The Impact of Place-Based Employment Tax Credits on Local Labor: Evidence from Tax Data

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Abstract

This paper examines the impact of the Empowerment Zone and Renewal Community (EZRC) employment tax credits on local labor market outcomes using administrative data with information on which firms claim these credits and where individuals work and live. We find strong evidence that the place-based employment tax credit improves outcomes of both EZRC and non-EZRC residents employed at firms utilizing this tax incentive. In our sample, treated firms represent a small share of EZRC resident labor demand, which limits the impact of this place-based policy on overall local labor.

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1 Introduction

This paper examines the impact of the federal Empowerment Zone and Renewal Community (EZRC) employment tax credit using administrative tax data. Employment tax credits are an important component of federal and state development programs, which are designed to promote economic development in disadvantaged areas. The federal program provides firms with tax credits if their business locates and employs residents within the designated zone. Businesses are eligible for an annual non-refundable tax credit of up to $1,500 for each employee who lives and works in a renewal community (RC) and $3,000 for each employee who lives and works in an empowerment zone (EZ). According to the U.S. Department of Housing and Urban Development [2008], the employment tax credit is the most frequently used tax incentive in the federal EZRC program.[1]

Evaluations of local development programs have found mixed results. Studies of state Enterprise Zone programs, which are similar to the federal program, usually find little or no effect of zone designation on local labor market outcomes (e.g., Bondonio and Engberg [2000], Greenbaum and Engberg [2000]; Greenbaum and Engberg [2004]; Bondonio and Greenbaum [2007], Elvery [2009], Neumark and Kolko [2010], and Lynch and Zax [2011]) with a few exceptions (e.g, Papke [1993], and Ham et al. [2011]). Similarly, the growing literature evaluating federal community development programs also finds mixed results. Oakley and Tsao [2006] find EZ designation to have no significant effect on local labor markets while Ham et al. [2011] and Busso et al. [2013] find EZ designation to significantly improve local labor markets. Ham et al. [2011] also find labor market improvement from Enterprise Community designation. Studies of similar programs in France find small positive impacts on local labor market outcomes (e.g., Givord et al. [2012] and Gobillon et al. [2012]).

This paper contributes to the literature evaluating place-based economic development programs in two ways. First, we provide an evaluation of a place-based federal employment tax credit using information about which firms claim the credit. Due to data limitations, the existing literature study the impact of zone "designation" as a proxy for participation in local development programs. Zone designation itself is associated with multiple subsidies and tax incentives so these studies are not able to discern to what extent firms utilize specific tax incentives or whether outcomes are directly attributable to a particular incentive. The

[1]The General Accounting Office also reported that the EZ employment tax credit was the most frequently used tax incentive in Round I EZs (General Accounting Office [2004] and General Accounting Office [2006])

inability to identify which firms were claiming credits, as opposed to just being located in a designated area, may be one reason why the existing literature has found mixed evidence on the impact of such programs. Our data, in contrast, allow us to estimate the direct impact of a specific provision of EZRC program, namely the employment tax credit, on those who actually participate in the program.

Second, we establish a link between claiming placed-based employment tax credits and labor outcomes of individuals based on whether they live and work in designated zones. One important feature of the employment tax credit is that it requires both firms and workers to be in the same designated zones to be eligible. This policy feature created a treated group from the labor demand side — firms that claim the credit, and a treated group from the labor supply side — workers that live in the designated zip codes. Our data allow us to explore both sides of treatment, an advantage over most existing studies that do not use data that allow them to identify both the locations in which an employee lives and works. Studies using aggregate data on zip codes (e.g., Bondonio and Engberg [2000]), census tracts (e.g., Ham et al. [2011]), and census block groups (e.g., Freedman [2013]) look at employment outcomes of zone residents, but cannot determine where zone residents work and, consequently, cannot study the impact on labor demand within designated zones. Studies using establishment level data (e.g., Greenbaum and Engberg [2004], Bondonio and Greenbaum [2007], Neumark and Kolko [2010], Lynch and Zax [2011], and Hanson and Rohlin [2013]) examine the impact of policies on labor demand by measuring outcomes of businesses located in designated zones. However, these studies cannot link changes in labor demand in designated zones to outcomes of designated zone residents.[2]

In this paper, we use administrative corporate and individual tax return data for tax years 2000 through 2004. The corporate tax return data allow us to observe whether firms reported qualified wages for the employment tax credit, which we infer to be "claiming" the credit.[3] We focus our analysis on outcomes of the Round II and Round III EZs and

[2]One exception is Busso et al. [2013], who use confidential data containing information on where residents work and live from the Journey to Work component from the Decennial Census to study the impacts of designation of Round I urban EZs on local labor market outcomes. The Journey to Work data contain information on where an individual lives and commutes for work, but does not explicitly match workers with specific firms. Our paper goes one step further by utilizing data that provides a link between workers and firms as well as whether firms claim the place-based tax incentive. Peters and Fisher [2002] and Freedman [2014] use an innovative method to look at how place-based policies affect both zone and non-zone residents in the absence of having data linking employers and employees. By comparing commuting times, both studies find evidence that jobs in locations with place-based tax incentives do not go to residents of these zones, but instead to residents outside of these targeted areas.

[3]Firms that have qualified EZRC wages are eligible to claim the EZRC employment tax credit, but may

RCs which became effective on January 1, 2002.[4] The estimation is restricted to non-consolidated firms and S-Corporations due to our limited ability to identify firm location among consolidated firms, the third type of firm that claims a sizable share of these credits.[5] While we acknowledge the restriction to local employment among non-consolidated firms and S-corporations limits the scope of our results, we believe this paper provides valuable information on how local labor markets are affected by place-based tax incentives, particularly among smaller firms. The individual income tax return data allow us to determine whether a worker lives in an EZRC. For the set of firms in our sample, we link the corporate tax return data to individual tax data to infer each firm's location.[6]

Our goal is to identify the treatment effect of the EZRC employment tax credits on local labor market outcomes of zone residents employed by treated firms. We recognize two potential identification issues related to selection into treatment: the EZRC zones were chosen non-randomly and eligible firms may select into treatment non-randomly. To address the nonrandom selection of zones, we construct a set of control zip codes using the propensity score matching (PSM) method in accordance with Busso et al. [2013], Papke [1994], Greenbaum and Engberg [2000], Greenbaum and Engberg [2004], Bondonio and Greenbaum [2007], and Elvery [2009]. In our baseline estimates, we include the set of non-treated firms that have employment in EZRC zip codes and the matched control zip codes. As a robustness check, we also use PSM to identify a set of non-treated firms that have similar pre-treatment characteristics as the treated firms to account for potential firm selection into treatment.[7] We also compare outcomes in designated zip codes to those of neighboring zip codes as in Ham et al. [2011] and test for geographic spillovers in this context.

Our results demonstrate the importance of differentiating the local labor impact of a place-based tax incentive by whether firms utilize the tax incentive and where individuals

be limited by the amount of tax liability owed and decide to carry the credit back one year or forward up to 20 years.

[4]This restriction excludes firms that locate in the Round I EZs, which may take the credit for the whole sample period and hence have no pre-treatment period.

[5]During our sample period, non-consolidated firms and S-Corporations claim 54% of these employment tax credits while consolidated corporations claim 35%. The distribution of employment tax credit among the different types of firms is based on the tentative employment tax credit, which is the amount of credit that a firm can claim prior to restricting the amount by tax liability.

[6]The address reported on corporate tax returns does not necessarily correspond to a firm's place of business.

[7]We do not use this set of results as our baseline because PSM at the firm level excludes new firms that enter into these local labor markets in the post-treatment period, which is a potentially important aspect of the treatment effect.

reside and work. Similar to the previous literature, we find modest evidence that zone designation improves local labor market outcomes. While zone designation is estimated to increase total wage by 7.5% for RC residents and 12% for EZ residents, it does not significantly impact the number of EZRC zone residents who are employed. When we focus on the effect from the EZRC employment tax credit and separate out the effects by firms that claim the credit, we find that the policy has a significant and positive impact on total wage and employment of EZRC zone residents who work for treated firms. Our baseline analysis shows that the tax incentive increased total wage by 18.7% and 21.4%, total employment by 13.9% and 21.3%, and number of employers by 16.3% and 15.3% among workers who reside and work for treated firms in RCs and EZs, respectively. We find similar improvements in outcomes among employees of these treated firms who reside in non-designated zone areas, suggesting that the place-based tax incentive generates positive spillovers to other employees within treated firms. We do not find evidence of negative spillovers to zone residents employed by non-treated firms. Our results remain robust when we use alternative control groups, including limiting the sample of non-treated firms to those with comparable pre-treatment characteristics as treated firms and comparing EZRC outcomes to those of their nearest neighbors.

The results provide insights on the future design and implementation of place-based tax policies. We find that the EZRC employment tax credit had a significant and positive average treatment effect on the treated: the credit created positive labor market results for residents of designated zones employed by firms that utilize this tax incentive. However, the impact on labor market outcomes of all zone residents may be much smaller if treated firms represent a small share of the labor demand for zone residents as in our sample.[8] As a result, examining the effect from overall zone designation on the population of zone residents, which is the norm in the existing literature, may not yield significant results. Furthermore, there is some documented evidence that take-up of the employment tax credit may be incomplete because firms were not aware of the credit.[9] Our findings suggest that

[8] In our baseline specification, over 80% of designated zone employment is attributed to firms that do not use this tax incentive. Busso et al. [2013] also find that roughly 80% of round I urban EZ residents work outside of the zones they live, and hence are not subjected to any policy treatments that are tied to zone businesses.

[9] According to a survey of businesses in round I EZs (General Accounting Office [1999]), "did not know about the credit" was the second most frequently cited reason, next to "not eligible," for urban businesses that were not claiming the credit and the most frequently cited reason for rural businesses in 1997. Similarly, Hebert et al. [2001] find that 49% of establishments in Round I EZs were unaware of the EZ employment credit in 2000, 6 years after the designation of these zones.

creating more public awareness about the place-based tax incentive could help the program cover a larger population and potentially have a broader positive impact on local labor market outcomes.

The paper is organized as follows: In Section 2, we provide background information on place-based local development programs. In Section 3, we describe our data. In Section 4, we present our empirical approach and baseline results. In Section 5, we conduct robustness checks. Section 6 concludes.

2 Background

U.S. state governments have been implementing place-based local development programs, known as Enterprise Zone programs, since 1982. By 2010, more than 40 states provided tax benefits to economically distressed areas to encourage economic development. While tax incentives provided by these programs vary from state to state, most offer employment tax credits.[10]

The federal place-based economic development program was created under the Omnibus Budget Reconciliation Act of 1993. Effective beginning in 1994, this Act created 11 (6 urban and 5 rural) round I EZs and 70 Enterprise Communities. Each urban EZ received $100 million and each rural EZ received $40 million in Title XX Social Services Block Grant (SSBG) funds over a 10-year period. Firms in EZs were eligible to claim an employment tax credit of up to $3,000 per year for each employee living in a zone, where the credit equaled 20% of the first $15,000 of an employee's salary.[11] The EZ employment tax credits are non-refundable and may be carried back one year and carried forward up to 20 years.

