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UNDERSTANDING THE EMPLOYMENT EFFECTS OF OPPORTUNITY ZONES

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ABSTRACT

The Opportunity Zone program was designed to encourage investment in distressed communities across the United States. 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 comprehensive perspective on the labor market impacts of OZs. We find that OZ designation increases job creation among businesses within zones. However, a large share of the newly created jobs in zones is offset by declines in nearby low-income communities. While we detect gains in OZ resident employment over the longer run, the increase comes from jobs with workplaces outside of OZs that, in light of the changing demographic composition of zones, are likely held by new as opposed to existing residents. Overall, our results suggest that OZs have limited benefits for existing residents of targeted areas and are associated mainly with a spatial reallocation of jobs and households.

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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. Under the auspices of the OZ program, 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 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] Thus, 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]

Early evidence on the effects of OZs was generally negative.[4] A 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.[5] Early research also tended to focus on only a single dimension of the program's effects on employment – for example, its effects on job creation by businesses, or its effects on employment among zone residents – with little attempt to reconcile what are sometimes ostensibly conflicting findings.

In this paper, we provide longer-term and more comprehensive evidence on the effects of OZs on local employment. To extend and enrich prior work on the OZ program's impacts, we

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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) and https://www.irs.gov/newsroom/treasury-irs-provide-guidance-for-opportunity-zone-investments-in-rural-areas-under-the-one-big-beautiful-bill.
[4] This evidence is discussed in Section II.
[5] For example, see https://eig.org/wp-content/uploads/2023/03/Examining-the-Latest-Multi-Year-Evidence-on-Opportunity-Zones-Investment.pdf.

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). Using inverse probability weighting (IPW) methods that leverage institutional rules for tract eligibility, we find that the OZ program increased job creation among businesses in targeted areas. However, a large share of the newly created jobs in zones is offset by declines in nearby low-income tracts. We detect gains in OZ resident employment over the longer run, but the increase comes from jobs at workplaces outside of OZs. Moreover, changes in migration patterns and the demographic composition of zones indicate that these new jobs are likely held by new as opposed to existing residents. Overall, while the OZ program may have increased the number of jobs located in designated zones, its impacts on overall employment, and on employment specifically among preexisting residents of targeted areas, have likely been modest.

Motivated by recently proposed policy changes that increased the relative size of tax incentives for OZ investments and loosened criteria for qualifying investments in rural areas, we also explore heterogeneity in the effects of OZ designation across different geographic areas. We find that, to date, the positive effects of OZ designation in terms of both workplace and resident employment growth are stronger for urban tracts than for rural tracts. We can rule out that this is driven by the higher average initial poverty rates in urban OZs, or by the fact that urban OZs are more likely to be geographically clustered. However, given limitations of our data, we cannot disentangle whether the more muted effects in rural OZs are attributable to more limited OZ investment – something that could potentially be remedied by larger tax benefits for investing in rural OZs – as opposed to a smaller impact of the investments that occur those zones.

Our results contribute to the literature on OZs, which as discussed in Section II, has largely considered the program's impacts on outcomes measured only within the first few years after its implementation (at most). It is plausible that the program's longer-run effects on employment could be smaller or larger than its short-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; Garg, 2025). Indeed, some evidence indicates that one-time increases in local job opportunities can have persistent impacts on communities (Freedman, 2017; Garin and Rothbaum, 2025). Moreover, there may have been meaningful changes in zone economic conditions as more OZ capital was deployed in targeted areas 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. We provide longer-term evidence on the effects of OZs on employment, covering a period extending well beyond the pandemic.

Prior work on OZs has also typically focused on a single measure or dimension of the program's labor market effects.[6] We provide a more comprehensive perspective than previous studies on the OZ program by examining its effects on both workplace and resident employment. We also consider the extent to which investments subsidized by the program have had positive or negative spillovers in nearby tracts, and whether they have 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.

[6] One exception is Arefeva et al. (2025), who also find that OZ designation has positive effects on workplace employment, but also that many of the new jobs are likely taken by residents of other tracts.

II. The Opportunity Zone Program

A. Program Structure

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 (Alm et al., 2021; Frank et al., 2022; Eldar and Garber, 2023; Corinth and Feldman, 2024).

Under the recent tax legislation (OBBB), 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). The OBBB also increased the size of tax incentives for OZ investments in rural areas relative to urban areas and loosened the criteria for qualifying investments in rural areas. 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 and program design more generally. Furthermore, findings on 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 outcomes measured at most within a few years of when OZ benefits took effect.

B. Background on Place-Based Policies and OZs

There has been renewed interest in place-based policies in recent years, spurred at least in part by research on the critical role that 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., 2025). 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 led people and families to move to regions with greater job opportunities (Austin et al., 2018; Zabek, 2024). 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).

While there may be some cause for optimism, there are also reasons to be more 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 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 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.[7] In place-based policies like the OZ program, in which subsidized firms can hire workers living outside targeted areas, any employment effects could also be geographically diffuse (Freedman, 2015; Cerqua and Pellegrini, 2022).

Third, like many past state enterprise zone 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

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

incentives.[8] 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 granularity at which OZs are defined (census tracts) may create substantial scope for reallocation of business activity. 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) and 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.[9]

C. Early Evidence on Opportunity Zones

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)

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[8] As a notable contrast, the California Competes Tax Credit (CCTC) directly incentivizes hiring and 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, have likely contributed to the CCTC's relative effectiveness at creating jobs (Freedman et al. 2023a, Hyman et al. 2023).

[9] 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.

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.

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.

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. For example, Arefeva et al. (2025) use the YourEconomy Time Series through 2021. Atkins et al. (2023) use Burning Glass data through March 2020, and ACS five-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 research differs by adopting a richer perspective that looks at multiple dimensions of the effects of OZs, as well as a longer-term assessment using more recent data than nearly all of the early studies.

III. 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). The public-use files we use provide tract-level averages for five-year periods.

Our primary employment outcomes are drawn from the LEHD Origin-Destination Employment Statistics (LODES) for 2013-2022. 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 at the census tract level. The LODES data specifically allow us to measure the number of resident jobs (i.e., total jobs held by individuals living in a tract), workplace jobs (i.e., total jobs held by individuals working for pay in a tract), and commuting flows by tract and year. The commuting flows give us information on the residential tracts of people working in OZs and other tracts, as well as the workplace tracts of people living in OZs and other tracts. As described below, we use these origin-destination data to ask whether jobs created at firms in OZs tend to go disproportionately to residents of OZs, residents of non-OZ LICs, or residents of non-LICs (the latter being

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

relatively more affluent areas). We similarly ask whether jobs held by people living in OZs are disproportionately located in OZs, non-OZ LICs, or non-LICs. We use all primary jobs in the LODES data.[11]

For our main analysis, we restrict attention to designated and eligible tracts that are LICs.[12] 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 large number of higher-income tracts as controls. These tracts are less comparable to the final set of designated tracts.

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. We construct a consistent sample across datasets and outcomes for our analyses, excluding tracts that are missing data for any necessary variables in either the ACS or the LODES (except using 2023 ACS data even though the LODES currently only extends through 2022).[13] Overall, our main sample includes 6,781 designated OZ tracts and 20,296 non-designated LICs.[14]

Descriptive statistics for the (unweighted) sample of non-OZ LICs and OZs 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

[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.
[12] We exclude from the analysis Puerto Rico, where all eligible LICs were designated as Opportunity Zones.
[13] There is one exception discussed in the notes to Table 7.
[14] Our results are robust to using the largest possible dataset for any given outcome, with one exception discussed in the notes to Table 4.

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 job count in OZs relative to non-OZs measured 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.

IV. Empirical Approach

A. Basic Event Study Design

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, for which we have annual data, 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 independently affect outcomes.[15] The year fixed effects in the model

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

(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} capture pre-and post-treatment differences in outcomes between OZs and comparison tracts each year. These 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 in which we have only outcomes measured as five-year averages, we cannot do a yearly event study. We instead estimate a simple difference-in-differences model with one five-year pre-treatment and one five-year post-treatment observation for each treated and control tract.[16] 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 five-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.[17,]

B. Selection and Parallel Trends

Previous work on the OZ program that used eligible tracts as controls pointed to violations of parallel trends in the pre-treatment period, with OZ designation being associated with prior economic improvements in tracts (Brazil et al., 2021; Eldar and Garber, 2023; Freedman et al., 2023b). We thus construct a control group using a data-driven approach to

[16] As described below, we also use the 2008-2012 ACS to provide some evidence on pre-treatment trends.
[17] 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.

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).

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.[18] 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)

[18] 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.

tracts if they were not treated. Note that we construct a separate set of weights for each outcome for which we estimate the model.

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 five-year period of the ACS data (2008-2012).

The IPW method models the treatment. Regression adjustment methods further allow us to model the outcome to account for non-random treatment assignment. Regression adjustment methods construct counterfactuals by fitting separate linear regression models for the treated and 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 (Tan, 2010). 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.

In one robustness test, we additionally include state-by-year fixed effects in our outcome models to absorb differential changes over time in outcomes across geographies at a higher level of aggregation than census tracts, perhaps attributable to state-level policy changes, impacts of the pandemic, etc.[19,20] In another robustness check, we winsorize the propensity weights, excluding control tracts in the top and bottom five 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.

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 and Analyses

We begin by studying the impacts of OZ designation on tracts, estimating effects on jobs held by residents ("resident jobs") and jobs among businesses in the tract ("workplace jobs"), as well as the employment rate, median earnings, and poverty rate of tract residents. We then

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[19] Note that the addition of state-by-year fixed effects is limited to the outcome models; the cross-sectional treatment model used to calculate the propensity weights is not affected.
[20] 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. As we show later, OZ designation has important spillover effects in geographically proximate areas.
[21] We show summary statistics for the inverse probability weights assigned to the control tracts in Appendix Table A1.

expand our analysis of the OZ program's employment effects along several dimensions. First, we study variation in the effects of OZ designation based on the type of tract in which one lives (looking at changes in workplace jobs), and the type of tract in which jobs are held (looking at changes in resident jobs). Specifically, we first estimate effects on workplace jobs in the tract, but characterizing jobholders in the tract based on where they live: in the tract, in other OZ tracts, in non-OZ LIC tracts, or in non-LIC tracts. We then reverse this, studying effects on jobs held by residents of OZ tracts, but characterizing those jobs based on their work location: in the tract, in other OZ tracts, in non-OZ LIC tracts, and in non-LIC tracts.

Second, we turn to estimating the effects of OZ designation on jobs in other nearby OZ tracts, on jobs in LIC tracts adjacent to OZ tracts, and on jobs in all tracts adjacent to OZ tracts. This analysis is motivated by research on other place-based policies that have often been shown to have spillover effects on employment in nearby areas (e.g., Hanson and Rohlin, 2013). These spillovers could, in principle, amplify or attenuate the impacts of the program on aggregate employment.

