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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 U.S. Very early evidence indicated no impact of the program on employment, earnings, or poverty of zone residents, but some evidence of increased employment among businesses in zones. Using the latest survey-based as well as administrative data, we adopt a longer-run perspective and find that OZ designation increases job creation within zones. However, newly created jobs in zones are largely taken by residents of higher-income census tracts rather than OZ residents or residents of other low-income tracts. Correspondingly, we find limited impacts on resident employment rates, earnings, or poverty rates.

Keywords: Opportunity Zones, Place-Based Policies, 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] OZs tax benefits currently are slated to end in 2026 (which pending legislation could push to 2028),[3] so it is thus an opportune and important time to evaluate their efficacy, and the return on the public's investment in this program.

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 perspective than previous papers. Leveraging quasi-experimental variation in OZ tract designation, we find that the program increased job creation among businesses in targeted areas. However, newly created jobs in zones are largely taken by residents of higher-income census tracts rather than OZ residents or residents of other low-income tracts. Correspondingly, we find limited impacts on resident

[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] See https://opportunitydb.com/2023/11/2023-oz-legislation/. Our understanding of the recently passed budget resolution in the U.S. House will extend Opportunity Zones (https://waysandmeans.house.gov/2025/02/25/house-passes-budget-resolution-to-advance-one-big-beautiful-bill-that-will-deliver-tax-relief-to-working-families/) .

employment rates, earnings, or poverty rates.

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 potentially complement other policies to aid in redistribution and can 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 to 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 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

[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 component, 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).

that does not benefit the intended beneficiaries – like building 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.

The a priori negative assessment of the likelihood that OZs would prove effective was largely substantiated by the earliest wave of research on OZs, although there is some mixed evidence.[7] The critical limitation of this earlier research, however, was just that – it was early.[8]

[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] Notably, 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 (Freedman et al., 2023a; Hyman et al., 2023).
[7] This evidence is discussed in Section III.
[8] And adding a couple more years (like Arefeva et al. (2024) extending through 2021) can confound the effects with the Covid-19 pandemic.

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]

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, 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

[9] For example, see https://eig.org/wp-content/uploads/2023/03/Examining-the-Latest-Multi-Year-Evidence-on-Opportunity-Zones-Investment.pdf. Then again, Corinth and Feldman (2023) study effects on real estate using data through 2022 and find no effects.

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 yielded benefits for residents of low-income communities as opposed to more affluent areas. Our results have important implications for ongoing discussions about the future of the OZ program, and for continuing evaluation of this program.

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 contiguous with 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 contiguous with 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 contiguous with an LIC and had median income less than 125% of the median income of the LIC with which it was

contiguous.

Overall, there were 42,176 tracts that were eligible to be OZs. These included 31,864 LICs and 10,312 non-LIC contiguous tracts. Governors selected 8,762 tracts as OZs. Of those selected, 8,532 (97%) were LICs while 230 (3%) were non-LIC contiguous 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).

OZs tax benefits currently are slated to end in 2026, although current budget negotiations and bills appear set to extend these benefits. It is thus an opportune and important time to evaluate their efficacy, and the return on the public's investment in this program. 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, one early analysis by Freedman et al. (2023b) focused on the impact of OZ designation on resident

employment. Freedman et al. (2023b) 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 has 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 do 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 average earnings of employed residents of designated tracts are positive but are economically small and not consistently statistically significant. Meanwhile, they find that zone designation is 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.[10]

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 among residents. For example, Atkins et al. (2023) finds limited evidence of increases in online job postings in OZs. Shen (2024) finds no evidence of employment growth or small business formation associated with OZs in New York City. However, Arefeva et al. (2024) 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). In Arefeva et al.'s (2024) main results, they rely on the Your Economic Time Series data; 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

[10] This evidence is illustrated in 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. The contrast – in particular, the disappearance of any evidence of gains in employment or reductions in poverty for zone residents – is clear.

affected 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 that this paper addresses is that, as noted above, nearly all of these studies use data that end within 2-3 years of the OZ program's introduction. Arefeva et al. (2024) use the Your-economy Time Series through 2021. Atkins et al. (2023) study Burning Glass 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 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. Shen (2024) deploys InfoGroup historical directories of small businesses in NY through 2023 – the one exception with more recent data, although in a limited application. And Snidal and Li (2024) use small business and residential loan origination data through 2019. In contrast, our data currently extend through 2023 (and will for at least some analyses extend one year further in a later version of this study).

