Firm Heterogeneity and the Incidence of Earned Income Tax Credits: Evidence from Italy

Valeria Zurla*

February 8, 2024

Abstract

This paper uses administrative data to analyze the incidence effects of a large, employer-administered EITC program in Italy. In a setting that allows to disentangle the wage from the employment effects of EITCs, I find no effect on gross wages at the market level. I then explore the role of firm-level mechanisms as determinants of tax incidence. The reform generates a firm-level experiment which creates more or less exposed firms as a share of their pre-reform eligible workers. I find significant heterogeneity in responses across firms. Earnings of eligible workers in more exposed firms decrease compared to comparable less exposed firms. Highly exposed firms capture up to 30% of the transfer. The effect is monotonic in the share of eligible workers, suggesting that pay-equity concerns are not the main drivers of the response. Both higher rent-seeking incentives or higher salience can explain the results. The results show significant heterogeneity in the incidence of tax credits across firms and highlight that firm-level channels in the transmission of incidence of wage subsidies are likely to be significant.

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

*CSEF, University of Naples Federico II, Population Wellbeing Initiative - UT Austin. Email: valeria.zurla@unina.it. I am very grateful to John Friedman, Emily Oster, and Neil Thakral for their invaluable guidance and feedback. I thank Anna Aizer, Brian Knight, Andrew Whitten, Marco Petterson, Pablo Garriga, and participants in the Brown Applied Microeconomics Seminar and the Health Breakfast, the NTA 2023 Annual Conference for their comments and suggestions. The INPS LoSaI data were provided by the Ministero Italiano del Lavoro e delle Politiche Sociali. I gratefully acknowledge financial support from the UniCredit Foundation. A version of this paper was previously circulated with the title "Firm Responses to Earned Income Tax Credits: Evidence from Italy".

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 the design 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.

A prime example of these types of programs are Earned Income Tax Credits (EITCs) which are among the most popular transfer programs that governments use to support low-income individuals while, at the same time, sustaining labor force participation. Despite a large body of literature has focused on evaluating the employment effects of these transfers, still relatively little is known about the effects of the introduction of EITCs on wages.

Estimating the wage effects of EITCs comes with several challenges. The standard approach in public finance to this problem has focused on market-level elasticities of aggregate labor supply and demand, completely abstracting from the role of firms. However, the importance of firm-level channels in the transmission of incidence of wage subsidies is likely to be significant. A growing literature has highlighted the role that firms play in wage determination and wage setting (Card et al., 2012; 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 (US: Advanced Earned Income Tax Credit, UK: Working Family Tax Credit, Switzerland: Familienzulagen, Argentina: Asignaciones Familiares, Brazil: Salário Família).

Understanding how firms react to EITCs and the mechanisms through which the economic incidence is shifted between workers and firms, therefore, becomes crucial to analyzing their incidence and welfare effects. In this paper, I investigate the role of firms in the incidence of EITCs by analyzing the introduction of a large, employer-administered EITC program in Italy. I will start with a broad research question: what are the wage effects of EITCs? Then, I will ask: What role, if any, do firms play in shaping the incidence of EITC programs?

I plan to 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*, which was introduced in 2014 with the stated objective of supporting low-income workers, stimulate consumption and sustain economic growth. Its introduction was unexpected and, according to many, motivated by electoral reasons[1]. 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 program is large both in terms of workers (5% of median income of workers) and in terms of government spending. The tax credit was distributed to all employees with annual gross earnings between €8,000 and €26,000, 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 based on their prediction of the annual income they would pay the workers. They distributed the tax credit monthly directly in the workers' paychecks. I argue that a number of peculiar characteristics of this program make it particularly suitable to study the wage effects of EITCs. For example, the tax credit was distributed to all employees with annual gross earnings between €8,000 and €26,000, regardless of personal characteristics.

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, occupation and type of contract, worker demographics, and firm characteristics.

I first outline a simple conceptual framework to guide the empirical analysis of the wage effects of the introduction of the tax credit. I start by describing the predictions of the standard tax incidence model and the empirical approach to identify the incidence of the program within this setting. I then discuss two main challenges of the standard model. First, given the structure of most EITC programs, it is hard to find a setting where it is possible to isolate the wage from the employment effects of EITCs. I argue that the

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

Italian setting, where EITC eligibility depends only on earnings and no other personal or family characteristics overcomes some of these challenges. The second main challenge in considering the incidence of EITC programs is that the standard approach assumes that firms passively accept market-level wages. I consider different reasons why firm-level exposure to the program might matter for the final incidence of the tax credit. First, more exposed firms may find it more worthwhile to engage in rent-seeking behavior or to overcome potential adjustment costs. Firm exposure to the policy might matter for the final incidence of the tax credit through pay-equity concerns. Finally, the higher the exposure to the policy, the more salient the tax credit was to employers, given the employers' role in the administration of the credit.

I start the empirical analysis by investigating the wage effects of the introduction of the EITC program at the market level. I test the implications of the standard model in my setting by comparing the evolution of gross (pre-tax and transfer) and net (after-tax and transfer) labor earnings of eligible workers to similar ineligible workers (where eligibility is defined pre-reform) and checking for discontinuities in these measures by eligibility after the reform. I find that, before the reform, gross and net earnings for eligible and ineligible workers trended similarly. After the reform, the gross wages of eligible workers did not adjust in response to the EITC. On the other hand, net wages of eligible workers exhibited a clear discontinuity. These results represent suggestive evidence that the incidence of the tax credit was fully on workers.

Comparing eligible and ineligible workers in the aggregate might not be enough to fully gauge the wage effects of the introduction of the program, as highlighted in the conceptual framework. In the second part of the paper, I therefore, take advantage of the fact that the reform not only generates an individual-level experiment but also a firm-level experiment. The key idea behind the firm-level experiment is that the reform can generate firm-specific reactions depending on a firm's share of ex-ante eligible workers. In particular, firms may be more likely to find it worthwhile to engage in rent-seeking behavior when there is a large share of workers to extract rents from.

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 by comparing the evolution of their outcomes within a difference-in-differences framework, following an empirical strategy widely used in the literature (Draca, Machin, and Van Reenen, 2011; Harasztosi and Lindner, 2019; Saez, Schoefer, and Seim, 2019). Taking advantage of persistent between-firm variation in the share of eligible workers before the reform, I find that eligible workers' earnings 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 decrease by around 2% in highly exposed firms compared to lower eligible firms. These estimates are compatible with a pass-through to employers of around 30%. The comparability of firms with different shares of eligible workers is supported by comparing their characteristics and the absence of pre-trends.

I perform several checks to assess the robustness of the results. 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 ensure the firm-level results are not driven by compositional effects. I also study the effect of the tax credit through-out the entire earnings distribution using a similar approach to the one of Cengiz et al. (2019). The results are also robust to different modeling choices and to the estimation of a treatment intensity model, using the pre-reform share of eligible workers as a proxy for treatment intensity.

Overall, the results show significant heterogeneity in earnings responses across firms. What are the mechanisms behind these responses? The effects are monotonic in the share of eligible workers, providing suggestive evidence that are not explained by pay-equity concerns. The fact that rent capture of the tax credit is higher in firms with a higher share of eligible workers can be explained by at least two mechanisms. On the one hand, firms may find it worthy to engage in rent-seeking behavior when there is a large share of workers to extract rents from. On the other hand, salience can also partially explain the results. I find suggestive, even though, not conclusive evidence, that the first mechanism might prevail. I further break down the aggregate effects by firm size and unionization level. I find that the effects is stronger in large firms with 50 or more employees and in firms that have a lower unionization rate. Finally, I test whether there are spillover effects to non-eligible workers. While at first glance, this does not seem to be the case, the results on non-eligible workers mask significant heterogeneity. I find that earnings

of lower-earnings non-eligible workers decrease in more exposed firms relative to less exposed firms.

This paper contributes to different strands of literature. First of all, it contributes to the extensive literature on the effects of Earned Income Tax Credits, which have mostly focused on their labor supply effects (surveyed by Hotz, 2003; Eissa and Hoynes, 2006; Meyer, 2010; Nichols and Rothstein, 2015; Hoynes, 2019). Most of this literature considers the equilibrium effects of the EITC to be negligible, implicitly assuming fixed wage rates. This paper contributes to the narrower literature on the incidence and wage effects of these programs. Rothstein (2008, 2010) and Leigh (2010) analyze the incidence of the EITC in the US. Rothstein (2008) finds that low-skilled mothers in the US keep only 70% of every dollar they receive in EITC because of wage decreases. Leigh (2010) finds that a 10% increase in the generosity of the EITC leads to wage reductions of 5% for high-school dropouts. Azmat (2019) estimates a similar effect focusing on male claimants of the Working Family Tax Credit in the UK. Bennmarker, Calmfors, and Seim (2014) investigate how both unemployment benefits and EITCs influence wages through their effects on the net replacement rate for the unemployed. These studies assess the wage effects of EITCs using worker-level variation and mostly ignoring firm-level factors that can influence the transmission of incidence. Gravoueille (2022) studies the wage and employment effects of wage subsidies using a large 2015 national-level reform in France that provides additional financial support to poor working households. This paper contributes to this literature by showing that neglecting to account for the role of firms in the incidence analysis misses an important part of the story. Finally, most of these studies remain inconclusive because of the difficulty in identifying wage effects using credible research designs and the unavailability of administrative and/or panel data.

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 most recent examples among such studies are Saez, Schoefer, and Seim (2019), documenting the effect of a payroll tax cut for young workers in Sweden, and Bíró et al., 2022, investigating the implications of payroll tax cuts in Hungary in a setup where firms play an active role in wage determination.

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) exploit a cohort-based discontinuity in social security contributions tax rates and show that there is full pass-through of employers' contributions to employers and of employees' contributions to employees. Bozio, Breda, and Grenet (2017) also find limited pass-through of employer social security contributions to wages for reforms that increased social security contributions with no tax-benefit linkage. On the other hand, they find evidence of full pass-through to workers in the case of a strong and salient relationship between contributions and expected benefits. Garriga and Tortarolo (2021) exploit a reform in Argentina that shifted the disbursement responsibility of child benefits from employers to a government agency in a staggered fashion and find that in firm-based systems employers are more able to extract rents.