Third, we examine changes in the composition of tract residents and residential mobility responses to OZ designation. This analysis relates to concerns that investment incentives cause displacement of original, often poorer residents (e.g., Newman and Wyly, 2006; Layser, 2019; Theodos, 2021). It also helps to address the question of whether it is new vs. existing residents who benefit from the creation of new jobs locally.

Finally, we turn to evidence that speaks to the targeting of OZ eligibility among LIC tracts. In particular, we distinguish the effects of OZs on resident and workplace job creation for OZs in urban vs. rural areas, as well as for other related zone characteristics (including initial poverty rates and the extent of zone clustering) that could account for different effects in urban vs. rural areas. The urban vs. rural question is of particular interest because the recent legislation re-authorizing the OZ program increases the tax incentives, and specifically the capital gains tax reductions, for investments in rural areas, as well as loosened criteria for rural investments to qualify.

V. Results

A. Direct Effects on Designated Areas

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 2022 for LODES variables, and 2023 for ACS 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 is associated with a general improvement in the economic circumstances of residents. The ACS-based estimates in columns (iii)-(v) point to an increase in the resident employment rate as well as a reduction in the resident poverty rate, but no discernible impact on median earnings of tract residents. 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 examining resident and workplace job levels. In column (vi) of the table, we find a statistically significant and economically meaningful 2.4% increase in resident employment in OZs relative to other LICs, based on the ACS data.[22] Meanwhile, in the LODES, we find a smaller 1.2% increase in resident jobs (column (i)), and no economically or statistically significant impact on workplace jobs (i.e., jobs in the tract, regardless of whether held by residents or not) – indeed, the point estimate is negative.

[22] Throughout, we report approximate percentage increases based on log specifications.

However, to the extent that OZs were on different trajectories than non-OZ LICs, there would be 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.[23] We validate the general pattern of differential pre-trends using a sample that also includes an earlier wave of the ACS.[24]

In Figure 2, we show event study estimates for the LODES data, which are annual. Panel (i) shows results for log resident jobs, while Panel (ii) shows results for log workplace jobs. 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 correspond to the "doubly robust" regression-adjusted IPW estimates.[25] Focusing first on the unweighted estimates, we see distinct patterns for resident and workplace jobs growth prior to OZ designation. Prior to designation, resident job counts appear to be low but trending upwards, while workplace job counts are higher in the eventually treated than the control tracts. After designation, resident job growth appears to increase modestly, whereas workplace jobs decrease (in line with the estimates in Table 2).

Reweighting the estimates (red dots) better balances treatment and control groups on pre-

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[23] 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.

[24] 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 (reflected in a negative coefficient on Opportunity Zone × 2008-2012, measured relative to the OZ differential for 2013-2017), 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).

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

treatment trends in both resident and workplace jobs. In the IPW-adjusted results, we continue to see an increase in resident jobs following zone designations, but also simultaneously see an increase in workplace jobs. The doubly robust estimates (green dots) are very similar.

The two alternative sets of treatment effect estimates for LODES resident and workplace job 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 little evidence of increased earnings and, if anything, increases in the poverty rate of residents of OZ-designated tracts (columns (iv)-(v)). We also find a more muted effect on resident employment rates, though the regression-adjusted IPW estimate is statistically significant at the 10% level (Panel B, column (iii)). The estimated positive impact on resident employment measured in the ACS persists, but it is smaller than in Table 2 (1.7% in Panel B, column (vi)). The estimated effects in the LODES data are now consistently positive for both resident and workplace jobs, with the effect being larger for workplace jobs than for resident jobs (1.3% vs. 0.8%). The larger resident jobs 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 five-year ACS averages.[26] We thus regard the LODES estimate as more reliable. The estimated effects on workplace jobs 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 fixed effects added, which can better control for the influences of COVID-19 (and associated policy responses) by state, as well as

[26] Freedman et al. (2023b) did not present results for log residential employment, but we add that to Appendix Table A2 and find the same evidence of differential prior trends.

other state policy changes.[27] Similarly, results using winsorized IPW weights are statistically indistinguishable from the main IPW results.[28] This suggests that the differences between the naive and IPW results are not driven by a small number of extreme-weighted observations.[29]

One possible explanation for the evidence from the ACS data of a positive impact on resident employment (echoed in the LODES), but a limited 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 (and hence a similar employment rate). This could drive increased resident employment levels, but have little bearing on measured employment rates, median earnings, or poverty rates. However, the evidence of this is weak at best. While positive, the estimated effects on the size of the adult civilian population are economically small (about 0.07% in the adjusted estimates) and statistically insignificant.[30] We discuss additional evidence on changes in the population of targeted tracts, including population composition, in Section V.E.

B. Employment Effects Based on Tract of Residence and Tract of Work

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 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, of other OZ tracts, of non-OZ LICs, or of non-LIC tracts. The results appear in Figure 3 and Table 4. We find that

[27] These results are reported in Panel A of Appendix Table A3.
[28] These results are reported in Panel B of Appendix Table A3.
[29] This is confirmed further in Appendix Figure A2, which shows the distribution of each set of weights used. Extreme values are not apparent.
[30] See Appendix Table A4.

the increase in workplace jobs is driven largely by increases in jobs held by residents of other tracts, including a mix of higher income tracts and other OZs.[31] Based on our doubly robust IPW estimates (Panel B of Table 4), for example, we find that OZ designation leads to a 1.9% increase in local workplace jobs held by residents of non-LICs (i.e., more affluent tracts), and a 1.7% increase in jobs held by residents of other OZs. In contrast, we estimate a statistically insignificant 0.4% increase for residents of non-OZ LICs and a statistically insignificant 0.5% decrease in jobs held by residents of the OZ itself.[32]

Based on the overall workplace job estimates in column (ii) Table 3 and those for OZ residents in columns (i) and (ii) of Table 4, fewer than one of every eight newly created jobs in the typical OZ goes to a resident of the same or other OZs. Meanwhile, over 75% of newly created jobs are held by residents of comparatively affluent non-LIC tracts. This result 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.

The larger estimate for resident jobs than for workplace jobs in the same tract (0.8% in column (i) of Table 3, vs. −0.5% (insignificant) in column (i) of Table 4) also suggests that at least some of the growth in OZ resident employment is driven by jobholding outside the OZ.[33] Our results in Figure 4 and Table 5, in which we directly estimate the effects of OZ designation on the location of residents' jobs, confirms this conjecture. We find no statistically or

[31] Arefeva et al. (2025) also find that most workplace jobs created in OZs are likely held by residents of other tracts. However, they do not break out different types of residential tracts for workers.
[32] The results in column (iii) of Table 4 are the one case in which the estimates differ when using the full set of observations for each outcome rather than a consistent sample. When using the largest possible sample, the estimated effect on workplace jobs of residents of non-OZ LIC tracts is about twice as large (1% vs. 0.4-0.5%) and statistically significant.
[33] Busso et al. (2013) report similar evidence for federal Empowerment Zones, with a large but statistically insignificant 12.3 log point increase in non-zone jobs held by zone residents.

economically meaningful changes in resident jobs held at worksites in OZs. Rather, we find that jobs held outside OZs, and even more so outside LICs, by and large fully account for the increase in jobs among residents of OZs. This could reflect changes in the composition of tract residents – an issue we take up below.

C. Workplace Employment Spillovers

Some of the workplace job growth that occurs within zones could come at the expense of surrounding communities. OZs target compact areas within broader labor markets, and employers may simply relocate investments or employment in order to take advantage of zone incentives. We explore this in Figure 5 and Table 6. First, with potential spillovers in mind, we repeat the main analysis excluding LIC control tracts that border OZs; if there are spillovers, border tracts are arguably the most likely to experience effects of the treatment. As shown in column (i) of Table 6, we still detect workplace job gains in OZs, though the magnitude of those gains is smaller than in our main results (0.9%, in Panel B, column (i) of Table 6 vs. 1.3% in Panel B, column (ii) of Table 3). We similarly see slightly more muted, but still positive effects using our IPW and doubly robust approaches in panel (i) of Figure 5. The smaller impact when we exclude adjacent LICs from the set of controls suggests that OZs may displace some workplace jobs from nearby LICs.

Next, we directly investigate spillover effects on low-income communities and other tracts near OZs. Keeping the control group the same, we estimate treatment effects for the LICs adjacent to OZ tracts. The results appear in column (ii) of Table 6 and panel (ii) of Figure 5. The estimates point to a 0.8% reduction in jobs in non-OZ LICs when an adjacent tract is designated as an OZ. Given the number of jobs hosted by each set of tracts prior to treatment (shown at the bottom of Table 6), the estimates imply that approximately 83% of job gains in OZs are offset by losses in nearby LICs.

In the final column of Table 6 (and panel in (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 spillover effect – a slightly larger percentage impact, but applied to a base that is about twice as large. 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 weakens agglomeration externalities.

E. Compositional Effects

OZ designation may have changed the composition of OZ residents. We saw some indirect evidence of this in Table 5, where we found that OZ designation increased the number of resident jobs in non-OZ LIC tracts and in non-LIC tracts. We now turn to more direct evidence on compositional changes among residents of OZs.

In Table 7, we report estimated effects of OZ designation on demographic characteristics of tract residents, again using two five-year ACS samples spanning 2013-2023. We find that OZ designation is associated with a significant increase in the share of residents who are White, and declines in the shares who are Hispanic (statistically significant) and Black (not statistically significant). We also find that OZ designation is associated with a statistically significant increase in the share of the population with at least a bachelor's degree. Consistent with in-migration driving the observed shifts in the demographic make-up of OZs, we find in the final two columns of Table 7 that OZs had a relatively high fraction of residents that moved in during the past year – including moves from other labor markets (as proxied by other metro- or micropolitan areas).[34] We have to be a cautious about the ACS results given that they are not based on annual data (in contrast to the LODES), and hence could be biased due to differential pre-treatment trends. Nonetheless, the results are suggestive of important compositional changes in OZs that could help explain the increase in jobs held by residents – while also indicating that the increase is not concentrated among the initial, less-advantaged residents.[35]

F. Urban vs. Rural OZs

We next turn to evidence on different dimensions of heterogeneity in OZ effects that relate to policy choices about the targeting of OZs. As noted earlier, the OBBB increased the size of tax incentives for OZ investments in rural areas relative to urban areas and made it easier for investments in rural areas to qualify. While we cannot yet test whether this newly introduced differential impacts outcomes, we can examine whether OZ effects in rural and urban areas have differed in the first iteration of the program.

To study this issue, we expand our estimating equations to include interactions between OZ designation and whether the tract is "urban."[36] We use a definition of urban that attempts to follow the text of the OBBB: cities and towns with populations greater than 50,000 as well as urbanized areas contiguous and adjacent to those cities and towns. We define these latter urbanized areas as the tracts contiguous and adjacent these cities and towns.[37] We also include

[34] Columns (v) and (vi) of Table 7 are the one instance in which tracts with missing values for the outcome are not dropped in the models for all other outcomes we study. The mover share variables have significantly more missing values than other outcome variables.