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.[11] Designated tracts appear in Figure 1.

Much of our analysis will eventually rely on restricted-access American Community

[11] See https://opportunityzones.hud.gov/home.

Survey (ACS) data for 2013-2023 (through likely 2024 by the final version of this paper), which we will access 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; the public-use data only provide tract-level information averaged over five years. Using the ACS data, we will focus on the effects of OZs on residents of designated areas. We will construct four main outcome measures: the employment-to-population ratio for residents, average 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 will aggregate the individual microdata to the tract-by-year level, using the person weights in the ACS. We will only keep tracts that have complete information for all our outcomes of interest.[12] However, because of disclosure requirements that preclude repeated releases as the analysis evolves, *for now* we are reporting results using the public-use files with tract-level 5-year averages; in our methods section below, we note and discuss this briefly.

We also use LODES data for the same tract-level analysis. The LODES data are derived from state unemployment insurance tax records and thus cover the near universe of workers in the U.S. Moreover, the LODES data are not confidential, and so with these data we can do the year-by-year analysis without disclosure constraints. 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 in the 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

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

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.[13]

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.

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.[14] 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,

[13] 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.

[14] We will update the datasets for one more year if possible. The ACS data are typically released in September of the following year, so we could add them in the early fall for the final version of the paper. The LODES release dates are not known.

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. 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 later in our data (in the LODES data, for now).

V. Empirical 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. The basic model is

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

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 designated; 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 also independently affect outcomes.[15] 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

[15] The tract fixed effect also subsumes the main effect for OZ_i.

year.[16] These are measured relative to 2017 (the omitted year). We use this model for the LODES. We cluster standard errors at the tract level, which allows for arbitrary patterns of heteroskedasticity across tracts and serial correlation within tracts.

For the current ACS analyses for which we have only the 5-year averages, we cannot do an event study, and instead we estimate a simply difference-in-differences model with a pre- vs. post-comparison. 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. In this case, defining $POST_t$ as dummy variable equal to one after the OZ program is enacted, the model simplifies to

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

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 for estimate this model with the LODES, to be comparable. We will ultimately, however, also estimate the event-study model with the confidential ACS data.

In one robustness test, we additionally include state-by-year fixed effects in our models in an effort to account for potentially 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. In a future draft, we will also experiment with city-by-year and 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

[16] "Effects" is in quotes because we do not think of the estimates as capturing causal effects in the pre-treatment period.

amplify bias attributable to spillovers of OZ effects across nearby tracts, especially with county-by-year fixed effects.

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. 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 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). For all methods, 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:

$$OZ_i^* = \alpha + \sum_{\tau=2013}^{2017} y_{i\tau} + v_i$$

That is, we predict OZ designation for all tracts in our sample of LICs based on each tract's outcome in each year between 2013 and 2017 (i.e., over the entire pre-treatment period).[18] The

[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.

[18] Note that for the 5-year public-use ACS files we are using in this draft, we simply use data for the 2013-2017 and 2019-2023 periods. As a consequence, we can match on 2013-2017 levels, but not

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 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.

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 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.[19] In our application of regression-adjusted IPW, we model both the outcome and the treatment using the same set of covariates.[20] By using

changes. However, in a supplemental analysis, we confirm past working pointing to differential pre-treatment trends in ACS-measured outcomes by incorporating an earlier wave of the ACS data (see Appendix Table A2). Following Freedman et al. (2023b), we will be able to match on the yearly evolution of these outcomes using the confidential ACS data.

