Taxing the Gender Gap: Labor Market Effects of a Payroll Tax Cut for Women in Italy*

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Abstract

Is gender-based taxation a successful strategy for closing the gender gap in labor market outcomes? This paper studies the labor market impacts of a large payroll tax cut for hiring unemployed women in Italy. The empirical approach combines rich matched employer-employee data with cross-municipality, cross-cohort and cross-profession discontinuities in exposure to the payroll tax cut generated by the 2012 reform. The results provide compelling evidence that the payroll tax cut led to long-lasting growth in female employment and, in contrast with standard tax incidence theory, had no effects on net-of-tax wages. I further find that the payroll tax cut significantly improves job tenure, while reducing the average non-employment duration of women entering unemployment insurance after the reform. Firms hiring many female workers experience significant growth in net worth, value added and capital per-worker, without raising male employees' layoffs. These findings suggest that payroll tax cuts are a successful strategy to promote female employment and business growth in contexts where gender attitudes are still traditional, but they are not sufficient for closing the gender wage gap.

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1 Introduction

Is gender-based taxation a successful strategy for closing the gender gap in labor market outcomes? In theory, differentiating taxes by gender can be an effective tool to curb gender labor market inequalities. This is particularly true if employers' discrimination (taste-based or statistical) is the key factor for the under-representation of women in the labor force (Becker 1957). Because cutting taxes for female workers would make them relatively cheaper, gender-based taxation might help overcoming prejudices and improving efficiency by raising female labor demand. However, if female labor force participation depends on cultural and social norms (see, e.g., Goldin 2006; Blau and Kahn 2007; Bertrand 2011), then tax reforms may not be the best way to address these issues.

Although it has long been recognized that gender may represent a useful tagging device in optimal tax and welfare programs (Rosen 1977; Akerlof 1978; Boskin and Sheshinski 1983; Kleven et al. 2009; Alesina et al. 2011; Gayle and Shephard 2019), there is very little empirical work on whether gender-based taxation has the potential to tackle gender inequalities in labor market outcomes. This lack of evidence is puzzling because providing a system of differentiated taxes by gender might address labor market inequalities in a less distortionary way than other gender-based policies (Alesina et al. 2011), make gender discrimination more costly for employers (Weber and Zulehner 2014), and compensate women for the fact that the possibility of having children can negatively affect their career prospects (Kleven et al. 2019). Furthermore, studying the effects of gender-based taxes on female labor market outcomes is particularly important in labor markets where gender attitudes are still traditional and it remains unclear whether female labor force participation depends on structural parameters on labor supply behavior or rather reflects labor market institutions.

In this paper, I use matched employee-employer data to study the labor market effects of a large payroll tax cut for unemployed women in Italy. Starting from January 2013, the payroll tax rate paid by the employer for new female hires is reduced by 50 percent for a period of up to 18 months (see law 92/2012).[1] The motivation for this reform was to stimulate demand for female workers in light of high female unemployment, as well as to boost business activity by reducing employer taxes.[2] Eligibility for the scheme depends on time elapsed in non-employment status and varies discontinuously depending on municipality of residence and age of the worker. Namely, in a first group of municipalities, the payroll tax cut applied to women with non-employment duration of at least 6 months. In a second group of municipalities, the minimum non-

[1] The total payroll tax rate is around one-third of the employee's gross compensation and it depends on the type of work performed by the firm, the number of employees and the employee's position.

[2] The idea of stimulating employment of workers facing high unemployment rates through tax cuts has been gaining political traction. For instance, most European countries have implemented payroll tax cuts for low earners or young workers (see, e.g., Cahuc et al. 2019 for France and Saez et al. 2019b for Sweden), while the United States has a history of targeted employer credits for disadvantaged groups (see Katz 1998).

employment duration requirement is 12 months for women older than 50; 24 months for those younger than 50. Furthermore, the preferential tax scheme is further extended to male-biased professions, based on a cutoff-rule. The features of the tax scheme thus allow the implementation of sharp research designs and provide a powerful way for study the effect of tax policy on curbing gender inequalities both in places and professions where gender attitudes are still traditional.

The received wisdom in public economics is that workers would ultimately bear payroll taxes (see, e.g., Hamermesh 1979; Fullerton and Metcalf 2002). Therefore, if wages are not rigid, the payroll tax cut should be fully shifted from employers to employees in the form of higher wages. To study the incidence of the payroll tax cut, I propose a simple empirical approach resting on individual-level variation in gross and net wages over job tenure and between eligible and not eligible jobs. Namely, I compare net and gross wages earned during the job when the payroll tax cut applied with the previous (not eligible) job, before and after the 18-month tenure cutoff when the preferential payroll tax scheme applies. I provide evidence that monthly *net* wages earned throughout the preferential tax scheme period are strikingly similar to wages earned during the previous job. By contrast, I find a dip in *gross* wages relative to the previous job during the payroll tax cut period. Most important, I show that gross wages discontinuously adjust to previous job's level at the 18-month cutoff. This provides striking and suggestive evidence that employers do not adjust wages in response to the payroll tax cut. This non-standard payroll tax incidence result is consistent with a recent series of empirical works focusing on upper earners in Greece (Saez et al. 2012), young workers in Sweden (Saez et al. 2019b), lower earners hired by small firms in France (Cahuc et al. 2019) and workers in Finland (Benzarti and Harju 2021).[3]

Next, I study employment effects by proposing several empirical approaches, resting on variation over time and across municipalities, cohorts and professions, where eligibility for the preferential payroll tax scheme changes discontinuously depending on the minimum non-employment duration requirement. All these empirical approaches point to the same conclusion: the payroll tax cut led to a large and long-lasting effect on female labor force participation. Motivated by the possibility that these aggregate analyses might be biased by the presence of other aggregate economic shocks or policies and measurement errors in determining payroll tax cut eligibility, I then perform a micro-level analysis resting on cross-individual variation in female labor force participation and payroll tax cut eligibility. Using this approach, I am able to exploit variation across individuals within a given municipality-cohort-month cell, thus accounting for any municipality $\times$ month, cohort $\times$ month and municipality $\times$ cohort fixed effects. My preferred estimate suggests an increase in labor force participation by around 2.9 percentage points compared to the pre-reform period. This effect translates in a labor force participation elasticity of 0.385. This result is consistent with the existing empir-

[3]Nonstandard tax incidence results have been documented also for the income tax (see, e.g., Bingley and Lanot 2002 and Kubik 2004) and for the Earned Income Tax Credit in the US (Rothstein 2010).

ical evidence showing that female labor supply is relatively elastic (see Killingsworth and Heckman 1986; Blundell and Macurdy 1999; Keane 2011; Chetty et al. 2013).

Given the time-limited nature of the payroll tax cut, a natural following question is whether employers retain workers even after the payroll tax cut no longer applies. In theory, the optimal strategy for a firm maximizing profits would be to minimize labor costs by keeping eligible workers up to the point when the preferential payroll tax scheme applies. This behavior would create a notch: a discontinuity in the choice set of labor cost versus job tenure. Such a notch introduces an incentive for offering jobs with duration up to the 18-month tenure cutoff and should thus create a hole in the job tenure distribution just above the cutoff and excess bunching just at the cut-off. I provide empirical evidence in contrast with this prediction. Comparing pre- vs post-reform job tenure of payroll tax cut recipients, I show that the tenure distribution shifted right over the post-reform period, suggesting that women spend more time employed. On average, my estimate suggests an increase of nearly 60 percent in job tenure, conditional on remaining employed after the payroll tax cut no longer applies. My preferred specification flexibly controls for interactions between individual fixed effects and characteristics of the job (e.g., full vs part time) and municipality-by-year fixed effects, thus controlling for any change in hours of works across jobs for a given individual and comparing tenure across individuals within a given local labor market.

I then ask whether the employment effects of the payroll tax cut reflect a response coming from unemployed women or it moves women out of the welfare system. If the latter is true, then missing revenue from the payroll tax cut would be counterbalanced by the reduction in fiscal costs covered by unemployment insurance (UI) benefits. To this purpose, I look at the effects of the payroll tax cut on UI benefit duration by using data on the universe of UI benefits' recipients. Leveraging variation in exposure to the payroll tax cut across municipalities and cohorts, I find that the payroll tax cut significantly decreases the duration of UI benefit: women located in municipalities or cohorts more exposed to the payroll tax cut spend around 39 days less on welfare. This implies that the payroll tax cut increases labor market tightness and reduces the fiscal externalities of unemployment benefits.

In the last part of the paper, I take a firm-level perspective to study the effect of hiring more female workers. Matching social security records with financial data, I leverage between-firm exposure to the payroll tax cut generated by the pre-reform gender composition of their workforce. As firms that started with a lower share of female workers are more likely to operate in places or industries that benefited of less binding payroll tax cut eligibility criteria, I compare firms by their pre-reform share of female workers. As a first step, I provide evidence in support of this empirical approach: firms presenting a lower pre-reform share of women in their workforce (defined as those in the bottom quintile of the pre-reform share of female worker distribution) hired much more female workers compared to similar firms with a relatively larger pre-reform share of women (the next quintile). Consistent with the findings presented above, I find no effect on male employment and only a slight increase in labor costs per-worker, thus implying that these firms grew in size by exploiting the lower labor costs of new female hires. Then, I show that the addition of female workers did significantly raise firm-level per-worker net worth (by 9.7 percent), value added (3.4 percent) and capital (3.3 percent).

The main contribution of this paper is to shed novel light on the effects of tax policy on female labor market outcomes. To the best of my knowledge, this paper provides the first evidence on whether payroll tax cuts can affect the gender employment and wage gap. The paper relates with several strands of the literature. My focus on the role of gender-specific taxation to promote female employment is not meant to imply that other policies and factors are unimportant. A range of other studies has provided evidence that many other factors can influence female labor market outcomes, such as technological development (Goldin and Sokoloff 1984; Goldin 1995), medical improvements (Goldin and Katz 2002; Albanesi and Olivetti 2016), cultural and social norms (Fernández 2007; Alesina et al. 2013), biological differences (Ichino and Moretti 2009), legal rights (Doepke and Tertilt 2009), household composition (Albanesi and Olivetti 2009), family policies such as parental leave and child care (Bertrand et al. 2010; Kleven 2019; Kleven et al. 2019), firm-specific pay premiums (Card et al. 2016; Casarico and Lattanzio 2019), industrial structure (Olivetti and Petrongolo 2016), and board quotas (Bertrand et al. 2019).

The paper also connects with the studies estimating the incidence of payroll taxes. Although several papers have estimated payroll tax incidence (see, e.g., Gruber 1997; Anderson and Meyer 2000; Kugler and Kugler 2009; Saez et al. 2012; Bozio et al. 2019; Cahuc et al. 2019; Saez et al. 2019a; Saez et al. 2019b; Benzarti et al. 2020; Benzarti and Harju 2020; Benzarti and Harju 2021), we lack evidence on the specific effects on female labor market outcomes.[4] Matching employer-employee data with financial data, I also show that payroll taxes depress employment and investment, in line with the evidence offered by Saez et al. (2019b) and Benzarti and Harju (2021). Furthermore, the paper speaks to the literature focusing on wage subsidies targeting disadvantaged groups (see, e.g., Katz 1998, for the US) and with an active literature focusing on the effects of tax reform on female labor supply behavior. This literature has mostly focused on employment effects of the Earned Income Tax Credit in the US (see, e.g., Eissa and Liebman 1996; Kleven 2019).

I also contribute to the literature estimating the labor market effects of time-limited earnings subsidies or credits. In a seminal contribution, Card and Hyslop (2005) exploit a randomly assigned 3-year subsidy in Canada designed to help welfare recipients to permanently enter in the labor market. The program provided a subsidy only to people who began working full time within one year of random assignment. The authors

[4]One notable exception is the study of the payroll tax cut in Sweden offered by Saez et al. (2019a). The authors explore heterogeneity responses by sex but do not find significant effects. One plausible explanation is that Sweden is a much more egalitarian country in gender attitudes compared to Italy.

offer both theoretical and empirical evidence that time-limited subsides created an "establishment" incentive to choose work over welfare once eligibility requirements are meet, and an "entitlement" incentive to leave welfare and find a job within a year of random assignment. Their results show significant but short-lived impacts on wages and welfare participation.[5] By contrast, my findings document hystheresis effects (i.e., a persistent employment effects lasting even when the original cause no longer applies). Hystheresis effects from payroll tax cuts have been recently studied by Saez et al. (2019a) in the context of the repeal of a preferential payroll tax scheme for young workers in Sweden. The authors provide evidence of labor demand-driven hysteresis that triple the direct employment effects of the reform.

The rest of the paper proceeds as follows. First, section 2 introduces the main features of the Italian payroll tax system and the details of the preferential tax scheme for new female hires. In section 3, I describe the data sources and the payroll tax cut's recipients main characteristics. Section 4 shows the effects of the payroll tax cut on several labor market outcomes, including wages (4.1), labor force participation (4.2), job tenure (4.3), and UI benefit duration (4.4). In Section 5, I take a firm-level perspective to show the effects of the payroll tax cut on various firm-level outcomes. Section 6 concludes.

2 Institutional framework

2.1 Gender gap in the Italian labor market

According to OECD Family Database, Italy ranks in lowest position regarding female labor market outcomes: full-time equivalent employment share of women was 40.3 percent, and the gender employment gap was 26.5 percent in 2018. Only Greece performs worst than Italy among European countries. Italy is thus a typical gender-conservative environment, which makes it a suitable setting for our study. Furthermore, there is striking heterogeneity in female employment across places and professions. For instance, the gender employment gap is about 12 percent in cities in Northern Italy such as Milan and Turin, but around 30 percent in Southern Italy cities such as Naples and Palermo; larger than 50 percent among engineers and up to 90 percent among electrical trades workers, but around -20 percent among teachers and -70 percent among personal care workers.

2.2 Italian payroll tax

Similarly to most developed countries, payroll taxes in Italy contribute to cover a range of welfare benefits, such as unemployment insurance, maternity leave and sick leave. According to OECD Tax Database, they account for around 13.3 percent of the GDP (or

[5] In the Swiss context, Lalive et al. (2008) find that temporary job subsidies shorten unemployment duration, while training and employment programmes have no effect.

31.3 percent of taxation) in Italy. The total payroll tax burden in Italy is relatively large, representing around one-third of the employee's gross compensation. Both employers and employees are statutorily liable for paying a given share of payroll taxes, with employers bearing a much higher portion. The payroll tax rate depends on the type of work performed by the firm, the number of employees, the firm's legal configuration, the employee's position and employee's registration fund.[6] Employers and employee are liable for the payroll tax from the first euro earned. The employer always deducts at source the payroll tax and liquidates the due payments to the local Social Security authorities by the 16th of each month.

Figure 1 depicts the evolution in the payroll tax rates since 2000. Both the employer and employee tax rate have been quite stable over the last two decades. The employer (normal) tax rate, presented as the blue line in the left-hand side panel, was 23.91 percent in 2000, 21.61 percent in mid-2000s, and 21.11 percent in 2020. The right-hand side panel depicts the employee payroll tax rate series, shown separately for the bottom (red squares) and top tax rate (blue circles) applying to earnings above 47,143 euros in 2020. The two series are flat and do not present much variation: the top tax rate has always been 0.3 percentage points larger than the bottom rate over the 2000-2020 period.

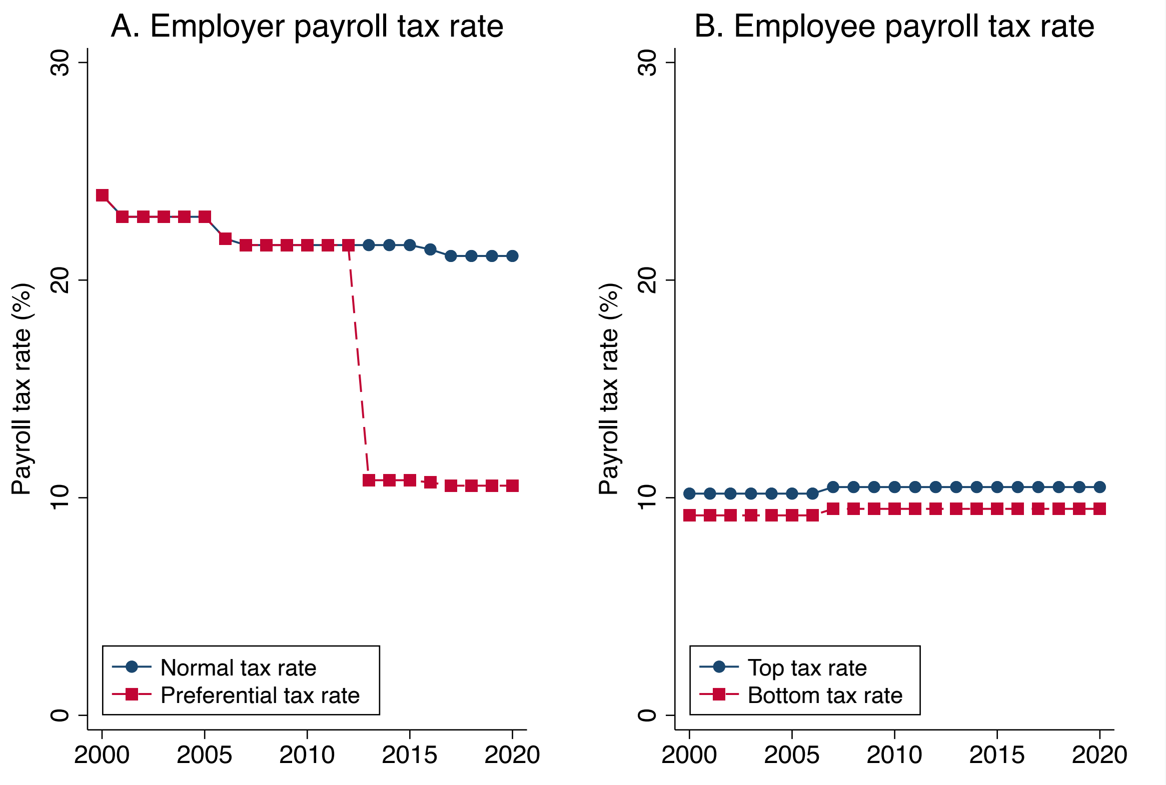
2.3 The 2012 reform

In the context of the sovereign debt crisis in 2011, the Italian labor market was weak and strongly segmented. The high public debt, low productivity rates and the steep growth in the unemployment rate called for structural interventions. As Italy's position on international markets worsened, the Berlusconi government resigned in November 2011 in an attempt to restore market confidence in Italy's capacity to tackle the crisis. This led to the appointment of a new government headed by Mario Monti. The new government swiftly adopted a major labor market reform. This reform, named after the labor minister Elsa Fornero as "Fornero Reform" (28 June 2012 law, n.92, *Disposizioni in materia di riforma del mercato del lavoro in una prospettiva di crescita*), was voted by parliament on June and became effective starting from the 1st of January 2013.

The Fornero reform introduced a permanent employer-borne payroll tax cut for employers that hired unemployed women. The aim of this reform was to spur female employment rate and to stimulate business activity by reducing labor costs. As shown by the blue circles in left-hand side panel of Figure 1, the preferential payroll tax scheme has reduced the employer portion of the payroll tax by 50 percent, hence dropping to 10.805 percent in the reform year and up to 10.55 percent after 2017. Table 1 presents the main features of the payroll tax reform. The eligibility criteria were designed to provide much stronger work incentives along two *not mutually exclusive* dimensions.

[6]Apart from income sources that are not included in the legal definition of earned income for tax purposes, the calculation of the tax base does not include items strictly established for social security purposes (e.g., performance or productivity bonuses, severance indemnity, family benefits).

Figure 1: Payroll tax rates



Note: The figure displays the evolution in the payroll tax rate since 2000. The employer (normal) tax rate, presented as the blue line in the left-hand side panel, was 23.91 percent in 2000, 21.61 percent in mid-2000s, and 21.11 percent in 2020. The right-hand side panel depicts the employee payroll tax rate series, shown separately for the bottom (red squares) and top tax rate (blue circles) applying to earnings above 47,143 euros in 2020. The two series are flat and do not present much variation: the top tax rate has always been 0.3 percentage points larger than the bottom rate over the 2000-2020 period.

First, eligibility criteria were relatively less binding in economically disadvantaged areas (named *eligible municipalities* hereafter).[7] Specifically, the payroll tax cut applies to women that spent at least 6 months in non-employment status and are resident in disadvantaged areas. In all the other municipalities (*not eligible municipalities* hereafter), the minimum non-employment duration requirement is 12 months for women older than 50; 24 months for those younger. Disadvantaged areas are classified as municipalities that are eligible for EU structural funds.[8]

Second, the eligibility criteria favor professions with greater gender imbalance. The

[7]Following evidence on the economic effects of place-based policies (see Kline and Moretti (2014) and Neumark and Simpson (2015) for reviews), the rationale for providing payroll tax rates in poorer areas was to increase economic activity. See also Becker et al. (2010) for the effect of EU structural fund on regional economic growth; Ku et al. (2020) for the role of place-based payroll taxes in stimulating local employment.

[8]A document from the Ministry of Labor clarifies that this must be an area indicated in the regional aid map approved for Italy (see *Decreto del Ministro dello Sviluppo Economico*, 27 March 2008 for a list of eligible areas; INPS document number 6319, 29 July 2014, for its application). Appendix A provides additional details. Figure A1 provides a map of municipalities eligible for structural funds.

