Employer Wage Subsidy Caps and Part-time Work

Joel A. Elvery
Federal Reserve Bank of Cleveland
Cleveland, OH 44101
Phone: (216) 579-3140
Email: Joel.Elvery@clev.frb.org

C. Lockwood Reynolds
Department of Economics
Kent State University
Kent, OH 44242
Phone: (330) 672-1089
Email: creynol9@kent.edu

and

Shawn M. Rohlin
Department of Economics
Kent State University
Kent, OH 44242
Phone: (330) 672-1098
Email: srohlin@kent.edu

June 2018

Abstract:
This study explores how capped-wage subsidies affect firms' labor market decisions, in particular, their reliance on part-time and low-skilled workers. We focus on the federal Empowerment Zone program, which offers firms in targeted areas a twenty percent wage subsidy (capped at $3,000 per year) for each employee who also resides in the Empowerment Zone. Results using three methods of identification (OLS, propensity score matching, and instrumental variables) suggest that firms respond to capped-wage subsidies by expanding their use of part-time workers, particularly where the subsidy cap is likely to bind. We also provide evidence of a shift toward lower-skilled workers.

JEL: H25, J23, J48.

1. Introduction

One set of tools that governments have for intervening in labor markets are subsidies or credits given to employers to hire workers. Such programs could theoretically be used to encourage hiring of disadvantaged workers, spur hiring during economic downturns, or encourage hiring in impoverished areas. Given these potential benefits, it is not surprising that there has been academic interest in studying these programs for several decades (see Kaldor (1936), literature reviews in Katz (1998) and Neumark (2013). The existing evidence, however, does not provide strong support for the effectiveness of such programs, at least in terms of job creation. While some authors find evidence of job creation in certain programs, or for specific populations, or in the short term (e.g. Katz (1998), Jaenichen and Stephan (2011), Boockmann et al. (2012), Faulk (2002), Hamersma (2008)), others find little evidence of employment increases, particularly in the long-run (e.g. Burtless (1985), Hamersma (2008)). Researchers have discussed and investigated when these programs could be successful, for example during recessions (e.g. Neumark (2013), Neumark and Grijalva (2013)), or how they can be designed and implemented differently to achieve their goals (e.g. Almeida, Orr and Robalino (2014), Bartik and Bishop (2009)).

The literature has identified a variety of reasons why this policy may not be effective at improving labor market conditions for workers. There is evidence that targeted workers may be negatively affected because of stigma effects (Burtless, 1985). These programs have also often suffered from low take-up rates by firms (for example, the Targeted Jobs Tax Credit (Bishop and Montgomery, 1993) and the Work Opportunity Tax Credit (WOTC) and Welfare-to-Work Tax Credit (WtW) (Hamersma, 2011)). This could be due to a lack of awareness of the programs, or administrative costs of identifying eligible workers. Using a survey, Hamersma and Heinrich

(2008) find that a large sample of firms were aware of the WOTC program but that it did not influence their hiring. And Hamersma (2011) finds evidence that there appears to be fixed costs of program participation for firms in the WOTC/WtW programs, possibly because of minimum job duration requirements.

We aim to study the effect of another common feature of these policies: a limit on the amount of money that is provided to firms. We do so by looking at the federal Empowerment Zone (EZ) program, which was enacted in 1994. The EZ program provided a 20% wage credit, up to $3,000, for each eligible employee at eligible firms. Importantly for us to focus on the credit cap, several features of the EZ program help to avoid some of the potential problems of wage subsidies previously identified. The program chose a set of economically distressed areas within cities, and firms are eligible to receive the credit if the firm was located in the area (or relocated to the area) and the worker was a resident of the area. So the program is designed not to directly target disadvantaged individuals, but to target areas with a large proportion of disadvantage individuals. As noted previously in the literature (see, for example, Katz (1998)), this location-based targeting could avoid some of the negative stigma problems that can occur with individual-targeted credits (Burtless (1985)).

Another difference is that firms may be more likely to respond to the program than other policies previously studied. First, the wage credit applies to both new and existing workers, so even firms who are not interested in hiring new workers have incentives to respond to the wage subsidy. Second, the program allowed firms to collect the credit for at least 10 years, with the possibility for more years if the program was renewed (the EZ program has been renewed through 2017). So firms have the potential to receive money for a long time period, making it more likely that the benefits of hiring outweigh any administrative or fixed costs of firm participation in the program.[1]

Our interest, however, is in how the $3,000 cap on the wage credit affects firm employment decisions, particularly in terms of weekly hours of work. The wage credit cap means that the marginal cost of an hour of work increases at annual earnings of $15,000. We then expect that, assuming firms hire permanent positions, some firms may respond to this change in marginal cost by either employing more workers at less than full-time hours, or by hiring workers in lower-wage occupations. Our argument is that if firms face low market wages, then the wage credit cap does not bind. At higher market wages, the wage credit cap binds at less than full-time work and firms have incentives to adjust their hiring. Firms may only hire additional workers at low hours or they may substitute workers and weekly hours, which allows them to claim more credits for the same total labor hours.

We are unaware of research that has studied the incentives created by capped-wage subsidies for firms to substitute toward part-time and low-wage work.[2] Neumark (2013) notes that targeting credits at employment could create incentives to hire at low hours. Hamersma (2011) notes that few workers in the WOTC/WtW are near the credit cap, so she cannot investigate firm responses to the cap. Some prior research has tested for rent-seeking behavior by firms. For example, Lorenz (1995) discusses problems in the design and implementation of the Targeted Jobs Creation Tax credit program that allowed rent-seeking. Hamersma (2011) finds no evidence that firms attempt to maximize their credits in their hiring decisions, possibly

[1] As discussed later, there is an extensive literature that attempts to evaluate the success of the EZ program as policy for local economic development. As discussed in more detail later, the focus of that literature is more about whether the targeted areas improved in some measurable way (e.g. housing values, unemployment, poverty). While we use similar methodologies to this literature, our goal is to use the EZ program to learn more about how firms respond to the design of wage subsidy programs.

[2] Researchers have found that firms respond with part-time work in response to other policies, most notably the Affordable Care Act (e.g. Even and MacPherson, forthcoming).

because it was administratively difficult to identify the subsidized workers, or because the WOTC/WtW programs had minimum job tenure that may be less easy to manipulate than weekly hours.[3] Ajilore (2012) studies the WOTC program and finds no evidence that a decrease in employment among workers who he argues are close substitutes to targeted workers. A report by the U.S. General Accounting Office (2001) finds no evidence that firms fired workers to claim credits on new hires in the WOTC program.

We present evidence that the wage subsidy cap in the federal EZ program is likely to bind for some firms. To investigate whether firms respond by using more part-time work, we use data from the Census Transportation Planning Packages. This is public-use data on workers aggregated to the place-of-work census tract. It provides information about the hours usually worked by tract workers in 1990 and 2000. Combining this data with other tract information from 1980 and 1990, we employ a differencing methodology comparing the change in the proportion of workers in EZ tracts working less than 35 hours a week from 1990 to 2000, to the change in a set of comparison tracts that were eligible, but not selected for, the EZ program. We also consider triple-difference models that account for trends in the surrounding cities, and estimate the effects using OLS regressions, propensity score matching and instrumental variable methods. Differencing methods are commonly used to analyze the effects of wage credits, and the specific empirical strategies we employ are common in the related literature that evaluates the effectiveness of the federal EZ program as a local economic development program (Neumark and Simpson, 2015).

[3] Hamersma (2011) does find that firms with a higher potential exposure to the wage credit, having a higher proportion of workers with job tenures exceeding the minimum thresholds, are more likely to claim the credits. Similarly, we argue that some firms are more likely to respond to the wage subsidy cap based on whether market wages cause the cap to bind.

Our results show a modest increase (approximately a 1 percentage point change from a base of 19 percent, so roughly a $\frac{1}{19} = 5.3$ percent increase) in part-time work among EZ workers, on average, although the effects are not always statistically significant. However, as discussed, only some firms are likely to face market wages at which the wage subsidy cap binds. We provide estimates showing that reliance on part-time work increases more in tracts with a higher proportion of workers earning at least $15,000 in the tract in 1990. This is consistent with a larger part-time response by firms in areas more likely to face the earnings cap of the subsidy. We also provide evidence of increase in part-time work among men, who are more likely to work part-time for economic reasons, but not among women. Finally, we provide evidence that some firms may have responded not by expanding the use of part-time work, but by hiring lower-wage workers. We find consistent evidence that the proportion of workers with less than a high school degree increased, and the proportion of workers with a college degree or more decreased, in response to the EZ program.

Overall, we see our findings as evidence that firms were responding to the cap on the wage credit in the federal EZ program. The findings provide further evidence that the implementation and design of wage credit programs are important factors in their success. In Section 2, we discuss the federal EZ program and present a more detailed discussion of potential firm responses. In Section 3, we discuss our data and estimation methodologies. In Section 4 we present the results of our estimations and we conclude with a discussion in Section 5.

