Firm-Level Incidence of Earned Income Tax Credits: Evidence from Italy

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November 6, 2024

Abstract

In this paper, I investigate the role of firms in the final incidence of Earned Income Tax Credits. Using administrative data and taking advantage of the introduction of a large, employer-administered EITC program in Italy, I study firm-level responses to the tax credit by exposure to the program. I find that the earnings of eligible workers in more exposed firms decrease compared to comparable less exposed firms, with highly exposed firms capturing around 30% more of the transfer through reduced wage rates. I find significant spillover effects of the reform, with the earnings of similar non-eligible workers in more exposed firms decreasing compared to less exposed firms. The results are consistent with a monopsonistic labor market and with rent-seeking behavior by firms. The analysis highlights the main trade-offs between employer and government-administered EITC for the design of these programs.

JEL: J13, J16, J22, H31, H53, I38.

Keywords: Earned Income Tax Credits, Tax Incidence, Firm Responses

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1 Introduction

A crucial question in the design of welfare programs is who bears their economic incidence. While policy makers design programs with a clear target of beneficiaries in mind, unintended consequences of policies might lead the economic incidence to be different from the statutory incidence. This is likely to be particularly important for government transfers in the form of wage subsidies, such as Earned Income Tax Credits (EITCs).

While the literature has thoroughly investigated the employment effects of Earned Income Tax Credits (see Hoynes, 2019, for a review), still relatively little is known about the effects of these transfers on wages. Estimating the wage effects of EITCs comes with several challenges. First, it requires a research design comparing two labor markets with different EITC-induced labor supply shocks but similar enough to attribute wage changes to the EITC and not to other determinants (Rothstein, 2008). Such settings are rare. Second, the standard approach in public finance to study incidence has focused on the standard competitive framework where the relative market-level elasticities of supply and demand determine the final incidence of EITCs. This approach assumes that firms passively accept market-level wages and implies that the final incidence of a tax credit is homogeneous across firms and workers. This assumption may not hold in practice as individual firms are likely to have a key role in wage determination and wage setting (Card, Heining, and Kline, 2013; Card et al., 2018). The relevance of firm-level channels is likely to grow over time, with employer-mediated transfers becoming more and more widespread[1]. Understanding how firms react to EITCs and how responses vary across firms becomes crucial to analyzing their incidence and welfare effects.

In this paper, I study how the impact and final incidence of EITC programs vary across firms, focusing on the role of firms' wage-setting practices. I tackle these questions by taking advantage of the introduction, in May 2014, of a large, employer-mediated EITC program in Italy, the so-called *80 Euros Bonus*. The program's objective was to stimulate consumption; its introduction was unexpected and plausibly motivated by electoral reasons[2]. Nevertheless, it represented a significant welfare reform and resulted in an immediate €80 ($90) increase in the monthly salary of eligible workers, which translated into a €960 ($1000) increase in their annual earnings. The tax credit was distributed to all employees with annual gross earnings between €8,000 and €26,000,

[1]Some recent examples of employer-mediated EITC programs are: the Advanced Earned Income Tax Credit in the US, the Working Family Tax Credit in the UK, Familienzulagen in Switzerland, Asignaciones Familiares in Argentina, Salário Família in Brazil.

[2]The program was introduced in April 2014, just a month before the European Parliament election.

regardless of any other personal or family characteristics. Notably, employers played a key role in the administration of the tax credit. They determined the eligibility of employees and distributed the tax credit monthly directly in the paychecks of workers. This setup created a large, employer-mediated wage subsidy with full automatic take-up, offering a distinctive setting to investigate firm-level responses to wage subsidies.

To illustrate the crucial role firms could play in the final incidence of the tax credit, I first illustrate how the characteristics of the program can translate into differences in responses at the firm level. I show how different levels of exposure to the program—in terms of firm-level share of pre-reform eligible employees—can drive different firm-level responses. While I argue that different mechanisms can rationalize the presence of firm-level wage effects by exposure to the program, I focus on two main channels: higher opportunities for rent-seeking behavior, by exploiting employees' perception of the transfer as part of their compensation package, and higher administration costs.

Empirically, I test whether firms with a higher concentration of eligible workers pre-reform respond differently to the introduction of the program relative to firms with a lower concentration of eligible workers. I compare the evolution of their outcomes within a difference-in-differences framework, following an empirical strategy used in the minimum wage literature (Draca, Machin, and Van Reenen, 2011; Harasztosi and Lindner, 2019). Key to the empirical strategy are the persistence in exposure across years within firms and the comparability of firms with different firm-level exposure. I evaluate the effects of the introduction of the program using administrative matched employer-employee data from the Italian Social Security Institute (*Istituto Nazionale di Previdenza Sociale* or *INPS*). I have access to a random sample that covers 7% of all salaried employees working in the private sector from 1985 to 2019. The data contain detailed information on annual labor earnings (before taxes and transfers), occupation and type of contract, worker demographics, and firm characteristics. Using these data, I measure firm-level exposure using the pre-reform (2013) firm-level share of eligible employees. In my baseline specification, I compare firms in the top quartile of the pre-reform distribution of the share of eligible employees (*high exposure*) to firms whose pre-reform share of eligible employees is between the 50th and the 75th percentile (*medium-high exposure*).

My main finding shows that the earnings of eligible workers in firms highly exposed to the reform significantly decrease relative to those in firms with lower exposure to the reform. I find that annual earnings of eligible workers decrease by around 2% in highly exposed firms compared

to lower exposed firms. Considering the average annual earnings of eligible employees in high exposure firms, the effect translates into a monetary loss of around €272. These estimates are compatible with a pass-through to employers of around 30%, consistent with the scarce literature on wage effects of EITCs (Rothstein, 2010; Azmat, 2019; Gravoueille, 2024). Decomposing the earnings results, I find that the effect is driven by wages rather than by labor supply responses. The results are robust to different robustness checks.

Using a similar approach to the one of Cengiz et al. (2019), I study the effect of the introduction of the tax credit throughout the earnings distribution and find that the decline in earnings happens across the board for all eligible workers, indicating that the main result stems from a broad reduction in earnings rather than being concentrated within specific categories of eligible workers. Notably, the decrease is not concentrated around the €24,000 kink, as would be expected if the results were driven by workers' behavioral responses to the tax credit schedule. The decline also appears among similar non-eligible workers, suggesting possible spillover effects.

In the second part of the empirical analysis, I therefore examine the potential spillover effects of the reform on non-eligible workers. Understanding the nature of these potential spillover effects on non-eligible workers is important and can help shed light on the potential mechanisms behind the observed responses. I find that earnings of non-eligible workers after the reform decrease in more exposed firms compared to less exposed firms after the reform. Notably, these spillover effects occur only for non-eligible workers who are similar to eligible workers—that is, those earning not too much above the eligibility cutoff. Additionally, these effects do not appear to be driven by non-eligible employees becoming eligible after the reform.

The final piece of evidence needed to fully understand the mechanisms behind these effects is the impact of the program on firm-level employment. I find that, after the reform, firm-level employment increases gradually in highly exposed firms relative to less exposed firms. The increase is mostly driven by an increase in the number of eligible workers. The effect on the observed number of non-eligible workers is negative but not statistically significant.

The empirical results show that earnings of eligible employees after the reform decrease in firms more exposed to the program relative to firms less exposed to the policy. On average, employers in more exposed firms capture up to 30% more of the transfer. The results are consistent with a labor market characterized by a degree of monopsonistic competition and with rent-seeking behavior by firms, where internal pay equity and labor demand substitution between eligible and similar non-eligible workers could explain the observed effects. In the last part of the empirical

analysis, I present some heterogeneity analysis with the aim of shedding light on the mechanisms behind the results. I find that the negative effects on earnings are driven by incumbents rather than new hires. The result is in line with existing evidence from Datta (2023), which shows that incumbents are less wage-sensitive than new hires. If wage is less salient for incumbents, employers have more room to exploit potential confusion of employees about the transfer for rent-seeking behavior with this group of workers. Notably, firms are able to extract rents from incumbent workers by overcoming the wage rigidities usually associated with incumbent workers through lower wage growth and promotion rates. The effect is also more pronounced in larger firms, which are likely to be more sophisticated in their wage-setting policies than smaller firms. Lastly, I find that the effect increases monotonically with the level of firm exposure to the reform, consistent with the rent-seeking channel.

The last part of the paper explores the policy implications of the results. By highlighting the role of firms in the transmission of incidence, the analysis calls into question the efficacy of using firms as intermediaries in the distribution of EITCs and sheds light on the potential trade-offs between take-up and distributional efficiency and incidence when giving employers an active role in the distribution of tax credits. I consider three main design features of EITC programs: take-up, timing, and incidence. Back of the envelope calculations suggest that, in the Italian context, making the take-up of the tax credit automatic increased, in 2015, the take-up from 8.9 million beneficiaries to 11.9 million beneficiaries, resulting in an additional 2.88 billions of benefits distributed. At the same time, the analysis shows that of the additional 2.88 billions benefits distributed because of the automatic take-up around 846 millions are captured by employers.

This paper contributes to the extensive literature on the effects of Earned Income Tax Credits, which has mostly focused on labor supply effects (Hotz, 2003; Eissa and Hoynes, 2006; Meyer, 2010; Nichols and Rothstein, 2015; Hoynes, 2019). This paper directly contributes to the scarce literature on the incidence and wage effects of these programs. There are only a handful of papers trying to estimate the wage effects of EITCs. Rothstein (2008, 2010) and Leigh (2010) analyze the incidence of the EITC in the US. Azmat (2019), Gravoueille (2024), and Garriga and Tortarolo (2024) focus on the UK, France, and Argentina, respectively. However, these efforts often fall short in addressing the challenges of estimating EITC, leaving the answer to this question unresolved. This paper contributes to the relatively limited literature on this topic by offering a new estimate of the incidence of EITCs in a context where the tax credit depends only on earnings and on no other personal characteristics, implying that eligible and non-eligible workers do not compete in

the same labor market. This setting is especially suitable for examining the wage effects of these programs. This paper is the first to show how the wage effects of EITCs can be heterogeneous across firms, highlighting that the standard competitive model overlooks key aspects for the analysis of the wage effects of these types of programs.

By taking a firm-level perspective to the analysis of the effects of tax policies, this paper contributes to a broader literature studying the firm-level transmission of tax incidence. The literature on the matter has exclusively focused on payroll taxes (Saez, Schoefer, and Seim, 2019; Bíró et al., 2022). Saez, Schoefer, and Seim (2019) study the firm-level effects of a payroll tax cut for young workers in Sweden, Bíró et al. (2022) find heterogeneity in the incidence of payroll tax cuts for older workers in Hungary. I contribute to this literature by studying the firm-level incidence of wage subsidies, which do not directly affect firms' labor costs but may still generate within-firm shocks. This paper is also related to several studies showing that the institutional and informational context plays a key role in determining tax incidence (Saez, Matsaganis, and Tsakloglou, 2012; Bozio, Breda, and Grenet, 2017).

The paper contributes to the broader literature showing that the traditional partial-equilibrium incidence, which suggests that the economic incidence of a tax credit depends only on the relative elasticity of supply and demand, might not be enough to explain the final incidence of these programs (Benzarti, 2024; Kroft et al., 2024; Benzarti et al., 2020; Chetty, Looney, and Kroft, 2009). Benzarti (2024) reviews the literature on the incidence of consumption and labor taxes and finds mounting evidence questioning the fundamental implications of the canonical model. It highlights how there is very little evidence on the incidence and potential anomalies in the case of income taxes, an area to which this paper directly contributes.

