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

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Abstract

This paper studies the role of government policies to stimulate female labor demand in gender imbalanced labor markets. I focus on a large employer-borne payroll tax cut for new female hires implemented in Italy since 2013. The preferential tax scheme provides more favorable eligibility criteria in contexts with low female participation rates. I combine social security data with several empirical approaches, leveraging the time-limited application of the tax scheme and discontinuities in eligibility criteria across municipalities, cohorts and occupations. I present four key results. First, employers pocket the tax cut: I find no effect on net-of-tax wages of directly treated workers, suggesting that tax incidence is mostly on firms. Second, I provide compelling evidence of long-lasting growth in female employment and a drop in the average non-employment duration of women entering unemployment insurance after the reform. Third, although the time-limited nature of the preferential tax scheme creates a substantial notch in the budget constraint of employers, just a few jobs terminate at the payroll tax cut expiration cutoff, indicating imperfect substitutability across workers and the presence of search and firing frictions. Fourth, firms hiring female workers experience significant growth in sales, profits, value added and capital per-worker, without raising male employees' layoffs. Effects are mostly concentrated among firms operating in industries where a large portion of the workforce has conservative gender beliefs. These findings suggest that employer-borne 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 pay gap.

Keywords: gender gap; female employment; payroll tax; tax incidence.

JEL Classification: H22; J21; J31.

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1 Introduction

Gender gaps in pay and employment are observed in every industrialized country, although to a various degree (see, e.g., Blau and Kahn 2003; Olivetti and Petrongolo 2016; Bertrand 2020). Gender equality has became a key goal for policy makers and economists alike. For instance, 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. Governments have proposed a variety of policies, including family policies such as parental leave and child care (see, e.g., Dahl et al. 2016; Kleven et al. 2019; Kleven et al. 2020), board quotas (Bertrand et al. 2019), political affirmative actions (Beaman et al. 2009), and pay transparency (Bennedsen et al. 2019).[1] Yet, the consensus on what is the best way to address gender labor market inequalities is far from being reached. If female labor force participation depends on cultural and social norms (Goldin 2006; Goldin 2014; Bordalo et al. 2019), then tackling gender inequality through government policies can be challenged by pervasive and sticky gender stereotypes.

This paper studies whether differentiating payroll tax rates by gender is a successful policy for closing the gender gap in labor market outcomes. Since the seminal work of Becker (1957), economists assume that discrimination (either statistical or taste-based) is the product of personal prejudices. Gender-prejudiced employers prefer to hire male workers even if less productive than female workers. By making gender discrimination more costly, a payroll tax cut for female hires would help overcoming prejudices and raise female labor demand by "taxing" discriminatory employers. Furthermore, even if nominally on employers, a payroll tax cut can translate into higher net wages and thus a lower gender pay gap if some of the reduction in labor costs is shared with workers (Hamermesh 1979; Fullerton and Metcalf 2002; Saez et al. 2019).

Although it has long been recognized that gender may represent a useful tagging device in optimal tax and welfare programs (Rosen 1977; Akerlof 1978; Kleven et al. 2009; Alesina et al. 2011; Gayle and Shephard 2019), I am not aware of any existing empirical research that evaluates the effects of differentiating tax rates by gender.[2] This lack of evidence is puzzling because gender-based tax rates 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).

To break new ground on this question, I study the labor market effects of a large

[1]See Profeta (2020) for a a comparative analysis of gender-targeted policies in Europe.

[2]According to optimal tax theory, a benevolent government should tax individuals who present a more elastic labor supply relatively less. Since labor supply of women is more elastic than labor supply of men (Blundell and Macurdy 1999; Keane 2011), tax rates should be lower for women than for men. Yet, as emphasized by Alesina et al. (2011), "this argument is known in the academic literature, but currently it is hardly taken seriously as a policy proposal."

employer-borne payroll tax cut for new female hires 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 12 (18) months for temporary (permanent) jobs (see law 92/2012).[3] As the total payroll tax rate in Italy is around one-third of the employee's gross compensation, this tax cut creates a large differential in labor costs between male and female hires. 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. Eligibility for the scheme depends on time elapsed in non-employment status and varies discontinuously by worker's municipality of residence and age. Eligibility criteria are meant to foster female participation rates especially in gender-imbalanced places and occupations. Namely, in a first group of municipalities, the payroll tax cut applies to women with non-employment duration of at least 6 months. In a second group of municipalities, the minimum non-employment duration requirement is 12 months for women older than 50; 24 months for those younger than 50. The minimum non-employment duration requirement is also 6 months for women hired in male-biased occupations, based on a cutoff-rule.

I access to linked employer-employee data provided by the Italian Social Security Institute. The data cover the universe of Italian workers in the non-agricultural private sector and include demographic characteristics, such as gender, date of birth and municipality of residence, along with detailed information on earnings and jobs for each month since 2005. I find a large and growing reform take-up rate: up to 31 December 2019, 218,768 women have been hired at least once through the preferential payroll tax scheme.

The received wisdom in public economics is that workers would ultimately bear payroll taxes (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 applies with the previous (not eligible) job, before and after crossing the tenure cutoff determining eligibility. I provide evidence that *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 as tenure in the firm crosses the cutoff determining payroll tax cut eligibility. This provides striking evidence that employers do not adjust wages in response to the payroll tax cut.

The tax incidence result implies that the payroll tax cut would raise labor demand

[3] Hiring credits have been gaining political traction. For instance, a number of European countries have implemented payroll tax cuts to counteract the employment effects of the Great Recession (see OECD 2010), while the U.S. has a history of employer credits targeting disadvantaged groups (Katz 1998).

for hiring (eligible) female workers, while making male hires (as well as not eligible female workers) relatively more costly. To study employment effects, I propose several empirical approaches, resting on differential exposure to the payroll tax cut across municipalities, cohorts and occupations. All these empirical approaches point to the same conclusion: the payroll tax cut led to a large and long-lasting effect on female employment. Namely, using an event study research design, I show that female employment raised by around 4.4 (12) percent in municipalities (cohorts) where the minimum non-employment duration requirement determining payroll tax cut eligibility is less binding. Employment effects build up gradually and are significant up to 8 years after the reform. Likewise, leveraging the less binding eligibility criteria favoring male-dominated occupations, I provide regression discontinuity evidence of a sudden increase in female employment in male-dominated occupations. Taken together, these findings suggest that the preferential tax scheme promotes integration of women into traditionally gender segregated places and occupations. Crucially, I do not find any offsetting decline in *male* employment.

Motivated by the possibility that these aggregate analyses might be biased by the presence of other aggregate economic shocks or measurement errors in determining payroll tax cut eligibility, I perform a micro-level analysis relating the employment status with payroll tax cut eligibility. Using this approach, I am able to exploit month-level variation in payroll tax cut eligibility across individuals within a given municipality-occupation-cohort-month cell, thus accounting for a number of time-varying policies, shocks and secular trends across places or occupations, as well as for unobserved heterogeneity across individuals. My preferred estimate suggests an increase in the probability of being employed for directly treated workers by around 1.4 percentage points (i.e., about 2.5 percent), compared to the pre-reform period. This effect maps into an elasticity between 0.107 and 0.268, depending on the empirical specification, which is likely due to labor demand effects rather labor supply responses because the net-of-tax wage of directly treated workers does not change. Ceteris paribus, the reform would explain around forty percent of the observed reduction in the gender employment gap in Italy over the last decade.[4]

Some of the employment effects comes from actively moving women out of the welfare system. By using data on the universe of unemployment insurance (UI) benefits' recipients, I show that the payroll tax cut significantly decreases the average duration of UI benefit. Women located in municipalities (cohorts) more exposed to the payroll tax cut spend around 8 (12) percent less time on welfare. This implies that the payroll tax cut increases labor market tightness and reduces the fiscal externalities of UI benefits.

The time-limited nature of the payroll tax cut creates a substantial "notch" in the

[4] According to OECD Family Database, the full-time equivalent gender employment gap in Italy reduces by 3.2 percentage points over the last decade (from 29.7 to 26.5 percent). This back-of-the-envelope calculation assumes the absence of spillover effects on men or on not eligible women.

budget constraint of employers, that is a discontinuity in the choice set of labor cost versus job duration. In frictionless labor markets, this notch should induce jobs who would otherwise lasted more to instead bunch right at the duration cutoff. Adapting existing methods for estimating behavioral responses to nonlinear incentives in similar settings (Saez 2010; Chetty et al. 2011; Kleven and Waseem 2013), I find a clear spike in the job counts with duration just at the limit. This is valid also by netting out round-number bunching by using a difference-in-bunching strategy, which account for any time-invariant reasons for locating at the notch. There is an excess mass of around 437 jobs just at the cutoff, which accounts for only 0.217 percent of the jobs in the sample. This estimate implies an elasticity of job duration with respect to the net-of-payroll tax rate by 0.019. The small elasticity reflects either the presence of labor market frictions that make turnover costly to firms or on-the-job learning and training that make (previously eligible) incumbent workers an imperfect substitute for other (payroll tax cut eligible) workers. In support of this explanation, I provide evidence that bunching responses are larger for more substitutable workers (i.e., low-skills and part-time workers) and in labor markets where the pool of payroll tax cut eligible candidates is larger.

In the last part of the paper, I study whether the tax cut-induced increase in female employment improved firm performance. Matching social security records with firm-level financial data, I leverage between-firm exposure to the payroll tax cut generated by the pre-reform workforce gender composition. The rationale for using this approach is that firms starting 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. 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, thus implying that these firms grew in size by exploiting the relatively lower labor costs of new female hires. Then, I show that the addition of female workers did significantly raise firm-level per-worker sales (by 6.4 percent), profits (5.3 percent), value added (6.9 percent) and capital (6.9 percent). Scaling up these treatment effects by the average firm-level increase in female employment, these effects suggest an elasticity of per-worker sales, profits, value added and capital with respect to female employment of 0.640, 0.530, 0.694, and 0.686, respectively.

Eliciting gender stereotypes from survey data, I show that these effects are mostly concentrated among firms operating in industries where a larger portion of the workforce has conservative gender beliefs.[5] This provides the first empirical evidence that

[5] I proxy gender stereotypes by the industry-level pre-reform share of workers who agree with the statement "when jobs are scarce, men should have more right to a job than women", using data from a nationwide survey conducted by the Italian Institute of Statistics in 2011.

breaking down gender stereotypes improves business performance: a well-known argument to economists since the seminal paper of Becker (1957). Furthermore, the positive effect of the tax cut on both labor and capital is significantly larger in firm that were liquidity constrained before the reform, suggesting that payroll tax cuts make firms more resilient during downturns by relaxing liquidity constraints (Saez et al. 2019; Benzarti and Harju 2021a). By contrast, I do not find any heterogeneity effect by skill level of female hires, suggesting that these effects come from mere integration of female workers in gender-imbalanced firms, rather than reflecting an improvement in the skill composition.

1.1 Literature Contribution

1.1.1 The Role of Government Policies on Female Labor Market Outcomes

The main contribution of this paper is to shed novel light on the effects of government policies 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. 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 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; Maida and Weber 2020). My findings relate to the literature on gender by providing empirical evidence that ad hoc tax policies can remove barriers for female employment in places and occupations that are particularly gender segregated. Despite a few studies have investigated whether 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 banking sector), there is no clear evidence about gender imbalanced occupations. These findings can be particularly relevant for countries considering further integration of women into male-dominated contexts.

1.1.2 Incidence of Payroll Taxes

The paper connects with the studies estimating the incidence of payroll taxes. My 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. 2019; Saez et al. 2021), lower earners hired by small firms in France

(Cahuc et al. 2019) and workers in Finland (Benzarti et al. 2020; Benzarti and Harju 2021a; Benzarti and Harju 2021b).[6] It is also in line with models showing that the unemployed have limited power to influence wage setting (see, e.g., Cahuc et al. 2006; Hall and Milgrom 2008). Furthermore, as the payroll taxes in Italy are not (directly) linked to social benefits for workers, my results are consistent with Bozio et al. (2019), who show that pass-through depends on the tax-benefit linkage. I offer two key insights on this literature. First, the Italian setup allows to evaluate the effectiveness of payroll tax cuts when targeting exclusively female hires. Although there is an active and mature literature interested on the effects of taxation on female labor supply and demand, the focus on payroll taxes is scant.[7] Second, the richness of the data and the quasi-experimental variations generated by the Italian reform allow me to evaluate the impact of the payroll tax cut on a wide range of outcomes not available on previous studies, such as job duration and time spent on welfare.

1.1.3 Effects of Gender Discrimination on Firm-Level Outcomes

This paper also speaks to the literature on the effects of discrimination on firms. This literature rests on Becker (1957) model showing that discrimination can hurt firm profitability. A few papers have shown that firms with more female employees earn higher profits and survive for longer (Hellerstein et al. 2002; Kawaguchi 2007; Weber and Zulehner 2014).[8] I depart from the correlational evidence offered by the existing literature by leveraging quasi-experimental variation. I identify a tax-induced increase in the cost of gender discrimination, and then use firm-level variation in exposure to this shock to estimate how making discrimination more costly affects firm-level performance. The existing literature contains little evidence on how changes in the cost of discrimination affect productivity and business performance.[9] The positive link between female employment and productivity relates with Dahl et al. (2021), who provide experimental evidence that exposure of men to women in a traditionally male-dominated environment - military in Norway - does not hurt male performance.

[6] 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).

[7] See, e.g., Eissa and Liebman (1996), Meyer and Rosenbaum (2001) and Kleven (2019) for the Earnings Income Tax Credit in the U.S. Blundell and Macurdy (1999) and Keane (2011) offer surveys of the literature on labor supply responses to taxes.

[8] The link between CEO gender and firm performance is insted mixed (see, e.g., Adams and Ferreira 2009; Post and Byron 2015; Flabbi et al. 2019). The existing literature has also provided evidence on the negative link between discrimination toward black players and performance of English soccer clubs (Szymanski 2000), and on the effects of Jewish managers' dismissal in Nazi Germany on large corporation profitability (Huber et al. 2021). See Altonji and Blank (1999) and Bertrand (2011) for surveys on how discrimination affects wages and hiring of women as well as other under-privileged workers.

[9] One exception is Hsieh et al. (2019), which use a structural Roy model and argue that declining discrimination against women and blacks stimulated productivity in the U.S.

1.1.4 Labor Market Effects of Time-Limited Hiring Credits

I also contribute to the literature estimating the labor market effects of time-limited employment subsidies or credits. Card et al. (2018) reviews the literature on the effects of active labor market effects focusing on over 200 studies, including employment subsidies. They show that private sector employment subsidies tend to have larger effects for the long-term unemployed.[10] 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 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.[11] By contrast, my findings document a persistent increase in labor force participation and a reduction in the UI benefits duration. Hystheresis effects from employer subsidies have been recently studied by Saez et al. (2021) 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. My results are also directly comparable to those yield by Cahuc et al. (2019), who leverage quasi-experimental variation generated by the 2009 French hiring credit to estimate the primitives of a search and matching models.

1.1.5 Roadmap

The rest of the paper proceeds as follows. Section 2 provides information on the Italian labor market and describes the preferential tax scheme for new female hires. In section 3, I present the data. Section 4 studies the incidence of the payroll tax. The impact of the payroll tax on employment is presented in section 5. Section 6 presents the impact on job duration. Section 7 reports the effects of the payroll tax cut on various firm-level outcomes. Section 8 concludes.

2 Institutional Framework

2.1 Gender Gap in the Italian Labor Market Versus Other Countries

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

[10]See also Grogger (2003) for an analysis of time-limited policies on a range of labor market outcomes among female-headed families in the U.S.

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

performs worst than Italy among European countries. Italy is thus a typical gender-conservative environment, which makes it a suitable setting for studying whether low female labor force participation depends on structural parameters on labor force participation, such as culture and social norms, or rather reflects labor market institutions. In terms of gender pay differences, Italy looks relatively better: the gender gap in median earnings of full-time employees was around 5 percent in 2018, against an average OECD value slightly larger than 13 percent (OECD Family Database). Using social security data, Casarico and Lattanzio (2019) show that the gender pay gap declined steadily over the last two decades in Italy.[12]

Female employment share widely differs across occupations. To offer an international perspective, Appendix Figure C1 provides a comparison between Italy and the US for a range of occupations. Italian estimates rely on data that will be presented below; US series are from the U.S. Census Bureau, 2019 American Community Survey, which provides information on the share of female workers for over 300 occupations based on survey data. The figure shows that the fraction of female workers in most occupations is remarkably similar across the two countries. For instance, stereotypically female jobs such as teachers and personal care aides workers are primarily held by women in both countries, while stereotypically male jobs such as truck drivers and police officers are mostly composed of male workers.

Furthermore, there is striking heterogeneity in female employment across places. Appendix Figure C2 depicts municipality-level female employment share measured over the period before 2013. A clear North-South divide in female employment emerges from the figure. 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.

2.2 Gender Stereotypes and Discrimination in Italy

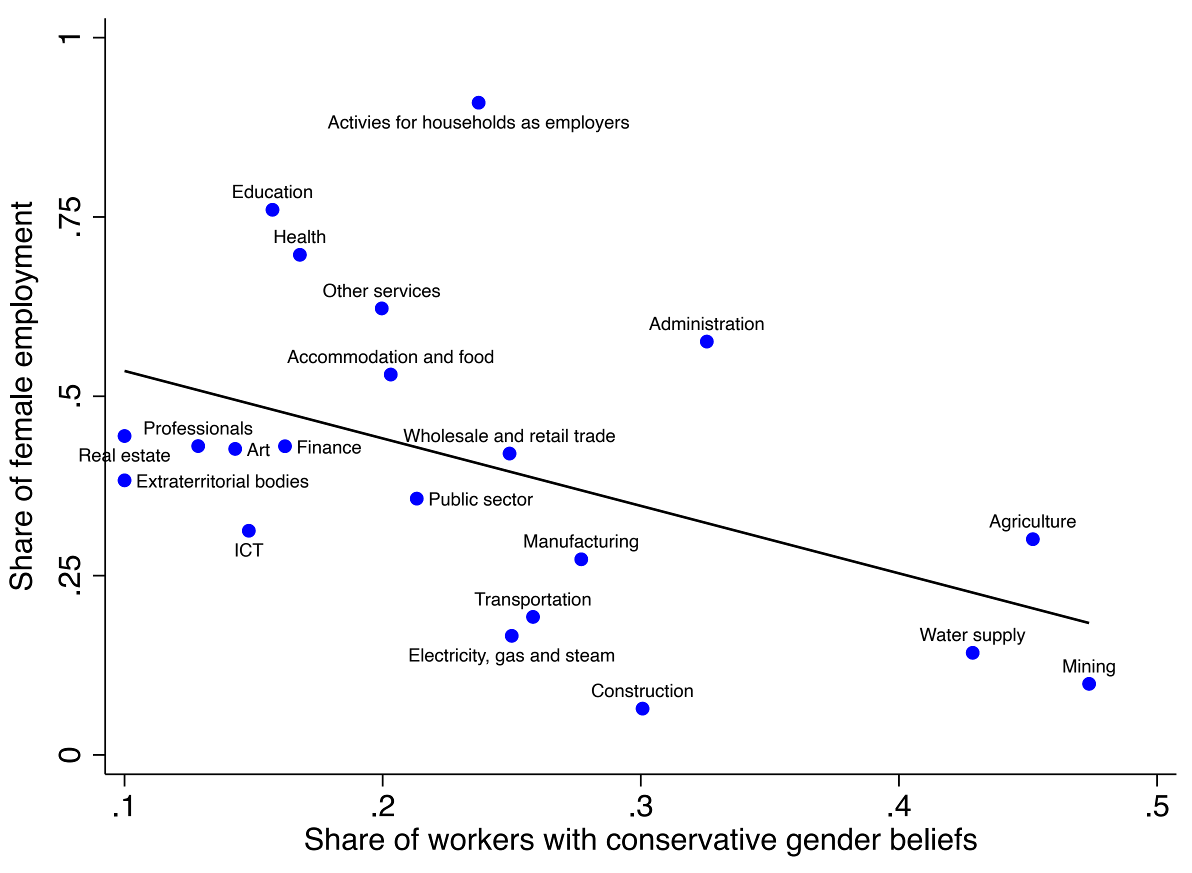
This subsection offers a picture on gender stereotypes and discrimination in Italy. Gender stereotypes are beliefs about what men and women should do. Economists interpret stereotypes as a manifestation of statistical discrimination (see, e.g., the recent review by Bertrand 2020). Under this view, stereotypes are rational beliefs based on the aggregate distribution of a skill in a group. Occupational segregation, which have been shown to account for a significant portion of the gender pay gap, is thus essentially attributed to discrimination being more pronounced in some occupations than others.[13]

[12]The authors show that the raw gender wage gap reduced from 22.5 to 15.5 log points over the 1995-2015 period. The general trend is almost unchanged when accounting for observable characteristics of workers, sector fixed effects, and firm fixed effects. They also provide evidence of a strong glass ceiling effect: the raw gap between male and female weekly earnings is approximately 47 log points at the top percentile, against a value slightly above 13 at the median.

[13]According to Goldin (2014), occupational sorting by gender accounts for at least one-fifth of the gender wage gap. A recent study by Kline et al. (2021) finds that the 2-digit industry explains roughly half of

To what extent do gender stereotypes map into job segregation by gender? Figure 1 compares the share of female workers in an industry (vertical axis), computed from the data that I will present below, with the share of workers agreeing with the statement "when jobs are scarce, men have more right to a job than women" (horizontal axis). Responses come from a nationwide survey conducted by the Italian Institute of Statistics (ISTAT) in 2011 (see *Indagine sulle discriminazioni in base al genere, all'orientamento sessuale, all'appartenenza etnica*).

Figure 1: Female Participation Rates and Gender Beliefs Across Industries



Note: The figure compares industry-level share of female workers with the share of workers who agree with the statement "when jobs are scarce, men have more of a right to a job than women." Estimates refer to 2011. Employment estimates are based on social security data. Gender stereotypes are based on a nationwide survey conducted by the Italian Institute of Statistics (ISTAT), called *Indagine sulle discriminazioni in base al genere, all'orientamento sessuale, all'appartenenza etnica*.

The figure shows a negative association, with a correlation coefficient of -0.46. This descriptive evidence makes a prima facie case that the reason for the underrepresentation of women in certain industries is related to gender stereotypes. There are two main interpretations for this result. At the one hand, it might reflect a "stereotype threat": gender stereotypes deter women to enter in male-biased industries. This can also be the result of choices made before entering in the labor market, such as when enrolling at college. At the other hand, the negative correlation can be attributed to discrimi-

the cross-firm variation in racial and gender gaps in employer response to fictitious résumé in the US.

nation being more pronounced in occupations with more conservative gender beliefs. Italy aimed at promoting female employment in male-biased occupations by granting gender segregated occupations weaker eligibility criteria for benefiting of the payroll tax cut. The reform thus allows to test whether gender stereotypes and prejudices are malleable and can be shaped through ad hoc policies.

Gender stereotypes are also more intense in poorer regions. For example, less than 20 percent of the population located in the Northern Italy points out that "when jobs are scarce, men have more of a right to a job than women," against around one-third of the Southern Italians agreeing with this statement. A similar pattern emerges when considering questions that emphasize the notion of a natural difference by sex. Around 42 percent of the South of Italy strongly or mildly agrees that "it is not natural that a male worker has a female supervisor", while the share almost halves in the North of Italy. In line with such geographical heterogeneity, the payroll tax cut provides less binding eligibility criteria in places where gender attitudes - as well as the gender employment gap - are more conservative.