The Taxpayer Relief Act of 1997 authorized the designation of 20 new (15 urban and 5 rural) round II EZs in 1998. Over 10 years, the 15 urban Round II EZs received a total of $330 million and the 5 rural Round II EZs received a total of $10 million in funding. Businesses located in the Round II EZs were not eligible for the employment tax credit until January 1, 2002. The Community Renewal Tax Relief Act of 2000 authorized the designation of an additional 40 RCs as well as 10 Round III EZs that were effective on January 1, 2002. Businesses in RCs were eligible for an annual non-refundable tax credit

[10] Only one state program (Oregon) out of the thirteen state Enterprise Zone programs studied by Ham et al. [2011] did not offer any employment tax incentives.

[11] Two supplemental EZs, Cleveland EZ and Los Angeles EZ, did not become eligible for the EZ tax credit until January 2000.

of up to $1,500 for each employee who lived and worked in the RC, which could have been carried back one year or carried forward up to 20 years. The RC employment tax credit was calculated as 15% of the first $10,000 of an employee's salary. Other tax incentives were provided to businesses in RCs and EZs including an increased Section 179 deduction, gross income exclusions for capital gains, credits for qualified zone academy bonds, and the work opportunity tax credit.[12] RC designation expired at the end of 2009 while EZ designation expired at the end of 2013.[13]

The goal of the federal EZRC program was to stimulate creation of new jobs for residents of economically distressed areas and to promote revitalization of these areas. The nomination and selection process for EZRC designation suggests that selection into treatment based on observable characteristics, and for EZs, selection based on unobservable characteristics could potentially be issues. Areas were eligible for EZRC designation only if they met certain constraints based on population, area size, poverty, unemployment, and general distress, such as high incidences of crime and homelessness.[14] The selection of nominated areas for EZ designation was based on the effectiveness of their strategic plans, the implementation assurance of the plan, and other criteria set by federal agencies.[15,16] Areas nominated for EZs were more likely to be chosen for designation if their strategic plan demonstrated that state and local governments and other stakeholders were committed to implementing the submitted plans. Consequently, nominated EZs believed to be the more likely to succeed based on their strategic plans were also more likely to receive EZ status. This selection into EZ treatment among unobservable local characteristics may lead to overstatement of the treatment effect if designated EZs would have performed better

[12] See IRS Publication 954 (Internal Revenue Service [2004]) for details.

[13] Both EZ and RC designations were originally set to expire at the end of 2009. EZ designation was extended to December 31, 2011 by the Tax Relief, Unemployment Insurance Reauthorization and Job Creation Act of 2010, and extended further to December 31, 2013 by the American Taxpayer Relief Act of 2012.

[14] Minimum required poverty level in nominated communities was 20% in all tracts (All EZs and RCs), 25% in 90% of tracts (Round II and III EZs), and 35% in 50% of tracts (Round I EZs). Minimum required unemployment rate was 6.3%, the 1990 national average unemployment rate, for urban EZs and 9.45% for RCs. No minimum unemployment rate was specified for rural EZs. The maximum population was 200,000 or the greater of 50,000 and 10% of the population of the most populous city within the nominated area for urban EZs, 30,000 for rural EZs, and 200,000 for RCs. RCs were required to have a minimum population requirement of 4,000 if any part of the proposed area was within a metro area and 1,000 otherwise. Urban and rural EZs were limited to a maximum of 20 and 1,000 square miles with up to three noncontiguous parcels. RCs are required to have continuous boundary but do not have a maximum area requirement.

[15] U.S. Department of Housing and Urban Development administered the urban EZ program while the U.S. Department of Agriculture administered the rural EZ program.

[16] For more details, see Office of the Federal Register [2013a], Office of the Federal Register [2013b], and Office of the Federal Register [2007].

than similarly economically distressed areas in the absence of the federal EZ program. The selection of nominated areas for RC designation was based on the rankings of poverty, unemployment, and in urban areas, income. Thus, selection on unobservables is less of an issue for RCs.

3 Data

We use administrative tax return data for tax years 2000 through 2004. The corporate tax return data are a representative sample of corporate tax returns created by Internal Revenue Service's Statistics of Income (SOI). Beginning in 1997, the SOI corporate tax data include all firms that are eligible to claim the EZRC employment tax credit. Table 1 contains the amount of tentative EZRC credit by type of firm, where the tentative amount is the total credit including carry forwards but not necessarily the amount applied against tax liability. During our time period of interest, 2002-2004, there were three types of firms that were eligible to claim the vast majority of tentative credit. Thirty percent of tentative EZRC employment credits were claimed by S-Corporations, 35% are claimed by consolidated corporations, and 24% are claimed by non-consolidated firms. S-corporations are a type of pass-through business entity in which tax liability is passed on to shareholders and taxed at individual rates. Consolidated corporations are generally large corporations that consist of a parent company and multiple subsidiaries. Non-consolidated corporations consist of one main business entity and are typically smaller than consolidated corporations.

We restrict our study to employment among non-consolidated firms and S-corporations due to limitations in our ability to identify firm and worker locations among consolidated firms.[17] Although we cannot estimate the total impact of the EZRC employment tax credit on local labor market opportunities for zone residents, the results will further our understanding of the relationship between firms that claim the place-based tax credit and local employment, particularly among smaller businesses.[18]

We define "treated firms" as those that report having qualified EZRC wages on their tax returns in at least one year between 2002-2004 and do not report having any qualified

[17]Consolidated firms generally consist of multiple subsidiaries that are associated with different EINs. A complete crosswalk of EINs linking consolidated firms with their subsidiaries and the EINs used when issuing W-2s to employees is not currently available. As a results, we are unable to completely identify W-2 workers employed by consolidated corporations.

[18]Our analysis presents results as if the labor supply and demand of non-consolidated firms and S-corporations do not overlap with those of consolidated firms. Data limitations do not allow us to determine to what extent this assumption is valid.

EZRC wages in the pre-treatment period 2000 through 2001. Firms that have zero qualified wages are called "non-treated firms." [19,20] By using this definition, we bypass any complication derived from firms carrying their credit forward or backwards, and instead, identify firms that hired workers who worked and lived in designated zones.[21] In sum, the sample restrictions reduce the number of unique non-treated firms from 163,407 to 120,367 and the number of unique treated firms from 3,278 to 1,991.

To determine local employment of firms in our sample, we use administrative individual tax return and W-2 data from the IRS's Compliance Data Warehouse, which houses the population of tax returns and information returns. Each firm's W-2s were extracted, and each employee's individual tax return was extracted when available. When an individual tax return was filed for a worker, we used the zip code on the individual tax return.[22] For workers without individual tax returns, which make up approximately 12% of W-2s linked to firms, we use the worker's zip code on the W-2.[23] When a worker's zip code is located in a designated zone and the worker is employed by a treated firm, we assume that the treated firm is located in this zone.[24]

The EZRC zones were designated using 1990 or 2000 Census Tracts. We use the correspondence between 1990 and 2000 Census Tracts to define designated zones by 2000 Census Tracts. To link worker residence to designated zones, we use the correspondences between 2000 Census Tracts and zip code tabulation areas (ZCTA). Any zip code that is part of an designated zone is defined as an EZRC zip code. EZRC zones defined using zip codes will generally be larger than the actual zone areas (Ham et al. [2011] and Elvery [2009]). Generally, this measurement error tends to understate treatment effects as some untreated areas will be classified as treated areas. This downward bias will be bigger if

[19]The sample of firms is restricted to those that file as non-consolidated or as a S-corporation in each year a corporate tax return is filed between 2000 and 2004. Filing as non-consolidated and as a S-Corporation includes being identified as non-consolidated or as an S-corporation in the SOI data as well as not attaching a Form 851 with their tax return and not identifying as being part of an affiliated group.

[20]Treated firms are not necessarily sampled in 2000 and 2001. As a result, we extracted pre-treatment tax returns for treated firms from the administrative tax data housed in IRS's Compliance Data Warehouse.

[21]We also drop non-treated firms that report having qualified EZRC wages years after 2004 to make sure that we do not include firms that end up being treated in later years. These late adopters make up less than 1% of the non-treated sample.

[22]In general, the employee's address on the individual tax return should be more accurate than the address on the W-2 because we cannot ascertain which W-2s are amended to correct for address information in the W-2 data.

[23]In cases where a worker has multiple W-2s that report different zip codes, we set the zip code equal to the modular zip code.

[24]Our approach excludes the District of Columbia empowerment zone as workers may live anywhere in the District of Columbia.

there are negative geographic spillovers and will exist even if there are positive geographic spillovers to non-treated areas that are incorrectly included in the treated areas, assuming the positive spillovers are smaller in magnitude than treated effect. Using the 2000 Census Tract and ZCTA correspondence gives us a sample of 775 zip codes that belong to one designated zone, allowing us to identify all Round II and Round III EZs and 39 out of the 40 RCs.[25] Appendix A1 provides the list of EZRC zones that are included in our study.

4 Empirical Strategy

We use difference-in-differences (DD) to compare employment changes among treated firms in designated zip codes to non-treated firms in control zip codes. The policy treatment is the EZRC employment tax credit, which became effective in January of 2002.

4.1 Control Zip Codes

As discussed in the background section, the designation of the EZRCs was not random, and there was both selection into treatment based on observable and unobservable characteristics. Although we cannot account for non-random selection of designated zones based on unobservable characteristics, we do account for non-random selection based on observable characteristics. The existing literature has used different methods to account for selection on observable characteristics. One commonly used approach is the propensity score matching (PSM) method (Busso et al. [2013], Papke [1994], Greenbaum and Engberg [2000], Greenbaum and Engberg [2004], Bondonio and Greenbaum [2007], and Elvery [2009]). Other approaches include using the nearest neighbors of designated zones (Ham et al. [2011], and Neumark and Kolko [2010]) and using non-treated areas that are close to the eligibility threshold as the control group (Freedman [2012]).[26]In this paper, we use the PSM method in our baseline estimates and the nearest neighbor method as a robustness check to test for geographic spillovers.

To implement the PSM, we pooled the EZRC zip codes and non-EZRC zip codes that exclude the four nearest neighbors of any EZRC zip code and used a probit regression to predict the EZRC status for all zip codes. We exclude the four nearest neighbors to mitigate

[25]The 2000 Census Tract and ZCTA correspondence yielded 798 EZRC zip codes, of which 23 belong in more than one EZRC.

[26]Freedman [2012] uses a discontinuity in the eligibility rule of census tracts for the New Market Tax Credit to study the impact of the program in low-income communities.

potential geographic spillovers. In the probit regression, we use year 2000 ZCTA characteristics as explanatory variables including population, population density, rural indicator, unemployment rates, poverty rates, median household income, racial compositions, share of native, share of different education attainment categories, and share of owner-occupied housing.[27] We then do a one-to-one non-repeated closest PSM. We dropped the five designated zip codes that have propensity scores outside of the common support of the set of non-designated zip codes. 770 out of the 775 unique EZRC zip codes are matched to one zip code. Of the 770 zip codes with a matched zip code, 769 have a positive number of W-2 workers in at least one year between 2000 and 2004. Figure A1 displays the propensity score distributions of the EZRC zip codes, PSM control zip codes, and the overall sample of non-EZRC zip codes. The distributions of propensity scores of the EZRC and PSM control zip codes look very similar while the distribution of the overall sample of non-EZRC zip codes looks different.