This work obviously relates to the literature studying the introduction of the *80 Euros Bonus* in Italy. Neri, Rondinelli, and Scoccianti (2015) analyze the effect of the introduction of the tax credit on household spending. Villamaina and Acciari (2023) analyze the intensive margin responses to the tax credit, finding no changes in labor supply. To the best of my knowledge, this is the first study investigating the effects of the program on wages.

Finally, this paper speaks to the literature on the design of Earned Income Tax Credits (Jones, 2010;Romich and Weisner, 2002), highlighting the main trade-offs between employer and government-administered EITC for the design of these programs.

The paper proceeds as follows. Section 2 outlines the institutional details of the Italian Earned Income Tax Credit and the data used in the analysis. Section 3 outlines the simple conceptual

framework that will guide the analysis. Evidence on the firm-level responses to the introduction of the program are presented in Section 4. Section 5 explores the heterogeneity of the results across different dimensions. Section 6 discusses the policy implications of the results and Section 7 concludes.

2 Institutional Background and Data

2.1 Institutional Background

2.1.1 The Program

In April 2014, the Italian government introduced the so-called *80 Euros Bonus*. The *80 Euros Bonus* is an Earned Income Tax Credit targeted at employees with annual gross income between €8,000 and €26,000. The tax credit was first distributed in May 2014 to around 10 million employees[3]. Its introduction was unexpected and, according to many, motivated by electoral reasons. I describe the main features of the program below.

Eligibility All individuals working as employees with a total annual gross income between €8,000 and €26,000 are eligible for the tax credit. Eligibility for the tax credit, conditional on being an employee, depends only on income and on no other personal or family characteristic. The eligibility range is in terms of *nominal* annual gross income and it is not adjusted annually for inflation.

Structure The structure of the program is described in Figure 1. The figure plots the annual tax credit received by annual gross income. From 2015 onwards the program was at full capacity, the tax credit was distributed every month and resulted in an annual tax credit of €960[4].

There are three relevant points in the structure of the tax credit: the *lower eligibility* cutoff of €8,000, the *phase-out* cutoff of €24,000 and the *upper eligibility* cutoff of €26,000. At the lower eligibility cutoff of €8,000, the program creates a sharp discontinuity in after-tax income. When the program is at full capacity, individuals earning just above the lower cutoff experience an increase in after-tax income of 12% compared to those earning just below. When annual gross income ex-

[3] *Source: Ministero dell'Economia e delle Finanze*

[4] The structure of the credit was slightly different in 2014, the first year the tax credit was introduced which was a transition year. Since the tax credit was distributed for the first time in May, it resulted in an annual tax credit of €640 instead of €960.

ceeds €24,000, the tax credit starts to phase-out and decreases until it reaches zero at €26,000. For incomes between €24,000 and €26,000 the amount of the tax credit is determined by the following formula: $\frac{(26{,}000 - annual\ gross\ income) \cdot 960}{2{,}000}$. Figure A1 shows the pre-reform distribution of gross annual earnings and the position of the lower eligibility, phase-out, and upper eligibility cutoff in the distribution.

Distribution of the Credit Workers do not need to apply to receive the credit. The distribution of the tax credit is automatic and administered by the tax withholding agent, the employer. The credit is distributed directly in the paychecks of workers (Figure 2). It either takes the form of a reduction in the tax withheld or, since the tax credit is refundable, of a transfer. The employer determines the eligibility of a given worker based on calculations on the annual labor earnings that the employer expects to pay the worker. The setting implies that, in practice, the eligibility for the tax credit is effectively based on annual gross *labor* income. While the tax credit is distributed monthly, eligibility is based on the annual gross income earned at the end of the year. Because annual gross income is not known with certainty at the moment of the distribution of the tax credit, this mechanism inevitably implies the possibility of mistakes that are corrected through adjustments during tax filing.[5]

Role of Employers Employers have a key role in the distribution and administration of the tax credit. Employers are responsible for determining each worker's eligibility for the tax credit, based on calculations on the projected annual labor income of the worker, and for distributing it monthly in workers' paychecks. In their role as tax withholding agents, employers advance the tax credit in employees' paychecks on a monthly basis. They subsequently remit the funds to the tax authority and, if the amount paid exceeds their tax liability, claim a refund.

This system has several implications for employers. First, employers have perfect knowledge of which employees qualify for the tax credit and can precisely distinguish between eligible and non-eligible workers. Second, because employers are responsible for the distribution and administration of the program, the setting makes the tax credit particularly salient to employers. Lastly, administering the tax credit implies processing, administrative, and disbursement costs for employers, highlighting the complexity and expense of their role.

[5] It was estimated that in 2014 around 1.5 million individuals had to return the tax credit during tax filing (*Ministero dell'Economia e delle Finanze*). These cases were mostly of workers whose annual gross income at the end of the year was lower than the lower eligibility cutoff of €8,000 because they worked only part of the year or lost their job during the year.

The program's distinctive features—its large size, salience, employer-led distribution, and automatic take-up—make it relevant to study. Additionally, the fact that eligibility is based solely on income, without consideration of personal or family characteristics, means that eligible and non-eligible workers may not necessarily compete within the same labor market. This contrasts with programs like the Earned Income Tax Credit (EITC), where eligibility often depends on additional personal or family characteristics (Hoynes, 2019). Crucially, the employers' central role in administering the program provides a unique opportunity to analyze firm-level responses to the introduction of the tax credit, making this setting particularly relevant to study.

2.2 Data

I use confidential administrative data from the Italian Social Security Institute (INPS) on a 7% random sample[6] of private-sector employees. My primary data source is matched employer-employee records at the annual level. For each worker-firm record, detailed information on the job relationship is available, such as: beginning and end date of the contract, type of contract (permanent vs. temporary, full-time vs. part-time), broad occupation group (blue-collar, white-collar or manager), annual gross labor earnings, number of weeks and days worked, and a unique firm and worker identifier. I link these records to workers' and firms' registers containing baseline demographic information, such as gender and age of employees and opening date, sector, and location of businesses.

For the main analysis, I restrict the sample to working-age individuals (aged between 25-65) working during the period spanning from 2010 to 2019. In the baseline specification, I consider individuals working for the entire pre-reform year. This restriction is helpful in determining with more precision the eligibility status of workers for the tax credit. To be even more precise, in some specifications I restrict to workers who, during the pre-reform year, are employed for the entire year by the same employer.

One potential limitation of these data for the analysis is that, while I have very good information on annual gross labor earnings, I do not observe hours worked, implying that I cannot investigate the effects of the reform on the hourly wage rate. I do, however, observe days worked, which allows me to use the daily wage rate as an outcome variable.

Table 1 reports summary statistics for the full sample and for the subsample of eligible workers in 2013, the last year before the introduction of the policy. Note that 51% of the employees in my

[6]The random sample is made up of workers born in 24 randomly selected birth dates.

sample are eligible for the tax credit. Overall, because the eligibility range for the tax credit is so wide, the characteristics of eligible employees are not remarkably different from the characteristics of workers in the full sample. Annual earnings are lower for eligible employees, but there are no important differences in terms of age, gender or share of workers employed with temporary contracts.

3 Conceptual Framework

In this section, I illustrate how program characteristics can translate into differences in responses at the firm-level, highlighting the crucial role firms could play in the final incidence of the tax credit.

In the canonical standard partial equilibrium model of tax incidence, the economic incidence of a tax credit depends on the relative elasticity of supply and demand, with the more elastic side shifting the tax burden to the more inelastic side. According to this model, the relative size of these elasticities is a sufficient statistic for tax incidence. It is commonly believed that the entire burden of tax credits falls on workers since, in a competitive market, firms pay workers their tax-adjusted marginal product of labor.

One of the main drawbacks of the canonical model to the incidence of EITC programs is that it assumes that firms passively accept market-level wages. The model implies that the final incidence of a tax credit is homogeneous across firms and workers. Within this framework, within-firm shocks generated by the introduction of the program and subsequent firm responses do not play any role in determining the incidence of the program. This assumption may not hold in practice as individual firms are likely to have a key role in wage setting and there are significant wage premium differences across employers (Card, Heining, and Kline, 2013; Card et al., 2018). Consequently, the incidence of tax credits could be heterogeneous across firms, which has important implications for understanding their overall welfare effects.

To guide the empirical analysis, I consider the specific institutional context of the *80 Euros Bonus*. The tax credit is distributed by employers directly into the paychecks of workers. Employers are responsible for determining each worker's eligibility for the tax credit, based on calculations on the projected annual labor income of the worker. In their role as tax withholding agents, employers advance the tax credit in employees' paychecks on a monthly basis. They subsequently remit the funds to the tax authority and, if the amount paid exceeds their tax liability,

claim a refund.

The way the tax credit is designed and distributed has several important implications for employers and for the incidence of the program. First, employers have perfect information about who is eligible for the tax credit and can precisely tag eligible and non-eligible workers. Second, the setting makes the tax credit particularly salient to employers who have the disbursement responsibility. Third, administering the tax credit is costly for employers in terms of processing, administrative, and disbursement costs. Finally, because the tax credit is integrated into workers' paychecks, employees may perceive it as part of their overall compensation. All these characteristics make the role of employers crucial in the analysis of the tax credit and its final incidence. The final incidence of the tax credit may not be homogeneous across firms as within-firm shocks generated by the introduction of the program can play a key role.

To study the firm-level incidence effects of the introduction of the tax credit, I consider firms that were more or less *exposed* to the program. Given the program characteristics, to measure exposure, I consider the *firm-level share of pre-reform eligible employees*. Different mechanisms could rationalize the presence of firm-level wage effects by exposure to the program.

First, after the introduction of the program, firms may easily recognize that eligible workers are better off due to the tax credit and might have opportunities for rent-seeking behavior by, for example, exploiting employees' perception of the transfer as part of their compensation package. For more exposed firms, engaging in rent-seeking behavior is more worthwhile, as they have a larger share of workers to extract rents from. These firms may also be more likely to overcome adjustment costs associated with changing their wage policies if their typical worker is affected by the policy (Chetty et al., 2011). Additionally, the incentive for rent-seeking behavior could vary across firms due to the interplay between the introduction of the tax credit and company wage-setting practices, such as internal pay equity concerns (Benzarti, 2024). If fairness considerations play a role, more exposed firms can have a higher scope for rent-seeking behavior when a large share of employees is eligible for the tax credit.

In the presence of a rent-seeking space, labor demand for eligible workers shifts outwards, allowing firms to offer a lower pre-transfer wage to eligible workers. Under this channel, employment of eligible workers should also increase. Therefore, we would expect wages of eligible employees to decrease in more exposed firms relative to less exposed firms. We would also expect negative spillover effects to non-eligible workers, due to substitution between eligible and non-eligible workers or fairness considerations.

Administration costs may be another mechanism behind firm-level responses by exposure. Administering the tax credit is costly for employers in terms of processing, administrative, and disbursement costs, as well as in terms of time employers have to spend assessing eligibility for the tax credit. These costs increase with the firm-level share of eligible employees, as a higher share of eligible employees increases the costs of disbursing the tax credit for employers. Under this scenario, the labor demand for eligible workers would shift inward, decreasing both the wage and the employment for eligible workers in firms more exposed to the program. We would also expect a substitution from eligible workers towards similar, non-eligible workers, whose wage might increase.