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

[20] With our matching approaches, the unit of observation is the census tract. We calculate heteroskedasticity robust standard errors following Wooldridge (2010).

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.

VI. Results

A. Naïve Difference-in-Differences Estimates

We begin by estimating simple difference-in-differences models for each of our outcomes, 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 results appear in Table 2.

The naïve regressions suggest that OZ designation leads to increases in resident employment rates as well as reductions in resident poverty rates, although point to little impact on average resident earnings. The magnitudes and statistical significance of these estimates closely align with those in Freedman et al. (2023b), who only consider effects through 2019. We further extend their results by looking at resident and workplace employment levels. In column

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

(4) 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 residential 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).

B. IPW Results

Prior work suggests that OZs were on different trajectories than non-OZ LICs. 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) show 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 we do have annually. 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. Focusing first on the blue dots, we see a 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

[22] See Appendix Figure A1.

[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).

decreases (in line with the estimates in Table 2).

Reweighting the regressions 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 average effects for these outcomes alongside IPW-adjusted ACS outcomes appear in Table 3. In the ACS data, consistent with Freedman et al. (2023b), in the adjusted estimates (using either approach) we now find limited evidence of changes in the employment rate or earnings, and evidence of increased poverty of residents of OZ-designated tracts. However, the estimated positive impact on resident employment 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 larger for worksite employment (1.2% vs. 0.7%). Moreover, the comparable estimates – for log resident employment – are qualitatively similar in the ACS and LODES data. Table 4 shows very similar results with the state-by-year controls, which can better control for the influences of Covid (and associated policy responses) by state, as well as other state policy changes. The estimated effects on workplace employment are qualitatively consistent with Arefeva et al.'s (2024) results using the Your-Economy Time Series data, although our LODES estimates are roughly half the size.

One 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 employment, earnings, and poverty levels as existing residents. This could drive increased resident employment levels, but translate into little to no effect on employment rates, average earnings, or

poverty rates. There is some evidence to support this, but it is not strong. In particular, estimated effects on the size of the adult civilian population, reported in Appendix Table A3, are positive (about 0.4% in the adjusted estimates), but not statistically significant.

The evidence from the LODES of stronger effects on workplace than resident employment indicates that some newly created jobs are not going to OZ residents. To shed more light on the connection between workplace jobs and residential location, we leverage the richness 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 LICs, or residents of non-LICs. The results appear in Figure 3 and Table 5. We find that the increase in workplace employment is driven predominantly 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 5), we find that OZ designation leads to a 3.2% increase in workplace jobs held by residents of non-LICs, compared to a 1.1% increase for residents of LICs, and much smaller and statistically insignificant 0.3% change for residents of the tract itself. Note that the larger estimate for resident employment in Table 3 (0.7%) suggests that some of the growth in resident employment occurs outside the OZ.[24] The evidence that the employment gains associated with OZ designation primarily benefit residents who are neither in the OZ nor in other LIC tracts 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.

[24] 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.

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 U.S. 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. We find evidence of positive effects of OZ designation on job creation by businesses within zones. However, we find that most of the new jobs created by businesses in OZs go to residents living in more affluent areas. Correspondingly, we also find limited effects on other measures of neighborhood disadvantage, including employment rates, poverty rates, and average earnings.