2. Labor Demand Responses to the Federal Empowerment Zone Program

2.1 Federal Empowerment Zone Program

The federal Empowerment Zone program was created as part of the 1993 Omnibus Budget Reconciliation Act (OBRA 1993, P.L. 103–66) to incentivize firms to locate in and hire workers living in specified distressed areas. State and local governments were encouraged to submit EZ applications that met two criteria: at least 20 percent of the population had to live below the poverty line and the area had to have an unemployment rate of at least 6.3% (GAO, 2004). A total of 78 (Wallace, 2004) applications were received and 6 urban areas within cities (located in Atlanta, Baltimore, Chicago, Detroit, Philadelphia/Camden, New York) and 3 rural areas (Kentucky Highlands, Mississippi Delta, and the Rio Grande Valley in Texas) were chosen.[4]

EZ areas received several benefits from the program. However, the most generous and utilized benefit (Hanson, 2011) was a capped refundable wage credit to businesses for operating in the EZ areas and hiring residents of the areas.[5] A business located in the EZ area would be allowed to claim a wage tax credit of 20 percent on the first $15,000 in wages paid to a qualified worker. Because the aim of the program was to attract employers to the area, the wage credit was available both to firms already operating in the EZ areas as well as to any firm that relocated to the EZ area. A qualified worker was someone who lived in the EZ, performed the majority of their work in the EZ, and was employed by the firm for at least 90 days. Unlike many policies aimed at increasing new employment by restricting the benefit to only new hires, such as the Job Creation Tax Credit, the EZ program allowed firms to apply the wage credit to all qualified workers, both existing and new employees. The wage credit, therefore, was available to both new and existing employers in the area and was applicable to both existing and new qualified employees.

[4] EZ areas did not have to be contiguous. See Figure 2 for example maps of the New York and Chicago EZ areas.
[5] The other incentives associated with EZ designation in urban areas was a one-time $100 million Social Service Block Grant (SSBG) and a set of relatively small capital incentives. Neither of these portions of the program should induce the labor demand changes that we document in this paper. The SSBG were spent differently in each city on a variety of projects such as transportation and consulting. The capital incentives included the ability to exclude capital gains on the sale of certain assets and increasing the amount of property that can be immediately expensed. For a complete description of the EZ incentives see (HUD, 2001).

The fact that the federal EZ program applies not just to newly hired workers could increase take-up rates, although with the possible concern that firms will simply claim the credit and not expand hiring. While targeting job creation is typically desired in wage subsidy policies to avoid rent-capture by firms, the EZ program was designed as a local economic development tool. In that case, while new job creation would be good, wage increases, additional hours, or firm relocation could all be beneficial outcomes. In fact, a wide literature has used the EZ program to investigate the effectiveness of place-based economic development programs, including a broad range of outcomes such as firm movement, property values, and resident outcomes (e.g. earnings, employment and poverty). Neumark and Simpson (2015) reviews this literature and generally finds that place-based policies lead to increases in local property values, while the evidence on resident outcomes is somewhat mixed.

2.2 Potential Firm Responses

For discussion purposes, assume that firms in the EZ areas employ a production function $Q = f(K, E)$ involving capital (K) and total weekly labor hours (E). For expositional ease, assume that all workers are hired for 52 weeks a year. The fact that firms are able to claim the credit for multiple years and only for workers employed at least 90 days helps justify this assumption. Firms are able to choose total weekly labor hours by combining the number of employees (N) and the hours worked (h). We will allow firms to set different hours for different workers so that $E = \sum_{i=1}^{N} h_i$. Further assume firms operate in a competitive labor market so that the labor cost per worker is $C_i = wh_i$ where w is the market wage. Finally, assume for now that there are no employment fixed costs, there are no adjustment costs and that the production technology allows firms to substitute between h and N.

The wage subsidy in the EZ program changes the marginal expense to the firm of employing an hour of work. Assuming that the worker is hired for 52 weeks a year, the marginal cost of employing an eligible employee for an additional hour of work will be $0.8w$ if $52wh \leq S$, where S is the annual earnings cap for the EZ program ($15,000). If $52wh > S$, then the marginal cost of an additional hour is simply w. This change in the marginal cost of employment could cause firms to adjust their employment decisions. For example, some firms could decide to hire new employees. Other firms could decide to expand hours of current employees (both new and current employees are eligible for the wage subsidy in the EZ program). How firms respond will depend on the level of the annual salary cap of the subsidy, since the marginal cost changes at the cap, the level of the wage that the firm must pay, and the hours that they choose to employ workers each week (assuming they hire for a full year). We expect that the weekly hours chosen by the firm will decrease as market wages increase, since firms will reach the maximum wage credit at lower levels of hours worked.

Figure 1 depicts the combination of weekly hours and market wages that exactly equal the federal EZ earnings cap of $15,000 (assuming 52 weeks of work). For any wage greater than $7.21 ($= \frac{\$15,000}{52h}$), firms would reach the earnings cap at less than 40 hours of weekly work.[6] For comparison, the federal minimum wage in 2000 was $5.15 and the median wage was $14.12.[7] Exact wage data on the EZ areas is not available since individual observations are not identified in public-use data at that small level of geography. However, the average wage in the area can be approximated using Census tract data by combining the aggregate earnings in a tract with the distribution of hours and weeks worked. The average of this calculation for the EZ tracts is

[6] In our analysis, we will define part-time work as working less than 35 hours a week. The equivalent wage, assuming 35 hours of work and 52 weeks a year, is $8.24.
[7] Authors' calculations of workers age 16-64 with non-zero wages in the 2000 Census, using IPUMS data.

$11.71, roughly the 37th percentile of the national distribution, consistent with these areas being distressed.

Given the wages in Figure 1, it seems likely that many firms in the EZ areas will have an incentive to employ a higher proportion of workers in a part-time capacity. The average EZ worker earns a wage high enough that the firm reaches the earnings cap well below full-time employment (approximately 25 hours, see Figure 1). Firms responding to the wage subsidy by hiring new workers will likely hire part-time employees, unless they pay well below the area average (for example, at the federal minimum). Similarly, while some firms may raise the hours of their employees, this effect is unlikely to cause an increase in full-time work because the earnings cap is achieved below 40 hours for any wage above $7.21. Again, only firms paying wages significantly lower than the area average are likely to respond by increasing hours of current workers to full-time status. For most firms, any increase in a worker's hours is likely to keep the worker part-time.

Finally, firms whose wage and hours combination is beyond the earnings cap frontier in Figure 1 have an incentive to change their total hours composition to employ more workers, but at lower weekly hours. For example, firms that face the average wage of $11.71, employing workers at 40 hours per week for 52 weeks a year will spend (40*52*11.71) = $24,356.80. Following the EZ designation, the firm can claim the full $3,000 wage credit for each employee. However, suppose the firm splits each position in half, employing twice as many workers for 20 hours each. Now each employee earns (20*52*11.71) = $12,178.40 and generates a wage credit of (0.2*12,178.40) = $2,435.68. The total wage credits per 40 hours of work are now (2*2,435.68) = $4,871.36, an increase of $1,871.36. Thus, firms attempting to maximize their wage subsidy will have an incentive to move toward part-time work, as long as they face high enough market wages. Firms paying low wages are not able to increase their wage credit by substituting workers and hours.

This discussion leads to the main empirical question that we are attempting to answer. Did the earnings cap on the EZ wage credit, in combination with predominant market wages, cause firms to respond by expanding their use of part-time work? While the structure of the wage subsidy and the predominant wages in EZ areas would appear to incentivize firms to shift to more part-time work, there are reasons why such a shift may not occur. For example, if there are administrative costs to take-up, as found in the WOTC/WtW program by Hamersma (2011), then there would be no overall effect. However, as discussed in the introduction, the fact that the wage credit applies to both new and current eligible workers increases the incentives for firms to take-up the wage credit.

Neumark (2013) notes that because labor demand elasticities tend to be larger for the extensive margin than the intensive margin, then we might expect that wage credits have a larger effect on hiring than on hours. If so, firms may not adjust hours of current workers, but could still choose to employ new part-time workers. Furthermore, under our assumption of permanent, full-year employment, the wage credit takes a form of a kink in the marginal cost of an hour of work. A similar kink in the cost of labor is caused by overtime rules, which firms have been found to respond to on the intensive margin (e.g. Costa (2000) and Hamermesh and Trejo (2000)).

Second, if firms face fixed costs for employment or some form of adjustment costs, then we may see muted responses. However, it seems unlikely that employment fixed costs are high enough to prevent firms from responding to the wage credit, especially since part-time workers often do not receive benefits, reducing their compensation costs. A fixed hiring cost would only be incurred for new hires, so adjusting existing workers hours in response to the credit is less affected by the fixed costs. Similarly, while there could be adjustment costs that cause firms to not change levels of employment quickly, that would again not apply to the current workers who could see their hours adjusted. Furthermore, we will be investigating worker outcomes six years after the EZ program was created, allowing substantial time for firms to adjust their workforces.

We have assumed that labor is divisible, thus leading firms to consider changing the composition of its workforce by lowering h and raising N in response to the wage credit, all while holding total employed hours (E) constant. If firms are unable to easily substitute hours and workers then this composition effect will be reduced. This could occur if the production technology requires a minimum amount of hours worked per person or if production tasks cannot be easily divided among many workers. Indivisible labor would tend to limit firms simply changing the composition of their workforce to maximize their wage credits. However, firms may still have incentives to expand hiring at part-time hours, as long as the minimum required hours is less than full-time.

Finally, it is possible that firms will respond not by hiring at low levels of hours, but by hiring at low wage levels. Assuming that the firms cannot pay below market wages for a particular job, this would mean that they hire more positions at the bottom of the wage scale, presumably lower-skilled workers. These workers can be hired for more hours before hitting the subsidy cap. In addition to exploring a change in part-time work, we also explore shifts in the education levels of workers in the EZ areas. If firms respond on this margin, we would expect to see a relative increase in the proportion of less-skilled workers in these areas.