Finally, note that an employee-driven channel is unlikely to explain firm-level responses. While workers might misperceive the tax credit as part of their compensation package and this misperception might lead workers to accept lower gross wages (Garriga and Tortarolo, 2024), this mechanism would not imply different firm-level responses.

In the empirical analysis, I therefore explore the role of firm-level mechanisms as potential determinants of tax incidence by taking advantage of the fact that the reform generates a firm-level experiment. The key idea behind the firm-level experiment is that the same reform can generate different firm-level exposure to the program depending on a firm's share of ex-ante eligible workers (Draca, Machin, and Van Reenen, 2011; Harasztosi and Lindner, 2019). I use this firm-level experiment to test whether firms more affected by the program react differently than firms less affected, both in terms of effects on eligible workers and on spillover effects to non-eligible workers.

4 Firm-Level Effects of the Tax Credit on Earnings

This section explores the role of firm-level mechanisms as potential determinants of tax incidence by taking advantage of the fact that the reform generates a firm-level experiment. The same reform generates different firm-level exposure to the program depending on a firm's share of ex-ante eligible workers. In this section, I study differential firm responses by exposure, focusing on direct responses on eligible workers and potential spillover effects on non-eligible workers.

4.1 Empirical Strategy

I test whether firms with a higher concentration of eligible workers respond differently to the introduction of the program relative to firms with a lower concentration of eligible workers. My empirical strategy relies on firm-level variation in the pre-reform share of eligible workers. I compare the evolution of key firm-level outcomes between firms with different concentrations of eligible workers following a methodology popular in the minimum wage literature (Draca, Machin, and Van Reenen, 2011; Harasztosi and Lindner, 2019; Giupponi and Machin, 2024).

Measuring Firm-Level Exposure to the Reform I consider an unbalanced panel of firms active in 2013[7]. For each firm, I calculate the share of eligible employees they employ in the baseline year, 2013. I define firm exposure to the policy in the baseline year to abstract from potential behavioral responses to the policy. I divide the panel of firms into four groups based on the quartiles of the pre-reform share of eligible employees. I define the quartiles restricting to firms with a non-zero share of eligible workers. Firms with exactly zero eligible workers in the baseline year are then included in the first group along with the firms in the bottom quartile. Results do not change when defining the quartiles without restricting to firms with a non-zero share of eligible workers. Figure A2 highlights the source of variation I use for the analysis by showing the distribution of firms in terms of the share of eligible workers in 2013. There is a spike in the distribution at zero, representing the 28 percent of firms with a share of eligible employees in 2013 of precisely zero. Over the range $(0, 1]$, the distribution is fairly dispersed. The average value of the share of eligible workers is 0.56, with a standard deviation of 0.28.

Observing only a sample of workers may create measurement error in calculating the exposure measure at the firm level. To usefully characterize the share of eligible workers measure, I restrict to firms with more than one sampled worker. In my baseline specification, I will consider firms with at least three sampled workers. This restriction implies that the sample of firms I consider in the baseline analysis will be skewed towards larger firms, as shown in Panel A of Figure A4. In the analysis, I assess the robustness of the results to this restriction. Given these restrictions, one might worry that the exposure measure might be correlated with firm size. This does not seem to be the case, as shown in Figure A4 and A5, Panel B.

[7] All results are robust to considering a balanced panel of firms active from 2010 to 2019.

Empirical Strategy In my baseline analysis, I will compare *medium-high exposure* firms (firms whose share of eligible employees in 2013 was between the 50th and the 75th percentile) to *high exposure* firms (firms in the top quartile of the share of eligible employees in 2013). This way, I compare firms with comparable observable characteristics that face heterogeneous exposure to the reform. Below, I also broaden the analysis to include less exposed firms and use a continuous exposure measure.

I study the effects of firm-level exposure to the policy by estimating a difference-in-differences model. I estimate the following model, at the firm level:

$$y_{ft} = \eta_f + \eta_t + \sum_{k=-m}^{q} \beta_k \left(T_f \cdot D_t^k \right) + \varepsilon_{ft} \tag{1}$$

where y_{ft} is a firm-level outcome of interest such as the average earnings of eligible employees, η_f are firm fixed effects (which capture time-invariant heterogeneity across firms), η_t are year fixed effects, and $D_t^k = \mathbb{1}\left(t = t_0 + k\right)$ where $t_0 = 2013$. In the baseline specification, T_f is equal to one if firm f's share of eligible employees in 2013 was in the top quartile of the pre-reform distribution of the share of eligible employees (*high exposure*) and equal to zero if firm f's share of eligible employees in 2013 was between the 50th and the 75th percentile (*medium-high exposure*). I perform several robustness checks estimating the same model using different definitions of T_f.

Identification relies on the assumption that more and less exposed firms would have had parallel trends in key outcomes absent the reform. This assumption can be assessed by evaluating the coefficients β_k for $k < 0$. Testing for their significance allows to establish whether firms that are differentially exposed to the reform have different trends in earnings dynamics.

There is considerable persistence in the share of eligible employees across groups of firms and years. Figure A3 depicts the average share of eligible workers each year for each group of firms. Note that the spike in 2013 is due to mean reversion, and it naturally follows from the definition of groups of firms: firms with a high share of eligible employees in 2013 are likely to have a lower share of eligible employees before and after. The opposite is true for firms with a low share of eligible employees.

It is also key for the empirical strategy that the characteristics of the different groups of firms are comparable. Table 2 provides descriptive statistics for the four groups of firms defined using the quartiles of the share of eligible workers in 2013. Firms in different groups are not hugely different in terms of observable characteristics. The characteristics of medium-high and high ex-

posure firms are particularly similar: the share of temporary workers, the gross annual earnings per eligible employee and the share of large firms are comparable. Along the same lines, medium-high and medium-low exposure firms are also very similar in terms of observable characteristics.

4.2 Effects on Eligible Workers

Figure A6 plots the raw average gross annual earnings of eligible workers in the treatment (*high exposure firms*) and the control group (*medium-high exposure firms*) (normalized to 2010). The figure shows that the two groups of firms have similar dynamics in terms of annual earnings of eligible workers in the pre-reform period but a clearly divergent pattern after the tax credit is introduced. Although this evidence is only descriptive, it helps illustrate that the treatment and control groups followed similar trends before the reform.

Figure 3, Panel A reports the results of the estimation of equation 1 using as outcome the (log) firm-level average of gross annual earnings of pre-reform eligible workers. The figure shows that, prior to the introduction of the program, the average annual earnings of pre-reform eligible workers evolved similarly in the treatment and control firms, providing suggestive evidence in favor of the parallel trends assumption. Annual earnings of the average pre-reform eligible worker in high-exposure firms decrease when the tax credit is introduced relative to medium-high-exposure firms. The response is starker from 2015 onwards. This pattern can be explained by the fact that 2014 was a transition year when the program was not yet at full capacity. Overall, the impact on the annual earnings of pre-reform eligible employees is around 1 to 2 percent over the years 2015-2019 in more exposed firms relative to less exposed firms.

Table 3 reports the reduced-form and pass-through estimates of the introduction of the program. The reduced-form estimates are obtained estimating the compact version of the difference-in-differences specification in equation 1. The reduced-form estimate of column 1 corresponds to the difference-in-differences coefficients where the *post* event includes all years after 2013. The estimates of column 2 and 3 report the effect in the short-run (2014-2016) and medium-run (2017-2019) respectively. The table summarizes the results from the previous figures and shows that the annual earnings of the average pre-reform eligible worker in high exposure firms decreases by 0.8 and 1.6 percent relative to lower exposure firms. The estimates imply that employers in higher exposed firms were able to capture about 17-29 percent more of the transfer relative to lower exposed firms. Considering the average annual earnings of eligible employees in high exposure firms, the effect translates into a monetary loss of around €272.

Continuous Treatment The results presented above are robust to different modeling choices. I estimate a treatment intensity model, using the pre-reform share of eligible workers as a proxy for treatment intensity. More in detail, I estimate the following model:

$$y_{ft} = \eta_f + \eta_t + \sum_{k=-m}^{q} \beta_k \left(Share\ Eligible\ 2013_f \cdot D_t^k \right) + \varepsilon_{ft} \tag{2}$$

where y_{ft} is a firm-level outcome of interest such as the average earnings of eligible employees, η_f are firm fixed effects, η_t are year fixed effects, and $D_t^k = \mathbb{1}(t = t_0 + k)$ where $t_0 = 2013$. $Share\ Eligible\ 2013_f$ is the share of eligible workers in firm f in 2013[8].

Figure 4 reports the results of the estimation of equation 2. The figure shows the results when the outcome is the firm-level average annual earnings of pre-reform eligible employees. Using this specification, the results are qualitatively confirmed. The results indicate that in more exposed firms, the earnings of eligible workers decrease relative to less exposed firms. Table 4 reports the reduced-form results estimating the compact version of the difference-in-differences specification in equation 2 where the *post* event includes all years after 2013. In terms of magnitude, the results indicate that a one-standard-deviation increase in the 2013 share of eligible employees is associated with a decrease in annual earnings of eligible workers of 1.9%.

Composition The analysis so far has shown that, in more exposed firms, the *firm-level average* annual earnings of eligible workers decrease after the reform compared to less exposed firms.

One concern with the firm-level analysis and with using firm-level averages is that the composition of workers may change for the treatment group post-reform. If the composition of workers changes and new workers are different in terms of characteristics, they could affect firm-level average wages through composition effects. To address this concern, I merge the firm-level data with the population of individual workers and follow individual workers over time, based on their pre-reform employer. To investigate the effects of the reform at the individual-level, I estimate the following model:

$$y_{it} = \eta_i + \eta_t + \sum_{k=-m}^{q} \beta_k \left(T_i \cdot D_t^k \right) + X_{it}'\delta + \varepsilon_{it} \tag{3}$$

where the outcome now is individual-level annual earnings, $D_t^k = \mathbb{1}(t = t_0 + k)$ where $t_0 =$

[8]I exclude firms in the bottom octile of the distribution of the share of eligible employees due to comparability reasons: these firms either have zero eligible employees or a very small share of eligible employees and therefore have different earnings dynamics than the other groups.

2013, and T_i is equal to one if the firm where individual i works in 2013 was in the top quartile of the pre-reform distribution of the share of eligible employees (*high exposure*) and equal to zero if the firm where individual i works in 2013 was between the 50th and the 75th percentile (*medium-high exposure*).

Figure 5 reports the results of the estimation of equation 3 for the sample of workers eligible for the tax credit in 2013. The figure shows that the annual earnings of eligible individuals working in 2013 (before the reform) in a firm with a large share of eligible employees significantly decrease after the reform relative to individuals working in a firm with a lower share of eligible employees. Prior to the introduction of the program, the annual earnings of eligible workers evolved similarly, providing suggestive evidence in favor of the parallel trends assumption. Note that the decrease is similar in magnitude to the firm-level estimates suggesting that the firm-level estimates are not primarily capturing changes in the composition of workers at firms rather than changes in earnings for individual workers.