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. They find that households that received the tax rebate increased their monthly consumption, in particular for food and means of transportation. To the best of my knowledge, this is the first study investigating the effects of the program on wages. Villamaina and Acciari (2023) study the labor supply responses to the tax credit and find no intensive margin responses to the tax credit around the €24,000 kink point.

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. Section 4 tests the implications of the standard model looking at the evolution of gross and net labor earnings for eligible workers compared to similar ineligible workers. Evidence on the firm-level responses to the introduction of the program are presented in Section 5. Section 6 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 in 2014[2]. 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[3] 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. Moreover, although the tax credit is targeted at employees only, the relevant income measure for eligibility is total annual gross income and not annual gross labor income.

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 paycheck of workers (Figure 2) by the employer. It either takes the form of reductions in the tax withheld or, since the tax credit is refundable, of a transfer. While the tax credit is distributed monthly, eligibility is based on the annual gross income earned at the end of the year. The employer determines the eligibility of a given worker based on calculations on the annual income that the employer expects to pay the worker. This implies that, in practice, the eligibility for the tax credit is effectively based on annual gross labor income. Because annual gross income is not known with certainty at the moment of the distribution of the tax credit, this mecha-

[2] *Ministero dell'Economia e delle Finanze*

[3] Provided that the tax due on income is larger than the tax deductions the worker is entitled to (*INPS*).

nism inevitably implies the possibility of mistakes that are corrected through adjustments during tax filing[4].

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.

The introduction of the tax credit generates three important points in the budget constraint of individuals: 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. This point corresponds to a *notch*[5]. When annual gross income exceeds €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: $\frac{(26{,}000 - annual\ gross\ income) \cdot 960}{2{,}000}$. Note that the phase-out cutoff of €24,000 constitutes a *kink*[6] in the budget constraint of individuals since it leads to a discrete increase in the marginal tax rate. The phase-out region is extremely steep and characterized by an extremely high effective marginal tax rate: almost 70% compared to the standard marginal tax rate of 31.5%.

2.2 Data

I use confidential administrative data from the Italian Social Security Institute (INPS) on a 7% random sample[7] of private-sector employees. My primary data source is matched employer-employee records at the annual level for the period 1985-2019. For each worker-

[4] 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.

[5] Kleven and Waseem (2013), Kleven (2016).

[6] Saez (2010).

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

firm record, the following information is available: beginning and end date of the contract, alongside the underlying motivation for termination (e.g., layoff, resignation); 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 worked, and a unique firm and worker identifier. I link these records to workers' and firms' registers containing baseline information, such as gender and age of employees and opening date, sector, and location of businesses.

For my wage information, I will rely on the measure of annual gross labor earnings. This is a potential drawback but a common feature of administrative social security data (see e.g. German IAB data). I do this mostly because I do not have any information on hours. However, as a robustness check, I will also look at the effects of the reform on daily wages, an outcome variable which suffer less from concerns on blending wage and labor supply outcomes. Following the literature (Saez, Schoefer, and Seim, 2019, Bíró et al., 2022), I will define net annual earnings as annual wage earnings after the transfer. This net wage measure is calculated before income tax and employee social security contributions are deducted. All monetary outcomes are in real terms, with a base year of 2013. For the main analysis, I restrict the sample to individuals aged 25-65 years old and working 52 weeks to help isolate wage effects from labor supply responses. Notice, however, that Villamaina and Acciari (2023)find no intensive margin responses to the tax credit around the €24,000 kink point, making the isolation of the wage effects from labor supply responses easier.

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 56% of the employees in the 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 obviously lower for eligible employees, but there are no important differences in terms of weeks worked, age, gender or share of workers employed with temporary contracts.

3 Conceptual Framework

In this section, I briefly describe the conceptual framework that can explain the mechanisms behind the incidence of the tax credit, and that will guide my empirical analysis. The standard approach in public finance suggests that the market-level elasticities of labor supply and demand determine the employment and wage impacts and the incidence of EITCs. When the tax credit is introduced, the after-tax and transfer wages of workers eligible for the tax credit change discontinuously. If demand is less than perfectly elastic, the increase in after-tax and transfer wages for eligible workers induces labor supply responses that bid down the pre-tax and transfer wages for eligible workers until a new equilibrium is reached. This implies that employers of eligible workers capture a portion of the intended EITC transfer.

Two main challenges come with this standard approach. First, given the structure of most EITC programs, it is hard to find a setting where it is possible to isolate the wage from the employment effects of EITCs. Most of the literature on the impact of EITC programs has focused on workers, estimating labor supply effects of the program by comparing individuals from the same labor market, some eligible for the tax credit and some not (for example, because of personal characteristics). However, while this strategy is suitable for studying the employment effects of EITCs, it does not allow for separate identification of the wage effects, as treatment and control workers participate in the same labor market, and one would expect wage effects to be the same for both groups. Identifying wage effects requires a research design that compares two separate labor markets, where workers not substitutes and at the same time are similar enough to interpret wage effects as due to the EITC and not to other determinants of wages. One would need between-skill comparisons, which are informative about labor demand (Rothstein, 2008; Nichols and Rothstein, 2015). In this sense, the Italian setting, where EITC eligibility depends only on earnings and no other personal or family characteristics, under the assumption that the position in the earnings distribution is a good proxy for skill level, overcomes some of these challenges and is therefore particularly suitable for this analysis.

The second main challenge in considering the incidence of EITC programs is that the

standard approach assumes that firms passively accept market-level wages and, therefore, the incidence 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.

I 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 reform can generate different firm-level exposure to the program depending on a firm's share of ex-ante eligible workers. I use this firm-level experiment to test whether firms more affected by the program react differently than firms less affected, to shade light on how firms can respond to EITCs and the mechanisms through which the economic incidence is shifted between workers and firms.

There are different reasons why firm-level exposure to the program might matter for the final incidence of the tax credit. Overall, firms may be more likely to engage in rent-seeking behavior when there is a large share of workers to extract rents from. This higher rent-seeking behavior could be due to the fact that the higher the share of eligible employees in the firm, the more worthwhile it is for firms to engage in rent-capture. Along the same lines, the higher a firm's exposure to the program, the more likely firms are to overcome adjustment costs to respond to the policy or potentially the lower these adjustment costs are (Chetty et al., 2011).

Firm exposure to the policy might matter for the final incidence of the tax credit through pay-equity concerns. If pay-equity concerns matter, pass-through to firms should be higher in firms with lowest and highest exposure to the program and lower when there is a mix of eligible and non-eligible workers.

Another important dimension in this context is the *salience* of the transfer to employers. Notably, employers played a key role in the administration of the tax credit. They determined the eligibility of employees based on their prediction of the annual income they would pay the workers. They distributed the tax credit monthly directly in the workers' paychecks. The higher the exposure to the policy, the more salient the tax credit was to employers, keeping constant the salience to employees.

4 The Aggregate Effects of the Tax Credit on Earnings

As mentioned in the Section 3, the Italian setting is a particularly suitable environment to investigate the aggregate effects of the introduction of the tax credit on wages. In this Section, we investigate the effects of the introduction of the tax credit at the market-level, assuming that the incidence 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.

To do so, we first present descriptive evidence on the evolution of annual earnings of eligible individuals before and after the introduction of the tax credit. To test the implications of the standard competitive model, I look at the evolution of annual gross (pre-tax and transfer) labor earnings and annual net (pre-tax after transfer) labor earnings for eligible workers, compared to similar ineligible workers. The main idea of this exercise is to check for discontinuities in one of these earnings measures for eligible workers after the reform. By definition, both earnings concepts cannot be continuous after the reform. If gross annual earnings are continuous, we can interpret it as suggestive evidence that the incidence of the tax credit is on worker's annual net earnings. On the other hand, if net annual earnings are continuous, firms capture the incidence of the tax credit. Plotting the net and gross earnings of comparable workers is a common way to show evidence of the incidence of the tax credit (Saez, Schoefer, and Seim, 2019). The fact that the tax credit was large, applied to all eligible workers (with no differences based on family characteristics), and automatically distributed is crucial to interpret the results of this exercise as credible evidence of the incidence of the tax credit. Moreover, to be able to interpret the results as reflecting wage effects, it is crucial to construct a control group of ineligible workers that is similar enough to the treatment group and unlikely to be close substitutes.

Figure 4, plots the evolution of average annual gross (dashed line) and net (solid line) earnings for eligible (blue) and similarly ineligible (red) workers relative to 2013, the last pre-reform year. I define eligible workers as workers earning, in 2013, between €20,000 and €23,000. I define ineligible workers as workers earning, in 2013, between €28,000 and €31,000. The definition of the groups is such that the treatment and control group

are not likely to respond by reaching the kink point and therefore the results less likely to reflect labor supply decisions. This assumption is also supported by the evidence in Villamaina and Acciari (2023), which find no intensive margin responses to the tax credit around the €24,000 kink point. Moreover, given the differences in earnings, the control group are unlikely to be close substitutes to the treatment group.

The first thing to notice is that, before the reform, the earnings of the treatment and the control group trended similarly, suggesting that ineligible workers are a good counterfactual for the earnings evolution of eligible workers absent the reform. As an additional check, Table 2 shows summary statistics for the treatment and control group in 2013. By construction, workers in the control group have higher gross annual earnings than workers in the treatment group. Workers in the control group are also more likely to be male and slightly older. After the reform, the gross annual earnings of control group workers are smoothly increasing; the same is true for the treatment group. Notably, the gross annual earnings of eligible workers exhibit no discontinuity after the reform with the introduction of the transfer. This evidence suggests that the gross annual earnings of eligible workers do not seem to adjust in response to the reform. As for net annual earnings, they evolve smoothly before and after the reform for the control group. On the other hand, net annual earnings of eligible workers display a sharp discontinuity after the reform, an almost one-to-one increase relative to the transfer. The combined results suggest that the tax credit incidence was entirely on workers.