[35] Consistent with in-migration, we also find that OZ designation is associated with increases in residential address counts based on HUD-USPS address count data (see Appendix Figure A6 and Appendix Table A5). In line with our estimated increases in workplace jobs in OZs, we also find increases in business address counts using the same data.

[36] We use only the IPW approach because the doubly robust approach is not amenable to the interactive specification.

[37] The OBBB's definition of urban vs. rural has been used in prior legislation (see 7 U.S.C. 1991(a)(13)(A)) and historically guided some of the USDA's rural development programs. Because of recent changes in the U.S. Census Bureau's classification criteria, the USDA has adopted a slightly

interactions between the urban dummy and year fixed effects to allow urban tracts to be on different trajectories over time. The results are reported in Table 8. We find no evidence of positive impacts on LODES measures of jobs or on ACS measures of employment, earnings, or poverty in rural tracts. However, we find positive and statistically significant effects of OZ designation on resident and workplace job growth in urban areas. OZ designation also appears to increase resident employment rates, raise median earnings, and reduce poverty rates in urban tracts (although we again raise the caveat that ACS results based on two 5-year periods may not control for pre-treatment trends to the same extent as the LODES results).[38]

The positive effects in urban areas but not rural areas could be because urbanity is correlated with other characteristics that might be complementary to OZ investment in driving stronger growth. We explore two features of urban OZs that might help explain the stronger effects in urban areas, and that could perhaps help better inform targeting of OZ benefits. First, urban OZs tend to have higher pre-OZ program poverty rates. Second, in part because they are smaller in terms of land area, urban OZs are more likely to be geographically clustered.[39]

To disentangle the potential role of these factors, we consider a model in which we fully interact OZ status with an urban indicator, the pre-OZ program tract poverty rate, and the share of neighboring tracts that are OZs to ask whether these other factors account for the relatively positive effects observed in urban OZs. In doing so, we also include interactions with the share of neighboring tracts that are LICs, as this bounds the share of neighboring tracts that could be

different definition of urbanized areas, which the OZ program may also adopt (IRS Notice 2025-50). We obtain very similar results using the more conventional definition of urban based on CBSAs.

[38] The stronger effects we find for urban tracts is in line with Arefeva et al. (2025), who also find stronger positive impacts of OZs on workplace employment in tracts located within metropolitan areas.

[39] See Appendix Table A6, in which we regress an indicator for being an OZ on pre-OZ program poverty rate and the share of surrounding tracts that are OZs (controlling for the share of surrounding tracts that are LICs). We find a strong association between an OZ's urbanity, poverty, and OZ clustering. The maps in Appendix Figures A3-A5 also provide anecdotal evidence on how clustering varies in more urban vs. rural parts of California and New York.

OZs. Economic conditions of neighboring tracts, which are in part reflected in LIC status, could also independently be an important determinant of the effects of OZ designation. We saturate the model by interacting the urban, poverty, and clustering variables (and their interactions) with year dummies as well, allowing for arbitrary changes in outcomes across time for tracts associated with different initial conditions.

The results appear in Table 9. We find some evidence that OZ designation has stronger effects on poverty reduction in initially higher poverty tracts, and that this accounts for much of the previously estimated differential effect of OZ designation in urban areas. However, we find little evidence that the stronger positive effects of OZ designation on resident and workplace job gains, as well as on median earnings, in urban tracts are attributable to their relatively higher poverty rates or to the fact that they tend to be clustered with other OZs. For these outcomes, coefficient estimates on the interaction between OZ status and the urban indicator in Tables 8 and 9 are very similar.

The results of this horserace suggest that OZs have more meaningful employment effects in urban areas per se, and not simply because OZs in urban areas tend to be higher poverty or because they often are more clustered. This implies that there is something else about urban tracts either that attracts more OZ investment, or that leads OZ investment to have stronger positive effects on employment. If it is the former, then the stronger incentives to invest in rural areas embedded in the OBBB may help improve the OZ program's efficacy outside of major cities. If it is the latter, however, the larger tax breaks for rural investments may not be as successful at inducing meaningful changes in economic conditions in rural areas. Teasing apart these different mechanisms, which would likely require information on the location and perhaps also type of OZ-subsidized investments, is an important question for future research. Future work could also directly exploit the additional variation in tax incentives across urban and rural areas introduced by the OBBB.

VI. 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 OZ program by comprehensively studying many dimensions of the OZ program's effects on employment, including its direct effects on resident and workplace jobs in designated areas as well as its spillover effects on other communities. We also provide longer-run evidence on the employment effects of OZs than prior literature.

We find that the OZ program increased job creation among businesses in targeted areas, but that a large share of the newly created jobs in zones is offset by declines in nearby low-income tracts. We detect gains in OZ resident employment over the longer run, but the increase comes from jobs with workplaces outside of OZs. Given changes in migration patterns and the demographic composition of zones we observe, these new jobs are likely held by new as opposed to existing residents. We also explore heterogeneity in effects and find that, to date, the program has had stronger positive effects in urban tracts than rural tracts. This might imply that recent changes to the OZ program that strengthen investment incentives in rural relative to urban areas might fail to generate positive impacts in rural areas, unless the strengthened incentives in rural areas spur positive impacts where the initial incentives failed to do so.

Our results not only provide a more comprehensive and 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 other neighborhoods, including many more affluent neighborhoods. This effectively undoes some of the redistributive goals of the program.

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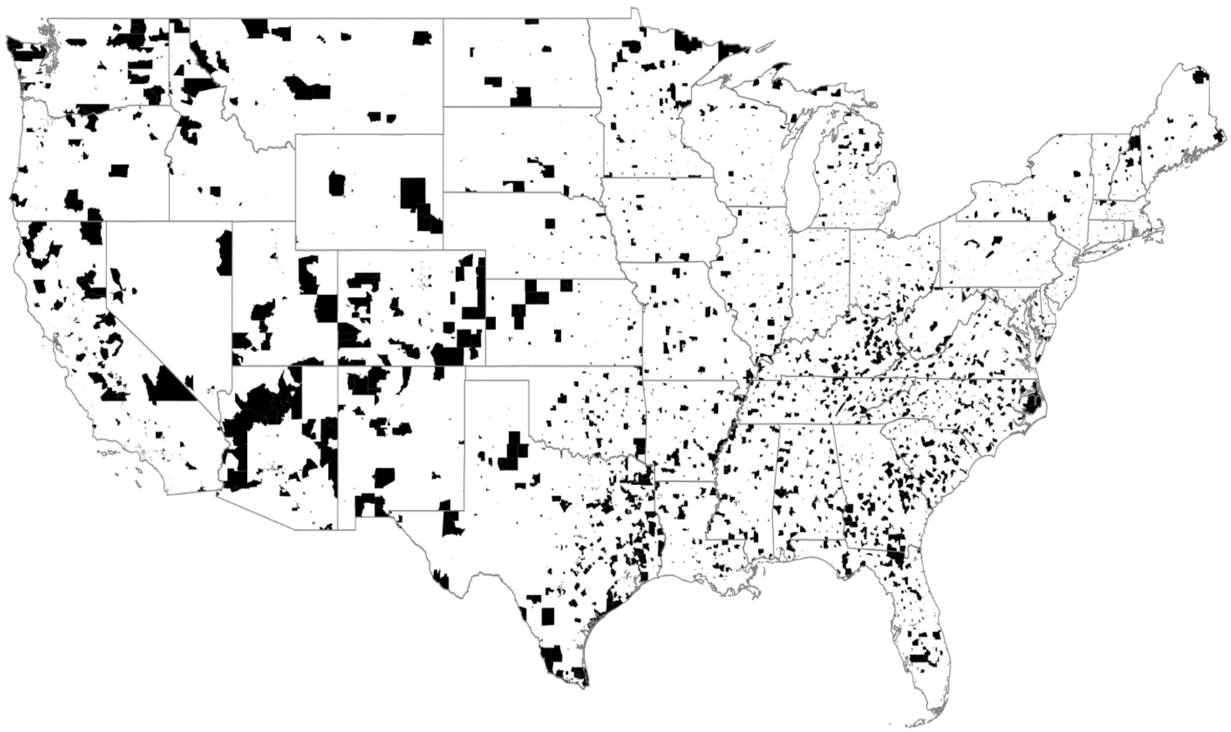
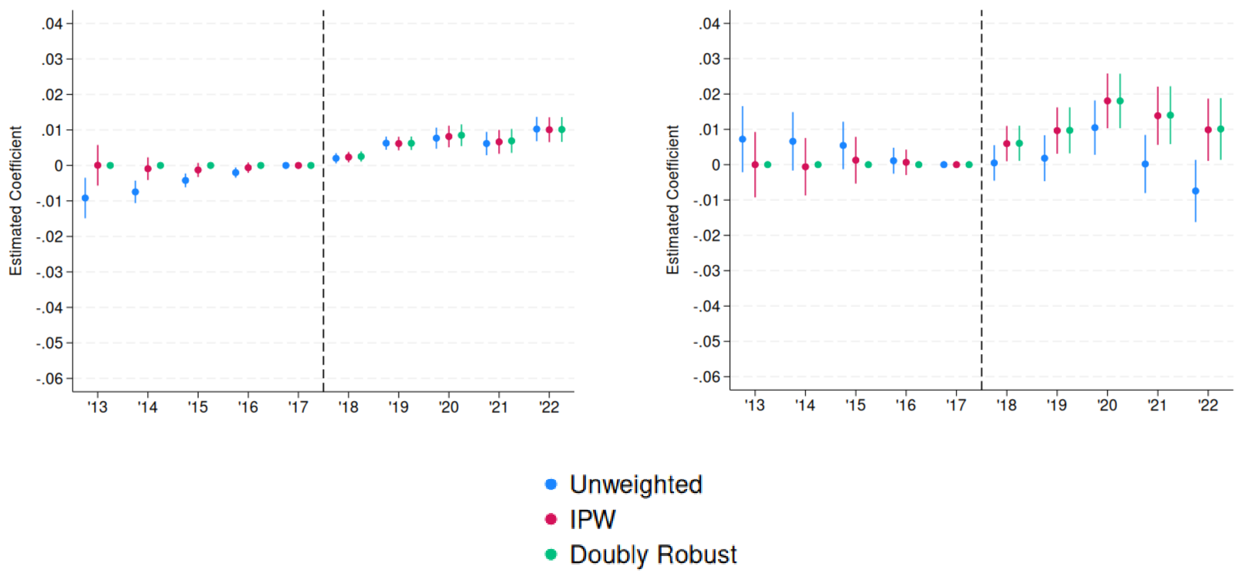
Figure 1. Opportunity Zones



Notes: Shaded areas on the map are census tracts designated as Opportunity Zones in 2018. Information on Opportunity Zones are from the Community Development Financial Institutions Fund at the U.S. Department of the Treasury.