Our results not only provide a longer-run perspective on the OZ program's impacts, but also may 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 suggest 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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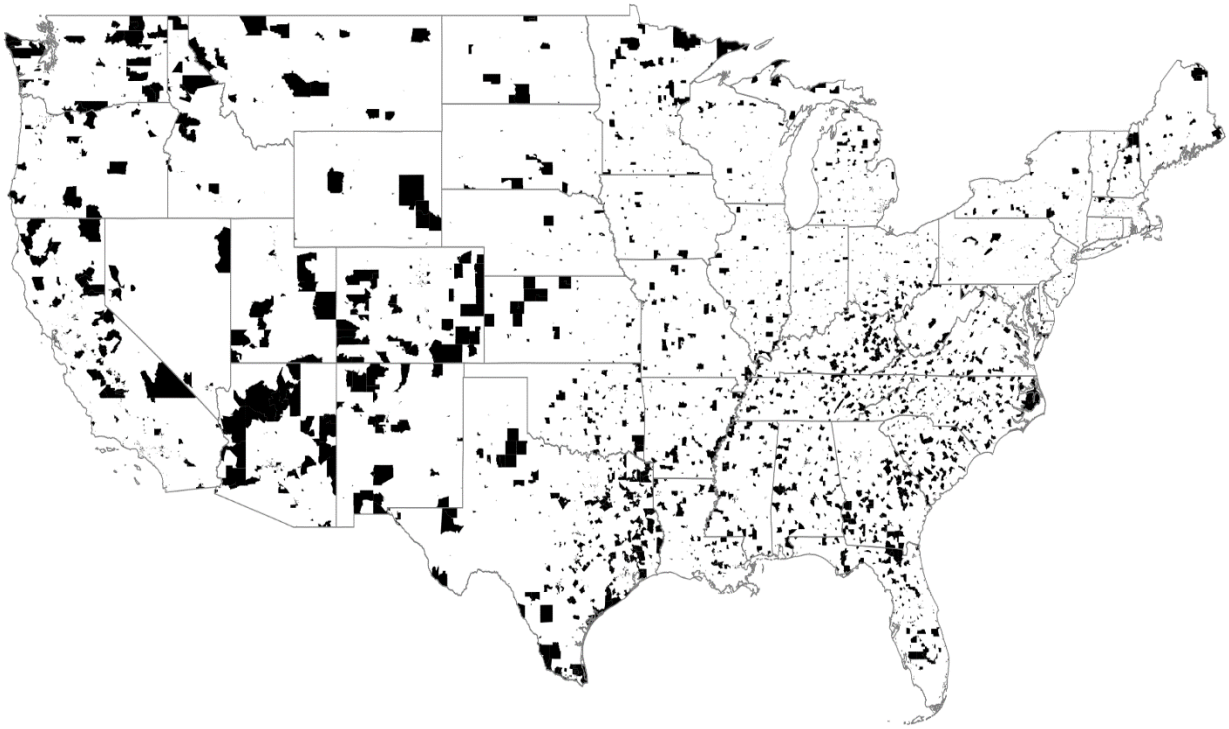
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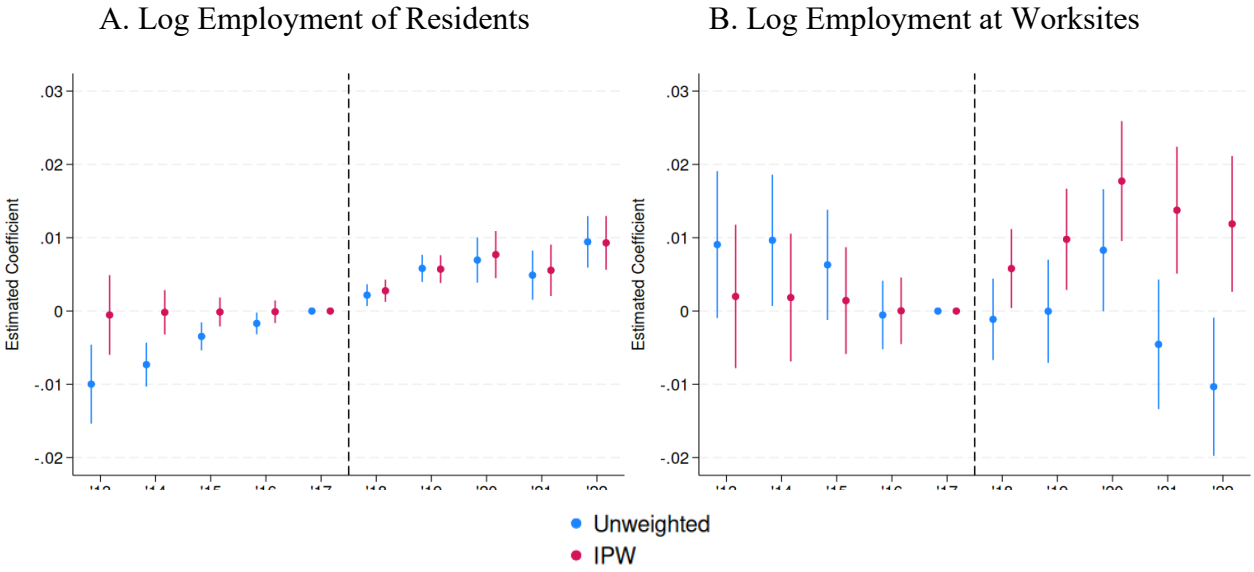
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Figure 1. Opportunity Zones