Table 1: Eligibility criteria for the payroll tax cut

	Non-employment duration (months) Age < 50	Age ≥ 50
	A. Geographical requirement	
Residence in a municipality eligibile for EU structural funds	6	6
Residence in a municipality not eligibile for EU structural funds	24	12
	B. Male-biased profession requirement	
Hired in a profession with gender employment gap ≥ 1.25 mean gap	6	6
Hired in a profession with gender employment gap < 1.25 mean gap	24	24

Note: This table presents the eligibility criteria for the application of the preferential payroll tax scheme for new female hires. The eligibility criteria were designed to provide much stronger work incentives along two *not mutually exclusive* dimensions. First, eligibility criteria were relatively less binding in economically disadvantaged areas. Specifically, the payroll tax cut applies to women that spent at least 6 months in non-employment status and are resident in disadvantaged areas. In all the other municipalities, the minimum non-employment duration requirement is 12 months for women older than 50; 24 months for those younger. Disadvantaged areas are classified as municipalities that are eligible for EU structural funds. Second, the eligibility criteria favor professions with greater gender imbalance. The minimum non-employment duration requirement is set to 6 months for women hired in professions where the gender employment gap is at least 25 percent larger than the average employment gap; 24 months for all the other professions.

minimum non-employment duration requirement is set to 6 months for women hired in professions where the gender employment gap is at least 25 percent larger than the average employment gap; 24 months for all the other professions. The Ministry of Labor published annually the list of professions eligible for the preferential tax scheme along with official profession-specific statistics on the gender employment gap. Professions are identified by the International Standard Classification of Occupations (ISCO) sub-major group.[9] I report series on the profession-specific gender employment gap in Appendix Table A1, along with information on the cutoff value determining eligibility for the payroll tax cut.

A distinctive feature of the reform is the time limit for benefiting from the payroll tax cut. Specifically, the preferential payroll tax scheme is valid for up to 12 months for temporary jobs; 18 months for permanent jobs.[10] Importantly, the legislator introduced an additional requirement: a firm can qualify for the payroll tax cut only if overall employment would not decrease with respect to past employment. This requirement aims at reducing layoff and to limit the possibility that employers could substitute

[9]The classification follows a grouping by education level and refers to the 2-digit ISCO-08 classification.

[10]The payroll tax cut is extended to 18 months when a temporary job is transformed in a permanent job by the same employer, and when a new hiring follows a previous temporary job.

eligible workers with not eligible workers. Administratively, the payroll tax cut was handled through a software developed by the Italian Social Security Institute, used by employers monthly for their payroll tax payments. This ensures that take-up of the payroll tax cut was immediate and salient, as well as cross-checking that eligibility requirements were satisfied.

2.4 Wage setting and unemployment insurance in Italy

In Italy, wages are set by collective agreement at national level between employer and employee representatives. Wage bargaining sets a wage floor that is a function of several employer and employee characteristics, including job task, tenure and occupational group (see law 289/1989). Unions can stipulate firm-specific contracts that raise these wage floors. Furthermore, firms can also distribute additional premiums and bonuses.[11] Therefore, the two-pillar Italian system can create considerable variation in wages across firms in the same job tasks and across workers within a firm (Guiso et al. 2005). For instance, Card et al. (2014) show that actual wages are above the wage floor for nearly all employees in Italy and the median worker enjoys a wage premium of about 24 percent.

The Italian Unemployment Insurance (UI) system is similar to the other continental European systems in terms of generosity (see De Vincenzi and De Blasio (2020) for details). Workers who become unemployed can benefit from regular UI by an amount that depends on previous earnings. The replacement rate (i.e., UI relative to gross monthly earnings) for the median earner is 75 percent of the average monthly salary received over the previous four years, and up to a yearly updated threshold (that was 1,328,76 euros in 2019), but it reduces by 3 percent after three months. The maximum UI potential duration also depends on work history: it is equal to half of the number of weeks of work during the last four years, and up to a cap of 24 months.

3 Data and recipients' characteristics

3.1 Social security records

I use linked employer-employee data provided by the Italian Social Security Institute (INPS, *Istituto Nazionale di Previdenza Sociale*) throught the VisitINPS program. The data cover the universe of Italian workers in the non-agricultural private sector. They include information on demographics characteristics, such as gender, date of birth, residence and nationality, along with detailed information on earnings and jobs, such as contract type, occupation, profession and reason for hiring or terminate the job contract (including whether the worker was hired with the preferential payroll tax scheme). The data also include several employer-level information, such as the number of employees, municipality of residence and industrial sector. The longitudinal structure of

[11] Wage floors can also differ across provinces, although this is not very common (Boeri et al. 2021).

the data allows me to link employees and employers through a (scrambled) identifier across time periods. Starting from 2005, a month-level version of the data is also made available, collecting the same information as above. I will thus focus on the period starting from 2005 up to the latest available date.

The observation unit in the data is the job spell. Since a worker can be employed for different employers in a given month, there are cases where multiple observations for a given individual in a given month are recorded. To deal with this issue, I select the job spell with the highest number of days worked.[12] Furthermore, I drop any duplicate based on observations that have the same information for a given employer-employee record in a given month.

In the paper, *gross earnings* refer to monthly wage earnings gross of payroll taxes, corresponding to the total labor cost paid by the employer for a given worker.[13] *Net earnings* are composed of monthly wage earnings net of the employer's portion of the payroll tax rate, but inclusive of the employee's payroll tax (and also including income taxes). In addition to regular wages and salaries, earnings also include bonuses, overtime pay and any pay in arrears. Therefore, earnings in Italian social security data represent a broad definition of cash employment income, which is used as the reference for computing the payroll tax burden and is also the standard reference for employer-employee compensation negotiations and decisions.

I define the period of non-employment as the time elapsed (in months) from two consecutive job spells. This approach has the limitation of not covering possible transitions towards public employment or self-employment. Therefore, the dependent variable will only capture employment in the private sector that is available in the Social Security archives. To assess the sensitivity of my results to these transitions, I will employ tax returns data grouped at the municipality-by-income bracket level providing information on the total number of taxpayers. These data would allow me to cross-checking whether any payroll tax-induced employment effects reflect a real response or rather reflect transitions in and out from Social Security archives. I will provide reassuring evidence that transitions toward public employment or self-employment are not the main reasons behind the documented employment effects.

I also collect data from an administrative dataset covering unemployment benefit recipients. This dataset (called *Prestazioni a Sostegno del Reddito*) collects information on start date, the duration and the amount of the benefit and allows to match these information with social security data using the individual identifier.

[12] They account for less than 0.3 percent of observations. Alternative methods, such as selecting observations with the highest wages per week, have no impact in practice.

[13] More precisely, labor costs should be slightly higher if employers also offer fringe benefits on top of regular earnings. Yet, such fringe benefits are not very common in Italy given that the social security system is generous.

3.2 Firms' financial data

The second source of data is firm's balance sheets, coming from the AIDA-Bureau Van Dijk dataset. This database collects annual information for all the companies that are legally obliged to report their financial statement to the Italian Business Register. The data are available for firms with annual sales above 500,000 euros. I observe information on total wage bills, net worth, the book value of capital (broken into several subcategories),[14] value added and labor costs, among the others. The dataset also reports detailed geographic information, the industry (categorized by a five-digit code), the dates of "birth" and closure of the firm (if applicable), and the firm's national tax number.

I match job-year observations for employees with firms' financial data through the fiscal code identifiers. As usual when matching balance sheets information with social security data (see, e.g., Card et al. 2014), the match rate is relatively high for larger firms, but is relatively weak for very small firms that are either unincorporated or fall below the 500,000 euros threshold. Namely, the match rate is larger than 90 percent for firms with 50 or more employee, 60 percent for firms with 15–49 employees, but less than 5 percent for firms below under 15 employees. As small firms are thus severely under-represented, I exclude firms with less than 15 employees in the firm-level analysis. Finally, to reduce the influence of outliers, I remove firm-year observations with unusually high or low values of net worth, value added and capital per worker (defined as those in the bottom or top percentile of these key variables).

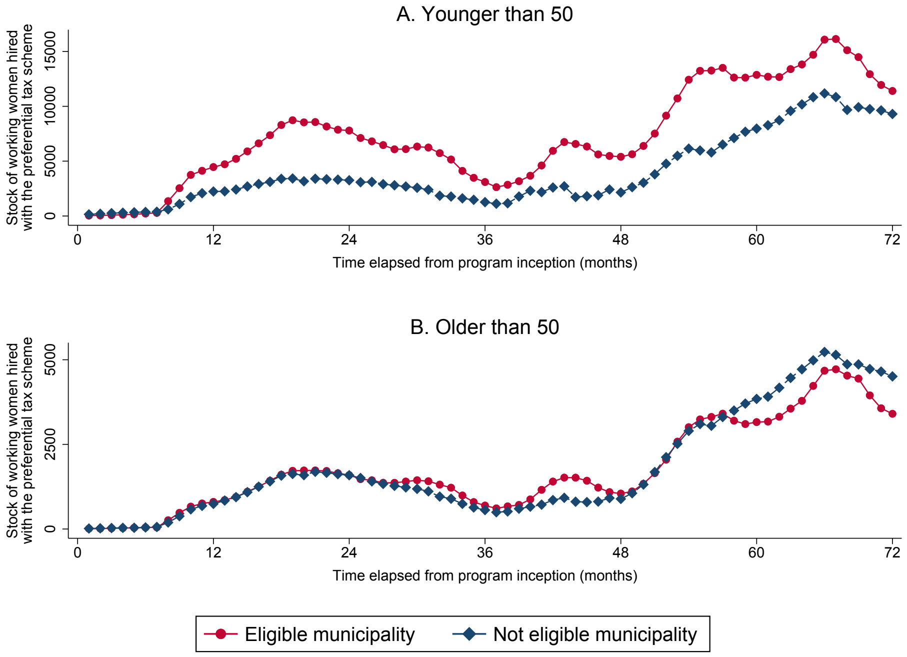
3.3 Payroll tax cut recipients' characteristics

Figure 2 shows the stock of female workers hired with the preferential payroll tax scheme in each month since the introduction (1st of January 2013) up to 31st of December 2018. The top panel presents the stock of new hired women younger than 50, for whom eligibility criteria are less binding in eligible municipalities; the bottom panel shows the same figure for women older than 50, where the eligibility criteria are relatively more similar across municipalities. For each panel, the figure reports separate series relative to eligible municipalities (red circles) and not eligible municipalities (blue squares). Three main findings emerge from the figure. First, hires of women younger than 50 began to rise around eight months after the reform in eligible municipalities, reaching 8,317 hires after 18 months. New hires emerged in non-eligible municipalities as well, although, as expected, to a lower extent (3,395). Second, the evolution in payroll tax cut recipients that are older than 50 is strikingly similar across eligible and not eligible municipalities, that is in line with the fact that eligibility criteria did not differ substantially for this group of women. Third, there is a non-monotonic

[14] The data report information on three broad categories of capital: i. tangible fixed assets (e.g., buildings and machinery); ii. intangible fixed assets (e.g., intellectual property, research and development investments, goodwill); iii. current assets or "working capital" (e.g., inventories, receivables, and liquid financial assets). In my main specification, I refer to "capital" as the sum of these three subcategories.

evolution in the stock of women hired with the preferential payroll tax scheme. After the initial growth, the number of recipients lowered significantly and almost halved after 36 months. Then, there has been a steady rise in the use of the payroll tax cut, especially among women older than 50. The peak was reached in June 2018, when the stock of women (of any age) hired with the preferential payroll tax scheme was 20,828 in eligible municipalities; 16,452 in not eligible municipalities.[15]

Figure 2: Stock of women hired with the preferential tax scheme



Note: The figure shows the stock of female workers hired with the preferential payroll tax scheme in each month since the introduction (1st of January 2013) up to 31st of December 2018. The top panel focuses on women younger than 50, for whom eligibility criteria are less binding in eligible municipalities; the bottom panel shows the same quantity on women older than 50, who faced relatively similar eligibility criteria. For each panel, the figure reports separate series relative to eligible municipalities (red circles) and not eligible municipalities (blue squares).

Appendix Table B1 and Table B2 display summary statistics of payroll tax cut recipients. The tables show the following. First, payroll tax cut recipients are low-wage women (average monthly gross salary is 1,200 euros). These women are mostly native, relatively young (average age is 38), and hired permanently in blue collar part-time jobs. At geographical level, around 60 percent of the payroll tax recipients are women resident in eligible municipalities, while around 44 percent of these women works in a municipality different from the municipality of residence.

Second, the representative employer benefiting from the payroll tax cut operates in large size, experienced firms (average number of employees is 20; average firm age is around 8.6 years). The composition by industrial sector reveals that most all sectors made use of the payroll tax cut, but with an over-representation of the wholesale and

[15]See Figure B1 for additional evidence in take-up rate by age and municipality over time.

retail trade (around half of the sample), followed by accommodation and food service activities (19 percent) and manufacturing (16 percent).

4 How does the payroll tax cut affect labor market outcomes?

Depending on the incidence of the payroll tax, changes in labor costs should lead to changes in wages, employment or both. I thus start the empirical analysis by studying the effects of the payroll tax cut on wages (section 4.1) and employment (section 4.2). Next, as the time-limited nature of the payroll tax cut might induce employers to retain employees until they are eligible for the payroll tax cut, I will focus on the effects of the payroll tax cut on job duration (section 4.3). Finally, I will study whether the payroll tax cut stimulates women to leave welfare by studying the average non-employment duration of women entering unemployment insurance after the reform (section 4.4).

4.1 Wages

4.1.1 Standard tax incidence model

Standard public economics theory suggests that payroll taxes are mostly borne by workers, even if they are nominally shared by employers and employees. The basic assumption behind this result is that labor demand is relatively more elastic than labor supply (see, e.g., Hamermesh 1979; Fullerton and Metcalf 2002).[16] For a simple illustration of this tax incidence result, consider a standard competitive labor market. If gender discrimination is absent, female and male workers with a similar level of human capital are almost naturally perfect substitutes. The introduction of the payroll tax cut makes female workers cheaper and should thus lead employers to hire more (treated) female workers and lay off male workers (or not eligible female workers). With upward-sloping labor supply, these employment effects bid up the wage of female (eligible) workers until the cost of the two groups are equalized. Therefore, in equilibrium, the standard labor market model predicts a wage increase for female workers equal to the payroll tax cut. Yet, this standard tax incidence result might be questioned by any institutional or discriminatory-based wage rigidity as well as frictions in costs of recruiting, training and laying off workers that would make the labor demand less than infinitely elastic and thus prevent wages to adjust.

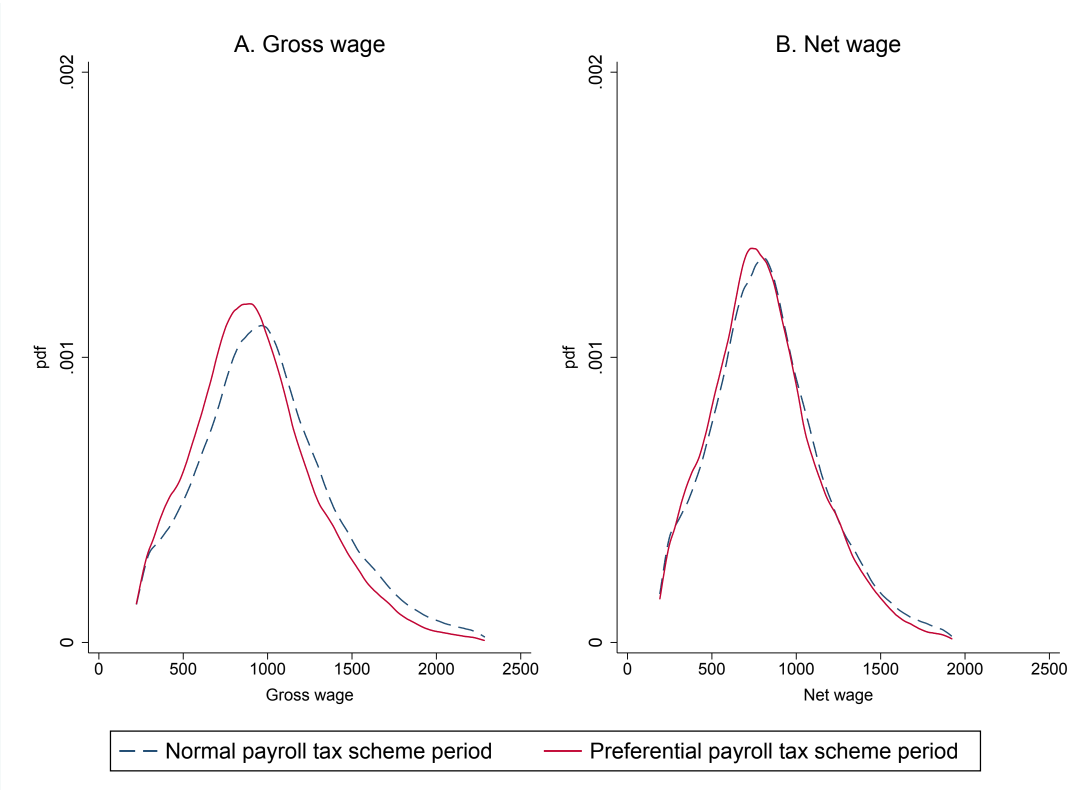
4.1.2 Empirical evidence

To offer prima facie evidence on tax incidence, Figure 3 plots kernel density estimates of the distribution of gross (panel A) and net wages (panel B), before (blue dashed line)

[16] Yet, in his seminal Hamermesh (1979) study, Daniel Hamermesh points out that tax incidence is ultimately "an empirical question, to be resolved using data and estimation techniques that allow the extent of shifting to be measured as accurately as possible." (p. 1210).

and after the 2012 reform (red solid line). The kernel density estimates include all the 135,926 payroll tax cut's recipients. Two main findings emerge from this figure. First, the figure provides graphical evidence that tax incidence is on firms: the *gross* wage post-reform distribution have shifted left, while the *net* wage distributions strongly overlap. Second, it shows that this tax incidence result is homogeneously distributed along the wage distribution. Yet, although suggestive, this graphical evidence is biased by any time-varying confounders that might affect labor costs (but not net wages), such as the business cycle or any change in labor demand and supply over time.

Figure 3: Comparing gross and net wages



Note: The figure depicts the distribution of net and gross of payroll tax monthly wages over the pre-reform period (blue dotted line) and the post-reform period (red solid line). The shifting of the gross wage distribution on the left over the post-reform period suggests that the payroll tax cut was mostly passed on firms. For graphical purposes, I drop observations in the bottom and top 1 percent (they are included in the main analysis). The sample is composed of payroll tax cut recipients that worked before.

The simplest way to account for these issues is to exploit the duration cutoff determining eligibility for the payroll tax cut. Therefore, I can evaluate tax incidence by assessing whether there is any discontinuous change at the cutoff during the job eligible for the payroll tax cut. Under standard tax incidence results, I should see no discontinuity in gross wages but a discontinuity for net wages. For this purpose, I employ the month-level version of the dataset and I focus on the sample of women hired *permanently* with the preferential payroll tax scheme *that have been employed before*.[17]

[17] As I will discuss below, this sample selection choice is necessitated by the empirical approach, as my estimate relies on within-individual between-job variation in net and gross wages, before and after the

I present this exercise in Figure 4, which depicts series on gross and net wages across jobs at different job tenure points (expressed as months since the job signing). The top panel shows the evolution in *gross wages*, defined as monthly wage earnings gross of payroll taxes (i.e., the labor costs paid by the employer). The bottom panel depicts *net wages*, that are monthly wage earnings net of employer's payroll taxes. The horizontal axis displays the evolution in these two variables since the job signing. The vertical line splits the job period length at 18 months, that is when the payroll tax cut expires. For each panel, the figure reports series for the job that started with the preferential payroll tax scheme (red circles) and the previous (not eligible) job (blue squares). The top panel shows a discontinuity in gross wages at the duration threshold over the treatment period, while the series covering the pre-treatment period is continuous. By contrast, the bottom panel displays a continuous series for both the two periods. This provides striking and suggestive evidence that employers do not adjust wages in response to the payroll tax cut.

To present these results more formally, I perform a difference-in-differences (DiD) approach comparing individual-level variation in wages between the job where the payroll tax cut applies with the previous job, before and after the duration cutoff defining eligibility for the payroll tax cut. In this way, I can account for individual-specific unobservables, such as ability, and on any other factors, including job-specific characteristics, that might induce wages to change both over time within a given job and across jobs for a given individual. Specifically, I run the following DiD regressions:

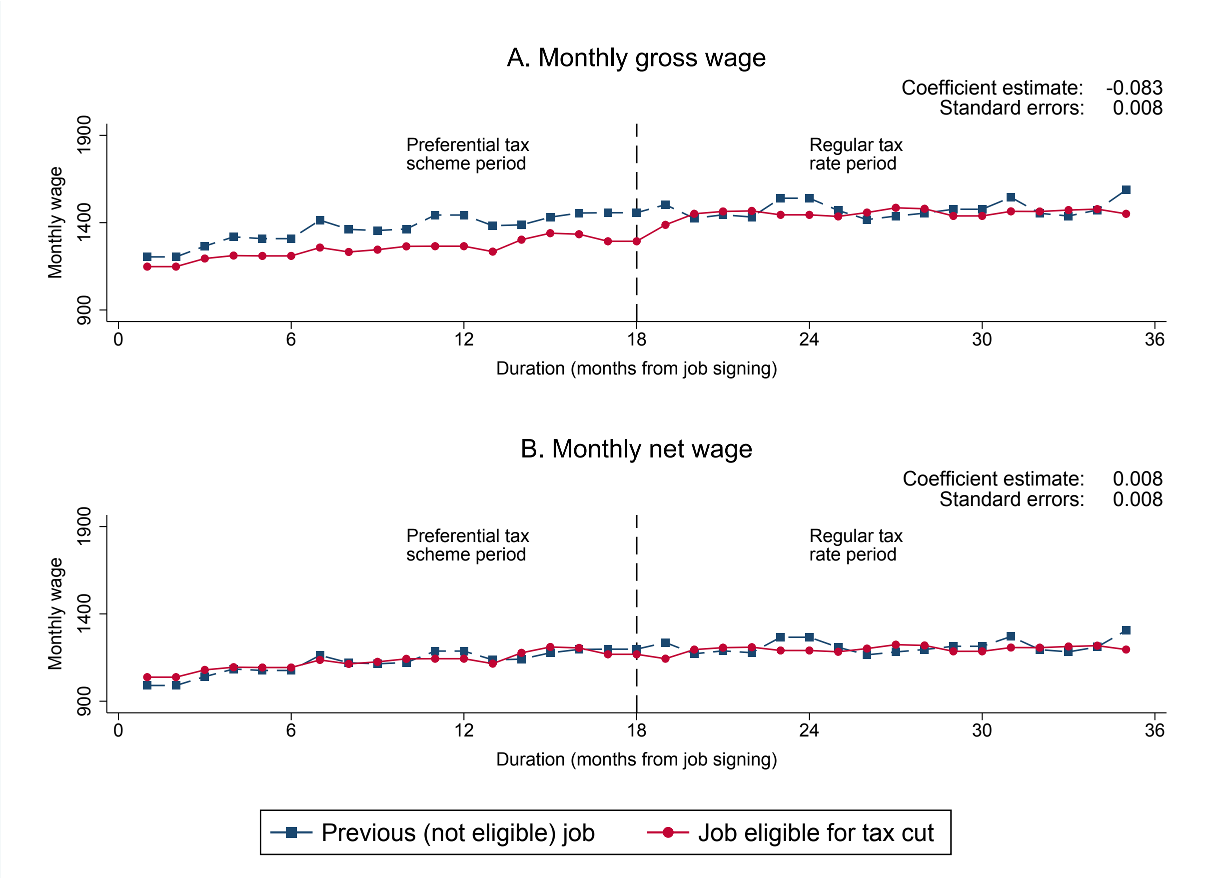
$$log(y_{i,t,j}) = \beta \cdot 1(t \leq 18) \cdot 1(j \in Eligible) + \gamma_{i,t} + \delta_j + u_{i,t,j} \tag{1}$$

where $y_{i,t,j}$ represents wages, gross or net of the employer payroll tax, of worker i at her t^{th} month of the job j. The treatment is given by the interaction between the period before the payroll tax cut expires, $1(t \leq 18)$, and a dummy for the job contract signed during the treatment period, $1(j \in Eligible)$. The coefficient of interest, β, measures the effect of the payroll tax cut on wages. It relies on the assumption that any change in net or gross wages for $t \leq 18$ during the treatment period is due to the payroll tax cut.