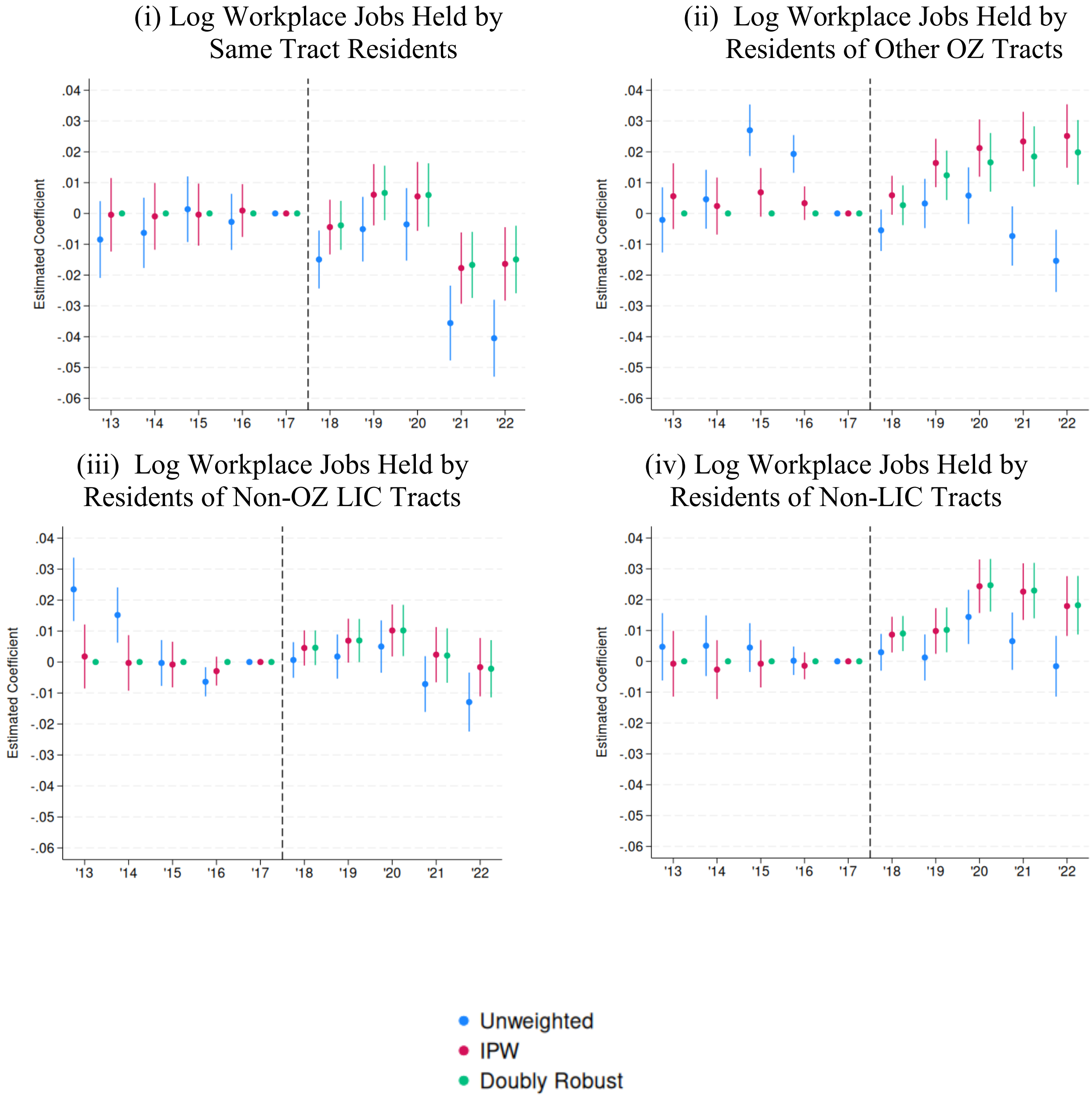
Figure 2. Event Studies for LODES Jobs

(i) Log Resident Jobs (ii) Log Workplace Jobs



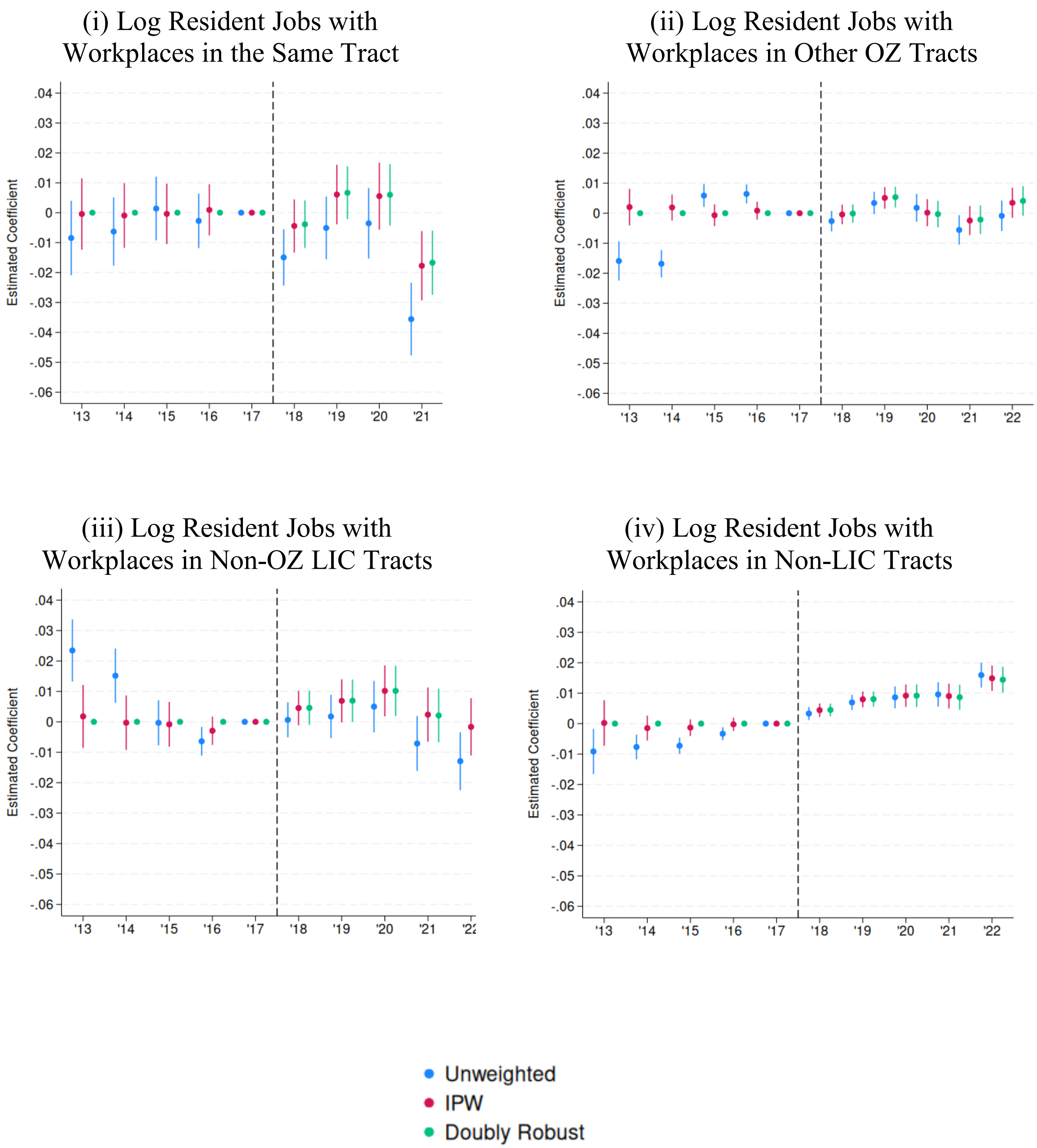
Notes: The panels show point estimates from event studies using all eligible but not designated LICs as controls. The blue dots in each figure correspond to the unweighted estimates. The red dots in each figure correspond to the IPW estimates. The green dots in each figure correspond to the "doubly robust" regression-adjusted IPW estimates.

Figure 3. Event Studies for LODES Workplace Jobs by Resident Location



Notes: The panels show point estimates from event studies using all eligible but not designated LICs as controls. The blue dots in each figure correspond to the unweighted estimates. The red dots in each figure correspond to the IPW estimates. The green dots in each figure correspond to the "doubly robust" regression-adjusted IPW estimates.

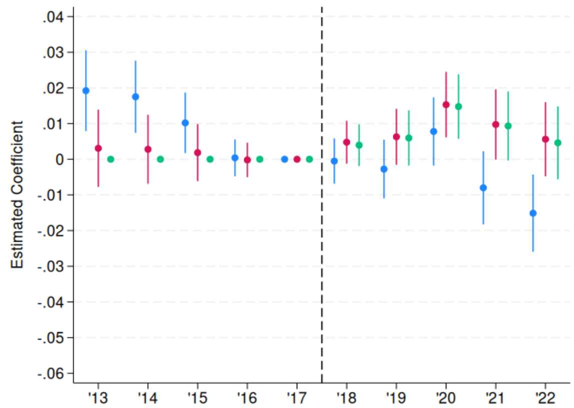
Figure 4. Event Studies for LODES Resident Jobs by Workplace Location



Notes: The panels show point estimates from event studies using all eligible but not designated LICs as controls. The blue dots in each figure correspond to the unweighted estimates. The red dots in each figure correspond to the IPW estimates. The green dots in each figure correspond to the "doubly robust" regression-adjusted IPW estimates.