The main identification assumption of the DD approach is that local employment in the treated group and the control group would not have been systematically different in the absence of the treatment. The summary statistics in Table 3 show that the pre-treatment characteristics of the matched zip codes are, on average, very similar to those of the EZRC zip codes in 2000. In contrast, designated zones have very different economic and demographic characteristics from the overall population of non-designated areas, providing evidence of selection based on these observable variables. These differences highlight that using the PSM procedure to identify a set of control group zip codes with similar pre-treatment characteristics as those of designated zones is necessary.

4.2 Firms

After restricting our sample of treated firms to those linked to W-2 workers who live in EZRCs and the sample of non-treated firms to those linked to W-2 workers who live in EZRC or control zip codes, the number of unique non-treated firms reduces from 120,367 to 63,055 and the number of unique treated firms reduces from 1,991 to 1,923. Treated firms may also employ workers in control zip codes. A possible concern is that treated firms may participate in other tax incentives. Among non-consolidated firms and S-corporations

[27] Ideally, the propensity score match would be based on pre-treatment trends instead of static 2000 characteristics. However, documentation from the Census (http://www.census.gov/geo/reference/zctafaq.html) state that areas defined by zip codes have changed between each Decennial Census. Therefore, pre-trends at the zip code level may not be reliable.

with tentative EZRC employment tax credit, only 3% had qualified Work Opportunity Tax Credit wages, 1% had qualified Welfare-to-Work credit wages, and almost none claimed other credits available to businesses locating in these zones (See Table A2 for details.). Based on the data, we do not expect the effects from other tax incentives to be captured in our estimates. However, firms may be affected by other incentives that are not observed in the tax data.[28]

Our baseline estimates will include the entire set of non-treated and treated firms in our sample. By including all firms, we allow for firm entry and exit. Summary statistics reported in Table 2 reveal that there are some differences in pre-treatment characteristics between treated and non-treated firms. As a robustness check, we use PSM to identify non-treated firms with similar pre-treatment characteristics as treated firms. This strategy alleviates concerns that there is selection into treatment based on observable characteristics, but also restricts the analysis to firms that exist in the pre-treatment period.

4.3 Outcome Variables

We study four employment outcomes for non-consolidated firms and S-corporations: 1) wages paid to employees living in the zip code, 2) the number of employees living in the zip code 3) average wage of employees living in the zip code, and 4) the number of firms that employed workers in the zip code. Table 4 displays summary statistics for designated EZRC zip codes and PSM control zip codes by pre-treatment and post-treatment time period. The top panel contains information for RCs and the bottom panel contains information for EZs.[29] In the post-treatment period, all of the outcome variables, except for average wage, declined in EZs, RCs, and control zip codes, with the exception of total wages in RCs which experienced a small average increase.

Table 5 reports summary statistics by EZRC and PSM control zip codes, by pre and post time period, and by treated firms. The descriptive statistics suggest that the post-treatment change differs by whether or not workers are employed by treated versus non-treated firms. With the exception of average wage, outcomes of workers employed by non-treated firms

[28] Firms in EZs and RCs are also eligible for increased deductions and more preferential treatment of certain capital gains. These tax incentives are not reported in the administrative tax data. In particular, firms located in EZs and RCs are also eligible to claim an increased Section 179 Deduction, deduction for depreciation of property used on Indian Reservations. In addition, EZs may rollover capital gains from sale of EZ assets and exclude a larger amount of capital gain from qualified small business stock. Round II EZ firms may also benefit from block grants.

[29] We analyze EZs and RCs separately because the size of the EZ employment credits is twice as big as the size of the RC employment credits, and EZs received some grants while RCs did not.

declined between the pre and post treatment periods, regardless of whether they lived in an EZRC. In contrast, some outcomes of workers employed by treated firms who live in EZRC and control zip codes improved over time. For example, on average, total wage and average wage increased between the pre and post-treatment years among workers of treated firms who live in both RCs and control zip codes. On average, total wages and total workers increased among workers of treated firms who live in EZs and total wages and average wage increased among workers of treated firms who live in control zip codes.

4.4 Regression Equations

We estimate two different sets of equations. The first set of estimates compares changes in outcomes by whether workers reside in EZRC or control zip codes. The second set compares changes in outcomes by worker residence and whether or not they are employed by a treated firm. The first set of regressions is analogous to the estimation strategies used in the existing literature which measure the impact of zone designation on outcomes. This strategy does not allow us to measure the impact from participating in specific incentives associated with being in a designated zone. In contrast, the second set of regressions uses the unique feature of our data to provide new insight on a place-based tax incentive by linking outcomes directly to firms claiming the incentive.

For the first set of regressions, we collapse our firm, zip code, and year level data to the zip code and year level to estimate the following:

$$\text{Log}(Y_{it}) = \alpha + \beta_1 \text{Post}_t + \beta_2 \text{EZRC Zip Code}_i + \beta_3 \text{Post}_t * \text{EZRC Zip Code}_i + \varepsilon_{it} \quad (4.1)$$

where Y_{it} is the outcome variable for residents of zip code i in year t. Post_t is a dummy that takes value 1 in 2002-2004 , 0 in 2000-2001. EZRC Zip Code_i is a dummy that equals 1 in EZRC zip codes, 0 in control zip codes. In accordance with the existing literature, the error term is clustered by county. The DD regression captures the treatment effect of being in a designated zone on outcomes of EZRC residents who work for S-corporations and non-consolidated firms. The coefficient of the interaction term β_3 is the difference between changes in outcomes in the EZRC zip codes and changes in outcomes in the control zip codes.

For the second set of regressions, we collapse the data by zip code, whether a firm is

treated or non-treated, and year to estimate the following:

$$\begin{aligned} \text{Log}(Y_{ijt}) &= \alpha + \gamma_1 \text{Post}_t + \gamma_2 \text{EZRC Zip Code}_i + \gamma_3 \text{Treated}_j && (4.2) \\ &+ \gamma_4 \text{Post}_t * \text{EZRC Zip Code}_i + \gamma_5 \text{Post}_t * \text{Treated}_j && (4.3) \\ &+ \gamma_6 \text{Treated}_j * \text{EZRC Zip Code}_i \\ &+ \gamma_7 \text{Post}_t * \text{EZRC Zip Code}_i * \text{Treated}_j + \varepsilon_{ijt} \end{aligned}$$

where Y_{ijt} is the outcome variable for residents of zip code i employed by firm group j in year t. j is an index for treated and non-treated firms. Again, the error term is clustered by county.

We use the second set of regressions to estimate changes in outcomes for four worker groups: (1) workers living in EZRCs and employed by treated firms (WG1), (2) workers living in control zip codes and employed by treated firms (WG2), (3) workers living in EZRCs and employed by non-treated firms (WG3), and (4) workers living in control zip codes and employed by non-treated firms (WG4). Table 6 summarizes the four worker groups by workers' residence and by whether they are employed by treated firms.

Our main goal is to identify the treatment effect of the EZRC employment tax credit on residents of EZRCs who are employed by treated firms (WG1), by comparing the difference between changes in this group to those of control zip code residents employed by non-treated firms (WG4). We assume that in the absence of the employment tax credit, treated firm employment of EZRC residents would have been similar to that of non-treated firm employment of control zip code residents.[30]

The estimated impact of the policy treatment on the other worker groups are also of interest. If workers living in EZRC and control zip codes are compliments or substitutes, then the credit could have positive or negative impacts on outcomes of non-zone residents employed by treated firms (WG2) when treated firms re-optimize their labor inputs. EZRC residents employed by non-treated firms (WG3) may be affected by the EZRC employment tax credit if there are spillovers. For instance, if non-treated firms that employed EZRC zone residents are negatively affected by the treated firms due to competition in the labor and product markets, then there will be negative spillovers on WG3. In contrast, if non-treated firms that employed EZRC zone residents are positively affected by treated firms due to agglomeration effects, then there will be positive spillovers on WG3.

[30]Ideally, we would conduct a placebo test using data in years prior to the establishment of the EZRCs as a robustness check. However, administrative data are not currently available for earlier years.

The compositions of the treated and control groups differ between the two sets of regressions. In the first set of regressions, the treated group is workers living in designated zones, i.e., WG1 and WG3, and the control group is workers living in control zones, i.e., WG2 and WG4. In the second set of regressions, the treated group is WG1 and the control group is WG4. The treatment effect on WG1 identified using WG4 as the control group shows how treated firms respond to the employment tax credit by altering employment of workers living in the EZRCs.

4.5 Baseline Results

In this section, we first estimate the impact of zone designation on local labor in EZRC zip codes. While firms in EZs may also be eligible for block grants, the impact from RC designation mainly stems from various tax incentives, including the RC employment tax credit. Table 7 contains regression results comparing outcomes of workers living in EZRC zip codes with workers living in control zip codes. The first four columns contain results for RCs. The estimated coefficients of the interaction term, the DD estimates, provide some evidence (statistically significant at the 10% level) that RC designation improves local labor market outcomes. In particular, RC designation is estimated to increase total wages paid to RC residents by 7.5%, average wage by 6.1%, and number of firms hiring RC residents by 2.6%. The coefficient estimates on the Post variable are generally negative and statistically significant except when the outcome is average wage, of which the coefficient estimate is positive and significant. The last four columns of Table 7 contain results for EZs. The DD estimates are positive for all outcome variables, but not always statistically significant. EZ designation is estimated to increase total wages paid to EZ residents by 12% (statistically significant at the 5% level) and average wage by 10.1% (statistically significant at the 10% level). Similar to the RC results, the coefficient estimates on the Post variable are statistically significant and negative except when the outcome is average wage, which is not estimated to change significantly in the post treatment period.

The positive impact of EZ designation on average wage is consistent with results found in Ham et al. [2011] and similar in magnitude with results found in Busso et al. [2013] for Round I EZs. Furthermore, the positive impact of RC designation on number of firms is consistent with the increase in the number of establishments found by Hanson and Rohlin [2011b] for Round I EZs. In accordance with Ham et al. [2011] and Busso et al. [2013], zone designation is predicted to increase total workers in both RCs and EZs; however, the

estimates are imprecisely estimated and not statistically significant.

When we specifically focus on the impact from the place-based employment tax credit and separate the treatment effect by whether residents work for treated firms, we find significant treatment effects among employees of treated firms. Table 8 reports the estimated percentage change in outcomes variables using control zip code residents employed at non-treated firms (WG4) as the control group in the DD regression specified in Equation 4.2.[31] The estimates suggest that the RC employment tax credit increased total wages by 18.7%, total workers by 13.9%, and number of employers by 16.3% among workers that reside and work for firms in RCs. Similarly, the EZ employment tax credit increased total wages by 21.4%, total workers by 21.3%, and number of employers by 15.3% among workers that reside and work for firms in EZs.

Table 8 also reports estimated percentage changes in outcome variables among residents of the control zip codes who are employed by treated firms (WG2). The estimates show that treated firms are also found to increase total wage and employment of workers living in control zip codes even though these workers do not increase the firm's employment tax credit. Among treated firm employees living in control zip codes, estimates show that the RC employment tax credit increased total wages by 24.4%, total workers by 17.3%, and number of treated firms employing control zip code residents by 12.8%, and the EZ employment tax credit increased, total workers by 19.0% and number of treated firms employing control zip code residents by 15.6%. The results suggest that there are positive labor market spillovers to control zip code residents, which could be due to labor complementarity between EZRC zip code residents and control zip code residents within treated firms. Our findings support results in Busso et al. [2013], who also find evidence that EZRC jobs held by control zip code residents increase in Round I EZs.[32]

Table 8 also contains the estimated percentage changes in outcomes among residents of EZRC zip codes employed by non-treated firms (WG3). The point estimates are small in magnitude and not statistically different from zero, implying that outcomes for this worker group are not differentially impacted by the employment tax credits relative to WG4. As a result, we do not find evidence that the employment tax incentive improved outcomes of EZRC residents employed by treated firms at the expense of worsening outcomes of EZRC

[31] The estimated percentage change is calculated as the effect on being in the Post treatment period 2002-2004 relative to WG4.