Gender differences in job searching behavior also arise. For instance, 32 percent of women states that "renounced to search for a job because of household tasks, such as meal preparation, house cleaning and grocery shopping, at least once during her life," against a value of almost 14 percent for men. Furthermore, around two-third of working women reports "to feel overwhelmed by household duties and are considering to resign from their jobs." To help balancing household chores by gender and mitigate gender gaps in labor market outcomes, Alesina et al. (2011) provide theoretical evidence in support of gender-based tax rates.

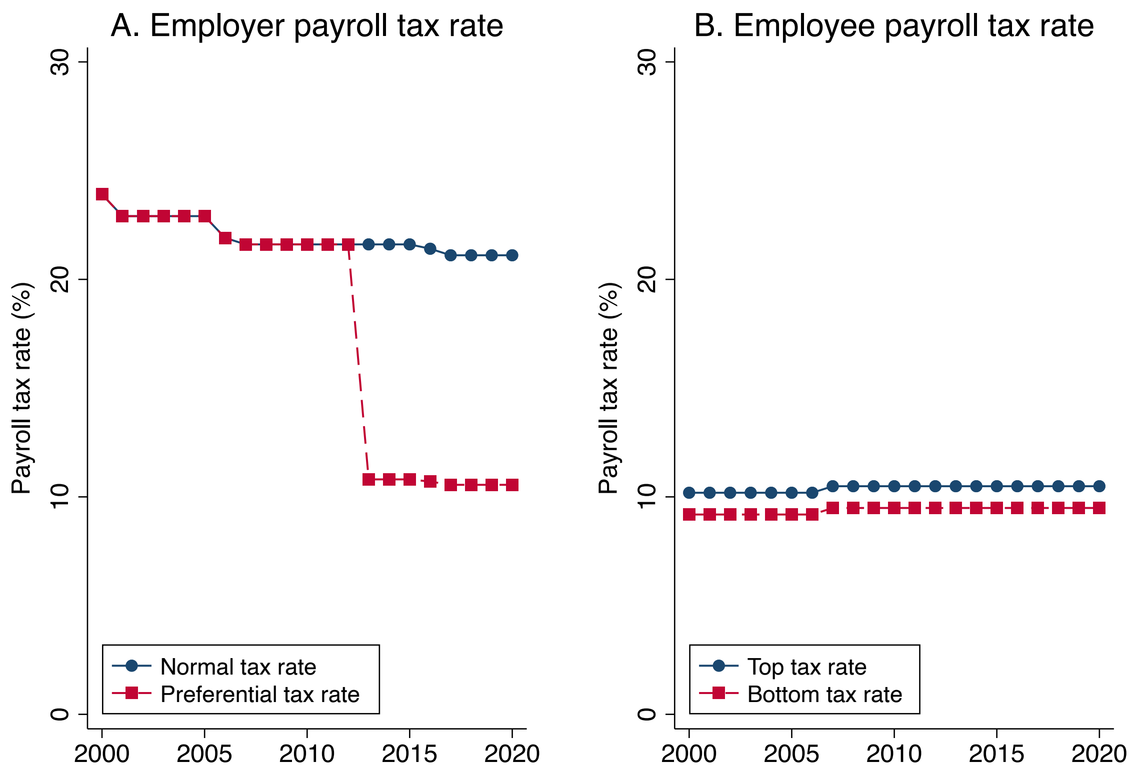
2.3 Italian Payroll Tax

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 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.[14] 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. 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.

[14] 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, such as performance or productivity bonuses, severance indemnity or family benefits.

Figure 2 depicts the evolution in the average 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, marked by the blue circles in the left-hand side graph, 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.

Figure 2: Payroll tax rates



Note: The figure displays the evolution in the average payroll tax rate since 2000. The employer (normal) tax rate, marked by the blue circles in the left-hand side graph, was 23.91 percent in 2000, 21.61 percent in mid-2000s, and 21.11 percent in 2020. The preferential payroll tax scheme series, marked by the red squares, 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. 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.

2.4 The Payroll Tax Cut for Female Hires (Law 92/2012)

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, that 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 an employer-borne payroll tax cut for new female hires. The aim of this reform was to spur female employment rate and to stimulate business activity by reducing labor costs. As shown by the red squares in left-hand side graph of Figure 2, 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. First, eligibility criteria are relatively less binding in economically disadvantaged areas (named *eligible municipalities* hereafter).[15] Disadvantaged areas are classified as municipalities that are eligible for EU structural funds.[16] Specifically, the payroll tax cut applies to new female hires 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 than 50.

Second, the eligibility criteria favor occupations with greater gender imbalance. The minimum non-employment duration requirement is set to 6 months for women hired in occupations where the gender employment gap is at least 25 percent larger than the average employment gap; 24 months for all the other occupations. The Ministry of Labor published annually the list of occupations eligible for the preferential tax scheme along with official occupation-specific statistics on the gender employment gap (see Appendix A). Eligibility for year t is based on gender employment gap estimates relative to year $t-2$. Occupations are identified by the International Standard Classi-

[15]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.

[16]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). 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 occupation requirement	
Hired in an occupation with gender employment gap $\geq$ 1.25 mean gap	6	6
Hired in an occupation 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 occupations with greater gender imbalance. The minimum non-employment duration requirement is set to 6 months for women hired in occupations where the gender employment gap is at least 25 percent larger than the average employment gap; 24 months for all the other occupations.

fication of Occupations (ISCO) sub-major group.[17] I report series on the occupation-specific gender employment gap in Appendix Table A1, along with information on the cutoff value determining eligibility for the payroll tax cut.[18]

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 (including transformations from a temporary to a permanent job by the same employer). Firms can use the payroll tax cut only if overall employment would not decrease with respect to past employment. This requirement aims at reducing layoffs and to limit the possibility that employers could substitute eligible workers with not eligible workers.

The reform was salient. In the public debate, politicians emphasized the oppor-

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

[18] Furthermore, the legislator introduced an additional requirement based on the industry where the employer operates (defined by the NACE 1-digit code). Earlier eligibility is also based on a similar cutoff-rule. However, I do not find any significant raise in take-up rate for industries benefiting of less binding eligibility criteria. There are two potential candidates for explaining this lack of response. First, the eligibility criteria in this case rests on *employer*-specific characteristics, while the other criteria depends on worker's characteristics. Second, the industry-level criteria are defined at a much broader level compared to the occupation-specific eligibility criteria, making identification less compelling.

tunity that the payroll tax cut would bring both for spurring business growth and for curbing the gender gap in labor market outcomes (see, e.g., Repubblica, February 2012; Repubblica, February 2013). Moreover, tax authorities sent out advertisements and explanatory documents (see document of 29 July 2013, n. 12212; document of 29 July 2014, n. 6319).

2.5 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 add an extra premium (a wage cushion) to workers.[19] 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.[20]

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 Matched Employer-Employee Data

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, tenure (in days), days worked and reason for hiring or terminate the job contract (including whether the worker was hired with the preferential payroll tax

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

[20] Ordine (1995) provides firm-level survey evidence on how Italian employers pay idiosyncratic wage premiums on top of the floors determination.

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 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 December 2019, that is 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 wage.[21] 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 daily (full-time equivalent) wage earnings gross of payroll taxes (in 2020 euros), corresponding to the total labor cost paid by the employer for a given worker.[22] *Net earnings* are composed of daily (full-time equivalent) wage earnings net of the employer's portion of the payroll tax rate (in 2020 euros), but inclusive of the employee's payroll tax (and also including income taxes).[23] 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.

To observe the occupation of each individual, I use a dataset (called *Comunicazioni Obbligatorie*) collecting six-digit level information on the occupation of each worker (merged with the main dataset through the scrambled worker identifier). This information is reported by the firm at the beginning of the job spell and updated in cases of any change.

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 benefit amount.

3.2 Firms' Financial Data

The second source of data is firm's balance sheets, coming from the CERVED dataset. This database collects annual information for all the companies that are legally obliged

[21] Alternative methods, such as selecting observations with the highest number of days worked, have no impact in practice.

[22] 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.

[23] Income taxes are based on annual incomes net of payroll taxes. Importantly, the income tax schedule is the same for both payroll tax cut eligible and not eligible workers, as well as over time. Therefore, the income tax does not add any further wedge between these two groups of workers (or within the same worker before and after the reform) and hence does not need to be accounted for in my analysis.

to report their financial statement to the Italian Business Register. I observe information on total wage bills, sales, profits (defined as earnings before interests, taxes, depreciation and amortization), value added, and the book value of capital (broken into several subcategories),[24] 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. 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 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 sales, profits, value added and capital per worker (defined as those in the bottom or top percentile of these key variables).

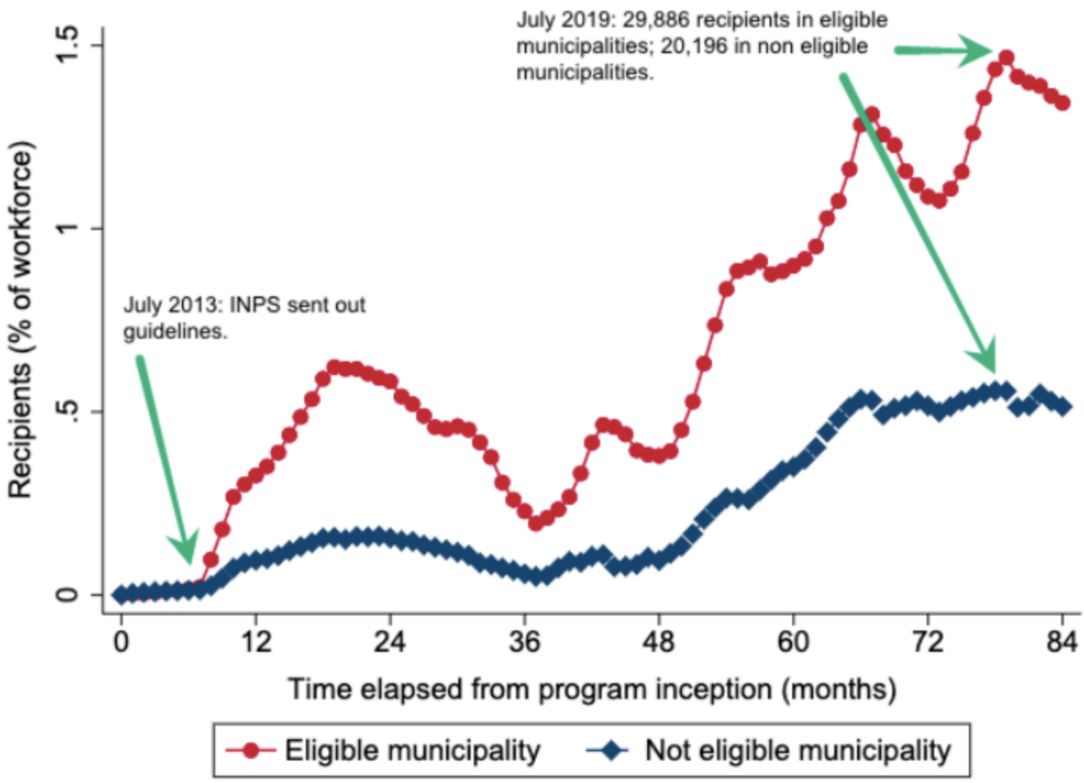
3.3 Take-Up Rate and Recipients' Characteristics

Up to December 2019, 218,768 women have been hired at least once through the preferential payroll tax scheme, corresponding to around 5 percent of the female workforce covered in Social Security archives. Figure 3 depicts the take-up rate separately by municipality for each month since the program inception (1st of January 2013). Red circles (blue diamonds) mark hires resident in municipalities where eligibility criteria are less (more) binding.

The pattern emerging from this figure leads to three main observations. First, in accordance with the eligibility criteria, the tax scheme is employed much more in eligible municipalities. Second, reform take-up was not immediate, since the Italian Social Security Institute published guidelines only in July 2013 (see INPS document n. 111/2013). Third, both the two series exhibit a non-monotonic evolution: after the initial growth, take-up rate declined significantly and almost halved after 36 months. This pattern can be the result of the temporaneous implementation of other (more generous) payroll tax cuts for low-earners (note, however, that they did not differentially target women by place, cohort or occupation). Then, there has been a steady rise in take-up rate. The highest absolute number in reform take-up rate was reached in July 2019, with 29,886 recipients in eligible municipalities and 20,196 in not eligible municipalities. Appendix Figure C3 further decomposes the reform take-up by age. In line with the eligibility criteria, the figure presents a clear spike at age 50 in non eligible

[24]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.

Figure 3: Take-up rate over time by municipality



Note: The figure depicts the portion of the female workforce hired through the preferential payroll tax scheme (vertical axis) for each month since the introduction (1st of January 2013) up to December 2019 (horizontal axis). Red circles denote take-up rate in municipalities benefiting of less binding eligibility criteria; blue diamonds mark municipalities facing more biding eligiblity criteria. Data on the universe of non-agricultural private sector workers provided by the Italian Social Security Institute.

municipalities.

Appendix Table C1 displays summary statistics of program recipients. The representative recipient earns a daily full-time equivalent gross salary of nearly 85 euros. Recipients are relatively young (average age is 38), hired temporaneously (67 percent) in blue collars (61 percent), part-time jobs (57 percent). The characteristics of employers benefiting from the payroll tax cut are presented in Appendix Table C2. They mostly operate 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 retail trade (around half of the sample), followed by accommodation and food service activities (19 percent) and manufacturing (16 percent).

4 Is the Payroll Tax Cut Shared with Workers?

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 whether the payroll tax cut has been passed on workers' wages.

4.1 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). 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.

4.2 Identification Strategy

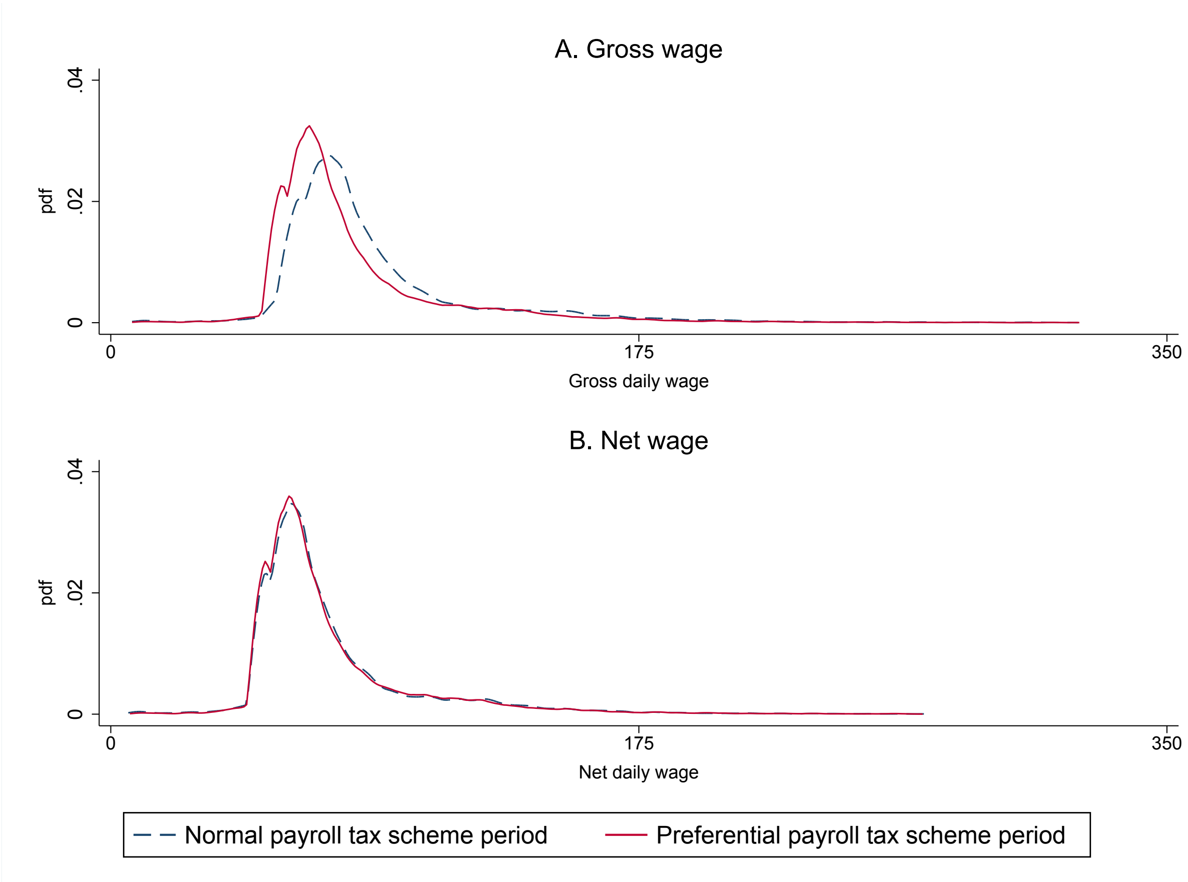
To offer prima facie evidence on tax incidence, Figure 4 plots kernel density estimates of the distribution of gross (panel A) and net wages (panel B), before (blue dashed line) and after the 2012 reform (red solid line). The kernel density estimates include the full sample of payroll tax cut's recipients. 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.

There are two main empirical issues challenging the estimation of tax incidence. First, a simple comparison in gross and net wages within the job spell, such as before and after crossing the tenure cutoff determining payroll tax cut eligibility, would not account for any factors leading wages to discontinuosly change over time.[25] Second, comparing the job eligible for the tax cut with the previous job might be concerned by endogeneity issues, since workers decide to change jobs at least partly by a comparison of their current wage to the wage in other jobs.

To account for these two concerns, I compare the evolution in gross and net wages

[25] Waldman (2012) reports that wage changes are usually discontinuous, similar to the predictions of tournament models such as Lazear and Rosen (1981) and consistent with returns to seniority or tenure (see, e.g., Buhai et al. 2014).

Figure 4: Comparing gross and net wage distributions



Note: The figure depicts the distribution of net and gross wages (in 2020 euros) 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 top 1 percent (they are included in the main analysis).

across (eligible and not eligible) jobs and before versus after crossing the cutoff determining payroll tax cut eligibility. In this way, I can evaluate tax incidence by assessing whether there is any discontinuous change in gross or net wages as the preferential tax scheme expires in the job where the payroll tax cut applies, compared to the previous (not eligible) job. Under standard tax incidence results, I should see no discontinuity in gross wages, but a discontinuity for net wages.

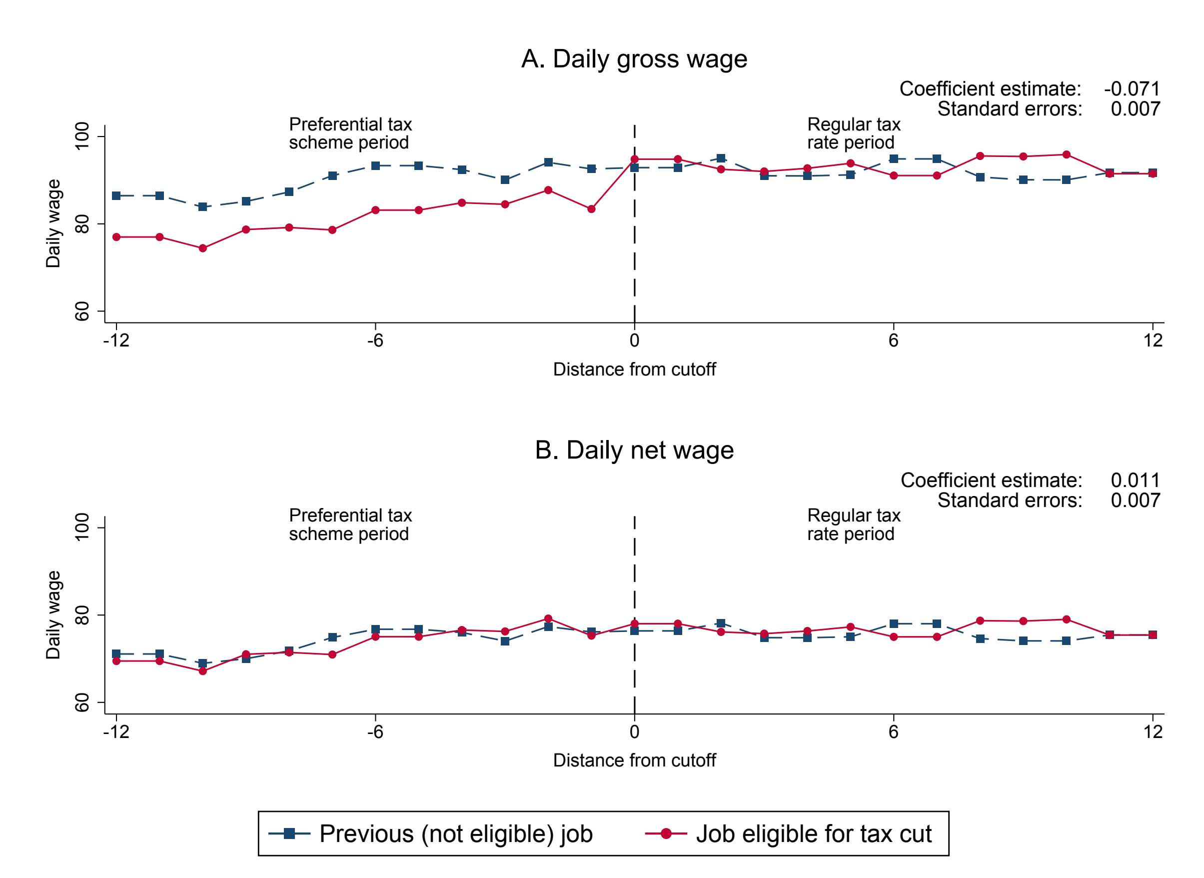
For this purpose, I employ the month-level version of the dataset and I focus on the sample of women hired with the preferential payroll tax scheme that have been employed before.[26] As the duration cutoff depends on the type of job (i.e., permanent vs temporary), I first center each time in the dataset at its respective duration cutoff. A value of 0 will thus represent a job duration exactly equal to 12 (18) months for temporary (permanent) jobs, while all other values represent deviations (in months) from crossing the duration cutoff. I then collapse net and gross wages normalized by

[26] Around 77.82 percent of women hired with the preferential payroll tax scheme has at least one job spell before 2013. In Appendix B, I run the analysis on the sample of new hires and discuss the results below.

months into monthly bins centered at the duration cutoff.

The results are presented in Figure 5. The top panel shows the evolution in *gross wages*, defined as daily (full-time equivalent) wage earnings gross of employer's payroll taxes. The bottom panel depicts *net wages*, that are daily (full-time equivalent) wage earnings net of employer's payroll taxes. The horizontal axis displays the evolution in these two variables over (normalized) tenure (defined as deviations (in months) from crossing the cutoff). 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 cutoff for the job that started with the preferential tax scheme, while the series focusing on the previous job is continuous. By contrast, the bottom panel displays a continuous series for both the two jobs. This provides suggestive evidence that employers do not adjust wages in response to the payroll tax cut.

Figure 5: 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).