The discussion so far has assumed that the wage subsidy has had no effect on market wages. A wage subsidy in a market can distort market wages, with the incidence dependent on the relative elasticity of labor supply and demand. Hamersma (2008) provides evidence that about 38% of the wage credit in the WOTC/WtW programs are passed through to workers in the form of higher wages. However, the wage subsidy in the EZ program could have less of an effect on market wages. First, the wage subsidy does not apply to the entire labor market but instead only applies to firms operating within a limited geographic area and hiring residents of that area. Second, the distressed nature of the EZ areas makes it likely there is initially an excess supply of labor, reducing the likelihood that wages are bid up. To the extent that wages are pushed up by the subsidy, it would further increase the likelihood of an overall shift towards part-time work since the earnings cap is achieved at lower hours of work as the wage rises.

3. Empirical Strategy

3.1 Data

Studying firm responses to the wage credit in the federal EZ program is difficult because individual firm or worker data is not generally publicly available.[8] Instead, we use data from the Decennial Census aggregated to the Census tract level, chosen because the empowerment zone applications were defined by tract geography. Thus, our unit of analysis is a Census tract, for which there are 273 tracts in EZ areas and 942 tracts in our comparison areas (described below).

Much of the existing literature on the EZ program uses data from the Summary File datasets of the Decennial Censuses, which are tabulations of population and housing characteristics within Census tracts. In addition to being utilized by previous researchers, it was also used in the application process for the EZ program. Importantly, these tabulations are of the population that *resides* in these areas. Our interest is in the response of firms who *employ* individuals in these areas. While we use some Summary File data from 1980 and 1990 to control

[8] That information is only available in the confidential, restricted access Decennial Census Long-form data that were used by Busso, Gregory, and Kline (2013). Accessing that data is cost-prohibitive for our project.

for tract characteristics and trends, we construct our outcome measures from the 1990 and 2000 Census Transportation Planning Packages. This is the only publicly available data source for the attributes of workers by place-of-work tract and it enables us to investigate changes in the characteristics of workers who work in the designated EZ areas.[9]

The Census Transportation Planning Package (CTPP) provides counts of workers by place-of-work for a variety of traits. It can be thought of as the equivalent to the Summary Files, except tabulated by place-of-work instead of place-of-residence. The universe for the CTPP is everyone in the Decennial Census Long-form sample (1-in-6 households) aged 16 and over who worked for pay in the week before the Census. The CTPP is primarily designed to meet the needs of transportation planners, but it also contains information on workers traits, including hours worked, annual earnings, occupation, industry, gender, ethnicity, and race.

In our primary analysis, we use the information on hours worked to investigate whether there is a shift towards more part-time work. Counts of workers by hours usually worked per week are provided for the following range of hours: < 15, 15-20, 21-34, 35-40, 41-55, and > 55. Dividing the number of workers in the first three bins by the total number of workers provides a measure of the proportion of workers whose weekly work is less than 35 hours a week. The percent of tract workers with weekly hours less than 35 hours a week will be our primary outcome of interest throughout our analysis. However, as mentioned previously, firms could respond to the wage subsidy incentives by increasing hiring primarily at low-wage levels, for which the earnings cap is less likely to bind. Thus, we will use information on the types of occupations to investigate changes in the skill mix of workers that might arise due to the incentives of the capped wage subsidy.

---

[9] Tract boundaries can change over time. We use a Graphical Information System (GIS) procedure to generate consistent geographies across 1980, 1990 and 2000.

There are two limitations of the CTPP data that will tend to cause our estimates to attenuate towards zero. One limitation of the CTPP is that it does not allow us to separately identify information about zone employees who live in the EZ area and those zone employees who do not. Because the wage tax credit applies only to zone employees who reside in the zone, it would be ideal to be able to study their hours and earnings separately. Not being able to separate zone residents from commuters means that we cannot study the effects of capped wage subsidy on only the eligible workers. A second limitation is that we only observe total (usual) hours of work across all jobs for workers. If firms respond to the subsidy cap by showing preference for part-time work, workers may respond by getting a second job. Because we do not observe hours at a particular job, we will fail to identify some shift towards part-time work if workers are using multiple jobs to reach 35 hours or more a week.

Both of these limitations result from the aggregated nature of our data. Since we cannot focus only on the qualified workers, our estimates will contain a number of zero effects from non-qualified workers, attenuating our results towards zero. Additionally, since we do not observe hours at each job, we will miss some proportion of the firms' shift towards part-time work. As discussed later, we find some evidence of the first problem as we tend to find larger effects when we weight the regressions by the share of workers in each tract who are zone residents.[10] However, we cannot solve the second limitation. Thus, our results may underestimate the true effect of changes in part-time work among the workers actually eligible for the subsidy.

3.2 Methodology

[10] The CTPP's Flows tables provide some tabulations for combinations of place-of-work tracts and place-of-residence tracts, but does not provide information on hours, earnings, or occupation. So while we cannot separate zone worker-residents outcomes from zone worker-nonresident outcomes, we can calculate the share of workers in a tract who are residents of the zone.

We utilize a differencing methodology, which compares worker outcomes in EZ areas to a set of counterfactual areas before and after the enactment of the policy. Using the federal EZ program to study how firms react to capped-wage subsidies presents three methodological issues typically found in the place-based policies literature: selection of counterfactuals, controlling for city effects, and endogeneity concerns. In order to correctly identify the effect of the wage subsidies on firms in the EZ areas we would need to know the labor decisions of firms in the EZ areas without the wage subsidy. Since this is impossible, we use EC areas that applied and qualified for the program as a basis for constructing our counterfactual areas. The use of these areas is common in the EZ evaluation literature (e.g. Hanson,2009, Busso et al., 2013, Reynolds and Rohlin 2015).

EC areas are used to construct counterfactuals for a number of reasons. First, EC areas received most of the incentives EZ areas were entitled except that they did not receive the wage subsidy, which we are attempting to study.[11] Comparing EZ areas to locations that were not entitled to the other incentives would only further confound our identification of the wage subsidy effect. Second, using counterfactual areas that applied to the program but did not receive the subsidy removes application bias. Third, EC areas are not located in the same cities as the areas that received EZ status. This is important because it has been shown that the EZ program had spillover effects within cities in which the EZ areas were designated (Hanson and Rohlin, 2013).[12]

[11] The other major difference is that the EC areas received much less Social Service Block Grants, but as discussed in footnote 2 those funds should have no impact on the use of part-time workers in the area.
[12] An additional concern about using areas within cities that received EZ status is endogenous selection by local policymakers since these alternative areas were specifically not included in the original application, despite the fact that they were similarly distressed.

While the EC and EZ areas all met the minimum qualifications for empowerment zone consideration, the EZ areas were more distressed in a variety of ways, as has been documented previously in the EZ literature (e.g. Busso et al., 2013; Hanson, 2009; Reynolds and Rohlin, 2014)). Table 1 presents summary statistics on characteristics of EZ and EC areas using data from 1980 and 1990. While both EZ and EC areas have high poverty rates and unemployment in 1990, the EZ areas exceed the EC areas in both categories by approximately 6 percentage points. Furthermore, the EZ areas were higher in both categories in 1980 as well, indicating that they were more distressed on average for at least 15 years prior to EZ designation. EZ areas also have lower median incomes, a higher welfare rate, lower home ownership and lower home values than EC areas. EZ areas also have a higher minority rate and a higher rate of female-headed households. These differences mean that while EC areas are useful for comparison, we cannot simply use them as counterfactuals. Consequently, we will include the 1980 and 1990 variables as covariates in our analysis to account for these differences.[13]

The simplest version of our identification strategy compares the change from 1990 to 2000 (the tax incentives were provided in 1995) in outcomes in EZ areas to changes in EC areas over the same time period. By differencing within each geographic area, we remove any time-invariant factors that may affect firms' labor demand decisions, such as access or proximity to transportation hubs and other business amenities that did not change during the decade. In these regressions the dependent variable takes the form:

$$Y_i = Y_{i2000} - Y_{i1990} \tag{1}$$

[13] Note that the reliance on part-time workers in both EZ and EC areas falls from 1990 to 2000, although the change is larger for the EC areas. Thus, unconditional estimates comparing the EZ and EC areas over time would suggest a relative increase in part-time work in EZ areas.

where *i* denotes a Census tract in an EZ or EC area and *Y* is an outcome of interest, for example the proportion of workers with weekly hours less than 35.

However, controlling for city-wide changes is warranted because EZ areas could be affected by overall economic trends in the cities in which they are located. Therefore, our preferred approach is a triple-difference that compares relative outcomes between EZ areas and the rest of their cities, with EC areas and their cities, before and after the policy. We isolate the effect of the EZ policy from city-specific effects by making a comparison across time. The underlying assumption being that the difference between these application areas and their cities would have grown the same without the policy. Therefore, the dependent variable used in our regressions accounting for city-wide effects takes the following form:

$$Y_i = (Y_{i2000} - \bar{Y}_{j2000}) - (Y_{i1990} - \bar{Y}_{j1990}) \qquad (2)$$

where Y_{i2000} and Y_{i1990} are defined as before and $\bar{Y}_{j2000}$ and $\bar{Y}_{j1990}$ represent the average outcomes in 2000 and 1990 respectively in the *j* census tracts in the respective EZ and EC cities.

