Zones on Residents IPW Robustness Checks

	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Data:	LODES	LODES	ACS	ACS	ACS	ACS
	Log Resident Emp.	Log Workplace Emp.	Emp. Rate	Median Earnings	Poverty Rate	Log Resident Emp.
A. IPW with State by Year Fixed Effects						
Opportunity Zone	0.008***	0.013**	0.002*	80.44	0.008***	0.016***
	(0.002)	(0.004)	(0.001)	(107.06)	(0.002)	(0.004)
Tract FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
State × Year FE	Y	Y	Y	Y	Y	Y
Tracts	30,814	30,817	30,871	30,815	30,862	30,860
Obs.	247,426	243,637	61,441	61,236	61,417	61,413
B. IPW with Winsorized Weights						
Opportunity Zone	0.010***	0.016***	0.005***	132.83	-0.003*	0.025***
	(0.002)	(0.004)	(0.001)	(107.34)	(0.001)	(0.004)
Tract FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
State × Year FE	N	N	N	N	N	N
Tracts	28,482	28,489	28,542	28,491	28,534	28,531
Obs.	253,736	253,728	56,801	56,657	56,800	56,781

Notes: 2018 omitted. IPW based on outcome specific to each model. Heteroskedasticity robust standard errors (in parentheses) are clustered on census tract.
Stars indicate p-values: * $p<0.05$ ** $p<0.01$ *** $p<0.001$

Table A4. ACS Employment and Population Effects

	(1)	(2)
	Log Employment	Log Adult Civ. Population
	A. Naïve Difference-in-Difference Estimates	
Opportunity Zone	0.0236***	0.0054
	(0.0037)	(0.0029)
	B. IPW Treatment on the Treated Estimates	
Opportunity Zone	0.0177***	0.0039
	(0.0039)	(0.0030)
	C. Regression-Adj. IPW Treatment on the Treated Estimates	
Opportunity Zone	0.0179***	0.0039
	(0.0038)	(0.0030)
Tracts	30,865	30,865
Observations	61,423	61,423

Notes: 2018 omitted. IPW based on outcome specific to each model. Heteroskedasticity robust standard errors (in parentheses) are clustered on census tract. Empirical work often uses treatment assigned following geographic boundaries. When the effects of treatment cross over borders, classical difference-in-differences estimation produces biased estimates for the average treatment effect. In this paper, I introduce a potential outcomes framework to model spillover effects and decompose the estimate's bias in two parts: (1) the control group no longer identifies the counterfactual trend because their outcomes are affected by treatment and (2) changes in treated units' outcomes reflect the effect of their own treatment status and the effect from the treatment status of 'close' units. I propose conditions for non-parametric identification that can remove both sources of bias and semi-parametrically estimate the spillover effects themselves including in settings with staggered treatment timing. To highlight the importance of spillover effects, I revisit analyses of three place-based interventions.

Stars indicate p-values: * $p<0.05$ ** $p<0.01$ *** $p<0.001$