To present these results more formally, I propose an approach resting on individual-level variation in wages between the job where the payroll tax cut applies with the previous job, before and after crossing the tenure cutoff determining eligibility. In this way, I can account for individual-specific unobservables and 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.[27] Specifically, I run regressions of the following form:

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

where $y_{i,t,j}$ represents wages, gross or net of the employer payroll tax, of worker i at her t^{th} month of tenure in the firm for job j. The treatment is given by the interaction between a dummy for the period before crossing the cutoff, $1(t < C)$, and a dummy for the job that started with the payroll tax cut, $1(j \in Eligible)$. The coefficient of interest, β, measures the effect of the payroll tax cut on wages.

One caveat is that the payroll tax scheme can affect job duration (I will analyze this outcome in section 6). This would make my sample endogenous to the reform and distort the job duration distribution across eligible and not eligible jobs. This issue is alleviated by using individual-month of the job fixed effects, $\gamma_{i,t}$, which account for any difference in the probability of job survival by leveraging only variation between jobs in a given individual-tenure month cell. In other words, identifying variation comes from within-individual cross-job comparison at the same tenure point. Then, the inclusion of job fixed effects, δ_j, absorbs 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.

4.3 Results

Table 2 shows the β estimates and standard errors obtained by running regressions as in equation (1). The results confirm the qualitative evidence presented in Figure 5: on average, gross wages drop by around 8.3 percent, while net wages present a not statistically significant growth by about 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. I estimate pass through to firms by 85.5 percent.

To examine tax incidence more thoroughly, the rest of the table reports the β coefficient obtained from selected samples of the payroll tax cut recipients' population. The table presents several robustness checks that confirm tax incidence is mostly on firms. I start by studying whether tax incidence varies over the (pre-reform) wage distribution, that would capture, at least in part, heterogeneous effects by skill groups. I find limited heterogeneity over the wage distribution: pass-through to firms is 1.12 (0.74) percent for workers in the bottom (top) half of the wage distribution; this is in line with

[27] I deliberately avoid comparing the salary offered to new female hires with that of new male hires or incumbent workers (either female or male), as this could be driven by unobserved heterogeneity and gender wage gaps.

the graphical evidence provided in Figure 4.

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*: Gross wage			
$1(t \leq C)$ $\times$ $1(j \in Eligible)$	-0.071*** (0.007)	-0.093*** (0.009)	-0.061*** (0.015)	-0.105*** (0.011)	-0.097*** (0.013)	-0.108*** (0.013)	-0.071*** (0.025)
				B. *Outcome*: Net wage			
$1(t \leq C)$ $\times$ $1(j \in Eligible)$	0.012 (0.007)	-0.010 (0.009)	0.021 (0.015)	-0.021 (0.011)	-0.013 (0.013)	-0.025 (0.013)	0.013 (0.025)
Observations	101,353	61,018	40,335	48,289	40,146	37,429	9,125
Ind. $\times$ tenure FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Job FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pass-through to firms	0.855	1.120	0.744	1.250	1.155	1.301	0.845

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-tenure fixed effects and job fixed effects. Panel A (B) shows the results on wages gross (net) of employer-borne payroll tax rate. Pass-through to firms is defined as the fraction of payroll tax that benefit the firm. 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 job that started with the preferential tax scheme. Standard errors in parenthesis clustered at individual level.

Another possibility is that wages do not change because of the presence of implicit contracts whereby firms offer the same wage of previous job. In column (4), I focus exclusively on workers that changed employer, who are not affected by implicit wage contracts by definition. The coefficient estimate does not significantly change. To further reinforce this result, I estimate payroll tax incidence by focusing exclusively on a sample of young workers (younger than 35) entering for the first time in the labor market. For the sake of space, I present this additional analysis in appendix B, where I run a difference-in-difference analysis comparing men and women's wages, before and after the introduction of a payroll tax cut. I start from a simple model with sex and year fixed effects, then I also leverage within-firm variation and account for any firm-specific economic shocks by including firm-year fixed effects. This analysis presents similar results to the baseline results presented above. I estimate pass-through to firms by 92 percent (see Table B1). This implies that new female hires over the post-reform period enjoy a 0.7 percent increase in net wages relative to male hires.

Women might also change 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 industry that would be more specialized in their job task.[28] Columns (5)-(6) show that this is not the case: each specification presents full pass-through to firms as in the baseline model.

An additional channel explaining wage rigidity 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.[29] 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 84.5 percent in this case, any growth in wages of female workers is small and not statistically significant at usual confidence intervals.

An additional possibility is that it takes time for wages to adjust. In Table C3, 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. Furthermore, Appendix Figure C4 shows limited heterogeneity effect by industry.

This absence of incidence on net wages is in contrast with the standard view in public economics, suggesting that tax incidence is on workers.[30] This result would thus suggest that the payroll tax cut lowers labor costs per-unit and stimulates employment. I will now investigate this possibility.

5 Employment Effects of the Payroll Tax Cut

To identify the employment effect of the payroll tax cut, 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 occupations 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 5.1). Second, focusing on not eligible municipalities, I relate employment of women younger than 50 with those older than 50, where the minimum non-employment duration requirement discontinuously drops by 12 months (section

[28] 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 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.

[29] 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.

[30] This result does not imply that other forms of rent sharing have been implemented, such as promise of longer tenure, better job tasks, increased fringe benefits, longer paid holidays or better offices. The data do not allow me to investigate these additional channels.

5.2). Third, I exploit the cutoff determining earlier eligibility (6 months instead of 24) for male-biased occupations in a regression discontinuity (RD) approach (section 5.3). Finally, I incorporate all these sources of identifying variation to leverage individual-level variation in payroll tax cut eligibility (section 5.4). Table C4 summarizes the main features of these empirical approaches.

5.1 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 minimum non-employment duration requirement. Namely, ignoring any differential cross-occupation exposure and focusing on women younger than 50, the minimum non-employment duration requirement drop by 18 months (from 24 to 6) in municipalities eligible for EU structural fund. This exercise is similar in spirit to the graphical evidence presented in Figure 3, 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 C5), including pre-existing female employment share (see Figure C2) - were on similar trend before the reform. Third, I can investigate the dynamics of employment changes.

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 $N_{m,t}$ is the number of workers (male or female) in municipality m at year t. 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, $\beta_j = 0 \; \forall j < 2012$.

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.[31] Then, γ_m accounts for for any time-invariant municipality policy or characteristics. Finally, $u_{m,t}$ is an error term. I cluster the standard errors at the municipality-level.

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

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

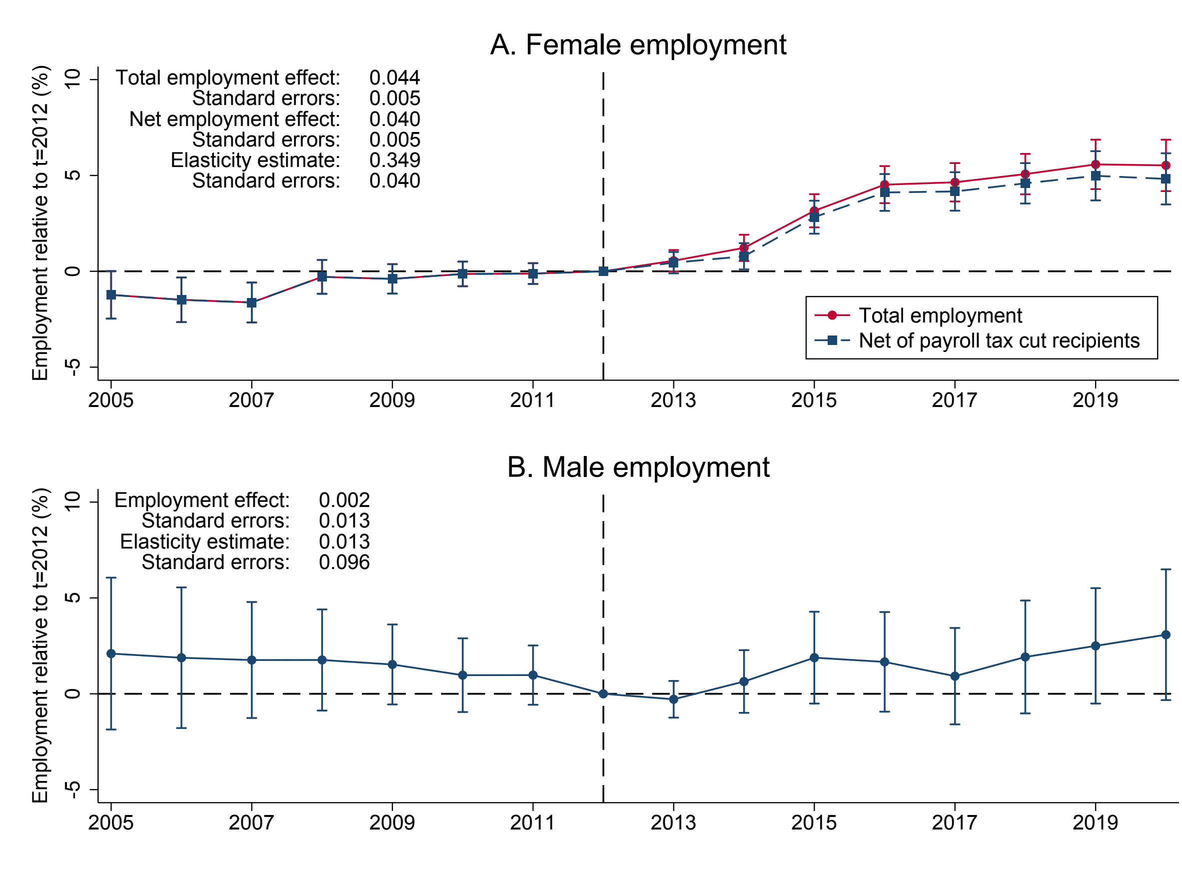
where the payroll tax rate in municipality m at time t, $log(1 - \tau_{m,t})$, is instrumented by the interaction between a dummy for eligible municipalities and a dummy for the post-reform period, $1(m \in Eligible) \cdot 1(t \in Post)$. I compute the net-of-payroll tax rate, $\tau_{m,t}$, as the post-reform 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 (reduced form) to the DiD of the log net-of-payroll tax rate (first stage).

Figure 6 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 the mechanical employment increase (i.e., net of workers that are currently subject to the preferential tax scheme), depicted by the blue squares. The figure provides three main findings. First, there is compelling evidence of employment responses: female employment 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. This effect translates in a net-of-payroll tax elasticity of 0.349. Second, the aggregate employment growth is much larger than the mechanical effects, that is the count of workers that are benefiting of the preferential payroll tax scheme. One suitable explanation for this result is that the payroll tax cut leads women not only to enter in the labor force, but also to remain employed even after the payroll tax cut no longer applies. In section 6, I will provide evidence in support of this fact by showing that the fraction of jobs terminating when the payroll tax cut expires is very small. Third, with the exception of years 2006 and 2007, 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

[31]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.

Figure 6: 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 obtained from equation (2): 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 $j < 2012$) relative to the year before the reform.

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 male workers. Panel B of Figure 6 shows that male-female substitution is not likely to be the case behind the document employment effect.

To further leading credence to this result, appendix Figure C5 reports the effect of the reform on days of work, aggregated by year and municipality and obtained by regressing equation (3). The figure shows an average increase of about 3.1 percent in days of work of female workers in eligible municipalities, compared to not eligible municipalities. The impact on men is not statistically significant.

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 proxy labor market tightness by the average unemployment rate observed in a municipality over the pre-reform period. Then, I implement a triple difference approach by further adding to equation (2) the triple interaction between eligibility dummy, year dummies and a dummy for municipalities having unemployment rate above the national median. Coefficient estimates and 95 percent confidence intervals obtained from the triple interaction are displayed in Figure C6. The figure shows that employment effects are significantly larger in places where the pre-reform unemployment rate was higher. This 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 C7, I perform an event study analysis as in equation (3) but using a dummy for commuting from non eligible to eligible municipalities as outcome variable. 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.[32] Figure C8 provides reassuring evidence that the effects are remarkably similar when I use this data source.

5.2 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.

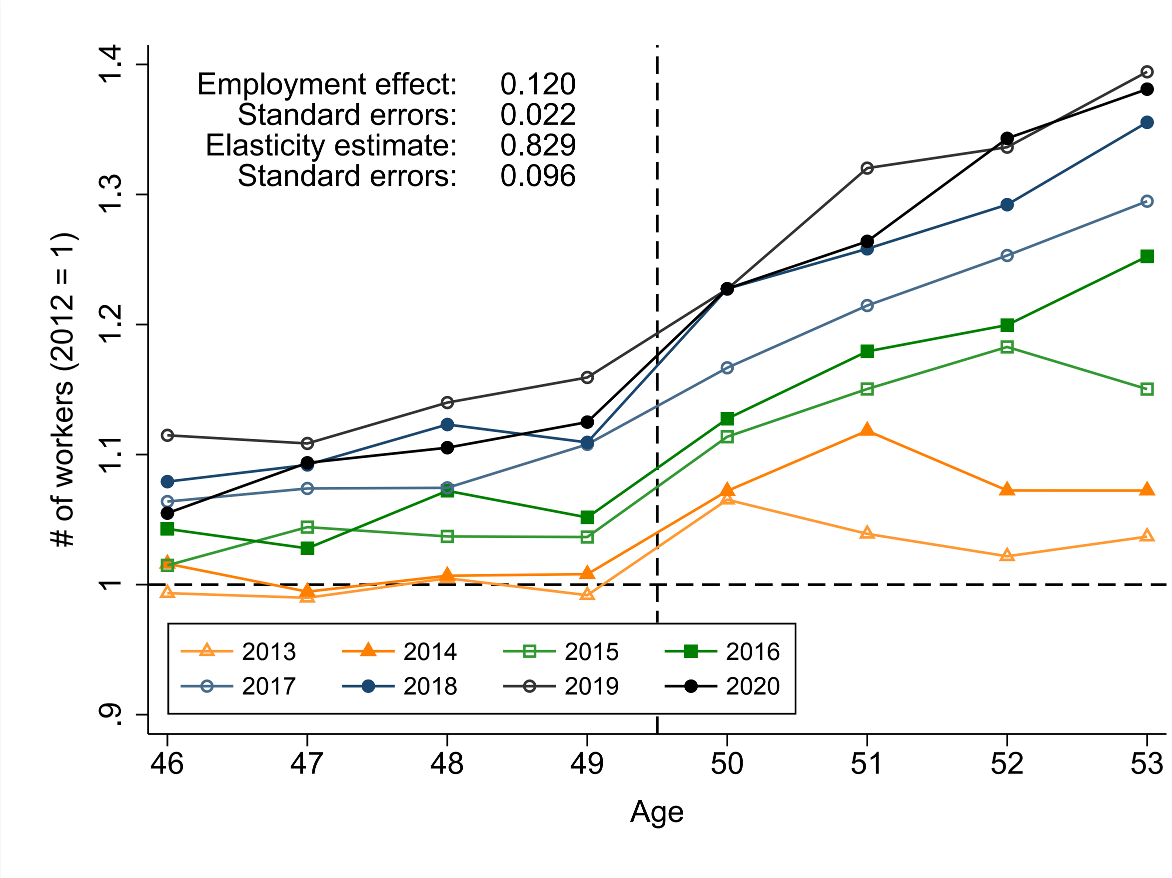
Figure 7 presents a difference-in-differences analysis by focusing on female workers with ages 46-53. The vertical axis measures the number of workers by age and year relative to 2012, which allows to absorb any cohort-specific persistent difference in employment. The figure thus displays the deviation 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.[33]

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

[32] I focus on taxpayers whose income is lower than 15,000 because it better approximates average payroll tax cut recipient's earnings. One caveat of this analysis is that the data do not provide separate series by gender. Hence, I am assuming that employment responses are only coming from female workers as in the main analysis.

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

Figure 7: 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. The figure also reports coefficient estimate and standard errors based on a DiD approach with age and year fixed effects.

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.

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 worker's age at year t. The outcome variable, $N_{a,t}$, is the count 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 payroll tax cut on employment. Finally, $u_{a,t}$ is the error term.

I also compute the net-of-payroll tax 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 $1(a \geq 50)$ and $1(t \in Post)$. I estimate an elasticity of 0.729. Compared to the cross-municipality approach, this larger coefficient estimate is likely to reflect strong substitutability across workers with presumably similar experience and human capital, but different exposure to the payroll tax cut. It should therefore be interpreted as an upper bound.

In the appendix, I present two additional pieces of evidence supporting this finding. First, I replicate the same exercise on days of work, aggregated by age and year. Figure C9 shows that a discontinuity in days of work for workers older than 50 emerged. More exposed cohorts work around 10.8 percent more days over the post-reform period compared to less exposed cohorts. Second, in Figure C10, 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.

5.3 Cross-Occupation Analysis

Would ad hoc policies be successful in occupations that are particularly gender segregated? If employers face implicit restrictions, stemming either from gender attitudes (Bertrand et al. 2020), gender identity concerns (Akerlof and Kranton 2000) or gender stereotypes that affect beliefs (Bordalo et al. 2019), there could be barriers for female employment in certain occupations. To shed light on this question, I take advantage of the cutoff-rule favoring male-biased occupations in determining eligibility for the preferential tax scheme. Namely, focusing on women younger than 50 and living in municipalities not eligible for EU structural fund, the minimum non-employment duration requirement reduces by 18 months (from 24 to 6) for women hired in occupations where the gender employment gap is at least 25 percent larger than the average employment gap observed two years before.

Although demand and supply for female workers are likely to differ in both observable and unobservable ways across occupations, these differences can be minimized by focusing at occupations where the gender employment gap is close to the cutoff. Intuitively, an occupation where the gender employment gap is barely above the cutoff is likely to be similar to an occupation 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 the payroll tax cut on female employment.

Yet, the dynamic nature of the eligibility process complicates the standard RD analysis. As the running variable is a *year-varying* function of the gender employment gap, an occupation 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: an occupation in the "control" group - one where the share of female workers is narrowly below the cutoff - might become treated in a successive year.[34]

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. The "optimal" bandwidth is computed using the algorithm proposed by Calonico et al. (2014). Then, I run a local linear regression of the following form:

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

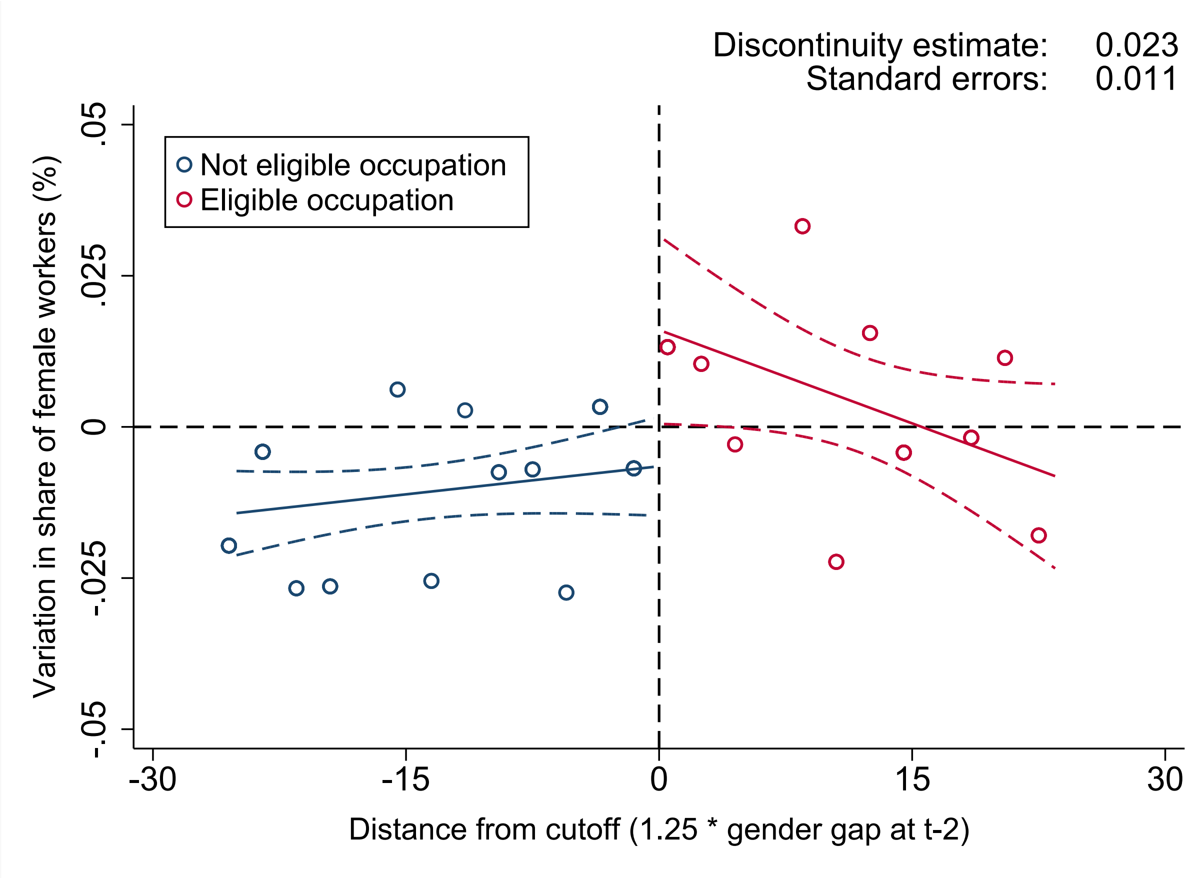
where the outcome variable, $\Delta Share_{o,t}$, is the first difference in the share of female worker in occupation o at time t. $1(Gap_{o,t-2} \geq C_{t-2})$ is an indicator for payroll tax cut eligibility after 6 months of non-employment (instead of 24 months); it is equal to 1 if the gender employment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

Figure 8 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 in gender segregated occupations. 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 occupations more exposed to the payroll tax cut.

In the appendix, I check the robustness of this result by presenting four additional tests. First, Figure C11 shows that the density of the gender employment gap is smooth around the cutoff, as would be expected in a valid RD design (McCrary 2008); the McCrary discontinuity estimate is 0.452 (0.579). Second, Table C6 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. Third, I depart from the baseline first difference model by using a model with occupation and year fixed effects. In this case, identification stems from within-occupation temporal variation in eligibility for the preferential payroll tax scheme. The

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

Figure 8: Employment effect, cross-occupation analysis



Note: The figure presents the effect of (a stricter exposure to) the payroll tax cut on the gender employment gap. The figure also reports the β coefficient and occupation-level clustered standard errors estimated from equation (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 an occupation. Scatter points are sample average over intervals of 2 cutoff points bins.

coefficient estimate does not change. Finally, in Figure C12, 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 occupations in this group of municipalities, I do not expect to find any discontinuity. Reassuringly, I find a smooth distribution around the cutoff.

5.4 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 rather than on actual eligibility. As eligibility varies along several non mutually exclusive dimensions, it is likely that my treatment definition - either based on differential exposure across municipalities, cohorts or occupations - may contain measurement errors, thus making treatment effects biased.

To overcome this issue, I draw the analysis on month-level data to create an individual-specific treatment that can incorporate all the sources of payroll tax cut eligibility presented above. By combining the period of non-employment (in months) from two consecutive job spells with the factors determining earlier eligibility (i.e., municipality of residence, age and occupation), I can create a dummy variable, $D_{i,t}$, equal to 1 as an individual i meets the minimum non-employment duration requirement at time (month) t; 0 otherwise.[35]

I will then compare within-individual variation in employment 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 employment: it is equal to 1 if individual i works a positive number of hours during a month t; 0 otherwise. 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. Month fixed effects, δ_t, account for any common month-level shocks. Finally, $u_{i,t}$ is an error term. Following the three-group criteria determining eligibility, I use three-way clustered standard errors by municipality, age and occupation.