One caveat is that the payroll tax scheme might have affected the average job duration. Potential effects on job duration would make my sample endogenous to the reform and distort the distribution of job duration across eligible and not eligible jobs. In fact, as I will show in section 4.3, the payroll tax cut significantly improves job duration. I address this issue by using individual-month of the job fixed effects, $\gamma_{i,t}$. These fixed effects account for any difference in the probability of job survival by leveraging only variation between jobs in a given individual-month of the job cell. This identification strategy ensures that the identifying variation comes from jobs that have the same

18-month duration cutoff. I find that 77.82 percent of women hired with the preferential payroll tax scheme has at least one job spell before 2013.

Figure 4: Incidence of the payroll tax



Note: The figure displays monthly gross (of employer payroll tax) wages in top panel; monthly net wages in the bottom panel. Blue squares refers to the previous (not eligible job); red circles to the job eligible for the preferential payroll tax scheme. The horizontal dashed line defines the duration cutoff determining eligibility of the payroll tax cut. The figure also displays the β coefficient estimate and individual-level clustered standard errors estimated by running equation (1). For graphical purposes, I restrict the analysis up to the 36th month.

duration for a given individual. Then, the inclusion of job fixed effects, δ_j, allows my estimate to rely on within-job variation by comparing wages before and after the 18-month cutoff, thus accounting for any common (intercept) shift in wage earned across eligible and not eligible jobs. Finally, $u_{i,t,j}$ is the error term. I cluster the standard errors at the individual-level.

Table 2 shows the β estimate by running equation (1). The results confirm the qualitative evidence presented in Figure 4: on average, gross wages reduce by 8.3 percent, while net wages present a not statistically significant growth by 0.8 percent. I also report tax incidence as the fraction of the payroll tax cut that benefits the employer, called "pass-through to firms" in the table and computed as the gross wage-coefficient divided by the gross-wage coefficient net of the net-wage coefficient. In the baseline specification, I estimate pass through to firms by 91.2 percent.

To examine tax incidence more thoroughly, in the rest of the table I compute the β coefficient from selected samples of the payroll tax cut recipients' population. I present three main results. First, I study whether tax incidence results vary over the (pre-reform) wage distribution, that, at least in part, would capture heterogeneous effects by skill-level. In principle, wages could be rigid because of the prevalence of collective industry-level wage agreements in Italy. Because these agreements set the industry-level minimum wage, but otherwise allow wages to vary flexibly, they should be binding for low-earners. Yet, I find very limited heterogeneous responses over the wage distribution: pass-through to firms is 87 (100) percent for workers in the bottom (top) half of the wage distribution; this is in line with the graphical evidence provided in Figure 3. This result suggests that collective wage agreements are not the main explanation for the nonstandard tax incidence results.

Table 2: Payroll tax incidence

	Full sample (1)	Below median (2)	Above median (3)	New emp. (4)	New occ. (5)	New mun. (6)	Female emp. (7)
	A. *Outcome*: Monthly gross wage						
$1(t \leq 18)$ $\times 1(j \in Eligible)$	-0.083***	-0.085***	-0.084***	-0.089***	-0.090***	-0.102***	-0.073***
	(0.008)	(0.013)	(0.005)	(0.009)	(0.010)	(0.012)	(0.021)
	B. *Outcome*: Monthly net wage						
$1(t \leq 18)$ $\times 1(j \in Eligible)$	0.008	0.007	0.008	0.003	0.002	-0.010	0.019
	(0.008)	(0.013)	(0.005)	(0.009)	(0.010)	(0.012)	(0.021)
Observations	1,294,215	548,769	378,305	1,085,835	877,153	690,861	241,654
Ind. $\times$ month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Job FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pass-through to firms	0.912	0.924	0.913	0.967	0.978	1.109	0.793

Note: This table presents the results on the incidence of the payroll tax. The coefficient estimate rests on within-individual cross-job variation in wages, before and after the period when the payroll tax cut applies. Each specification includes individual-month of the job fixed effects and job fixed effects. Panel A shows the results on monthly wages gross of employer-borne payroll tax rate. Panel B shows the results on monthly wages net of employer-borne payroll tax rate. The coefficient estimate thus presents the percent change in wages during the period with a preferential payroll tax scheme. Pass-through to firms is defined as the fraction of payroll tax that benefit the firm. The sample in column (1) is composed of women hired with the preferential tax scheme having at least a job spell before 2013. In columns (2)-(7), the analysis is based on the following sub-samples: workers having wages below or above the median (based on pre-reform wages); workers that changed employers; workers that are hired in a new occupation; workers employed in a different municipality; workers having a female employer in the post-reform job. Standard errors in parenthesis clustered at individual level.

Second, I focus on workers that changed employer, industry or the municipality where the workplace is located. Intuitively, eligible workers might bargain more aggressively for a pay increase if willing to increase the geographical sphere of her job search, or to look more extensively for finding a better match with an employer or an industry that would be more specialized in their job task.[18] Yet, columns (4)-(6) show

[18]Comparing the average wage change for men and women that move across firms, Card et al. (2016) show that women benefit relatively less from firm-to-firm mobility. In the Italian context, Del Bono and Vuri (2011) investigate the contribution of gender differences in job mobility to the emergence

that this is not the case: each specification presents full pass-through to firms as in the baseline model.

Third, another possibility is that women bargain less aggressively for a pay increase - and thus obtain a smaller share of the surplus associated with the payroll tax cut - because are influenced by the gender of their employer.[19] Ceteris paribus, transition from a male to a female employer might help overcoming employer gender-related barriers and lead workers to negotiate higher wages. In column (7), I examine this possibility. Although pass-through to firms reduces to around 80 percent in this case, any growth in wages of female workers is small and not statistically significant at usual confidence intervals.[20]

To summarize, this absence of incidence on net wages is in contrast with the standard view in public economics, suggesting that tax incidence is on workers. This result would thus suggest that the payroll tax cut lowers labor costs per-unit and stimulates employment. I will now investigate this possibility.

4.2 Labor force participation

Having established that lower payroll taxes reduce labor costs, I next explore the effects on employment. The Becker (1957) standard model predicts that lower taxes will drive out discrimination, so an exogenous shock to female labor costs through a payroll tax cut should lead to an improvement in women's relative labor market position if there was discrimination during the pre-reform period.

To identify employment effects, I propose multiple empirical approaches, resting on different identifying variations and samples. This choice is motivated by the possibility to shed light on whether payroll tax cuts are a successful strategy when they target places, age groups or professions under a common institutional framework and comparable labor market conditions. Specifically, by exploiting the differential exposure to the payroll tax cut based on the eligibility criteria presented in Table 1, I propose four empirical approaches. First, I compare female employment between municipalities eligible for EU structural fund with those not eligible in an event study framework (section 4.2.1). Second, focusing on municipalities that did not received EU structural fund, I relate employment of women younger than 50 with those older than 50, where the non-employment duration requirement discontinuously drops by 12 months (section 4.2.2). Third, I perform a cross-profession analysis by implementing a regression discontinuity (RD) approach exploiting the discrete threshold which reduces the non-

of a gender wage gap. The authors show that job mobility accounts for nearly one-third of total log wage growth for men, but less than one-tenth for women, and that this difference is mainly due to differences in returns to mobility.

[19]For instance, Bowles et al. (2007) show that the gender of the evaluator is a key driver of the gender gap in the propensity to initiate compensation negotiations.

[20]An additional possibility is that it takes time for wages to adjust. In Table B3, I run equation (1) separately by job signing year to check whether my baseline results differ over time. I find that estimates are relatively similar across years, thus suggesting that at least over the medium-run tax incidence is on firms. This is also consistent with the long-run tax incidence results presented by Saez et al. (2019b).

employment duration requirement by 18 months in eligible professions (section 4.2.3). Finally, my preferred approach (presented in 4.2.4) incorporates the identifying variations presented above to leverage individual-level variation in payroll tax cut eligibility in a given municipality $\times$ cohort $\times$ month cell. Table 3 summarizes the main features of these empirical approaches.

Table 3: Empirical approaches to identify employment effects

Sec.	Empirical strategy	Sample	Outcome variable	Identifying variation
4.2.1	Event study	Women age 25-49	Female workforce in a municipality $\times$ year cell	Cross-municipality differential exposure to the payroll tax cut based on eligibility for EU structural fund
4.2.2	DiD	Women age 46-53 in municipalities not eligible for EU structural fund	Female workforce in a age $\times$ year cell	Cross-cohort differential exposure to the payroll tax cut based on age
4.2.3	RD	Women in municipalities not eligible for EU structural fund	Share of female workers in a profession $\times$ year cell	Cross-profession differential exposure to the payroll tax cut based on gender employment gap
4.2.4	DiD	Full sample	Employment status (0/1)	Cross-individual variation in eligibility in a given municipality $\times$ month $\times$ cohort cell

Note: This table summarizes the four empirical approaches proposed to identify employment effects of the payroll tax cut. The first approach is an event study resting on the differential exposure to the payroll tax cut across municipalities. The second strategy is a difference-in-differences approach comparing employment growth for women older than 50 with those younger, where the non-employment status requirement discontinuously reduced by 12 months in municipalities not eligible for EU structural fund. The third approach exploits the cutoff-rule defining professions where the non-employment duration is reduced to 6 months. In the final approach, identification comes from women that reach the eligibility requirement at different points in time, thus allowing to compare labor force participation and payroll tax cut eligibility in a given municipality-cohort-month cell, before and after the introduction of the payroll tax cut.

4.2.1 Cross-municipality analysis

I start by putting the data into an event study framework to compare employment across municipalities eligible for EU structural fund with those not eligible. The empirical approach rests on the differential exposure to the payroll tax cut based on the non-employment duration requirement. Namely, focusing on women younger than 50 and holding constant any cross-profession variation, the non-employment duration requirement increases from 6 to 24 months depending on eligibility for receiving EU structural fund. This exercise is similar in spirit to the graphical evidence presented in Figure 2 - comparing payroll tax cut recipients across eligible and not eligible municipalities - but a formal event study approach is valuable for three main reasons. First, I can go beyond the mechanical effects (i.e., those directly hired with the preferential payroll tax scheme) by looking at overall female employment as well as on male employment, which are the effects of interest for welfare analysis and policy implications. Second, I can test whether these two groups of municipalities - that differ in several labor market outcomes (see Table B5), including pre-existing female employment share (see Figure B3) - were on similar trend before the reform. Third, I can investigate the dynamics of employment changes after the introduction of the preferential payroll tax scheme.

I run a difference-in-differences (DiD) event study specification of the following form:

$$log(N_{m,t}) = \sum_{j \neq 2012} \beta_j \cdot 1(m \in Eligible) \cdot 1(t = t_j) + \gamma_m + \delta_{t,r(m)} + u_{m,t}, \tag{2}$$

where $log(N_{m,t})$ is the log of the number of workers (male or female) in municipality m at year t. Employment is based on the data described in section 3.1, which provide employment counts for the universe of private-sector employers in Italy. The interaction between a dummy for municipalities eligible for EU structural fund and years, $1(m \in Eligible) \cdot 1(t = t_j)$, omits the year before the reform (denoted by $j = 2012$), so that the DiD coefficient β_j can be interpreted as the employment effect at year t relative to the year before the reform. In the absence of differential pre-existing trends across the two groups of municipalities, $\beta_j = 0 \; \forall j < 2012$. By contrast, for $j > 2012$, the coefficients β_j show the dynamic effects of payroll tax cuts on labor force participation. γ_m are municipality fixed effects, that control for any time-invariant municipality policy or characteristics.

Identification of the β_j coefficients rests on the assumption that observations from not eligible municipalities can be used as a counterfactual for observations from eligible municipalities. Since trends in employment and other socio-economic outcomes are likely to widely differ geographically, I will augment equation (2) by including macro region-year fixed effects, $\delta_{t,r(m)}$. The inclusion of these fixed effects allows me to construct potentially more realistic counterfactuals by comparing changes across municipalities with different exposure to the payroll tax cut within a given macro region.[21] Finally, $u_{m,t}$ is an error term. I cluster the standard errors at the municipality-level.

I also estimate net-of-payroll tax employment elasticity, ϵ, by running a two-stage least squares (2SLS) regression specification of the following form:

$$log(N_{m,t}) = \epsilon \cdot log(1 - \tau_{m,t}) + \gamma_m + \delta_{t,r} + u_{m,t}, \tag{3}$$

[21]Specifically, I interact year dummies with dummies for the following macro regions: i. North-East; ii. North-West; iii. Center-South. Center and Southern Italy are jointly considered as the treatment does not present enough variation across municipalities in the Southern Italy. Likewise, other finest-level interactions, such as province- or region-year fixed effects, are not feasible since there are only few cases when the treatment varies across municipalities within a given region or province.

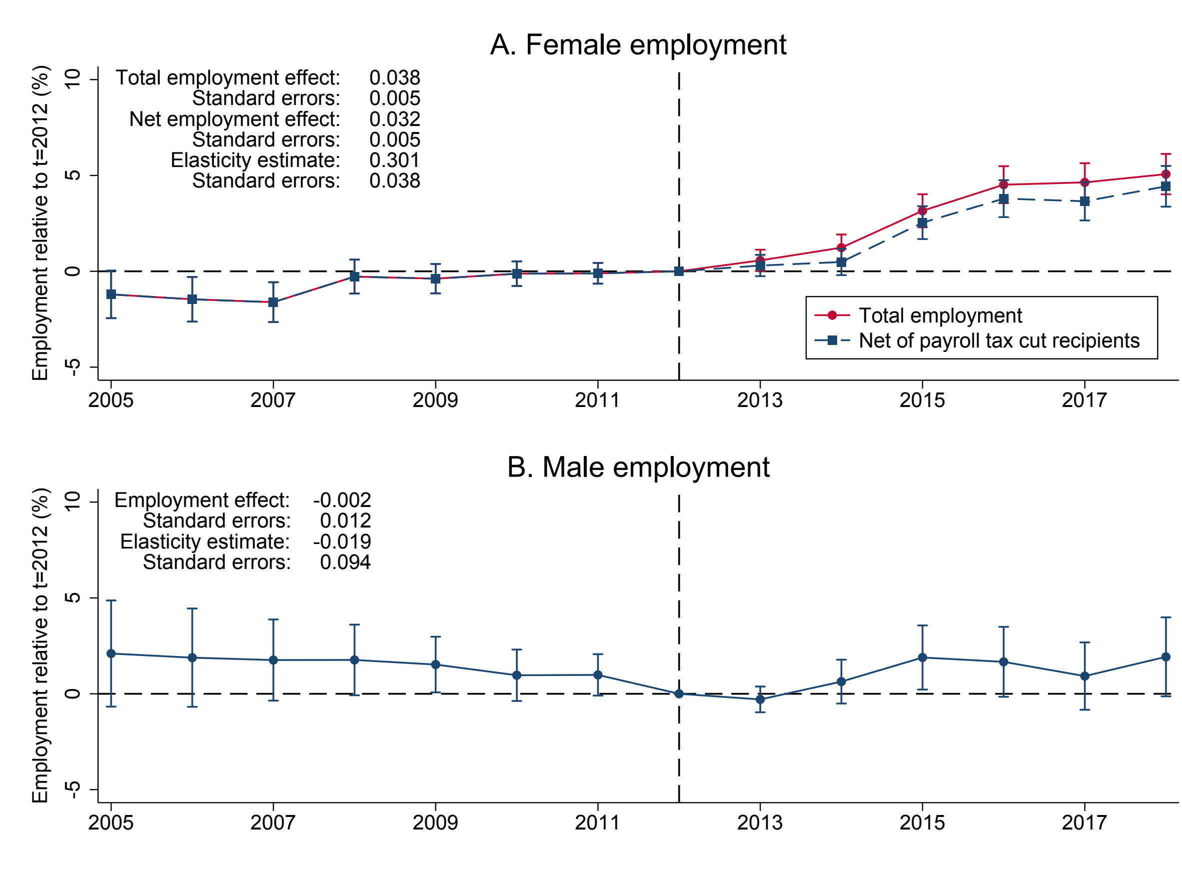
where the payroll tax rate in municipality m at time t, $log(1 - \tau_{m,t})$, is instrumented by the interaction between a dummy for eligible municipalities and a dummy for the post-reform period, $1(m \in Eligible) \cdot 1(t \in Post)$. I compute the net-of-payroll tax rate, $\tau_{m,t}$, as the post-reform payroll tax rate for eligible municipalities over the post-reform period; as the pre-reform payroll tax rate for not eligible municipalities. The elasticity estimate, ϵ, is the Wald ratio of the DiD of the log number of workers to the DiD of the log net-of-payroll tax rate.

Figure 5 displays the β_j coefficient estimates from equation (2): each point shows the effect of having implemented the payroll tax cut for j years (if $j > 0$) or of falsely simulating the reform j years before (if $j < 0$) relative to the year just before the reform. Panel A provides β_j estimates separately for total employment effects (red circles) and net of workers that are currently facing the preferential payroll tax scheme (blue squares). The figure provides three main findings. First, there is compelling evidence of employment responses: female labor force participation increases by around 3.8 percent in municipalities more exposed to the payroll tax cut compared to less exposed municipalities. Strikingly, this effect more than doubled by the third and fourth year into the reform. In elasticity terms, this effect translates in a net-of-payroll tax extensive margin elasticity of 0.301. Second, these effects are more than six times larger than the mechanical effects generated by hiring new workers through the preferential payroll tax scheme. One suitable explanation for this result is that the payroll tax cut improves job duration and let women to remain in the labor force even after the payroll tax cut no longer applies. I will return to this point in section 4.3. Third, the figure shows that there are no pre-existing differences in the female labor force participation trend across the two groups of municipalities.

Given the lack of incidence on net wages, an employer may save 50 percent of payroll taxes by hiring female workers. As long as male and female workers are close substitutes, a profit maximizing firm should hire more (eligible) female workers and lay off male workers (or not eligible female workers). To analyze this possibility, I present β estimates and 95 percent confidence intervals by estimating equation (3) on the full sample of male workers. Panel B of Figure 5 shows that male-female substitution is not likely to be the case behind the document employment effect. This result suggests that firms did not increase layoff to hire new workers at lower cost. I will provide additional evidence in support of the absence of male-female workers' substitutabily in section 5, where I will present firm-level results.

The employment dynamics following the reform might depend on labor market tightness. For example, when maternity leave expires and mothers get pushed off welfare, their ability to find a job will depend on the availability of jobs at the time. I tests this hypothesis by leveraging within-province cross-municipality variation in female employment with labor market tightness - proxied by the pre-exisiting municipality-specific unemployment rate. I report the result of this exercise in Figure B4, which shows larger employment effects in places where unemployment rate was higher. This

Figure 5: Employment effect, cross-municipality approach



Note: The figure depicts the impact of payroll tax cut on the log of (aggregated) female employment in top panel; male employment in bottom panel. The figure plots coefficient estimates and the 95 percent confidence intervals: each point shows the effect of having implemented the payroll tax cut for j years (if $j > 2012$) or of starting the policy in j years (if $j \leq 2012$) relative to the reform inception year. Regressions include municipality fixed effects and year-by-macro area fixed effects. Standard errors clustered at municipality-level. The sample includes the full sample of workers younger than 50.

result is in line with models of employer taste discrimination showing that it is easier to discriminate when labor markets are slack (see, e.g., Black 1995).

Does the payroll tax cut stimulate workers to expand the geographical sphere of their job search? Ideally, since payroll tax cut eligibility is based on residence, workers in eligible municipalities might become more likely to commute if employers in not eligible municipalities raise their labor demand towards (earlier) eligible workers. In Figure B5, I perform an event study based on the probability of commuting from not eligible to eligible municipalities by running regressions as in (3). The figure shows that workers did not alter their work location choices.