Robustness I test the sensitivity of the results to different definitions of the treatment and control groups. One particular concern is that the policy change could change the incentives of workers' in the treatment group to stay within the tax subsidy range (below €24,000) or the incentives of workers' in the control group to move within the tax subsidy range.

Figure A1 shows the results with definitions of treatment and control groups increasingly further away from the phase-out cutoff. In particular, Panel A shows the results where the treatment group is defined as workers earning, in 2013, between €20,000 and €22,000 and the control group is defined as workers earning, in 2013, between €28,000

and €30,000. Panel B shows the results where the treatment group is defined as workers earning, in 2013, between €19,000 and €21,000 and the control group is defined as workers earning, in 2013, between €29,000 and €31,000.

The results of these robustness checks confirm the main results, while at the same time reflecting the main trade-off in selecting the treatment and control group: using a bandwidth further away from the phase-out cutoff increases the likelihood of having dissimilar earning trends between the treated and control workers, as reflected in Panel B.

5 The Effects of the Tax Credit on Earnings at the Firm-Level

As pointed out in Section 3, comparing eligible and ineligible workers in the aggregate might not be enough to fully gauge the wage effects of the introduction of the program. 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. As explained in Section 3, the key idea behind the firm-level experiment is that the reform can generate firm-specific reactions depending on a firm's share of ex-ante eligible workers. I test for differential firm responses by firms' concentration of eligible employees. I exploit firm-level variation in exposure to the policy generated by preexisting, persistent composition of their workforce to understand whether firms more exposed to the policy respond differently to the introduction of the program and through which mechanisms.

5.1 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 concentration of eligible workers following a methodology popular in the minimum wage literature (Draca, Machin, and Van Reenen, 2011, Harasztosi and Lindner, 2019) and that was recently applied to study the firm-level transmission of incidence in the context of payroll taxes (Saez, Schoefer, and Seim, 2019).

Specifically, I consider a panel of firms active every year from 2010 to 2019. I divide the panel of firms into four groups based on the quartiles[8] of the share of eligible employees they employ in the baseline year, 2013. I define firm exposure to the policy in the baseline year in order to abstract from potential behavioral responses to the policy.

Table 3 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 extremely different in terms of observable characteristics. The characteristics of medium-high and high exposure firms are particularly similar: the share of temporary workers, the gross annual earnings per eligible employee and the share of large firms are comparable. The distribution of firms across industries is also similar, with almost the majority of firms in both groups operating in manufacturing. The two groups of firms mostly differ in terms of gross annual earnings per eligible employee.

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 share eligible 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 multiple period difference-in-differences model. I estimate the following model, at the firm-level:

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

where $y_{f,t}$ is a firm-level outcome of interest such as the average earnings of eligible

[8] 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.

employees, η_f are firm fixed effects (which capture time-invariant heterogeneity across firms) and η_t are year fixed effects. $D_t^k = \mathbb{1}(t = t_0 + k)$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.

Figure A2 shows the source of variation I will use for the empirical strategy. It shows the distribution of firms in terms of the share of eligible workers in 2013. Note that 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.

Critical to this empirical strategy is the persistence of the share of eligible workers across years within firms. If firms respond to the policy by changing their composition of workers, the estimates might be biased. For example, if, after the introduction of the policy, the share of eligible employees at medium-high exposure firms strongly decreases, we would observe a decrease in average gross earnings in high-exposure firms relative to medium-high exposure firms that would be wrongly attributed to differential responses between groups of firms but would instead be due to composition effects. Figure 5 depicts the average share of eligible workers each year for each group of firms. There is considerable persistence in the share of eligible employees across groups of firms and years. This evidence is reassuring and increases the confidence that compositional effects do not drive the effects. 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.

Note that the fact that I observe a sample of workers presents additional challenges for the firm-level analysis, as there might be measurement errors in calculating the share of eligible workers at the firm level. Therefore, to usefully characterize the share of eligible workers measure, I consider firms with at least three sampled workers in my baseline analysis. This restriction, however, implies that the sample considered for the baseline analysis will be skewed towards bigger firms, which could imply that highly exposed firms are over-represented in larger firms.

I do different things in order to address these concerns. First, I show that my results are robust to considering firms with at least two sampled workers instead of three. I also compare the distribution of firms by firm-size class when considering firms with at least three vs. at least two sampled workers. As shown in Figure A3 and A4, Panel A, the sample, when considering firms with at least three sampled workers, is more skewed towards bigger firms. However, importantly, firm size does not seem to be correlated with the firm-level share of eligible employees, as shown in Figure A3 and A4, Panel B.

5.2 Results

Figure A5 plots the raw average gross annual earnings of eligible and non-eligible workers in the treatment (*high exposure firms*) and the control group (*medium-high exposure firms*) (normalized to 2010). Panel A 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. On the other hand, annual earnings of non-eligible workers exhibit a similar trend in the treatment and control groups, both before and after the reform, as shown in Panel B. Although this evidence is only descriptive, it helps show that the treatment and control groups followed similar trends before the reform.

Figure 6, 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, suggesting that the control firms are likely a good counterfactual for the treatment firms.

Annual earnings of the average pre-reform eligible worker in high-exposure firms decreased when the transfer was 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, and there was still uncertainty on whether the program would be extended to 2015. Overall, the impact on the annual earnings of pre-reform eligible employees is around 1-2 percent over the years 2015-2019 in more exposed firms relative to less exposed firms.

Figure 6, Panel B reports the same estimates for the average earnings of workers who were not eligible for the credit in the pre-reform year. This within-firm group of workers can be identified as a group not directly affected by the tax credit. The only way through which this group's earnings might be affected is through spillover effects. The figure shows no differential trend in average earnings of non-eligible workers between treatment and control group firms. At first glance, this result can be interpreted as suggestive evidence that there were no spillover effects from the introduction of the tax credit to non-eligible individuals.

To quantify the results, Table 4 reports reduced-form and pass-through quantitative estimates of the results. 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 reduced-form estimate of column (2) and (3) reports the effect in the short-run (2014-2016) and medium-run (2017-2019) respectively. The table summarizes the results from previous figures and shows that the annual earnings of the average pre-reform eligible worker in high exposure firms decreases by 0.7 and 1.4 percentage points relative to lower exposure firms. These estimates translates in a pass-through rate approximately between 13 and 27 percent.

Continuous Treatment The results presented above are robust to different modeling choices. In particular, instead of comparing firms in the top two quartiles of the distribution, I estimate a treatment intensity model, using the pre-reform share of eligible workers as a proxy for treatment intensity. More in details, I estimate the following model:

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

where $y_{f,t}$ 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) and η_t are year fixed effects. $D_t^k = \mathbb{1}\left(t = t_0 + k\right)$where $t_0 = 2013$. $Share\ Eligible\ 2013_f$ is the share of eligible workers in firm f in 2013. 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.

Figure 7 reports the results of the estimation of equation 2. The figure shows that the results are robust to this different specification that uses more variation in treatment dosage. The figure shows the results when the outcome are the average annual earnings of pre-reform eligible employees. Using this specification, the results are qualitatively confirmed but, notably, are bigger in magnitude. The impact on the annual earnings of eligible employees was around 0.4-0.8 percentage points over the years 2015-2019 in more exposed firms relative to less exposed firms. This result points towards the idea that the ability or willingness of firms to adjust their wage-setting policies is a direct function of the share of eligible employees they employ and it is consistent with more exposed firms having more incentives or ability to respond to the introduction of the tax credit.Section 5.3 will investigate this mechanisms in more detail.

Composition One concern with the firm-level analysis 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:

$$y_{ift} = \eta_{if} + \eta_t + \sum_{k=-m}^{q} \beta_k \left(T_i \cdot D_t^k \right) + X'_{ift}\delta + \varepsilon_{ift} \tag{3}$$

where the outcome now is the individual-level annual earnings, $D_t^k = \mathbb{1}(t = t_0 + k)$ where $t_0 = 2013$ 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 8 reports the results of the estimation of equation 3 for the sample of eligible for the tax credit in 2013. The figure shows that the annual earnings of 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, suggesting that the parallel-trend assumption is likely to hold. Note that the decrease is similar in magnitude to the firm-level estimates suggesting that the firm-level estimates are not mainly capturing changes in the composition of workers at firms rather than changes in earnings for individual workers.

Effects across the wage distribution I also study the effect of the tax credit throughout the entire earnings distribution using a similar approach to the one of Cengiz et al. (2019). Figure 9 reports 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. Note that there is a clear and significant negative difference in the change in earnings for individuals in treatment firms relative to control firms for workers who, in the pre-reform year, were eligible for the tax credit. For non-eligible earnings bins, workers in the two groups of firms appear to have had similar changes in earnings.

Other Robustness Checks Figure A9 reports the results of the same bin-to-bin analysis for placebo years and 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 A6 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 A8 shows that the results are not dependent on this restriction by relaxing the restriction and considering firms with at least two sampled workers.

An additional concern relates to the fact that, using annual earnings as the main outcome variables, does not allow to fully separate wage and labor supply outcomes, even when restricting the sample to workers for which labor supply responses are likely to be negligible. Figure A7 reports the results of the estimation of equation 1 where the dependent variable is the average daily wage of pre-reform eligible workers.

5.3 Heterogeneity and Potential 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 27% of the transfer. In this section, I discuss some competing channels that could explain this response. As pointed out in Section 3, there might be a number of reasons behind the differential earnings response of firms differentially exposed to the program. Overall, firms may be more likely to engage in rent-seeking behavior when there is a large share of workers to extract rents from. This higher rent-seeking behavior could be due to the fact that the higher the share of eligible

employees in the firm, the more worthwhile it is for firms to engage in rent-capture or the more likely firms are to overcome adjustment costs to respond to the policy. On the other hand, firm exposure to the policy might matter for the final incidence of the tax credit through *pay-equity concerns*. If pay-equity concerns matter, pass-through to firms should be higher in firms with lowest and highest exposure to the program and lower when there is a mix of eligible and non-eligible workers.