[32] Their estimated impact using a baseline DD regression without controls is 16.1% (significant at the 10 percent level). The estimated impact is positive, but statistically insignificant in the specifications that include controls and parametric re-weighting.

residents employed by non-treated firms.

By linking firms that claim the EZRC employment tax credit with worker residence, we find that local labor outcomes significantly improved, but only among these treated firms. In our sample, these firms represent a relatively small portion of the labor demand for EZRC residents and the majority of EZRC residents are employed by non-treated firms. Using the summary statistics in Table 4 and Table 5, we calculate that, in the pre-treatment period, employees of non-treated firms represent 80.6% of RC residents and 84.7% of EZ residents. Consequently, our results suggest that estimating the impact of zone designation on the population of zone residents may not be sufficient to evaluate the effectiveness of place-based tax incentives, particularly if firms that benefit from the tax incentive only employ a limited portion of local labor.

We examined characteristics of treated firms who began hiring EZRC residents in the post-treatment period to gain a better understanding of the types of firms that enter into the population.[33] The industry distribution of new firms shows that the highest shares were in retail (19%), health care and social assistance (11%), manufacturing (10%), construction (8%), wholesale (8%), and administrative and support, waste management, and remediation services (8%). These results are generally consistent with the prediction that employment tax credits should have a greater impact on industries that are more labor than capital intensive as detailed in Hanson and Rohlin [2011a]. On average, these new firms employ 169 W-2 workers for a total of $2.3 million in wages. Looking at the distribution of W-2 workers, almost 70% of new firms have fewer than 50 W-2 employees, demonstrating the most treated firms that enter into the tax filing population in the post-treatment period were smaller businesses.

5 Robustness Checks

In this section, we check the robustness of our baseline results.

[33]Details may be found in Appendix Table A3. A treated firm may show up in both the existing and new hire category if they employ workers in both the pre and post-treatment time periods in one EZRC zip code and begin employing workers in the post-treatment time period in a different EZRC zip code. A total of 2,507 or 49% of treated firms show up both as an existing and new hire.

5.1 Firm Selection into Treatment

In our baseline estimation, we used PSM method to account for potential selection of EZRC zones based on observable characteristics. Selection bias may also arise if there is non-random selection of firms into treatment. Table 2 contains summary statistics for treated and non-treated firms by pre and post treatment years and demonstrates that there are indeed differences between firms by treatment status. The industry distributions are different, and, on average, treated firms are smaller than non-treated firms in terms of total assets, generated gross profits, and total income.[34] While total W-2 wages are on average greater among treated than non-treated firms, average number of W-2 workers are higher among non-treated firms. As a consequence, average wage is greater among non-treated firms than treated firms. When we categorize firms into whether they have less than 50 W-2 employees, 50-100 W-2 employees, or more than 100 W-2 employees, we find that treated firms tend to be smaller than non-treated firms. This suggests that there are several large treated firms that are causing the average number of W-2 workers to be larger among treated than non-treated firms. Among non-treated firms in the pre-treatment period, 31% have less than 50 W-2 employees, 19% have 50-100 W-2 employees, and 50% have more than 100 W-2 employees. Among treated firms, 59% have less than 50 W-2 employees, 15% have 50-100 employees, and 27% have more than 100 W-2 employees. A higher share of treated firms report a profit than non-treated firms (97% versus 92%). The share of non-consolidated firms is higher among treated firms at 81% than among non-treated firms (42%).

To account for selection into treatment based on observable characteristics, we use the one-to-one PSM with repetition to identify a set of non-treated firms with comparable pre-treatment characteristics as those of treated firms. For the PSM, we pooled the set of treated and non-treated firms that file corporate tax returns in both 2000 and 2001.[35] The main drawback to implementing a PSM method to identify a sample of non-treated firms is that this restricts our analysis to firms that file tax returns in the pre-treatment period. As a result, the treatment effect estimated using the matched set of non-treated firms will

[34] Total assets, generated gross profits, and total income are reported on the Form 1120. Generated gross profits equals gross receipts less cost of good sold.

[35] The probit regression predicting the probability of being a treated firm includes the following explanatory variables: firm industry dummy variables; levels, squared, and cubic terms of assets, profits, W-2 wages, number of W-2 workers, and average W-2 wage in 2000 and 2001; indicators in 2000 and 2001 for whether the firm has less than 50 W-2 workers and 50-100 W-2 workers; and the percentage change and squared percentage change in assets, profits, W-2 wages, number of W-2 workers, and average W-2 wage between 2000 and 2001. The PSM was carried out separately for the set of S-corporations and non-consolidated firms.

not include impacts on local labor caused by differential changes in the birth or death of firms by treatment status. Summary statistics of treated and non-treated firms identified by the PSM method for S-corporations and non-consolidated firms demonstrate that the pre-treatment characteristics of the treated and control firms are generally similar for both type of firms (Table A4 and Table A5).[36]

Table 9 reports the estimated percentage change in outcomes variables due to the EZRC employment tax credit for the different worker groups using the set of PSM identified non-treated firms. Similar to the baseline estimates, the results suggest that local labor outcomes improved among treated firm employees who live in EZRC and control zip codes. In general, the estimated impact on wages is greater while the estimated impact on the number of employers is smaller when we compare the results using the PSM identified set of non-treated firms to those using the entire sample of non-treated firms. In particular, the results show that the RC employment tax credit increased total wages by 44.8%, total workers by 11.7%, average wage by 33.1%, and number of employers by 8.9% among workers that reside and work for firms in RCs. Similarly, we estimate that the EZ employment tax credit increased total wages by 34.8%, total workers by 13.9%, and number of employers by 7.9% among workers that reside and work for firms in EZs. Among workers that reside in control zip codes and work for treated firms (WG2), the RC employment tax credit increased total wages by 47%, total workers by 18.5%, average wage by 28.5%, and number of employers by 9.3% while the EZ employment tax credit increased total wages by 34.9%, total workers by 17.4%, and number of employers by 12.5%. In addition, average wages are estimated to increase significantly when using the PSM non-treated firms while the positive effects are statistically insignificant in the baseline estimates. Similar to the baseline results, the employment tax credit is not estimated to significantly affect outcomes among EZRC residents employed by PSM identified non-treated firms (WG3).

Overall, when we account for firm selection into treatment based on observable characteristics, we find that the results are similar to those estimated in the baseline. Using PSM to identify the set of non-treated firms causes us to exclude firms that enter into the sample in the post-treatment period. This exclusion of new firms may explain some of the

[36]Since we use one-to-one PSM with repetition, the number of control firms is smaller than the number of treated firms. As a consequence, the size of WG3, designated zone residents employed by control firms, and WG4, control zone residents employed by control firms, is significantly reduced. From Table A7, we can calculate the share of WG2 in total workers of the control zip codes that work for the firms in the restricted sample is now 65.5% for RC control zones and 61.8% for EZ control zones, while the share of WG3 in total workers of the designated zones is now 13.5% for RCs and 18.9% for EZs.

differences in magnitude and statistical significance between the baseline and and set of estimates presented in this section.

5.2 Geographic Spillovers to WG4

The literature has used both PSM and nearest neighbors to identify areas comparable to designated zones. For our baseline analysis, we presented results from the PSM method because it generated a set of zip codes that had more similar pre-treatment characteristics to those of the EZRCs. In this section, we redo the analysis using nearest neighboring zip codes as the control to test the robustness of our baseline results as well as investigate whether there is evidence of geographic spillovers from the place-based policy.

When treated and non-treated firms are located near each other, non-treated firms may be positively affected through agglomeration or negatively affected through competition in the product and input markets. Such spillovers, if they exist, may introduce biases in the estimated treatment effect. In particular, if there are positive (negative) spillovers to the control group, then the treated effect will be under (over) estimated. Following Ham et al. [2011], we first estimate our baseline specifications using the first four nearest neighbors as a comparison group to EZRCs and then re-estimate the regressions excluding the first nearest neighbor. If the exclusion of the first neighbor alters the estimates, then this provides evidence of geographic spillovers to non-treated firms located near treated firms.

We use Census 2000 gazetteer files for ZCTAs to identify the nearest neighbors of EZRC zip codes by using the distance between centroids of EZRC zip codes and non-EZRC zip codes. Table 10 contain Census 2000 summary statistics for the first nearest neighbor, nearest 2nd through 4th neighbors, and first four nearest neighbors. In general, the characteristics are roughly the same across the 3 different neighbor groups. However, we find that EZRC zip codes are generally worse off in terms of average unemployment rate, poverty rates, median household income, and educational attainment compared to their neighbors.

Table 11 reports the estimated change in outcomes using residents of the first four neighbors of designated zip codes employed by non-treated firms as the control group. Similar to the baseline estimates and estimates using PSM to select non-treated firms, the nearest neighbor results show that the place-based employment tax credit improved local labor market outcomes among treated firm employees living in EZRCs. The estimates suggest that the RC employment tax credit increased total wages by 22.3%, total workers

by 11%, average wage by 11.3% (significant at 10% level), and number of employers by 15.6% among workers that reside and work for firms in RCs. Similarly, we estimate the EZ employment tax credit increased total wages by 13.2%, total workers by 16.9%, and number of employers by 12.6% among workers that reside and work for firms in EZs. Furthermore, the credit also improves local labor market outcomes among treated firm employees living in neighboring zip codes, providing further evidence that the employment tax credits generate positive spillovers within treated firms. Additionally, estimates show that the employment tax credits decreased the number of RC residents employed at non-treated firms by 3.9% (significant at 10% level) and increased the average wage of this worker group by 11.3%. Similarly in EZs, total number of residents employed at non-treated firms decreased by 6%. These results imply that designated zone residents could potentially be switching jobs and that part of the increase in employment among treated firms may be explained by these job switchers.[37] In our baseline results, the estimated effect of the place-based tax credits on designated zone residents employed by non-treated firms are also negative, but smaller in magnitude and not statistically significant.

Table 12 reports the estimated percentage changes in outcomes variables when the control area excludes the first nearest neighbor. These results are not statistically different from results including the first nearest neighbor. These results are consistent with the generally positive, but insignificant geographic spillover effects of local development programs to nearby areas found in Ham et al. [2011].

5.3 Other Checks

As another robustness check, we tried dropping one RC and one EZ at a time along with its matched control zip code to make sure that the baseline results are not driven by a particular zone. While dropping one zone affects the statistical significance when estimating the impact of zone designation on outcomes, the magnitude of the estimates remain similar. Unreported results for the set of regressions estimating the impact of the place-based employment tax credit on the various worker groups do not provide evidence that the baseline results are being driven by a particular zone.[38]

[37] The increase in employment among treated firms may also come from workers moving from consolidated corporations or other types of businesses to treated firms as well as from reducing the number of individuals who were previously unemployed.

[38] One exception is the DD estimate of the impact of the employment tax credit on average wage among non-treated firm employees living in EZs, which is sometimes statistically significant at the 5% and 10% when a zone is dropped.