One limitation of the data employed in this study is that employment responses might potentially reflect transition in dependent work from public employment or self-employment. As Social Security archives only collect employment information in the private sector, the estimated effects might be misleading. To assess the sensitivity of my results to this possibility, I use information on the total number of taxpayers reporting annual (taxable) incomes below 15,000 using tax returns data provided by the Ministry of Economy and Finance.[22] Figure B6 provides reassuring evidence that the effects are remarkably similar when I use this data source.

4.2.2 Cross-cohort analysis

My second empirical approach compares employment for cohorts of women close to age 50 in municipalities not eligible for EU structural fund. In this context, the minimum non-employment duration requirement drops from 24 to 12 months as a woman turns 50.

Figure 6 presents a difference-in-differences analysis by focusing on female workers with ages 46-53. The vertical axis measures the number of workers by age and year relative to 2012, which allows to absorb any cohort-specific persistent difference in the number of workers. The figure thus displays the deviation of employment by age and year relative to 2012. The basic assumption is that there are no other policy changes or shocks that differently affect women as they become 50, but I allow for any unobserved heterogeneity in employment across age groups.[23]

The figure provides compelling evidence of a gradual and persistent employment effect. One year after the reform, I do not observe any increase in employment for women younger than 50: this is because the basic requirement is that they accrued at least 24 months of non-employment. By contrast, an immediate jump emerges for women older than 50, where the non-employment duration requirement is 12 months. Employment gradually raises over time for both the two groups. Seven years after the reform, the figure shows an increase in the number of workers by around 10 (22.6) percent for women younger (older) than 50.[24]

The figure also displays regression results on the employment response to the payroll tax, based solely on the aggregate cohort-year time series as depicted in the figure. I group the data in age-year cells to run the following difference-in-differences (DiD) specification:

$$log(N_{a,t}) = \beta \cdot 1(a \geq 50) \cdot 1(t \in Post) + \gamma_a + \delta_t + u_{a,t}, \tag{4}$$

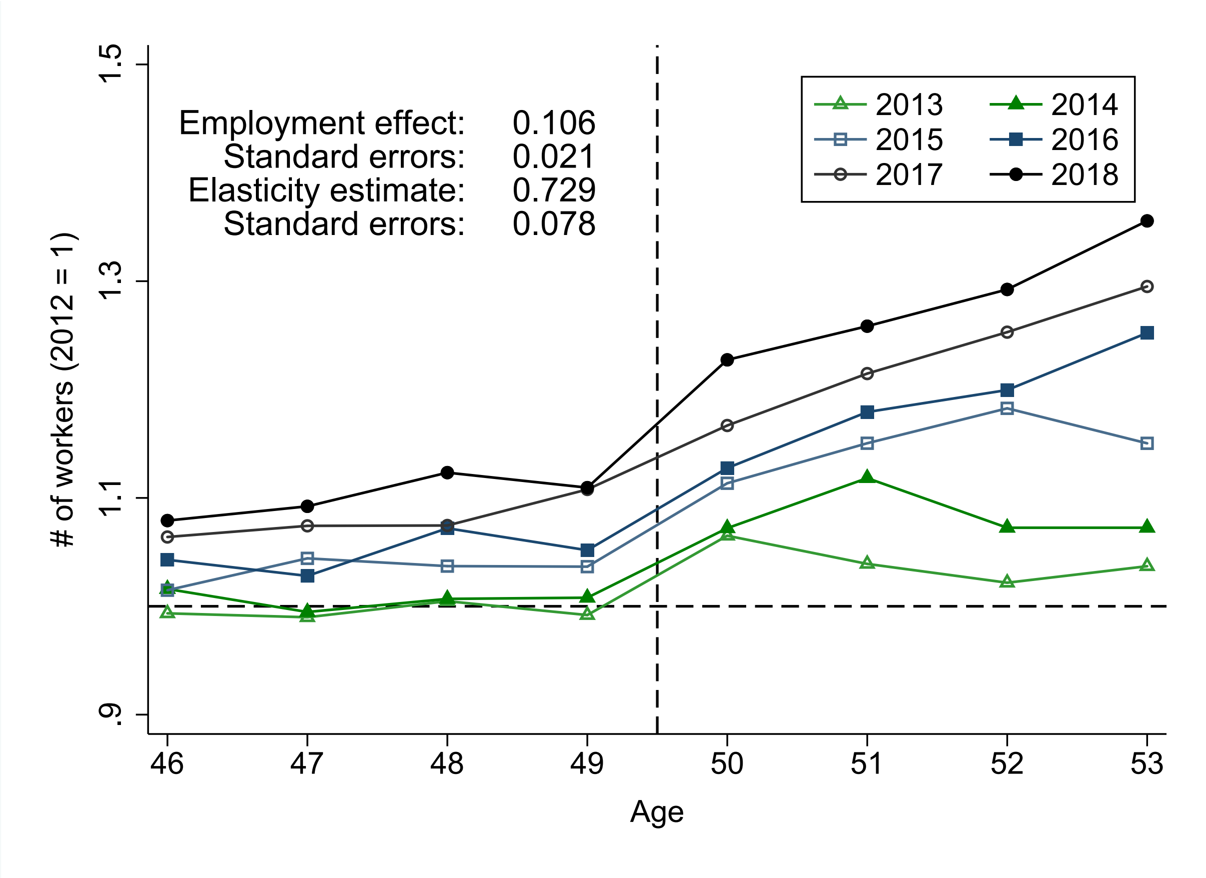
where a = 46, ..., 53 denotes the 8 age categories and t denotes a year over the 2012-2018 period. The outcome variable, $y_{a,t}$, is the log of the number of female workers for age a at period t. The coefficient of interest, β, is computed by interacting the dummy for women older than 50, $1(a \geq 50)$, and the post-reform dummy, $1(t \in Post)$; it measures the effect of (a stricter exposure to) the preferential payroll tax cut on employment. Finally, $u_{a,t}$ is the error term.

[22] I focus on taxpayers whose income is lower than 15,000 because it better approximates average payroll tax cut recipient's earnings. Since the data do not provide separate series by gender, I am assuming that the effects are only coming from women.

[23] Using this approach, I cannot study male employment since there are other tax incentives targeting men that change discontinuously at age 50.

[24] In Figure B8, I add the pre-reform years 2005-2011, which can serve as a placebo test. For these years, I do not detect any discontinuity in employment effects at age 50.

Figure 6: Employment effect, cross-cohort approach



Note: The figure presents a difference-in-differences analysis by focusing on female workers with ages 46-53 in not eligible municipalities, where those younger than 50 create the control group since they were less exposed to the payroll tax cut in municipalities not eligible for EU structural fund. Employment rate is measured relative to 2012, which allows to account for any time-invariant employment difference across cohorts.

I also compute the net-of-payroll tax labor force participation elasticity by running two stage least squares regressions in the spirit of equation (3), that is by instrumenting the net-of-payroll tax rate by the interaction between the dummy for age larger than 50, $1(a \geq 50)$, and the post-reform dummy, $1(t \in Post)$. I estimate an elasticity of 0.729.

4.2.3 Cross-profession analysis

So far, the empirical analysis has shown that the payroll tax cut was successful in raising employment both in places where female employment was particularly weak and for older women. Would the payroll tax cut be successful in professions that are particularly gender segregated?[25] To look at the effects of the payroll tax cut on the share of female employees in gender segregated professions, I focus on women younger than 50 living in municipalities not eligible for EU structural fund. In this context,

[25]Some studies find that economic policies can successfully led to a rise in the share of female employees in a industry (see, e.g., Ashenfelter and Hannan (1986) and Black and Strahan (2001) for the effect of banking sector deregulation on female employment). Yet, these studies focused on industries that were not particularly hostile toward female workers.

the eligibility criteria favor professions with greater gender imbalance: the minimum non-employment duration requirement is set to 6 months for women hired in professions where the gender employment gap is at least 25 percent larger than the average employment gap; 24 months for all the other professions.

Although demand and supply for female workers are likely to differ in both observable and unobservable ways across professions, these differences can be minimized by focusing at professions where the gender employment gap is close to the cutoff defining eligibility for the payroll tax cut. Intuitively, a profession where the gender employment gap is barely above the cutoff is likely to be similar to a profession where the gender employment gap is below the cutoff by the same margin. Thus, I can implement a regression discontinuity (RD) design to identify the causal impact of payroll tax cut on female employment.

Yet, the dynamic nature of the eligibility process complicates the standard RD analysis.[26] As the running variable is a *year-varying* function of the gender employment gap, a profession where the share of female workers is narrowly above the cutoff in a given year is likely to move narrowly below the cutoff in a successive year if the payroll tax cut shortly spurred female labor force participation. In this context, each year is a sharp RD, but the possibility of immediate employment effects introduces fuzziness: a profession in the "control" group - one where the share of female workers is narrowly below the cutoff - might become treated in a successive year.

As the traditional (either sharp or fuzzy) RD design cannot account for this issue, I account for the possibility of dynamic effects in eligibility assignment by exploiting only short-time responses. Following the recommendations of Imbens and Lemieux (2008) and Gelman and Imbens (2019), my main specification uses local linear regressions within a given bandwidth of the treatment cutoff, and controls for the running variable (i.e., 1.25 * average gender employment gap defined at $t = -2$) on either side of the cutoff.[27] Specifically, I estimate a first-difference RD design by running the following equation:

$$\begin{aligned} \Delta Share_p = \beta \cdot 1(Gap_{p,t-2} \geq C_{t-2}) + \gamma \cdot (Gap_{p,t-2} - C_{t-2}) + \\ \delta \cdot (Gap_{t-2} - C_{t-2}) \cdot 1(Gap_{p,t-2} \geq C_{t-2}) + \Delta u_p, \end{aligned} \tag{5}$$

where the outcome variable, $\Delta Share_p$, is the first difference in the share of female worker in profession p. $1(Gap_{p,t-2} \geq C_{t-2})$ is an indicator for payroll tax cut eligibility after 6 months of non-employment (instead of 24 months); it is equal to 1 if the gender employment gap of profession p at time $t-2$, $Gap_{p,t-2}$, is above the cutoff C_{t-2}. β is the coefficient of interest; it measures the local average treatment effect (LATE) of (a stricter exposure to) the payroll tax cut on the gender employment gap. Finally, Δu_p is

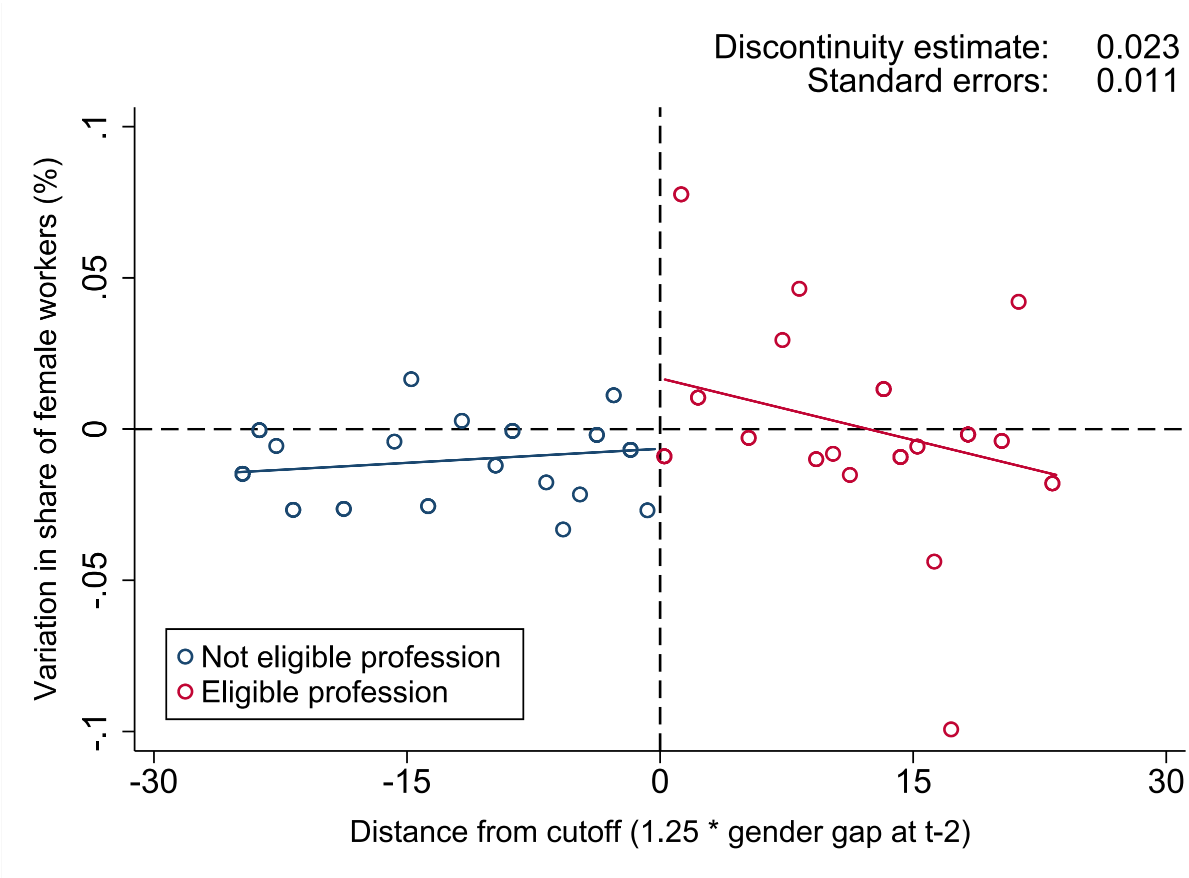
[26] See Cellini et al. (2010) for an attempt to account for dynamic treatment effects in a RD design.

[27] Specifically, I run local linear regressions with a triangular kernel optimal bandwidth, computed using the algorithm proposed by Calonico et al. (2014).

the error term. Standard errors are clustered at the profession level.

Figure 7 presents the effect of stricter exposure to a payroll tax cut on the gender employment gap. The graph shows a discontinuity in this relationship, thus providing graphical evidence that the preferential payroll tax scheme significantly curbs the gender employment gap by increasing female labor force participation. I estimate a β coefficient of 0.023, suggesting an annual growth rate of about 2.3 percent in the share of female workers in professions more exposed to the payroll tax cut.

Figure 7: Employment effect, cross-profession analysis



Note: The figure presents the effect of stricter exposure to a payroll tax cut on the gender employment gap. The figure also reports the β coefficient and profession-level clustered standard errors estimated from (5); it measures the LATE of (a stricter exposure to) the payroll tax cut on the gender employment gap. The horizontal axis is the distance from the cutoff (i.e., 1.25 * average gender employment gap defined at $t = -2$). The vertical axis is the first-difference in the share of female workers in a profession. Scatter points are sample average over intervals of 2 cutoff points bins.

I check the robustness of this result in the online appendix by presenting three additional tests. First, Table B6 tests the sensitivity of my baseline estimate to the choice of polynomial order. The coefficient estimate remains unchanged, but it is less precisely estimated when adding second- or third-factor polynomials. Second, I depart from the baseline first difference model by using a model with profession and year fixed effects. In this case, identification stems from within-profession year-to-year variation in eligibility for the preferential payroll tax scheme. The coefficient estimate does not change. Finally, in Figure B9, I focus on municipalities eligible for EU structural fund, which can serve as a placebo test. As the minimum non-employment duration requirement does not change across professions in this group of municipalities, I do not expect to find any discontinuity. Reassuringly, I find a smooth distribution around the cutoff.

4.2.4 Micro-level analysis

An important concern for the estimation strategies presented so far is that the definition of treated and control groups is based on differences in *exposure* to the payroll tax cut. This might lead to bias in the estimated effect of the payroll tax cut on female labor force participation. To overcome this issue, I create an individual-specific treatment that can incorporate all the sources of payroll tax cut eligibility presented above. For this purpose, here I draw the analysis on month-level data to create an individual-specific treatment based on the period of non-employment (in months) from two consecutive job spells. By observing municipality of residence, age and profession of each individual, I can thus create a dummy, $D_{i,t}$, which is equal to 1 as an individual i meets the minimum non-employment duration requirement at time (month) t; 0 otherwise. Using this approach, I am thus able to leverage variation in eligibility for the payroll tax cut, instead of some measures of exposure to the payroll tax.

Specifically, I compare within-individual variation in labor force participation and payroll tax cut eligibility by running difference-in-differences (DiD) equations of the following form:

$$y_{i,t} = \alpha \cdot D_{i,t} + \beta \cdot D_{i,t} \cdot 1(t \in Post) + \gamma_i + \delta_t + u_{i,t}, \tag{6}$$

where $y_{i,t}$ defines labor force participation: it is equal to 1 if individual i works a positive number of hours during a month t; 0 otherwise. The coefficient of interest is β, which computes the effect of payroll tax cut eligibility on labor force participation. The treatment status is given by the interaction between a dummy for the post-reform period, $1(t \in Post)$, and a dummy for treatment eligibility, $D_{i,t}$. Individual fixed effects, γ_i, account for any time-invariant individual-specific factors.[28] Month fixed effects, δ_t, account for any common month-level shocks. Finally, $u_{i,t}$ is an error term. I cluster the standard errors at the municipality-level.[29]

Top panel in Table 4 shows the β coefficient estimates from equation (6), while the bottom panel presents the effects in elasticity terms by regressing labor force participation on the net-of-payroll tax rate. I start from a simple model containing individual fixed effects and then I add time (month) fixed effects interacted with municipality fixed effects (column 2). These fixed effects allow to absorb any municipality-specific

[28] $D_{i,t}$ does vary within-individual over time because it turns on only after having accrued enough months of non-employment. Obviously, $D_{i,t}$ can be equal to 1 multiple times for a given individual. It might present variation in a given municipality-month cell because not all individuals reach eligibility at the same time.

[29] Standard errors estimates hardly differ when I use alternative clustering approach, such as the municipality-cohort level.

time-varying factors in labor market demand (e.g., improved public transportation that lower commuting times and makes working less costly) or supply (e.g., opening of a new firm plant in a given municipality) by leveraging variation across individuals within a given municipality-month cell. In columns (3) and (4), I add month-by-age fixed effects and municipality-by-age fixed effects. These fixed effects reduce the chances that unknown shocks or policies that differentially affect women with different demographic characteristics are confounding the effect I ascribe to the payroll tax cut. The coefficient estimate is precisely measured and stable across specifications. My most conservative estimate suggests that the participation rate of eligible women during the post-reform period increased by a statistically significant 2.9 percentage points (5.2 percent increase). This translates into a net-of-payroll tax extensive margin elasticity of 0.385.

Table 4: Employment effect, micro-level evidence

	Outcome: $1(i \in$ labor force at time $t)$			
	(1)	(2)	(3)	(4)
	A. Employment effect			
$D_{i,t} \cdot 1(t \in Post)$	0.054***	0.034***	0.035***	0.029***
	(0.002)	(0.002)	(0.002)	(0.001)
	B. Elasticity estimate			
$log(1-\tau_{i,t})$	0.413***	0.391***	0.396***	0.385***
	(0.012)	(0.012)	(0.012)	(0.012)
Observations	948,345,949	948,345,949	948,345,949	948,345,949
Individuals	8,841,137	8,841,137	8,841,137	8,841,137
Individual FE	Yes	Yes	Yes	Yes
Municipality × month FE	No	Yes	Yes	Yes
Month × age FE	No	No	Yes	Yes
Municipality × age FE	No	No	No	Yes
Mean dependent (pre-reform)	0.556	0.556	0.556	0.556

Note: This table reports the effect of the payroll tax cut on female labor force participation (top panel), and the net-of-payroll tax rate female labor supply elasticity at the extensive margin (bottom panel). The outcome variable is a dummy equal to 1 if a worker is employed at time (month) t; 0 otherwise. The sample include all women covered in Social Security archives during the 2005-2018 period. Standard errors in parenthesis clustered at individual level.

4.3 Job tenure

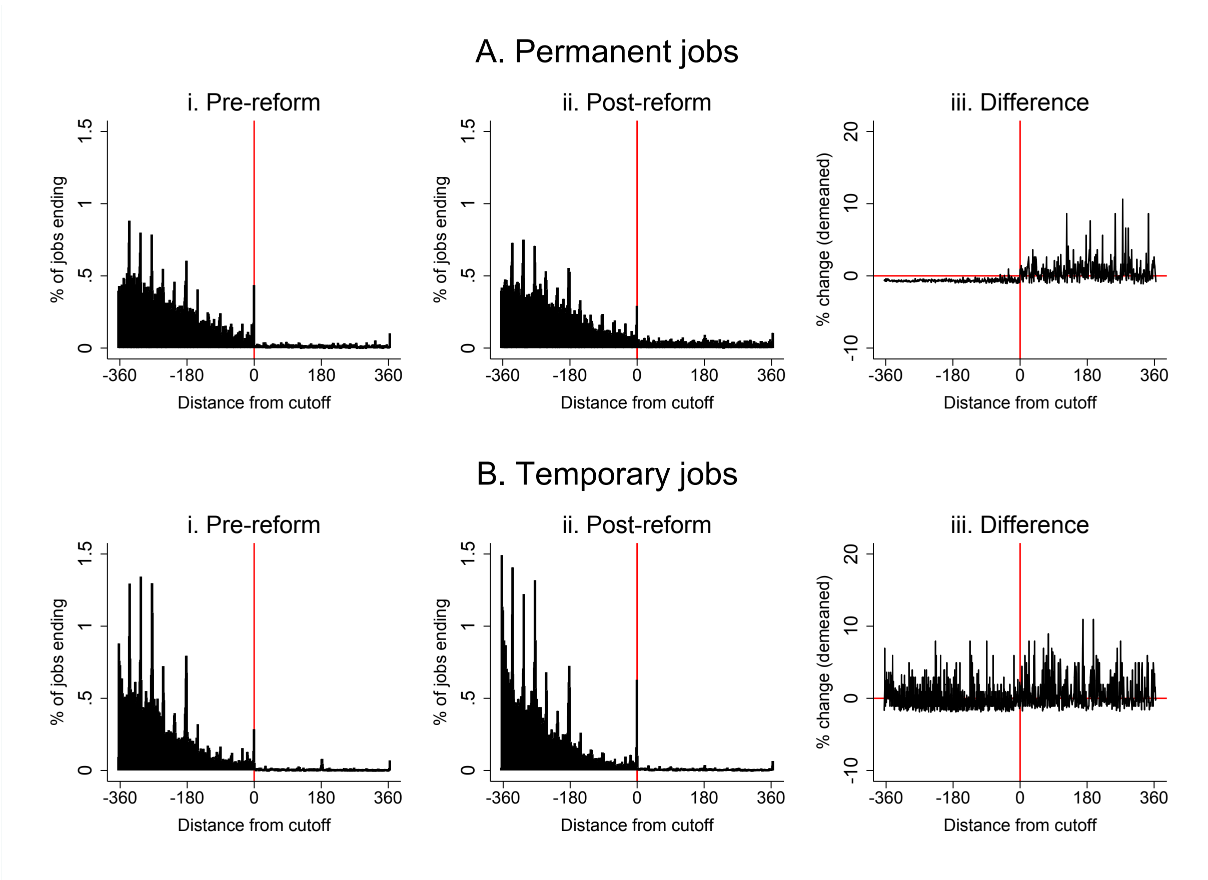
The payroll tax incidence analysis implies that payroll tax cuts create incentives to hire and retain eligible women, at least until the expiration of the preferential tax scheme. A natural following question is: what happened next? Do employers retain them even after the payroll tax cut no longer applies?