The first piece of evidence in support of the effect not being driven by pay-equity concerns comes from directly testing whether the response is monotonic in the pre-reform share of eligible employees. Figure 10 shows the change in annual earnings of eligible employees by pre-reform exposure before (2013) and after (2015) the reform. The figure clearly shows that, after the reform, there is a monotonic relationship between the change in average annual earnings and pre-reform share of eligible employees. That is, firm responses are higher in firms with a higher share of eligible employees. If the results were driven by pay-equity concerns, we would have expected a U-shaped relationship between firm responses and pre-reform exposure.

The fact that rent capture of the tax credit is higher in firms with a higher share of eligible workers can be explained by at least two mechanisms. On the one hand, firms may find it worthy to engage in rent-seeking behavior when there is a large share of workers to extract rents from. On the other hand, *salience* can also partially explain the results. Since employers played a key role in the administration of the tax credit, the tax credit was particularly salient to employers. The higher the exposure to the policy, the more salient the tax credit was to employers. Salience could explain the main results of higher firm-responses in higher exposure firms, as pointed out by Azmat (2019) and Garriga and Tortarolo (2021).

While we cannot directly test whether the main driver of the differential response by firm exposure is higher salience rather than higher possibilities for rent-capture, we can provide some suggestive evidence by taking advantage of the fact that, while the salience from the employer's perspective is constant across the earnings distribution, the salience from the employee's perspective is not. In particular, from the employee's perspective the salience of the transfer is smaller as we move up the earnings distribution. On the other

hand, for firms employing on average more eligible workers from the lower part of the distribution, engaging in rent-seeking behavior could be more worthwhile as a percentage of labor costs. We therefore estimate equation 1 separately for firms employing a higher share of eligible workers earning below the median (conditional on eligibility) and for firms employing a higher share of eligible workers earning above the median. The results are reported in Figure 11, which shows suggestive, even though not conclusive evidence, that the second mechanism might prevail.

To shed additional light on the mechanisms behind the firm-level responses to the policy, we further break down the aggregate effects by firm size and unionization level. In Figure 13 Panel A, I consider the heterogeneity in responses by firm size. The effect is stronger in large firms with 50 or more employees. In Figure 13 Panel B, I consider the heterogeneity in responses by sector-level unionization rates. The effect is stronger in firms that have a lower unionization rate. The results are summarized in Table 5. Note that, in the case of large firms, the pass-through rate is 0.446, suggesting that employers are capturing, in the medium-run, more than 50% of the transfers. This result is consistent with the fact that larger firms 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. Moreover, larger firms with a high concentration of eligible employees benefit more from adjusting their wage policies in response to the introduction of the tax credit, which might also explain the results. The results also show that pass-through to employers is higher in low unionization sectors, where firms are overall less monitored. This is a notable result that provides suggestive evidence that unions could play an important role in preventing employers from capturing part of the tax credit (Lee and Saez, 2012).

Spillover Effects to Non-Eligible Workers Finally, I test whether there are spillover effects to non-eligible workers. At first glance, this does not seem to be the case, as shown in Figure 6 Panel B. However, the results on earnings of all non-eligible workers might mask significant heterogeneity. In Figure 13, I estimate the model from equation 1, using as dependent variables the average gross annual earnings of the high earnings non-eligible workers (above the median of the conditional distribution) and of the low earnings non-

eligible workers. The Figure shows that, while there is no differential responses between high and low exposed firms in terms of earnings of the high earnings non-eligible workers, the earnings of the low-earnings eligible workers seem to decrease in higher exposed firms relative to lower exposed firms.

6 Conclusions

This paper provides an assessment of the incidence effects of the introduction of a large and salient EITC program in Italy. My analysis suggests that firms are an important vector for the pass-through of the effects of the tax credit and shows the importance of considering the role of employers in the analysis of public policies. Firms play a key role in the wage formation process and should not be ignored when analyzing the incidence of welfare programs. This paper shows that abstracting from the role of firms would miss an important channel of transmission of incidence and would lead to incomplete conclusions in the incidence analysis.

I find that firms with a higher concentration of eligible employees in the workforce, and therefore more affected by the introduction of the program, respond more in terms of annual earnings than firms with a lower concentration of eligible employees in the workforce. My estimates suggest that, three years after the introduction of the program, annual earnings of eligible individuals in highly exposed firms decreased by 2% relative to annual earnings of eligible individuals in less exposed firms relative to before the introduction of the program, implying a pass-through to firms of 30%.

In terms of policy implications, by highlighting the role of firms in the transmission of incidence, my analysis calls into question the efficacy of using firms as intermediaries in the distribution of Earned Income Tax Credits. In particular, my findings suggest that there might be a trade-off when giving employers an active role in the distribution of tax credits: on the one hand, using firms as intermediaries in the distribution of the credits allows for the possibility of monthly transfers (which are preferred to yearly transfers if individuals have liquidity constraints) and reduces problems of low take-up by making the distribution of the credit automatic, on the other hand, giving employers perfect infor-

mation on who receives the credit and on the magnitude of the transfer is likely to make it easier for firms to capture part of the benefits of the tax credit destined to workers. Future research should investigate deeper the connection between the way tax credits are designed, the role of firms and incidence.

References

Azmat, Ghazala. 2019. "Incidence, salience, and spillovers: The direct and indirect effects of tax credits on wages." *Quantitative Economics* 10 (1):239–273.

Barattieri, Alessandro, Susanto Basu, and Peter Gottschalk. 2014. "Some evidence on the importance of sticky wages." *American Economic Journal: Macroeconomics* 6 (1):70–101.

Bennmarker, Helge, Lars Calmfors, and Anna Seim. 2014. "Earned income tax credits, unemployment benefits and wages: empirical evidence from Sweden." *IZA Journal of Labor Policy* 3 (1):54.

Bíró, Anikó, Réka Branyiczki, Attila Lindner, Lili Márk, and Dániel Prinz. 2022. "Firm heterogeneity and the impact of payroll taxes." .

Bozio, Antoine, Thomas Breda, and Julien Grenet. 2017. "Incidence of social security contributions: evidence from France." *Working Paper* .

Card, David, Ana Rute Cardoso, Joerg Heining, and Patrick Kline. 2018. "Firms and labor market inequality: Evidence and some theory." *Journal of Labor Economics* 36 (S1):S13–S70.

Card, David, Jörg Heining, and Patrick Kline. 2013. "Workplace heterogeneity and the rise of West German wage inequality." *The Quarterly Journal of Economics* 128 (3):967–1015.

Card, David, Alexandre Mas, Enrico Moretti, and Emmanuel Saez. 2012. "Inequality at work: The effect of peer salaries on job satisfaction." *American Economic Review* 102 (6):2981–3003.

Cengiz, Doruk, Arindrajit Dube, Attila Lindner, and Ben Zipperer. 2019. "The effect of minimum wages on low-wage jobs." *The Quarterly Journal of Economics* 134 (3):1405–1454.

Chetty, Raj, John N Friedman, Tore Olsen, and Luigi Pistaferri. 2011. "Adjustment costs, firm responses, and micro vs. macro labor supply elasticities: Evidence from Danish tax records." *The quarterly journal of economics* 126 (2):749–804.

Dickens, William T, Lorenz Goette, Erica L Groshen, Steinar Holden, Julian Messina, Mark E Schweitzer, Jarkko Turunen, and Melanie E Ward. 2007. "How wages change: micro evidence from the International Wage Flexibility Project." *Journal of Economic Perspectives* 21 (2):195–214.

Draca, Mirko, Stephen Machin, and John Van Reenen. 2011. "Minimum wages and firm profitability." *American Economic Journal: Applied Economics* 3 (1):129–51.

Dube, Arindrajit, Laura Giuliano, and Jonathan Leonard. 2019. "Fairness and frictions: The impact of unequal raises on quit behavior." *American Economic Review* 109 (2):620–63.

Eissa, Nada and Hilary W Hoynes. 2006. "Behavioral responses to taxes: Lessons from the EITC and labor supply." *Tax policy and the economy* 20:73–110.

Eissa, Nada and Jeffrey B Liebman. 1996. "Labor supply response to the earned income tax credit." *The Quarterly Journal of Economics* 111 (2):605–637.

Garriga, Santiago and Dario Tortarolo. 2021. "Wage effects of employer-mediated transfers." .

Gravoueille, Maxime. 2022. "Wage and Employment Effects of Wage Subsidies." Tech. rep., Working Paper.

Harasztosi, Péter and Attila Lindner. 2019. "Who Pays for the minimum Wage?" *American Economic Review* 109 (8):2693–2727.

Hotz, V Joseph. 2003. "The earned income tax credit." In *Means-tested transfer programs in the United States*. University of Chicago press, 141–198.

Hotz, V Joseph and John Karl Scholz. 2001. "The earned income tax credit."

Hoynes, Hilary. 2019. "The earned income tax credit." *The Annals of the American Academy of Political and Social Science* 686 (1):180–203.

Jones, Damon. 2010. "Information, preferences, and public benefit participation: Experimental evidence from the advance EITC and 401 (k) savings." *American Economic Journal: Applied Economics* 2 (2):147–63.

Kaur, Supreet. 2019. "Nominal wage rigidity in village labor markets." *American Economic Review* 109 (10):3585–3616.

Kleven, Henrik J and Mazhar Waseem. 2013. "Using notches to uncover optimization frictions and structural elasticities: Theory and evidence from Pakistan." *The Quarterly Journal of Economics* 128 (2):669–723.

Kleven, Henrik Jacobsen. 2016. "Bunching." *Annual Review of Economics* 8:435–464.

Lee, David and Emmanuel Saez. 2012. "Optimal minimum wage policy in competitive labor markets." *Journal of Public Economics* 96 (9-10):739–749.

Leigh, Andrew. 2010. "Who benefits from the earned income tax credit? Incidence among recipients, coworkers and firms." *The BE Journal of Economic Analysis & Policy* 10 (1).