The coefficient of interest, β, computes the percentage change in the probability of entering in the labor force for workers eligible for the payroll tax cut during the post-reform period. Differently from the previous aggregate analyses, it measures the "treatment on the treated" (TOT) impact of the payroll tax cut on "compliers" women, that is female hires that meet eligibility and then were actually hired with the preferential payroll tax scheme.[36]

Top panel in Table 3 shows the β coefficient estimates from variants of equation (6), while the bottom panel presents the effects in elasticity terms by regressing labor force participation on the net-of-payroll tax rate, instrumented by the interactions between the eligibility dummy and the post-reform dummy. I start from a simple model containing individual fixed effects and time (month) fixed effects. I estimate an employment increase of 5.4 percentage points over the post-reform period, which translates into an elasticity of 0.413.

In columns (2)-(4), I add interactions between years and the three factors determining earlier eligibility: municipality of residence, occupation (defined at the 2-digit ISCO group) and age group (above or below 50). These interactions reduce the chances

[35]Specifically, $D_{i,t}$ is set to 0 in each month t where an individual i is not observed in social security data. As discussed previously (section 3.1), this approach has the limitation of not covering possible transitions towards public employment or self-employment.

[36]Imperfect take-up rate would make β to measure an "intent-to-treat" (ITT) effect and lead the β estimate to be valid exclusively for compliers. In the case of imperfect treatment take-up, the TOT can be recovered by implementing a two-stage least squares model that scales ITT effects of actual treatment take-up on labor force participation (the "reduced form") by the first-stage estimate regressing treatment take-up on potential eligibility. However, there are only a few cases where eligibility did not map into treatment take-up. Therefore, the sampling variation in first and second stage is almost identical and ITT are not significantly different from the TOT effects.

that unknown shocks or policies that differentially affect women with different demographic characteristics or operating in different places or industries are confounding the effect I ascribe to the payroll tax cut. The coefficient substantially drops: the probability of labor force participation is between 1.5 and 1.6 percentage points, while the elasticity estimate is between 0.117 and 0.123. In columns (5) and (6), I further include interactions between these fixed effects, so to exploit variation within a given municipality-occupation-age group-month cell. Coefficients are precisely estimated and are not sensitive to the inclusion of these fixed effects.

Finally, column (7) interacts the individual fixed effects with occupation fixed effects. This interaction allows to account for any sorting of individuals in occupations eligible for less binding eligibility criteria over the post-reform period. I find that sorting explains little, if any, of the estimated impact.

Because the net-of-tax wage of directly treated workers does not change, this employment response is likely to reflect labor demand effects rather than labor supply responses. My elasticity estimates are similar to the 0.21 baseline elasticity estimated by Saez et al. (2019) for young workers in Sweden. Given the average labor force participation probability of 55.6 percent, my most conservative estimate thus suggests that female employment raised by about 2.5 percent in response to the introduction of the preferential tax scheme. As the employment gap in Italy reduces by around 3.2 percentage points over the period of interest, the reform would explain, ceteris paribus, around forty percent of the reduction in the gender employment gap.

5.5 Does the Payroll Tax Cut Move Women Out of Welfare?

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 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

Table 3: Employment effect, micro-level evidence

	Outcome: 1(individual i is employed at t)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	A. Employment effect						
$D_{i,t} \cdot 1(t \in Post)$	0.054***	0.034***	0.015***	0.016***	0.015***	0.015***	0.014***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
	B. Elasticity estimate						
$log(1 - \tau_{i,t})$	0.413***	0.268***	0.122***	0.123***	0.117***	0.116***	0.107***
	(0.012)	(0.008)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)
Observations	948,345,949						
Individuals	8,841,137						
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mun × year FE	No	Yes	Yes	Yes	Yes	Yes	Yes
Occ × year FE	No	No	Yes	Yes	Yes	Yes	Yes
Age × year FE	No	No	No	Yes	Yes	Yes	Yes
Mun × age FE	No	No	No	No	Yes	Yes	Yes
Mun × occ FE	No	No	No	No	Yes	Yes	Yes
Age × occ FE	No	No	No	No	Yes	Yes	Yes
Mun × age × occ FE	No	No	No	No	No	Yes	Yes
Ind × occ FE	No	No	No	No	No	No	Yes
Mean dependent	0.556	0.556	0.556	0.556	0.556	0.556	0.556

Note: This table reports the effect of the payroll tax cut on female labor force participation in the top panel; the bottom panel reports net-of-payroll tax elasticity estimate of female employment. The outcome variable is a dummy equal to 1 if a worker is employed at time (month) t; 0 otherwise. The first column includes municipality and month fixed effects. In columns (2)-(7), I cumulatively add municipality-by-year fixed effects (column 2), occupation-by-year fixed effects (column 3), age group-by-year fixed effects (column 4), municipality-by-age group, municipality-by-occupation and age group-by-occupation fixed effects (column 5), municipality-by-age group-by occupation fixed effects (column 6), and individual-by-occupation fixed effects (column 7). The sample includes the full sample of female workers covered in Social Security archives. Standard errors in parenthesis clustered at municipality-occupation-age group level.

payroll tax cut, we should expect limited, if any, effects.[37]

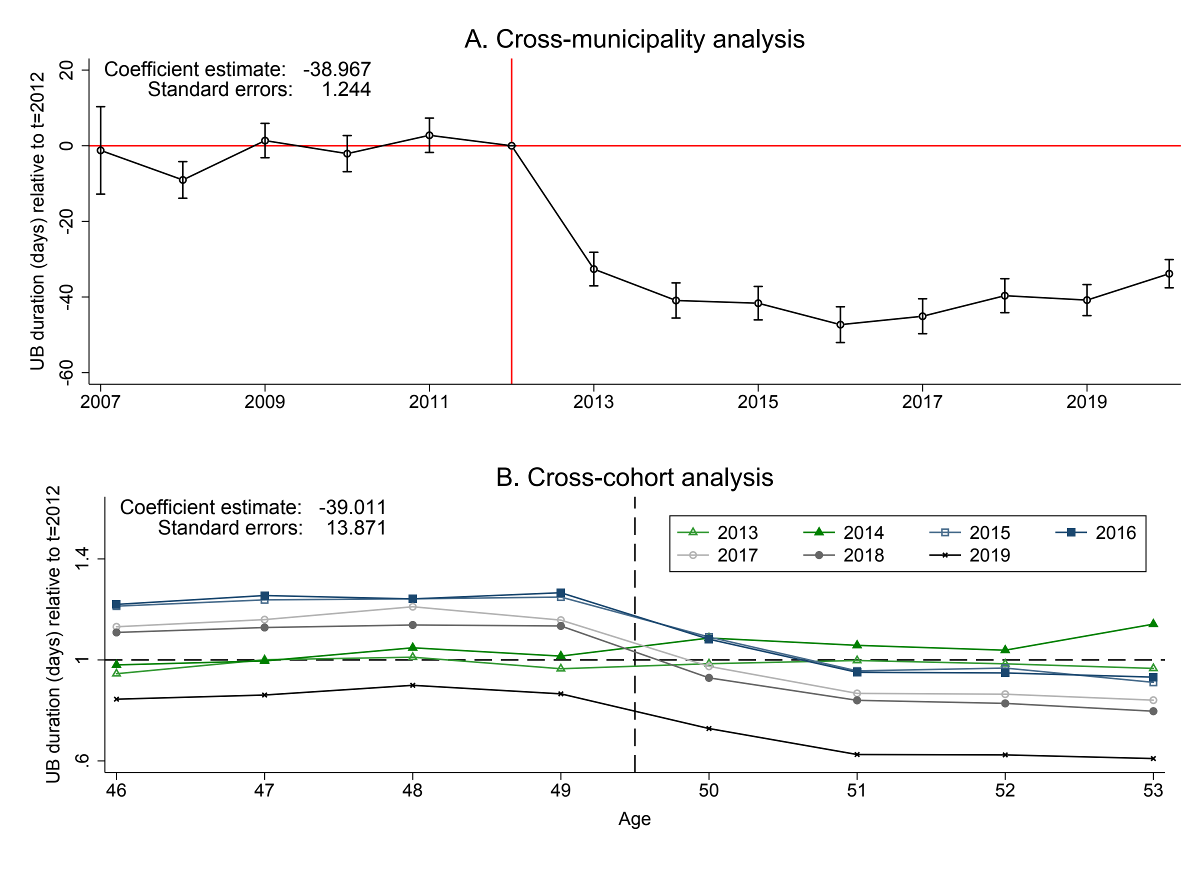
To investigate this question, I collect data on the universe of recipients of unemployment benefits. Then, I study the relationship between average UI benefits duration and exposure to the payroll tax cut by using the cross-municipality and cross-cohort empirical approaches presented previously (see 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.

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

[37]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.

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 C13 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 UI benefits



Note: This figure shows the effect of the payroll tax cut on the effective duration of unemployment insurance (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.[38]

5.6 Taking Stock of Employment Effects

Overall, the broad picture emerging from different identification strategies and samples is that the reform successfully stimulated female labor force participation. The absence of any crowding out effect on male employment maps into a reduction in the gender employment gap. The increase in female labor demand in gender imbalanced places and occupations suggests that gender stereotypes and prejudices are not sticky, but they are malleable and can be shaped through ad hoc policies. By making gender discrimination more costly, the payroll tax cut reduced discrimination by "taxing" gender prejudiced employers, thus leading them to raise their demand for female labor.

6 Does the Payroll Tax Cut Affect Job Duration?

The payroll tax incidence analysis implies that the payroll tax cut created an incentive to hire and retain eligible women, at least until the expiration of the preferential payroll tax scheme. A natural following question is: Do employers offer job contracts with limited duration, that is up to the limit where payroll tax cut eligibility applies?

6.1 A Notch in the Labor Cost vs Job Duration Distribution

To test this question, first consider a standard frictionless labor market model where firms maximize profits using labor and capital as inputs. During the period before the introduction of the preferential payroll tax scheme, the payroll tax rate does not vary by workers' tenure within the firm: firms face a (flat) payroll tax rate τ that applies to gross earnings y, regardless of job duration d. Job duration is distributed according to a smooth density distribution $h(d)$ and any heterogeneity in job duration is due to preferences or any idiosyncratic shocks affecting employers or workers (e.g., a worker retires or dies, firms run out of business, etc.) that would lead a job to terminate.

The introduction of a time-limited preferential payroll tax scheme creates a notch: a discontinuity in the firm choice set of labor cost versus job duration.[39] That is, hiring a payroll tax cut eligible female worker reduces labor cost by $\Delta\tau$ up to the job duration cutoff d^*. As shown in the Appendix Figure C14, such a notch introduces an incentive for terminating jobs just as the duration cutoff is meet, thereby creating a hole in the job duration distribution falling into a segment $[d^*, d^* + \Delta d^*]$, and excess bunching just at the duration cutoff.

[38] One potential spillover effect is that the female-specific payroll tax cut might lead men to raise their period spent on welfare. For instance, Levine (1993) finds that increases in the replacement rate of UI benefits decreases the time that workers ineligible for UI spent unemployed. Using the same empirical approaches as above, I do not find any significant effect on men's UI benefits duration.

[39] As the tax incidence analysis shows that the payroll tax cut mostly benefited firms, I model the time-limited preferential payroll tax scheme as a notch in the choice set of *employers* rather than workers.

The figure also provides an example on how job duration changes in response to the payroll tax cut. I denote workers H as those with larger job duration in the absence of the preferential tax scheme, having preferences for working up to $d^* + \Delta d$. Workers L are those with job duration exactly at the cutoff even in the absence of the tax scheme, since their indifference curves remain tangent to the lowest part of the budget set. After the introduction of the tax scheme, a profit maximizing firm offer jobs of duration d^* to workers of type H.

This simple frictionless model predicts that the preferential payroll tax scheme would produce a spike in job terminations just at the duration cutoff. The basic assumption is that outside (payroll tax cut eligible) workers are perfect substitutes for incumbent (no longer payroll tax cut eligible) workers and thus predicts that firms can minimize labor costs by simply replacing workers with different payroll tax burdens. However, this result might be questioned by frictions, such as search or firing costs, or if the firm's production process relies on specific human capital that affect firm's demand for incumbents.

6.2 Empirical Strategy

To estimate bunching responses, I first center each job duration in the dataset at its respective cutoff. A value of 0 will thus represent a job duration exactly equal to 12 (18) months for temporary (permanent) jobs, while all other values represent days deviations from the duration cutoff. I then group these normalized job duration into j bins centered at the duration cutoff m_j and count the number of job ending in each bin, n_j.

To estimate the counterfactual distribution, the standard approach is to first define an excluded range around the cutoff, $[m_L, m_U]$, such that $m_L < 0 < m_U$, and then to run a regression of the following form:

$$n_j = \sum_{i=0}^{p} \beta_i \cdot (m_j)^i + \sum_{i=L}^{U} \gamma_i \cdot 1(m_j = i) + u_j, \tag{7}$$

where the first term on the right-hand side is a p-th degree polynomial in job duration; the second term is an indicator function for bins located in the excluded range. Following Chetty et al. (2011), I compute standard errors by using a bootstrap procedure in which a large amount of job duration distributions are generated by random resampling the error term u_j.

The counterfactual bin counts are calculated as the predicted values from equation (7) omitting the contribution of dummies in the excluded range, i.e.:

$$\widehat{n_j} = \sum_{i=0}^{p} \beta_i \cdot (m_j)^i. \tag{8}$$

To estimate excess bunching, one can then compare the observed and counterfactual job duration distributions absent the preferential tax scheme in the excluded range:

$$\widehat{B} = \sum_{j=L}^{0} (n_j - \widehat{n}_j) = \sum_{j=L}^{0} \widehat{\gamma}_j, \tag{9}$$

while the corresponding missing mass is $\widehat{M} = \sum_{j>0}^{U} (n_j - \widehat{n}_j) = \sum_{j>0}^{U} \widehat{\gamma}_j$.

There are three main issues that need be to solved. First, the standard approach to define the excluded range is to cover the entire area affected by bunching responses. In general, due to optimization errors, search and hiring costs, and randomness in job separations, this is likely to represent an asymmetric area around the cutoff or even areas not immediately above the cutoff. Therefore, the diffuseness of the hole above the notch may make difficult to visually determine the upper bound m_U. To deal with this issue, I follow Kleven and Waseem (2013) approach to estimate equation (7) together with m_U through an iterative procedure that ensures bunching mass equals missing mass.

Second, the duration cutoff may represent a reference point (see Kleven and Waseem (2013) for round-number bunching in reported taxable income). That is, 12 and 18 months are salient round dates for job duration. This makes the cutoff a natural focal point for job duration other than the tax incentive. In this case, the counterfactual distribution obtained from equation (7) would be upward biased. For netting out round-number bunching, I use the observed distribution before the introduction of the preferential tax scheme. Using such difference-in-bunching strategy, I can identify the effect of the preferential tax scheme on job duration by taking the difference in bunching before vs after the introduction, for the same set of individual (see Best et al. (2020) for an example of this kind of approach). In this way, excess bunching would be computed as the difference in job counts before and after the introduction of the preferential tax scheme, that is:

$$\Delta\widehat{B} = \sum_{j=L}^{0} (n_{j,t} - n_{j,t-1}). \tag{10}$$

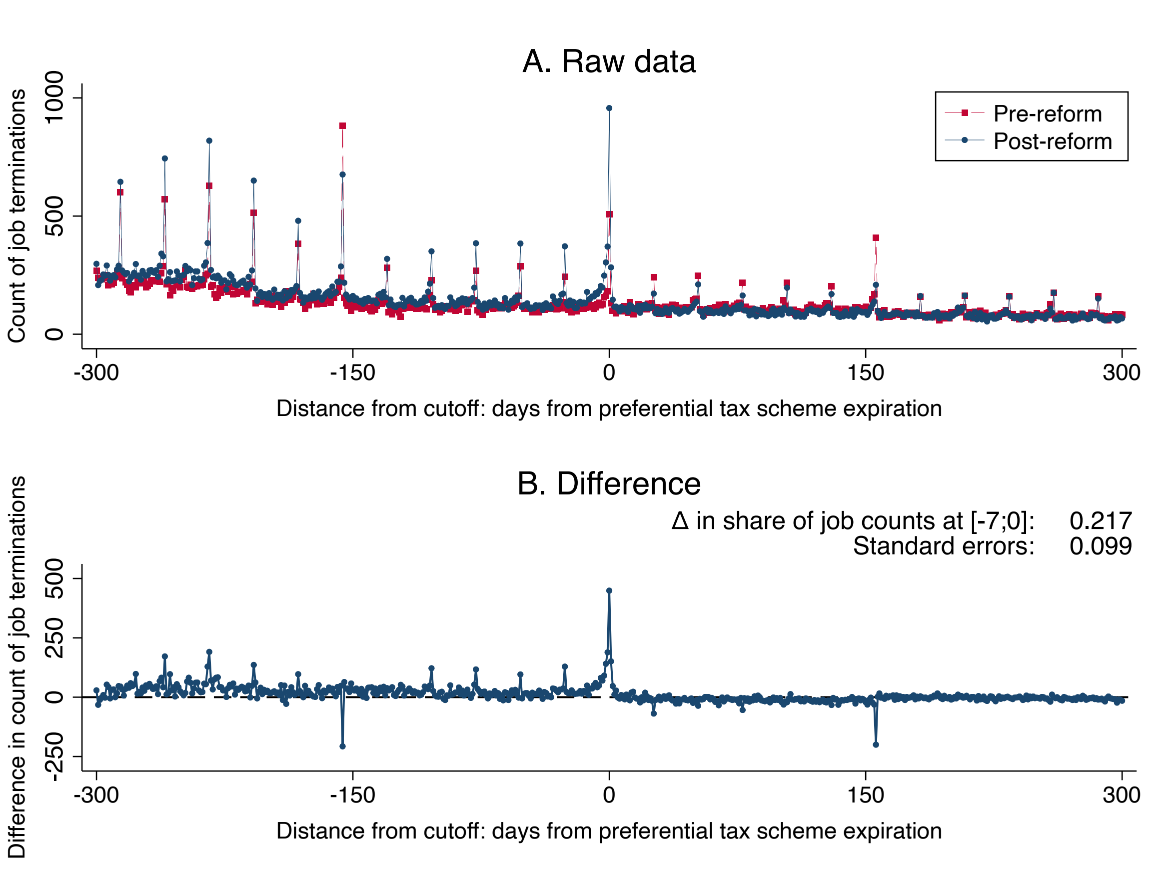
Third, the average job duration for a given payroll tax cut recipient might not be comparable over time, as she might still be employed in the job starting with the payroll tax cut. To overcome this issue, I focus on the sample of payroll tax cut recipients with limited job duration (i.e., that resigned or that have been fired). In the spirit of the payroll tax incidence analysis presented in section 4, this sample selection choice allows to compare job duration across eligible and not eligible jobs for a given constant set of individuals.[40]

[40] Another potential issue is that equation (7) does not account for the left shift in the post-reform observed job duration distribution in the interior of the upper bracket. As argued by Kleven (2016), unless bunching responses are very large and the density distribution is steep, the interior shift will have little impact on observed bin counts in a range just above the cutoff. There are two main reasons. First, the interior shift in the distribution tends to be a very small number due to the local nature of bunching responses. Second, the distribution left-shifting depends on the distribution slope; hence, it will have a significant effect on bin counts around the threshold only if the distribution is sufficiently

6.3 Results

Figure 10 plots the count of job terminations for payroll tax cut eligible jobs in 1-day bin (vertical axis). The horizontal axis is the deviation (in days) from the cutoff determining eligibility for the preferential payroll tax scheme. Although my estimation rests on the full sample, the figure narrows the analysis to the range of 300 days before or after crossing the cutoff determining the period when the payroll tax cut applies. Appendix Table C7 reports bunching coefficient estimates and bootstrapped standard errors.

Figure 10: The Impact of The Payroll Tax Cut on Job Duration



Note: The graph in the top panel plots the density of job duration over the pre-payroll tax cut period (red squares) and the post-payroll tax cut period (blue circles). The vertical axis depicts the count of job terminations in a given 1-day bin. The horizontal axis is the deviation (in days) from the cutoff determining eligibility for the preferential payroll tax scheme. The bottom panel plots the difference in count of job terminations between the two series. The bottom panel also reports excess bunching in the share of the difference in job termination counts and bootstrapped standard errors.

The following main insights emerge from Figure 10. First, the top panel presents the raw data on the job duration distribution: it clearly shows that round-number bunching in job duration is an issue in this setup. Estimating the counterfactual distributions and the excluded range as described above, I estimate excess bunching by 504 jobs during the pre-reform period, and 1,688 jobs during the post-reform period. Comput-

steep.

ing the excluded range around the cutoff, $[m_L, m_U]$, by following Chetty et al. (2011), the pre-reform period shows excess bunching of 0.428 percent of jobs, while the post-reform period presents excess bunching of 1.311 percent of jobs.

Second, the bottom panel nets out round bunching by presenting the difference in job termination counts with respect to the previous job. The figure shows a clear spike just at the cutoff: there is an excess mass of 437 jobs terminating just at the cutoff (0.941 percent of the jobs in the sample). Considering the whole excluded range around the cutoff, there is an excess mass of 0.217 percent of jobs. I can express this effect into elasticity terms by scaling the percent change in bunched jobs (0.00217) with the percent change in the net-of-payroll tax rate ((1-0.11)-(1-0.22)). The corresponding elasticity is 0.019, suggesting that a 1 percent raise in the net-of-payroll tax rate would raise job duration (in days) by 0.019 percent.

Third, there are non-trivial holes for jobs terminating before and after 6 months from the notch.[41] A suitable interpretation of this result is the presence of optimization frictions in choosing job length. In fact, in Italy job contracts are usually expressed in 6-month intervals. Furthermore, 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.

6.4 Mechanisms

The anatomy of bunching is consistent with the predictions of a model in which employers face optimization frictions, such as search costs for switching (not anymore eligible with newly eligible) workers and hiring or firing costs.[42] Such a model would predict that workers that are more substitutable should generate larger elasticities, reflecting that firms face relatively lower search costs when looking at less substitutable workers and thus create more aggregate bunching at the job duration cutoff for this type of workers. To shed light on the role of workers' substitutability to explain the observed bunching responses, I compare bunching around the threshold in contexts and among workers where, ceteris paribus, substitutability would differ.

Figure 11 presents bunching responses in four groups of workers. It depicts the difference in job terminations in 7-day bins, to improve readability. It also reports excess bunching estimates, obtained from equation (10), and bootstrapped standard errors. Additional results and robustness to choosing μ_L are reported in Appendix Table C8.

There main findings emerge from the figure. First, panel A shows that bunching at the duration cutoff is significantly lower among high-skilled (white collar) workers

[41] As in the data tenure is expressed as *working* days, 6 month corresponds to 156 working days.

[42] See Chetty et al. (2011) for a seminal contribution on how such optimization frictions affect the anatomy of bunching in Denmark. A recent stream of the literature has attempted to quantify the impact of optimization frictions using a notch or kink approach. For instance, Kleven and Waseem (2013) find that as much as 90 percent of workers does not adjust labor supply due to some forms of optimization frictions.