The differencing between tract and city in equation (2) eliminates any differences in outcomes due to city-specific changes between 1990 and 2000, including changes in city-wide economic growth, as well as any city-wide local government policies that influence firms' labor demand decisions. Another possible approach to controlling for city-specific changes would be to use similarly distressed areas within the EZ and EC cities. These areas more closely match the distressed areas EZ and EC areas within each city, and therefore may more closely capture the local economic trends in distressed areas of the cities. However, these areas may be susceptible to contamination, as Hanson and Rohlin (2013) provide evidence that these areas experience negative spillover effects as firms relocated within cities to the EZ areas. As it is difficult to definitively argue whether these areas are better controls for local trends, we choose to present triple-difference estimates using either the entire remainder of the city or just the economically similar areas within each city.[14]

Our base methodological approach to determine how firms respond to capped-wage subsidies is a standard OLS regression. The estimating equation we use is

$$Y_i = \alpha + \beta EZ_i + X_i'\delta + \epsilon_i \qquad (3)$$

where *i* indicates census tracts, *EZ* represents a dummy variable for whether the EZ wage subsidy is provided (meaning the census tract is in an EZ area). A vector of control variables is represented by *X* and includes the 1990 and 1980 variables summarized in Table 1.

We will also use two additional approaches that are used in the place-based policy literature to address potential endogeneity concerns: propensity score reweighting (Busso et al., 2013; Reynolds and Rohlin, 2015) and instrumental variables (Hanson 2009, Hanson and Rohlin (2011a, 2011b)). The first approach attempts to reweight the EC areas to create counterfactuals that more closely match the distribution of 1990 and 1980 characteristics observed in the EZ areas.[15] Propensity score matching is a technique for balancing the distributions of observable covariates through the use of the propensity score, or predicted probability of treatment conditional on the observable characteristics (Rosenbaum and Rubin, 1983). In our context, we first estimate a logit of

$$EZ_i = \alpha + X_i'\delta + \epsilon_i \qquad (4)$$

and then calculate the propensity score as the predicted probabilities $\hat{P}(X)$. We then use these predicted probabilities in a nearest neighbor matching algorithm, where the estimated treatment

[14] We identify economically similar areas by estimating, within each city, the probability of being an EZ or EC tract using the 1990 and 1980 covariates in Table 1. We then use a nearest neighbor algorithm with a single match to find the tract within the city that most closely matches each EZ or EC tract.

[15] Importantly, we do not directly match on reliance of part-time workers in 1990 because that is part of our differenced dependent variable.

effect is the average difference between the outcome in each EZ tract and the average outcome of the *n* closest matches. Specifically, for each EZ tract we find the *n* EC tracts with the closest values of $\hat{P}(X)$. The choice of n involves a bias/variance tradeoff: higher numbers of matches produce more precise estimates but may do so at the cost of higher bias in the estimates. We choose $n = 10$ but limit matches to be within one percentage point of $\hat{P}(X)$. Thus, we use up to 10 possible matches but only within an acceptable range of match quality (commonly called the caliper). Our matching method does a good job of balancing covariates, reducing the difference in means for all controls variables between EZ and EC areas, and the remaining differences are usual not statistically significant (see Appendix Table A-1). For inference, we use a block-bootstrap procedure with 1000 replications. All of our main results are robust to our choice of *n* and caliper width.

The other approach for addressing possible endogeneity concerns is using an instrumental variable approach. The potential worry is that EZ areas were chosen based on their future economic expectations, either positive or negative.[16] The ideal instrument for obtaining EZ status would be correlated with receiving EZ status while being unrelated to our outcomes (the share of workers employed in part-time or low-wage jobs). We follow the existing literature on the federal EZ program and use two instruments: a dummy variable for whether the area had a representative on the United States House of Representatives Ways and Means Committee, and the number of years that the representative was in office at the time of EZ designation. These political variables have been shown to be highly correlated with EZ designation (Wallace (2004) and Hanson (2009)) and are demonstrated to be so in Appendix Table A-2. These instruments

[16] For a complete discussion of the selection process see Hanson (2009).

are individually significant as well as jointly significant shown by their respective *p*-values and instrument F-test.[17]

4. Results

4.1 Part-time versus Full-time Employment

We begin by testing our hypothesis that the subsidy cap will encourage part-time work. Table 2 presents the baseline results of the effect of EZ designation on the proportion of workers employed less than 35 hours a week, across our various specifications. Each cell represents the results of a specific estimation strategy defined by the type of differencing (double difference, city-wide triple difference, or partial-city triple difference), empirical method (OLS, propensity score matching, or IV), and set of covariates (none, 1990, or 1980 and 1990). Standard errors clustered by city are presented for the OLS and IV estimates, while block-bootstrapped standard errors are presented for the propensity score estimates.

The unconditional estimate in the first row of column (i) of the top panel indicates that EZ areas experienced a 1.4 percentage point increase in the proportion of area workers working less than 35 hours a week compared to EC areas. Table 1 indicates that part-time workers made up roughly 19 percent of the workforce in EZ and EC areas so this is roughly a $\frac{1.4}{19} = 7.4$ percent increase in part-time workers. Including 1990 controls in column (ii) has no effect on this estimate, and adding 1980 and 1990 controls in column (iii) slightly reduces the estimate to a 1.1 percentage increase. These results are consistent with the expectation that the earnings cap on the wage subsidy increased the use of part-time work. The propensity score matching estimates in the second row of the top panel present similar results, although the estimates are not

[17] We test for over-identification using the Sargon–Hansen J-statistic and find a Hansen J-statistic of 1.547 and Chi-sq p-value of .2136.

statistically significant. Finally, the third row of the top panel presents the IV estimates. While the point estimate without any controls is twice as large as the OLS estimates, inclusion of 1990 and 1980 controls produces similar estimates as the OLS and matching methods but the estimates are imprecise and not statistically significant. In general, all methods produce similar results, with the main differences being precision. This is not surprising as propensity score matching and IV estimates tend to be less precise than OLS estimates.[18]

These results present suggestive evidence of an increased use of part-time workers from our simple difference-in-difference strategy between EZ and EC areas. However, these results do not account for city-wide trends in part-time work that might be confounding our estimates. The middle panel of Table 2 presents our triple-differencing strategy that removes any city-wide trends in part-time work. These triple-difference estimates are very similar, if slightly smaller, than the simple difference estimates in the top panel. The bottom panel present the triple-difference results that use similarly distressed areas within the cities to account for local trends. These estimates are smaller than the top two panels, suggesting somewhere between a 0.5 and 1 percentage point increase in part-time work, although the estimates are generally imprecise.

In general, Table 2 suggests a modest increase in part-time work, although the magnitude of the effect (and statistical significance) decreases as we add controls and move from double-differencing to triple-differencing models. Given the discussion in Section 2, these results are somewhat surprising. However, there are two important caveats. First, these results likely underestimate the use of part-time work among qualified workers in the EZ areas because of

[18] The precision of propensity score estimates is related to the algorithm used; algorithms using more controls observations will have lower variance but potentially higher bias. We replicated results using propensity score reweighting which tends to be more efficient than nearest neighbor matching (Busso, DiNardo and McCrary, 2014). We found slightly larger point estimates and somewhat smaller standard errors; however the main conclusions are unchanged.

limitations with our outcome data, as discussed in the previous section. The fact that our outcome includes both subsidy-eligible and ineligible workers would attenuate our results. We find some evidence of this in Appendix Table A-3, where we weight our estimates based on the proportion of workers that we know live in EZ areas (based on CTPP data). Essentially, we would expect that tracts with very few eligible workers would show less response compared to tracts with many eligible workers. We generally find larger effects of EZ designation on part-time work using such weights, but only in the double-difference and entire-city triple difference models. So there may be some attenuation due to our inability to identify the effects among eligible workers. Furthermore, we suspect that we are missing workers having multiple part-time jobs, equating to over 35 hours of total work. This would also attenuate our results. The other caveat is that there are reasons to suspect that these average effects mask important heterogeneity in the response of firms, which we explore in the next two sections.

4.2 Results by Subsidy Cap Constraint

Our discussion in Section 2 argued that the prevailing wages in the EZ areas would tend to incentivize the use of part-time work. However, we know that there is variation in wages within the EZ areas and we argued that the incentives to increase the use of part-time work should increase with wages. We can directly test the role of the prevailing wages by using information on the percent of workers in each tract whose annual earnings are at, or above, the subsidy cap of $15,000. More workers above that threshold in 1990 would suggest that firms in these areas have jobs that are more likely constrained by wages and the cap, thereby encouraging part-time work. So the distribution of the annual earnings of workers in 1990 allows us to directly test whether the earnings cap has an effect on the hiring decisions of firms.

To perform this test, we put tracts in three categories based on the proportion of workers in 1990 earning at least $15,000. The first group are tracts with less than 50% of workers above the salary cap, the second group are tracts with 50-75% of workers above the cap, and the last group are tracts with more than 75% of workers above the cap. These groups loosely correspond to tracts where few firms are constrained, more firms are constrained and many firms are constrained, respectively. We then include dummy variables for these categories, and their interaction with the EZ dummy variable, in our OLS regression framework. Our left-out category is the first group, the least constrained tracts. For simplicity, we present only the two triple-difference models, which account for local trends, and those that include covariates (we do not report the naïve estimates, those without covariates, or the results of the double-difference models, all the results are similar and available upon request).[19]

The first two columns of Table 3 present the results from the triple-differencing strategy that accounts for city-wide trends. The coefficient on the EZ dummy variable represents the effect on the reliance on part-time workers in EZ tracts that are least constrained (have less than 50% of workers above the earnings cap). Regardless of controls, the coefficients suggest that these less-constrained tracts experienced a decrease in part-time work among employees, approximately a 2.7 percent decline. The coefficients on the non-interacted terms suggest that, on average, part-time use increased across the EZ/EC tracts from 1990 to 2000 relative to the least constrained tracts.[20]

[19] Our cutpoints put most tracts in the middle category: approximately 12 percent of tracts are categorized as least constrained, 74 percent of tracts are in the middle category and 14 percent of tracts are in the most constrained category. We chose our cutpoints both because we feel that they are easy to interpret but also because they roughly match the inflection points from a model that fully interacts a cubic function of the percent of workers making over $15,000 with the EZ indicator. The results from that specification, while somewhat less easy to interpret, match the overall results from our categorical specification.