Meyer, Bruce D. 2010. "The effects of the Earned Income Tax Credit and recent reforms." *Tax policy and the economy* 24 (1):153–180.

Meyer, Bruce D and Dan T Rosenbaum. 2001. "Welfare, the earned income tax credit, and the labor supply of single mothers." *The Quarterly Journal of Economics* 116 (3):1063–1114.

Neri, Andrea, Concetta Rondinelli, and Filippo Scoccianti. 2015. "The marginal propensity to consume out of a tax rebate: the case of Italy." *Bank of Italy, mimeo* 3:341–354.

Nichols, Austin and Jesse Rothstein. 2015. "The earned income tax credit." In *Economics of Means-Tested Transfer Programs in the United States, Volume 1*. University of Chicago Press, 137–218.

Paradisi, Matteo. 2019. *Essays on Firms and Public Policies*. Ph.D. thesis, Harvard University, Graduate School of Arts & Sciences.

Romich, Jennifer and Thomas Weisner. 2002. "How Families View and Use the Earned Income Tax Credit." *Making Work Pay. Russell Sage Foundation, New York* .

Rothstein, Jesse. 2008. "The unintended consequences of encouraging work: Tax incidence and the EITC." *Center for Economic Policy Studies, Princeton University* 165.

———. 2010. "Is the EITC as good as an NIT? Conditional cash transfers and tax incidence." *American Economic Journal: Economic Policy* 2 (1):177–208.

Saez, Emmanuel. 2010. "Do taxpayers bunch at kink points?" *American Economic Journal: Economic Policy* 2 (3):180–212.

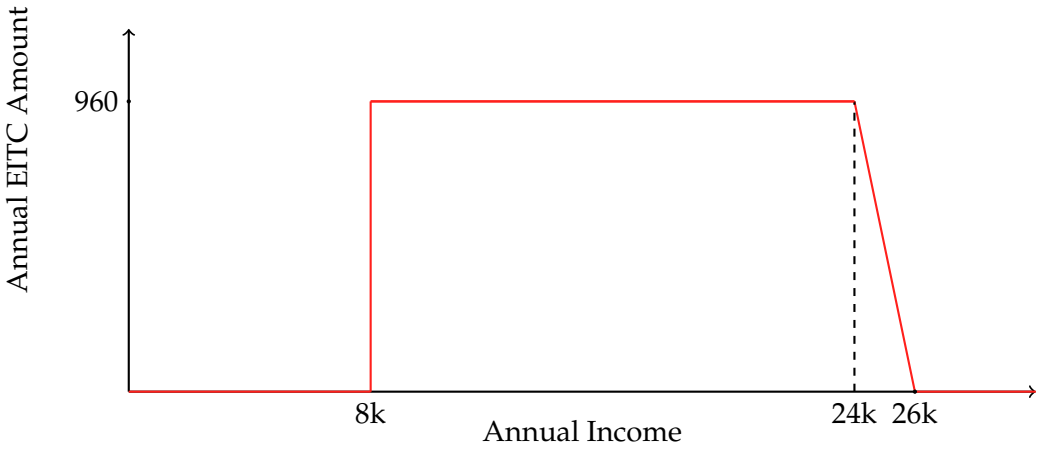
Saez, Emmanuel, Manos Matsaganis, and Panos Tsakloglou. 2012. "Earnings determination and taxes: evidence from a cohort-based payroll tax reform in Greece." *The Quarterly Journal of Economics* 127 (1):493–533.

Saez, Emmanuel, Benjamin Schoefer, and David Seim. 2019. "Payroll taxes, firm behavior, and rent sharing: Evidence from a young workers' tax cut in Sweden." *American Economic Review* 109 (5):1717–63.

Villamaina, Luca and Paolo Acciari. 2023. "Taxation and labour supply decisions: an evaluation of the earned income tax credit in Italy." Tech. rep.

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: PAYCHECK OF WORKERS: EXAMPLE

Totale : 1.823,55

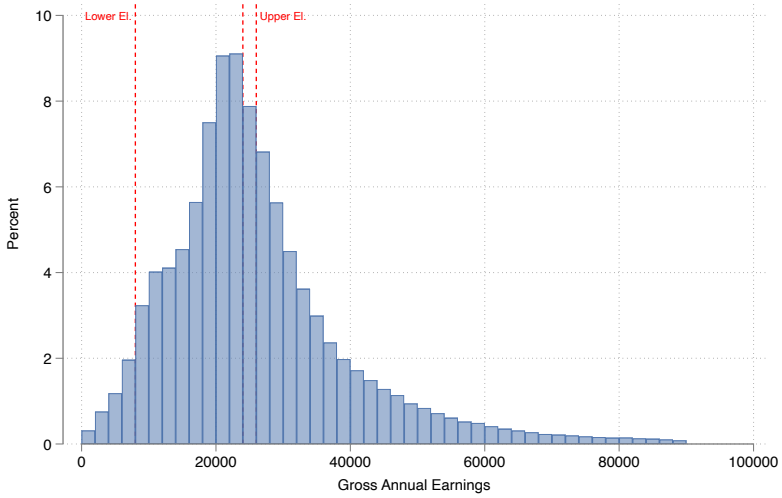
COD.	DESCRIZIONE	ORE / GG	%	DATO BASE	RITENUTE	COMPETENZE
1	IMPORTO ORDINARIO	26,00		70,13654		1.823,55
111	STRAORDINARIO 15%	2,00	15,00	12,48262		24,97
999	TOT.LORDO SOGG.CONTR			1.848,52		
1	CONTRIB.FAP 9.19%		9,1900	1.849,00	169,92	
58	FONDO EST TERZ.			2,00	2,00	
991	SOLID. M980			10,00		
125	FONDO 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

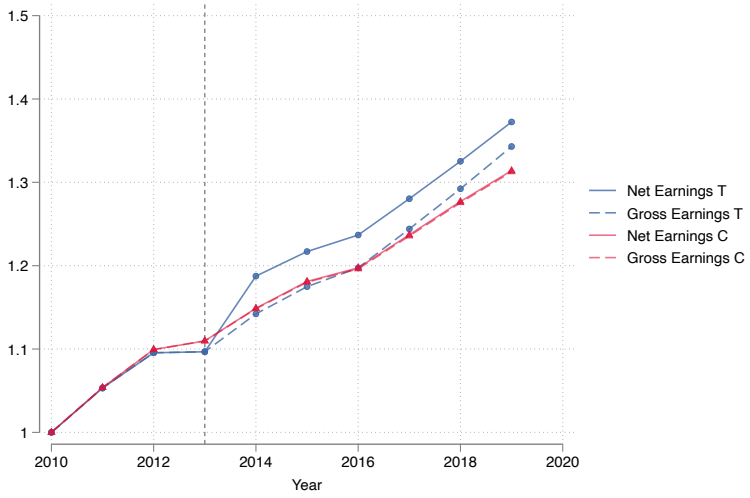
Notes: This figure shows an example of the paycheck of an Italian worker. The red square denotes the line indicating the amount of the 80 Euros Bonus which is added directly to the paycheck of workers every month.

Figure 3: Pre-Reform Distribution of Earnings Levels



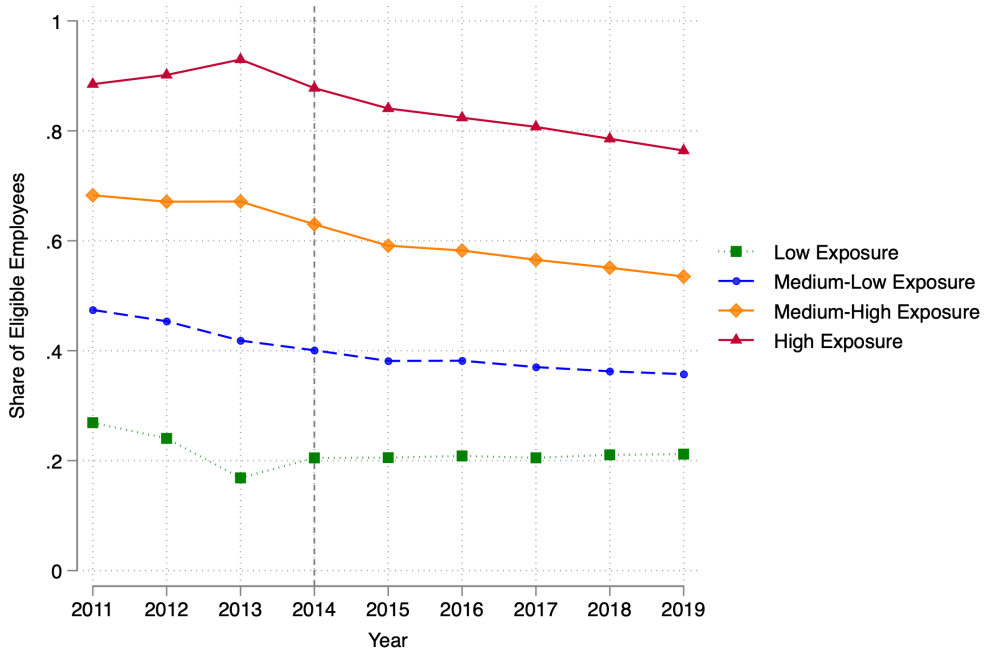
Notes: The figure depicts the distribution of employees by gross annual earnings in 2013. The vertical red lines indicate the lower and upper eligibility cutoffs for the tax credit.

Figure 4: The Effect of the Bonus on Gross and Net Earnings



Notes: This figure depicts the evolution of average annual gross (dashed line) and net (solid line) earnings for eligible (blue) and similarly ineligible (red) workers relative to 2013, the last pre-reform year. I define eligible workers as workers earning, in 2013, between €20,000 and €23,000. I define ineligible workers as workers earning, in 2013, between €28,000 and €31,000.