Figure 11: The Role of Workers' Substitutability

Note: The figure depicts the difference in job terminations in 7-day bins (vertical axis) against the deviation (in days) from the cutoff determining eligibility for the preferential payroll tax scheme (horizontal axis). Each panel reports separate series by white collar vs blue collar workers (panel A), part-time vs full-time jobs (panel B), municipalities eligible for less binding payroll tax cut eligibility criteria vs not eligible (panel C), and for workers 45-49 vs 50-54, with the latter being more exposed to the payroll tax cut in non eligible municipalities (panel D). The figure reports excess bunching (i.e., the share of excess jobs in the interval [-7;0] days before meeting the cutoff) and bootstrapped standard errors.

compared to low-skilled (blue collar) workers. In the bunched region, there are 0.919 percent excess job terminations for blue collars, while they are around one-fourth lower for white collars. A simple explanation is that on-the-job training and learning that generate job match-specific skills (and ultimately create rents) matter much more for high-skill workers.

Second, frictions are less likely to constrain adjustments for part-time workers than full-time workers. Specifically, 0.534 percent of jobs terminations are in the bunched regions against of part-time jobs, against 0.358 percent for full-time jobs.

Third, there is slightly larger excess bunching in labor markets where the pool of eligible workers is relatively higher. Panel C shows that the probability of terminating the job as the payroll tax cut expires is somewhat larger for workers resident in eligible municipalities. Similarly, panel D focuses on non eligible municipalities and shows the count of job terminations for workers aged 50-54 is larger compared to 45-49. As in these contexts substitutability is easier, employers are more likely to overcome any form of frictions.

7 Does the Payroll Tax Cut Improve Firm Performance?

Becker (1957) seminal work on labor market discrimination assumes that discrimination is the product of personal prejudice. Gender-prejudiced employers prefer to hire male workers even if less productive than female workers. This behavior would predict i) segregation of female workers towards less prejudiced employers; ii) lower profits and worse business performance (and ultimately market failure) for discriminatory employers in competitive labor markers. By making gender discrimination more costly, the payroll tax cut should reduce discrimination by "taxing" gender prejudiced employers. In this section, I take a firm-level perspective to study what happened 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?

7.1 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.[43]

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 C15). 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 death, I focus on firms that already existed before the reform.[44] I then run the following basic DiD specification:

[43]This empirical approach is similar in spirit to Saez et al. (2019), 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).

[44]Firm survival is an outcome of interest in its own right. For instance, Weber and Zulehner (2014) show that the firm-level share of female workers correlates with firm survival rate. In my sample, I find that firms with a low pre-existing share of female workers are no more likely to survive (or die) than firms with a fairly low share of female workers.

$$log(y_{i,t}) = \beta \cdot 1(i \in LowShareFemale) \cdot 1(t \in Post) + \gamma_i + \delta_t + u_{i,t}, \tag{11}$$

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 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).[45]

In order to express the estimate in elasticity terms, I relate the differential change in the outcome variable of interest with the differential change in female employment generated by the payroll tax cut. Intuitively, the elasticity corresponds to the Wald estimator, that is the ratio of the reduced form estimate and the first-stage estimate obtained by regressing equation (11) and using the log of female employment as outcome variable. Following this logic, I employ the following two-stage least squares model:

$$log(y_{i,t}) = \epsilon \cdot \widehat{log(Fem_{i,t})} + \gamma_i + \delta_t + u_{i,t}, \tag{12}$$

where $log(Fem_{i,t})$ is the number of female workers (plus one when taking the log) instrumented by the interaction between a dummy for low female share firms and a dummy for the post-reform period, $1(i \in LowShareFemale) \cdot 1(t \in Post)$. ϵ computes the elasticity of the outcome variable of interest with respect to female employment, that is the percent change in the outcome variable of interest for a 1 percent (payroll tax cut-induced) change in female employment.

Table C9 provides summary statistics during the pre-reform period on the two groups of firms. Four points are worth noting. First, firms present 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 made lower profits over the pre-reform period. Third, the two groups of firms present some small differences in their use of part-time jobs (7.7 percent of the workforce in low share female vs 9 percent in fairly low share female) and in the share of temporary jobs (23.9 percent of the workforce vs 17.3 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.

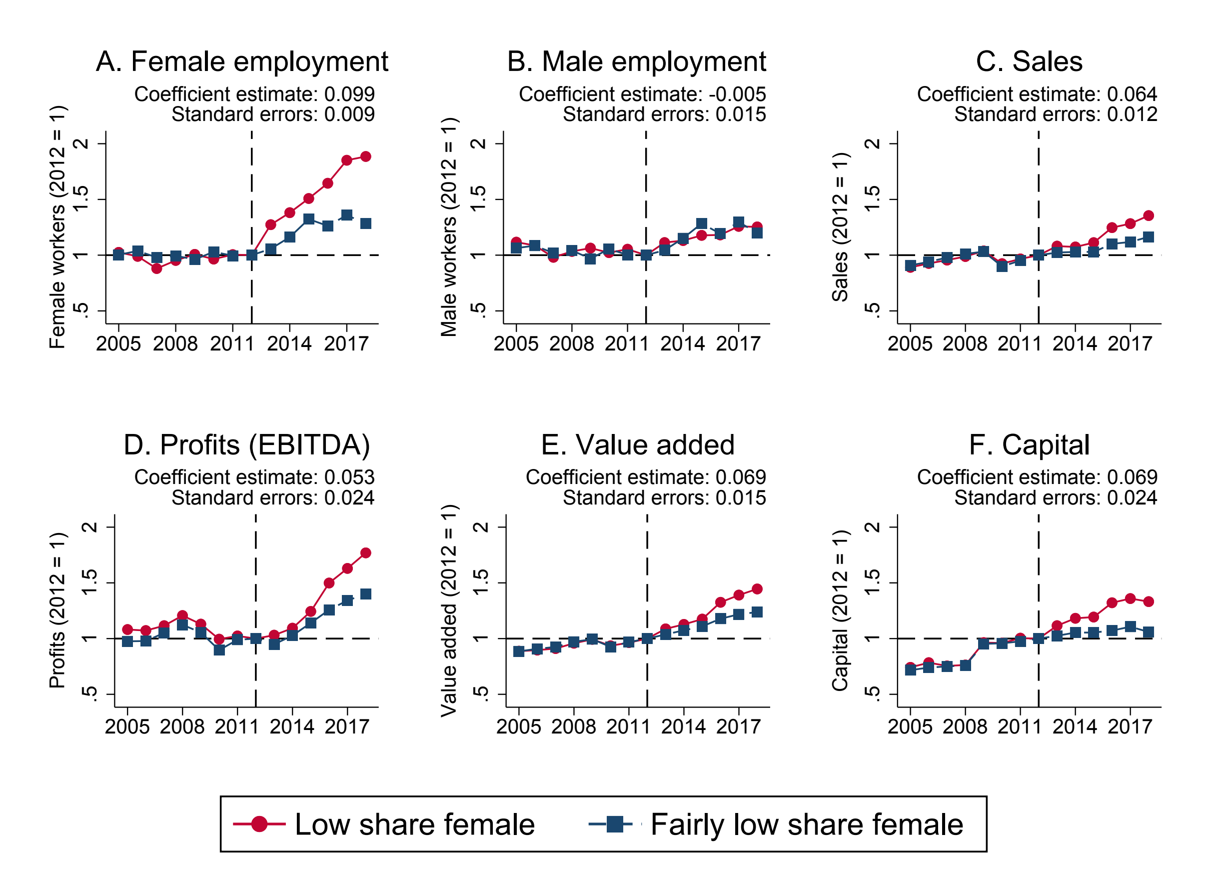
7.2 Results

I start by providing graphical evidence in Figure 12, which plots the time series of average outcomes for groups of firms from 2005 to 2018. For each group of firms,

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

I normalize outcomes relative to the year before the reform. In each panel, the figure traces out various longitudinal outcomes. Panels C-F present series in per-worker terms. 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, sales, profits (defined as earnings before interests, taxes, deduction and amortization), value added and capital. By contrast, the series on male employment present a parallel trend even after the reform, thus implying that employers did not substitute cheaper female workers with more expensive male workers.

Figure 12: The Impact of The Payroll Tax Cut on Per-Worker Firm-Level Outcomes



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.

The firm-level regression results, obtained from variants of equation (11) and (12), are presented in Table 4. 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) includes interactions between years and firm size (defined as those with employment below and above the median value). For each panel, I report the "reduced form" estimate, obtained from equation (11), in the first row, and the elasticity of the outcome variable with respect to the number of female workers, obtained from equation (12), in the second row.

The regression results confirm the graphical evidence. Firms with a lower pre-reform share of female workforce hired much more female workers compared to similar firms with a relatively larger pre-reform share. On average, I estimate a 9.9 percent growth in the number of female workers in low share female firms compared to fairly low share female firms. Consistent with the cross-municipality analysis (presented in section 5.1), I find that firms did not substitute female workers with male workers. Therefore, low share female firms grew in size by exploiting the lower labor costs of new female hires. The addition of female workers did significantly raise per-worker sales (by 6.4 percent), profits (5.3 percent), value added (6.9 percent) and capital (6.9 percent). Scaling up these effects by the average firm-level payroll tax-induced growth in female employment, these effects suggest an elasticity of per-worker sales, profits, value added, and capital with respect to female employment by 0.640, 0.530, 0.694, and 0.686, respectively. Accounting for any industry-, size-, and region-specific time-varying factors absorbs some of the identifying variations and reduces the coefficient estimates, but leaves the overall picture substantially similar.

7.3 Mechanisms

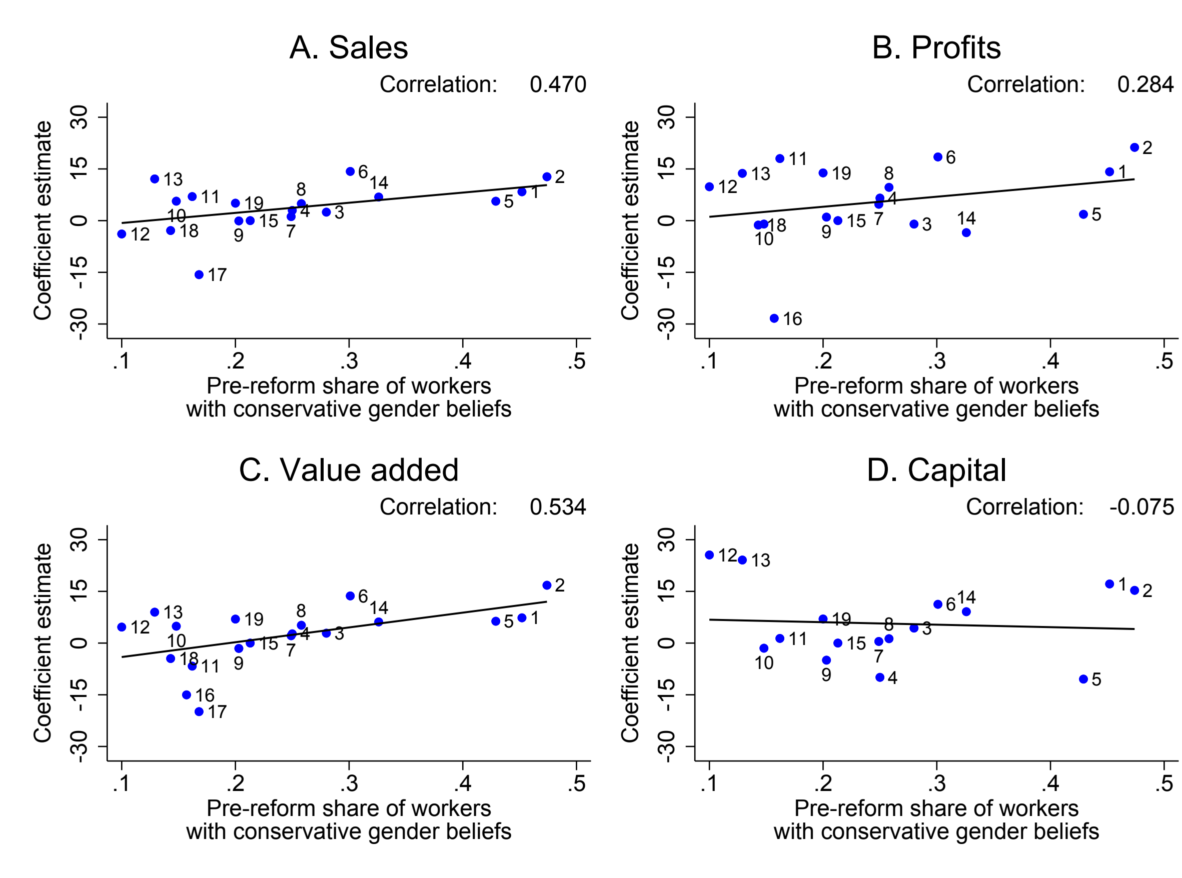
7.3.1 Breaking Down Gender Stereotypes

A few papers have shown that firms with more female employees earn higher profits and survive for longer (Hellerstein et al. 2002; Kawaguchi 2007; Weber and Zulehner 2014). A suitable explanation is that stereotypes cause beliefs distortion and led discriminatory firms to under-perform in the market. By breaking down gender stereotypes, the policy-induced increase in female employment can then improve business performance particularly more in firms that had more conservative beliefs.

Figure 13 tests this hypothesis by relating the industry-specific coefficient estimate obtained from equation (11) with pre-reform gender beliefs, proxied by the share of workers who agree with the statement "when jobs are scarce, men should have more right to a job than women." The figure shows a positive association between the reform-induced improvement in business performance and the pre-existing degree of conservative gender beliefs. The payroll tax cut led per-worker sales, profits and value added to grow more in industries with more conservative gender beliefs, while I do not find any significant pattern for capital. For instance, the addition of new female workers in stereotypically male industries such as agriculture and mining (marked as "1" and "2" in the figure) yields a relatively larger increase in sales, profits and value added compared to stereotypically female industries such as real estate and professionals.

To present these results more formally, I implement a triple difference approach, where I interact the post-reform dummy-treated group interaction with a dummy for

Figure 13: Comparing Firm-Level Outcomes By Industry-Level Gender Beliefs



Note: This figure relates the β coefficient estimate obtained from equation (11) with pre-reform gender beliefs, proxied by the share of workers who agree with the statement "when jobs are scarce, men should have more right to a job than women." Labels in the figure refers to the following industries (2-digit ateco groups): 1 = agriculture; 2 = mining; 3 = manufacturing; 4 = electricity, gas, and steam; 5= water supply; 6 = construction; 7 = wholesale and retail trade; 8 = transportation; 9 = accommodation and food; 10 = ICT; 11 = finance; 12 = real estate; 13 = professionals; 14 = administration; 15 = public sector; 16 = education; 17 = health; 18 = art.

industries where the average value of discrimination is above the median. Appendix Table C10 confirms this result. This result suggests that integration of female workers in male-dominated industries improves business performance particularly more in gender conservative industries.

7.3.2 Attracting High-Skill Female Workers

In this subsection, I investigate whether the improvement in business performance stems from the fact that the payroll tax cut allowed firms to attract high-skill female workers. To test this hypothesis, I first compute the share of high-skill female workers (proxied by those having at least a bachelor degree) for each firm over the pre- and post-reform period. Then, I create a dummy equal to 1 whether the share in post-reform period is larger than the share in the pre-reform period. The dummy will thus capture *upskilling*: an increase in the share of high-skill female workers.

Appendix Table C11 displays coefficient estimate by augmenting equation (11) with the triple interaction between the post-reform dummy, the treated group dummy, and the dummy for firms experiencing upskilling. The coefficient estimate is not statistically significant when looking at sales, value added and capital per-workers, while there is a statistically significant 4.7 percent increase in profits. This implies that the tax cut make firms that were able to attract high-skill female workers more profitable. Regarding the other financial outcomes, this result suggests that the payroll tax cut does not make firms more able to grow by attracting high-skill female workers. Rather, it is the mere fact of hiring female workers that improved performance, regardless of their skill level.

One explanation for this result is that the payroll tax cut is not the best instrument for attracting high-skill workers. There are two main reasons. First, as the incidence is mostly on firms, there are small, if any, wage incentives for attracting workers. Second, the pool of potential eligible workers is mostly composed of low-skill workers that are unemployed at the time of hiring. This implies that payroll tax-induced upskilling was limited by definition.

7.3.3 Liquidity effects

Another potential explanation 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.[46]

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 (11) 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 C12 shows that the positive effect measured on all the outcome variables of interest are mostly concentrated among firms that were liquidity constrained before the implementation of the payroll tax cut. This finding relates with Saez et al. (2019) amd Benzarti and Harju (2021a), who show that payroll tax cuts make firms more resilient during downturns by relaxing liquidity constraints.

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

Table 4: The Impact of The Payroll Tax Cut on Per-Worker Firm-Level Outcomes

	(1)	(2)	(3)	(4)
	A. *Outcome*: log of female workers			
$1(i \in Low) \cdot 1(t \in Post)$	0.099***	0.100***	0.118***	0.119***
	(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)
$\widehat{log(Fem_{i,t})}$	-0.089	-0.126	-0.133	-0.143
	(0.055)	(0.089)	(0.094)	(0.095)
	C. *Outcome*: log of sales per-worker			
$1(i \in Low) \cdot 1(t \in Post)$	0.064***	0.054***	0.050***	0.048***
	(0.012)	(0.012)	(0.013)	(0.012)
$\widehat{log(Fem_{i,t})}$	0.640***	0.545***	0.427***	0.403***
	(0.118)	(0.117)	(0.097)	(0.093)
	D. *Outcome*: log of profits (EBITDA) per-worker			
$1(i \in Low) \cdot 1(t \in Post)$	0.053**	0.038*	0.038*	0.036*
	(0.024)	(0.021)	(0.021)	(0.021)
$\widehat{log(Fem_{i,t})}$	0.530**	0.383*	0.321*	0.301*
	(0.230)	(0.210)	(0.171)	(0.168)
	E. *Outcome*: log of value-added per-worker			
$1(i \in Low) \cdot 1(t \in Post)$	0.069***	0.057***	0.048***	0.046***
	(0.015)	(0.015)	(0.013)	(0.013)
$\widehat{log(Fem_{i,t})}$	0.694***	0.574***	0.409***	0.388***
	(0.154)	(0.144)	(0.102)	(0.099)
	F. *Outcome*: log of capital per-worker			
$1(i \in Low) \cdot 1(t \in Post)$	0.069***	0.058***	0.036**	0.034**
	(0.024)	(0.022)	(0.017)	(0.017)
$\widehat{log(Fem_{i,t})}$	0.697***	0.587***	0.307**	0.284**
	(0.252)	(0.218)	(0.146)	(0.143)
Observations	364,380	364,380	364,380	364,380
# of firms	57,490	57,490	57,490	57,490
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 (11) and (12), displayed in first and second row of each panel, respectively. 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.

8 Conclusion

The labor market participation of women has spectacularly increased over the recent decades. However, women participation rates are still systematically lower than those of men. One key question is whether governments have the power to curb gender gaps in labor market outcomes though policy. This paper studies whether a payroll tax cut for new female hires tackle gender inequalities in the Italian labor market.

Starting from 2013, the payroll tax rate is reduced by 50 percent for a period of up to 12 for temporary jobs; 18 month for permanent jobs. I use matched employer-employee data on the universe of private sector workers to study the effect of the payroll tax cut on several labor market outcomes, including wages, labor force participation, tenure and unemployment insurance benefits duration.

I provide three main results. First, I find that employer-specific payroll tax cuts are sticky. Under such nonstandard tax incidence result, reducing employer payroll taxes increase employment, while not affecting net wages. In particular, the preferential tax scheme promotes integration of women into traditionally gender segregated places and occupations, without crowding out male employment. This result implies that employer-specific payroll tax cuts are a successful outcome for curbing the gender *employment* gap even in contexts where gender attitudes are still traditional, but it is an undesirable outcome if policy-makers want to reduce the gender *wage* gap.

Second, despite the time-limited nature of the preferential tax scheme creates a substantial notch in the budget constraint of employers, I find small, although statistically significant, bunching in job duration. This result suggests that outsiders workers are only imperfect substitutes for insiders. This might reflect both frictions, such as search or firing costs, or that the firm's production process relies on specific human capital that affect firms' demand for incumbents.

Finally, I show that the payroll tax cut improves firms' performance and profitability by promoting integration of female workers in traditionally male-dominated firms. This result suggests that governments have the power to curb gender discrimination and spur business profitability by "taxing" gender prejudiced employers.

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Appendices

A Payroll Tax Cut on Female Hires (Law 92/2012)

The legislator defines occupations following the standard International Standard Classification of Occupations (ISCO). Specifically, occupations are grouped in 37 2-digit ISCO group, which is the so called "sub-major" group classification. Every year, occupation-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 occupations, 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 occupations. Year-specific gender employment gap statistics refer to the following sources:

- 2011 and 2012: *nota prot. 43956*, published by the Italian Institute of Statistics (ISTAT) on the 26th of June 2013, and by the Ministry of Labor on the 2nd of September 2013.
- 2013: *nota prot. 23128*, published by the Italian Institute of Statistics (ISTAT) on the 26th of November 2014, and by the Ministry of Labor on the 22nd of December 2014.
- 2014: *nota prot. 14869*, published by the Italian Institute of Statistics (ISTAT) on the 26th of August 2015, and by the Ministry of Labor on the 13th of October 2015.
- 2015: *nota prot. 17604*, published by the Italian Institute of Statistics (ISTAT) on the 26th of September 2016, and by the Ministry of Labor on the 27th of October 2016.
- 2016: *nota prot. 983853/17*, published by the Italian Institute of Statistics (ISTAT) on the 2nd of October 2017, and by the Ministry of Labor on the 10th of November 2017.
- 2017: *nota prot. 1468268/18*, published by the Italian Institute of Statistics (ISTAT) on the 9th of October 2018, and by the Ministry of Labor on the 28th of November 2018.
- 2018: *nota prot. 2769966/19*, published by the Italian Institute of Statistics (ISTAT)

on the 18th of October 2019, and by the Ministry of Labor on the 25th of November 2019.