[20] This could be due to higher wage areas expanding more in part-time work because they face higher labor costs in general, or because of the industrial mix in these areas. Estimates show that these tracts have a higher proportion of service and retail jobs, which are two sectors more likely to employ part-time workers (part-time workers are

Importantly, the interaction terms suggest that as earnings cap in the EZ areas begins to bind more, as more workers earn above the cap, then the use of part-time work increases among workers. The coefficient on the interaction of the EZ dummy variable with the middle category of tracts (50-75% of workers above the cap) is consistent with a 3.6% increase in part-time work relative to the less constrained tracts. The coefficient on the interaction term for the highest category (greater than 75% of workers above the cap) suggests a 5.7 percent increase in part-time work, relative the least constrained tracts. So, while both EZ and EC areas saw an increase in part-time work in the higher income tracts, the effect is roughly double for the EZ tracts. The results using the triple-differencing strategy that accounts for partial-city trends, presented in columns 3 and 4, are very similar, if a bit larger.

These results are broadly consistent with the discussion of firm responses in Section 2.2. Areas where less workers are above the earnings cap are areas where firms are likely to face lower market wages for the types of jobs at their firm. Such firms are unlikely to be constrained by the earnings cap when making decisions about weekly hours. Therefore, they are more likely to either expand worker hours to full-time, or to hire new employees at full-time hours. As the constraint begins to bind for more workers, consistent with hourly wages being higher in the distribution, firms are less likely to hire at full-time and have a stronger incentive to substitute between hours and workers to maximize the wage credit. Thus, while Table 2 presents suggestive evidence of small increases in part-time work in EZ areas, the evidence in Table 3 demonstrates that firms adjust part-time work in both directions, depending on whether the earnings cap is binding.

4.3 Results by Gender

---

approximately 42 percent for retail workers, 31 percent for service workers, and 14 percent for all other workers, based on author's calculations from the 1990 and 2000 census).

While we cannot separate outcomes for workers who are eligible for the EZ wage credit from those ineligible, we do have some information about hours of different workers. One potentially interesting contrast is to compare responses by gender. There are several reasons to believe gender differences in part-time work could happen in this context. First, because of the gender wage gap, it may be that men are more likely be working at or above the earnings cap on the wage credit. Given the evidence in Table 3, that could mean that men are more likely to see expansions in part-time work. Second, women are more likely to choose to work part-time than are men. For example, 26 percent of employed women report working part-time for non-economic reasons in 2017 versus 10 percent of employed men.[21] This means men's part-time work is more likely than women's to be at the insistence of the employer. Therefore, if the wage subsidy is causing firms to shift to more part-time workers, then we would expect that it is easier to identify the effect in men.

To investigate gender differences, we use the CTPP data to calculate the share of men (women) working less than 35 hours a week. We then replicate our analyses from Table 2 using these gender-specific outcomes as the dependent variable. However, for simplicity, we focus only on the triple-difference estimates with controls and present these results in Table 3.[22] Overall, we find that the results pooling both men and women in Table 2 are driven by an increase in the number of men being employed in part-time work. This finding is robust to various controls, econometric approach, and fixed effect specification. OLS regressions produce results between 1.8 to 2.0 percentage point increases in part-time male workers, statistically significant at conventional levels, depending on the use of fixed effects and inclusion of different covariates. Results from propensity score matching produces the largest effects, around 4

[21] See https://www.bls.gov/cps/cpsaat08.htm .
[22] Unconditional and double-difference results are similar and available upon request.

percentage points, when only using 1990 controls. However, the estimate is reduced to approximately 1.5 percentage points when including both 1990 and 1980 controls. IV estimation produces estimates in the middle of the differencing and propensity score matching, with relatively stable results in the range of 2.8 and 3.5 percentage points increase with varying levels of statistical significance. Unlike the results for male part-time workers, the results for female part-time workers have coefficients that are close to zero, typically negative and are quite noisy. Overall, Table 3 provides evidence that there was a substantive change in the use of part-time workers, driven by hiring of men.*4.4 The Skill Mix of Workers*

To this point our results suggest that when firms are offered wage subsidies that are capped toward the lower part of the income distribution ($15,000) firms respond by increasing their reliance on part-time workers. As discussed in Section 2.2, firms could respond to the subsidy cap not by hiring low levels of hours, but by hiring more low-wage workers. If true, we would expect that the share of lower-skilled and lower-educated workers employed in the area to correspondingly increase. Therefore, we also investigate how the skill mix of work changes in the EZ areas.

The publically available CTPP data that provides place of work information at small geographic scale does not provide education information but does provide the occupational mix of the area. Therefore, we use the distribution of education of each occupation group to impute the changes in the area's share of workers across education levels. The CTPP groups workers into 12 occupation groups, which we use to generate a proxy for educational attainment. Using 1990 and 2000 Census data from IPUMS, we calculate the nationwide rates of educational attainment of workers aged 16 and over (the same universe as the CTPP) for the 12 occupation groups in the CTPP. We combine that with the CTPP's occupational counts to calculate the weighted average of occupations' educational attainment rate, using occupations' shares of employment in the tract as the weight. This yields the expected education shares of workers in the tract. While not perfect, we believe this imputed measure of educational attainment has value as a measure of skill mix. As a robustness check, we also estimated changes in the proportion of workers in different occupational groups.[23]

Overall, Table 5 shows a statistically and economically meaningful increase in the share of workers with less than a high school education, no matter the controls, method or use of city fixed effects. OLS regressions find an increase in workers without a high school diploma of between 0.8 to 1.1 percentage points. Again, propensity score matching produces similar results while the IV results tend to have slightly higher point estimates, but lower precision. If it is true that firms are adjusting their reliance on less educated workers, then we should see a reciprocating effect on the share of workers with a college degree or more. Table 6 shows that the share of workers employed in the zone with a bachelor's degree or more fell by 2.1 to 2.9 percentage points in OLS regressions and 2.3 to 3.5 percentage points using propensity score matching. The IV method produces a more mixed picture with some of the largest negative effects of 4 to 5 percentage point decreases but some null estimates as well.

Taken together, Tables 5 and 6 provide evidence that firms are utilizing workers with less education in response to a capped-wage subsidy. Note that our results on the education level of workers *employed* in the EZ area stands in contrast to the EZ evaluation literature that finds an increase in the educational attainment of individuals who *reside* in the zone. Thus, our estimates

[23] In theory, we could also test whether there is an increase in workers making earnings below $15,000. However, as previously discussed, we cannot identify earnings at a particular job and workers can respond by taking on multiple jobs. The results of this test, presented in Appendix Table A-4, show some evidence of an increase in workers earning less than $15,000 but the results are somewhat sensitive to estimation strategy.

suggest that firms are shifting towards lower-skilled workers.[24] We find similar evidence of a shift toward lower-skilled and lower-wage occupations by examining changes in the distribution of specific occupations in the EZ areas. We find evidence of a decrease in the proportion of professional occupations and an increase in the proportion of production and transportation workers (Appendix Table A-5). Together, we take the evidence of changes in the skill-mix of workers as further evidence that the structure of the wage subsidy induced behavioral shifts in hiring by firms.

7. Conclusion

This paper contributes to the hiring wage credit literature in two ways. First, we show that a capped-wage subsidy creates an incentive for firms to use part-time or low-wage workers in order to get the maximum subsidy possible. Second, we use federal Empowerment Zones (EZs) to test whether firms increase their use of part-time workers in response to a capped 20 percent wage tax credit. EZs are a good way to test this because the combination of rejected applicant areas and the literature evaluating the EZ program allow us to construct good comparisons for the EZ tracts and the EZ wage credit is relatively large, long-lived, and less prone to stigma effects than other hiring tax credits. As far as we know, this is the first paper to test the economic incentives of the wage subsidy in the EZ program, as the exiting EZ literature has focused on evaluating the program as a tool for local economic development.

We find evidence of modest increases in the use of part-time workers in EZ areas from 1990 to 2000, approximately equal to 1 percentage point or a 5 percent increase relative to the

[24] One interpretion of our findings is that they provide potential support for recent work (Reynolds and Rohlin, 2015) that the EZ program induced residential gentrification in the area.

baseline. However, that average effect masks important heterogeneity. We find that EZ tracts where the $15,000 annual earnings cap is least likely to bind saw a relative decrease in part-time work while the EZ tracts where the cap ismore likely to bind have a large increase in part-time . From this we conclude that, at least for the EZ program, the capped wage subsidy led to a shift to more part-time employment. Our results are supported by estimates showing that the change in part-time work is concentrated among men, who are more likely than women to work part-time because of economic reasons, such as not being offered full-time work. Complementary results show that firms in EZs shift towards lower-skilled workers – another way to stay under the earnings cap and maximize the subsidy received.