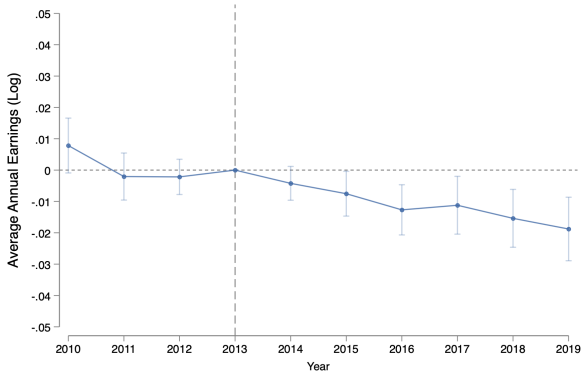
Figure 5: EVOLUTION OF SHARE OF ELIGIBLE WORKERS, BY 2013 SHARE OF ELIGIBLE WORKERS GROUPS



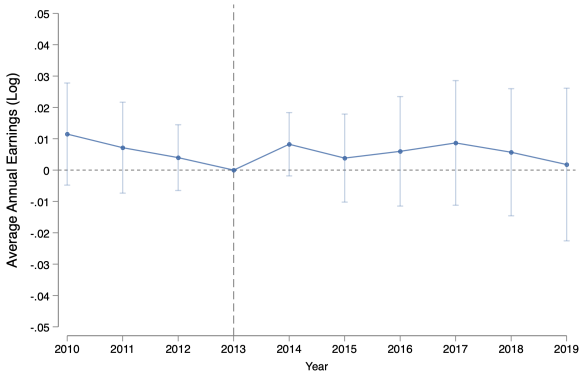
Notes: The figure depicts the average share of eligible workers in each year for the four groups of firms defined by the quartiles of share of eligible employees in 2013. The spike around 2013 is due to mean reversion: firms with a high share of eligible employees in 2013 tend to have a lower share before 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 6: Firm-Level Effects of the Bonus on Earnings

(a) Eligible Workers

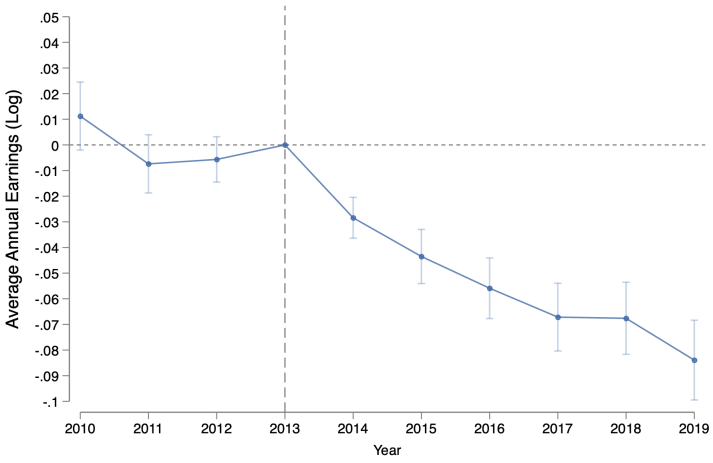


(b) Non-Eligible Workers



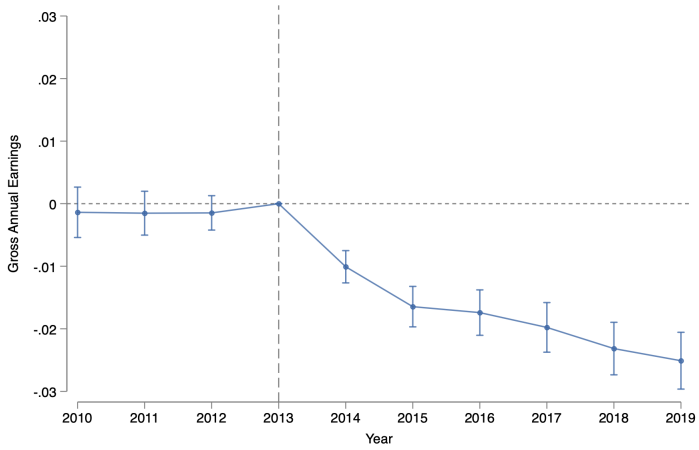
Notes: The figures show the results from a difference-in-differences specification (equation 1) comparing firms with a high share of eligible employees (top quartile) to firms with a medium-high share of eligible employees (between the 50th and the 75th percentile) in the last pre-reform year. Panel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

Figure 7: Firm-Level Effects of the Bonus on Earnings: Continuous Treatment



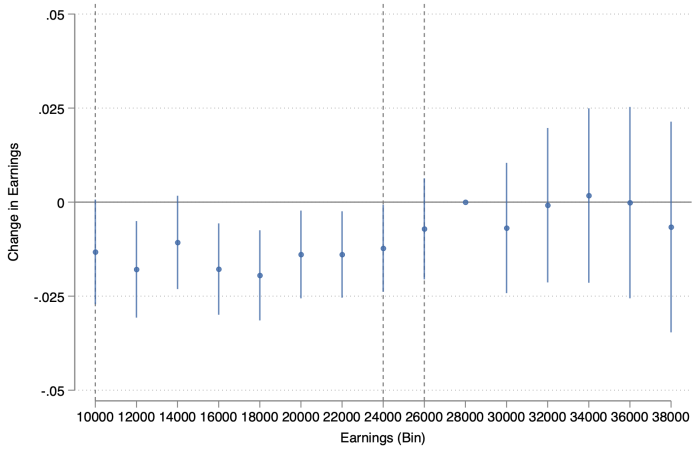
Notes: The figures show the results from a difference-in-differences specification (equation 2) which estimate a treatment intensity model, using the pre-reform share of eligible workers as a proxy for treatment intensity. The figure shows the results when the outcome is the log of the firm-level average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

Figure 8: Firm-Level Effects of the Bonus: Individual-Level Estimates



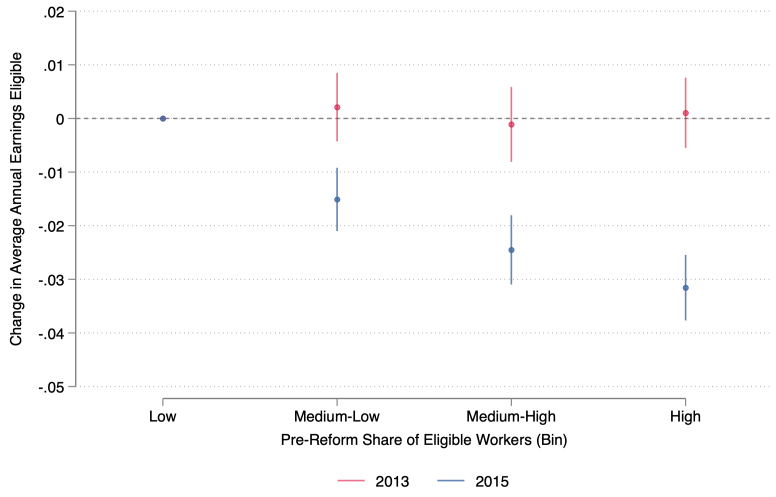
Notes: The figures show the results from the individual-level difference-in-differences model outlined in equation3, which compares individuals employed in 2013 in high exposure firms (top quartile) to individuals employed in 2013 in firms with a medium-high share of eligible employees (between the 50th 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 9: Firm-Level Effects of the Bonus 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 10: Firm-Level Effects of the Bonus By Firm Exposure

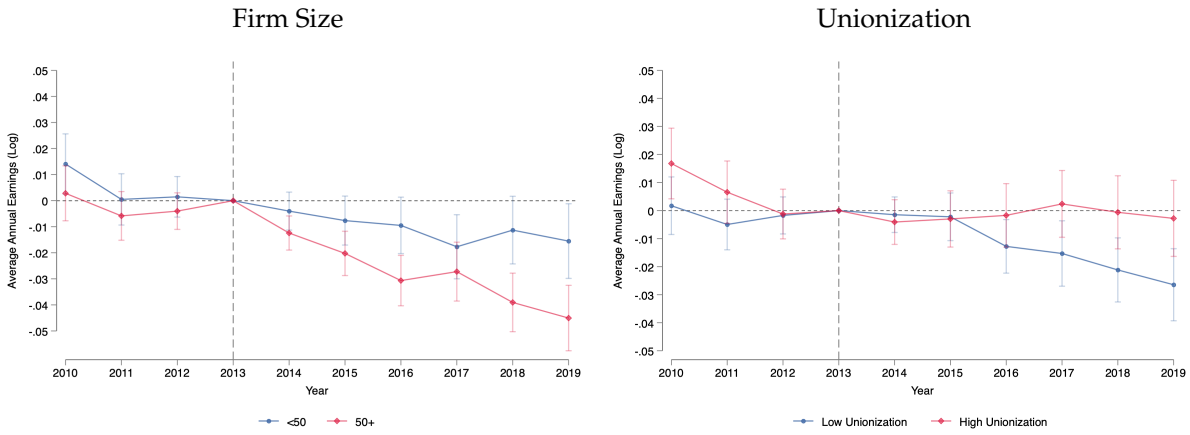


Notes: The figures show the two-year change in average annual earnings of eligible employees by pre-reform exposure before and after the reform, relative to the bottom quartile of pre-reform exposure.

Figure 11: Firm-Level Effects: Mechanisms

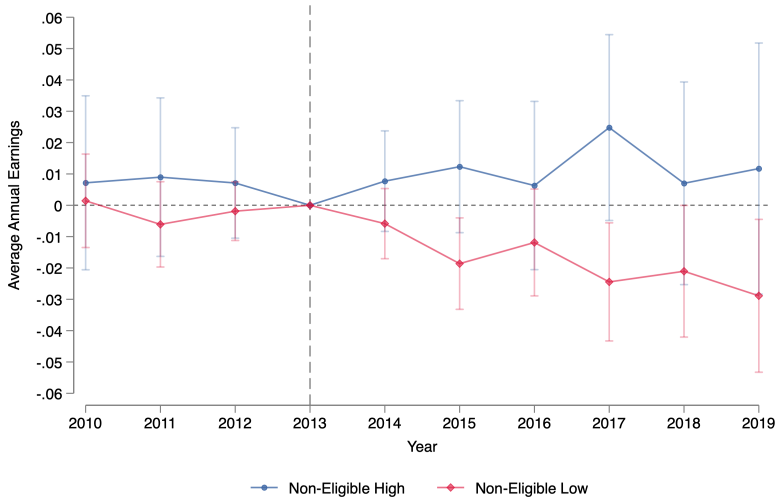
Notes: The figure shows the results from the difference-in-differences specification in equation 1 estimated separately for firms employing a higher share of eligible workers earning below the median (conditional on eligibility) and for firms employing a higher share of eligible workers earning above the median. Standard errors are clustered at the firm-level.