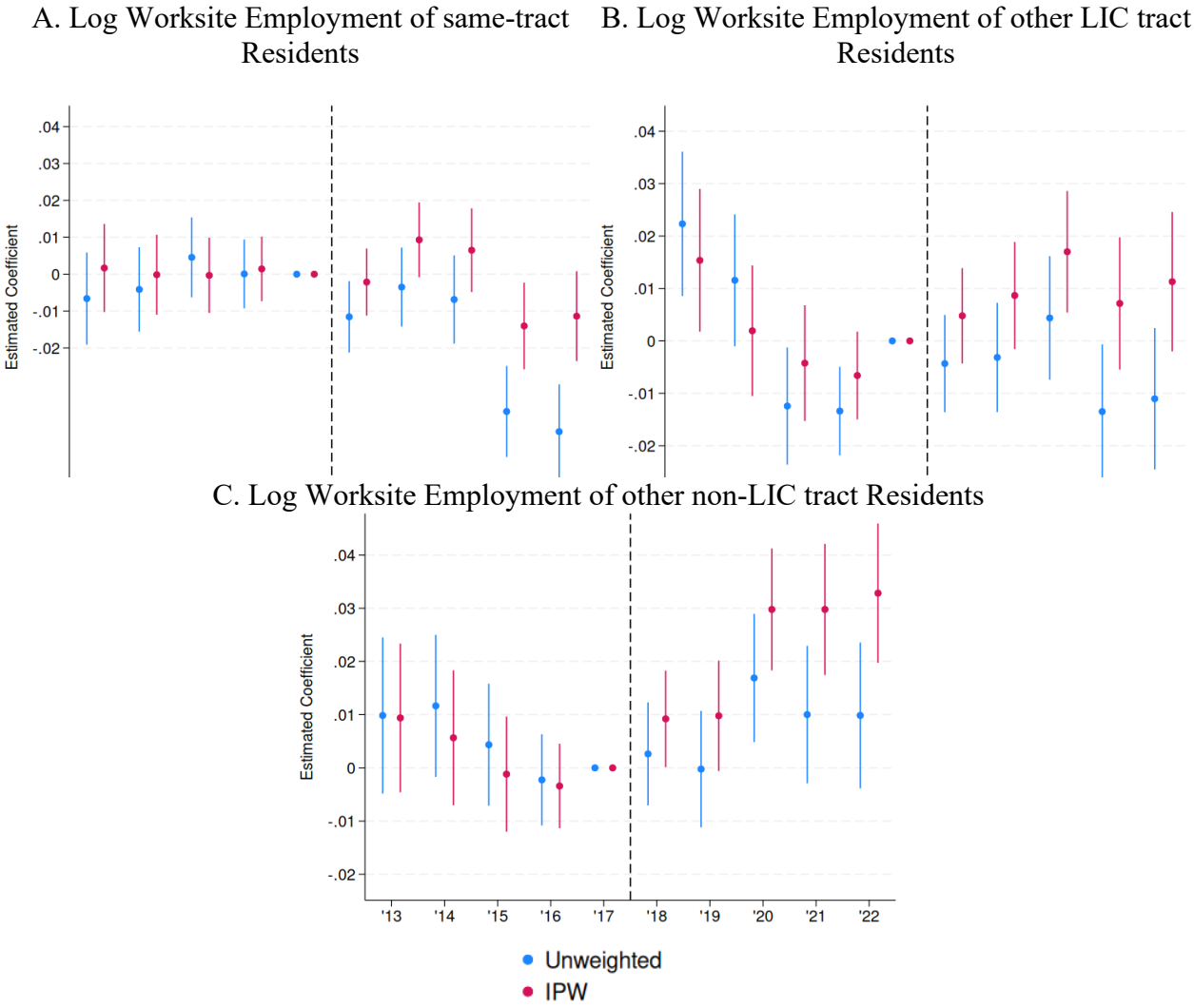
Notes: Shaded areas are census tracts designated as Opportunity Zones. Information on Opportunity Zones are from the Community Development Financial Institutions (CDFI) Fund at the U.S. Department of the Treasury.