Decomposing Earnings Responses The results presented above show that the firm-level average annual *earnings* of eligible employees decrease in more exposed firms compared to less exposed firms. Using earnings as the main outcome variable implies that the estimated effects blend together both labor supply and wage rate responses. I provide different pieces of evidence showing that the effects are not driven by labor supply responses. First, the results are similar when considering daily wages instead of annual earnings. Figure 6 reports the results of the estimation of equation 1 where the dependent variable is the average daily wage of pre-reform eligible workers. The figure shows that, following the reform, daily wages decline in highly exposed firms relative to less exposed firms, mirroring the trend seen in overall earnings. Second, the main results also hold when I focus on employees working full-time and for the entire year. Finally, the finding that the earnings response is not driven by changes in labor supply aligns with existing literature on the labor supply effects of the introduction of the program. Villamaina and Acciari (2023), for example, find no intensive margin or bunching responses to the introduction of the tax credit.

I also study the effect of the tax credit throughout the earnings distribution using a similar approach to the one of Cengiz et al. (2019). This analysis is important to understand whether the decrease in earnings in more exposed firms happens across the board or is driven by specific categories of eligible employees. Figure 7 reports the difference in the change in firm-level average

earnings between 2014 and 2013 (the pre-reform year) between treatment firms (*high exposure*) and control firms (*medium-high exposure*) for each €2000 gross annual earnings bin (relative to the bin corresponding to €32,000). The bins are defined based on the pre-reform earnings distribution. There is a clear and significant negative difference in the change in average earnings between treatment firms and control firms for the bins corresponding to workers who, in the pre-reform year, were eligible for the tax credit. This decline in earnings appears consistent across all eligible workers, indicating that the main result stems from a broad reduction in earnings rather than being concentrated within specific categories of eligible workers. Two additional observations are important. First, the decrease is not concentrated around the €24,000 kink, as would be expected if the results were driven by workers' behavioral responses to the tax credit schedule. Second, the decline also appears among similar non-eligible workers, suggesting possible spillover effects. The potential for these spillover effects is further examined in Section 4.3.

Robustness Figure A7 reports the results of the bin-to-bin analysis described above for placebo years, considering the change in earnings between 2013 and 2012. The figure shows, reassuringly, that there is no significant difference in the change in earnings of workers in the treatment firms relative to workers in the control firms before the reform.

Figure A8 reports the results of the estimation of equation 1 comparing firms with a share of eligible employees in 2013 between the 50th and the 75th percentile (*medium-high exposure*) to firms with a share of eligible employees in 2013 between the 25th and the 50th percentile (*medium-low exposure*). The results are robust to using this different definition of treatment and control group.

As mentioned above, the fact that I observe a sample of workers presents additional challenges for the firm-level analysis, in particular related to the share of eligible employees measure. In my baseline analysis, I consider firms with at least three sampled workers to be able to more reliably characterize the share of eligible employees measure. Figure A9 shows that the results are not dependent on this restriction and are robust to relaxing the restriction and considering firms with at least two sampled workers.

4.3 Spillover Effects on Non-Eligible Workers

The next part of the analysis asks whether the introduction of the tax credit has spillover effects on non-eligible workers. As outlined in Section 3, the introduction of the tax credit might have spillover effects to non-eligible workers, that are expected to be stronger in firms more exposed to

the program. Understanding the nature of these potential spillover effects to non-eligible workers is important and can help shed light on the potential mechanisms behind the observed responses.

As discussed above, the evidence in Figure 7 is suggestive of the presence of spillover effects to non-eligible workers. I further study the effects of the introduction of the policy on the earnings of non-eligible workers using the same empirical strategy of the main analysis. In Figure 8, I estimate the model from equation 1, using as dependent variables the average gross annual earnings of non-eligible workers, distinguishing between "similar" non-eligible workers (lower-earning non-eligible workers earning between €26,000 and €32,000) and other non-eligible workers (higher-earning non-eligible workers earning more than €32,000). The figure shows that, while there is no differential responses between high and low exposed firms in terms of earnings of the higher-earning non-eligible workers, the earnings of the lower-earning eligible workers decrease in higher exposed firms relative to lower exposed firms. This result suggests that the effects of the introduction of the tax credit do spillover to similar non-eligible workers, who see their earnings decrease by a similar magnitude as eligible workers. These spillover effects do not seem to be driven by non-eligible employees becoming eligible after the reform, as shown in Figure A10.

This result can be consistent with a model of rent-seeking behavior by firms and, potentially, with the notion that pay-equity concerns are relevant only with respect to a reference group (Dube, Giuliano, and Leonard, 2019) or with a model of substitution between eligible and non-eligible workers. The results are not consistent with administrative costs being the main mechanisms behind the results.

4.4 Employment

The results so far have shown that the earnings of eligible workers decrease in more exposed firms relative to less exposed firms. There are also significant spillover effects to "similar" non-eligible workers. The final piece of evidence needed to fully understand the mechanisms behind these effects is the impact of the program on firm-level employment. Figure 9 estimates model 1 using as outcome the observed number of employees by firm, normalized relative to the pre-reform year[9]. The figure shows that, after the reform, firm-level employment appears to increase gradually in highly exposed firms relative to less exposed ones. In terms of magnitude, the effect corresponds to a 2% increase in the observed number of employees in more exposed firms relative

[9]Because of the structure of the data, this normalization allows to interpret the results as percentage change in firm size relative to the pre-reform year.

to less exposed firms. Table 6 shows that the increase is mostly driven by an increase in the number of eligible workers. The effect on the observed number of non-eligible workers is negative but not statistically significant. The effects are in line with a response to a change in relative cost between eligible and non-eligible workers and with labor demand substitution between the two groups of workers.

Interpretation In a perfectly competitive labor market, we would expect workers of a given skill to be paid the market wage and, as a consequence, eligible workers in different firms to receive the same wage. At the same time, workers of different skills should receive different wages even if employed by the same firm. The effects documented so far provide suggestive evidence of a labor market characterized by a degree of monopsonistic competition where firms have some degree of wage-setting power. The results are overall consistent with labor market monopsony and rent-seeking behavior. Internal pay equity and labor demand substitution between eligible and similar non-eligible workers could both explain the observed effects.

What Do Firms Do With the Pocketed Tax Credit? Given that firms are able to retain approximately 30% of the tax credit in the medium term, a crucial question arises: what do firms do with the pocketed tax credit? Understanding how the program affects firm outcomes is essential for moving beyond a partial equilibrium incidence framework toward a more comprehensive general equilibrium approach. Although I am unable to conduct this analysis due to data limitations, it would be a valuable direction for future research.

5 Heterogeneity and Mechanisms

The results above show that earnings of eligible employees after the reform decrease in firms more exposed to the program relative to firms less exposed to the policy. On average, employers in more exposed firms capture up to 30% more of the transfer. The overall results are consistent with a labor market is characterized by a degree of monopsonistic competition and with rent-seeking behavior by firms. In this section, I present some heterogeneity analysis with the aim of shedding light on the mechanisms behind the results.

I first ask whether the main results are driven by incumbent workers or new hires. Figure 10 reports the results of the estimation of model 1 where the dependent variable is the firm-level average earnings of pre-reform eligible incumbent workers. The figure shows how the negative

effect on earnings of eligible employees is mostly driven by incumbent workers. The result is also in line with existing evidence from Datta (2023), which suggests that incumbents are less wage-sensitive than new hires and attributes this difference to differences in wage saliency between the two groups. If wage is less salient for incumbents, this suggests that employers have more room to exploit potential confusion of employees about the transfer for rent-seeking behavior with this group of workers. This result suggests two additional things. First, firms are able to extract rents from incumbent workers by overcoming the wage rigidities usually associated with incumbent workers through lower wage growth and promotion rates. Second, the result can be interpreted as suggestive evidence in support of the fairness mechanisms, as pay equity constraints are likely to be more binding for incumbent workers.

I further break down the aggregate effects by firm size. Table 7 reports the results broken down by firm size, considering firms with more or less than 50 employees. The effect is stronger in large firms which are able to pocket almost 30% of the tax credit over the years 2014-2019. On the other hand, smaller firms pocket only 16% of the tax credit. Wage effects might be more prevalent in larger firms, which are likely to be more sophisticated in their wage-setting policies and overall more able to respond to the introduction of the tax credit than smaller firms.

Lastly, I investigate the relationship between the effects on eligible workers and firms' exposure to the policy. If the results are driven by the rent-seeking channel (through adjustment costs or fairness), we would expect to find a monotonic relationship between exposure and firm-responses to the introduction of the program. Figure 11 shows the two-year firm-level change in annual earnings of eligible employees by pre-reform exposure before (2013) and after (2015) the reform. The figure shows that, before the reform, there is no difference in the change in earnings of eligible workers by firm exposure. After the reform, the figure shows a stark monotonic relationship between the change in average annual earnings and the pre-reform share of eligible employees, with earnings of eligible workers decreasing more in firms with a higher share of eligible employees. This result aligns well with the rent-seeking channel, as firms are more likely to rent-seek when they have a larger pool of workers to extract rents from.

6 Discussion and Policy Implications

The results presented so far can have important implications when thinking about the design of Earned Income Tax Credits programs. By highlighting the role of firms in the transmission

of incidence, the analysis calls into question the efficacy of using firms as intermediaries in the distribution of Earned Income Tax Credits and sheds light on the potential trade-offs between take-up and distributional efficiency and incidence when giving employers an active role in the distribution of tax credits. This analysis contributes to a long-standing debate on the optimal design of EITCs, considering the costs and benefits of different design features of EITCs. In this section, I will weigh the costs and benefits of employer-administered tax credits[10].

Take-Up One common and widespread problem with EITC programs disbursed annually through the tax system is the overall low take-up of the tax credit. The EITC in the US has an estimated incomplete take-up rate of 25 percent[11]. The figure was similar for the Family Credit in the UK which was distributed through the tax system and had an incomplete take-up rate of around 34 percent. The incomplete take-up rate decreased to 24 percent with the introduction of the Working Family Tax Credit (WFTC), which was distributed directly into the paychecks of workers. In the case of the *80 Euros Bonus*, the fact that it was employer-administered and its distribution automatic, allowed to achieve a nearly universal take-up, greatly increasing coverage and presumably improving targeting than the standard EITC (Jones, 2015). The increase in take-up associated with the firm-level distribution of the credit is likely to lead to an increase in welfare and is therefore normatively desirable. Bhargava and Manoli (2015) show that psychological frictions and lack of program awareness are key factors in explaining the low EITC take-up, while stigma and high perceived costs of claiming do not. They find that the lowest earners in their sample were disproportionately harmed by psychological frictions, suggesting that improving take-up is normatively desirable.

Back-of-the-envelope calculations suggest that, in the Italian context, making the take-up of the tax credit automatic increased, in 2015, the take-up from 8.9 million beneficiaries to 11.9 million beneficiaries, resulting to an additional 2.88 billions of benefits distributed.

Timing A key difference between the EITC and other forms of income support is timing. The majority of EITC programs disburse benefits as one annual lump-sum payment. This feature is commonly raised as an impediment to providing assistance to families throughout the year

[10]The main discussion in the literature about these design features of EITC programs come from the analysis of the Advance EIC program in the US (Nichols and Rothstein, 2015). Until 2011, EITC recipients could choose to receive a portion of their credit with each paycheck rather than as a lump sum at tax filing time. The Advance EIC allowed for negative withholding from the weekly, biweekly, or monthly paycheck. But take-up of this option was very low, only 1–2 percent of EITC claimants, leading to its cancellation.