6 Conclusion

Our paper is the first to link the use of a place-based tax incentive to local labor market outcomes. Since the credit required both firms and workers to be in an EZRC, it created a treated group from the labor demand side – firms that claim the credit, and a treated group from the supply side – workers who live in EZRCs. Our matched data allowed us to explore both sides of the policy. We find that the EZRC employment tax credit significantly and positively affects total wage and employment of EZRC zone and non-zone residents who work for firms that claim this credit. Our paper also provides an evaluation of the impact of designations of the Round II and Round III EZs and RCs, which have not been studied before.[39] When we look at total wage and employment of zone residents who work in and outside of designated zones, we find some evidence of positive impacts from zone designation.

There are two limitations to our study. The first limitation is that we restrict the analysis to S-corporations and non-consolidated firms. A large share of the tentative EZRC employment credit (about 35%) was claimed by consolidated corporations, which were likely larger employers than the firms in our sample. The second limitation is that our analysis is conducted at the zip code level. Using zip code level data likely causes us to include observations that are not part of an designated zone, which are defined at the census tract level. Our estimated treatment effects may be biased if the zip codes definition of EZRCs are bigger than the census tract definitions. Assuming that there are negative geographic spillovers or that any positive geographic spillover will be smaller than the positive effect on the treated group, we expect the imprecision from using the zip code definition of EZRCs to cause our treatment effects to be downward biased.

Although we acknowledge these limitations prevent us from being able to generalize our results more broadly, we believe that our study makes an important contribution to the existing literature by being the first to examine the relationship between utilization of the EZRC employment tax credit and labor market outcomes. We find strong evidence that observing which firms participate in this incentive is necessary to evaluate the effectiveness of this policy. Our results show that even though the treated employers do respond significantly to the policy, the group of treated employers may not generate a significant improvement of labor market conditions for the overall population in designated zones if

[39] Freedman [2012] studies the $15 billion New Market Tax Credit program, another component of the Community Renewal Tax Relief Act of 2000, and finds modest positive impact on neighborhood conditions in low-income communities.

they employ a small share of residents. Combined with the evidence from round I EZs (General Accounting Office [1999] and Hebert et al. [2001]) that a significant share of eligible employers were not aware of the EZ employment policy, our results suggest that better advertising of the policy is important to improve the average treatment effect on the intention-to-treat population.

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7 Tables

Table 1: Weighted Empowerment Zone/Renewal Community Tentative Credit by Tax Year

Tax Year	S-Corporations		Consolidated		Non-consolidated		Total Credit
	% of Total Credit	# of Firms	% of Total Credit	# of Firms	% of Total Credit	# of Firms	in Millions
2002	0.35	3192	0.32	561	0.24	1854	285.03
2003	0.30	3077	0.37	743	0.22	1929	351.66
2004	0.25	3674	0.37	791	0.25	2688	382.88
Overall	0.30		0.35		0.24		

Statistics are constructed using weighted IRS-SOI corporate data. Credit amount is the tentative credit amount and is reported in 2012 dollars.

Table 2: Summary Statistics by Treated and Post

	Non-Treated, Pre		Non-Treated, Post		Treated, Pre		Treated, Post	
	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev
Agriculture	0.02	(0.13)	0.02	(0.13)	0.04	(0.21)	0.04	(0.21)
Mining	0.01	(0.08)	0.01	(0.09)	0.00	(0.07)	0.00	(0.07)
Utilities	0.00	(0.03)	0.00	(0.03)	0.00	(0.03)	0.00	(0.03)
Construction	0.13	(0.34)	0.13	(0.34)	0.09	(0.28)	0.09	(0.28)
Manufacturing	0.20	(0.40)	0.19	(0.40)	0.16	(0.37)	0.16	(0.36)
Wholesale	0.14	(0.35)	0.14	(0.35)	0.14	(0.35)	0.14	(0.35)
Retail	0.16	(0.36)	0.16	(0.36)	0.22	(0.41)	0.21	(0.41)
Wholesale and Retail not allocable	0.00	(0.01)	0.00	(0.01)	0.00	(0.02)	0.00	(0.02)
Transportation and Warehousing	0.02	(0.15)	0.02	(0.15)	0.03	(0.16)	0.03	(0.16)
Information	0.04	(0.19)	0.03	(0.18)	0.01	(0.11)	0.01	(0.11)
Finance and Insurance	0.04	(0.20)	0.04	(0.20)	0.04	(0.20)	0.04	(0.20)
Real Estate, Rental, and Leasing	0.04	(0.20)	0.04	(0.20)	0.02	(0.13)	0.02	(0.14)
Professional, Scientific, and Tech Services	0.08	(0.27)	0.08	(0.28)	0.05	(0.22)	0.05	(0.22)
Management (Holding Companies)	0.00	(0.04)	0.00	(0.03)	0.00	(0.00)	0.00	(0.00)
Admin and Support, Waste Management, Remediation Services	0.02	(0.15)	0.02	(0.15)	0.03	(0.18)	0.04	(0.18)
Educational Services	0.00	(0.06)	0.00	(0.06)	0.00	(0.06)	0.00	(0.07)
Health Care and Social Assistance	0.03	(0.17)	0.03	(0.17)	0.06	(0.24)	0.06	(0.24)
Arts, Entertainment, and Recreation	0.01	(0.11)	0.01	(0.11)	0.00	(0.05)	0.00	(0.06)
Accommodation and Food Services	0.03	(0.18)	0.03	(0.18)	0.05	(0.22)	0.05	(0.22)
Other Service	0.02	(0.12)	0.02	(0.12)	0.04	(0.21)	0.04	(0.21)
Total Assets (in 1000s)	63105.69	(7.36e+06)	6.70e+06	(2.34e+09)	11445.91	(51770.33)	11571.58	(50007.77)
Generated Gross Profits (in 1000s)	9717.34	(30975.03)	10476.92	(37495.49)	7649.45	(38304.23)	7924.82	(38734.68)
Gross Receipts (in 1000s)	10344.58	(52221.46)	11121.74	(59001.13)	7013.57	(33014.23)	7600.16	(45696.05)
Total W-2 Wages (in 1000s)	6532.96	(17526.07)	6680.11	(19211.53)	4977.52	(21236.98)	5207.95	(22807.74)
Total W-2 Workers	243.16	(820.45)	230.90	(795.63)	390.63	(2846.74)	375.86	(2791.42)
Average Wage	42971.05	(75802.97)	45555.11	(104311.91)	27018.14	(28624.14)	27553.77	(26902.01)
Less than 50 W-2 Wage Employees	0.31	(0.46)	0.32	(0.47)	0.59	(0.49)	0.59	(0.49)
50-100 W-2 Wage Employees	0.19	(0.40)	0.20	(0.40)	0.15	(0.35)	0.15	(0.36)
Greater than 100 W-2 Wage Employees	0.50	(0.50)	0.48	(0.50)	0.27	(0.44)	0.26	(0.44)
Profit	0.92	(0.27)	0.93	(0.26)	0.97	(0.18)	0.97	(0.17)
Non-Consolidated Firms	0.42	(0.49)	0.42	(0.49)	0.81	(0.39)	0.81	(0.39)
Observations	91511		133195		3348		5359	

Data are at the firm and year level. Monetary variables are in 2012 dollars.

Table 3: Characteristics of Empowerment Zones and Renewal Communities (EZRCs), Control Zip Codes and Propensity Score Matched (PSM) Control Zip Codes, 2000

	EZRC		Control Zip Codes		PSM Control Zip Codes	
	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev
Unemployment Rate	5.82	(3.50)	3.42	(2.98)	5.89	(5.11)
Family Poverty Rate	22.44	(12.24)	9.58	(8.72)	22.04	(14.77)
Individual Poverty Rate	26.82	(11.98)	12.53	(9.46)	26.50	(14.23)
Median Household Income	27261.36	(9891.80)	40036.67	(16192.18)	27354.95	(9838.85)
High School Dropouts	32.37	(13.21)	20.94	(12.05)	32.32	(14.66)
High School Graduates	29.91	(8.96)	34.71	(11.12)	29.57	(9.15)
Some College Education	17.79	(5.22)	20.18	(6.89)	17.69	(6.10)
Associate Degree	4.46	(2.38)	6.02	(3.32)	4.40	(2.47)
Bachelor Degree	9.58	(6.96)	11.83	(8.27)	9.97	(7.54)
Graduate Degree	5.88	(6.13)	6.32	(6.54)	6.05	(7.44)
Native Born	91.68	(13.69)	95.29	(8.15)	91.24	(12.46)
Male	49.06	(4.39)	49.85	(3.21)	49.07	(4.97)
White	52.30	(28.78)	85.82	(19.61)	52.00	(30.48)
Black	35.91	(30.85)	6.62	(14.67)	36.02	(31.81)
Asian	2.15	(5.66)	1.37	(4.04)	2.18	(5.77)
Hispanic	12.68	(23.12)	6.17	(13.03)	13.29	(22.03)
Log Population	8.92	(1.55)	7.90	(1.74)	8.90	(1.71)
Log Population Density	6.17	(2.77)	4.56	(2.29)	6.14	(2.57)
Owner Occupied Housing	57.56	(23.45)	75.66	(14.53)	56.91	(23.14)
Observations	769		30836		769	

Data constructed using U.S. Census data. Statistics are at the zip code level.

Table 4: Summary Statistics by EZRC and Post

	EZRC Zip, Pre		EZRC Zip, Post		Control Zip, Pre		Control Zip, Post	
	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev
Renewal Communities								
Total Wages in 1000s	12103	(17308)	12122	(18257)	13068	(16684)	12565	(16179)
Total Workers	697	(878)	608	(792)	763	(858)	661	(756)
Average Wage	17516	(11451)	19738	(12951)	16993	(15391)	18698	(10323)
Number of Firms	176	(199)	163	(184)	199	(222)	184	(209)
Observations	1000		1500		1000		1500	
Empowerment Zones								
Total Wages in 1000s	14252	(16622)	13855	(16235)	13075	(16470)	12518	(15912)
Total Workers	816	(868)	734	(822)	710	(749)	624	(661)
Average Wage	19756	(22840)	21727	(24433)	18455	(15610)	20978	(21677)
Number of Firms	208	(187)	194	(180)	194	(210)	180	(195)
Observations	538		807		538		807	

Data are at the zip code and year level. Monetary variables are in 2012 dollars.

Table 5: Summary Statistics by EZRC, Treated, and Post

	Treated, EZRC Zip Code (WG1)		Treated, Control Zip Code (WG2)		Non-Treated, EZRC Zip Code (WG3)		Non-Treated, Control Zip Code (WG4)	
	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev
Renewal Communities								
Pre								
Total Wages in 1000s	1657	(2680)	446	(785)	10446	(16505)	12622	(16278)
Total Workers	135	(223)	54	(94)	562	(770)	709	(812)
Average Wage	13675	(8420)	10283	(27322)	18176	(12160)	17527	(15487)
Number of Firms	17	(15)	7	(8)	160	(194)	191	(216)
Observations	1000		1000		1000		1000	
Post								
Total Wages in 1000s	1754	(2837)	484	(846)	10368	(17403)	12081	(15728)
Total Workers	124	(205)	52	(87)	484	(694)	609	(712)
Average Wage	15063	(8530)	11243	(20813)	20571	(14084)	19453	(10477)
Number of Firms	18	(16)	8	(9)	145	(179)	176	(202)
Observations	1500		1500		1500		1500	
Empowerment Zones								
Pre								
Total Wages in 1000s	1864	(3008)	414	(697)	12388	(15362)	12661	(16099)
Total Workers	124	(153)	46	(79)	691	(790)	664	(701)
Average Wage	18127	(27082)	9966	(9443)	19730	(23106)	19044	(15801)
Number of Firms	15	(11)	7	(8)	193	(181)	187	(204)
Observations	538		538		538		538	
Post								
Total Wages in 1000s	1966	(2821)	457	(779)	11890	(14988)	12062	(15472)
Total Workers	135	(183)	47	(76)	599	(723)	577	(613)
Average Wage	17564	(31153)	10554	(8662)	22164	(25565)	21837	(22919)
Number of Firms	16	(12)	8	(8)	178	(173)	172	(189)
Observations	807		807		807		807	

Data are at the zip code and year level. Monetary variables are in 2012 dollars.