To study the effect of the preferential payroll tax scheme on job tenure, I exploit variation created by the cutoff determining eligibility for the preferential tax scheme.

The time-limit application of the payroll tax cut creates a notch: a discontinuity in the choice set of labor cost versus job duration. Such a notch introduces an incentive for offering job with duration just below the duration cutoff, thereby creating a hole in the job duration distribution just above the duration cutoff and excess bunching just below the duration cutoff.

To test this prediction, I focus on the sample of payroll tax cut's recipients that worked before 2013. For offer prima facie evidence on whether the payroll tax cut has any impacts on job duration, Figure 8 depicts the distribution of job duration for permanent (top panel) and temporary jobs (bottom panel), before (left-hand side graphs) and after (central graphs) the payroll tax cut. The horizontal axis shows the distance (in days) from reaching the cutoff determining when the preferential payroll tax applies (i.e., 18 months for permanent jobs and 12 months for temporary jobs). In the right-hand side graphs, I show the difference in job duration between post- and pre-reform period, demeaned by the average difference between the two periods. The figure provides evidence that jobs starting with the preferential payroll tax scheme lasted longer.

Figure 8: Job duration



Note: This figure depicts the distribution of job duration before (left-hand side panel) and after (central panel) the introduction of a preferential payroll tax scheme for permanent jobs (top panel) and temporary jobs (bottom panel). The right-hand side graphs depict the percentage change in job duration after the reform. The sample includes all workers hired with the preferential payroll tax scheme.

To quantify this effect more formally, I implement a difference-in-differences approach comparing the average duration of jobs signed before and after the preferential tax scheme inception, for job duration below and above the cutoff determining the period when the payroll tax cut applies. I run regressions of the following form:

$$log(d_{i,t}) = \alpha \cdot 1(d_{i,t} > cutoff) + \beta \cdot 1(d_{i,t} > cutoff) \cdot 1(t \in Post) + \gamma_i + \delta_t + u_{i,t} \quad (7)$$

where the outcome variable, $d_{i,t}$, measures job duration (in days). The coefficient of interest is β, which measures the percent change in job duration over the post-reform period, conditional on remaining employed even after the payroll tax cut no longer applies. I include individual fixed effects, γ_i, and year (of job signing) fixed effects, δ_t. The error term is $u_{i,t}$. Standard errors are clustered at the individual level.

Table 5 reports the α and β coefficient estimates. I start from a baseline model with individual and year fixed effects (column 1). The model in column (2) exploits variation in job duration across individual in a given municipality by augmenting the baseline model by municipality-by-year fixed effects. Column (3) controls for the possibility that any effect on job duration could reflect the mechanical increase in experience for a given individual across the two jobs by interacting the year dummies with age fixed effects. Another possibility is that the payroll tax cut impacts employment not only over the extensive margin, but also on hours worked. In column (4), I further interact individual fixed effects with dummies for full-time and part-time jobs, which capture any change in hours worked across jobs for a given worker. The results are very stable across specifications and indicate that the payroll tax cut increases average job duration by about 60 percent (i.e., by 207 days).

There are several explanations for the increase in job tenure experienced by payroll tax cut recipients. First, the absence of substitution between (previously eligible) incumbent workers with non-employed eligible workers might reflect frictions in the cost of hiring (search costs) or terminating a job (firing costs).[30] Second, there might be on-the-job training and learning that generate job match-specific skills and ultimately create rents. These rents would make any new (payroll tax cut eligible) hires an imperfect substitute for incumbent workers (Manning 2011).

4.4 Duration of unemployment insurance benefits

So far, the empirical analysis has been silent on whether the rise in female labor force participation reflects a response coming from unemployed women or it move women out of the welfare system. Shedding light on this mechanism is important for evaluating the welfare effects of the payroll tax cut. If additional employment is coming from women that were benefiting from unemployment insurance (UI) benefits, then missing

[30] Job security legislation is strongly regulated in Italy. Firms can terminate permanent job contracts exclusively for either objective reasons (i.e., financial distress) or subjective reasons such as improper conduct by the worker.

Table 5: The effects of the payroll tax cut on job duration

	Outcome: log of job duration (days)			
	(1)	(2)	(3)	(4)
$1(d_{i,t} > Cutoff)$	2.388***	2.377***	2.364***	2.339***
	(0.012)	(0.015)	(0.015)	(0.016)
... $\times 1(t \in Post)$	0.601***	0.607***	0.621***	0.612***
	(0.015)	(0.018)	(0.018)	(0.019)
Observations	183,752	183,752	183,752	183,752
# of individuals	51,040	51,040	51,040	51,040
Individual FE	Yes	Yes	Yes	Yes
Year (of job signing) FE	Yes	Yes	Yes	Yes
Municipality-year FE	No	Yes	Yes	Yes
Age-year FE	No	No	Yes	Yes
Individual-job type FE	No	No	No	Yes
Mean outcome (days)	345	345	345	345

Note: This table shows the effects of starting a job with the preferential payroll tax scheme on job duration. The outcome variable the log of job duration (in number of days). Each specification includes individual and year (of job signing) fixed effects. Column (2) includes municipality-by-year fixed effects. Column (3) controls for age-by-year fixed effects. In column (4), I add interactions between individual fixed effects and job contract (i.e., full-time vs part-time). The sample includes payroll tax cut's recipients that worked before 2013. Standard errors in parenthesis clustered at individual level.

revenue from the payroll tax cut would be counterbalanced by the reduction in fiscal costs due to UI benefits.

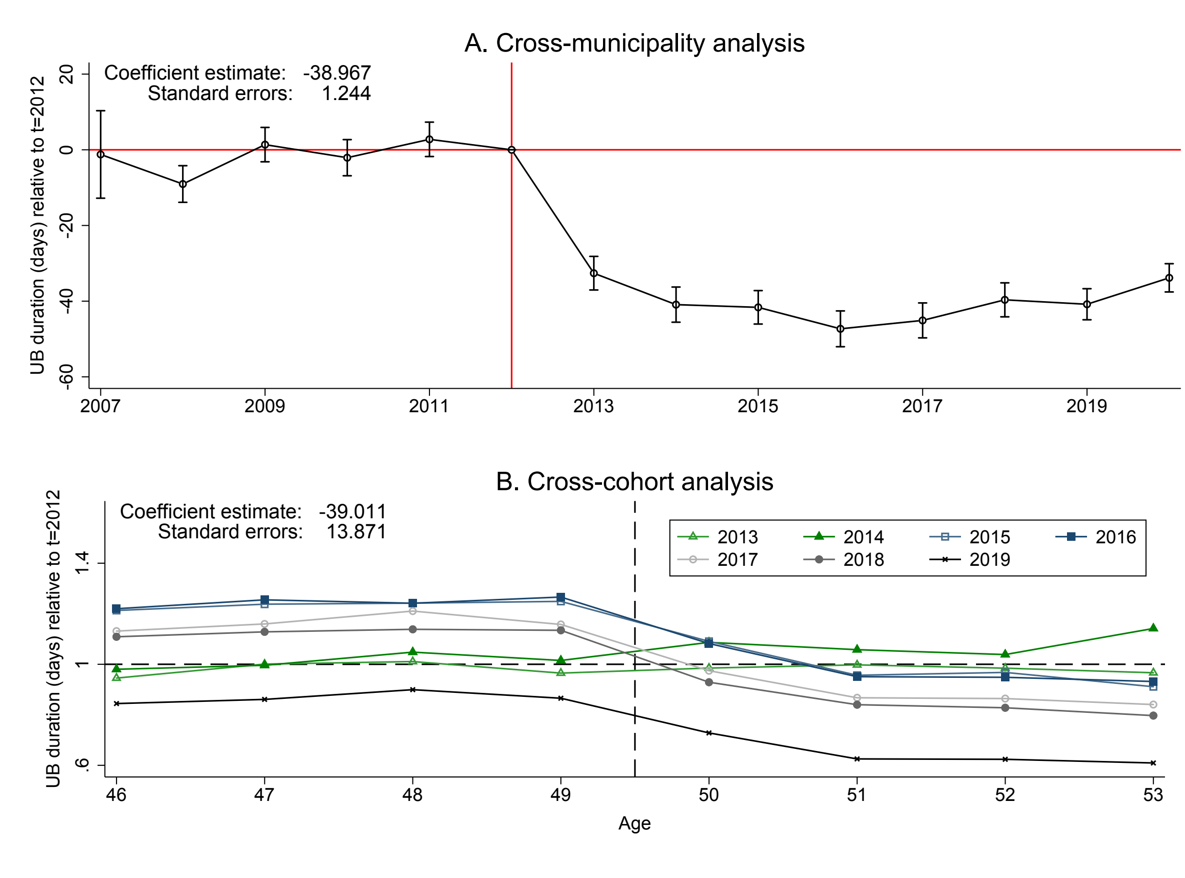
Since the payroll tax cut does not directly alter the compensation for unemployment provided by the UI, it is not a priori obvious whether the payroll tax cut affects workers' probability to reduce UI benefits duration. At the one hand, we expect an increase in demand for (eligible) unemployed women after the introduction of the payroll tax scheme. Therefore, by raising the number of job vacancies, the payroll tax cut should lower the average duration of UI benefits. On the other hand, there might be an "entitlement" incentive (Card and Hyslop 2005) for workers to choose welfare over work until eligibility requirements for UI benefits are meet, and to leave welfare and find a job only as soon as the UI expires. In this case, given that the maximum benefit duration is larger than the non-employment duration cutoff determining eligibility for the payroll tax cut, we should expect limited, if any, effects.[31]

To investigate this question, I collect data on the universe of recipients of unemployment benefits. Then, I study the relationship between average UI benefits duration and exposure to the payroll tax cut by using the cross-municipality and cross-cohort empirical approaches presented previously (see equation (2) for the cross-municipality approach and (4) for the cross-cohort approach). The average time spent on welfare is captured by the *effective* duration, expressed in number of days, of the UI benefits.

[31]Search theory also yields an ambiguous prediction on the effects of the reform on UI benefits duration: payroll tax cut-induced higher labor demand raises the probability of receiving a job offer, but it also tends to increase reservation wages.

[Figure 9](#) presents the effects of the payroll tax cut on the duration of UI benefits. In panel A, I replicate the cross-municipality analysis presented in equation (2) using the duration of UI benefit as outcome variable. The graph shows that the payroll tax cut significantly decreases the duration of UI benefits: women located in eligible municipalities spend around 39 days less on welfare compared to those located in not eligible municipalities. Given that the average UI benefits duration over the pre-reform year was 480.95 days in eligible municipalities, this estimate translates into a drop by 8.1 percent. Furthermore, Appendix Figure B10 shows that the payroll tax cut almost entirely closes the geographical gap in terms of UI benefits' duration.

Figure 9: Payroll tax cut and duration of unemployment insurance



Note: This figure shows the effect of the payroll tax cut on the effective duration of UI benefits (in days). The top panel relies on cross-municipality variation in exposure to the payroll tax cut; the bottom panel on variation across cohorts in municipalities not eligible for EU structural fund. The figure reports coefficient estimate and standard errors clustered at municipality (top panel) or cohort level (bottom panel) from equations as in (2) and (4), but using UI benefits duration as outcome variable.

In panel B, I report results from the cross-cohort analysis as in specification (4). In line with the cross-municipality approach, the figure provides evidence in support of the negative effect of the preferential payroll tax scheme on the UI benefits duration. As the average number of days spent on UI benefits was 324.33 days for women between 50 and 54 years old in not eligibile municipalities, the coefficient estimate translates into a reduction of 12.03 percent.

These results imply that the payroll tax cut reduced the fiscal externalities of unemployment benefits: by receiving benefits for a shorter time, women release resources that can be invested in other public budget items and offset, at least in part, the revenue losses from the payroll tax cut.

5 What happens when female employment raise?

The worker-level analysis has provided clear evidence that the payroll tax cut was passed on firms in terms of lower labor costs. A following natural question is to ask what happens to firms that hired many (eligible) female workers. Do these firms just replace female with male workers? Do they substitute labor with capital inputs? Does the payroll tax cut ultimately improve the performance of these firms?

5.1 Empirical strategy

To address these questions, I leverage between-firm exposure to the payroll tax cut generated by the pre-reform gender composition of their workforce. As the eligibility criteria for the payroll tax cut target areas and industries with larger gender employment gap, firms with a lower share of female workers would be more likely to receive the payroll tax cut by hiring a female worker. Therefore, I can implement a longitudinal analysis on the effect of raising female employment on firm-level outcomes by comparing firms by their pre-reform share of female workers.[32]

Specifically, I combine a broad range of firm-level outcomes from firms' balance sheets with information on the gender composition of their workforce from the Social Security archives. I compute the share of female workers for each firm and then I divide firms by quintile of this key variable (see Figure B11). My empirical approach exploits the differential exposure to the payroll tax cut between firms in the bottom quintile (denoted as low share female) against firms in the next quintile (called fairly low share female) of the pre-reform female share distribution. As the payroll tax cut could have affected firm entry or die, I focus on firms that already existed before the reform.[33] I then run the following basic DiD specification:

$$log(y_{i,t}) = \beta \cdot 1(i \in LowShareFemale) \cdot 1(t \in Post) + \gamma_i + \delta_t + u_{i,t}, \tag{8}$$

where the outcome, $y_{i,t}$, is measured for firm i at year t. The coefficient of interest, β, measures the treatment effect by the interaction between firms with a low share of female workers, $1(i \in LowShareFemale)$, and the post-reform period, $1(t \in Post)$. γ_i

[32]This empirical approach is similar in spirit to Saez et al. (2019b), which leverage firm-level variation in exposure to the payroll tax cut in Sweden generated by pre-existing, persistent age composition of their workforce. Furthermore, the literature focusing on the effects of minimum wage has implemented similar empirical strategies by exploiting between-firm variation in the fraction of minimum wage workers (see, e.g., Card and Krueger 2000).

[33]Firm survival is an outcome of interest in its own right. I find that firms with a low pre-exisiting share of female workers are no more likely to survive (or die) than firms with a fairly low share of female workers.

and δ_t are firm and year fixed effects, respectively. $u_{i,t}$ is the error term. The standard errors are clustered at the 2-digit industry-level (about 98 clusters).[34]

Table B7 provides summary statistics during the pre-reform period on the two groups of firms. Four points are worth noting. First, firms presents a significant difference in the number of female workers (1.8 in low share female vs 6.5 in fairly low share female), but they differ less in overall employment (42 vs 44), thus suggesting cross-firm differences in gender composition rather than in their size. Second, low share female firms were relatively less productive, employed less capital per-worker and present a lower net worth over the pre-reform period. Third, the two groups of firms present some small differences in their use of part-time jobs (7.5 percent of the workforce in low share female vs 8.9 percent in fairly low share female) and in the share of temporary jobs (23.7 percent of the workforce vs 17.1 percent). Finally, as expected, firms with a lower share of female workers are much more likely to operate in municipalities eligible for EU structural fund.

5.2 Results

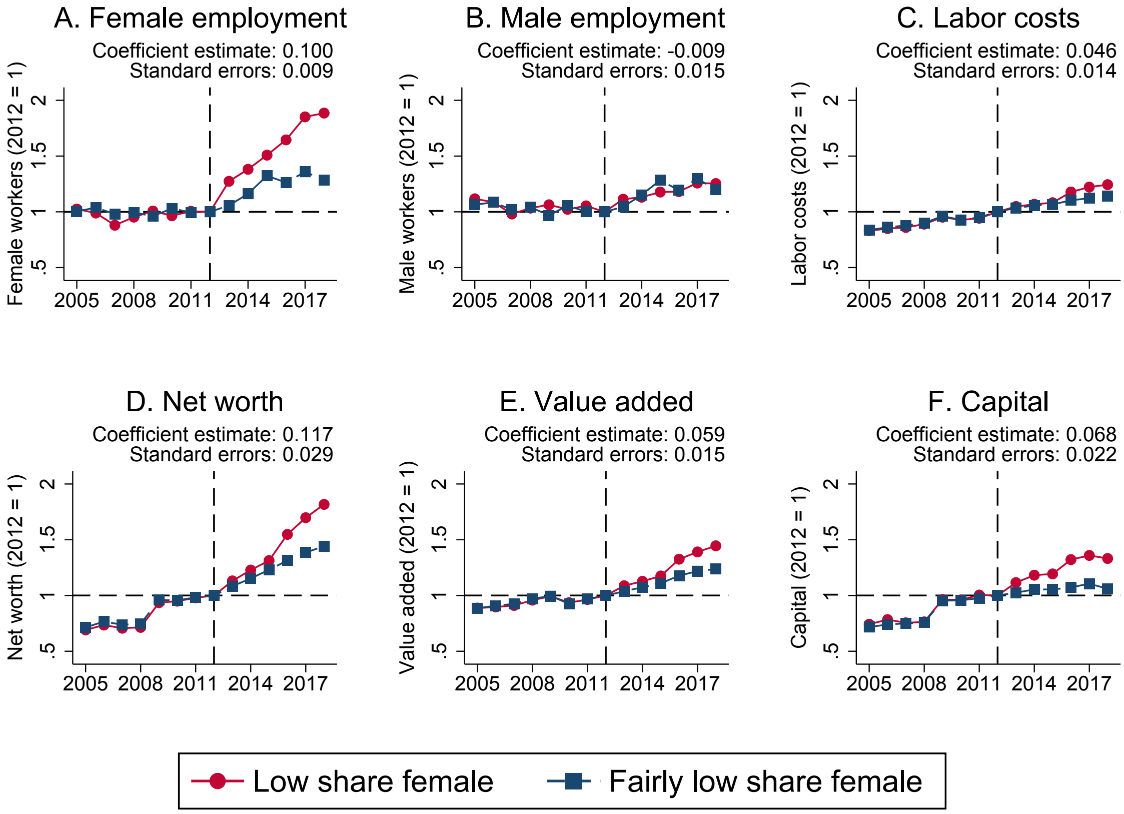
I start by providing graphical evidence in Figure 10, which plots the time series of average outcomes for groups of firms from 2005 to 2018. For each group of firms, I normalize outcomes relative to the year before the reform. In each panel, the figure traces out various longitudinal outcomes. All panels show that the two groups of firms have parallel pre-reform trends and the group with the lower share of female workers (and hence more exposed to the payroll tax cut) experiences faster growth in female employment, per-worker net worth, value added and capital. By contrast, the series on male employment present a parallel trend even after the reform. This has two main implications. First, employers did not substitute cheaper female workers with male workers. Second, the labor costs response is mostly due to an increase in employment (rather than an increase in earnings).

The firm-level regression results, obtained from variants of equation (8), are presented in Table 6. Column (1) displays the baseline effects with firm and year fixed effects. In column (2), I add region-by-year fixed effects to account for any geographical shocks or policies that differently affected the two groups of firms. Column (3) addresses the concern that any sector-specific shocks (defined at the two-digit level) might have affected male intensive firms less. Finally, column (4) include interactions between years and firm size (defined as those with employment below and above the median value).

The table provides the following results. First, firms presenting a lower pre-reform share of women in their workforce hired much more female workers compared to similar firms with a relatively larger pre-reform share of women. On average, there is

[34] These standard errors are slightly larger than those obtained by clustering at the firm level, reflecting a shared industry-level component of residual variance.

Figure 10: Firm-level analysis



Note: This figure shows the effect of the 2012 reform on firm-level outcomes. It relies on between-firm variation in pre-reform gender composition of the workforce. Red circles (blue squares) refer to firms more (relatively less) exposed to the payroll tax cut. The figure also reports coefficient estimate and standard errors clustered at industry-level from a DiD model with firm and year fixed effects.

an annual 10 percent growth in female workers in low share female firms compared to fairly low share female firms. Consistent with the cross-municipality analysis studying employment effects, I find that firms did not substitute female workers male employment. Therefore, low share female firms grew in size by exploiting the lower labor costs of new female hires. Third, the addition of female workers did significantly raise per-worker net worth (by 11.7 percent), value added (5.9 percent) and capital (6.8 percent).

One potential explanations for the positive effect of payroll tax cut on *both* labor and capital is liquidity effects: firms did not have enough cash to fund their operations before the introduction of the payroll tax cut. Liquidity effects are often ignored from the standard constant elasticity of substitution (CES) framework. If the liquidity effects are larger than the standard substitution effects predicted by the CES model, the payroll tax-induced reduction in the price of labor will lead to an increase in both capital and labor even if capital and labor are substitutes.[35]

[35] Studies in the corporate finance literature (see, e.g., Fazzari et al. 1988) have shown that cash windfalls significantly affect firms' performance.