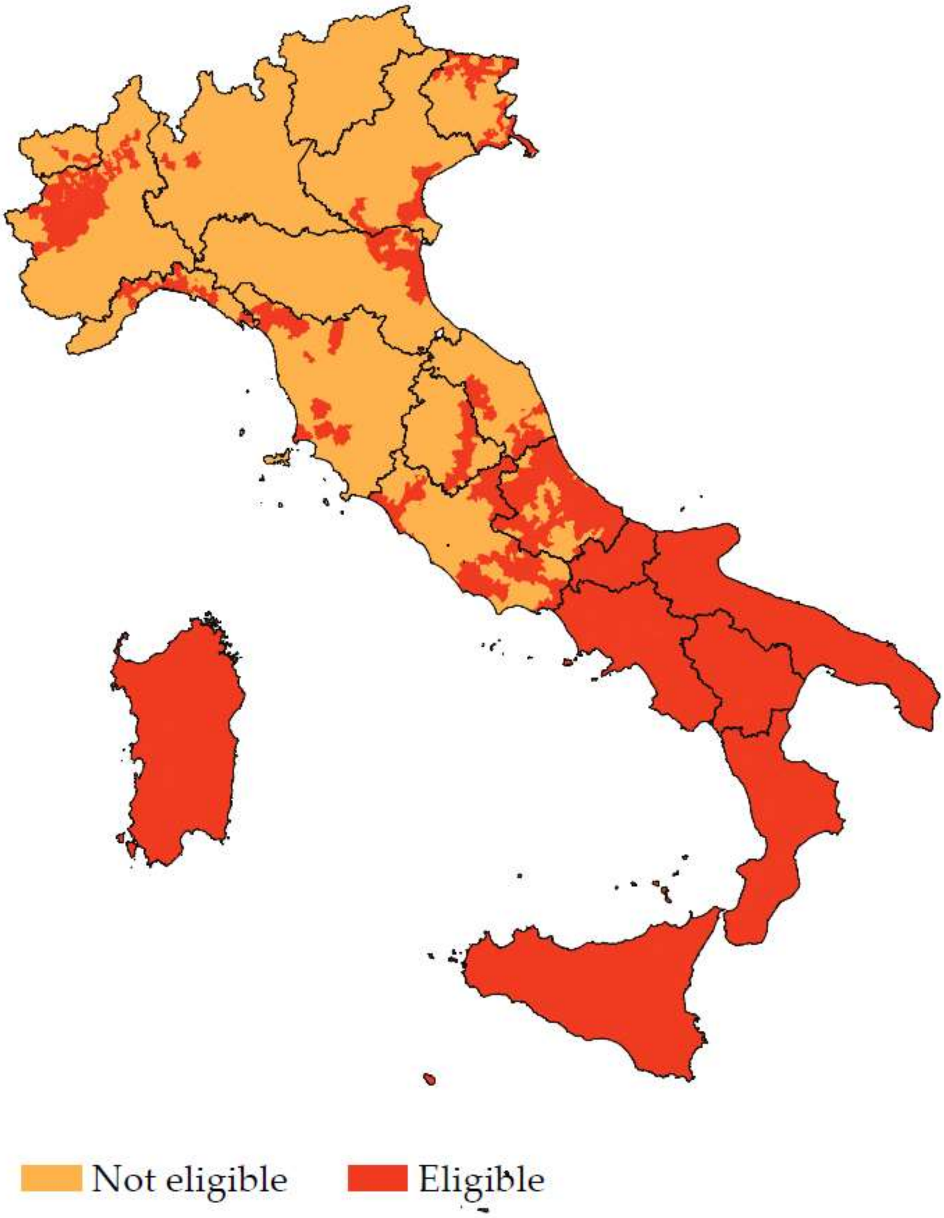
- 2019: *nota prot.* by the Italian Institute of Statistics (ISTAT) on the 29th of September 2020, and published by the Ministry of Labor on the 16th of October 2020.

Table A1: Gender employment gap by occupations

CP2011	Occupation	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 occupations (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. Occupations 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 Payroll Tax Incidence Analysis on Young New Hires

The payroll tax incidence analysis, presented in section 5, focus on the sample of female hires that worked before. This choice was dictated by the empirical approach: gross and net wages earned during the previous job at each month of tenure are subtracted by wages earned during the payroll tax cut eligible job. This selection has one main weakness: it rules out workers that entered for the first time in the labor market and were eligible for the payroll tax cut.

In this Appendix, I study payroll tax incidence by focusing exclusively on young workers entering for the first time in the labor market. I select workers (of both sex) younger than 35 and without any previous job history in social security archives over the 2005-2019 period. This selection gives me a sample of 7,404,543 individuals.

I run a difference-in-difference analysis comparing men vs women's wages, before and after the introduction of the payroll tax cut for new female hires. Specifically, I estimate the β coefficient from the following equation:

$$log(y_{i,t}) = \beta \cdot 1(i \in Female) \cdot 1(t \in Post) + \gamma_{s(i)} + \delta_t + u_{i,t}, \tag{1}$$

where $y_{i,t}$ are daily wages (gross or net of the employer-portion of the payroll tax rate) of worker i at year t. $beta$ is the coefficient of interest: it measures the percent change in wages earned by female workers during the post-reform period. I also account for female worker fixed effects, $\gamma_s(i)$, to account for any (permanent) difference in wages by sex (that is, the gender pay gap at career starting). δ_t include year dummies. $u_{i,t}$ are firm-level clustered standard errors.

The β coefficient estimates are presented in Table B1. I start from a simple model with sex and year fixed effects. Column (2) includes municipality-year fixed effects, that allow to account for any local labor market shocks and policies. In column (3), I add firm-year fixed effects, so to exploit within-firm variation and to control for any firm-specific economic shocks. Finally, columns (4) and (5) interact job type and contract characteristics (i.e., permanent vs full time contract; full-time vs part-time job) with year fixed effects.

This analysis presents results in line with the baseline results presented in Table 2. I find that at least 92 percent of the payroll tax cut remains in the firm. This implies that new female hires over the post-reform period enjoy a 0.7 percent increase in net wages relative to male hires.

Table B1: Payroll tax incidence on new hires

	(1)	(2)	(3)	(4)	(5)
			A. *Outcome*: Gross wage		
$1(i \in Female)$	-0.095***	-0.097***	-0.090***	-0.085***	-0.085***
$\times 1(t \in Post)$	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
			B. *Outcome*: Net wage		
$1(i \in Female)$	-0.003	-0.005	0.002	0.007***	0.007***
$\times 1(t \in Post)$	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
Observations	7,404,543	7,404,543	7,404,543	7,404,543	7,404,543
# of firms	919,753	919,753	919,753	919,753	919,753
Sex FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Municipality-year FE	No	Yes	Yes	Yes	Yes
Firm-year FE	No	No	Yes	Yes	Yes
Job contract-year FE	No	No	No	Yes	Yes
Job type-year FE	No	No	No	Yes	Yes
Job contract-sex FE	No	No	No	No	Yes
Job type-sex FE	No	No	No	No	Yes
Pass-through to firms	1.033	1.054	0.978	0.924	0.924

Note: This table presents the β coefficient obtained from equation 1 and standard errors clustered at firm-level. I start from a simple model with sex and year fixed effects. Column (2) includes municipality-year fixed effects, that allow to account for any local labor market shocks and policies. In column (3), I add firm-year fixed effects, so to exploit within-firm variation and to control for any firm-specific economic shocks. Finally, columns (4) and (5) interact job type and contract characteristics (i.e., permanent vs full time contract; full-time vs part-time job) with year fixed effects. Pass-through to firms (shown in the last row of the table) is defined as the fraction of payroll tax that benefit the firm. The sample includes all the workers younger than 35 entering for the first time in the labor market over the 2005-2019 period.

C Additional Tables and Figures

Table C1: Summary statistics, employees

	# of women (1)	Mean (2)	SD (3)	Min (4)	Max (5)
Daily (full-time equivalent) wage (euros)	218,768	84.609	218.536	40.944	317.460
Eligible municipality (0/1)	218,768	0.585	0.493	0	1
Commuter (0/1)	218,768	0.433	0.492	0	1
Age	218,768	38.180	11.611	18	65
Age 18-29 (%)	218,768	0.280	0.446	0	1
Age 30-39 (%)	218,768	0.265	0.434	0	1
Age 40-49 (%)	218,768	0.242	0.421	0	1
Age 50-65 (%)	218,768	0.213	0.406	0	1
Blue collar (0/1)	218,768	0.613	0.485	0	1
White collar (0/1)	218,768	0.385	0.484	0	1
Manager (0/1)	218,768	0.000	0.006	0	1
Other workers (0/1)	218,768	0.002	0.040	0	1
Permanent jobs (0/1)	218,768	0.275	0.429	0	1
Temporary jobs (0/1)	218,768	0.671	0.451	0	1
Seasonal jobs (0/1)	218,768	0.054	0.221	0	1
Full-time jobs (0/1)	218,768	0.322	0.457	0	1
Part-time jobs (0/1)	218,768	0.568	0.485	0	1
Other jobs (0/1)	218,768	0.110	0.305	0	1

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

Table C2: 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 C3: 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				
$1(t \leq 18)$	-0.077***	-0.065***	-0.082***	-0.164***	-0.112
$\times\ 1(j \in Eligible)$	(0.013)	(0.011)	(0.015)	(0.042)	(0.200)
	B. *Outcome*: Monthly net wage				
$1(t \leq 18)$	0.005	0.018	-0.001	-0.082	-0.029
$\times\ 1(j \in Eligible)$	(0.013)	(0.011)	(0.015)	(0.052)	(0.200)
Observations	20,266	50,615	25,177	1,056	422
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 C4: Empirical approaches to identify employment effects

Sec.	Empirical strategy	Sample	Outcome variable	Identifying variation
5.1	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
5.2	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
5.3	RD	Women in municipalities not eligible for EU structural fund	Share of female workers in a occupation $\times$ year cell	Cross-occupation differential exposure to the payroll tax cut based on gender employment gap
5.4	DiD	Full sample	Employment status (0/1)	Cross-individual variation in eligibility in a given municipality $\times$ month $\times$ occupation $\times$ cohort cell

Note: This table summarizes the empirical approaches proposed to identify employment effects of the payroll tax cut. The first approach is an event study design resting on the differential exposure to the payroll tax cut across municipalities. The second strategy is a DiD approach comparing employment growth for women older than 50 with those younger, where the minimum non-employment status requirement reduces by 12 months in municipalities not eligible for EU structural fund. The third approach exploits the cutoff-rule defining occupations where the minimum non-employment duration reduces by 6 months. In the final approach, I perform a micro-level analysis by relating labor force participation with payroll tax cut eligibility in a given municipality-month-occupation-cohort cell, before and after the introduction of the payroll tax cut.

Table C5: 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 C6: Cross-occupation 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	-
Occupation 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 an occupation; in column (4) is the share of female worker in an occupation. Column (1) presents the baseline RD estimate depicted in Figure 8. In column (2) and (3), I further control for second- and third-order polynomials, respectively. Column (4) reports RD estimate including occupation and year fixed effects, thus exploiting within-occupation over-time variation in treatment eligibility.

Table C7: The Impact of the Payroll Tax Cut on Job Duration

μ_L	Count of job terminations (1)	Share of job terminations (x100) (2)
	A. Not eligible jobs	
From Chetty et al. (2011)	504	0.428***
		(0.193)
	B. Eligible jobs	
From Chetty et al. (2011)	1,688	1.311***
		(0.190)
	C. Difference-in-bunching	
0	437.270***	0.941***
	(1.254)	(0.004)
$[-7;0]$	128.250***	0.217***
	(43.525)	(0.099)
$[-14;0]$	81.237***	0.125***
	(27.588)	(0.060)

Note: This table reports bunching (panel A and B) and difference-in-bunching (panel C) coefficient estimate and bootstrapped standard errors, obtained regressing equations (9) and (10). Column (1) in panel A and B displays the coefficient estimate relative to job termination counts, while panel C reports the coefficient estimate relative to the difference in job termination counts between eligible and non eligible jobs. Column (2) reports the same estimate scaled by the total number of jobs in each period. The coefficient in columns 2 are multiplied by 100. In panel A and B, the excluded region $[m_L - m_U]$ is computed using the stata program "*bunch count*", written by Chetty et al. (2011). I use a 7th degree polynomial to compute counterfactual bins. In panel C, I use three definitions of the bunched region, μ_l: the area just at the cutoff (0); the area covering 1 week before meeting the cutoff ($[7-0]$); and the area covering 2 weeks before meeting the cutoff ($[14-0]$). See Figure 10 for a graphical representation and section 6 for details on the estimation strategy.

Table C8: The Heterogeneous Impact of the Payroll Tax Cut on Job Duration

μ_L	Difference in job terminations (1)	Share of jobs (x100) (2)	Difference in job terminations (3)	Share of jobs (x100) (4)
	A. By worker			
	i. White collar		ii. Blue collar	
0	98.883***	0.214***	229.885***	0.919***
	(0.561)	(0.001)	(0.571)	(0.002)
[7 – 0]	36.201***	0.078***	66.736***	0.267***
	(9.238)	(0.020)	(26.201)	(0.105)
[14 – 0]	25.899***	0.056***	39.457***	0.158**
	(5.800)	(0.013)	(16.554)	(0.066)
	B. By job			
	i. Part-time		ii. Full-time	
0	200.383***	0.534***	69.340***	0.358***
	(0.530)	(0.001)	(0.417)	(0.002)
[7 – 0]	62.887***	0.168***	21.638**	0.112**
	(17.970)	(0.048)	(8.415)	(0.043)
[14 – 0]	39.776***	0.106***	13.563***	0.070***
	(12.741)	(0.034)	(4.801)	(0.025)
	C. By municipality			
	i. Non eligible		ii. Eligible	
0	176.495***	0.500***	260.775***	0.509***
	(0.686)	(0.002)	(0.782)	(0.002)
[7 – 0]	54.552***	0.154***	73.697***	0.144**
	(19.430)	(0.055)	(30.739)	(0.060)
[14 – 0]	31.521***	0.089**	49.715***	0.097***
	(12.211)	(0.035)	(16.230)	(0.032)
	D. By age (in non eligible municipalities)			
	i. 45-49		ii. 50-54	
0	46.467***	0.486***	99.195***	0.974***
	(0.200)	(0.002)	(0.213)	(0.002)
[7 – 0]	14.710***	0.154***	25.748***	0.253**
	(6.114)	(0.064)	(10.916)	(0.107)
[14 – 0]	9.492***	0.099***	15.962***	0.157***
	(3.606)	(0.037)	(6.072)	(0.060)

Note: This table reports excess bunching estimates for four different subgroups of the population of payroll tax cut's recipients: a. white collar vs blue collar workers; b. part-time vs full-time jobs; c. workers resident in municipalities eligible for less binding eligibility criteria vs non eligible municipalities; d. workers age 45-49 vs 50-54 resident in non eligible municipalities. Coefficient estimate are obtained using a difference-in-bunching strategy, obtained by subtracting the count of not eligible jobs in each 1-day bin from the count of eligible jobs. For each group, the table reports the coefficient estimate relative to the difference in the count of job terminations (columns 1 and 3) between eligible and non eligible jobs, and the same estimate scaled by the total number of jobs in each group (columns 2 and 4). The coefficient in columns 2 and 4 are multiplied by 100. I use three definition of the bunched regions, μ_l, the area just at the cutoff (0), the area covering 1 week before meeting the cutoff ($[7-0]$), and the area covering 2 weeks before meeting the cutoff ($[14-0]$). Bootstrapped standard errors in parentheses. See Figure 11 for a graphical representation and section 6 for details on the estimation strategy.

Table C9: Summary statistics by firm

	Bottom quintile		Next quintile	
	# of firms	Mean	# of firms	Mean
	(1)	(2)	(3)	(4)
Female workers (#)	35,146	1.790	31,790	6.524
Male workers (#)	35,146	39.850	31,790	37.662
Total workers (#)	35,146	41.640	31,790	44.187
Sales (per-worker 1,000 euros)	35,146	130.700	32,356	221.330
Profits (EBITDA) (per-worker 1,000 euros)	35,146	11.481	31,790	17.517
Value added (per-worker 1,000 euros)	35,146	36.904	31,790	49.515
Capital (per-worker 1,000 euros)	35,146	40.275	31,790	63.401
Earnings (per-worker, 1,000 euros)	35,146	15.142	31,790	19.965
Temporary jobs (0/1)	35,146	0.239	31,790	0.173
Permanent jobs (0/1)	35,146	0.807	31,790	0.860
Full-time jobs (0/1)	35,146	0.935	31,790	0.922
Part-time jobs (0/1)	35,146	0.077	31,790	0.090
Subsidiary firm (0/1)	35,146	0.056	31,790	0.058
Parent company (0/1)	35,146	0.105	31,790	0.111
Single member company (0/1)	35,146	0.839	31,790	0.832
Eligible municipality (0/1)	35,146	0.517	31,790	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 C10: Heterogeneity by Industry-Level Gender Beliefs

	Outcome variable: log of per-worker			
	sales	profits	value added	capital
	(1)	(2)	(3)	(4)
$1(i \in Low) \cdot 1(t \in Post)$	0.053***	0.042*	0.043***	0.002
	(0.013)	(0.023)	(0.013)	(0.023)
$... \cdot 1(i \in Disc)$	0.032***	0.030***	0.076***	0.013
	(0.012)	(0.014)	(0.020)	(0.020)
Observations	364,380	364,380	364,380	364,380
# of firms	57,490	57,490	57,490	57,490
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 (11) by the interaction between the post-reform treatment group interactions and a dummy for firms operating in an industry where the share of workers with conservative gender beliefs are above the median. Gender beliefs are proxied by the share of workers who agree with the statement "when jobs are scarce, men should have more right to a job than women." 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.

Table C11: Heterogeneity by Skill Upgrading

	Outcome variable: log of per-worker			
	sales	profits	value added	capital
	(1)	(2)	(3)	(4)
$1(i \in Low) \cdot 1(t \in Post)$	0.062***	0.041	0.070***	0.073***
	(0.013)	(0.025)	(0.016)	(0.024)
$... \cdot 1(\Delta_i > 0)$	0.005	0.047***	-0.007	-0.020
	(0.008)	(0.014)	(0.009)	(0.013)
Observations	364,380	364,380	364,380	364,380
# of firms	57,490	57,490	57,490	57,490
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 (11) by the interaction between the post-reform treatment group interactions and a dummy for firms where the post-reform share of high-skill workers is larger than the pre-reform share. 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.

Table C12: The role of liquidity constraints

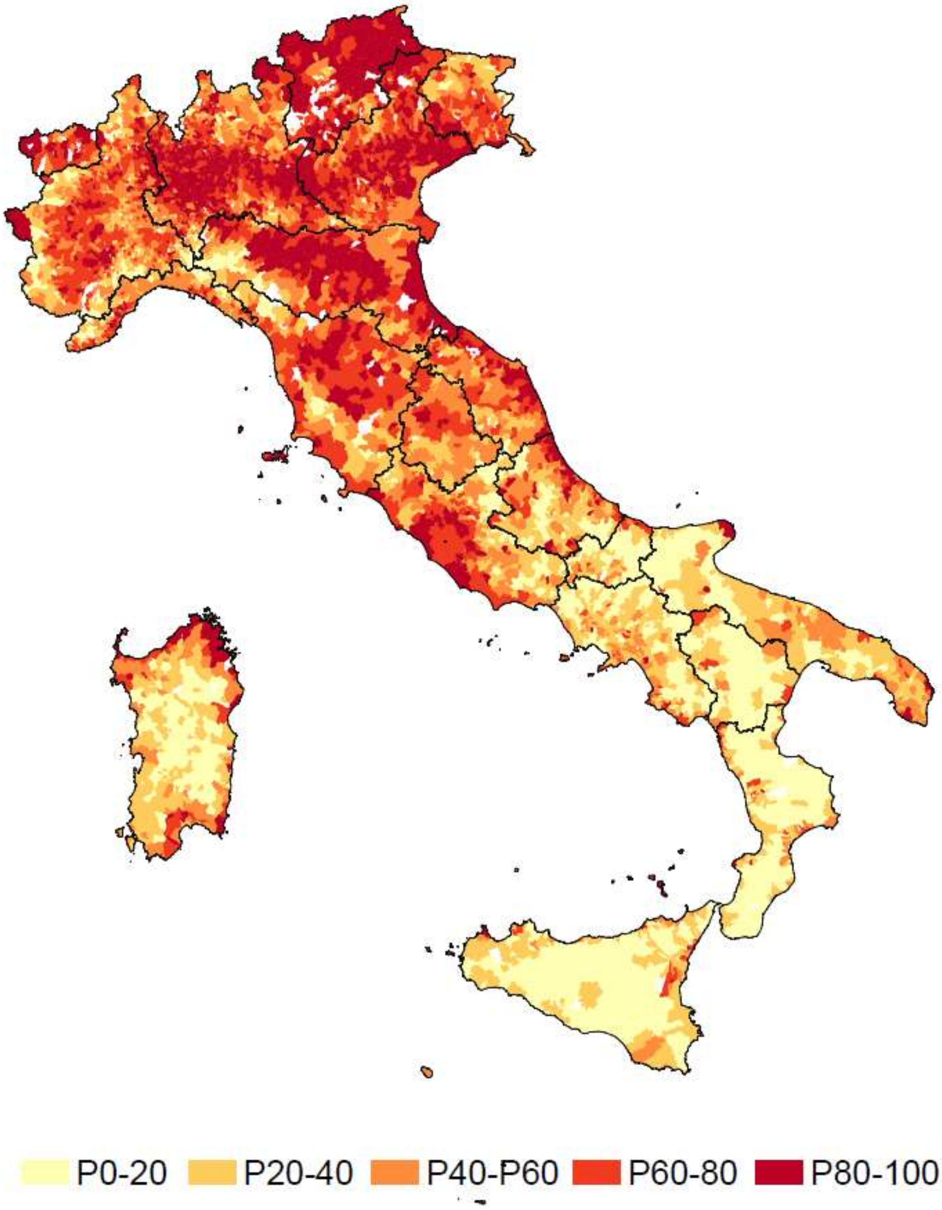
	Outcome variable: log of per-worker			
	sales	profits	value added	capital
	(1)	(2)	(3)	(4)
	A. *Proxy for liquidity constraints*: share of liquid assets			
$1(i \in Low) \cdot 1(t \in Post)$	-0.011	-0.031*	-0.003	0.013
	(0.007)	(0.017)	(0.012)	(0.020)
$... \cdot 1(i \in Const)$	0.150***	0.169***	0.145***	0.113***
	(0.013)	(0.021)	(0.011)	(0.016)
	B. *Proxy for liquidity constraints*: sales			
$1(i \in Low) \cdot 1(t \in Post)$	-0.014*	-0.017	-0.001	0.017
	(0.007)	(0.026)	(0.012)	(0.018)
$... \cdot 1(i \in Const)$	0.148***	0.132***	0.133***	0.099***
	(0.011)	(0.023)	(0.011)	(0.025)
Observations	364,380	364,380	364,380	364,380
# of firms	57,490	57,490	57,490	57,490
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 (11) 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 C1: Gender employment gap, Italy versus the US

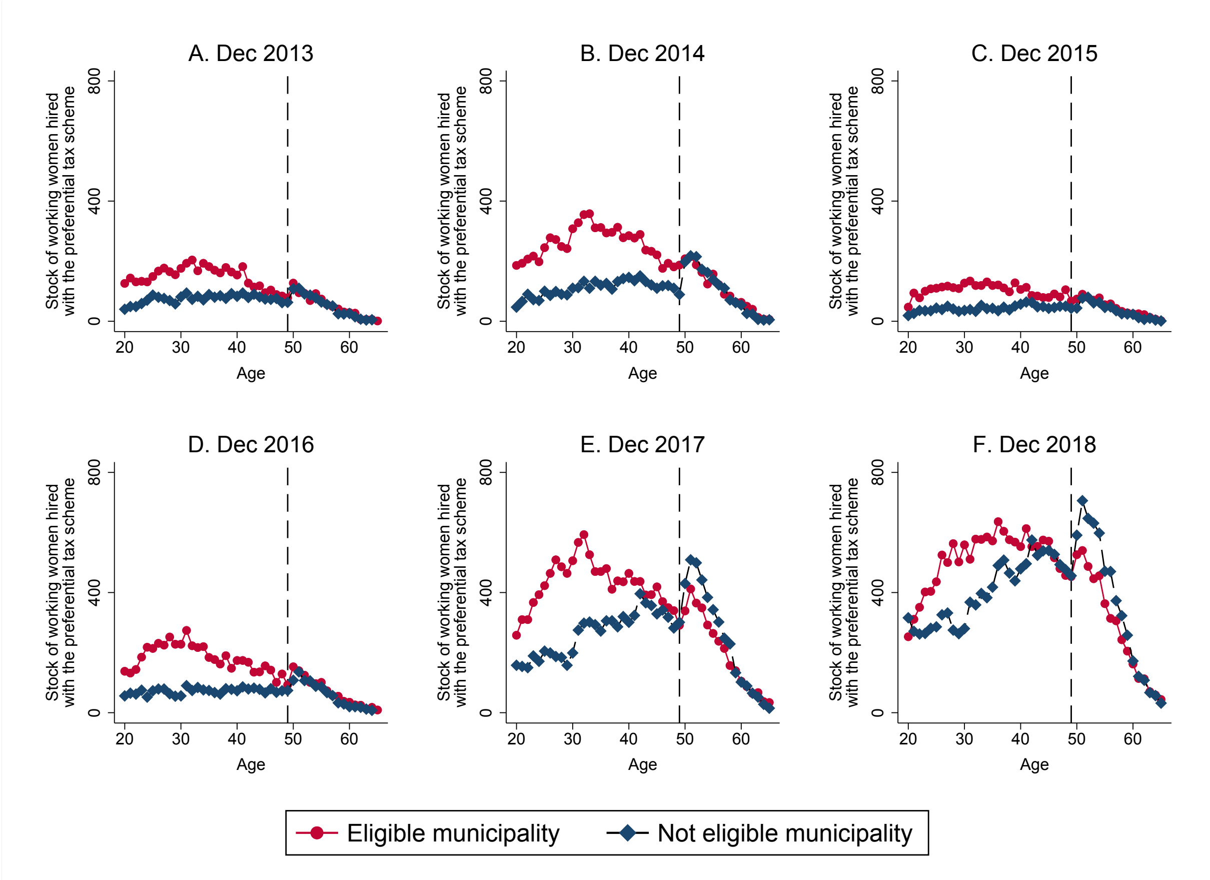
Note: This figure compares the occupation-specific share of female workers in Italy (vertical axis) and in the US (horizontal axis). Italian estimates are collected from documents published by the Ministry of Labor, relying on labor force survey data (see Appendix A for details). Occupations are identified by the International Standard Classification of Occupations (ISCO) sub-major group. US series are from the U.S. Census Bureau, 2019 American Community Survey, which provides information on the number of women and men full-time workers and their annual earnings in over 300 occupations. The estimates are limited to occupations with at least 100 observations. The choice of how to link US-Italy occupation classification has been dictated by the denomination of each occupation (using a semantic criterion). Detailed information on this link is available upon request. Female employment share estimates refer to the latest available data (2018 for Italy and 2019 for the US).