Table 6: Worker Groups by Workers' Residence and Employer

	EZRC Zone Residents	Control Zone Residents
Treated Firm	WG1	WG2
Non Treated Firms	WG3	WG4

Table 7: Differences-in-Differences Regression Estimates

	Renewal Communities				Empowerment Zones			
VARIABLES	Total Wage	Total Workers	Average Wage	# of Firms	Total Wage	Total Workers	Average Wage	# of Firms
Post	-0.091***	-0.159***	0.068***	-0.102***	-0.147***	-0.149***	0.001	-0.103***
	(0.029)	(0.013)	(0.024)	(0.008)	(0.053)	(0.017)	(0.046)	(0.014)
EZRC Zip Code	0.025	-0.028	0.053	-0.031	0.273	0.286	-0.013	0.265
	(0.162)	(0.139)	(0.041)	(0.125)	(0.244)	(0.199)	(0.080)	(0.173)
Post * EZRC Zip Code	0.075*	0.014	0.061*	0.026*	0.120**	0.019	0.101*	0.011
	(0.039)	(0.019)	(0.031)	(0.013)	(0.057)	(0.035)	(0.051)	(0.019)
Constant	15.260***	5.661***	9.598***	4.537***	15.310***	5.659***	9.651***	4.539***
	(0.093)	(0.083)	(0.030)	(0.065)	(0.131)	(0.121)	(0.046)	(0.092)
Observations	5,000	5,000	5,000	5,000	2,690	2,690	2,690	2,690
R-squared	0.001	0.002	0.011	0.001	0.007	0.009	0.002	0.011
Number of Clusters	105	105	105	105	48	48	48	48

Outcomes variables are in logs. Robust standard errors clustered at the zip code level are in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 8: Estimated Changes in Employment Among Treated Firms, Includes All Non-Treated Firms

	Total Wages	Total Workers	Average Wage	# of Firms
Renewal Communities				
EZRC Employment Among Treated Firms (WG1)				
% Change	0.187***	0.139***	0.047	0.163***
	(0.061)	(0.042)	(0.045)	(0.021)
	[0.065 , 0.308]	[0.056 , 0.223]	[-0.041 , 0.136]	[0.122 , 0.204]
Control Zip Code Employment Among Treated Firms (WG2)				
% Change	0.244***	0.173***	0.071	0.128***
	(0.087)	(0.025)	(0.08)	(0.016)
	[0.071 , 0.418]	[0.124 , 0.223]	[-0.088 , 0.229]	[0.097 , 0.16]
EZRC Employment Among Non-Treated Firms (WG3)				
% Change	0.027	-0.011	0.039	-0.006
	(0.039)	(0.024)	(0.033)	(0.015)
	[-0.05 , 0.104]	[-0.059 , 0.036]	[-0.027 , 0.104]	[-0.035 , 0.022]
Observations	10,000	10,000	10,000	10,000
Empowerment Zones				
EZRC Employment Among Treated Firms (WG1)				
% Change	0.214***	0.213***	0.001	0.153***
	(0.075)	(0.053)	(0.064)	(0.028)
	[0.063 , 0.364]	[0.106 , 0.32]	[-0.128 , 0.13]	[0.097 , 0.208]
Control Zip Code Employment Among Treated Firms (WG2)				
% Change	0.139	0.19***	-0.052	0.156***
	(0.11)	(0.035)	(0.101)	(0.021)
	[-0.083 , 0.361]	[0.12 , 0.261]	[-0.256 , 0.152]	[0.115 , 0.198]
EZRC Employment Among Non-Treated Firms (WG3)				
% Change	0.068	-0.019	0.087	-0.008
	(0.065)	(0.034)	(0.053)	(0.023)
	[-0.062 , 0.198]	[-0.088 , 0.05]	[-0.02 , 0.194]	[-0.054 , 0.038]
Observations	5,350	5,350	5,350	5,350

Notes: Difference-in-difference (DD) estimates are calculated using Worker Group 4 (WG4) as the control group, which consists of non-treated firm employees living in control zip codes. Each entry gives the total estimated percentage change in the outcome variable presented in each column from being in the Post treatment period 2002-2004 relative to WG4. Specifically, % Change is calculated by aggregating the coefficient estimates from the DD to obtain the total effect of the Post variable on outcomes for each Worker Group. Regressions are estimated separately for Renewal Communities and Empowerment Zones. Each row includes a different Worker Group. Robust standard errors are clustered at the county level. 95% confidence intervals are reported beneath standard errors.

Table 9: Estimated Changes in Employment Among Treated Firms, PSM to Identify Non-Treated Sample

	Total Wages	Total Workers	Average Wage	# of Firms
Renewal Communities				
EZRC Employment Among Treated Firms (WG1)				
% Change	0.448***	0.117**	0.331***	0.089***
	(0.111)	(0.046)	(0.098)	(0.022)
	[0.227 , 0.669]	[0.026 , 0.209]	[0.136 , 0.526]	[0.045 , 0.132]
Control Zip Code Employment Among Treated Firms (WG2)				
% Change	0.47***	0.185***	0.285**	0.093***
	(0.135)	(0.037)	(0.123)	(0.021)
	[0.202 , 0.738]	[0.111 , 0.258]	[0.042 , 0.529]	[0.051 , 0.135]
EZRC Employment Among Non-Treated Firms (WG3)				
% Change	-0.03	0.05	-0.08	-0.009
	(0.153)	(0.041)	(0.14)	(0.019)
	[-0.334 , 0.274]	[-0.03 , 0.131]	[-0.358 , 0.198]	[-0.048 , 0.029]
Observations	9,720	9,720	9,720	9,720
Empowerment Zones				
EZRC Employment Among Treated Firms (WG1)				
% Change	0.348**	0.139**	0.209	0.079**
	(0.162)	(0.064)	(0.146)	(0.033)
	[0.021 , 0.676]	[0.011 , 0.268]	[-0.085 , 0.503]	[0.013 , 0.144]
Control Zip Code Employment Among Treated Firms (WG2)				
% Change	0.349*	0.174***	0.175	0.125***
	(0.199)	(0.051)	(0.185)	(0.03)
	[-0.051 , 0.75]	[0.071 , 0.277]	[-0.198 , 0.548]	[0.065 , 0.184]
EZRC Employment Among Non-Treated Firms (WG3)				
% Change	0.18	0.012	0.168	0.004
	(0.209)	(0.058)	(0.188)	(0.037)
	[-0.24 , 0.6]	[-0.105 , 0.129]	[-0.21 , 0.546]	[-0.071 , 0.079]
Observations	5,140	5,140	5,140	5,140

Notes: Difference-in-difference (DD) estimates are calculated using Worker Group 4 (WG4) as the control group, which consists of non-treated firm employees living in control zip codes. Each entry gives the total estimated percentage change in the outcome variable presented in each column from being in the Post treatment period 2002-2004 relative to WG4. Specifically, % Change is calculated by aggregating the coefficient estimates from the DD to obtain the total effect of the Post variable on outcomes for each Worker Group. Regressions are estimated separately for Renewal Communities and Empowerment Zones. Each row includes a different Worker Group. Robust standard errors are clustered at the county level. 95% confidence intervals are reported beneath standard errors. Non-treated firms are identified by propensity score matching.

Table 10: Characteristics of Empowerment Zones and Renewal Communities (EZRCs) and Their Nearest Neighbors, 2000

	EZRC		1st Nearest Neighbor		2nd-4th Nearest Neighbors		1st-4th Nearest Neighbors	
	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev
Unemployment Rate	5.83	(3.51)	4.81	(3.68)	3.99	(2.91)	4.30	(3.24)
Family Poverty Rate	22.46	(12.25)	15.00	(12.83)	12.41	(10.48)	13.38	(11.47)
Individual Poverty Rate	26.84	(11.98)	18.43	(13.11)	15.71	(11.60)	16.73	(12.25)
Median Household Income	27256.80	(9886.18)	35292.59	(17517.40)	38518.83	(17132.88)	37312.43	(17337.51)
High School Dropouts	32.36	(13.20)	26.44	(15.08)	23.40	(14.53)	24.54	(14.80)
High School Graduates	29.90	(8.97)	30.22	(10.51)	30.07	(10.46)	30.12	(10.47)
Some College Education	17.81	(5.24)	19.06	(6.80)	20.04	(6.60)	19.67	(6.69)
Associate Degree	4.47	(2.39)	5.02	(2.81)	5.56	(3.04)	5.36	(2.96)
Bachelor Degree	9.58	(6.95)	11.74	(8.86)	13.18	(9.78)	12.64	(9.47)
Graduate Degree	5.87	(6.13)	7.52	(9.03)	7.75	(7.76)	7.67	(8.26)
Native Born	91.69	(13.69)	91.74	(13.06)	92.04	(12.39)	91.93	(12.64)
Male	49.07	(4.38)	48.84	(4.01)	49.03	(4.05)	48.96	(4.03)
White	52.31	(28.77)	69.03	(28.49)	72.19	(25.47)	71.01	(26.67)
Black	35.86	(30.86)	21.42	(27.62)	18.37	(24.54)	19.51	(25.76)
Asian	2.15	(5.65)	2.05	(4.06)	2.48	(5.19)	2.32	(4.80)
Hispanic	12.66	(23.11)	11.05	(22.49)	10.48	(20.49)	10.69	(21.25)
Log Population	8.92	(1.56)	8.44	(1.81)	8.81	(1.70)	8.67	(1.75)
Log Population Density	6.16	(2.78)	6.19	(2.64)	6.05	(2.55)	6.10	(2.58)
Owner Occupied Housing	57.59	(23.45)	65.70	(21.69)	67.82	(18.52)	67.02	(19.78)
Observations	770		307		514		821	

Data constructed using U.S. Census data. Statistics are at the zip code level.