While inducing firms to shift to using part-time workers is an economic distortion from the capped wage subsidy, it is unclear how policymakers would view this distortion. Distributing the employment benefits over a larger number of workers may be in line policy goals, even if it means fewer hours per employed worker. Encouraging firms to hire lower-skilled workers, who often have more limited job prospects than higher-skilled workers, may also be consistent with policymaker goals.

If policymakers do not want to induce firms to shift to part-time workers they could cap the credit by hourly wage instead of by total earnings. If structured this way, the credit would be the same for each marginal hour of employment, eliminating the incentive to use part-time workers. However, the incentive to use lower-waged workers would remain because wages beyond the hourly wage cap would not increase the amount of wage subsidy received. To give a specific example, suppose that there is a 20 percent wage credit applied to the first $10 of each eligible worker's hourly wage. Then the credit would be 20 percent for all wages up to $10 per hour and less than 20 percent for all wages beyond $10 per hour. In this way the credit could be targeted to lower-wage workers while not inducing part-time employment.

References

Bartik, Timothy J. and John H. Bishop, 2009. “The Job Creation Tax Credit: Dismal Projections for Employment Call for a Quick, Efficient and Effective Response.” Economic Policy Institute Briefing Paper No. 248. Washington D.C.: Economic Policy Institute.

Bishop, John H. and Mark Montgomery, 1993. “Does the Targeted Jobs Tax Credit Create Jobs at Subsidized Firms?” *Industrial Relations*, 32(3), 289-306.

Boockmann, Bernhard, Thomas Zwick, Andreas Ammermuller, and Micael Maier, 2012. “Do Hiring Subsidies Reduce Unemployment Among Older Workers? Evidence from Natural Experiments.” *Journal of the European Economic Association*, 10(4), 735-764.

Burtless, Gary, 1985. “Are Targeted Wage Subsidies Harmful? Evidence from a Wage Voucher Experiment.” *ILR Review*, 39(1), 105-114.

Busso, Matias, John DiNardo and Justin McCrary, 2014. “New Evidence on the Finite Sample Properties of Propensity Score Reweighting and Matching Estimators.” Review of Economics and Statistics, 96(5), 885-897.

Busso, Matias, Gregory, Jesse, Kline, Patrick, 2013. Assessing the Incidence and Efficiency of a Prominent Place-based Policy.” *American Economic Review* 103 (2), 897–947.

Costa, Dora L. 2000. “Hours of Work and the Fair Labor Standards Act: A Study of Retail and Wholesale Trade, 1938-1950.” *ILR Review*, 53(4), 648-664.

Even, William E. and David A. Macpherson, forthcoming. “The Affordable Care Act and the Growth of Involuntary Part-time Employment.” *ILR Review*.

Faulk, Dagney, 2002. “Do State Economic Development Incentives Create Jobs? An Analysis of State Employment Tax Credits.” *National Tax Journal*, 55(2), 263-280.

Hamersma, Sarah, 2008. “The Effects of an Employer Subsidy on Employment Outcomes: A Study of the Work Opportunity and Welfare-to-Work Tax Credits.” *Journal of Policy Analysis and Management*, 27(3), 498-520.

Hamersma, Sarah, 2011. “Why Don’t Eligible Firms Claim Hiring Subsidies? The Role of Job Duration.” *Economic Inquiry*, 49(3), 916-934.

Hamersma, Sarah and Carolyn Heinrich, 2008. “Temporary Help Service Firms’ Use of Employer Tax Credits: Implications for Disadvantaged Workers’ Labor Market Outcomes.” *Southern Economic Journal*, 74(4), 1123-1148.

Hamermesh, Daniel and Stephen Trejo, 2000. “The Demand for Hours of Labor: Direct Evidence from California.” *Review of Economics and Statistics*, 82(1), 38-47.

Hanson, Andrew, 2009. “Local Employment, Poverty, and Property Value Effects of Geographically-targeted Tax Incentives: an Instrumental Variables Approach”. *Regional Science and Urban Economics*, 39, 721–731.

Hanson, Andrew, 2011. “Utilization of Employment Tax Credits: An Analysis of the ‘Empowerment Zone’ Wage Tax Credit.” *Journal of Public Budgeting and Finance*, 31(1), 23–36.

Hanson, Andrew, Rohlin, Shawn, 2011a. “Do location-based tax incentives attract new business establishments? *Journal of Regional Science*, 51 (3), 427–449.

Hanson, Andrew, Rohlin, Shawn, 2011b. “The effect of location-based tax incentives on establishment location and employment across industry sectors,” *Public Finance Review*, 39(2), 195–225.

Hanson, Andrew, Rohlin, Shawn, 2013. “Do spatially targeted redevelopment programs spill-over?” *Regional Science and Urban Economics* 43 (1), 86–100.

Jaenichen, Ursula, and Gesine Stephan, 2011. “The Effectiveness of Targeted Wage Subsidies for Hard-to-place Workers.” *Applied Economics*, 43(10), 1209-1225.

Kaldor, Nicholas, 1936, "Wage Subsidies as a Remedy for Unemployment," *Journal of Political Economy*, 44(6), 721-742.

Katz, Lawrence F., 1998. “Wage Subsidies for the Disadvantaged.” in: Richard B. Freeman and Pere Gottschalk (eds) *Generating Jobs: How to Increase Demand for Less-Skilled Workers*, Russell Sage Foundation: New York.

Lorenz, Edward C., 1995. “TJTC and the Promise and Reality of Redistributive Vouchering and Tax Credit Policy.” *Journal of Policy Analysis and Management*, 14(2), 270-290.

Neumark, David, 2013. “Spurring Job Creation in Response to Severe Recessions: Reconsidering Hiring Credits.” *Journal of Policy Analysis and Management*, 32(1), 142-171.

Neumark, David and Diego Grijalva, 2016. “The Employment Effects of State Hiring Credits.” *ILR Review*, 70(5), 1111-1145.

Neumark, David, and Helen Simpson. 2015, ‘Place-based policies’. in: Duranton Gilles, Henderson JV & Strange William (eds) *The Handbook of Regional and Urban Economics*. Elsevier/Academic Press: Amsterdam

Reynolds, C. Lockwood, Rohlin, Shawn, 2014. Do location-based tax incentives improve quality of life and quality of business environment? *Journal of Regional Science* 54 (1), 1–32.

Reynolds, C. Lockwood, Rohlin, Shawn, 2015. The effects of location-based tax policies on the distribution of household income: Evidence from the federal Empowerment Zone program? *Journal of Urban Economics* 88, 1–15.

Steven Ruggles, Katie Genadek, Ronald Goeken, Josiah Grover, and Matthew Sobek. Integrated Public Use Microdata Series: Version 6.0 [dataset]. Minneapolis: University of Minnesota, 2015. http://doi.org/10.18128/D010.V6.0.

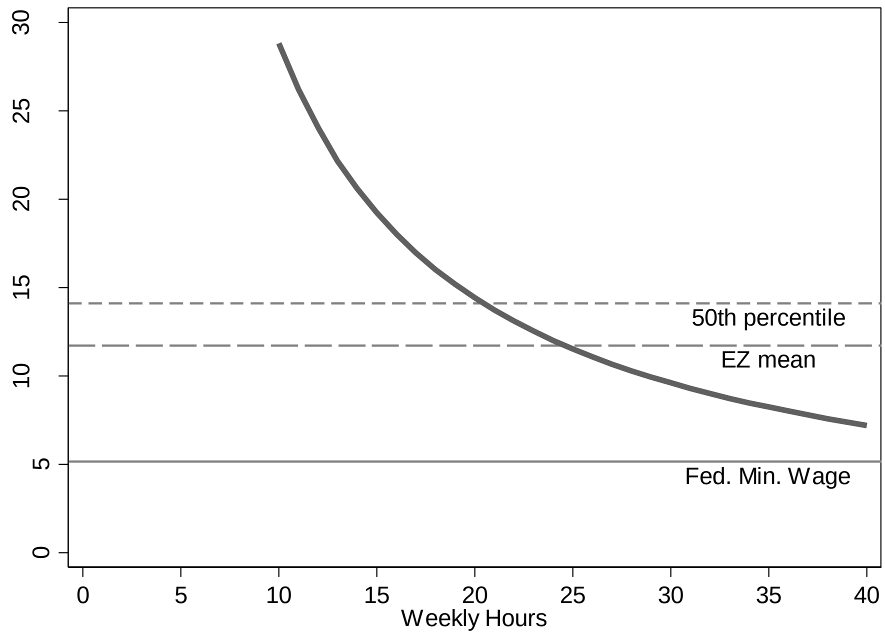
U.S. Development of Housing and Urban Development. 2001. "Interim Assessment of the Empowerment Zones and Enterprise Communities Program: A Progress Report." Washington, D.C.

U.S Government Accountability Office. 2001. "Work Opportunity Tax Credit: Employers do not Appear to Dismiss Employees to Increase Tax Credits." GAO-01-329, Washington D.C.

U.S. Government Accountability Office. 2004. "Federal Revitalization Programs Are Being Implemented, but Data on the Use of Tax Benefits Are Limited: Report to Congressional Committees." GAO–04–306, Washington, D.C.

Wallace, M., 2004. Congressional Considerations and Urban Characteristics in the Selection of Empowerment Zones and Enterprise Communities. *Journal of Urban Affairs*, 26(5): 593-609.