To study the role of liquidity constraints, I divide firms by using two proxies (measured over the pre-reform period) that have been used in the corporate finance literature (see, e.g., Farre-Mensa and Ljungqvist 2016): i. the share of liquid assets; ii. sales (small firms are much more likely to be financially constrained). Then, I implement a triple difference approach by augmenting equation (8) with the interaction between the post-reform treatment group dummies and a dummy equal to 1 for liquidity constrained firms, that are those presenting values below the median. Table B8 shows that employment and capital growth effects on firms are mostly concentrated among firms that were liquidity constrained before the implementation of the payroll tax cut.

Table 6: Firm-level evidence

	(1)	(2)	(3)	(4)
	A. *Outcome*: log of female workers			
$1(i \in Low) \cdot 1(t \in Post)$	0.100***	0.101***	0.120***	0.122***
	(0.009)	(0.008)	(0.007)	(0.007)
	B. *Outcome*: log of male workers			
$1(i \in Low) \cdot 1(t \in Post)$	-0.009	-0.013	-0.016	-0.014
	(0.015)	(0.014)	(0.007)	(0.006)
	C. *Outcome*: log of labor costs per-worker			
$1(i \in Low) \cdot 1(t \in Post)$	0.061***	0.051***	0.041***	0.039***
	(0.011)	(0.011)	(0.010)	(0.009)
	D. *Outcome*: log of net worth per-worker			
$1(i \in Low) \cdot 1(t \in Post)$	0.117***	0.112***	0.079***	0.077***
	(0.029)	(0.022)	(0.012)	(0.012)
	E. *Outcome*: log of value-added per-worker			
$1(i \in Low) \cdot 1(t \in Post)$	0.059***	0.051***	0.046***	0.044***
	(0.015)	(0.014)	(0.013)	(0.013)
	F. *Outcome*: log of capital (total assets)			
$1(i \in Low) \cdot 1(t \in Post)$	0.068***	0.058***	0.037**	0.035**
	(0.022)	(0.020)	(0.016)	(0.016)
Observations	392,726	392,726	392,726	392,726
# of firms	67,402	67,402	67,402	67,402
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Region-year FE	No	Yes	Yes	Yes
Industry-year FE	No	No	Yes	Yes
Size-year FE	No	No	No	Yes

Note: This table reports coefficient estimates and standard errors clustered at the 2-digit industry-level (98 clusters) obtained by regressing equation (8). The sample is composed of firms in the bottom quintile of the pre-reform distribution in the share of female workers (treatment group) and the next quintile of the same distribution (control group). Each specification includes firm fixed effects and year fixed effects.

6 Conclusion

Gender equality is a key policy for policy makers and economists alike. In its most recent commitment to implement the *2030 Sustainable Development Goals*, the European Commission identified gender equality as one of the most urgent issue for future sustainability-oriented policies. This paper offers a comprehensive analysis on whether ad hoc tax policies are a successful strategy to curb the gender gap observed in several labor markets.

The results presented in this paper suggest that employer-specific payroll tax cuts are likely to be sticky. Under such nonstandard tax incidence result, reducing employer payroll taxes increase employment while not affecting net wages. This is a desiderable outcome for curbing the gender *employment* gap, but it is an undesirable outcome if policy-makers want to reduce the gender *wage* gap. If the policy goal is to improve take-home earnings of female workers, then the *employee* tax cut option is likely to be more effective than the *employer* tax cut option.

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Appendices

A 2012 Reform

The legislator defines a profession by relying on standard International Standard Classification of Occupations (ISCO). Specifically, professions are grouped in 37 2-digit ISCO group, which is the so called "sub-major" group classification. Every year, profession-specific statistics on the gender employment gap are published by the Ministry of Labor, along with the overall national gender employment gap and the cutoff defining eligibility for the preferential payroll tax scheme. These statistics refer to values reported two years before and are based on Italian labor force survey.

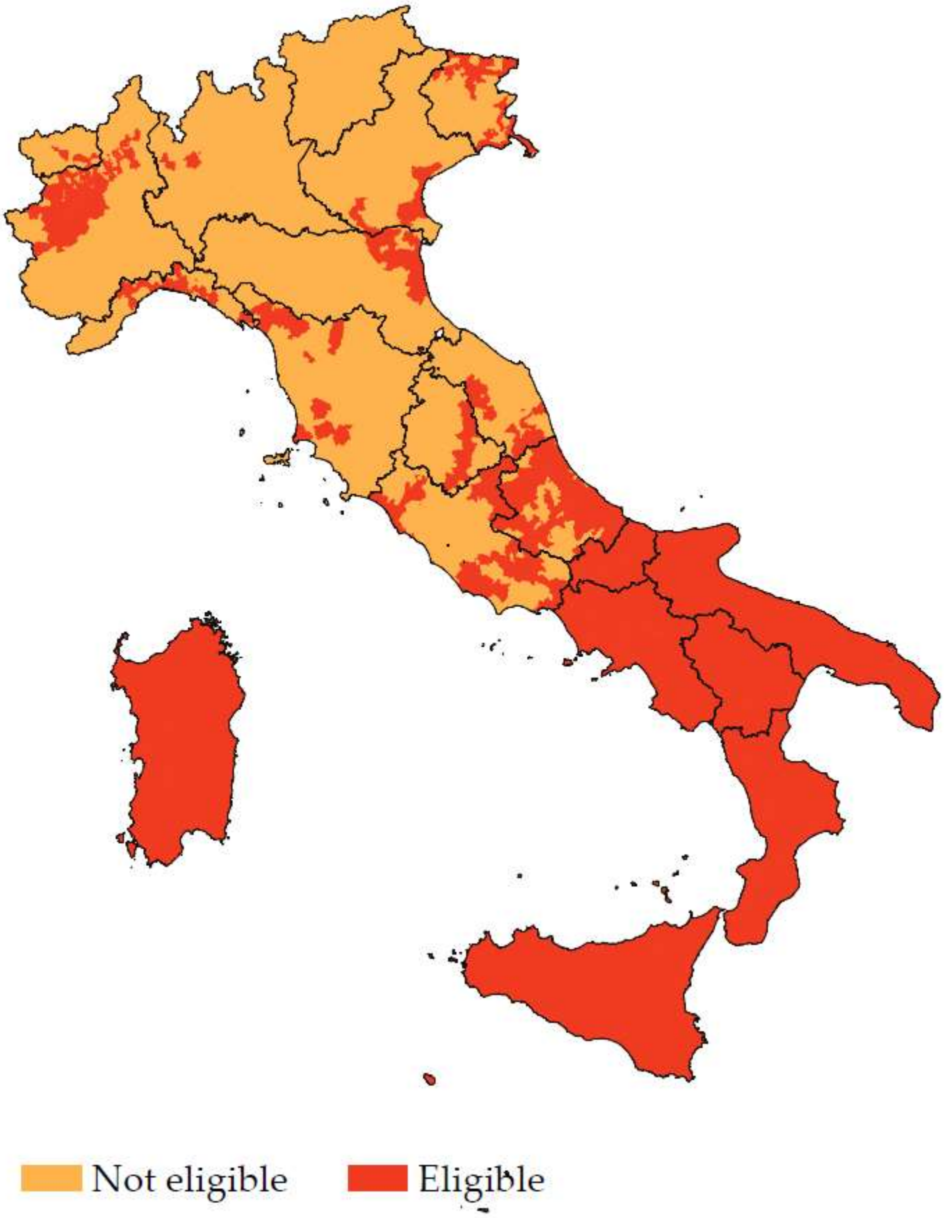
A major weakness of the data published in official public documents is that they report exclusively information relative to eligible professions, that is those where the gender employment gap is larger than 25 percent of the national average. To account for this issue, I use to the same source of data to compute the same statistics for not eligible professions.

Table A1: Gender employment gap by profession

CP2011	Profession	Gender employment gap 2011	2012	2013	2014	2015	2016	2017	2018
11	CEO, senior officials and legislators	0.194	0.114	0.202	0.245	0.227	0.136	0.129	0.068
12	Administrative and commercial managers	0.648	0.671	0.652	0.678	0.632	0.620	0.620	0.632
13	Production and specialized services managers	0.481	0.495	0.485	0.483	0.467	0.446	0.452	0.463
21	Science and engineering professionals	0.560	0.585	0.606	0.584	0.578	0.572	0.543	0.549
22	Health professionals	0.574	0.555	0.593	0.588	0.559	0.553	0.516	0.519
23	Teaching professionals	-0.172	-0.241	-0.166	-0.183	-0.212	-0.226	-0.260	-0.250
24	Business and administration associate professionals	0.271	0.224	0.213	0.262	0.260	0.198	0.163	0.192
25	ICT professionals	0.109	0.092	0.116	0.096	0.081	0.101	0.100	0.083
26	Legal, social, cultural and related social professionals	-0.596	-0.583	-0.583	-0.584	-0.604	-0.607	-0.594	-0.606
31	Science and engineering associate professionals	0.727	0.710	0.706	0.719	0.715	0.699	0.699	0.706
32	Health associate professionals	-0.386	-0.387	-0.378	-0.376	-0.376	-0.398	-0.413	-0.390
33	Business and administration associate professionals	0.128	0.133	0.122	0.096	0.128	0.149	0.139	0.120
34	Legal, social and cultural associate professionals	-0.019	0.014	0.024	0.032	0.054	0.023	0.024	0.073
41	General and keyboard clerks	-0.438	-0.483	-0.493	-0.487	-0.475	-0.479	-0.468	-0.458
42	Customer services clerks	-0.258	-0.289	-0.323	-0.315	-0.311	-0.307	-0.294	-0.322
43	Numerical and material recording clerks	-0.045	-0.007	-0.073	-0.098	-0.104	-0.098	-0.072	-0.065
44	Other clerical support workers	-0.108	-0.109	-0.128	-0.131	-0.090	-0.019	-0.031	-0.085
51	Personal service workers	-0.122	-0.142	-0.131	-0.128	-0.143	-0.136	-0.127	-0.125
52	Sales workers	-0.103	-0.116	-0.128	-0.128	-0.114	-0.127	-0.121	-0.115
53	Personal care workers	-0.712	-0.692	-0.681	-0.711	-0.717	-0.673	-0.696	-0.698
54	Protective services workers	-0.200	-0.237	-0.256	-0.263	-0.277	-0.300	-0.284	-0.287
61	Artisans and skilled workers in the mining, industry, and construction	0.898	0.943	0.959	0.968	0.973	0.967	0.971	0.963
62	Skilled artisans, metalworkers, and installers and maintainers of electrical equipment	0.937	0.945	0.956	0.955	0.954	0.958	0.953	0.949
63	Artisans and workers specialized in precision mechanics, craftsmanship and printing	0.416	0.390	0.446	0.422	0.424	0.408	0.397	0.453
64	Market-oriented skilled forestry, fishery and hunting workers	0.542	0.526	0.528	0.546	0.534	0.536	0.539	0.528
65	Artisans and skilled workers in food processing, textiles, clothing, and the entertainment industry	0.255	0.281	0.299	0.294	0.266	0.278	0.254	0.229
71	Building and related trades workers (excluding electricians)	0.694	0.684	0.723	0.722	0.745	0.715	0.719	0.729
72	Metal, machinery and related trades workers	0.276	0.294	0.302	0.323	0.328	0.355	0.345	0.356
73	Fixed machinery operators in agriculture and the food industry	0.376	0.338	0.293	0.299	0.260	0.264	0.293	0.286
74	Drivers of vehicles, mobile and lifting machinery	0.955	0.962	0.956	0.956	0.965	0.958	0.956	0.963
81	Stationary plant and machine operators	0.057	0.070	0.067	0.075	0.095	0.126	0.133	0.125
82	Assemblers	-0.807	-0.809	-0.803	-0.795	-0.797	-0.800	-0.774	-0.783
83	Drivers and mobile plant operators	0.280	0.314	0.378	0.434	0.450	0.442	0.468	0.465
84	Laborers in mining, construction, manufacturing and transport	0.674	0.658	0.681	0.666	0.714	0.710	0.700	0.671
91	Commissioned armed forces officers	0.949	0.918	0.944	0.992	0.944	0.939	0.917	0.881
92	Non-commissioned armed forces officers	0.976	0.968	0.976	0.982	0.997	0.990	0.962	0.971
93	Armed forces occupations, other ranks	0.955	0.940	0.922	0.933	0.935	0.920	0.912	0.900
	(Unweighted) Gender employment gap	0.113	0.102	0.095	0.093	0.098	0.099	0.092	0.093
	Cutoff (1.25*gender employment gap)	0.141	0.127	0.119	0.116	0.123	0.123	0.115	0.116

Note: This table reports the gender employment gap in each profession (identified by the CP2011, i.e., the ISCO-08 sub-major group) over the 2011-2018 period. In the last two rows, the table shows the average gender employment gap and the cutoff defining eligibility for the preferential payroll tax scheme. Professions where the gender employment gap is larger than the cutoff value defined two years before are eligible for the preferential payroll tax scheme. These series are based on data from the Italian labor force survey and published annually by the Ministry of Labor.

Figure A1: Eligibility for EU structural fund



Note: This graph depicts in red the areas (municipalities) receiving structural funds from the European Union. Black lines refer to regional boundaries.

B Additional tables and figures

Table B1: Summary statistics, employees

	# of women (1)	Mean (2)	SD (3)	Min (4)	Max (5)
Monthly gross wage (euros)	135,926	1,192.93	659.88	0	11,352.24
Monthly net wage (euros)	135,926	963.51	532.976	0	9,169
Eligible municipality (0/1)	135,926	0.594	0.491	0	1
Foreign born (0/1)	135,926	0.108	0.311	0	1
Commuter (0/1)	135,926	0.437	0.493	0	1
Age	135,926	38.313	11.307	18	65
Age 18-29 (%)	135,926	0.273	0.446	0	1
Age 30-39 (%)	135,926	0.280	0.449	0	1
Age 40-49 (%)	135,926	0.242	0.428	0	1
Age 50-65 (%)	135,926	0.205	0.403	0	1
Blue collar (0/1)	135,926	0.623	0.483	0	1
White collar (0/1)	135,926	0.375	0.482	0	1
Manager (0/1)	135,926	0.000	0.005	0	1
Other workers (0/1)	135,926	0.002	0.042	0	1
Permanent jobs (0/1)	135,926	0.462	0.447	0	1
Temporary jobs (0/1)	135,926	0.226	0.360	0	1
Seasonal jobs (0/1)	135,926	0.312	0.447	0	1
Full-time jobs (0/1)	135,926	0.253	0.397	0	1
Part-time jobs (0/1)	135,926	0.639	0.465	0	1
Other jobs (0/1)	135,926	0.108	0.266	0	1

Note: This table presents summary statistics of payroll tax cut's recipients.

Table B2: Summary statistics, employers

	# of employers (1)	Mean (2)	SD (3)	Min (4)	Max (5)
	A. General information				
Firm age	67,592	8.659	10.110	0	89
Employees (#)	67,592	20.495	281.907	1	30,874
Annual labor costs (euros per-worker)	67,592	9,680.60	6,973.49	0	160,254.33
Permanent jobs (% of workers)	67,592	0.647	0.329	0	1
Temporary jobs (% of workers)	67,592	0.383	0.331	0	1
Full-time jobs (% of workers)	67,592	0.407	0.374	0	1
Part-time jobs (% of workers)	67,592	0.631	0.366	0	1
Subsidiary firm (%)	67,592	0.045	0.148	0	1
Parent company (%)	67,592	0.038	0.129	0	1
Single member company (%)	67,592	0.917	0.265	0	1
Eligible municipality (0/1)	67,592	0.601	0.485	0	1
	B. Economic activity (NACE 2008)				
A. Agriculture, forestry and fishing	67,592	0.003	0.054	0	1
B. Mining and quarrying	67,592	0.000	0.020	0	1
C. Manufacturing	67,592	0.155	0.352	0	1
D. Electricity, gas, steam and air conditioning supply	67,592	0.000	0.020	0	1
E. Water supply; sewerage, waste management and remediation activities	67,592	0.003	0.050	0	1
F. Construction	67,592	0.042	0.190	0	1
G. Wholesale and retail trade; repair of motor vehicles and motorcycles	67,592	0.247	0.420	0	1
H. Transportation and storage	67,592	0.020	0.135	0	1
I. Accommodation and food service activities	67,592	0.189	0.381	0	1
J. Information and communication	67,592	0.025	0.153	0	1
K. Financial and insurance activities	67,592	0.013	0.110	0	1
L. Real estate activities	67,592	0.011	0.104	0	1
M. Professional, scientific and technical activities	67,592	0.063	0.240	0	1
N. Administrative and support service activities	67,592	0.067	0.241	0	1
O. Public administration and defence; Compulsory social security	67,592	0.000	0.009	0	1
P. Education	67,592	0.018	0.131	0	1
Q. Human health and social work activities	67,592	0.058	0.230	0	1
R. Arts, entertainment and recreation	67,592	0.012	0.103	0	1
S. Other service activities	67,592	0.062	0.236	0	1
T. Activities of household as employers; undifferentiated goods and services-producing activities of household for own use	67,592	0.001	0.029	0	1
U. Activities of extraterritorial organizations and bodies	67,592	0.000	0.005	0	1

Note: This table presents summary statistics of employers that hired at least one worker through the preferential payroll tax scheme.

Table B3: Payroll tax incidence, results by year of job signing

	Year of job signing:				
	2013	2014	2015	2016	2017
	(1)	(2)	(3)	(4)	(5)
	A. *Outcome*: Monthly gross wage				
$1(t \leq 18)$	-0.125***	-0.110***	-0.109***	-0.065***	-0.135***
$\times\ 1(j \in Eligible)$	(0.021)	(0.017)	(0.029)	(0.022)	(0.020)
	B. *Outcome*: Monthly net wage				
$1(t \leq 18)$	-0.032	-0.019	-0.019	0.026	-0.044
$\times\ 1(j \in Eligible)$	(0.021)	(0.017)	(0.029)	(0.022)	(0.030)
Observations	88,374	163,995	100,656	158,769	303,143
Ind. $\times$ month FE	Yes	Yes	Yes	Yes	Yes
Job FE	Yes	Yes	Yes	Yes	Yes
Pass-through to firms	1.344	1.209	1.211	0.714	1.484

Note: This table presents the results on the incidence of the payroll tax separately by the year of job signing. The coefficient estimate rests on within-individual cross-job variation in wages, before and after the period when the payroll tax cut applied. Each specification includes individual-month of the job fixed effects and job fixed effects. The coefficient estimate thus presents the percent change in wages during the period with a preferential payroll tax scheme. Pass-through to firms is defined as the fraction of payroll tax that benefit the firm. Standard errors in parenthesis clustered at individual level.

Table B4: Payroll tax incidence by industry

	log of gross wage (1)	log of net wage (2)
	A. Agriculture, mining and quarrying	
β	-0.097	-0.006
	(0.084)	(0.084)
Observations	1,006	1,006
Pass-through to firms	1.066	1.066
	B. Construction, electricity and water supply	
β	-0.072	0.020
	(0.044)	(0.044)
Observations	15,827	15,827
Pass-through to firms	0.783	0.783
	C. Wholesale and retail trade; transportation and storage	
β	-0.119***	-0.027
	(0.044)	(0.044)
Observations	177,484	177,484
Pass-through to firms	1.293	1.293
	D. Accommodation and food and service activities	
β	-0.028	0.064**
	(0.031)	(0.031)
Observations	177,484	177,484
Pass-through to firms	0.304	0.304
	E. Financial, insurance and real estate activities	
β	-0.136***	-0.043
	(0.037)	(0.037)
Observations	23,063	23,063
Pass-through to firms	1.462	1.462
	F. Professional, scientific and technical activities	
β	-0.132***	-0.040
	(0.037)	(0.037)
Observations	30,142	30,142
Pass-through to firms	1.435	1.435
Ind. × month FE	Yes	Yes
Job FE	Yes	Yes

Continue on next page

	log of gross wage (1)	log of net wage (2)
	G. Administrative and support service activities	
β	-0.037	0.055*
	(0.037)	(0.037)
Observations	158,382	158.382
Pass-through to firms	0.402	0.402
	H. Human health and social work activities	
β	-0.098**	-0.006
	(0.042)	(0.042)
Observations	28,949	28,949
Pass-through to firms	1.065	1.065
	I. Arts, entertainment and recreation activities	
β	-0.025	0.068
	(0.072)	(0.072)
Observations	6,408	6,408
Pass-through to firms	0.269	0.269
	J. Other service activities	
β	-0.098*	-0.006
	(0.056)	(0.056)
Observations	30,061	30,061
Pass-through to firms	1.065	1.065
Ind. × month FE	Yes	Yes
Job FE	Yes	Yes

Note: This table presents the results on the incidence of the payroll tax. The coefficient estimate rests on within-individual cross-job variation in wages, before and after the period when the payroll tax cut applied. Each specification includes individual-month of the job fixed effects and job fixed effects. Standard errors in parenthesis clustered at individual level.

Table B5: Summary statistics by municipality

	Pre-reform, 2005-2012				Post-reform, 2013-2018			
	Eligible		Not Eligible		Eligible		Not Eligible	
	#	Mean	#	Mean	#	Mean	#	Mean
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Monthly wage	5,276,476	12,208	3,259,495	9,263	5,076,478	14,086	3,181,970	10,331
Age	5,276,476	36.757	3,259,495	35.732	5,076,478	39.207	3,181,970	37.987
Blue collar (0/1)	5,276,476	0.462	3,259,495	0.509	5,076,478	0.454	3,181,970	0.513
White collar (0/1)	5,276,476	0.442	3,259,495	0.414	5,076,478	0.459	3,181,970	0.422
Manager (0/1)	5,276,476	0.002	3,259,495	0.001	5,076,478	0.002	3,181,970	0.001
Other (0/1)	5,276,476	0.029	3,259,495	0.018	5,076,478	0.033	3,181,970	0.020

Note: This table presents summary statistics on labor market characteristics of female workers living in municipalities eligible for EU structural fund and those not eligible. The table splits observations by pre-reform (2005-2012) and post-reform period (2013-2018).