Table 11: Estimated Changes in Employment Among Treated Firms, Includes All Non-Treated Firms, Nearest 4 Neighbors

	Total Wages	Total Workers	Average Wage	# of Firms
Renewal Communities				
EZRC Employment Among Treated Firms (WG1)				
% Change	0.223***	0.11**	0.112*	0.156***
	(0.078)	(0.043)	(0.066)	(0.022)
	[0.069 , 0.377]	[0.026 , 0.195]	[-0.017 , 0.242]	[0.113 , 0.199]
Control Zip Code Employment Among Treated Firms (WG2)				
% Change	0.35***	0.212***	0.138	0.159***
	(0.104)	(0.036)	(0.088)	(0.021)
	[0.145 , 0.556]	[0.14 , 0.284]	[-0.036 , 0.313]	[0.118 , 0.2]
EZRC Employment Among Non-Treated Firms (WG3)				
% Change	0.074	-0.039*	0.113**	-0.014
	(0.057)	(0.02)	(0.05)	(0.014)
	[-0.039 , 0.187]	[-0.079 , 0.001]	[0.014 , 0.211]	[-0.042 , 0.014]
Observations	9,920	9,920	9,920	9,920
Empowerment Zones				
EZRC Employment Among Treated Firms (WG1)				
% Change	0.132*	0.169***	-0.037	0.126***
	(0.074)	(0.041)	(0.067)	(0.026)
	[-0.015 , 0.28]	[0.087 , 0.251]	[-0.169 , 0.096]	[0.075 , 0.177]
Control Zip Code Employment Among Treated Firms (WG2)				
% Change	0.266***	0.181***	0.085	0.108***
	(0.092)	(0.041)	(0.085)	(0.022)
	[0.083 , 0.449]	[0.1 , 0.262]	[-0.083 , 0.254]	[0.064 , 0.151]
EZRC Employment Among Non-Treated Firms (WG3)				
% Change	0.01	-0.06**	0.069	-0.03
	(0.06)	(0.025)	(0.049)	(0.022)
	[-0.11 , 0.13]	[-0.109 , -0.01]	[-0.028 , 0.167]	[-0.073 , 0.013]
Observations	6,190	6,190	6,190	6,190

Notes: Difference-in-difference (DD) estimates are calculated using Worker Group 4 (WG4) as the control group, which consists of non-treated firm employees living in control zip codes. Each entry gives the total estimated percentage change in the outcome variable presented in each column from being in the Post treatment period 2002-2004 relative to WG4. Specifically, % Change is calculated by aggregating the coefficient estimates from the DD to obtain the total effect of the Post variable on outcomes for each Worker Group. Regressions are estimated separately for Renewal Communities and Empowerment Zones. Each row includes a different Worker Group. Robust standard errors are clustered at the county level. 95% confidence intervals are reported beneath standard errors. Non-treated firms are restricted to those with employment in EZRCs or in the EZRC's nearest four neighboring zip codes.

Table 12: Estimated Changes in Employment Among Treated Firms, Includes All Non-Treated Firms, Nearest 2-4 Neighbors

	Total Wages	Total Workers	Average Wage	# of Firms
Renewal Communities				
EZRC Employment Among Treated Firms (WG1)				
% Change	0.236***	0.102**	0.134*	0.146***
	(0.087)	(0.044)	(0.074)	(0.023)
	[0.064 , 0.408]	[0.015 , 0.19]	[-0.012 , 0.279]	[0.1 , 0.192]
Control Zip Code Employment Among Treated Firms (WG2)				
% Change	0.331***	0.214***	0.117	0.165***
	(0.119)	(0.04)	(0.104)	(0.023)
	[0.096 , 0.567]	[0.136 , 0.292]	[-0.088 , 0.323]	[0.12 , 0.21]
EZRC Employment Among Non-Treated Firms (WG3)				
% Change	0.087	-0.047**	0.134**	-0.024
	(0.074)	(0.023)	(0.065)	(0.016)
	[-0.058 , 0.232]	[-0.092 , -0.001]	[0.006 , 0.262]	[-0.055 , 0.007]
Observations	8,140	8,140	8,140	8,140
Empowerment Zones				
EZRC Employment Among Treated Firms (WG1)				
% Change	0.147**	0.173***	-0.026	0.13***
	(0.07)	(0.043)	(0.064)	(0.027)
	[0.008 , 0.286]	[0.087 , 0.26]	[-0.154 , 0.102]	[0.077 , 0.184]
Control Zip Code Employment Among Treated Firms (WG2)				
% Change	0.237**	0.187***	0.05	0.103***
	(0.112)	(0.042)	(0.107)	(0.024)
	[0.015 , 0.459]	[0.103 , 0.27]	[-0.162 , 0.262]	[0.054 , 0.151]
EZRC Employment Among Non-Treated Firms (WG3)				
% Change	0.025	-0.055**	0.08*	-0.026
	(0.053)	(0.027)	(0.045)	(0.018)
	[-0.081 , 0.13]	[-0.109 , -0.001]	[-0.011 , 0.17]	[-0.062 , 0.01]
Observations	4,820	4,820	4,820	4,820

Notes: Difference-in-difference (DD) estimates are calculated using Worker Group 4 (WG4) as the control group, which consists of non-treated firm employees living in control zip codes. Each entry gives the total estimated percentage change in the outcome variable presented in each column from being in the Post treatment period 2002-2004 relative to WG4. Specifically, % Change is calculated by aggregating the coefficient estimates from the DD to obtain the total effect of the Post variable on outcomes for each Worker Group. Regressions are estimated separately for Renewal Communities and Empowerment Zones. Each row includes a different Worker Group. Robust standard errors are clustered at the county level. 95% confidence intervals are reported beneath standard errors. Non-treated firms are restricted to those with employment in EZRCs or in the EZRC's nearest second through fourth neighboring zip codes.

A Appendix Figures and Tables

For Online Publication

Figure A1: Propensity Score Distribution of EZRC, PSM Control, and Non-EZRC Zip Codes



Table A1: A List of EZRC Zones in the Study

City (Name if Rural)	State	EZRC Status	Rural Status
Aroostook County Empowerment Zone	ME	EZ	rural
Boston	MA	EZ	
Buffalo-Lackawanna	NY	RC	
Burlington	VT	RC	rural
Camden	NJ	RC	
Central Louisiana Renewal Community	LA	RC	rural
Charleston	SC	RC	
Chattanooga	TN	RC	
Chicago	IL	RC	
Cincinnati	OH	EZ	
Columbia/Sumter	SC	EZ	
Columbus	OH	EZ	
Corpus Christi	TX	RC	
Cumberland County	NJ	EZ	
Desert Communities Empowerment Zone	CA	EZ	rural
Detroit	MI	RC	
Eastern Kentucky Renewal Community	KY	RC	rural
El Paso County	TX	RC	rural
El Paso	TX	EZ	
FUTURO Empowerment Zone	TX	EZ	rural
Flint	MI	RC	
Fresno	CA	EZ	
Gary/Hammond/East Chicago	IN	EZ	
Greene-Sumter	AL	RC	rural
Griggs-Steele Empowerment Zone	ND	EZ	rural
Hamilton	OH	RC	
Huntington Ironton	WV/OH	EZ	
Jacksonville	FL	EZ	
Jamestown	NY	RC	rural
Knoxville	TN	EZ	
Lawrence	MA	RC	
Los Angeles	CA	RC	
Lowell	MA	RC	
Memphis	TN	RC	
Miami/Dade County	FL	EZ	
Milwaukee	WI	RC	
Minneapolis	MN	EZ	
Mobile County	AL	RC	
New Haven	CT	EZ	
New Orleans	LA	RC	
Newark	NJ	RC	
Niagara Falls	NY	RC	
Norfolk/Portsmouth	VA	EZ	
Northern Louisiana Renewal Community	LA	RC	rural
Oglala Sioux Tribe Empowerment Zone	SD	EZ	rural
Oklahoma City	OK	EZ	
Orange Cove	CA	RC	rural
Ouachita Parish	LA	RC	
Parlier	CA	RC	rural
Philadelphia	PA	RC	
Pulaski	AR	EZ	
Rochester	NY	RC	
San Antonio	TX	EZ	
San Diego	CA	RC	
San Francisco	CA	RC	
Santa Ana	CA	EZ	
Schenectady	NY	RC	
Southern Alabama Renewal Community	AL	RC	rural
Southernmost Illinois Delta Empowerment Zone	IL	EZ	rural
Southwest Georgia United Empowerment Zone	GA	EZ	rural
St. Louis E. St. Louis	MO/IL	EZ	
Syracuse	NY	EZ	
Tacoma	WA	RC	
Tucson	AZ	EZ	
Turtle Mountain Band of Chippewa Renewal Community	ND	RC	rural
West-Central Mississippi Renewal Community	MS	RC	rural
Yakima	WA	RC	
Yonkers	NY	EZ	
Youngstown	OH	RC	

Table A2: Take-Up of Tax Incentives Among Firms with EZRC Credit

	Mean	Std Dev
Claim Empowerment Zone Employment Tax Credit	0.17	(0.38)
Claim Renewal Community Employment Tax Credit	0.23	(0.42)
Claim Work Opportunity Tax Credit	0.03	(0.16)
Claim Welfare-to-Work Credit	0.01	(0.11)
Claim New Markets Tax Credit	0.00	(0.00)
Claim Indian Employment Tax Credit	0.00	(0.04)
Claim Qualified Zone Academy Bond Credit	0.00	(0.00)
Claim Qualified Community Development Credit	0.00	(0.00)
Observations	8707	

Data are at the firm and year level.

Table A3: Summary Statistics of Treated Firms that Employ EZRC Residents in 2002-2004

	Existing		New Hire		New Firm	
	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev
Agriculture	0.05	(0.21)	0.04	(0.19)	0.03	(0.18)
Mining	0.01	(0.07)	0.00	(0.07)	0.00	(0.00)
Utilities	0.00	(0.03)	0.00	(0.00)	0.00	(0.00)
Construction	0.09	(0.28)	0.09	(0.29)	0.08	(0.27)
Manufacturing	0.16	(0.37)	0.17	(0.38)	0.10	(0.30)
Wholesale	0.14	(0.35)	0.15	(0.36)	0.08	(0.28)
Retail	0.22	(0.41)	0.20	(0.40)	0.19	(0.39)
Wholesale and Retail not allocable	0.00	(0.03)	0.00	(0.02)	0.00	(0.00)
Transportation and Warehousing	0.02	(0.15)	0.03	(0.16)	0.06	(0.24)
Information	0.01	(0.11)	0.01	(0.11)	0.02	(0.15)
Finance and Insurance	0.04	(0.19)	0.03	(0.16)	0.04	(0.19)
Real Estate, Rental, and Leasing	0.02	(0.13)	0.02	(0.14)	0.01	(0.12)
Professional, Scientific, and Tech Services	0.05	(0.22)	0.04	(0.20)	0.04	(0.21)
Management (Holding Companies)	0.00	(0.00)	0.00	(0.00)	0.00	(0.00)
Admin and Support, Waste Management, Remediation Services	0.03	(0.18)	0.05	(0.21)	0.08	(0.26)
Educational Services	0.00	(0.07)	0.01	(0.07)	0.00	(0.00)
Health Care and Social Assistance	0.06	(0.24)	0.06	(0.24)	0.11	(0.31)
Arts, Entertainment, and Recreation	0.00	(0.05)	0.00	(0.05)	0.01	(0.12)
Accommodation and Food Services	0.05	(0.21)	0.06	(0.23)	0.07	(0.26)
Other Service	0.05	(0.21)	0.03	(0.17)	0.06	(0.24)
Not Allocable	0.00	(0.05)	0.00	(0.05)	0.00	(0.00)
Total Assets (in 1000s)	12392.06	(52885.63)	18042.42	(66866.04)	4281.95	(12194.73)
Generated Gross Profits (in 1000s)	8347.71	(41028.52)	13448.32	(54575.89)	3759.95	(10280.61)
Gross Receipts (in 1000s)	7899.73	(48363.32)	10588.09	(60134.79)	4220.06	(13874.57)
Total W-2 Wages (in 1000s)	5519.99	(24176.46)	8898.93	(32057.30)	2281.13	(6717.41)
Total W-2 Workers	407.36	(2968.56)	701.46	(3970.24)	169.06	(663.16)
Less than 50 W-2 Wage Employees	0.58	(0.49)	0.45	(0.50)	0.69	(0.46)
50-100 W-2 Wage Employees	0.15	(0.36)	0.17	(0.38)	0.09	(0.29)
Greater than 100 W-2 Wage Employees	0.26	(0.44)	0.37	(0.48)	0.22	(0.41)
Profit	0.97	(0.17)	0.98	(0.15)	0.97	(0.18)
Average Wage	27302.22	(23594.11)	25821.76	(24464.02)	20886.16	(24058.87)
Non-Consolidated Firms	0.81	(0.39)	0.74	(0.44)	0.86	(0.34)
Observations	4715		2604		357	

Data are at the firm, zip code, and year level. Monetary variables are in 2012 dollars.