Figure 1: Earnings Cap of the Federal Empowerment Zone Wage Credit by Weekly Hours and Hourly Wage, 2000



Notes:
1) The curve represents the combination of weekly hours and hourly wages at which firms would reach the earnings cap of $15,000 if they employed workers for 52 weeks a year.
2) The federal minimum wage in 2000 was $5.15. The median wage in 2000 among workers 16-64 years old was $14.12 (authors' calculations from IPUMS data). The average wage in the EZ areas is calculated from Summary 3 tract data of the aggregate earnings of area residents combined with the distribution of weeks worked and usual hours per week worked.

Figure 2: Maps of the New York (left) and Chicago (right) Empowerment Zone Program

Table 1: Summary Statistics

Variable	EZ	EC	EZ - EC
Poverty rate, 1990	0.465	0.409	0.056***
Unemployment rate, 1990	0.227	0.169	0.059***
Percent non-white, 1990	0.843	0.683	0.161***
Percent BA or higher, 1990	0.065	0.081	-0.015***
Median income ($10,000), 1990	18.748	21.402	-2.654***
Home ownership rate, 1990	0.192	0.316	-0.124***
Median house value ($10,000), 1990	45.617	70.484	-24.868***
Percent female-headed households, 1990	0.637	0.543	0.094***
Percent welfare, 1990	0.353	0.260	0.092***
Average housing age, 1990	38.369	37.009	1.360
Average housing age squared, 1990	1598.226	1455.462	142.763***
Poverty rate, 1980	0.407	0.337	0.070***
Unemployment rate, 1980	0.176	0.126	0.050***
Percent non-white, 1980	0.803	0.635	0.168***
Percent BA or higher, 1980	0.041	0.063	-0.022***
Median income ($10,000), 1980	19.034	22.269	-3.325***
Home ownership rate, 1980	0.185	0.329	-0.144***
Median house value, 1980			
Percent female-headed households, 1980	0.558	0.480	0.078***
Percent welfare, 1980	0.339	0.244	0.095***
Average housing age, 1980	34.668	32.538	2.130***
Average housing age squared, 1980	1309.31	1121.080	188.23***
Worker characteristics			
Percent workers with hours less than 35, 1990	0.180	0.192	-0.012**
Percent workers with hours less than 35 , 2000	0.160	0.158	0.002
Percent workers with earnings less than $15000, 1990	0.336	0.379	-0.043***
Percent workers with earnings less than $15000, 2000	0.232	0.244	-0.012*
N	273	942	

Notes:
1) All data are tabulated at the Census tract level. Area characteristics come from Summary 3 files while worker characteristics come from Census Transportation Planning Package (CTPP) files.
2) Asterisks denote statistical significance at the 10% (*), 5% (**) and 1% (***) levels.

Table 2: The Effect of Capped-waged Subsidies on Firms' Reliance on Part-time Workers.

Dependent Variable: Percent of workers with less than 35 hours per week

		Double-difference	
	Naïve	1990 Controls	1980 and 1990 Controls
OLS	0.014**	0.014***	0.011**
	(0.006)	(0.005)	(0.005)
Propensity Score Matching	-	0.015	0.009
	-	(0.009)	(0.010)
Instrumental Variables	0.028	0.015	0.010
	(0.017)	(0.020)	(0.022)

		Triple-difference: city-wide	
	Naïve	1990 Controls	1980 and 1990 Controls
OLS using Differencing	0.010**	0.012**	0.010*
	(0.005)	(0.004)	(0.005)
Propensity Score Matching	-	0.012	0.008
	-	(0.009)	(0.010)
Instrumental Variables	0.020	0.009	0.006
	(0.015)	(0.021)	(0.023)

		Triple-difference: matched sub-city	
	Naïve	1990 Controls	1980 and 1990 Controls
OLS using Differencing	0.007*	0.007*	0.006
	(0.004)	(0.004)	(0.004)
Propensity Score Matching	-	0.008	0.004
	-	(0.009)	(0.010)
Instrumental Variables	0.011	0.003	0.005
	(0.007)	(0.012)	(0.013)

Notes:
1) The outcome is the change from 1990 to 2000 and the policy was designated in 1994.
2) Each point estimate represents the effect of the federal EZ capped-wage subsidy from a different specification.
3) Standard errors clustered at the city-level are presenting below OLS and IV estimates. Standard deviations from 1000 block-bootstrapped replications are presented below propensity score matching estimates. Asterisks denote statistical significance at the 10% (*), 5% (**) and 1% (***) levels.

Table 3: Estimates of the Effect on Firm's Reliance on Part-time Workers by Portion of Tract Workers Earning Above the Annual Wage-salary Cap ($15,000)

Dependent Variable: Percent of workers with less than 35 hours per week

	Triple-difference: city-wide		Triple-difference: matched sub-city	
EZ	-0.027*	-0.028*	-0.030**	-0.031*
	(0.015)	(0.016)	(0.014)	(0.016)
EZ * I(50-75% above cap)	0.036***	0.039***	0.034***	0.036***
	(0.013)	(0.013)	(0.013)	(0.013)
EZ * I(>75% above cap)	0.058***	0.057**	0.062***	0.060***
	(0.023)	(0.023)	(0.023)	(0.022)
I(50-75% above cap)	0.041***	0.041***	0.041***	0.040***
	(0.010)	(0.009)	(0.010)	(0.011)
I(>75% above cap)	0.058**	0.053***	0.047***	0.047***
	(0.023)	(0.010)	(0.011)	(0.011)
1990 controls	Y	Y	Y	Y
1980 controls		Y		Y

Notes:
1) The outcome is the change from 1990 to 2000 and the policy was designated in 1994.
2) I(50-75% above cap) is an indicator that between 50 and 75% of workers in 1990 in a tract earned more than the annual cap on the wage subsidy. Similarly, I(>75% above cap) indicates that more than 75% of workers in 1990 in a tract earned more than the annual cap on the wage subsidy. The left out category are tracts where less than 50% of workers in 1990 earned above the annual cap on the wage subsidy.
3) All estimates are from OLS regressions and standard errors are clustered at the city level. Asterisks denote statistical significance at the 10% (*), 5% (**) and 1% (***) levels.

Table 4: The Effect of Capped-waged Subsidies on Firms' Reliance on Part-time Workers by Gender

Dependent Variable: Percent of workers with less than 35 hours per week

	Triple-difference: city-wide			
	Males		Females	
	1990 Controls	1980 and 1990 Controls	1990 Controls	1980 and 1990 Controls
OLS	0.018***	0.020***	0.002	-0.004
	(0.006)	(0.005)	(0.009)	(0.009)
Propensity Score Matching	0.043*	0.015	-0.004	-0.000
	(0.023)	(0.013)	(0.019)	(0.016)
Instrumental Variables	0.029	0.033	-0.028	-0.037
	(0.021)	(0.024)	(0.027)	(0.027)

	Triple-difference: matched sub-city			
	Males		Females	
	1990 Controls	1980 and 1990 Controls	1990 Controls	1980 and 1990 Controls
OLS	0.017**	0.018***	-0.004	-0.009
	(0.007)	(0.005)	(0.007)	(0.008)
Propensity Score Matching	0.041*	0.015	-0.009	-0.005
	(0.023)	(0.013)	(0.019)	(0.016)
Instrumental Variables	0.028	0.035	-0.029	-0.029
	(0.019)	(0.024)	(0.020)	(0.024)

Notes:
1) The outcome is the change from 1990 to 2000 and the policy was designated in 1994.
2) Each point estimate represents the effect of the federal EZ capped-wage subsidy from a different specification.
3) Standard errors clustered at the city-level are presenting below OLS and IV estimates. Standard deviations from 1000 block-bootstrapped replications are presented below propensity score matching estimates. Asterisks denote statistical significance at the 10% (*), 5% (**) and 1% (***) levels.

Table 5: The Effect of Capped-waged Subsidies on Firms' Reliance on Workers with Less Than a High School Education.

Dependent Variable: Percent of workers with less than a high school education.

	Triple-difference: city-wide	
	1990 Controls	1980 and 1990 Controls
OLS	0.008***	0.006**
	(0.002)	(0.002)
Propensity Score Matching	0.009***	0.005**
	(0.003)	(0.003)
Instrumental Variables	0.017*	0.017
	(0.009)	(0.011)

	Triple-difference: matched sub-city	
	1990 Controls	1980 and 1990 Controls
OLS	0.011***	0.009***
	(0.003)	(0.003)
Propensity Score Matching	0.011***	0.009***
	(0.003)	(0.003)
Instrumental Variables	0.013	0.012
	(0.013)	(0.012)

Notes:

1) The outcome is the change from 1990 to 2000 and the policy was designated in 1994.

2) Each point estimate represents the effect of the federal EZ capped-wage subsidy from a different specification.

3) Standard errors clustered at the city-level are presenting below OLS and IV estimates. Standard deviations from 1000 block-bootstrapped replications are presented below propensity score matching estimates. Asterisks denote statistical significance at the 10% (*), 5% (**) and 1% (***) levels.

Table 6: The Effect of Capped-waged Subsidies on Firms' Reliance on Workers with a Bachelor's Degree or Higher.

Dependent Variable: Percent of workers with a bachelor's degree or higher.