[11]Source: IRS .

(Hoynes, 2019) and has implications for savings and consumption (Nichols and Rothstein, 2015). If households are liquidity constrained, a lump-sum payment has a smaller effect on welfare than smaller monthly payments. Distributing the tax credit monthly and directly in the paychecks of workers is therefore likely to be welfare improving relative to the standard EITC.

Administration and Incidence While having employers administering the tax credit and directly distributing it into the paychecks of workers have advantages relating to the take-up and the timing of the tax credit, the analysis shows that it can have important costs in terms of the final incidence of the tax credit. The results show that firms are able to capture part of the tax credit in addition to the market-level incidence effects and therefore decreasing the portion of the tax credit going to workers. The potential firm-level incidence effects are usually not considered in the standard discussions on the design of EITC which rest on the assumption of perfectly competitive labor markets. For example, Nichols and Rothstein (2015) argue that incidence considerations cannot explain the low take-up of the Advance EITC in a neoclassical model. While they open to the possibility of violations of the perfectly competitive model that would allow employers distributing the tax credit to discriminate against workers, they consider this possibility limited and the neoclassical model's insight that any such discrimination is limited by the worker's ability to take another job with an employer who pays the going wage likely to be robust. This paper shows that the possibility is not limited.

The estimates reported in the previous sections suggest that around 30% of the tax credit is captured by employers. Of the additional 2.88 billions benefits distributed because of the automatic take-up around 846 millions are captured by employers.

7 Conclusions

This paper studies the role of firms in the final incidence of Earned Income Tax Credits. Using administrative data and taking advantage of the introduction of a large, employer-administered EITC program in Italy, I study firm-level responses to the tax credit by exposure to the program. I find that the earnings of eligible workers in more exposed firms decrease compared to comparable less exposed firms, with highly exposed firms capturing around 30% more of the transfer. Decomposing the earnings results, I find that the effect is driven by wages rather than by labor supply responses. Notably, the decrease in earnings stems from a reduction in earnings across

the board for all eligible employees rather than being concentrated within specific categories of eligible workers.

The introduction of the program also has significant spillover effects on non-eligible workers. I find that earnings of non-eligible workers after the reform decrease in more exposed firms compared to less exposed firms after the reform. Notably, these spillover effects occur only for non-eligible workers who are similar to eligible workers—that is, those earning not too much above the eligibility cutoff.

Overall, the empirical results are consistent with a labor market characterized by monopsonistic competition and with rent-seeking behavior by firms. Internal pay equity and labor demand substitution between eligible and similar non-eligible workers can explain the dynamics. Importantly, this evidence challenges a fundamental assumption of the standard incidence model: that firms passively accept market wages, leading to homogenous incidence of that tax credit across firms and workers. Instead, my results underscore that firms actively influence wage-setting, thereby shaping the actual incidence of EITCs in practice.

In terms of policy implications, these results raise important questions about the efficacy of using firms as intermediaries in EITC distribution. While employer-administered credits may facilitate more frequent (e.g., monthly) transfers and mitigate low take-up by automatically distributing benefits, they also provide employers with perfect information about recipient eligibility and benefit amounts, potentially enabling them to capture part of the credit intended for workers. Consequently, policymakers may need to consider this trade-off when structuring EITC programs and other income support mechanisms.

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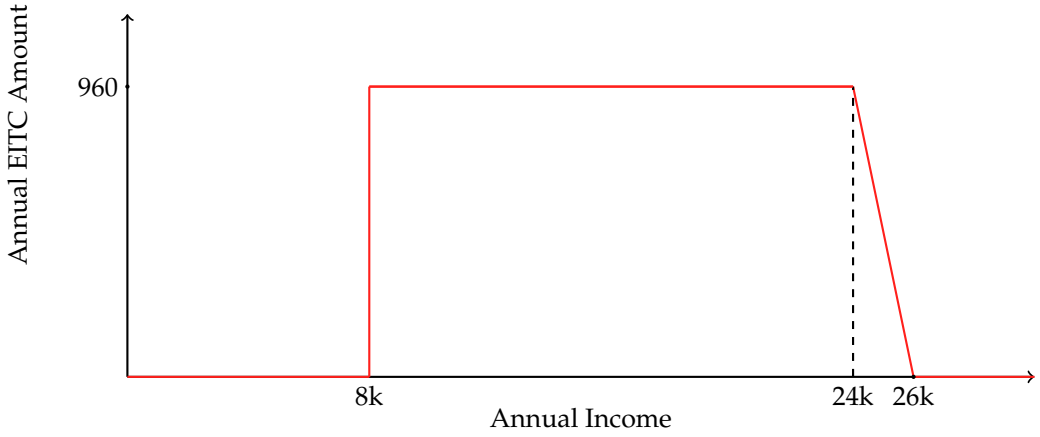
Figures

Figure 1: STRUCTURE



Notes: This figure shows the structure of the tax credit. Individuals with annual gross income between €8,000 and €24,000 are eligible for an annual tax credit of €960. For employees whose annual gross income is between €24,000 to €26,000 the tax credit due is calculated as $\frac{(26{,}000 - annual\ gross\ income) \cdot 960}{2{,}000}$.

Figure 2: PAYCHECKS OF WORKERS: EXAMPLE

Totale : 1.823,55

COD.	DESCRIZIONE	ORE / GG	%	DATO BASE	RITENUTE	COMPETENZE
1	IMPORTO ORDINARIO	26,00		70,13654		1.823,55
111	STRAORDINARIO 15%	2,00	15,00	12,48262		24,97
999	TOT.LORDO SOGG.CONTR			1.848,52		
1	CONTRIB.FAP 9.19%		9,1900	1.849,00	169,92	
58	FONDO EST TERZ.			2,00	2,00	
991	SOLID. M980			10,00		
125	FONDO INT. SALAR.(5)		0,1500	1.849,00	2,77	
581	ALLEATA PREV.100%TFR		100,0000	(125,83)	(125,83)	
	INAIL Pat: 91825354/97 Tar: 0722			(1.849,00)		
	Ctr.Ass.San. Progr.			(36,00)		
	Ctr.Ass.San. dedotti			(12,00)	(2,00)	
	TFR versato al fondo			(378,21)	(378,21)	
	Tot. rit. sociali			172,69		
	Imponibile Fiscale			1.673,83		
	Impon. fiscale netto			1.673,83		
	Rit. Fis. mese lorda			401,93		
	Imponibile detraz.			23.154,02		
	Detrazioni fiscali			101,62		
	Rit. Fis. mese netta				300,31	
	Credito Art.13 TUIR					80,00
	Add.Reg. rata pagata (Cod.: LO)				29,93	
	Add.Com. rata pagata (cod.: E265)				11,53	

NOTE	TOTALE RITENUTE	TOTALE COMPETENZE
	521,69	1.930,05

			ARR. PRECEDENTE	ARR. ATTUALE

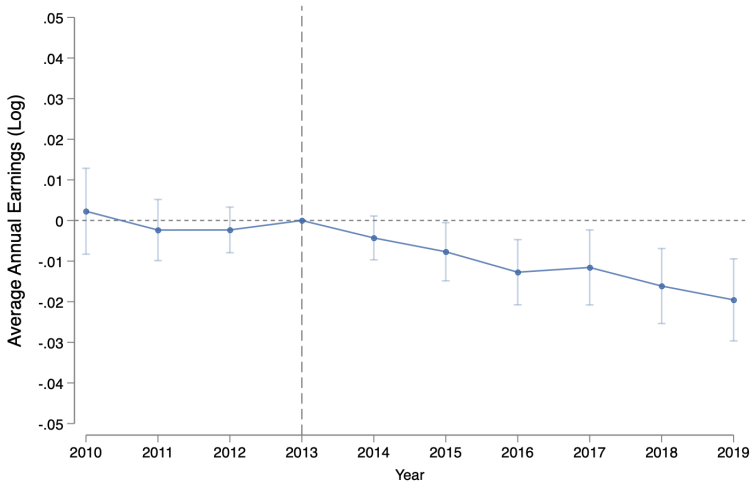
NETTO IN BUSTA

Continua su foglio numero

00000043689

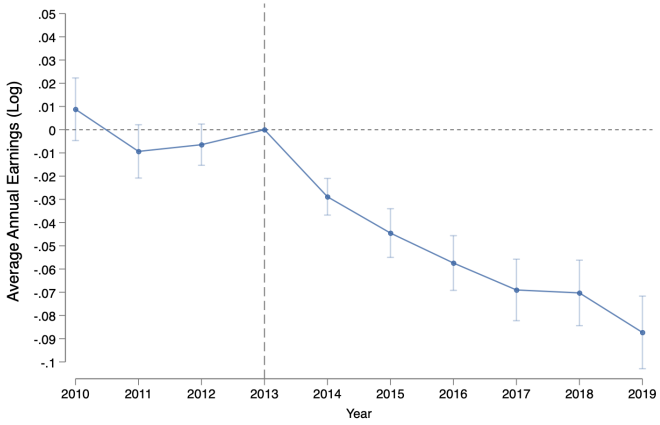
Notes: This figure shows an example of the paycheck of an Italian worker. The red square denotes the line indicating the amount of the 80 Euros Bonus which is added directly to the paychecks of workers every month.

Figure 3: FIRM-LEVEL EFFECTS OF THE TAX CREDIT ON EARNINGS



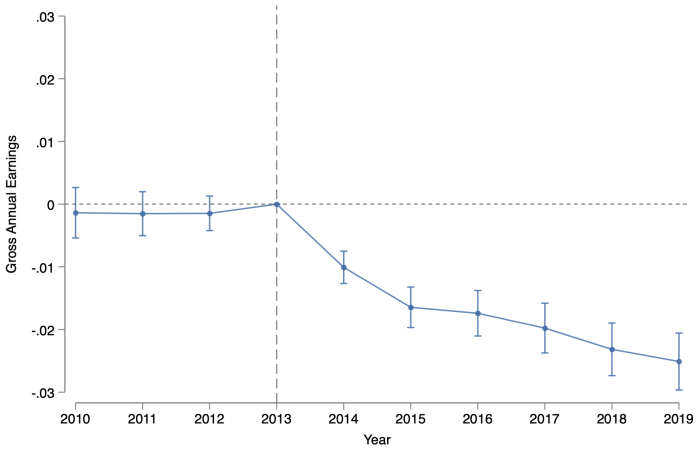
Notes: The figure shows the results from a difference-in-differences specification (equation 1) comparing firms with a high share of eligible employees (top quartile) to firms with a medium-high share of eligible employees (between the 50th and the 75th percentile) in the last pre-reform year. The outcome is the log of the firm-level average annual earnings for pre-reform eligible workers. Standard errors are clustered at the firm level.