Figure C2: Share of female workers, pre-reform period



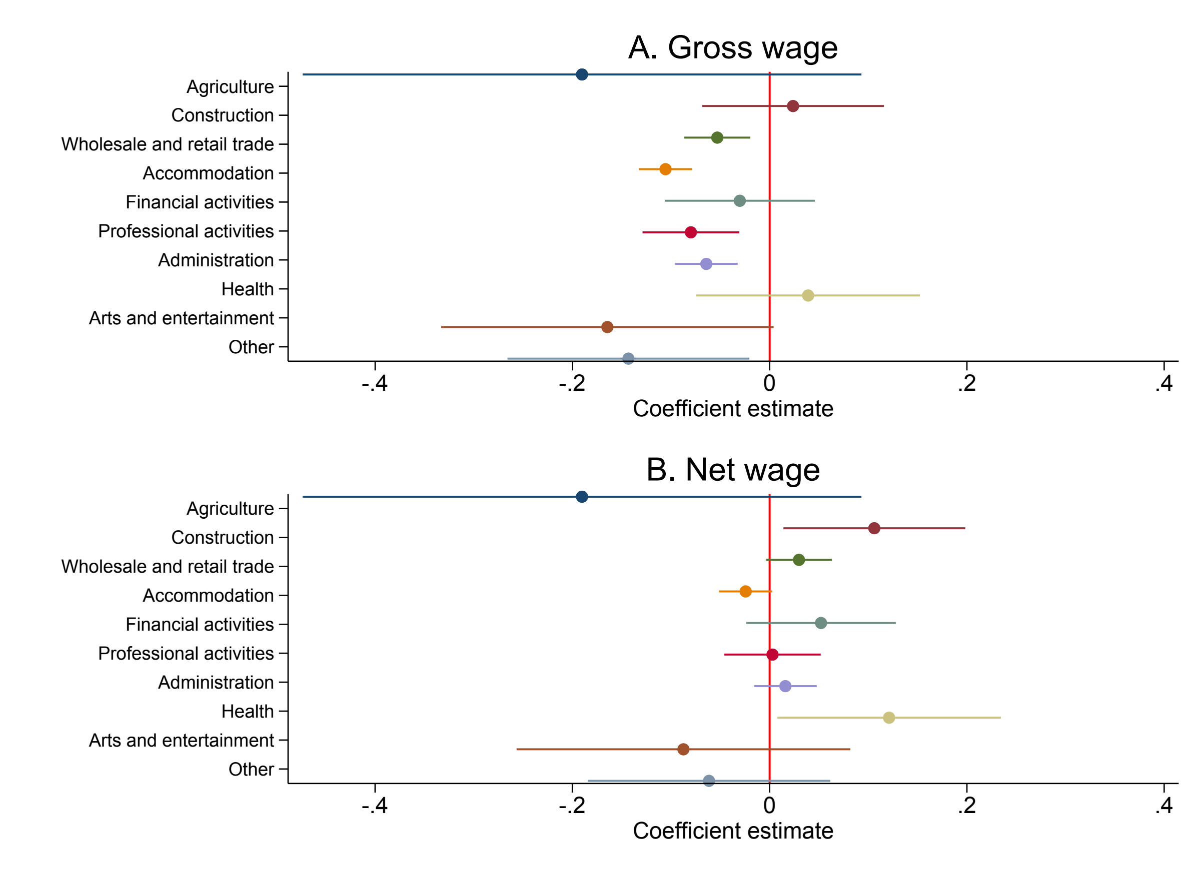
Note: This graph depicts the female employment share (in quintiles) over the period before the Fornero reform (2005-2012). The areas (municipalities) in red (yellow) are those where female employment is larger (lower). Black lines refer to regional boundaries.

Figure C3: Take-up rate by age and municipality over time



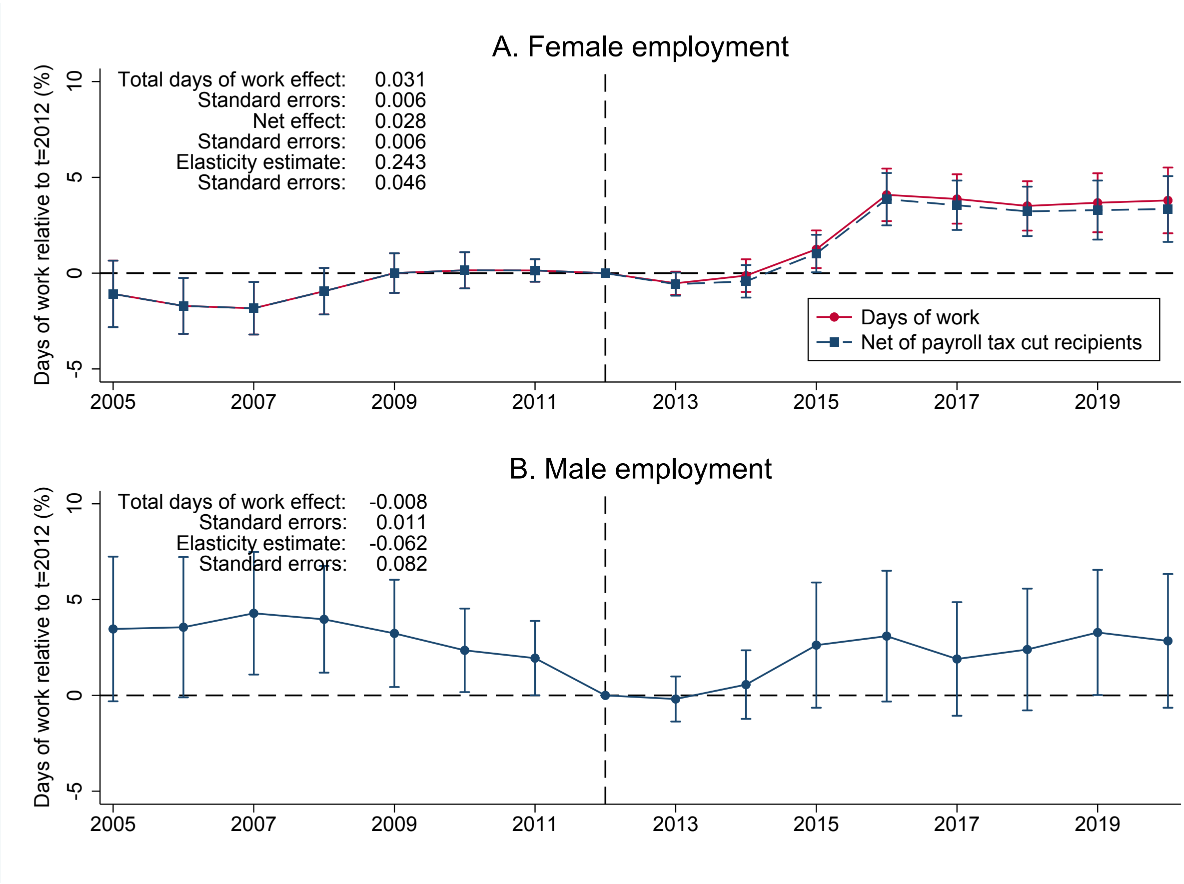
Note: The figure shows the stock of new female hires employed with the preferential payroll tax scheme (vertical axis) and the age of the recipient (horizontal axis) at the end of each year over the 2013-2019 period. For each panel, the figure reports separate series relative to eligible municipalities (red circles) and not eligible municipalities (blue squares).

Figure C4: Payroll tax incidence heterogeneity by industry



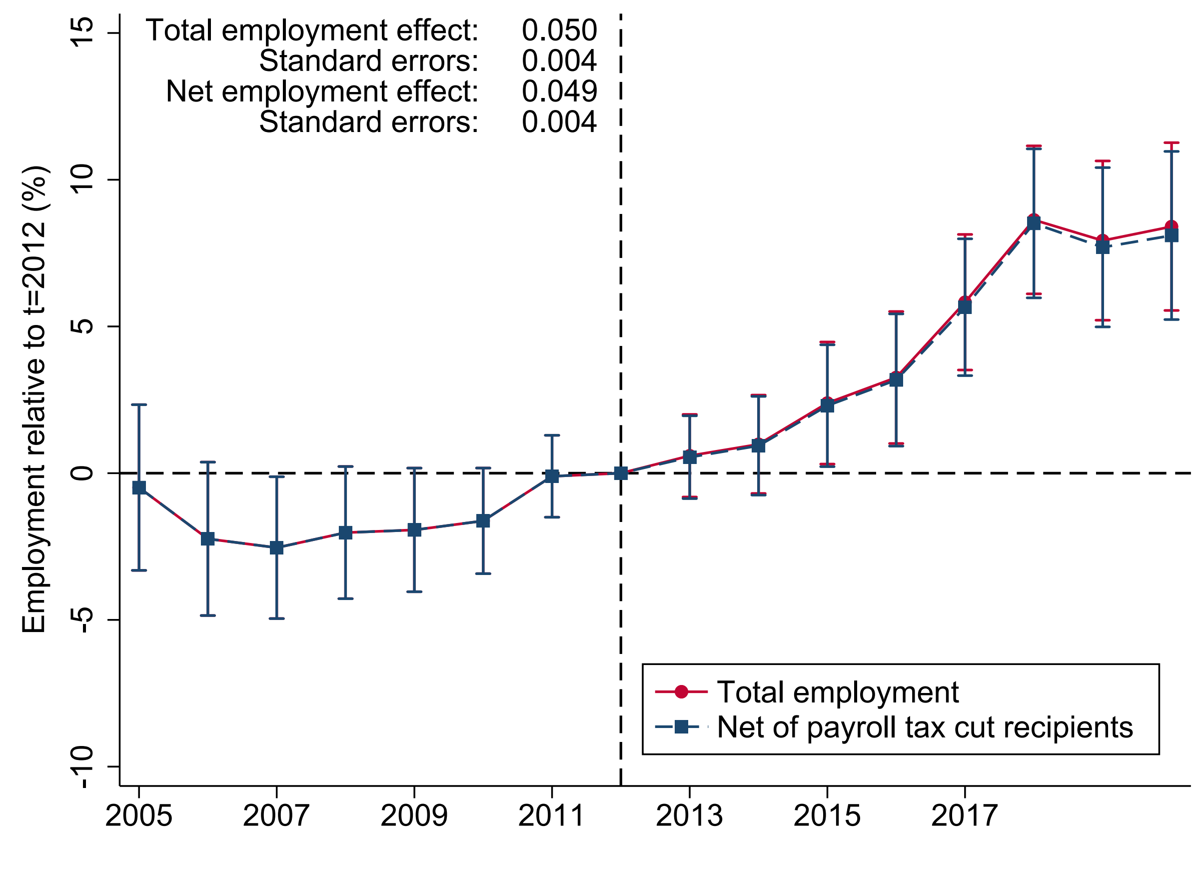
Note: The figure reports coefficient estimate and standard errors on the effect of the payroll tax cut on gross wages (top panel) and net wages (bottom panel) obtained regressing equation (1) for sub-samples of industries.

Figure C5: The impact of the payroll tax cut on days of work



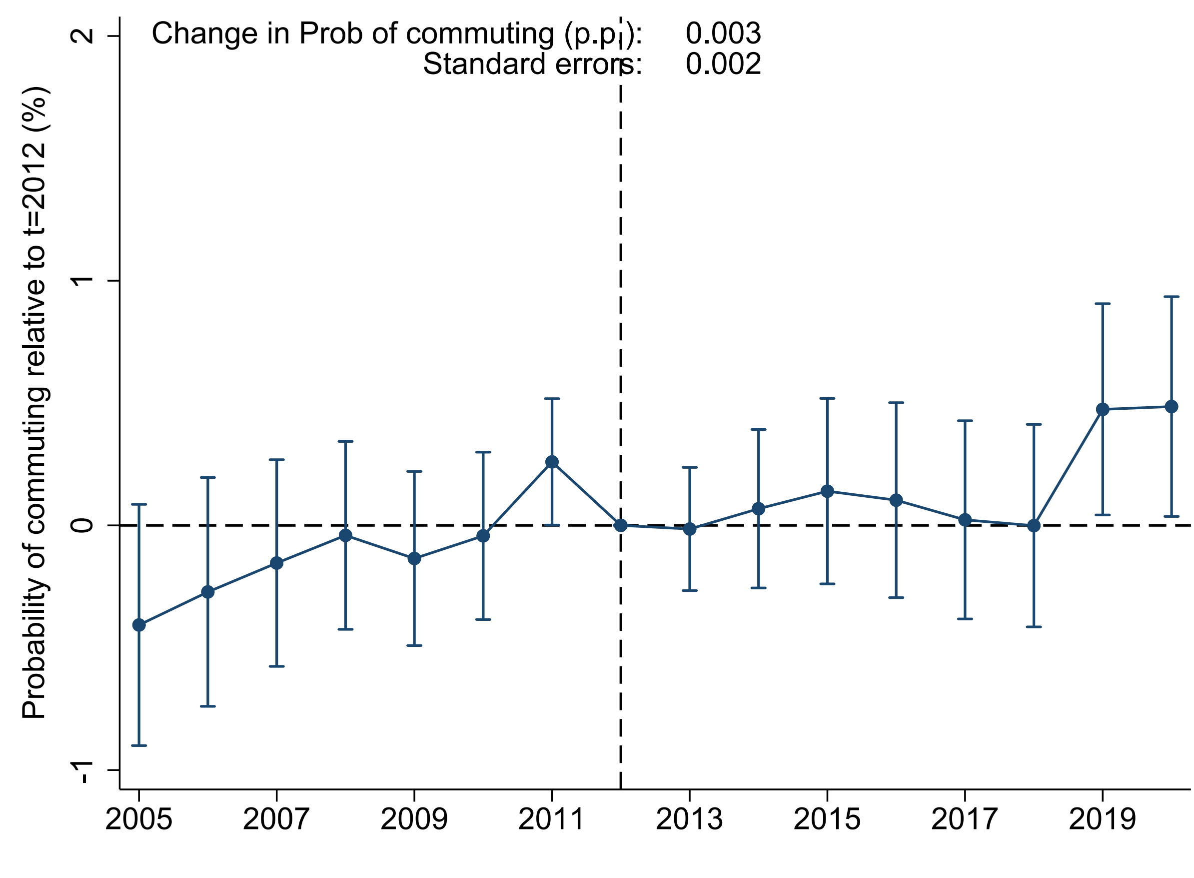
Note: The figure depicts the impact of payroll tax cut on days of work, aggregated by municipality and year. The figure plots coefficient estimates and the 95 percent confidence intervals: each point shows the effect of having implemented the payroll tax cut for j years (if $j > 2012$) or of starting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

Figure C6: Employment effect, cross-municipality within-province approach



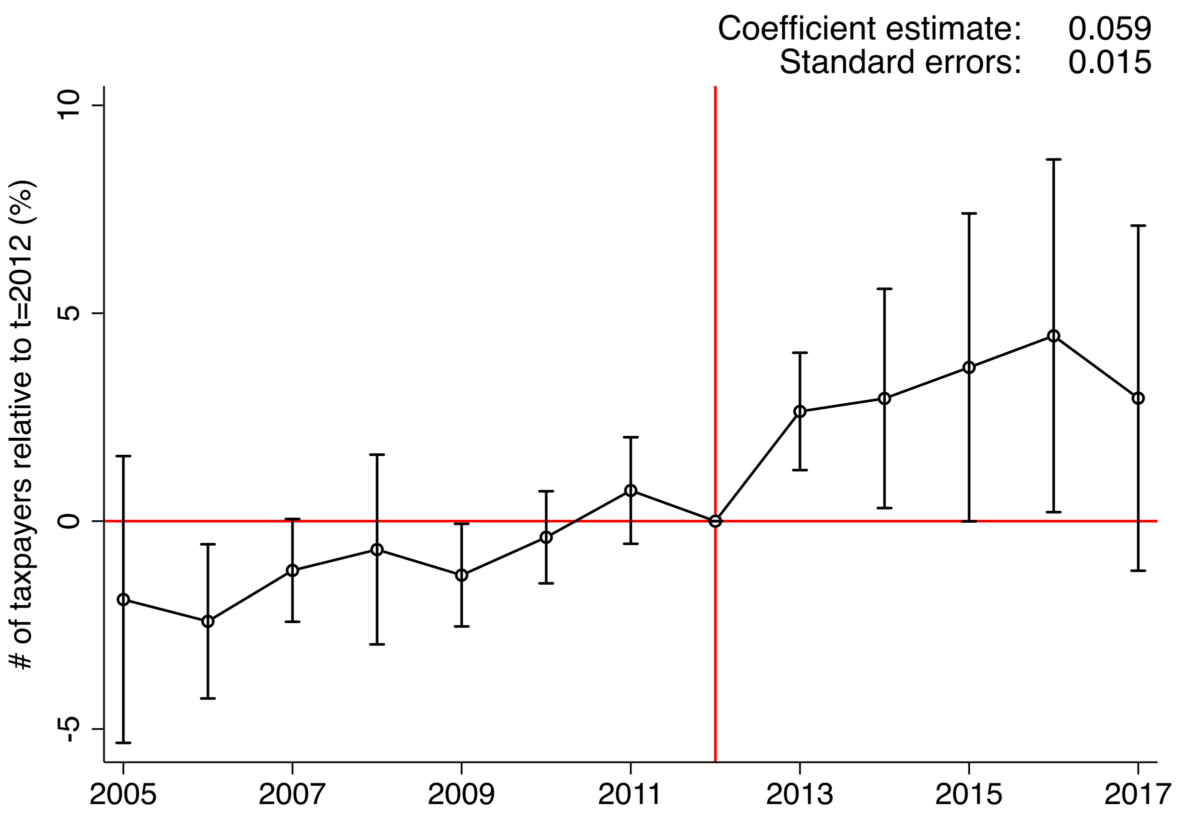
Note: The figure depicts the impact of payroll tax cut on employment exploiting cross-municipality labor market slackness. The dummy $1(m \in Slack)$ is equal to 1 in municipalities where is female unemployment share is lower than the median value. The figure plots coefficient estimates and the 95 percent 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 C7: Cross-municipality commuting



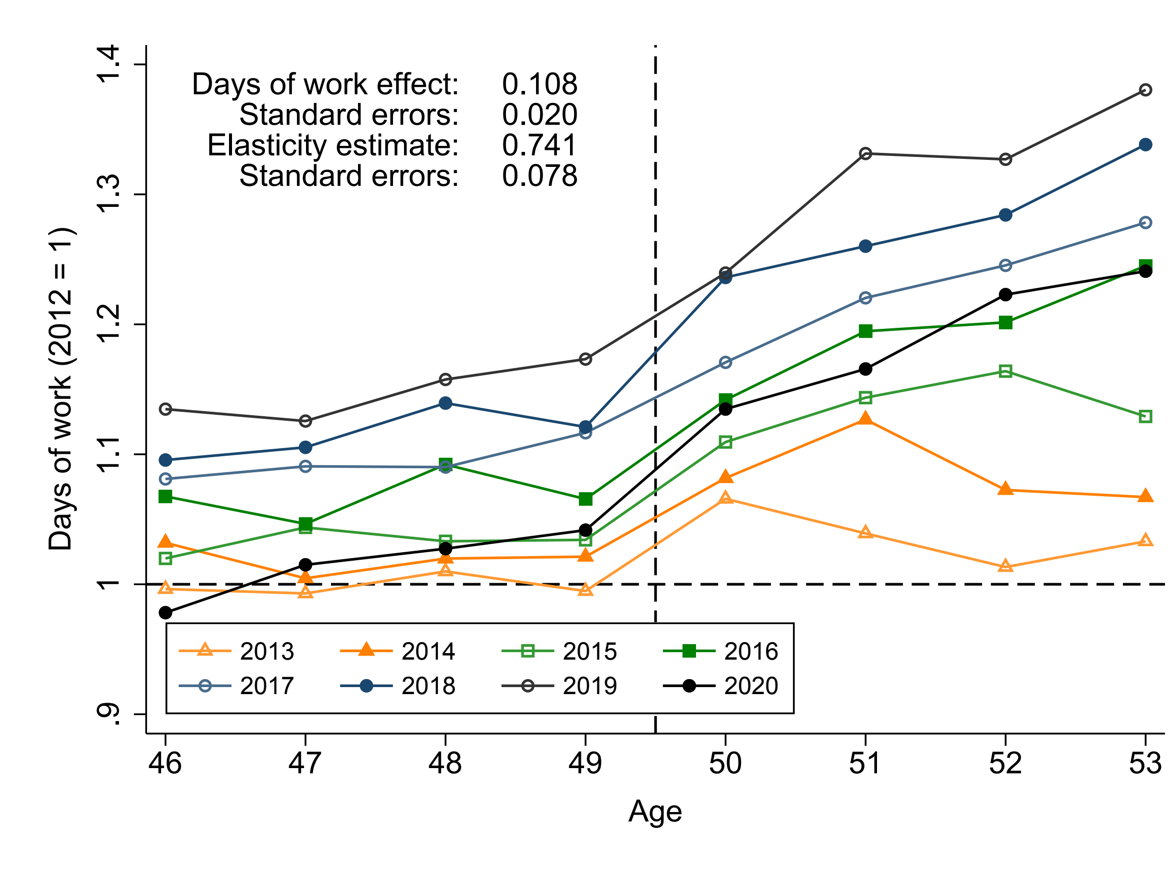
Note: The figure depicts the impact of payroll tax cut on the probability of commuting. The figure plots coefficient estimates and the 95 percent confidence intervals: each point shows the effect of having implemented the payroll tax cut for j years (if $j > 2012$) or of starting the policy in j years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