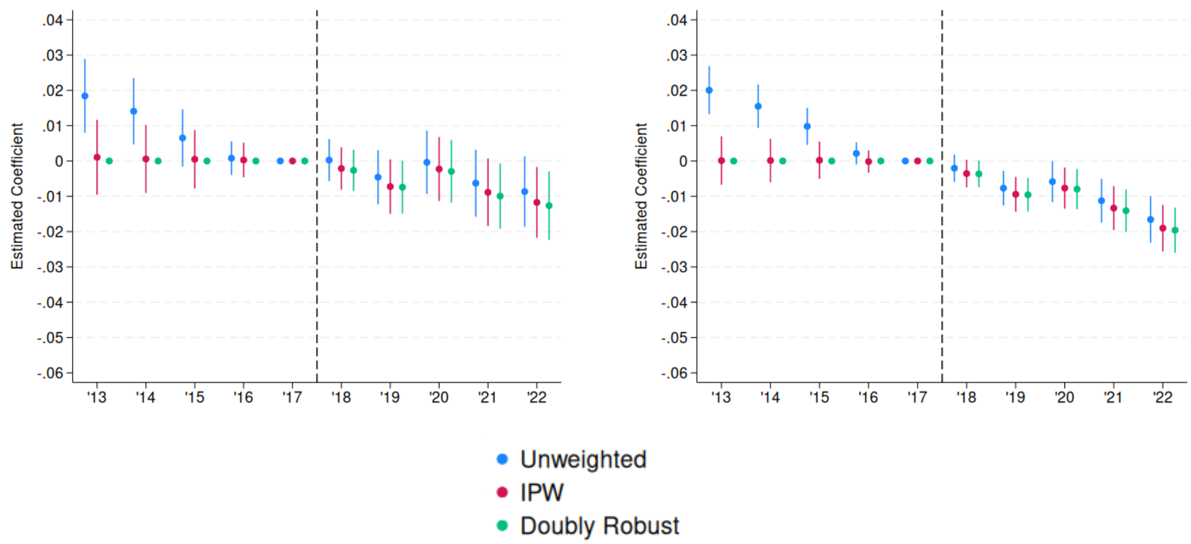
Figure 5. Event Studies for LODES Workplace Jobs in OZs and Adjacent Tracts

(i) Log Workplace Jobs Omitting Adjacent Tracts from Controls



(ii) Log Workplace Jobs in Adjacent LICs, Non-Adjacent LICs as Controls

(iii) Log Workplace Jobs in All Adjacent Tracts, All Tracts Adjacent to LIC but Not OZ as Controls



Notes: Panel (i) shows event study estimates of the effects of OZ designation using only non-adjacent eligible tracts as controls. Panel (ii) shows event study estimates of the effects of OZ designation on adjacent eligible tracts, again using non-adjacent eligible tracts as controls. Panel (iii) shows event study estimates of the effects of OZ designation on all nearby tracts, using tracts that are not adjacent to a designated OZ but are adjacent to eligible LICs as controls. The blue dots in each figure correspond to the unweighted estimates. The red dots in each figure correspond to the IPW estimates. The green dots in each figure correspond to the "doubly robust" regression-adjusted IPW estimates.

Table 1. Descriptive Statistics for Opportunity Zones and Control Tracts

	Unweighted				ATT Weights	
	Untreated (non-OZ LICs)		Treated (OZs)		Untreated (non-OZ LICs)	
	(i)	(ii)	(iii)	(iv)	(v)	(vi)
	2013-2017	2019-2023*	2013-2017	2019-2023*	2013-2017	2019-2023*
Panel A: ACS 5-Year Averages						
Resident employment rate	56%	57%	53%	55%	53%	55%
	(10%)	(10%)	(10%)	(11%)	(11%)	(11%)
Resident median earnings	$26,293	$38,127	$24,113	$35,827	$24,286	$36,002
	($7,055)	($11,057)	($6,951)	($10,609)	($6,783)	($10,437)
Adult population	4,238	4,503	4,143	4,404	4,159	4,421
	(1,889)	(2,204)	(1,949)	(2,240)	(1,871)	(2,179)
Resident poverty rate	23%	19%	29%	24%	28%	23%
	(11%)	(11%)	(13%)	(12%)	(14%)	(13%)
Resident employment	1,872	2,079	1,718	1,953	1,767	1,969
	(914)	(1,112)	(906)	(1,111)	(886)	(1,073)
Panel B: Annual LODES Data						
Resident jobs	1,639	1,682	1,538	1,596	1,564	1,610
	(745)	(826)	(748)	(834)	(723)	(800)
Workplace jobs	1,705	1,745	2,786	2,832	2,991	3,064
	(3,649)	(3,850)	(5,083)	(5,163)	(6,668)	(6,983)
Workplace jobs held by residents						
…in the same tract	75	75	119	117	104	103
	(110)	(108)	(172)	(167)	(139)	(137)
…in other OZ tracts	139	142	363	365	446	451
	(260)	(268)	(475)	(477)	(822)	(828)
...in non-OZ LIC tracts	558	562	768	768	727	726
	(1,006)	(1,021)	(1,300)	(1,296)	(1,386)	(1,393)
...of non-LIC tracts	909	937	1,490	1,533	1,558	1,605
	(2,244)	(2,367)	(3,162)	(3,243)	(3,826)	(4,004)
Tracts	20,296	20,296	6,781	6,781	20,296	20,296

Notes: Variables in Panel A are derived from ACS 5-year averages for the years specified in each column. Variables in Panel B are derived from the LODES for the years specified in each column. *The post-period is limited to 2019-2022 for the LODES due to data limitations. Standard deviations in parentheses. In our main sample, we require consistent balanced LODES and ACS panels. Some tracts (1,782 treated and 3,037 control) are dropped due to missing values or zeroes for variables for which we use logs. All tracts in Alaska, Mississippi, and Michigan are excluded because of the lack of jobs data in the LODES for at least part of our sample period.

Table 2. Naïve Difference-in-Difference Estimates of OZ Effects on Jobs and Residents

	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Data:	LODES	LODES	ACS	ACS	ACS	ACS
	Log Resident Jobs	Log Workplace Jobs	Emp. Rate	Avg. Earnings	Poverty Rate	Log Resident Emp.
Opportunity Zone	0.0122***	-0.00281	0.00842***	-120.4	-0.0118***	0.0240***
	(0.00176)	(0.00408)	(0.00104)	(112.0)	(0.00132)	(0.00366)
Tract FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
2013-17 25th %ile	1,110	419	0.487	21,632	0.164	1,170
2013-17 Mean	1,676	2,023	0.550	25,747	0.247	1,833
2013-17 75th %ile	2,105	2,123	0.619	29,782	0.308	2,335
Tracts	27,077	27,077	27,077	27,077	27,077	27,077
Obs.	243,693	243,693	54,154	54,154	54,154	54,154

Notes: "Opportunity Zone" refers to the OZ × POST term in our estimating equation. 2018 is omitted. Heteroskedasticity robust standard errors (in parentheses) are clustered on census tract.
* $p<0.10$ ** $p<0.05$ *** $p<0.01$

Table 3. IPW and Regression-Adjusted IPW Estimates of OZ Effects on Jobs and Residents

	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Data:	LODES	LODES	ACS	ACS	ACS	ACS
	Log Resident Jobs	Log Workplace Jobs	Emp. Rate	Avg. Earnings	Poverty Rate	Log Resident Emp.
	A. IPW Treatment on the Treated Estimates					
Opportunity Zone	0.00832***	0.0126***	0.00258**	-2.887	0.00409***	0.0196***
	(0.00180)	(0.00409)	(0.00106)	(111.8)	(0.00154)	(0.00374)
	B. Regression-Adj. IPW Treatment on the Treated Estimates					
Opportunity Zone	0.00795***	0.0130***	0.00169*	4.611	0.00481***	0.0168***
	(0.00138)	(0.00355)	(0.00101)	(112.1)	(0.00150)	(0.00376)
Tract FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
2013-17 25^{th} %ile	1,110	419	0.487	21,632	0.164	1,170
2013-17 Mean	1,676	2,023	0.550	25,747	0.247	1,833
2013-17 75^{th} %ile	2,105	2,123	0.619	29,782	0.308	2,335
Tracts	27,077	27,077	27,077	27,077	27,077	27,077
Obs.	243,693	243,693	54,154	54,154	54,154	54,154

Notes: "Opportunity Zone" refers to the OZ × POST term in our estimating equation. 2018 is omitted. IPW is based on pre-treatment outcomes specific to each model. Regression-Adj. IPW refers to the doubly robust difference-in-difference method developed in Sant'Anna and Zhao (2020) and generalized in Callaway and Sant'Anna (2021). Heteroskedasticity robust standard errors (in parentheses) are clustered on census tract.

* p<0.10 ** p<0.05 *** p<0.01

Table 4. OZ Effects on Workplace Jobs by Resident Location

	(i)	(ii)	(iii)	(iv)
	Log Workplace Jobs Held by Residents of			
	...the Same Tract	…Other OZ Tracts	...Non-OZ LIC Tracts	...Non-LIC Tracts
A. IPW Treatment on the Treated Estimates				
Opportunity Zone	-0.00546	0.0179***	0.00490	0.0198***
	(0.00447)	(0.00459)	(0.00427)	(0.00446)
B. Regression-Adj. IPW Treatment on the Treated Estimates				
Opportunity Zone	-0.00475	0.0168***	0.00426	0.0190***
	(0.00422)	(0.00428)	(0.00373)	(0.00389)
Tract FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
2013-17 25^{th} %ile	18	37	154	169
2013-17 Mean	88	201	627	1,078
2013-17 75^{th} %ile	97	228	699	1,051
Tracts	27,077	27,077	27,077	27,077
Observations	243,693	243,693	243,693	243,693

Notes: "Opportunity Zone" refers to the OZ × POST term in our estimating equation. 2018 is omitted. IPW is based on pre-treatment outcomes specific to each model. Regression-Adj. IPW refers to the doubly robust difference-in-difference method developed in Sant'Anna and Zhao (2020) and generalized in Callaway and Sant'Anna (2021). Heteroskedasticity robust standard errors (in parentheses) are clustered on census tract.

* p<0.10 ** p<0.05 *** p<0.01

Table 5. Effects on Resident's Jobs by Workplace Location

	(i)	(ii)	(iii)	(iv)
	Log Resident Jobs at Workplaces in			
	...the same tract	…other OZ tracts	...non-OZ LIC tracts	...non-LIC tracts
A. IPW Treatment on the Treated Estimates				
Opportunity Zone	-0.00546	0.000746	0.0141***	0.0108***
	(0.00447)	(0.00220)	(0.00235)	(0.00207)
B. Regression-Adj. IPW Treatment on the Treated Estimates				
Opportunity Zone	-0.00475	0.00175	0.0137***	0.0101***
	(0.00422)	(0.00194)	(0.00230)	(0.00166)
Tract FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
2013-17 25^{th} %ile	18	148	315	448
2013-17 Mean	88	283	529	779
2013-17 75^{th} %ile	97	365	675	1,017
Tracts	27,077	27,077	27,077	27,077
Observations	243,693	243,693	243,693	243,693

Notes: "Opportunity Zone" refers to the OZ × POST term in our estimating equation. 2018 is omitted. IPW is based on pre-treatment outcomes specific to each model. Regression-Adj. IPW refers to the doubly robust difference-in-difference method developed in Sant'Anna and Zhao (2020) and generalized in Callaway and Sant'Anna (2021). Heteroskedasticity robust standard errors (in parentheses) are clustered on census tract.