Table B6: Cross-profession analysis

	(1)	(2)	(3)	(4)
$1(Gap_{p,t-2} \geq C_{t-2})$	0.023**	0.023	0.023	0.022*
	(0.011)	(0.015)	(0.018)	(0.012)
Observations	89	89	89	89
Polynomial order	1	2	3	-
Profession FE	No	No	No	Yes
Year FE	No	No	No	Yes
Model	FD	FD	FD	FE

Note: This table presents β coefficient estimate and standard errors obtained by running variants of equation (5). The outcome variable in columns (1)-(3) is the first difference in the share of female worker in a profession; in column (4) is the share of female worker in a profession. Column (1) presents the baseline RD estimate depicted in Figure 7. In column (2) and (3), I further control for second- and third-order polynomials, respectively. Column (4) reports RD estimate including profession and year fixed effects, thus exploiting within-profession over-time variation in treatment eligibility.

Table B7: Summary statistics by firm

	Bottom quintile		Next quintile	
	# of firms	Mean	# of firms	Mean
	(1)	(2)	(3)	(4)
Female workers (#)	35,046	1.805	32,356	6.533
Male workers (#)	35,046	39.896	32,356	37.595
Total workers (#)	35,046	41.700	32,356	44.130
Labor costs (per-worker 1,000 euros)	35,046	25.753	32,356	32.427
Net worth (per-worker 1,000 euros)	35,046	30.084	32,356	51.099
Value added (per-worker 1,000 euros)	35,046	36.312	32,356	48.154
Capital (per-worker 1,000 euros)	35,046	41.918	32,356	66.079
Earnings (per-worker, 1,000 euros)	35,046	15.309	32,356	20.121
Temporary jobs (0/1)	35,046	0.237	32,356	0.171
Permanent jobs (0/1)	35,046	0.808	32,356	0.863
Full-time jobs (0/1)	35,046	0.936	32,356	0.923
Part-time jobs (0/1)	35,046	0.075	32,356	0.089
Subsidiary firm (0/1)	35,046	0.057	32,356	0.058
Parent company (0/1)	35,046	0.106	32,356	0.112
Single member company (0/1)	35,046	0.837	32,356	0.829
Eligible municipality (0/1)	35,046	0.520	32,356	0.343

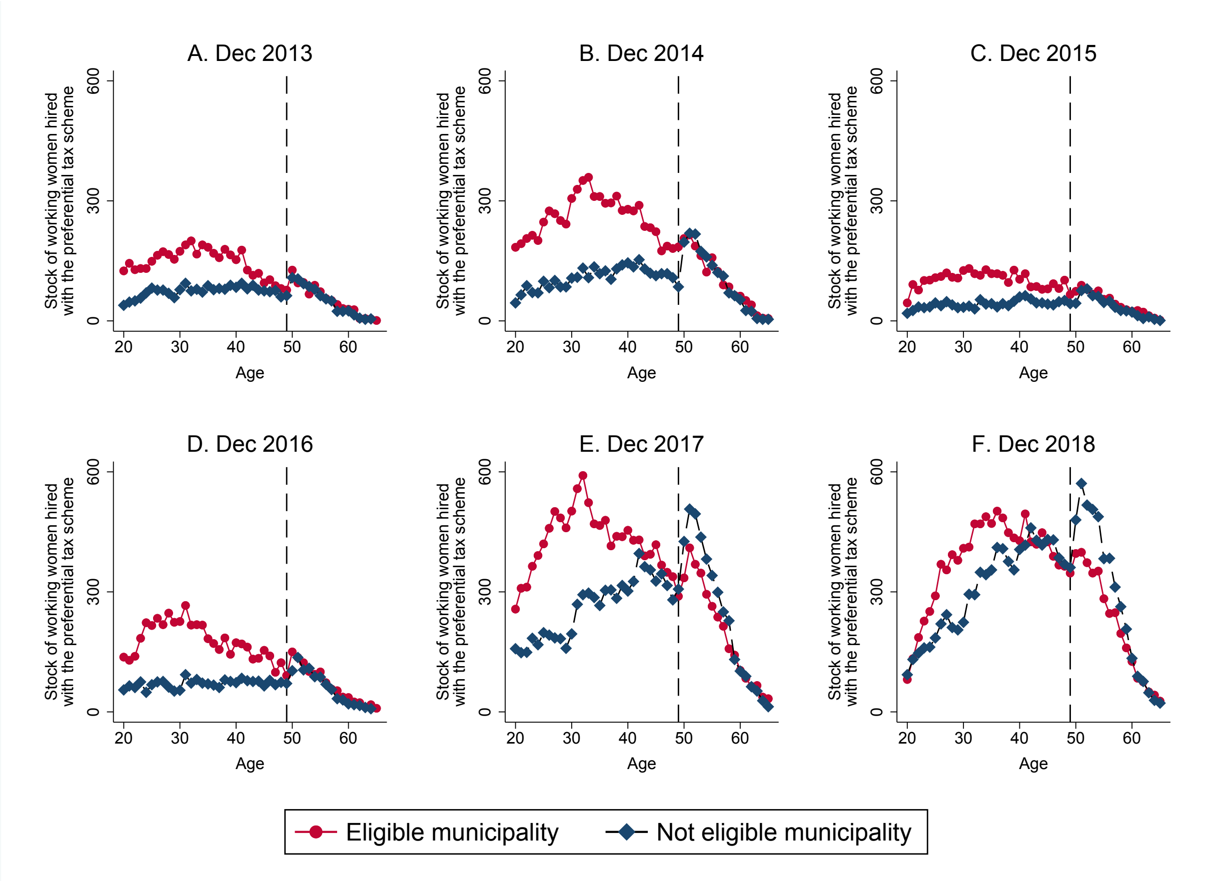
Note: Values are firm-level average (weighted by firm employment) over the pre-reform period (2005-2012). The first two columns reports the number of firms and the average value for firms having a share of female workers in the bottom quintile of the pre-reform share of female workers distribution. The next two columns report the same information for firms in the next quintile.

Table B8: The role of liquidity constraints

	Outcome variable: log of per-worker			
	labor costs	net worth	value added	capital
	(1)	(2)	(3)	(4)
	A. *Proxy for liquidity constraints*: share of liquid assets			
$1(i \in Low) \cdot 1(t \in Post)$	0.008	0.047**	-0.015	0.023
	(0.010)	(0.021)	(0.012)	(0.022)
$... \cdot 1(i \in Const)$	0.098***	0.131***	0.139***	0.085***
	(0.011)	(0.020)	(0.013)	(0.024)
	B. *Proxy for liquidity constraints*: sales			
$1(i \in Low) \cdot 1(t \in Post)$	0.003	0.057**	-0.018	0.013
	(0.010)	(0.026)	(0.012)	(0.018)
$... \cdot 1(i \in Const)$	0.116***	0.120***	0.154***	0.111***
	(0.009)	(0.017)	(0.010)	(0.017)
Observations	392,726	392,726	392,726	392,726
# of firms	67,402	67,402	67,402	67,402
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

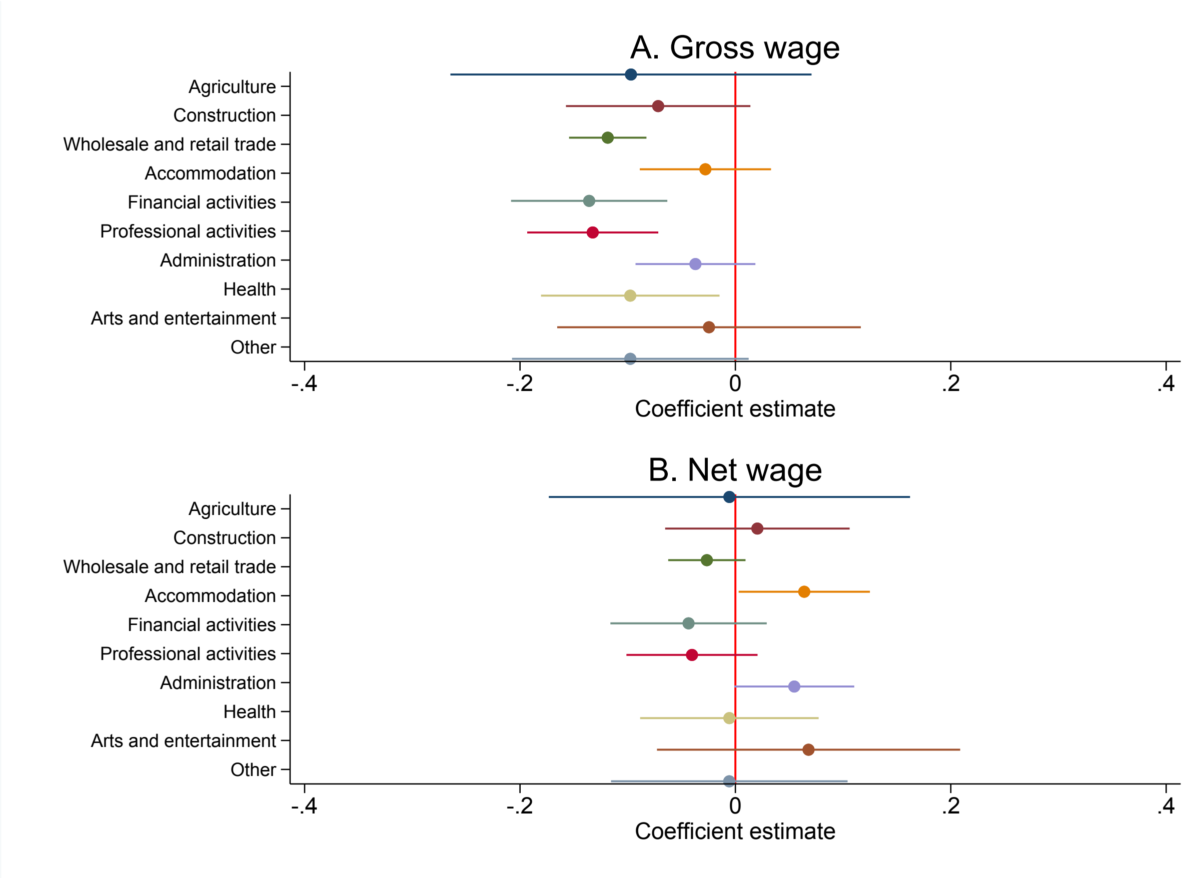
Note: This table reports coefficient estimates and standard errors clustered at the 2-digit industry-level (98 clusters) obtained by augmenting equation (8) by the interaction between the post-reform treatment group interactions and a dummy for liquidity constrained firms (defined as those presenting values below the median). I use two proxies for liquidity constrained firms: i. the share of liquid assets (panel A); ii. sales (panel B). The sample is composed of firms in the bottom quintile of the pre-reform distribution in the share of female workers (treatment group) and the next quintile of the same distribution (control group). Each specification includes firm fixed effects and year fixed effects.

Figure B1: Payroll tax cut recipients by age and municipality



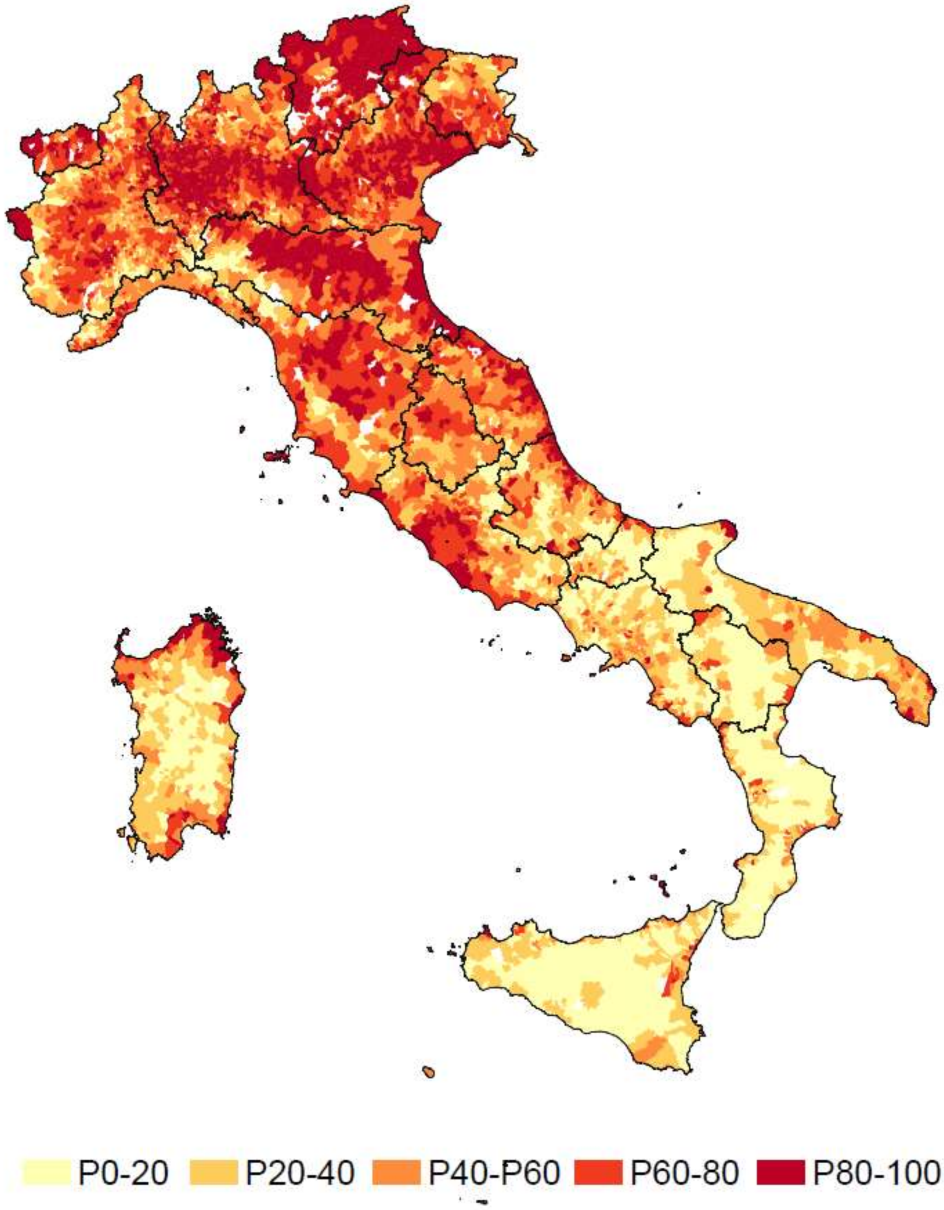
Note: The figure shows the stock of female workers hired with the preferential payroll tax scheme (vertical axis) and the age of the recipient (horizontal axis) at the end of each year over the 2013-2018 period. For each panel, the figure reports separate series relative to eligible municipalities (red circles) and not eligible municipalities (blue squares).

Figure B2: Payroll tax incidence heterogeneity by industry



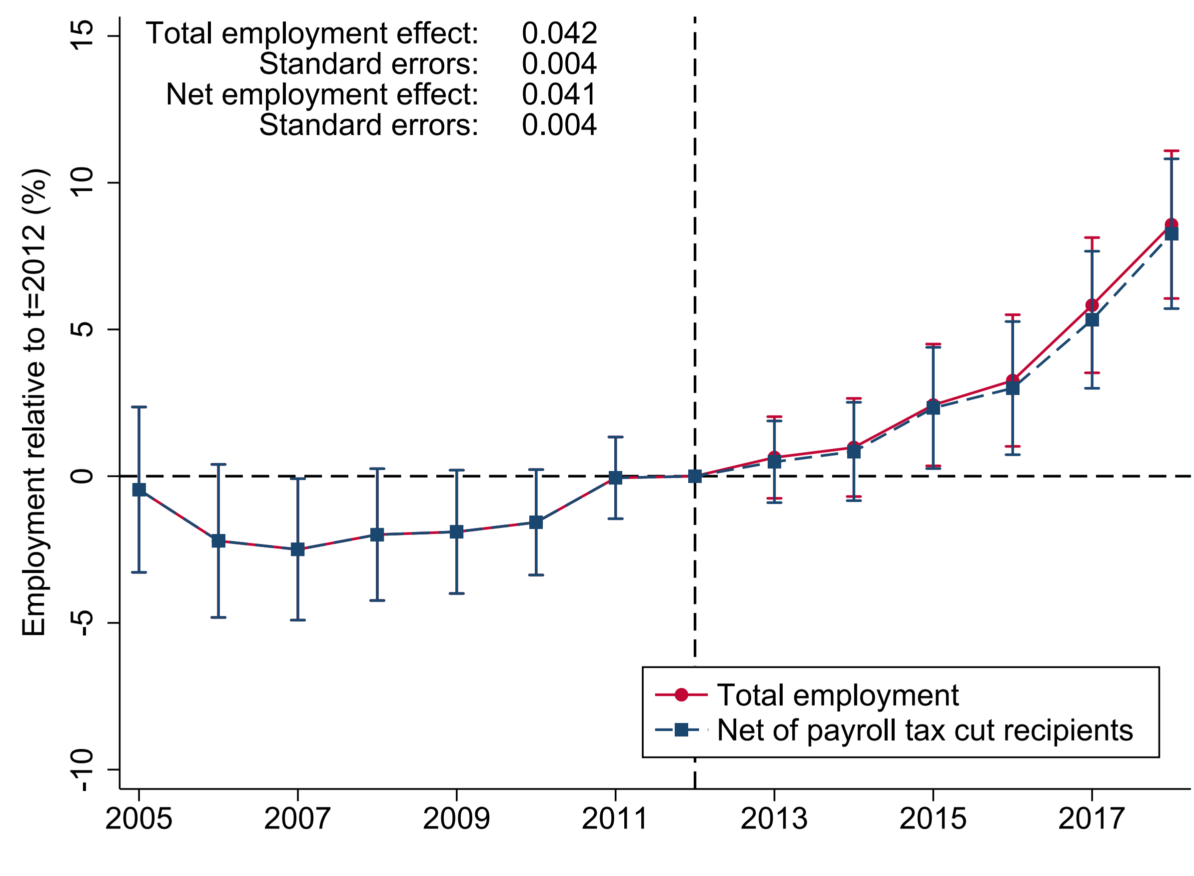
Note: The figure reports coefficient estimate and standard errors on the effect of the payroll tax cut on gross wages (top panel) and net wages (bottom panel) obtained regressing equation (1) for sub-samples of industries.

Figure B3: Share of female workers, pre-reform period



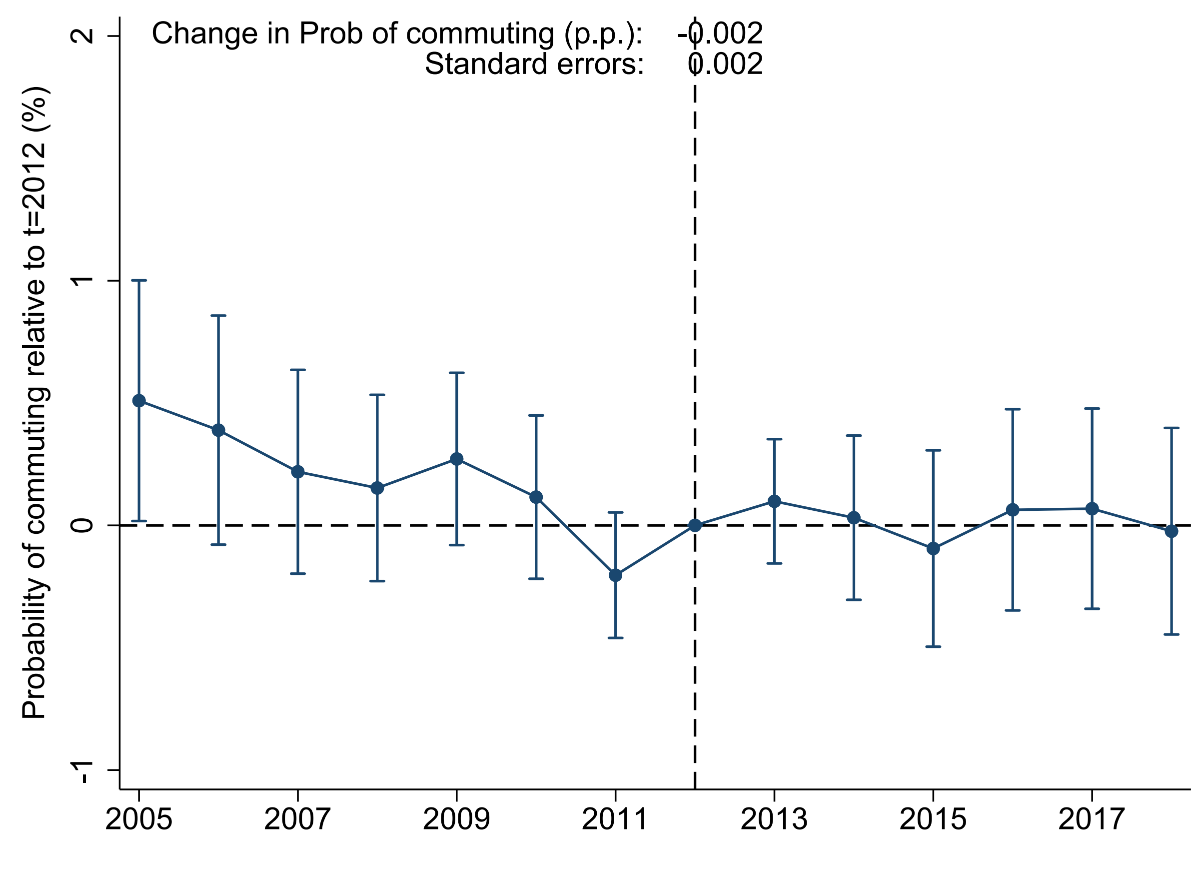
Note: This graph depicts the female employment share (in quintiles) over the period before the Fornero reform (2005-2012). The areas (municipalities) in red (yellow) are those where female employment is larger (lower). Black lines refer to regional boundaries.