Figure 4: FIRM-LEVEL EFFECTS OF THE TAX CREDIT ON EARNINGS: CONTINUOUS TREATMENT



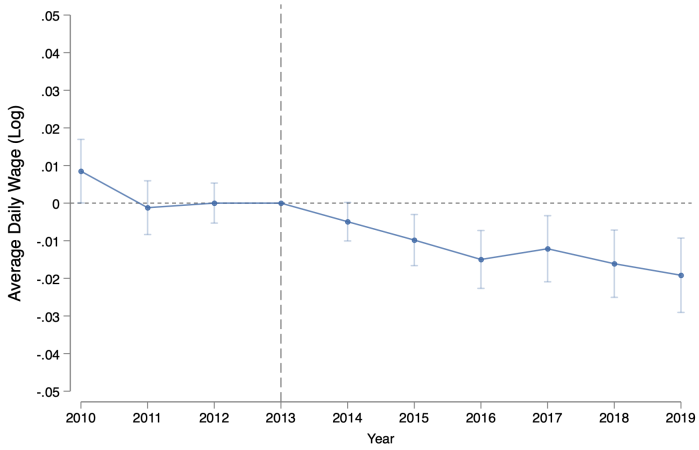
Notes: The figure shows the results from a difference-in-differences specification (equation 2) which estimate a treatment intensity model, using the pre-reform share of eligible workers as a proxy for treatment intensity. The figure shows the results when the outcome is the log of the firm-level average gross annual earnings for pre-reform eligible workers. Standard errors are clustered at the firm level.

Figure 5: FIRM-LEVEL EFFECTS OF THE TAX CREDIT ON EARNINGS: INDIVIDUAL-LEVEL ESTIMATES



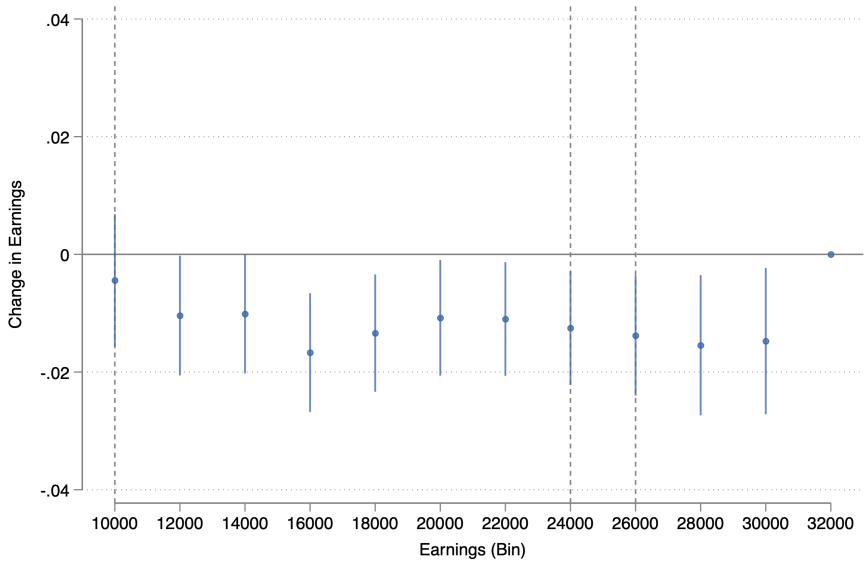
Notes: The figure shows the results from the individual-level difference-in-differences model outlined in equation3, which compares individuals employed in 2013 in high exposure firms (top quartile) to individuals employed in 2013 in firms with a medium-high share of eligible employees (between the 50th and the 75th percentile). The sample is restricted to individuals eligible for the tax credit in 2013. The outcome variable is the log average gross annual earnings. The standard errors are clustered at the individual-firm-level.

Figure 6: FIRM-LEVEL EFFECTS OF THE TAX CREDIT ON EARNINGS: DAILY WAGE



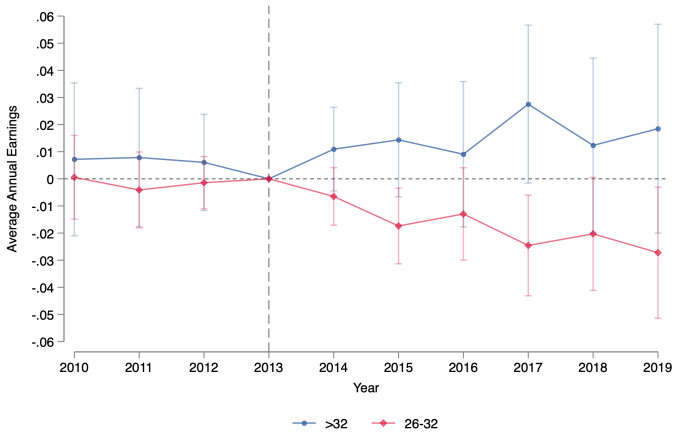
Notes: The figure shows the results from a difference-in-differences specification (equation 1) comparing firms with a high share of eligible employees (top quartile) to firms with a medium-high share of eligible employees (between the 50th and the 75th percentile) in the last pre-reform year. The outcome is the log of the firm-level average daily wage for pre-reform eligible workers. Standard errors are clustered at the firm level.

Figure 7: FIRM-LEVEL EFFECTS OF THE TAX CREDIT ACROSS THE EARNINGS DISTRIBUTION



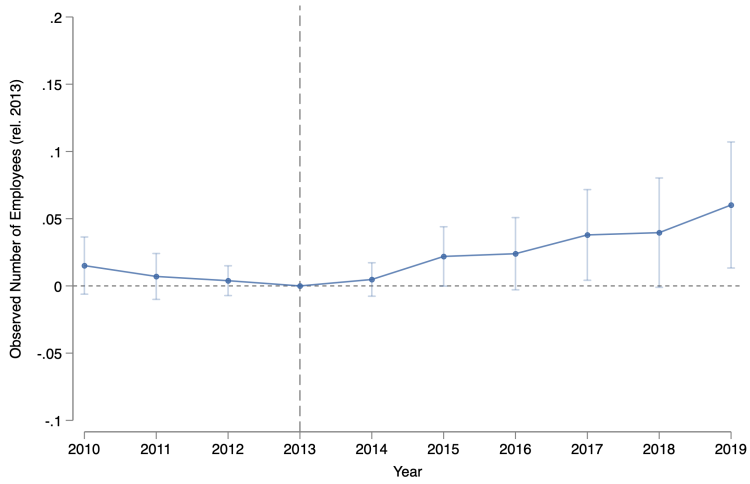
Notes: The figure shows the difference in the change in earnings between 2014 and 2013 (the pre-reform year) for individuals working in treatment firms (high exposure) relative to individuals working in control firms (medium-high exposure) for each €2000 gross annual earnings bin (relative to the first non-eligible earnings bin). The bin are defined based on the pre-reform earnings distribution.

Figure 8: FIRM-LEVEL EFFECTS OF THE TAX CREDIT ON EARNINGS: SPILLOVERS



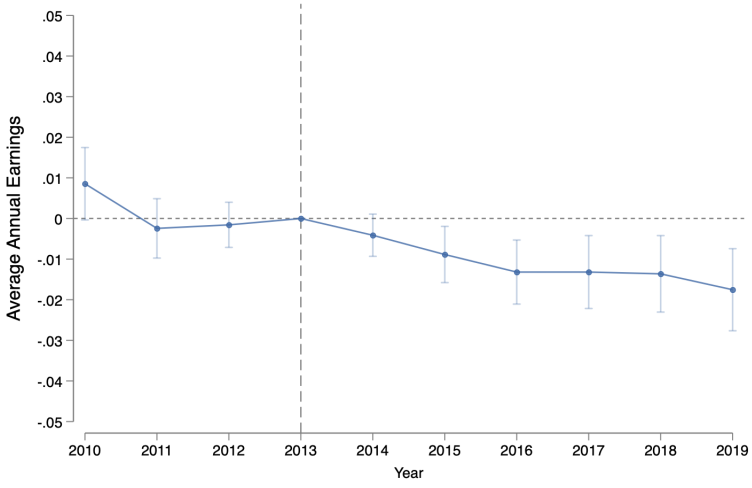
Notes: The figure shows the results from the difference-in-differences specification in equation 1. The dependent variable for the blue series is the log average firm-level gross earnings of high-earnings pre-reform non-eligible employees. The dependent variable for the red series is the log average firm-level gross earnings of low-earnings pre-reform non-eligible employees. Standard errors are clustered at the firm level.

Figure 9: FIRM-LEVEL EFFECTS ON OBSERVED NUMBER OF EMPLOYEES BY EXPOSURE



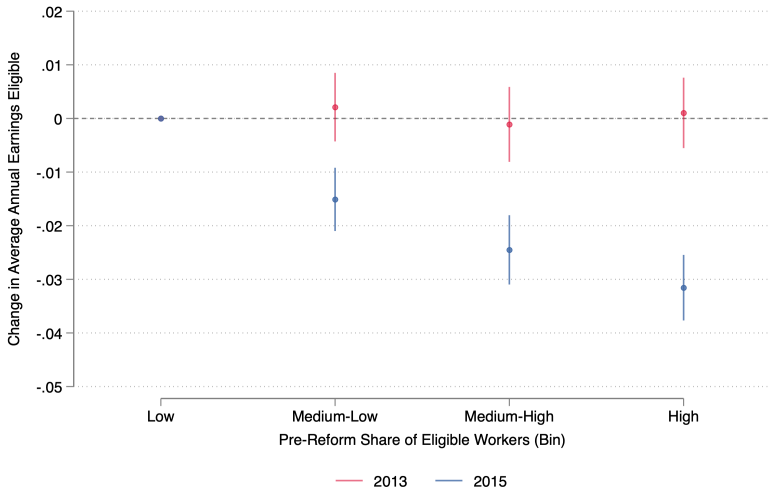
Notes: The figure shows the results from a difference-in-differences specification (equation 1) comparing firms with a high share of eligible employees (top quartile) to firms with a medium-high share of eligible employees (between the 50th and the 75th percentile) in the last pre-reform year. The outcome is the firm-level number of observed workers, normalized to 2013. Standard errors are clustered at the firm level.

Figure 10: FIRM-LEVEL EFFECTS: INCUMBENTS



Notes: The figure shows the results from the difference-in-differences specification in equation 1 estimated using as outcome variables the firm-level average gross annual earnings of incumbents workers eligible before the reform. Standard errors are clustered at the firm level.

Figure 11: FIRM-LEVEL EFFECTS OF THE TAX CREDIT BY FIRM EXPOSURE



Notes: The figures show the two-year change in average annual earnings of eligible employees by pre-reform exposure before (2013) and after (2015) the reform, relative to the bottom quartile of pre-reform exposure.

Tables

Table 1: SUMMARY STATISTICS

	Full Sample		**Eligible**	
	Mean	Std. Dev	Mean	Std. Dev
Annual Earnings	28,708.46	18,977.77	18,998.28	4,877.16
Age	42.4	9.54	41.09	9.54
Male	0.59	0.49	0.55	0.50
Temporary Contract	0.10	0.17	0.09	0.19
Working in Firm 50+	0.52	0.50	0.41	0.49
Eligible	0.51	0.49		
Observations	529,167		270,246	

Notes: This table shows summary statistics for 2013. The first two columns report descriptive statistics (mean and standard deviation) for the full sample while the last two columns report descriptive statistics for the subsample of individuals eligible for the tax credit (i.e., whose annual gross earnings are between €8,000 and €26,000). All monetary variables are expressed in Euros.