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, as well as using as controls eligible tracts weighted based on the estimated propensity to be treated based on the pre-treatment values of the outcome specific to each model.

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



Notes: Data derived from the LODES 8. The panels show point estimates from event studies using as controls all eligible but not designated LICs, as well as using as controls eligible tracts weighted based on the estimated propensity to be treated based on the pre-treatment values of the outcome specific to each model.

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: Variables Measured in ACS 5-year Averages						
Resident Employment Rate	55.29% (10.65%)	57.12% (10.85%)	52.15% (10.76%)	54.75% (11.01%)	52.59% (11.09%)	55.03% (11.15%)
Resident Median Earnings	$26,031 ($7,187)	$37,792 ($11,149)	$23,945 ($6,966)	$35,699 ($10,691)	$24,145 ($6,845)	$35,840 ($10,459)
Adult Population	3,220 (1,478)	3,473 (1,762)	3,147 (1,518)	3,405 (1,768)	3,202 (1,439)	3,457 (1,712)
Resident Poverty Rate	23.99% (11.66%)	19.94% (11.35%)	29.25% (12.93%)	24.13% (12.44%)	28.98% (14.70%)	23.23% (13.82%)
Resident Employment	1,787 (923)	1,989 (1,114)	1,656 (910)	1,885 (1,111)	1,722 (886)	1,924 (1068)
Panel B: Variables Measured in LODES						
Resident Employment	1,571 (753)	1,611 (830)	1,487 (755)	1,542 (836)	1,530 (723)	1,575 (797)
Worksite Employment	1,586 (3,534)	1,637 (3,804)	2,696 (4,963)	2,723 (5,056)	2,927 (6,631)	3,004 (6,894)
...of same-tract residents	70 (106)	70 (104)	114 (169)	111 (163)	101 (137)	100 (134)
...of other LIC residents	448 (930)	453 (959)	628 (1,256)	626 (1,251)	718 (1,539)	722 (1,555)
...of non-LIC residents	775 (2,130)	809 (2,269)	1,329 (3,052)	1,365 (3,143)	1,435 (3,733)	1,488 (3,914)
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. *Post-period limited to 2018-2022 for LODES variables. "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.

Table 2. Naïve Difference-in-Difference Estimates

	(1)	(2)	(3)	(4)	(5)	(6)
	Measured in ACS				Measured in LODES	
	Emp. Rate	Avg. Earnings	Poverty Rate	Log Resident Emp.	Log Resident Emp.	Log Workplace Emp.
Opportunity Zone	0.009***	34.55	-0.011***	0.024***	0.011***	-0.006
	(0.001)	(108.4)	(0.001)	(0.003)	(0.002)	(0.0042)
Tract FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Tracts	30,875	30,847	30,870	30,870	30,980	30,975
Obs.	61,445	61,268	61,425	61,423	247,821	244,627