Figure 12: Firm-Level Effects: Firm Size And Unionization



Notes: The figure shows the results from the difference-in-differences specification in equation 1 estimated separately for firms with more or less than 50 employees (Panel A) and firms in sector above or below the median unionization rate (Panel B). The outcome variable 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 13: Firm-Level Effects of the Bonus 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 (above the median of the conditional distribution). The dependent variable for the red series is the (log) average firm-level gross earnings of low-earnings pre-reform non-eligible employees (below the median of the conditional distribution). Standard errors are clustered at the firm-level.

Tables

Table 1: **Summary Statistics**

	Full Sample		**Eligible**	
	Mean	Std. Dev	Mean	Std. Dev
Annual Earnings	24,410.84	17,959.39	17,562.2	5,115.55
Weeks Worked	48.07	7.70	47.57	7.81
Age	42.4	9.54	41.09	9.54
Male	0.59	0.49	0.55	0.50
Temporary Contract	0.11	0.31	0.13	0.34
Working in Firm 50+	0.50	0.49	0.41	0.49
Eligible	0.57	0.49		
Observations	780,487		443,655	

Notes: This table shows summary statistics for the sample of workers used in the analysis in 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: SUMMARY STATISTICS FOR THE TREATMENT AND CONTROL GROUPS IN 2013

	Control			*Treatment*		
	N	Mean	SD	N	Mean	SD
Gross Annual Earnings	55610	28362.12	5716.67	107895	20393.74	4933.76
Male	55610	0.707	0.455	107895	0.627	0.484
Tenure	55610	14.89	7.87	107895	12.50	7.18
Age	55422	43.25	9.27	107150	40.27	10.07
Full Time	53936	0.997	0.073	100518	0.989	0.104
Permanent	55610	0.951	0.215	107895	0.887	0.317

Notes: This table shows summary statistics for the sample of workers used in the analysis in 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 3: 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 year 2013. All monetary variables are expressed in Euros.

Table 4: FIRM-LEVEL REGRESSION RESULTS

	(1)	(2)	(3)
	All Post Periods	**Short Run**	**Medium Run**
	2014-2019	2014-2016	2016-2019
Gross Annual Earnings Eligible	-0.0102***	-0.00707**	-0.0141***
	(0.00348)	(0.00335)	(0.00442)
Pass-through Estimate	0.195	0.135	0.270
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	58,125	58,125	58,125
Avg. Gross Annual Earnings Eligible at Baseline	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 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.

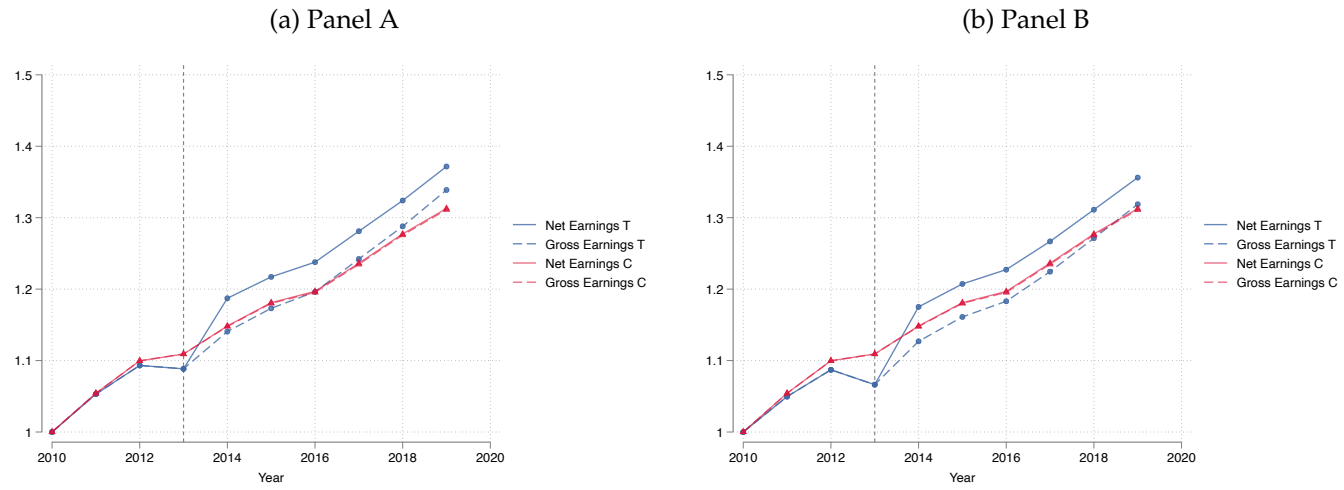
Table 5: FIRM-LEVEL REGRESSION RESULTS: HETEROGENEITY

	(1)	(2)	(3)	(4)
	Small	**Large**	**Low**	**High**
	<50	50+	**Unionization**	**Unionization**
Gross Annual Earnings Eligible	-0.0201***	-0.0314***	-0.0137***	-0.00957*
	(0.00423)	(0.00389)	(0.00403)	(0.00400)
Pass-through Estimate	0.385	0.601	0.262	0.183
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	27,728	29,989	35,381	22,724

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 and unionization level. The reduced-form estimates corresponds to the difference-in-differences coefficients where the *post* event includes all years after 2013. Standard errors are clustered at the firm level.

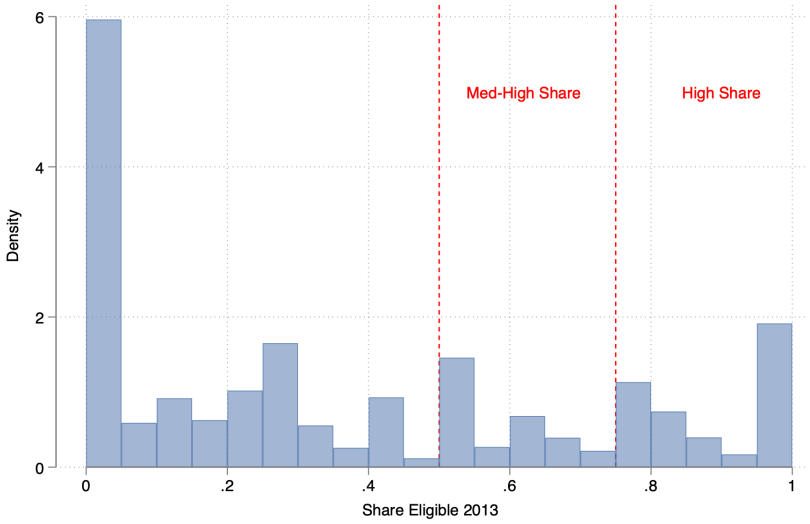
Appendix

Figure A1: THE EFFECT OF THE BONUS ON GROSS AND NET EARNINGS: ROBUSTNESS



Notes: These figures show the results with definitions of treatment and control groups increasingly further away from the phase-out cutoff. In particular, Panel A shows the results where the treatment group is defined as workers earning, in 2013, between €20,000 and €22,000 and the control group is defined as workers earning, in 2013, between €28,000 and €30,000. Panel B shows the results where the treatment group is defined as workers earning, in 2013, between €19,000 and €21,000 and the control group is defined as workers earning, in 2013, between €29,000 and €31,000.

Figure A2: Construction of Groups

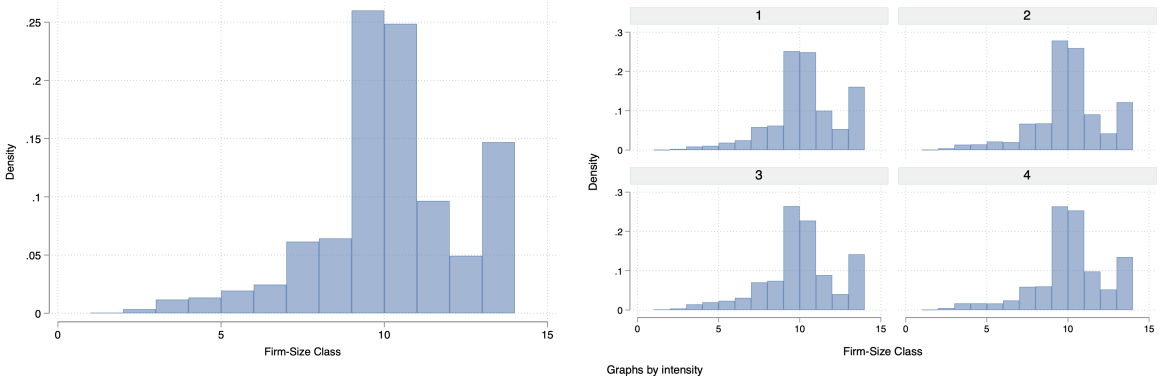


Notes: The figure depicts the distribution of share of eligible workers across firms in 2013 considering firms with at least three sampled workers in the pre-reform year.