Figure C8: Employment effects using tax returns data



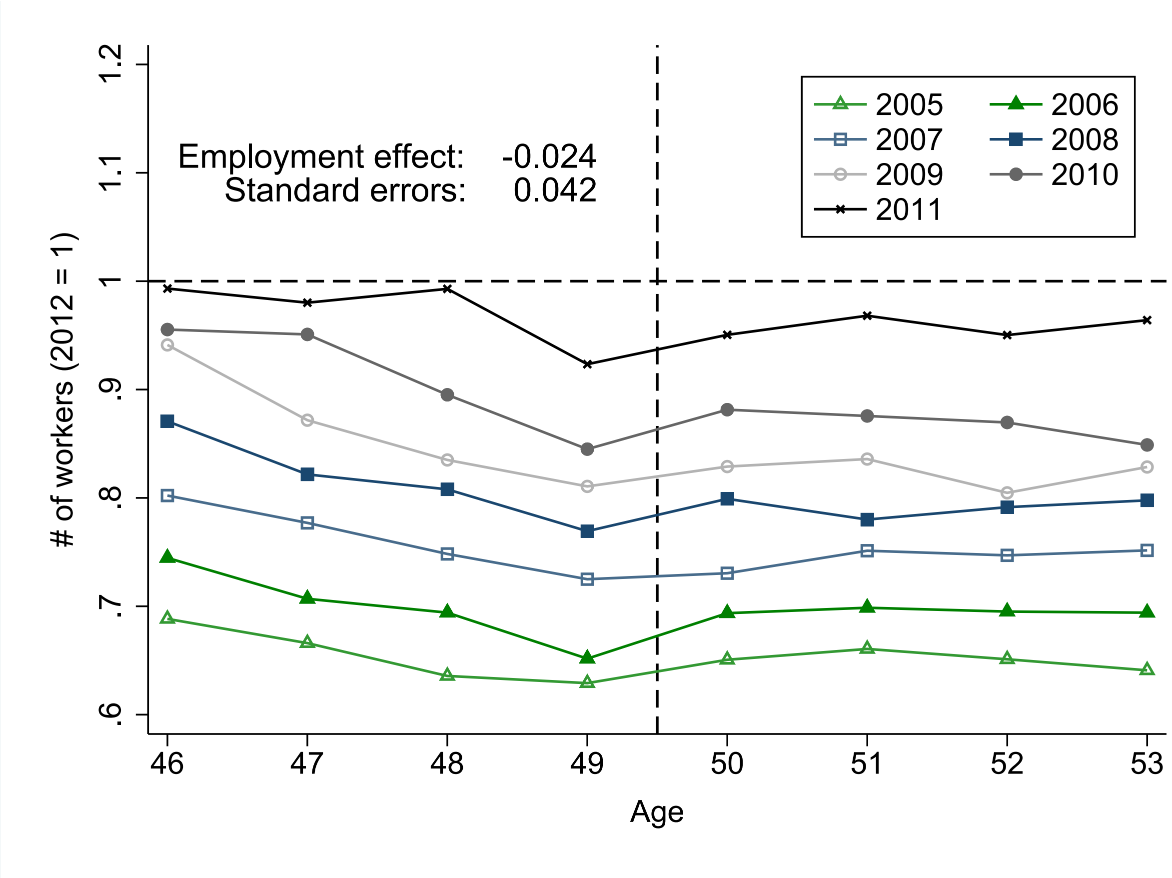
Note: The figure depicts the impact of payroll tax cut on the log of the number of taxpayers reporting positive (taxable) income below 15,000 euros. The figure plots coefficient estimates and the 95 percent confidence intervals: each point shows the effect of having implemented the payroll tax cut for 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 C9: Cross-cohort analysis on days of work



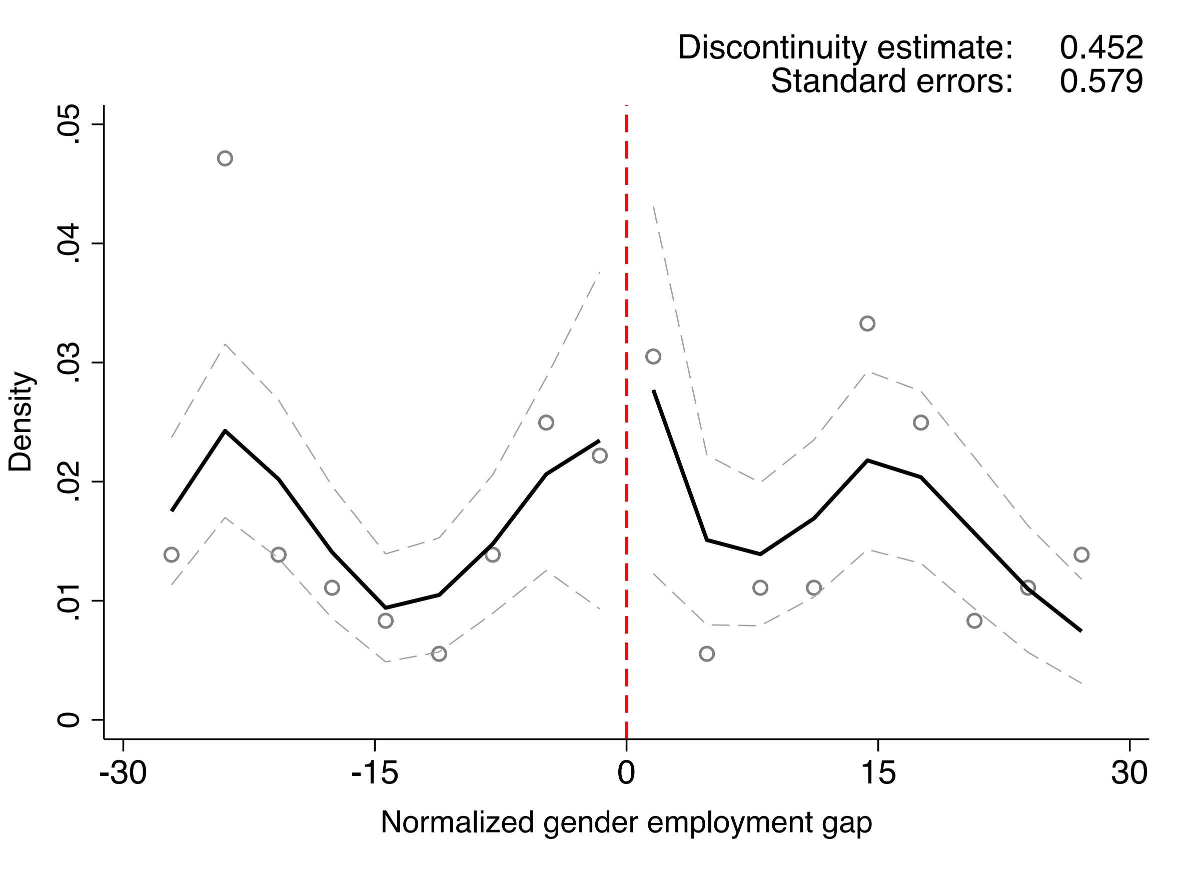
Note: The figure presents a difference-in-differences analysis by focusing on female workers with ages 46-53 in not eligible municipalities, where those younger than 50 create the control group since they were less exposed to the payroll tax cut. Days of work is measured relative to 2012, which allows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

Figure C10: Cross-cohort analysis over the pre-reform period



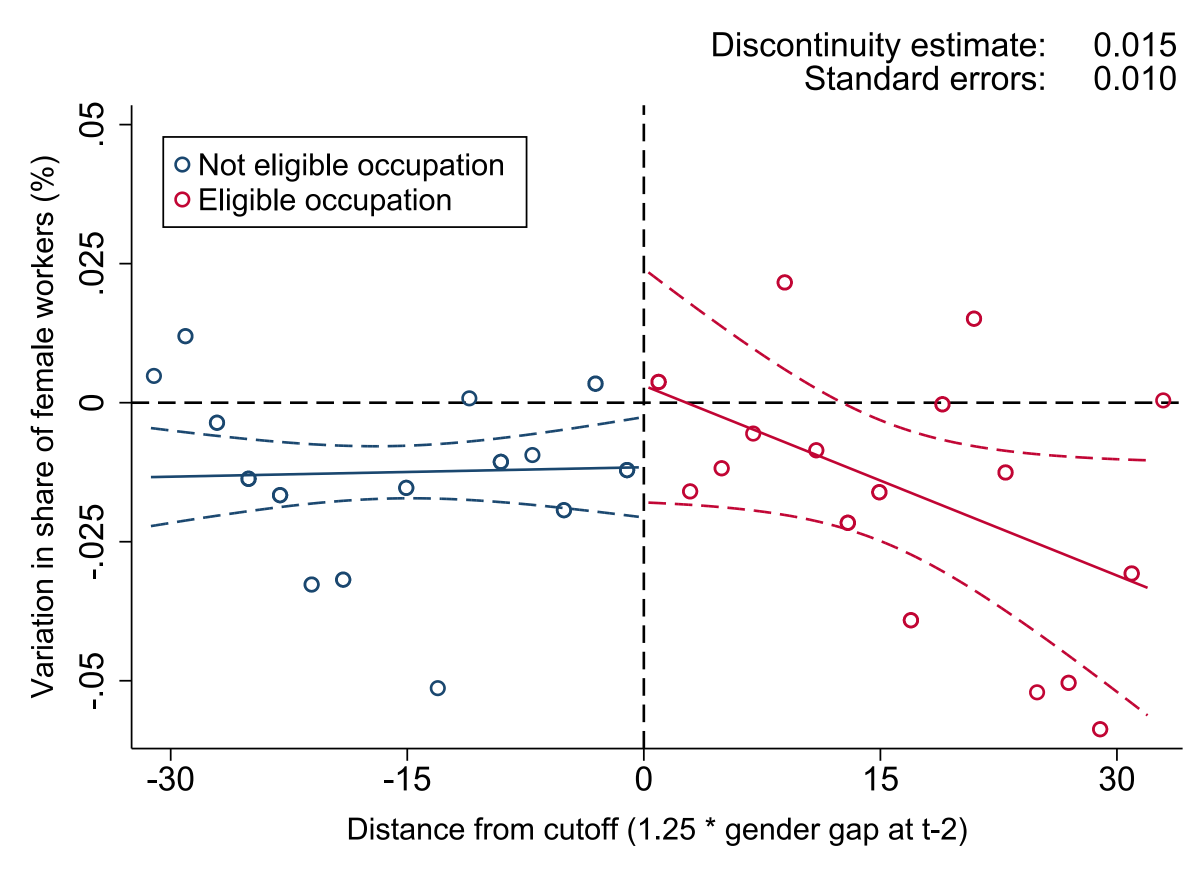
Note: The figure presents a difference-in-differences analysis by focusing on female workers with ages 46-53 in not eligible municipalities, where those younger than 50 create the control group since they were less exposed to the payroll tax cut. Employment rate is measured relative to 2012, which allows 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 C11: Density of running variable



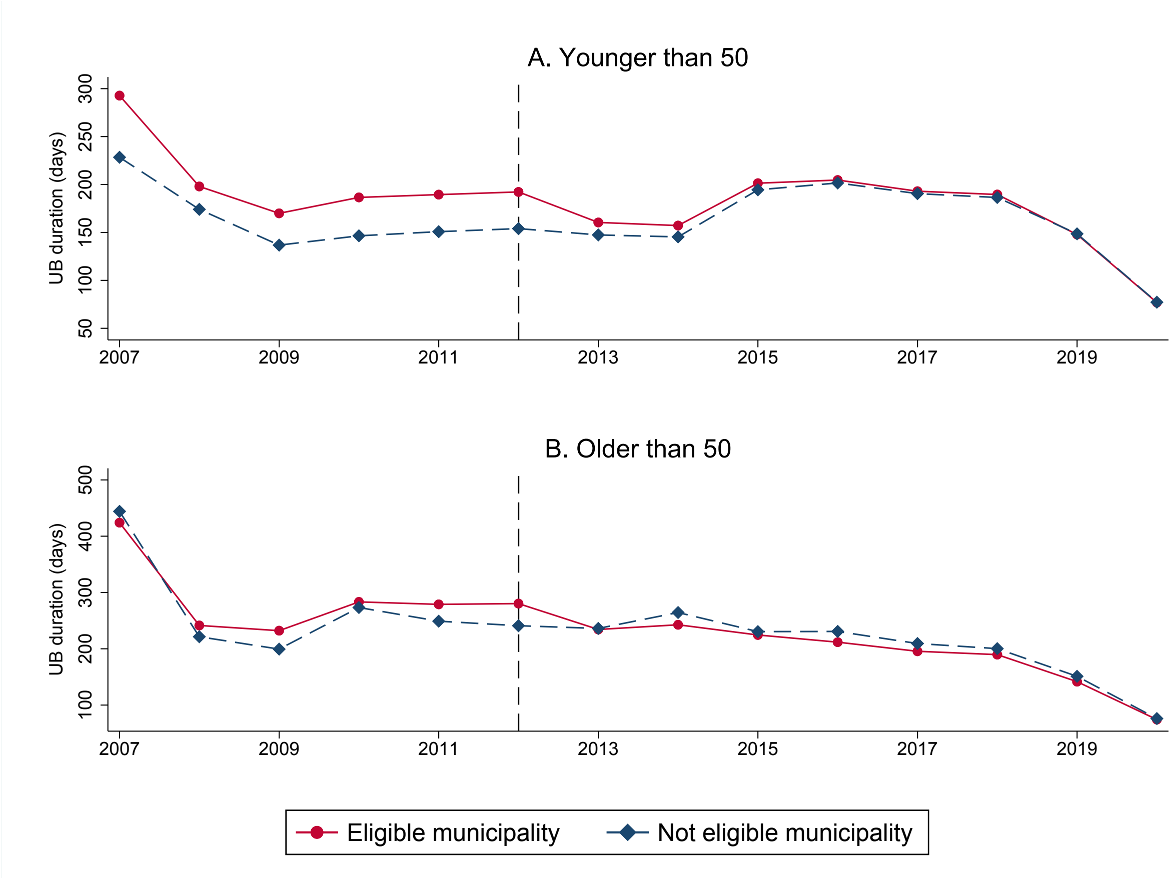
Note: The figure shows the distribution of the (normalized) occupation-specific gender employment gap around the eligibility cutoff defining a stricter exposure to the payroll tax cut (red vertical line) in occupations where the normalized gender employment gap is between -30 and 30 percent. Circles represent the average observed difference between the gender employment gap and the cutoff. The central solid line is a kernel estimate; the lateral lines represent the 95 percent confidence intervals. Discontinuity estimate (standard errors) is -.452 (.579).

Figure C12: Placebo test for cross-occupation analysis



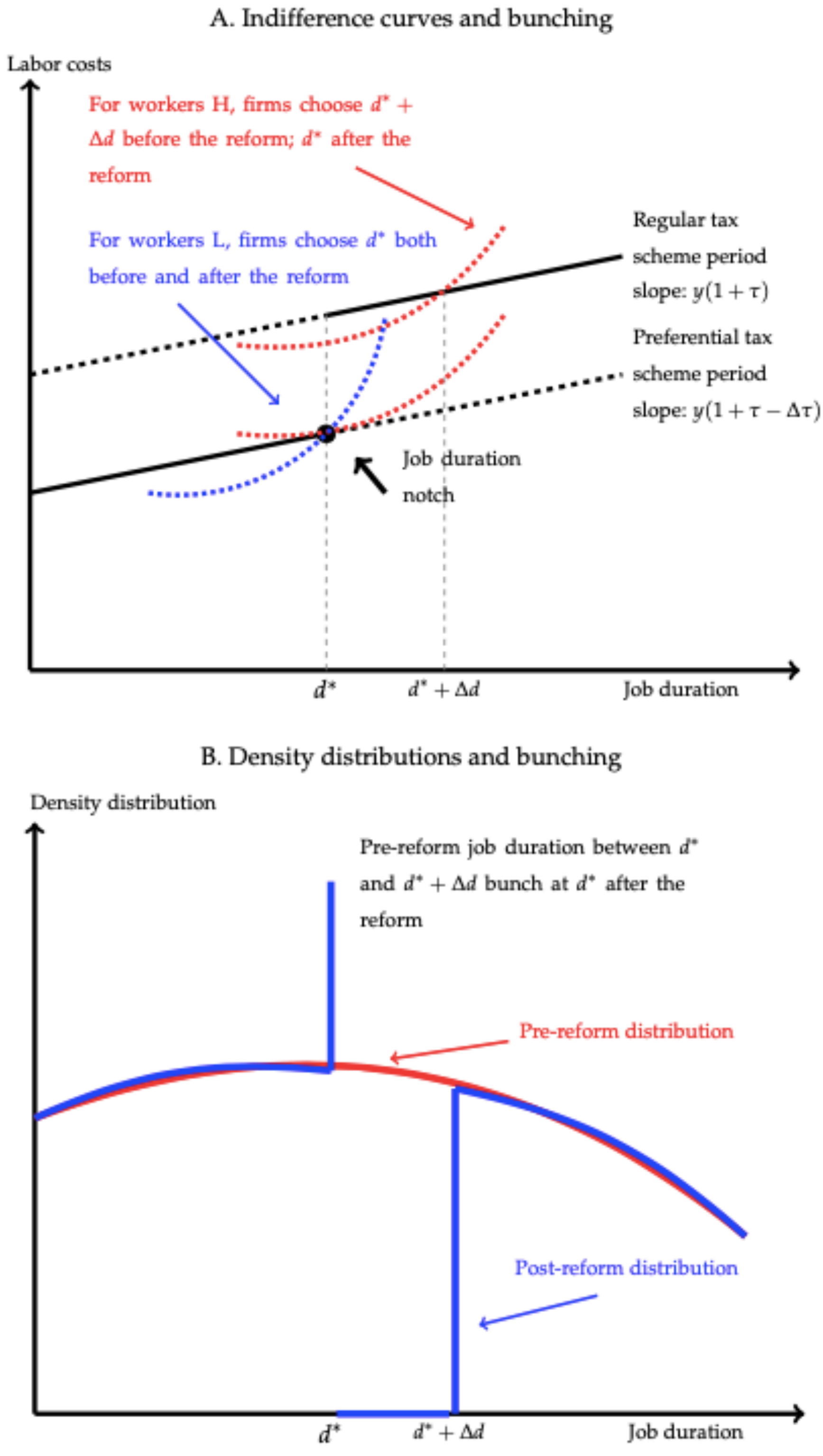
Note: The figure presents a placebo test for the cross-occupation analysis. I focus on female workers located in eligible municipalities, where the minimum non-employment duration requirement does not change discontinuously across occupations in these municipalities. The figure also reports the β coefficient and occupation-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 an occupation. Scatter points are sample average over intervals of 2 cutoff points bins.

Figure C13: Trend in duration of unemployment benefits



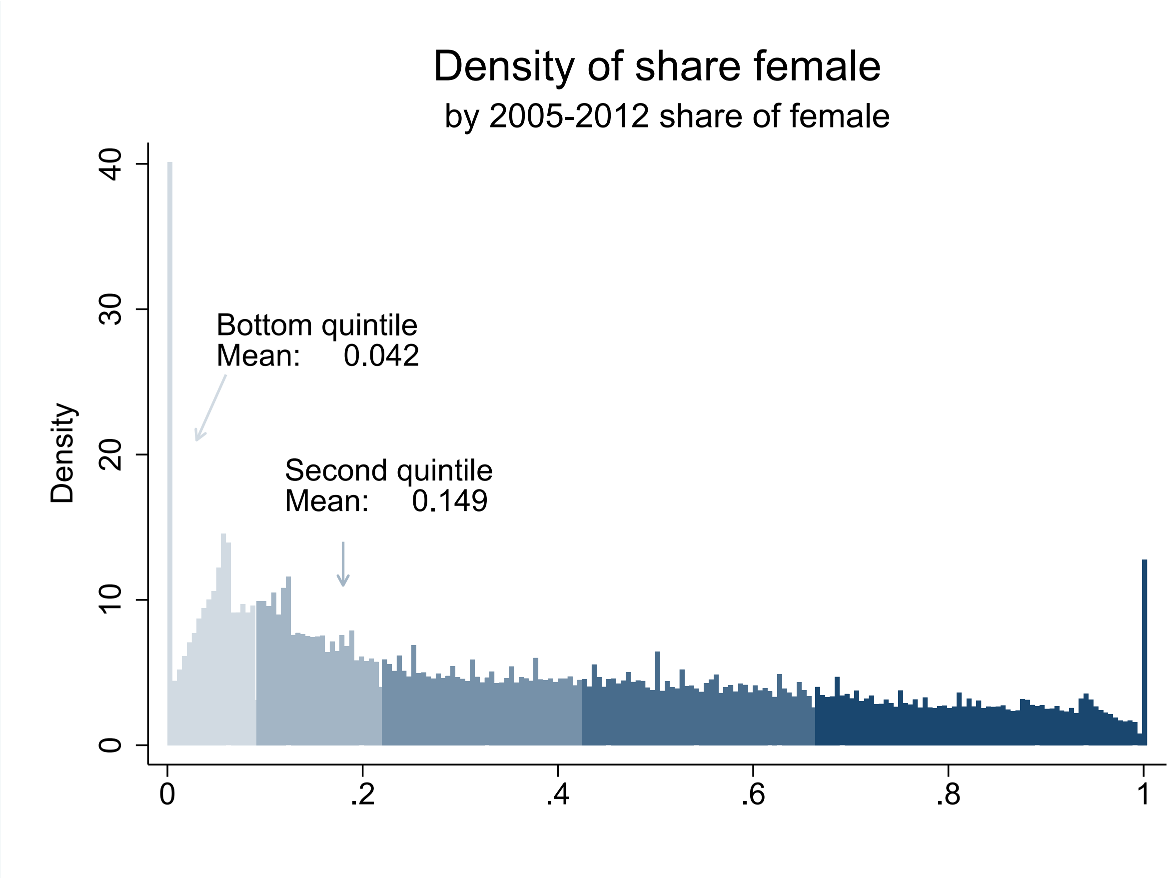
Note: The figure depicts the trend in the average UI benefits' duration (in days) across eligible (red circles) and not eligible (blue squares) municipalities. The top panel focuses on women younger than 50; the bottom panel on those older than 50.

Figure C14: The Impact of The Payroll Tax Cut on Job Duration



Note: This figure depicts the impact of introducing a preferential payroll tax scheme on job duration. The top panel displays the effect on job duration choices of introducing a tax notch in the budget set by reducing the tax rate τ by $\Delta\tau$ up to the duration d^*. For workers L, firms choose duration d^* both before and after the introduction of the payroll tax cut. For workers H, firms choose d^* after the reform, while they chose $d^* + \Delta d^*$ before the reform. The bottom panel depicts the effects of introducing the notch on the job duration density distribution. The pre-reform density is smooth around the cutoff d^*. After the reform, all workers with job duration between d^* and $d^* + \Delta d^*$ before the reform, bunch at d^*, creating a spike in the density distribution just at d^* and a hole in the segment $d^* + \Delta d^*$.

Figure C15: 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.