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

	(1)	(2)	(3)	(4)	(5)	(6)
	Measured in ACS				Measured in LODES	
	Emp. Rate	Average Earnings	Poverty Rate	Log Resident Emp.	Log Resident Emp.	Log Worksite Emp.
A. IPW Treatment on the Treated Estimates						
Opportunity Zone	0.002	138.4	0.008***	0.018***	0.007***	0.012**
	(0.001)	(108.7)	(0.018)	(0.004)	(0.002)	(0.004)
B. Regression-Adj. IPW Treatment on the Treated Estimates						
Opportunity Zone	0.0024*	136.17	0.005***	0.018***	0.007***	0.012***
	(0.0010)	(108.72)	(0.002)	(0.004)	(0.001)	(0.004)
Tracts	30,871	30,815	30,862	30,860	30,889	30,817
Obs.	61,441	61,236	61,417	61,413	247,097	243,637

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 4. Estimates of the Effects of Opportunity Zones on Residents with State-by-Year Fixed Effects

	(1)	(2)	(3)	(4)	(5)	(6)
	Measured in ACS				Measured in LODES	
	Emp. Rate	Average Earnings	Poverty Rate	Log Resident Emp.	Log Resident Emp.	Log Worksite Emp.
	A. IPW Treatment on the Treated Estimates					
Opportunity Zone	0.0023*	80.44	0.0076***	0.0164***	0.0079***	0.0133**
	(0.0011)	(107.1)	(0.0017)	(0.0039)	(0.0017)	(0.0042)
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	30,871	30,815	30,862	30,860	30,889	30,817
Obs.	61,441	61,236	61,417	61,413	247,097	243,637

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 5. Effects on Workplace Jobs by Resident Location

	(1)	(2)	(3)
	Log Workplace Jobs of Residents		
	...in the same tract	...in other LIC tracts	...in non-LIC tracts
	A. Naive Difference-in-Difference Estimates		
Opportunity	-0.021***	-0.007	0.004
Zone	(0.005)	(0.005)	(0.006)
	B. IPW Treatment on the Treated Estimates		
Opportunity	-0.003	0.010	0.023***
Zone	(0.005)	(0.005)	(0.006)
	C. Regression-Adj. IPW Treatment on the Treated Estimates		
Opportunity	0.003	0.011*	0.032***
Zone	(0.005)	(0.005)	(0.005)
Tracts	29,821	30,302	29,727
Obs.	264,483	267,809	255,694

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: * $p<0.05$ ** $p<0.01$ *** $p<0.001$

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).

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

	Mean	Std. Dev.	Skewness	Kurtosis	# 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 Worksite 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 other-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

Table A2. Naïve Event Study Estimates for ACS Variables

	(1)	(2)	(3)
	Employment Rate	Average Earnings	Poverty Rate
Opportunity Zone × 2008-2012	-0.0024***	-74.14	0.00068
	(0.0010)	(108.5)	(0.0013)
Opportunity Zone × 2019-2023	0.00844***	29.42	-0.0111**
	(0.0010)	(108.5)	(0.0013)
Tract Fixed Effects	Y	Y	Y
Year Fixed Effects	Y	Y	Y
Tracts	30,889	30,869	30,874
Observations	92,313	92,123	92,281

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. ACS Employment and Population Effects

	(1)	(2)
	Log Employment	Log Adult Civ. Population
	A. Naïve Difference-in-Difference Estimates	
Opportunity	0.0236***	0.0054
Zone	(0.0037)	(0.0029)
	B. IPW Treatment on the Treated Estimates	
Opportunity	0.0177***	0.0039
Zone	(0.0039)	(0.0030)
	C. Regression-Adj. IPW Treatment on the Treated Estimates	
Opportunity	0.0179***	0.0039
Zone	(0.0038)	(0.0030)
Tracts	30,865	30,865
Obs.	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.
Stars indicate p-values: * p<0.05 ** p<0.01 *** p<0.001