* p<0.10 ** p<0.05 *** p<0.01

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

	(i)	(ii)	(iii)
		Log Workplace Jobs	
		A. IPW Treatment on the Treated Estimates	
Opportunity Zone	0.00778		
	(0.00480)		
Near OZ		-0.00796	-0.01239***
		(0.00464)	(0.00300)
		B. Regression-Adj. IPW Treatment on the Treated Estimates	
Opportunity Zone	0.00871**		
	(0.00444)		
Near OZ		-0.00820**	-0.01273***
		(0.03400)	(0.00261)
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: Column (i) estimates the effects on designated OZs using distant (non-adjacent) LICs as controls. Column (ii) estimates spillovers on adjacent LICs, again using non-adjacent LICs as controls. Column (iii) estimates spillovers on all nearby tracts, using tracts that are not adjacent to a designated OZ but are adjacent to eligible LICs as controls. IPW is based on pre-treatment outcomes specific to each model. Regression-Adj. IPW refers to the doubly robust difference-in-difference method developed in Sant'Anna and Zhao (2020) and generalized in Callaway and Sant'Anna (2021). Heteroskedasticity robust standard errors (in parentheses) are clustered on census tract. * p<0.10 ** p<0.05 *** p<0.01

Table 7. Effects on Residential Composition

	(i)	(ii)	(iii)	(iv)	(v)	(vi)
	White Share	Hispanic Share	Black Share	Share College Educated	Share Moved in Past year	Share Moved Areas
			A. IPW Treatment on the Treated Estimates			
Opportunity Zone	0.00734***	-0.00301***	-0.00152	0.00307***	0.00387***	0.00316***
	(0.00104)	(0.00101)	(0.00101)	(0.000952)	(0.00107)	(0.000840)
			B. Regression-Adj. IPW Treatment on the Treated Estimates			
Opportunity Zone	0.00732***	-0.00301***	-0.00152	0.00305***	0.00359***	0.00311***
	(0.00104)	(0.00101)	(0.00101)	(0.000952)	(0.00108)	(0.000840)
Tract FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
2013-17 25^{th} %ile	0.175	0.035	0.017	0.105	0.104	0.18
2013-17 Mean	0.475	0.247	0.198	0.193	0.174	0.55
2013-17 75^{th} %ile	0.760	0.396	0.283	0.239	0.221	0.73
Tracts	27,077	27,077	27,077	27,077	25,825	25,825
Observations	54,154	54,154	54,154	54,154	51,650	51,650

Notes: "Opportunity Zone" refers to the OZ × POST term in our estimating equation. 2018 is omitted. IPW is based on pre-treatment outcomes specific to each model. Regression-Adj. IPW refers to the doubly robust difference-in-difference method developed in Sant'Anna and Zhao (2020) and generalized in Callaway and Sant'Anna (2021). "Share Moved Areas" refers to the share of residents which, in the previous year, lived outside their current metro- or micropolitan area. Areas not inside either a metro- or micropolitan area are treated as one residual rural area. Heteroskedasticity robust standard errors (in parentheses) are clustered on census tract.
* p<0.10 ** p<0.05 *** p<0.01

Table 8. Heterogeneity by Urban vs. Rural Status, IPW Treatment on the Treated Estimates

	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Data:	LODES	LODES	ACS	ACS	ACS	ACS
	Log Resident Jobs	Log Workplace Jobs	Emp. Rate	Median Earnings	Poverty Rate	Log Resident Emp.
Opportunity Zone	-0.00396	-0.00516	-0.000449	-325.3**	0.00645***	-0.00267
	(0.00254)	(0.00533)	(0.00149)	(151.7)	(0.00194)	(0.00554)
Opportunity Zone × Urban	0.0250***	0.0347***	0.00557***	651.6***	-0.00653**	0.0388***
	(0.00353)	(0.00797)	(0.00210)	(220.3)	(0.00294)	(0.00752)
Tract FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Urban × Year FE	Y	Y	Y	Y	Y	Y
2013-17 25th %ile	1,110	419	0.487	21,632	0.164	1,170
2013-17 Mean	1,676	2,023	0.550	25,747	0.247	1,833
2013-17 75th %ile	2,105	2,123	0.619	29,782	0.308	2,335
Tracts	27,077	27,077	27,077	27,077	27,077	27,077
Obs.	243,693	243,693	54,154	54,154	54,154	54,154

Notes: “Opportunity Zone” refers to the OZ × POST term in our estimating equation. The “Urban” indicator is based on the OBBB definition of urban tracts. 2018 is omitted. IPW is based on pre-treatment outcomes specific to each model. Heteroskedasticity robust standard errors (in parentheses) are clustered on census tract.
* p<0.10 ** p<0.05 *** p<0.01

Table 9. Heterogeneity by Urbanity, Poverty Intensity, OZ Clustering

	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Data:	LODES	LODES	ACS	ACS	ACS	ACS
	Log Resident Jobs	Log Workplace Jobs	Emp. Rate	Median Earnings	Poverty Rate	Log Resident Emp.
Opportunity Zone	-0.00871*	-0.00824	-0.00238	-165.4	-0.00135	-0.0101
	(0.00502)	(0.00998)	(0.00269)	(310.1)	(0.00452)	(0.0104)
Opportunity Zone × Urban	0.0244***	0.0393***	0.00350	560.8**	-0.00297	0.0364***
	(0.00403)	(0.00891)	(0.00232)	(249.7)	(0.00281)	(0.00826)
Opportunity Zone × Initial Poverty (Z-score)	-0.00453**	-0.00353	-0.000438	383.6**	-0.00794**	-0.00574
	(0.00230)	(0.00504)	(0.00132)	(136.6)	(0.00302)	(0.00492)
Opportunity Zone × Share of Neighbors OZ	-0.0244**	0.0159	0.00286	208.5	0.00333	-0.0215
	(0.00897)	(0.0206)	(0.00519)	(522.2)	(0.00619)	(0.0185)
Opportunity Zone × Share of Neighbors LIC	0.0151*	0.00758	-0.00214	-198.3	0.0119	0.0102
	(0.00816)	(0.0172)	(0.00440)	(505.6)	(0.00739)	(0.0164)
Tract FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Year FE × Urban	Y	Y	Y	Y	Y	Y
Year FE × Initial Poverty (Z-score)	Y	Y	Y	Y	Y	Y
Year FE × Share of Neighbors OZ	Y	Y	Y	Y	Y	Y
Year FE × Share of Neighbors LIC	Y	Y	Y	Y	Y	Y
2013-17 25th %ile	1,110	419	0.487	21,632	0.164	1,170
2013-17 Mean	1,676	2,023	0.550	25,747	0.247	1,833
2013-17 75th %ile	2,105	2,123	0.619	29,782	0.308	2,335
Tracts	27,077	27,077	27,077	27,077	27,077	27,077
Obs.	243,693	243,693	54,154	54,154	54,154	54,154

Notes: "Opportunity Zone" refers to the OZ × POST term in our estimating equation. The "Urban" indicator is based on the OBBB definition of urban tracts. "Initial Poverty" is based on poverty measured in the 2013-2017 ACS. "Share of Neighbors OZ" and "Share of Neighbors LIC" are the share of adjacent tracts that are designated OZs and LICs, respectively. 2018 is omitted. IPW is based on pre-treatment outcomes specific to each model. Heteroskedasticity robust standard errors (in parentheses) are clustered on census tract.

* $p<0.10$ ** $p<0.05$ *** $p<0.01$

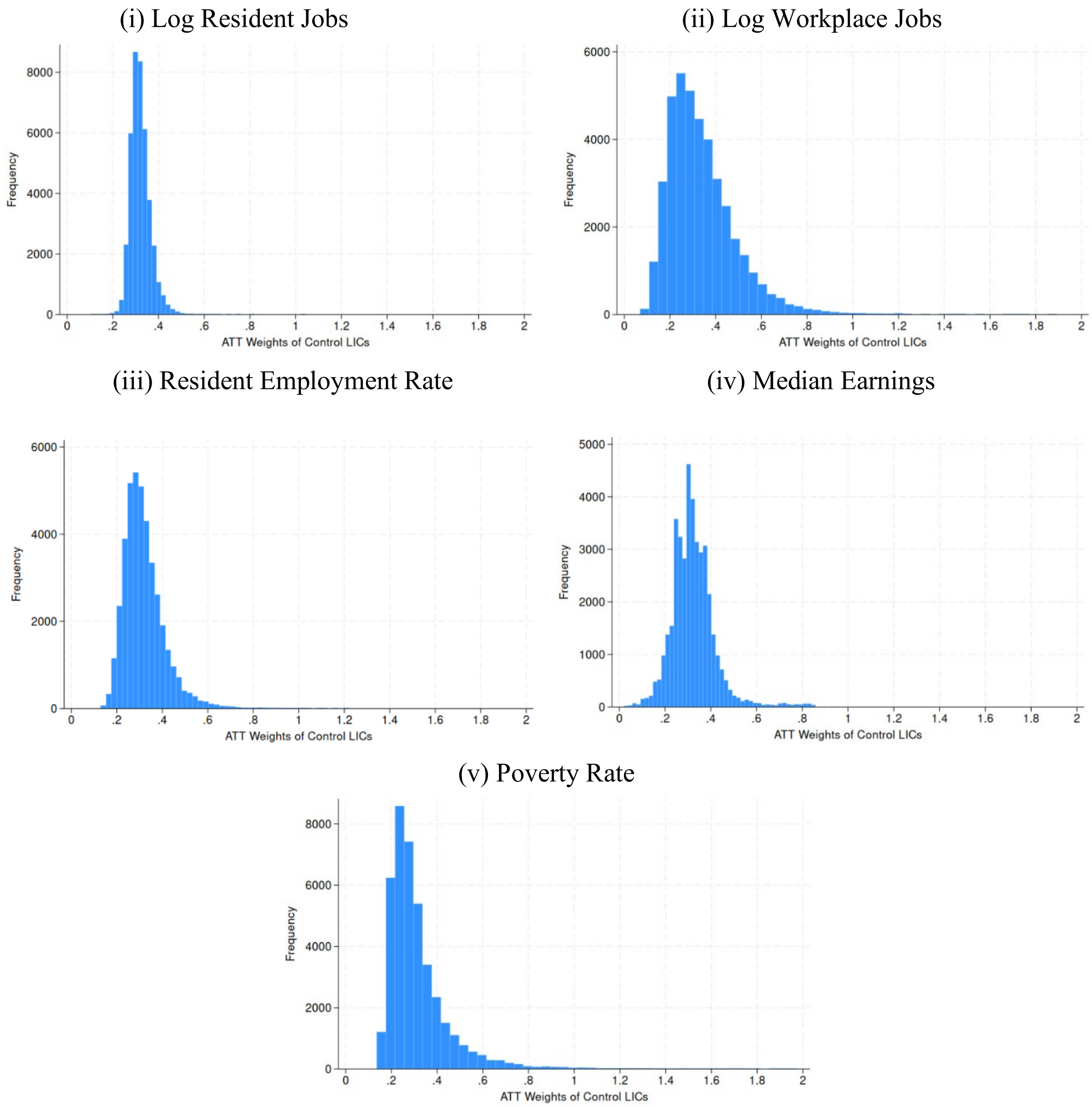
Appendix Figures and Tables

Figure A.1 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, 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



Notes: 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.