Table 2: FIRM DESCRIPTIVE STATISTICS BY SHARE OF ELIGIBLE EMPLOYEES IN 2013

	Low	**Medium-Low**	**Medium-High**	**High**
	(1)	(2)	(3)	(4)
Share Eligible	0.08	0.42	0.67	0.93
Gross Annual Earnings per Employee	39,239.6	27,494.1	22,683.8	18,280.8
Gross Annual Earnings per Eligible Employee	20,491.9	20,051.7	19,136.7	17,670.4
Share Temporary Workers	0.03	0.03	0.04	0.06
Large (50+)	0.876	0.860	0.836	0.861
Observations	10,451	3,536	2,660	3,381

Notes: This table provides summary statistics for a panel of firms with more than 3 employees sampled each year. The table provides statistics for four groups of firms based on their share of eligible employees in 2013. Column 1 considers firms whose share of eligible employees is in the first quartile (0-25) or equal to zero in 2013 (*Low Exposure*), column 2 considers firms whose share of eligible employees is in the second quartile (25-50) in 2013 (*Medium-Low Exposure*), column 3 considers firms whose share of eligible employees is in the third quartile in 2013 (50-75) (*Medium-High Exposure*) and column 4 considers firms whose share of eligible employees in 2013 is in the top quartile (*High Exposure*). All statistics are for 2013. All monetary variables are expressed in Euros.

Table 3: FIRM-LEVEL REGRESSION RESULTS: HIGH VS. MEDIUM-HIGH

	All Post Periods	**Short Run**	**Medium Run**
	2014-2019	2014-2016	2016-2019
Gross Annual Earnings Eligible (Log)	-0.0117***	-0.0086***	-0.0156***
	(0.00330)	(0.00312)	(0.00431)
Pass-through Estimate	0.224	0.165	0.298
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	49,533	49,533	49,533
Avg. Gross Annual Earnings Eligible	18371.16	18371.16	18371.16

Notes: The table reports reduced-form estimates obtained estimating the compact version of the difference-in-differences specification in equation 1, where the dependent variable is the (log) firm-level average of gross annual earnings of pre-reform eligible workers. The reduced-form estimate of column (1) corresponds to the difference-in-differences coefficients where the *post* event includes all years after 2013. The reduced-form estimate of column (2) and (3) reports the effect in the short-run (2014-2016) and medium-run (2017-2019) respectively. Standard errors are clustered at the firm level.

Table 4: FIRM-LEVEL REGRESSION RESULTS: CONTINUOUS TREATMENT

	All Post Periods	**Short Run**	**Medium Run**
	2014-2019	2014-2016	2016-2019
Gross Annual Earnings Eligible (Log)	-0.0545***	-0.0406***	-0.0727***
	(0.0045)	(0.0041)	(0.0060)
Pass-through Estimate	0.195	0.135	0.270***
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	100,021	100,021	100,021

Notes: The table reports reduced-form estimates obtained estimating the compact version of the difference-in-differences specification in equation 2, where the dependent variable is the (log) firm-level average of gross annual earnings of pre-reform eligible workers. The reduced-form estimate of column (1) corresponds to the difference-in-differences coefficients where the *post* event includes all years after 2013. The reduced-form estimate of column (2) and (3) reports the effect in the short-run (2014-2016) and medium-run (2017-2019) respectively. Standard errors are clustered at the firm level.

Table 5: FIRM-LEVEL REGRESSION RESULTS: SPILLOVER EFFECTS

	All Non-Eligible	**Non-Eligible Low**	**Non-Eligible High**
		26k-32k	>32k
Gross Annual Earnings Non-Eligible (Log)	-0.0051	-0.0156**	-0.0099
	(0.0066)	(0.0066)	(0.0106)
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	49,533	49,533	49,533
Avg. Gross Annual Earnings Eligible	18371.16	18371.16	18371.16

Notes: The table reports reduced-form estimates obtained estimating the compact version of the difference-in-differences specification in equation 1. The reduced-form estimates correspond to the difference-in-differences coefficients where the *post* event includes all years after 2013. The dependent variable in Column (1) is the (log) firm-level average of gross annual earnings of pre-reform non-eligible workers. The dependent variable in Column (2) is the (log) firm-level average of gross annual earnings of pre-reform non-eligible workers earning between €26,000 and €32,000. The dependent variable in Column (3) is the (log) firm-level average of gross annual earnings of pre-reform non-eligible workers earning more than €32,000. Standard errors are clustered at the firm level.

Table 6: FIRM-LEVEL REGRESSION RESULTS: EMPLOYMENT

	All Employees	**Eligible**	**Non-Eligible**
Observed N. of Employees	0.0228*	0.0826***	-0.0590
	(0.0136)	(0.0139)	(0.0436)
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	49,533	49,533	26,373

Notes: The table reports reduced-form estimates obtained estimating the compact version of the difference-in-differences specification in equation 1. The reduced-form estimate of Column (1) corresponds to the difference-in-differences coefficients where the *post* event includes all years after 2013. The dependent variable in Column (1) is the total firm-level observed number of employees normalized to 2013. The dependent variable in Column (2) is the total firm-level observed number of eligible employees normalized to 2013. The dependent variable in Column (3) is the total firm-level observed number of non-eligible employees normalized to 2013. Standard errors are clustered at the firm level.

Table 7: FIRM-LEVEL REGRESSION RESULTS: HETEROGENEITY

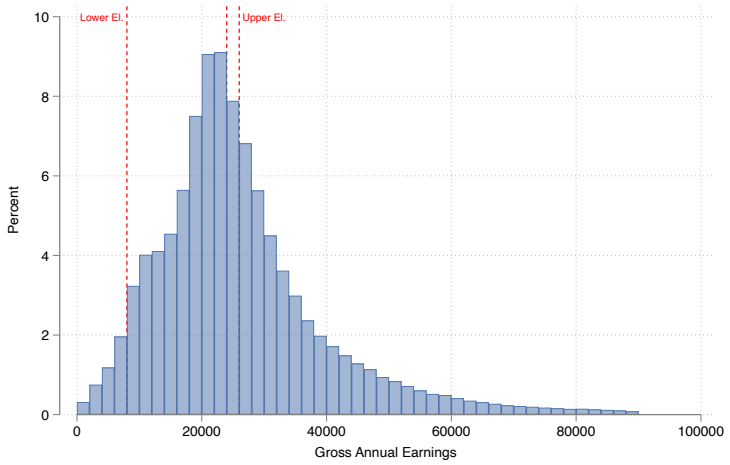
	(1)	(2)
	Small	**Large**
	<50	50+
Gross Annual Earnings Eligible	-0.0087*	-0.0152***
	(0.0049)	(0.0043)
Pass-through Estimate	0.166	0.291
Firm FE	Yes	Yes
Year FE	Yes	Yes
Observations	23,802	25,744

Notes: The table reports reduced-form estimates obtained estimating the compact version of the difference-in-differences specification in equation 1, estimated separately by firm size. The reduced-form estimates correspond to the difference-in-differences coefficients where the *post* event includes all years after 2013. Standard errors are clustered at the firm level.

Online Appendix

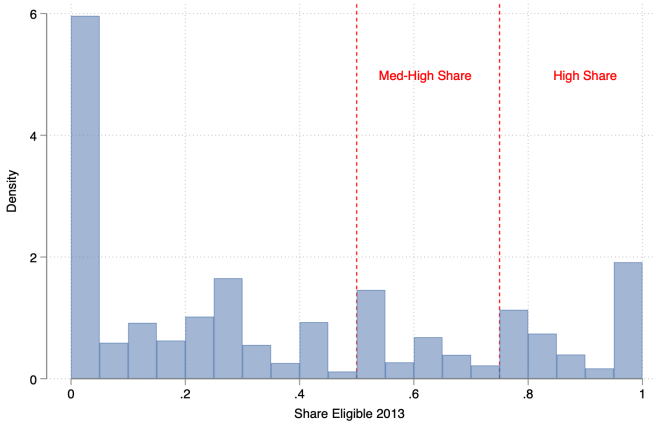
A Additional Figures and Tables

Figure A1: PRE-REFORM DISTRIBUTION OF EARNINGS LEVELS



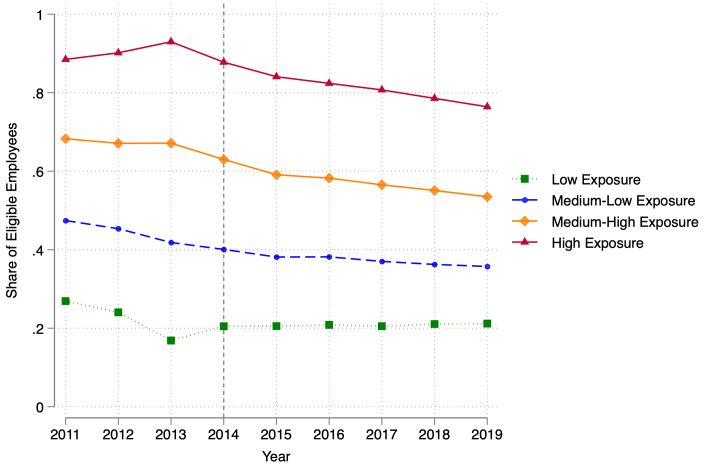
Notes: The figure depicts the distribution of employees by gross annual earnings in 2013. The vertical red lines indicate the lower and upper eligibility cutoffs for the tax credit.

Figure A2: CONSTRUCTION OF GROUPS



Notes: The figure depicts the distribution of share of eligible workers across firms in 2013 considering firms with at least three sampled workers in the pre-reform year.

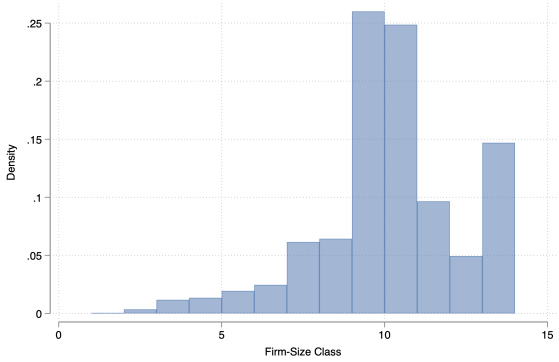
Figure A3: EVOLUTION OF SHARE OF ELIGIBLE WORKERS, BY 2013 SHARE OF ELIGIBLE WORKERS GROUPS



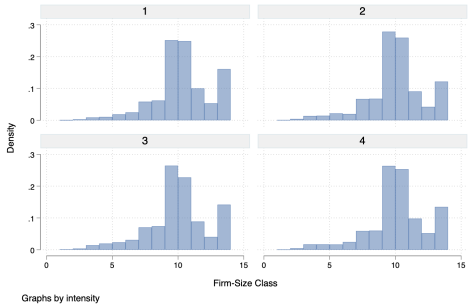
Notes: The figure depicts the average share of eligible workers in each year for the four groups of firms defined by the quartiles of share of eligible employees in 2013. The spike around 2013 is due to mean reversion: firms with a high share of eligible employees in 2013 tend to have a lower share before and after. The opposite is true for firms with a lower share of eligible employees in 2013. There is substantial persistence in the share of eligible employees across years.