Table A4: Summary Statistics by Treated and Post, S Corporations

	Non-Treated, Pre		Non-Treated, Post		Treated, Pre		Treated, Post	
	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev
Agriculture	0.02	(0.13)	0.02	(0.14)	0.01	(0.10)	0.01	(0.10)
Mining	0.00	(0.00)	0.00	(0.00)	0.00	(0.06)	0.00	(0.06)
Utilities	0.00	(0.00)	0.00	(0.00)	0.00	(0.00)	0.00	(0.00)
Construction	0.08	(0.27)	0.08	(0.27)	0.06	(0.25)	0.07	(0.25)
Manufacturing	0.29	(0.45)	0.29	(0.45)	0.27	(0.45)	0.27	(0.45)
Wholesale	0.23	(0.42)	0.24	(0.42)	0.21	(0.41)	0.21	(0.41)
Retail	0.13	(0.34)	0.13	(0.34)	0.17	(0.38)	0.17	(0.38)
Wholesale and Retail not allocable	0.00	(0.00)	0.00	(0.00)	0.00	(0.00)	0.00	(0.00)
Transportation and Warehousing	0.01	(0.12)	0.02	(0.12)	0.03	(0.16)	0.02	(0.15)
Information	0.00	(0.00)	0.00	(0.00)	0.00	(0.00)	0.00	(0.00)
Finance and Insurance	0.03	(0.17)	0.03	(0.18)	0.03	(0.17)	0.03	(0.16)
Real Estate, Rental, and Leasing	0.02	(0.15)	0.03	(0.16)	0.02	(0.13)	0.02	(0.13)
Professional, Scientific, and Tech Services	0.05	(0.23)	0.05	(0.22)	0.05	(0.21)	0.05	(0.21)
Management (Holding Companies)	0.00	(0.00)	0.00	(0.00)	0.00	(0.00)	0.00	(0.00)
Admin and Support, Waste Management, Remediation Services	0.03	(0.18)	0.03	(0.18)	0.05	(0.21)	0.05	(0.21)
Educational Services	0.00	(0.00)	0.00	(0.00)	0.00	(0.00)	0.00	(0.00)
Health Care and Social Assistance	0.02	(0.13)	0.02	(0.13)	0.02	(0.15)	0.02	(0.14)
Arts, Entertainment, and Recreation	0.00	(0.06)	0.00	(0.06)	0.00	(0.06)	0.00	(0.06)
Accommodation and Food Services	0.07	(0.26)	0.07	(0.25)	0.07	(0.26)	0.07	(0.26)
Other Service	0.00	(0.00)	0.00	(0.00)	0.01	(0.08)	0.01	(0.08)
Total Assets (in 1000s)	28455.04	(49434.80)	32176.79	(69994.22)	37488.91	(89636.92)	41601.79	(95466.09)
Less than 50 W-2 Wage Employees	0.12	(0.32)	0.11	(0.32)	0.11	(0.31)	0.10	(0.30)
50-100 W-2 Wage Employees	0.16	(0.37)	0.15	(0.36)	0.15	(0.36)	0.14	(0.35)
Generated Gross Profits (in 1000s)	21219.00	(45154.56)	24298.78	(60560.64)	29717.38	(83612.62)	32291.75	(87592.44)
Average Wage	33074.64	(27241.23)	35558.31	(28852.28)	32001.48	(42813.99)	34317.15	(40223.25)
Total W-2 Wages (in 1000s)	14079.22	(22557.07)	15067.12	(25246.85)	18015.91	(42111.31)	20148.83	(46444.37)
Observations	582		789		620		898	

Data are at the firm and year level. Monetary variables are in 2012 dollars. Non-treated firms are identified by propensity score matching.

Table A5: Summary Statistics by Treated and Post, Non-Consolidated

	Non-Treated, Pre		Non-Treated, Post		Treated, Pre		Treated, Post	
	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev
Agriculture	0.04	(0.19)	0.04	(0.19)	0.05	(0.22)	0.05	(0.23)
Mining	0.01	(0.10)	0.01	(0.09)	0.00	(0.07)	0.00	(0.07)
Utilities	0.00	(0.03)	0.00	(0.04)	0.00	(0.04)	0.00	(0.03)
Construction	0.10	(0.30)	0.10	(0.30)	0.09	(0.29)	0.09	(0.29)
Manufacturing	0.14	(0.35)	0.14	(0.34)	0.13	(0.34)	0.13	(0.34)
Wholesale	0.14	(0.34)	0.13	(0.34)	0.13	(0.34)	0.13	(0.34)
Retail	0.21	(0.41)	0.22	(0.41)	0.23	(0.42)	0.23	(0.42)
Wholesale and Retail not allocable	0.00	(0.00)	0.00	(0.00)	0.00	(0.03)	0.00	(0.03)
Transportation and Warehousing	0.02	(0.14)	0.02	(0.15)	0.02	(0.16)	0.02	(0.15)
Information	0.02	(0.14)	0.01	(0.10)	0.01	(0.12)	0.01	(0.12)
Finance and Insurance	0.05	(0.22)	0.06	(0.23)	0.04	(0.20)	0.04	(0.20)
Real Estate, Rental, and Leasing	0.02	(0.14)	0.02	(0.14)	0.02	(0.13)	0.02	(0.13)
Professional, Scientific, and Tech Services	0.06	(0.23)	0.05	(0.23)	0.05	(0.22)	0.05	(0.22)
Management (Holding Companies)	0.00	(0.00)	0.00	(0.00)	0.00	(0.00)	0.00	(0.00)
Admin and Support, Waste Management, Remediation Services	0.03	(0.17)	0.02	(0.15)	0.03	(0.17)	0.03	(0.17)
Educational Services	0.01	(0.07)	0.01	(0.08)	0.00	(0.07)	0.00	(0.07)
Health Care and Social Assistance	0.07	(0.25)	0.07	(0.26)	0.07	(0.26)	0.07	(0.26)
Arts, Entertainment, and Recreation	0.01	(0.07)	0.01	(0.07)	0.00	(0.05)	0.00	(0.05)
Accommodation and Food Services	0.04	(0.19)	0.04	(0.19)	0.04	(0.20)	0.04	(0.20)
Other Service	0.04	(0.21)	0.05	(0.21)	0.05	(0.23)	0.05	(0.23)
Total Assets (in 1000s)	8012.73	(39412.55)	9362.10	(47999.53)	5450.28	(36260.63)	5448.99	(32672.87)
Less than 50 W-2 Wage Employees	0.64	(0.48)	0.62	(0.48)	0.69	(0.46)	0.69	(0.46)
50-100 W-2 Wage Employees	0.16	(0.36)	0.18	(0.38)	0.15	(0.36)	0.16	(0.37)
Generated Gross Profits (in 1000s)	3094.35	(8955.54)	3829.83	(10762.34)	2612.00	(8767.29)	2871.78	(10685.63)
Average Wage	26893.77	(18508.65)	29277.37	(21915.51)	26182.42	(24239.75)	27050.75	(23322.27)
Total W-2 Wages (in 1000s)	2057.20	(4583.59)	2397.98	(5416.53)	2025.05	(10397.94)	2218.00	(12570.86)
Observations	1800		2094		2590		3720	

Data are at the firm and year level. Monetary variables are in 2012 dollars. Non-treated firms identified by propensity score matching.

Table A6: Summary Statistics by EZRC and Post, Propensity Score Matched Non-treated firms

	EZRC Zip, Pre		EZRC Zip, Post		Control Zip, Pre		Control Zip, Post	
	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev
Renewal Communities								
Total Wages in 1000s	1885	(2801)	1898	(2884)	799	(1349)	796	(1370)
Total Workers	156	(239)	138	(218)	84	(123)	75	(106)
Average Wage	13472	(7951)	14950	(8227)	10927	(13192)	12083	(13076)
Number of Firms	21	(18)	20	(17)	13	(13)	12	(12)
Observations	972		1458		972		1458	
Empowerment Zones								
Total Wages in 1000s	2144	(3133)	2121	(2858)	744	(980)	742	(1018)
Total Workers	153	(172)	149	(189)	76	(104)	72	(103)
Average Wage	17312	(25802)	17225	(31453)	11601	(10035)	12530	(10716)
Number of Firms	21	(15)	20	(14)	13	(12)	12	(12)
Observations	514		771		514		771	

Data are at the zip code and year level. Monetary variables are in 2012 dollars.

Table A7: Summary Statistics by EZRC, Treated, and Post, Propensity Score Matched Non-treated firms

	Treated, EZRC Zip Code (WG1)		Treated, Control Zip Code (WG2)		Non-Treated, EZRC Zip Code (WG3)		Non-Treated, Control Zip Code (WG4)	
	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev
Renewal Communities								
Pre								
Total Wages in 1000s	1652	(2677)	453	(785)	234	(424)	346	(975)
Total Workers	135	(224)	55	(95)	21	(52)	29	(65)
Average Wage	13676	(8601)	10288	(27363)	12716	(12516)	13245	(14795)
Number of Firms	16	(15)	7	(8)	5	(6)	6	(6)
Observations	972		972		972		972	
Post								
Total Wages in 1000s	1676	(2744)	476	(822)	222	(451)	320	(977)
Total Workers	119	(201)	52	(85)	19	(57)	23	(51)
Average Wage	15084	(8731)	11386	(23605)	13430	(12277)	15277	(18037)
Number of Firms	16	(15)	7	(8)	4	(5)	5	(5)
Observations	1458		1458		1458		1458	
Empowerment Zones								
Pre								
Total Wages in 1000s	1850	(2999)	419	(699)	293	(394)	324	(473)
Total Workers	124	(152)	47	(80)	29	(43)	29	(52)
Average Wage	18092	(27344)	10007	(9539)	13294	(13874)	14719	(19460)
Number of Firms	15	(11)	7	(7)	6	(6)	6	(6)
Observations	514		514		514		514	
Post								
Total Wages in 1000s	1841	(2705)	450	(761)	280	(401)	291	(430)
Total Workers	125	(175)	47	(75)	24	(36)	25	(53)
Average Wage	17909	(32027)	10703	(9100)	13857	(11942)	15834	(14535)
Number of Firms	14	(10)	7	(7)	6	(5)	5	(5)
Observations	771		771		771		771	

Data are at the zip code and year level. Monetary variables are in 2012 dollars.