Figure A3. Maps of LICs and OZs in Select Areas of California

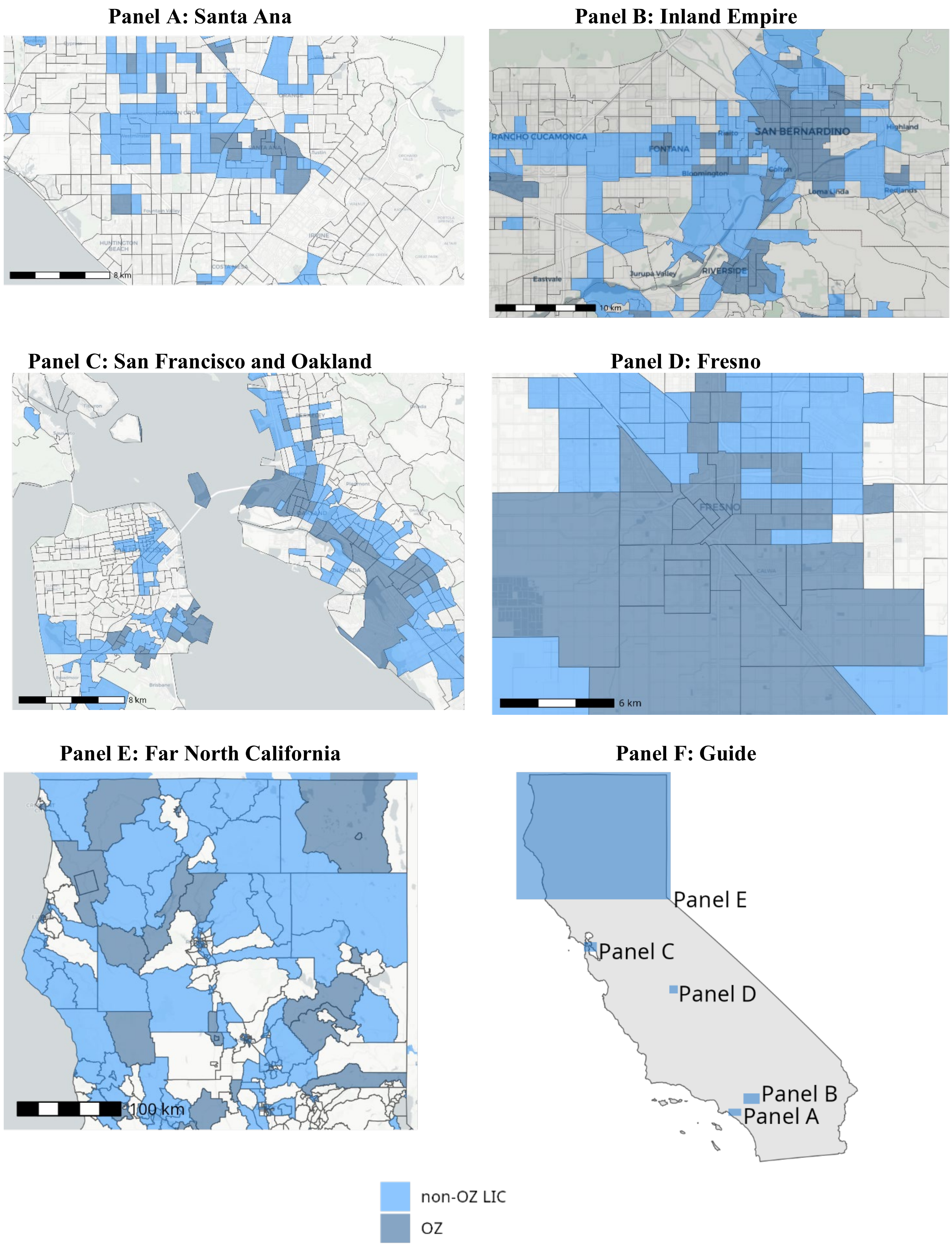


Figure A4. Maps of LICs and OZs in New York at Various Scales

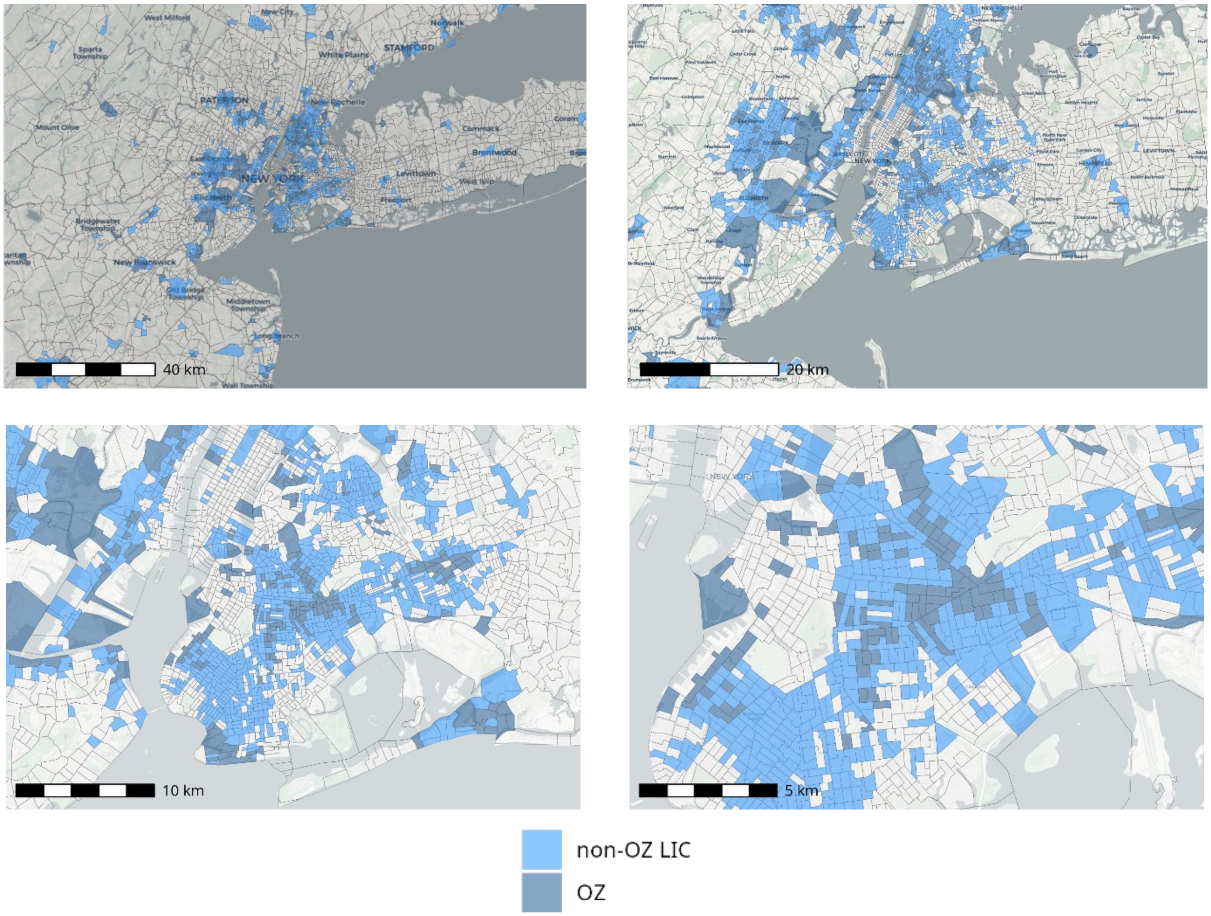


Figure A5. Maps of LICs and OZs in the San Francisco Bay Area at Various Scales

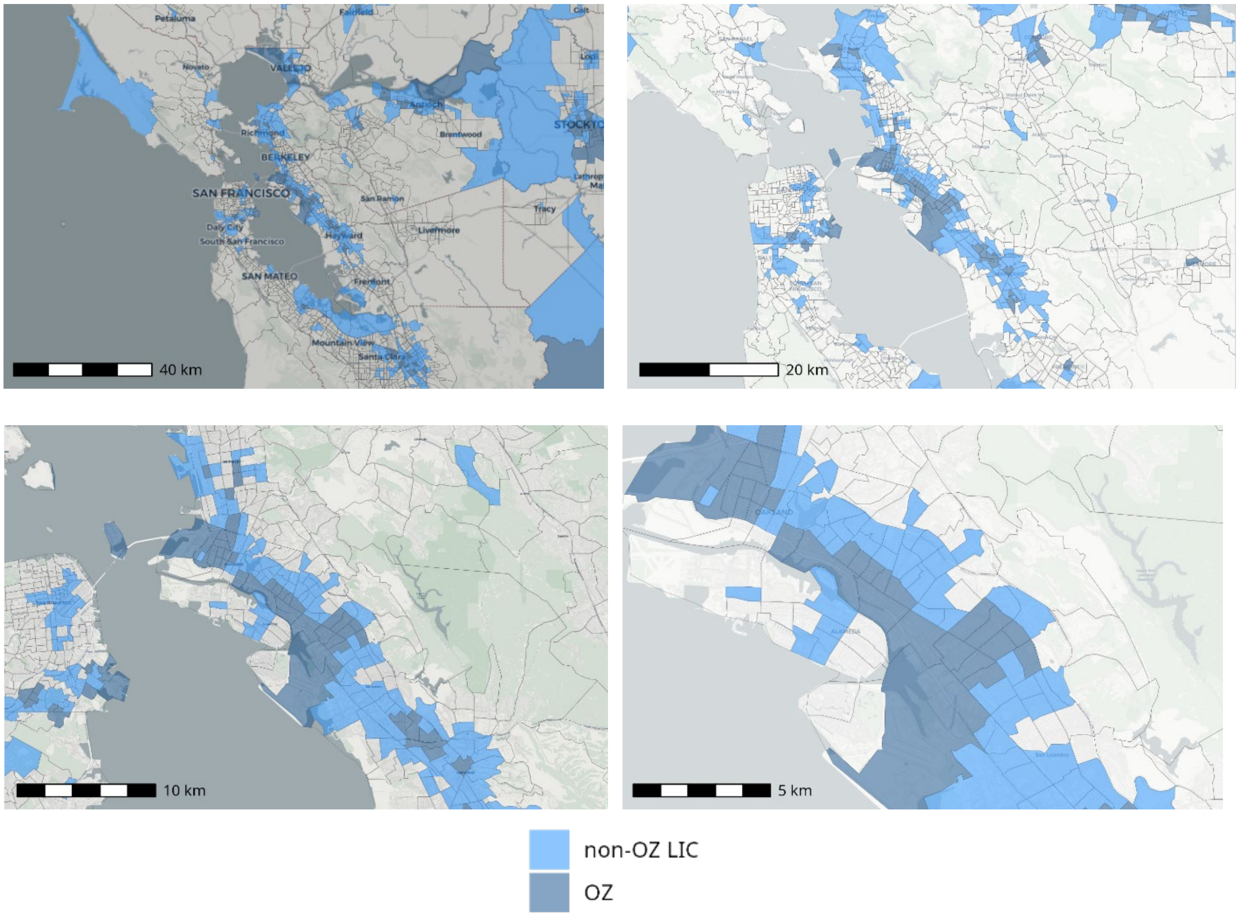
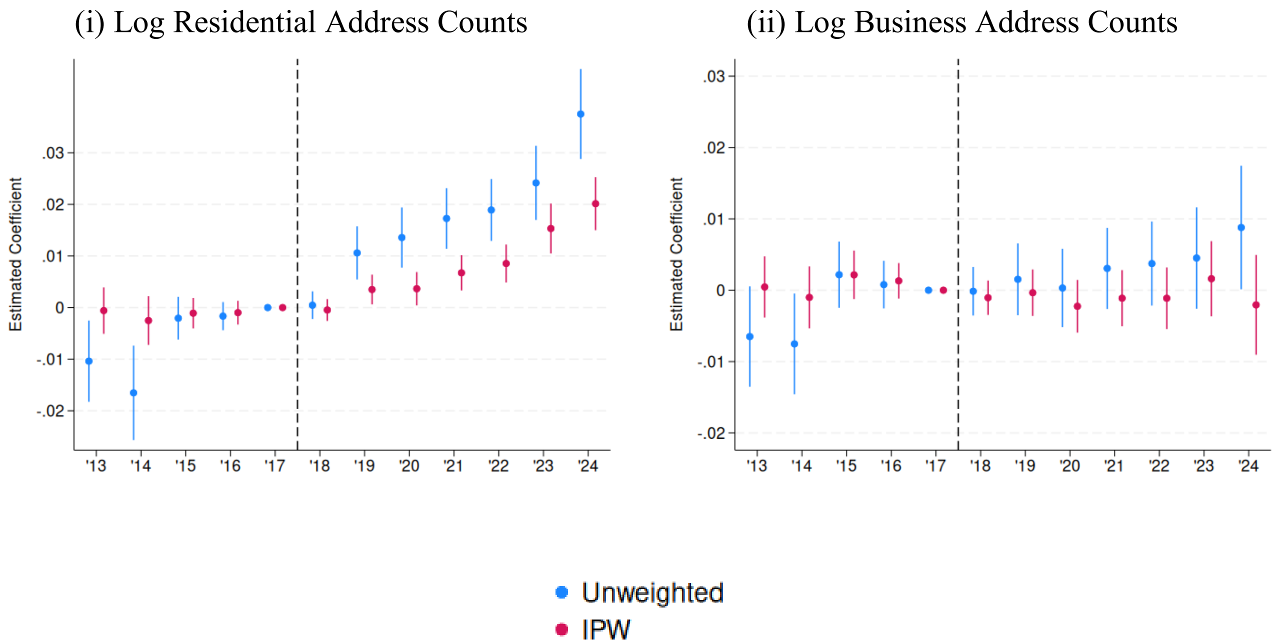