Figure A4: Firm-Level Exposure And Firm Size: Three Sampled Workers

(a) Density of Firms by Firm-Size Class



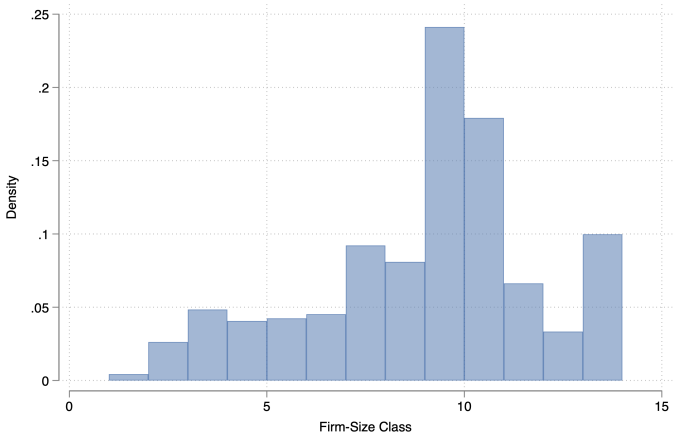
(b) Density of Firm-Size Class by Exposure



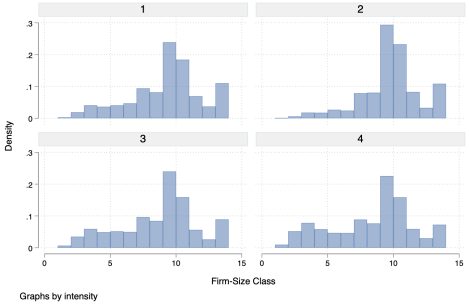
Notes: Panel A depicts the distribution of firms by firm-size class in 2013. Panel B depicts the distribution of firm-size class by firms exposure group, where 1 indicates low exposure firms (bottom quartile), 2 medium-low exposure (between the 25th and the 50th percentile), 3 medium-high exposure (between the 50th and the 75th percentile) and 4 high exposure firms (top quartile). Both figures consider firms with at least three sampled workers.

Figure A5: Firm-Level Exposure And Firm Size: Two Sampled Workers

(a) Density of Firms by Firm-Size Class

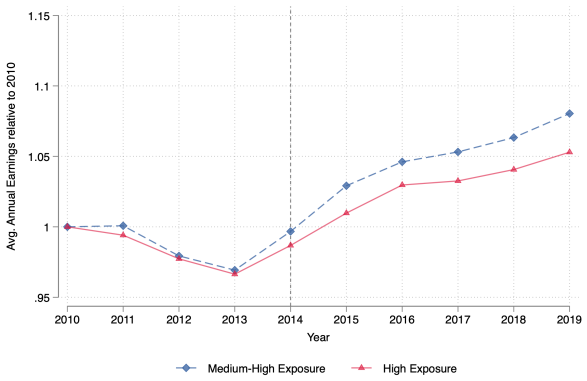


(b) Density of Firm-Size Class by Exposure



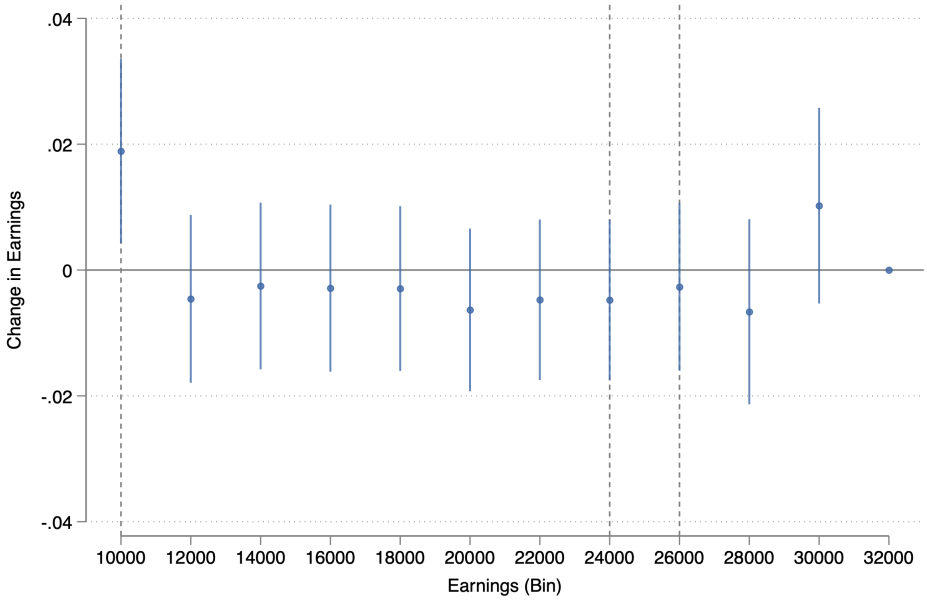
Notes: Panel A depicts the distribution of firms by firm-size class in 2013. Panel B depicts the distribution of firm-size class by firms exposure group, where 1 indicates low exposure firms (bottom quartile), 2 medium-low exposure (between the 25th and the 50th percentile), 3 medium-high exposure (between the 50th and the 75th percentile) and 4 high exposure firms (top quartile). Both figures consider firms with at least two sampled workers.

Figure A6: FIRM-LEVEL EFFECTS OF THE TAX CREDIT ON EARNINGS OF ELIGIBLE: DESCRIPTIVE EVIDENCE



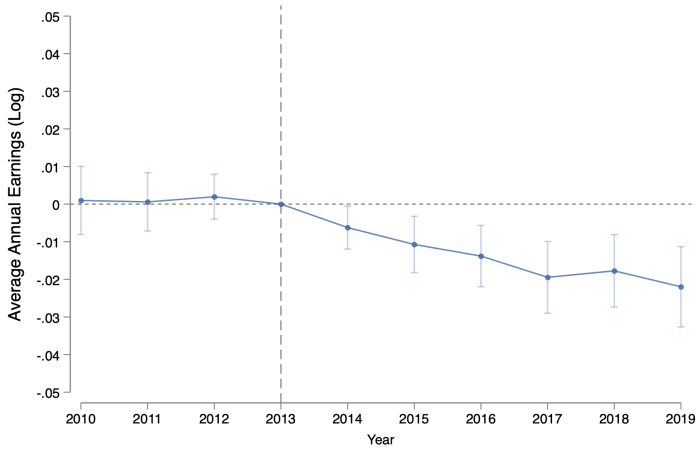
Notes: The figure traces out the evolution of the raw average annual earnings for eligible workers (relative to 2010) in medium-high exposure firms (firms with a pre-reform share of eligible employees between the 50th and 75th percentile) and high exposure firms (firms with a pre-reform share of eligible employees in the top quartile of the distribution).

Figure A7: FIRM-LEVEL EFFECTS OF THE TAX CREDIT ACROSS THE EARNINGS DISTRIBUTION: PLACEBO



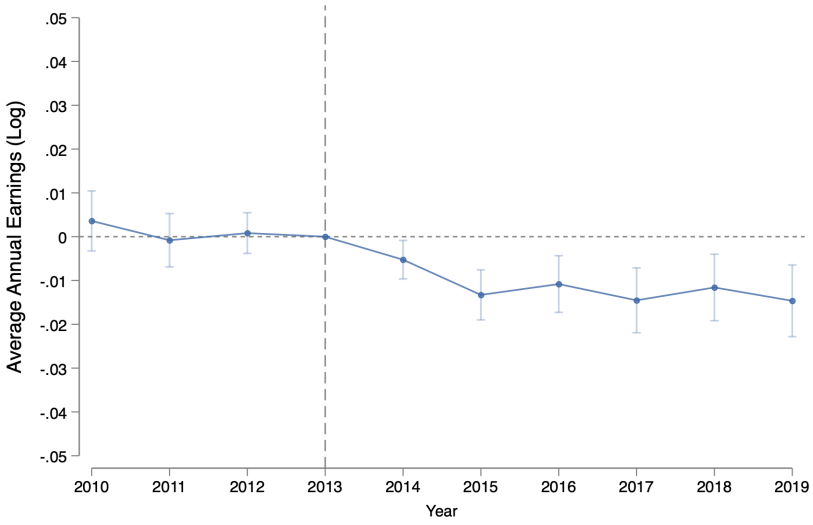
Notes: The figure shows the difference in the change in earnings between 2013 and 2012 for individuals working in treatment firms (high exposure) relative to individuals working in control firms (medium-high exposure) for each €2000 gross annual earnings bin (relative to the first non-eligible earnings bin). The bin are defined based on the pre-reform earnings distribution.

Figure A8: FIRM-LEVEL EFFECTS OF THE TAX CREDIT ON EARNINGS: DIFFERENT TREATMENT DEFINITIONS



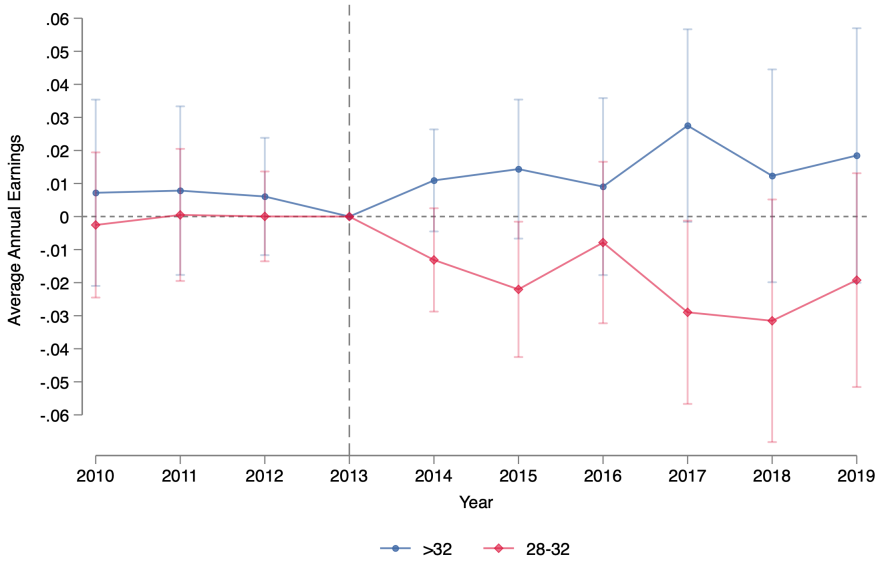
Notes: The figure shows the results from a difference-in-differences specification (equation 1) comparing firms with a medium-high share of eligible employees (between the 50th and the 75th percentile) to firms with a medium-low share of eligible employees (between the 25th and the 50th percentile) in the last pre-reform year. The figure shows the results where the outcome is the log of the firm-level average annual earnings for pre-reform eligible workers. Standard errors are clustered at the firm level.

Figure A9: FIRM-LEVEL EFFECTS OF THE TAX CREDIT: ROBUSTNESS TO RESTRICTIONS



Notes: The figure shows the results from a difference-in-differences specification (equation 1) comparing firms with a high share of eligible employees (top quartile) to firms with a medium-high share of eligible employees (between the 50th and the 75th percentile) in the last pre-reform year. The outcome is the log of the firm-level average gross annual earnings for pre-reform eligible workers. The sample consider firms with at least two sampled workers. Standard errors are clustered at the firm level.

Figure A10: FIRM-LEVEL EFFECTS: SPILLOVERS, ROBUSTNESS TO DEFINITIONS



Notes: The figure shows the results from the difference-in-differences specification in equation 1. The dependent variable for the blue series is the (log) average firm-level gross earnings of high-earnings pre-reform non-eligible employees. The dependent variable for the red series is the (log) average firm-level gross earnings of low-earnings pre-reform non-eligible employees. Standard errors are clustered at the firm level.