Figure B4: Employment effect, cross-municipality within-province approach



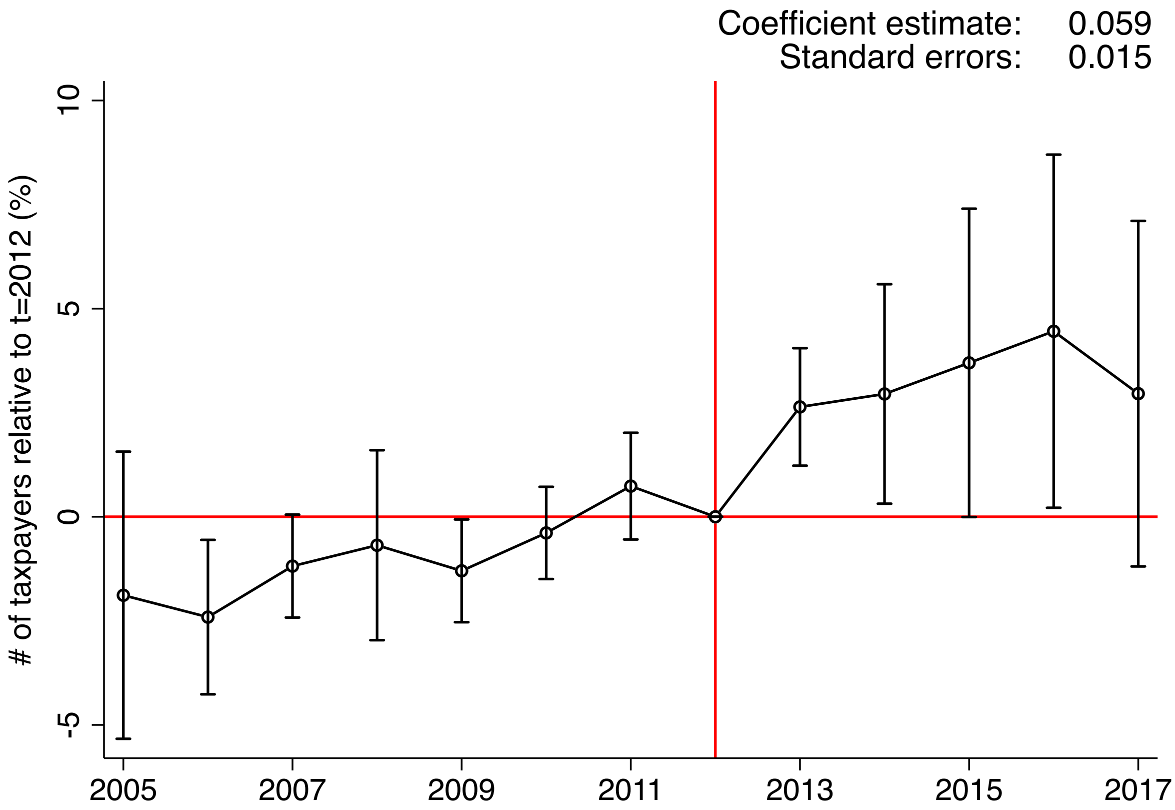
Note: The figure depicts the impact of payroll tax cut on employment exploiting cross-municipality labor market slackness. The dummy $1(m \in Slack)$ is equal to 1 in municipalities where is female unemployment share is lower than the median value. The figure plots coefficient estimates and the 95 percent confidence intervals: each point shows the effect of having implemented the payroll tax cut for j years (if $j > 2012$) or of starting the policy in j years (if $j \leq 2012$) relative to the reform inception year. Standard errors clustered at municipality-level.

Figure B5: Cross-municipality commuting



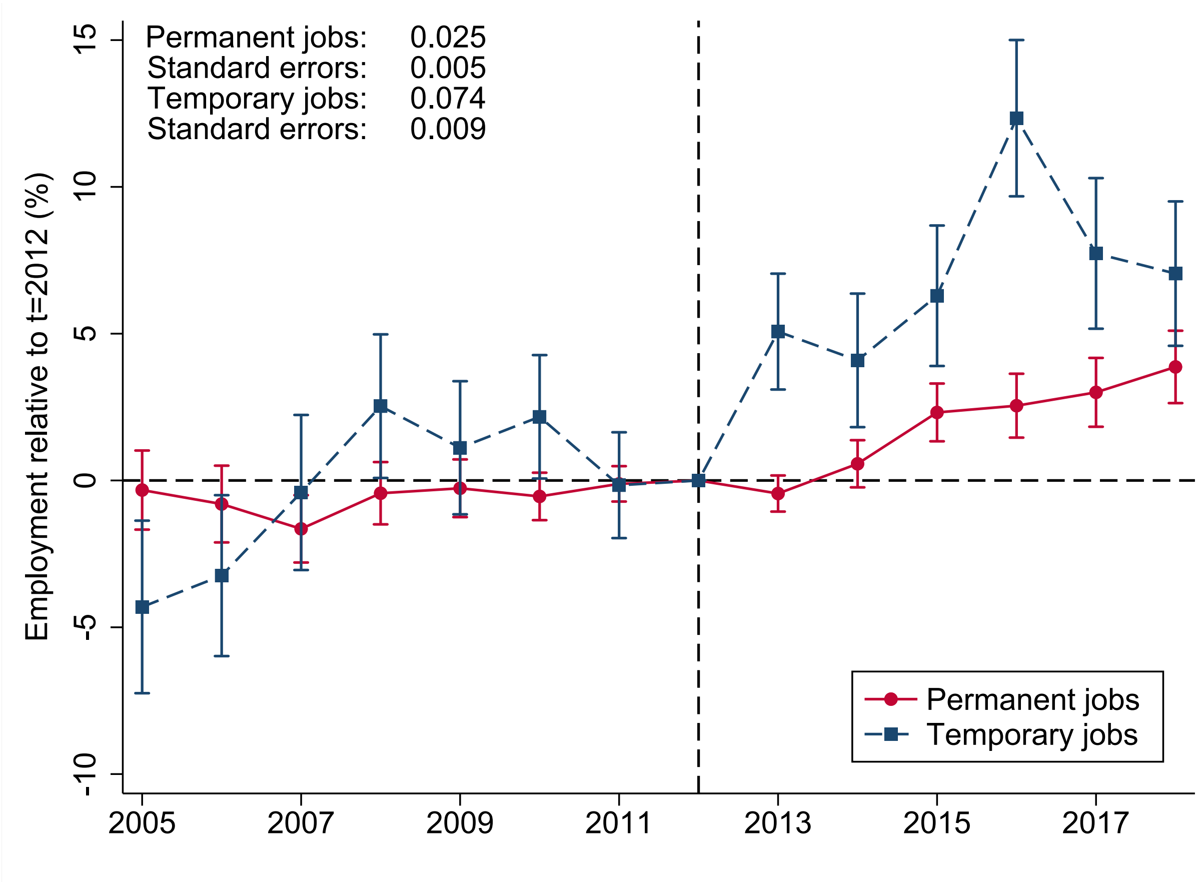
Note: The figure depicts the impact of payroll tax cut on the probability of commuting. The figure plots coefficient estimates and the 95 percent confidence intervals: each point shows the effect of having implemented the payroll tax cut for j years (if $j > 2012$) or of starting the policy in j years (if $j \leq 2012$) relative to the reform inception year. Standard errors clustered at municipality-level.

Figure B6: Employment effects using tax returns data



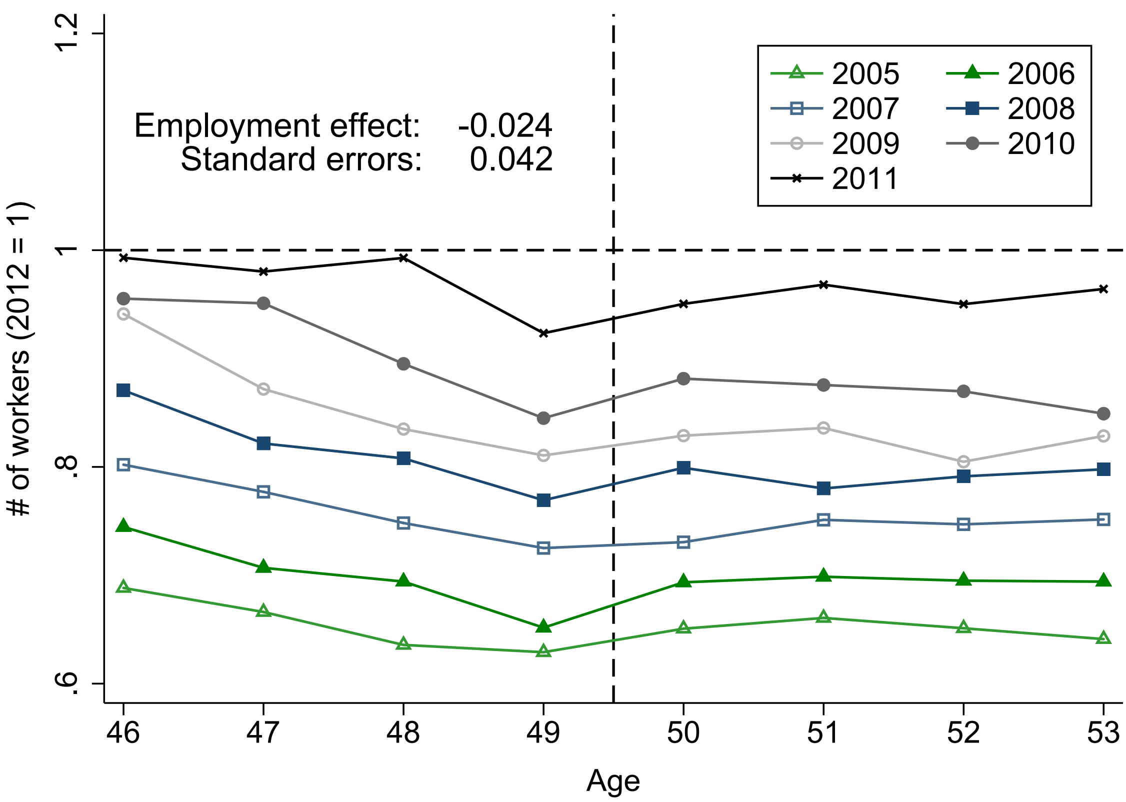
Note: The figure depicts the impact of payroll tax cut on the log of the number of taxpayers reporting positive (taxable) income below 15,000 euros. The figure plots coefficient estimates and the 95 percent confidence intervals: each point shows the effect of having implemented the payroll tax cut for j years (if $j > 2012$) or of starting the policy in j years (if $j \leq 2012$) relative to the reform inception year. Standard errors clustered at municipality-level.

Figure B7: Employment effects by work contract



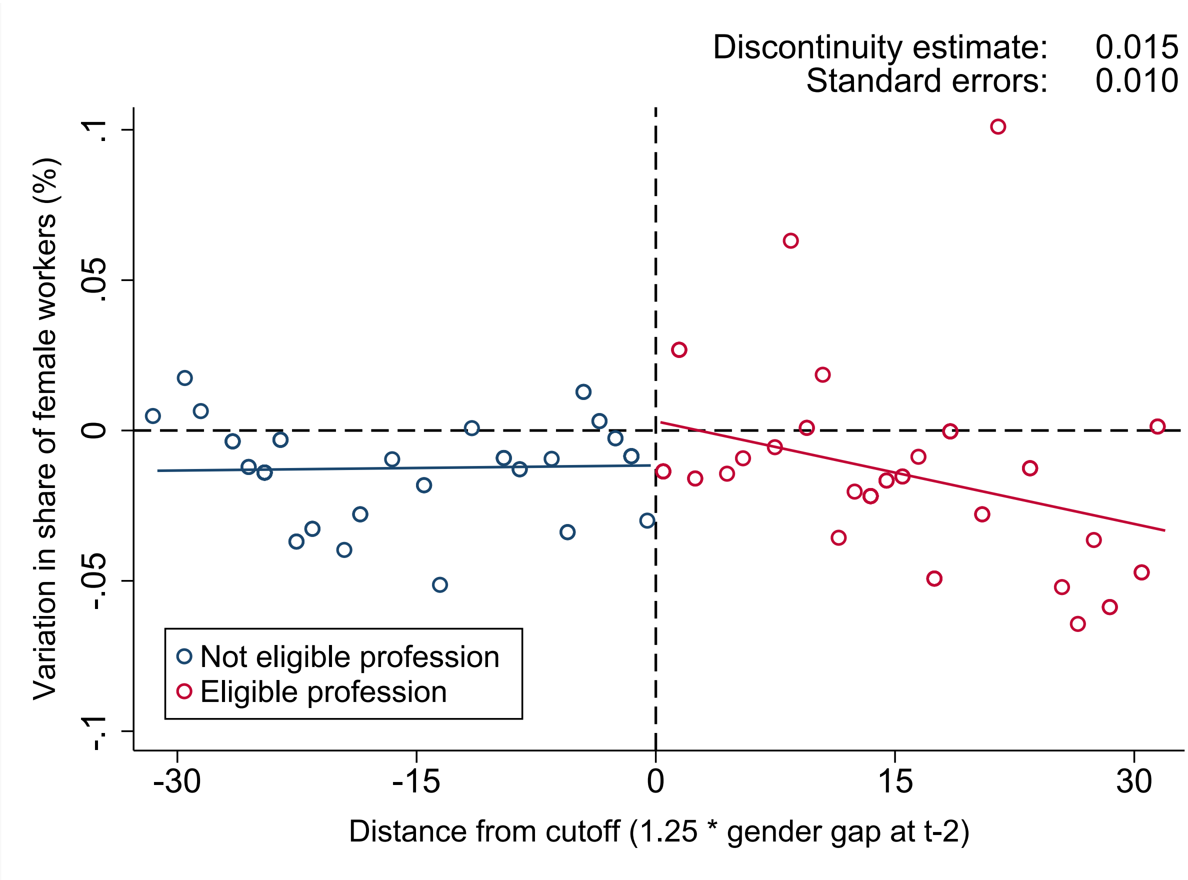
Note: The figure depicts the impact of payroll tax cut on the log of female workers hired in a permanent (red line) vs temporary job (blue line). The figure plots coefficient estimates and the 95 percent confidence intervals: each point shows the effect of having implemented the payroll tax cut for j years (if $j > 2012$) or of starting the policy in j years (if $j \leq 2012$) relative to the reform inception year. Standard errors clustered at municipality-level.

Figure B8: Cross-cohort analysis over the pre-reform period



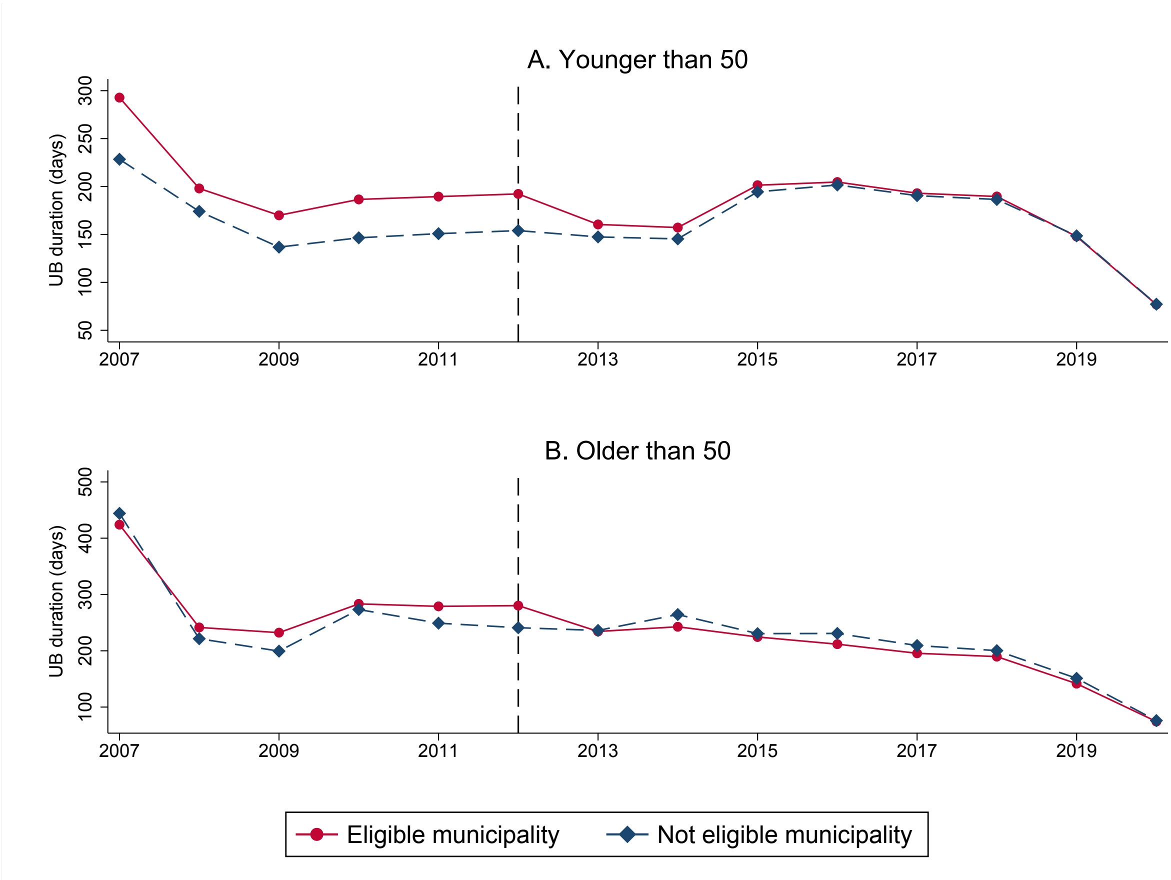
Note: The figure presents a difference-in-differences analysis by focusing on female workers with ages 46-53 in not eligible municipalities, where those younger than 50 create the control group since they were less exposed to the payroll tax cut. Employment rate is measured relative to 2012, which allows to account for any time-invariant employment difference across cohorts. The figure thus displays the deviation of employment rate by age and year relative to 2012.

Figure B9: Placebo test for cross-profession analysis



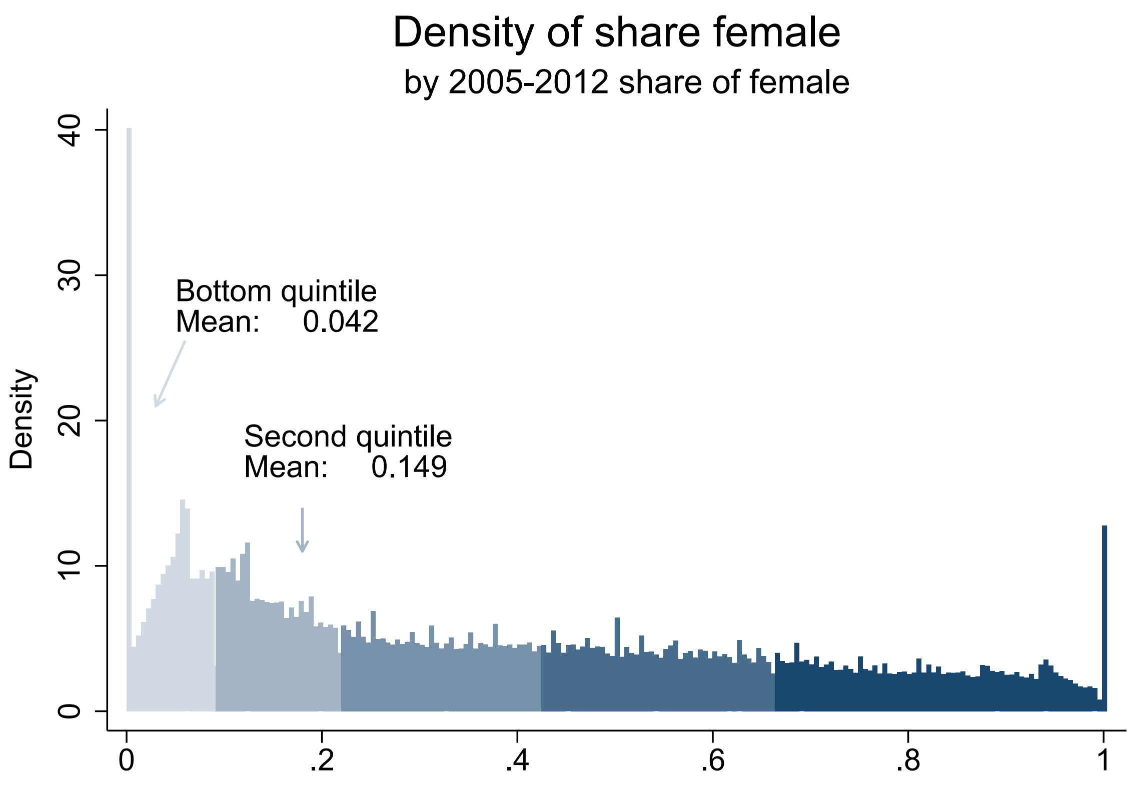
Note: The figure presents a placebo test for the cross-profession analysis. I focus on female workers located in eligible municipalities, where the minimum non-employment duration requirement does not change discontinuously across professions in these municipalities. The figure also reports the β coefficient and profession-level clustered standard errors estimated from (5). The horizontal axis is the distance from the cutoff (i.e., 1.25 * average gender employment gap defined at $t = -2$). The vertical axis is the first-difference in the share of female workers in a profession. Scatter points are sample average over intervals of 2 cutoff points bins.

Figure B10: Trend in duration of unemployment benefits



Note: The figure depicts the trend in the average UI benefits' duration (in days) across eligible (red circles) and not eligible (blue squares) municipalities. The top panel focuses on women younger than 50; the bottom panel on those older than 50.

Figure B11: Histogram of firms' share of female workers



Note: This figure depicts the histogram of gender composition of firms during the pre-reform period (2005-2012). The sample includes all firms having at least one observation over the pre-reform period.