Figure A6. Event Studies for Address Counts



Notes: The panels show point estimates from event studies using all eligible but not designated LICs as controls. The blue dots in each figure correspond to the unweighted estimates. The red dots in each figure correspond to the IPW estimates. The green dots in each figure correspond to the "doubly robust" regression-adjusted IPW estimates. The data for these figures are derived from HUD-aggregated USPS administrative data on address vacancies. Residential and business address counts are totals of occupied and vacant addresses in USPS administrative data. It excludes non-deliverable addresses, addresses at structures under construction, and addresses marked by carriers as unlikely to be occupiable.

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	0.319	0.0919	1.85	10.8	20,269
Resident median earnings	0.322	0.0967	1.42	8.393	20,269
Log resident poverty rate	0.317	0.1598	5.16	60.91	20,269
Panel B: Variables Measured in LODES					
Log residential jobs	0.320	0.0436	1.31	10.77	20,269
Log workplace jobs	0.339	0.151	1.79	9.89	20,269
Log workplace jobs held by residents of					
…the same tract	0.333	0.100	0.865	3.68	20,269
…other OZs	0.351	0.428	5.65	72.32	20,269
…non-OZ LICs	0.333	0.081	0.87	5.64	20,269
…non-LICs	0.337	0.134	1.37	6.91	20,269

Notes: Summary statistics for IPW weights for control tracts. See text for details on the construction of the weights.

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	-0.00199*	-62.62	0.000311	-0.00540
	(0.000936)	(66.12)	(0.00122)	(0.00276)
Opportunity Zone × 2019-2023	0.00842***	-120.4	-0.0118***	0.0240***
	(0.00104)	(112.0)	(0.00132)	(0.00366)
Tract FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
2013-17 25th %ile	0.487	21,632	0.164	1,170
2013-17 Mean	0.550	25,747	0.247	1,833
2013-17 75th %ile	0.619	29,782	0.308	2,335
Tracts	27,077	27,077	27,077	27,077
Observations	81,231	81,231	81,231	81,231

Notes: "Opportunity Zone" is an indicator for OZ designation. The reference period (omitted OZ interacti
is 2013-2017. Heteroskedasticity robust standard errors (in parentheses) are clustered on census tract.
* p<0.10 ** p<0.05 *** p<0.01

Table A3. IPW and Regression-Adjusted IPW Estimates of OZ Effects on Jobs and Residents, Robustness Tests

	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Data:	LODES	LODES	ACS	ACS	ACS	ACS
	Log Resident Jobs	Log Workplace Jobs	Emp. Rate	Median Earnings	Poverty Rate	Log Resident Emp.
			A. IPW with State-by-Year Fixed Effects			
Opportunity Zone	0.00937***	0.0128**	0.00303**	-50.59	0.00376**	0.0188***
	(0.00167)	(0.00404)	(0.00105)	(109.7)	(0.00151)	(0.00370)
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
2013-17 p25	1,110	419	0.487	21,632	0.164	1,170
20013-17 Mean	1,676	2,023	0.550	25,747	0.247	1,833
2013-2017 p75	2,105	2,123	0.619	29,782	0.308	2,335
Tracts	27,077	27,077	27,077	27,077	27,077	27,077
Obs.	243,693	243,693	54,154	54,154	54,154	54,154
			B. IPW with Winsorized Weights			
Opportunity Zone	0.0117***	0.0171***	0.00464***	18.94	-0.00366**	0.0262***
	(0.00175)	(0.00409)	(0.00106)	(110.6)	(0.00136)	(0.00369)
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
2013-17 25th %ile	1,104	439	0.493	21,891	0.169	1,208
2013-17 Mean	1,617	1,661	0.551	25,589	0.242	1,791
2013-17 25th %ile	20,15	1,951	0.614	29,299	0.300	2,276
Tracts	25,049	25,047	25,049	25,047	25,047	25,044
Obs.	225,441	225,423	50,098	50,094	50,094	50,088

Notes: "Opportunity Zone" refers to the OZ × POST term in our estimating equation. 2018 is omitted. IPW is based on pre-treatment outcomes specific to each model. Regression-Adj. IPW refers to the doubly robust difference-in-difference method developed in Sant'Anna and Zhao (2020) and generalized in Callaway and Sant'Anna (2021). Estimates in Panel A include state-by-year fixed effects in the outcome regression. Estimates in Panel B use winsorized weights. Heteroskedasticity robust standard errors (in parentheses) are clustered on census tract.
p<0.10 ** p<0.05 *** p<0.01

Table A4. Estimates of OZ Effects on Employment and Population

	(1)	(2)
	Log Employment	Log Adult Civilian Population
	B. IPW Treatment on the Treated Estimates	
Opportunity Zone	0.0196^{***}	0.0018
	(0.0037)	(0.0030)
	C. Regression-Adj. IPW Treatment on the Treated Estimates	
Opportunity Zone	0.0168^{***}	0.0007
	(0.0038)	(0.0030)
Tract FE	Y	Y
Year FE	Y	Y
2013-17 25^{th} %ile	1,170	2,837
2013-17 Mean	1,833	4,214
2013-17 25^{th} %ile	2,335	5,273
Tracts	27,077	27,077
Observations	54,154	54,154

Note: "Opportunity Zone" refers to the OZ × POST term in our estimating equation. 2018 is omitted. IPW is based on pre-treatment outcomes specific to each model. Regression-Adj. IPW refers to the doubly robust difference-in-difference method developed in Sant'Anna and Zhao (2020) and generalized in Callaway and Sant'Anna (2021). Heteroskedasticity robust standard errors (in parentheses) are clustered on census tract.

* p<0.10 ** p<0.05 *** p<0.01

Table A5. IPW and Regression-Adjusted IPW Estimates of OZ Effects on Address Counts

	(i) Log Residential Address Count	(ii) Log Business Address Count
A. IPW Treatment on the Treated Estimates		
Opportunity Zone	0.0104***	0.0048**
	(0.0020)	(0.0020)
B. Regression-Adj. IPW Treatment on the Treated Estimates		
Opportunity Zone	0.0097***	0.0051***
	(0.0017)	(0.0017)
Tract FE	Y	Y
Year FE	Y	Y
2013-17 25^{th} %ile	1,057	42
2013-17 Mean	1,594	145
2013-17 25^{th} %ile	2,034	180
Tracts	30,693	30,620
Observations	332,674	331,764

Notes: "Opportunity Zone" refers to the OZ × POST term in our estimating equation. 2018 is omitted. IPW is based on pre-treatment outcomes specific to each model. Regression-Adj. IPW refers to the doubly robust difference-in-difference method developed in Sant'Anna and Zhao (2020) and generalized in Callaway and Sant'Anna (2021). Data derived from HUD aggregated USPS administrative data on address vacancies. Residential and business address counts are totals of occupied and vacant addresses in USPS administrative data. These exclude non-deliverable addresses, addresses at structures under construction, and addresses marked by carriers as unlikely to be occupiable. Heteroskedasticity robust standard errors (in parentheses) are clustered on census tract.

* p<0.10 ** p<0.05 *** p<0.01

Table A6: Correlates of Urbanity

	(i) OLS	(ii) OLS
	Indicator for Urban Area (In or adjacent to place of pop 50,000+)	
Pre-OZ Poverty (Z-score)	0.0586***	0.0660***
	(0.00586)	(0.00577)
Share of Surrounding Tracts that are OZs	0.349***	0.301***
	(0.0267)	(0.0260)
Share of Surrounding Tracts that are LICs	0.415***	0.385***
	(0.0236)	(0.0232)
State FE		Y
Sample	OZs	OZs
2013-17 Mean	0.552	0.552
Obs.	6,781	6,781

Notes: All columns display cross-sectional OLS regressions. Robust standard errors in parentheses. * $p<0.10$ ** $p<0.05$ *** $p<0.01$