Figure A3: Firm-Level Exposure And Firm Size: Three Sampled Workers

(a) Density of Firms by Firm-Size Class

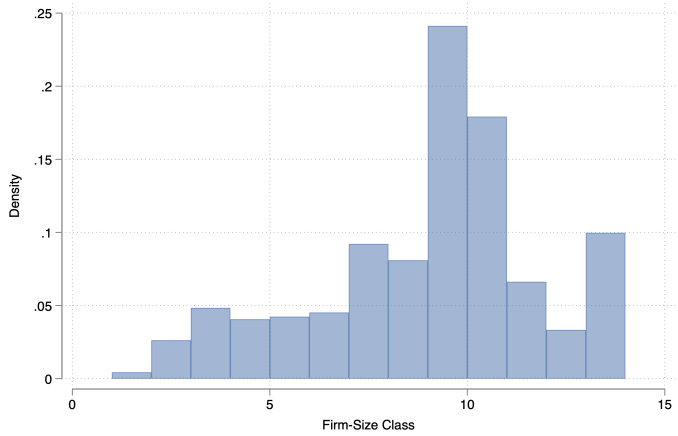
(b) Density of Firm-Size Class by Exposure



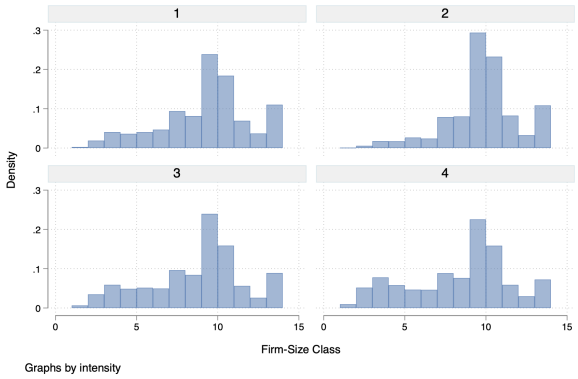
Notes: Panel A depicts the distribution of firms by firm-size class in 2013. Panel B depicts the distribution of firm-size class by firms exposure group, where 1 indicates low exposure firms (bottom quartile), 2 medium-low exposure (between the 25th and the 50th percentile), 3 medium-high exposure (between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

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

(a) Density of Firms by Firm-Size Class



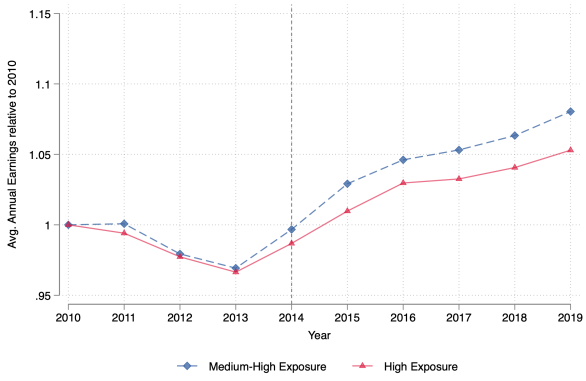
(b) Density of Firm-Size Class by Exposure



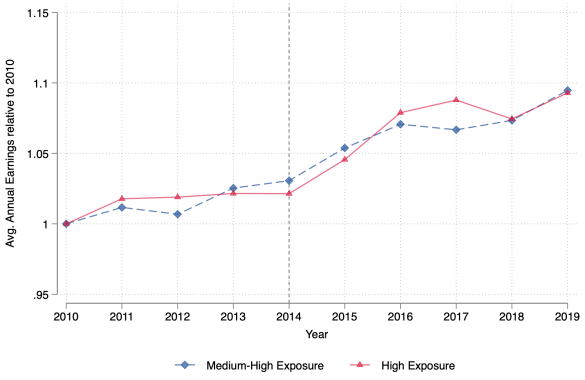
Notes: Panel A depicts the distribution of firms by firm-size class in 2013. Panel B depicts the distribution of firm-size class by firms exposure group, where 1 indicates low exposure firms (bottom quartile), 2 medium-low exposure (between the 25th and the 50th percentile), 3 medium-high exposure (between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

Figure A5: Firm-Level Effects of the Bonus on Earnings: Descriptive Evidence

(a) Eligible Workers

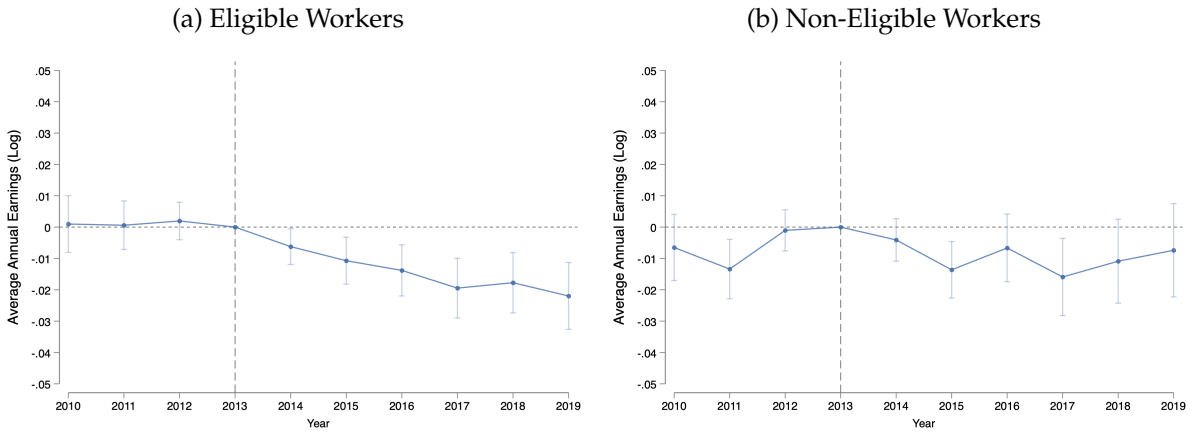


(b) Non-Eligible Workers



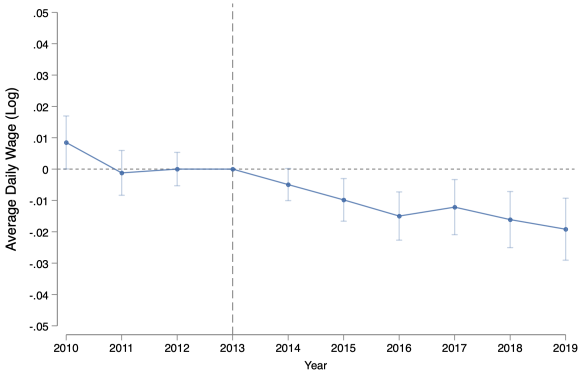
Notes: The figure traces out the evolution of the raw average annual earnings for eligible and non-eligible workers (relative to 2010) in medium-high exposure firms (firms with a pre-reform share of eligible employees between the 50th and 75th percentile) and high exposure firms (firms with a pre-reform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

Figure A6: Firm-Level Effects of the Bonus on Earnings: Different Treatment Definitions



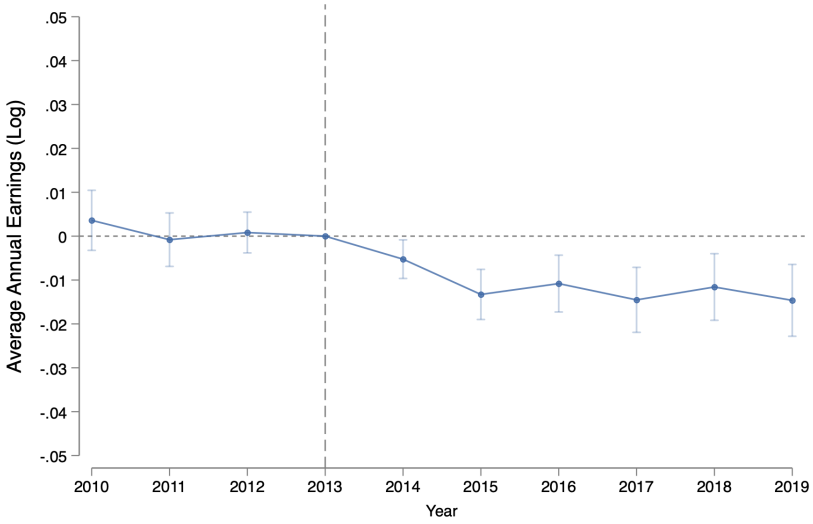
Notes: The figures show the results from a difference-in-differences specification (equation 1) comparing firms with a medium-high share of eligible employees (between the 50th and the 75th percentile) to firms with a medium-low share of eligible employees (between the 25th and the 50th percentile) in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

Figure A7: Firm-Level Effects of the Bonus on Earnings: Daily Wage



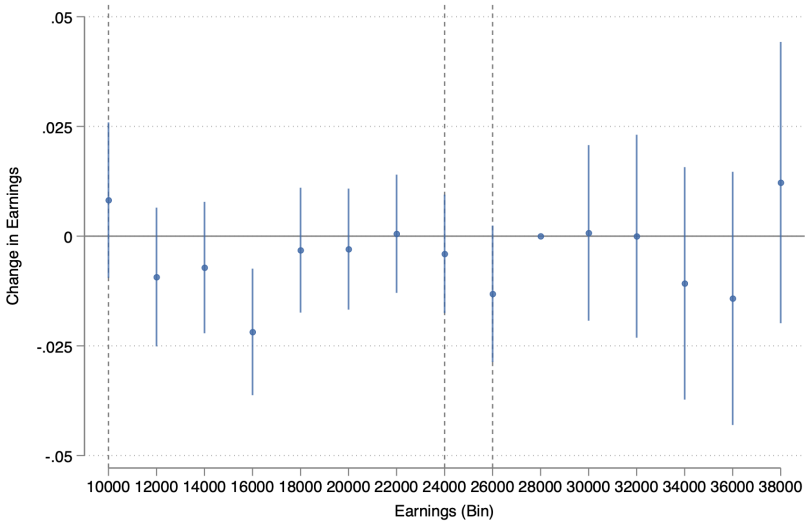
Notes: The figures show the results from a difference-in-differences specification (equation 1) comparing firms with a high share of eligible employees (top quartile)to firms with a medium-high share of eligible employees (between the 50th and the 75th percentile) in the last pre-reform year. The outcome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

Figure A8: FIRM-LEVEL EFFECTS OF THE BONUS ACROSS THE EARNINGS DISTRIBUTION: ROBUSTNESS TO RESTRICTIONS



Notes: The figures show 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 A9: FIRM-LEVEL EFFECTS OF THE BONUS 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.