	Triple-difference: city-wide	
	1990 Controls	1980 and 1990 Controls
OLS	-0.029***	-0.023***
	(0.009)	(0.008)
Propensity Score Matching	-0.035***	-0.026**
	(0.013)	(0.013)
Instrumental Variables	-0.053**	-0.047
	(0.026)	(0.031)

	Triple-difference: matched sub-city	
	1990 Controls	1980 and 1990 Controls
OLS	-0.026**	-0.021**
	(0.010)	(0.009)
Propensity Score Matching	-0.029**	-0.023*
	(0.013)	(0.013)
Instrumental Variables	-0.008	0.001
	(0.054)	(0.048)

Notes:
1) The outcome is the change from 1990 to 2000 and the policy was designated in 1994.
2) Each point estimate represents the effect of the federal EZ capped-wage subsidy from a different specification.
3) Standard errors clustered at the city-level are presenting below OLS and IV estimates. Standard deviations from 1000 block-bootstrapped replications are presented below propensity score matching estimates. Asterisks denote statistical significance at the 10% (*), 5% (**) and 1% (***) levels.

Table A-1: Balancing Tests For the Propensity Score Method

	Unmatched		Matched		Matched	
Variable	EZ	EC	EZ	EC	EZ	EC
Poverty rate, 1990	0.465	0.409***	0.457	0.472	0.450	0.460
Unemployment rate, 1990	0.227	0.169***	0.223	0.228	0.226	0.220
Percent non-white, 1990	0.843	0.683***	0.836	0.849	0.806	0.810
Percent BA or higher, 1990	0.065	0.081***	0.067	0.067	0.064	0.065
Median income ($10,000), 1990	18.748	21.402***	18.963	18.805	17.986	19.369
Home ownership rate, 1990	0.192	0.316***	0.203	0.215	0.178	0.189
Median house value ($10,000), 1990	45.617	70.484***	48.158	47.108	5.30E+04	4.50E+04
Percent female-headed households, 1990	0.637	0.543***	0.635	0.653	0.62	0.585
Percent welfare, 1990	0.353	0.260***	0.348	0.343	0.348	0.335
Average house age, 1990	38.369	37.009	38.676	39.597	37.993	37.867
Average house age squared, 1990	1598.226	1455.462***	1608.662	1666.56	1599.717	1581.503
Poverty rate, 1980	0.407	0.337***			0.394	0.412
Unemployment rate, 1980	0.176	0.126***			0.180	0.173
Percent non-white, 1980	0.803	0.635***			0.773	0.756
Percent BA or higher, 1980	0.041	0.063***			0.037	0.051**
Median income ($10,000), 1980	19.034	22.269***			18.795	18.801
Home ownership rate, 1980	0.185	0.329***			0.170	0.189
Median house value, 1980	0	0.000***			0.000	0.000***
Percent female-headed households, 1980	0.558	0.480***			0.554	0.522
Percent welfare, 1980	0.339	0.244***			0.339	0.317
Average house age, 1980	34.668	32.538***			34.623	34.276
Average house age squared, 1980	1309.31	1121.080***			1323.667	1292.385
Hotelling test (F-stat.)		25.365***		0.829		0.743
(p-value)		(0.000)		(0.611)		(0.790)

Notes:
1) Table presents asterisks representing statistical significance from pair-wise t-tests of differing means between EZ and EC areas for each covariate. The Hotelling test is a test of whether there is an overall difference in means across covariates between EZ and EC areas. Asterisks denote statistical significance at the 10% (*), 5% (**) and 1% (***) levels.

Table A-2: First-stage Regression for Instrumental Variable Estimation

	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Ways and Means Member	0.145***		-0.271***	-0.144***	-0.093	-0.093
	(0.035)		(0.054)	(0.054)	(0.058)	(0.214)
Terms on Committee		0.024***	0.047***	0.031***	0.027***	0.027
		(0.003)	(0.005)	(0.005)	(0.005)	(0.021)
1990 controls				Y	Y	Y
1980 controls					Y	Y
Clustered standard errors						Y
R^2	0.017	0.048	0.062	0.247	0.342	0.339
F-stat	16.99***	49.44***	46.97***	32.23***	30.28***	1.45
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.241)

Notes:
1) Standard errors are presented in parentheses below point estimates. P-values are presented in parentheses below F-statistics. Asterisks denote statistical significance at the 10% (*), 5% (**) and 1% (***) levels.

Table A-3: The Effect of Capped-waged Subsidies on Firms' Reliance on Part-time Workers.
(Weighting by share of employees who live in the EZ or EC)

Dep. Var: % of Workers with less than 35 hours per week	Without City Fixed Effects		
	Naïve	1990 Controls	1980 and 1990 Controls
OLS using Differencing	0.011*	0.017***	0.015**
	(0.006)	(0.006)	(0.007)
Propensity Score Matching		0.018	0.008
		(0.011)	(0.012)
Instrumental Variables	0.009	0.008	0.009
	(0.024)	(0.019)	(0.021)

Dep. Var: % of Workers with less than 35 hours per week	With City Fixed Effects		
	Naïve	1990 Controls	1980 and 1990 Controls
OLS using Differencing	0.009*	0.016***	0.015**
	(0.005)	(0.005)	(0.007)
Propensity Score Matching	-	0.015	0.007
	-	(0.011)	(0.012)
Instrumental Variables	0.020	0.017	0.020
	(0.021)	(0.025)	(0.034)

Dep. Var: % of Workers with less than 35 hours per week	With Matched Sub-city Fixed Effects		
	Naïve	1990 Controls	1980 and 1990 Controls
OLS using Differencing	0.007	0.013**	0.012
	(0.004)	(0.006)	(0.007)
Propensity Score Matching	-	0.012	0.004
	-	(0.011)	(0.011)
Instrumental Variables	0.002	-0.007	-0.006
	(0.011)	(0.014)	(0.013)

Notes:
1) The outcome is the change from 1990 to 2000 and the policy was designated in 1994.
2) Each point estimate represents the effect of the federal EZ capped-wage subsidy from a different specification.
3) Standard errors clustered at the city-level are presenting below OLS and IV estimates. Standard deviations from 1000 block-bootstrapped replications are presented below propensity score matching estimates. Asterisks denote statistical significance at the 10% (*), 5% (**) and 1% (***) levels.

Table A-4: The Effect of Capped-waged Subsidies on Firms' Reliance on Workers earning less than $15,000.

Dependent Variable: Percent of Workers with earning less than $15,000.

	Without City Fixed Effects		
	Naïve	1990 Controls	1980 and 1990 Controls
OLS using Differencing	0.032**	0.029**	0.021
	(0.013)	(0.013)	(0.015)
Propensity Score Matching	-	0.031**	0.018
	-	(0.012)	(0.012)
Instrumental Variables	0.019	-0.008	-0.009
	(0.050)	(0.056)	(0.056)
	With City Fixed Effects		
	Naïve	1990 Controls	1980 and 1990 Controls
OLS using Differencing	-0.000	0.007	0.002
	(0.007)	(0.007)	(0.009)
Propensity Score Matching	-	0.006	-0.004
	-	(0.011)	(0.012)
Instrumental Variables	-0.041	-0.046	-0.040
	(0.025)	(0.030)	(0.0333)
	With Matched Sub-city Fixed Effects		
	Naïve	1990 Controls	1980 and 1990 Controls
OLS using Differencing	0.006	0.015*	0.011
	(0.009)	(0.009)	(0.010)
Propensity Score Matching	-	0.015	0.004
	-	(0.011)	(0.011)
Instrumental Variables	-0.003	0.004	0.011
	(0.018)	(0.023)	(0.027)

Notes:
1) The outcome is the change from 1990 to 2000 and the policy was designated in 1994.
2) Each point estimate represents the effect of the federal EZ capped-wage subsidy from a different specification.
3) Standard errors clustered at the city-level are presenting below OLS and IV estimates. Standard deviations from 1000 block-bootstrapped replications are presented below propensity score matching estimates. Asterisks denote statistical significance at the 10% (*), 5% (**) and 1% (***) levels.

Table A-5: Estimates of the Effect of Capped-waged Subsidies on Proportion of Workers in Selected Occupations, Including 1980 and 1990 Controls

	Triple-difference: city-wide					
	Management	Professional	Technicians	Craftsmen	Production	Transportation
OLS using Differencing	-0.001	-0.022***	0.001	-0.001	0.012	0.007
	(0.005)	(0.007)	(0.004)	(0.004)	(0.010)	(0.006)
Propensity Score Matching	0.004	-0.022*	0.002	-0.003	0.015**	0.006
	(0.007)	(0.014)	(0.005)	(0.009)	(0.007)	(0.008)
Instrumental Variables	0.008	-0.031	-0.017	0.038	0.008	0.029
	(0.012)	(0.030)	(0.027)	(0.030)	(0.031)	(0.025)
	Triple-difference: matched sub-city					
	Management	Professional	Technicians	Craftsmen	Production	Transportation
OLS using Differencing	-0.009	-0.029**	-0.005	0.004	0.013	0.016**
	(0.005)	(0.013)	(0.004)	(0.008)	(0.008)	(0.006)
Propensity Score Matching	-0.004	-0.027**	-0.004	-0.000	0.015**	0.015**
	(0.007)	(0.014)	(0.005)	(0.010)	(0.007)	(0.008)
Instrumental Variables	-0.026	0.006	-0.048	0.019	0.012	0.017
	(0.018)	(0.067)	(0.042)	(0.020)	(0.016)	(0.037)

Notes:
1) The outcome is the change from 1990 to 2000 and the policy was designated in 1994.
2) Each point estimate represents the effect of the federal EZ capped-wage subsidy from a different specification.
3) Standard errors clustered at the city-level are presenting below OLS and IV estimates. Standard deviations from 1000 block-bootstrapped replications are presented below propensity score matching estimates. Asterisks denote statistical significance at the 10% (*), 5% (**) and 1% (***) levels.