

Rubolino, Enrico

Working Paper

Taxing the gender gap: Labor market effects of a payroll tax cut for women in Italy

Upjohn Institute Working Paper, No. 23-386

Provided in Cooperation with:

W. E. Upjohn Institute for Employment Research, Kalamazoo, Mich.

Suggested Citation: Rubolino, Enrico (2023) : Taxing the gender gap: Labor market effects of a payroll tax cut for women in Italy, Upjohn Institute Working Paper, No. 23-386, W.E. Upjohn Institute for Employment Research, Kalamazoo, MI, <https://doi.org/10.17848/wp23-386>

This Version is available at:

<https://hdl.handle.net/10419/283980>

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.

7-31-2023

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

Enrico Rubolino

University of Lausanne, Dept. of Business and Economics, enrico.rubolino@unil.ch

Upjohn Institute working paper ; 23-386

Citation

Rubolino, Enrico. 2023. "Taxing the Gender Gap: Labor Market Effects of A Payroll Tax Cut for Women in Italy." Upjohn Institute Working Paper 23-386. Kalamazoo, MI: W.E. Upjohn Institute for Employment Research. <https://doi.org/10.17848/wp23-386>

This title is brought to you by the Upjohn Institute. For more information, please contact repository@upjohn.org.

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

Upjohn Institute Working Paper 23-386

Enrico Rubolino

University of Lausanne, Department of Business and Economics

Email: enrico.rubolino@unil.ch

July 2023

ABSTRACT

This paper studies the labor market effects of a large payroll tax cut for female hires in Italy. Starting in January 2013, the payroll tax rate paid by the employer for female hires was reduced by 50 percent for a period of 12 months for temporary jobs and 18 months for permanent jobs. Eligibility for the tax cut depends on the time elapsed in nonemployment status and varies discontinuously by the worker's municipality of residence, age, and occupation. Combining social security data on the universe of Italian private-sector workers with several empirical approaches, I find that the tax cut increases female employment and spurs business performance, especially where gender biases are more severe. By contrast, the tax cut does not raise workers' net wages. A cost-benefit analysis implies that the net cost of the policy is around one-fourth of the budgetary cost. These findings provide the first empirical evidence that differentiating payroll taxes by gender helps to reduce the gender employment gap, but not the gender pay gap.

JEL Classification Codes: H22, J21, J31

Key Words: gender gaps; female employment; payroll tax; tax incidence

Acknowledgments: I thank Wiji Arulampalam, Sonia Bhalotra, Antoine Bozio, Marius Brulhart, Maria De Paola, Edoardo Di Porto, Naomi Feldman, Evelina Gavrilova-Zoutman, Jarkko Harju, Andrea Ichino, Niels Johannesen, Rafael Lalive, Salvatore Lattanzio, Marco Manacorda, Sauro Mocetti, Paolo Naticchioni, Matteo Paradisi, Caterina Pavese, Emmanuel Saez, Vincenzo Scrutinio, Juan Carlos Suarez Serrato, Dario Tortarolo, and seminar participants at the University of Barcelona, the University of Copenhagen, the University of Lausanne, the European University Institute, IFN Stockholm, INPS, AIEL, EALE, EEA, HPF, NTA, SIEP, SIE, the SEPR-Bank of Italy Workshop, the CESifo Conference on Public Economics, the IZA Workshop on Labor Market Institutions, and the Brucchi Luchino Workshop. I gratefully acknowledge funding and data access through the VisitINPS program, the Early Career Research Award granted by the W.E. Upjohn Institute for Employment Research, and the Swiss National Science Foundation (Grant No. 192546). The findings and conclusions expressed are solely those of the author and do not represent the views of INPS.

Upjohn Institute working papers are meant to stimulate discussion and criticism among the policy research community. Content and opinions are the sole responsibility of the author.

1 Introduction

Gender gaps in pay and employment are observed in every industrialized country, although to a varying degree (see, e.g., [Blau and Kahn 2003](#); [Olivetti and Petrongolo 2016](#)). Gender equality has become a key goal for policymakers 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 issues 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. 2020](#)) and gender quotas ([Bertrand et al. 2019](#)). However, a consensus on 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](#)) or if gender discrimination creates barriers to female employment in certain occupations or firms ([Card et al. 2021](#); [Kline et al. 2022](#)), then tackling gender inequality through government policies can be challenged by pervasive and sticky gender stereotypes.

This paper studies the impact of differentiating payroll tax rates by gender on labor market outcomes and gender gaps. Since the seminal work of [Becker \(1957\)](#), economists have assumed that discrimination is the product of personal prejudice. Gender-prejudiced employers prefer to hire male workers even if they are less productive than female workers. By making gender discrimination more costly, a payroll tax cut for female hires would raise demand for female labor 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 ([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.¹ 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 topic, I study the labor market effects of a large employer-

¹According to optimal tax theory, a benevolent government should tax individuals who present a more elastic labor supply relatively less. Since the labor supply of women is more elastic than the 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” (p.1).

borne payroll tax cut for female hires in Italy. Starting from January 2013, the payroll tax rate paid by the employer for female hires was reduced by 50 percent for a period of up to 12 (18) months for temporary (permanent) jobs (see law 92/2012).² As the employer portion of the payroll tax rate in Italy is around 22 percent of the employee's gross compensation, this tax cut creates a large differential in labor costs between male and female hires. Eligibility for the scheme depends on the time elapsed in nonemployment status and varies discontinuously by worker's municipality of residence, age, and occupation. Eligibility criteria are meant to foster female participation rates, especially in gender-imbalanced places and occupations. Specifically, in a first group of municipalities, the payroll tax cut applies to women with a nonemployment duration of at least six months. In a second group of municipalities, the minimum nonemployment duration requirement is 12 months for women older than 50, 24 months for those younger than 50. The minimum nonemployment duration requirement is also reduced to six months for women hired in male-dominated occupations, defined by the law as those where the female employment share is larger than 25 percent of the mean gender employment gap.

Using social security data on the universe of Italian private-sector workers, the first part of the paper evaluates the labor market impacts of the payroll tax cut. I propose several empirical approaches, resting on discontinuities in the eligibility criteria across municipalities, birth cohorts, and occupations. All these empirical approaches point to the same conclusion: the tax cut leads to lasting growth in female employment, but it does not affect workers' net wages. Namely, using an event-study research design and a triple difference approach, I show that female employment increases by around 3.3 (2.0) percent in municipalities (birth cohorts) where tax-cut eligibility is less binding. Employment effects build up gradually and are significant for up to eight years after the reform. By contrast, workers' net wages present a clear and precisely estimated pattern of zero effects. The evolution in days worked is similar to that of employment, suggesting that firms do not substitute new hires benefiting from the tax cut for incumbent workers. In fact, I do not find any offsetting decline in *men's* labor market outcomes. 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.

Motivated by the possibility that the aggregate analyses could be biased by the presence of other contemporaneous policies, economic shocks, or measurement errors in determining payroll-tax-cut eligibility, I propose two microlevel analyses that can account for these concerns. They both provide converging evidence. First, I estimate

²Hiring credits have been gaining political traction. For instance, several 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](#)). Yet, as recently emphasized by [Cahuc et al. \(2019\)](#), "there is very little empirical evidence about the effects of hiring credits" (p. 593).

microlevel employment responses by leveraging variation in tax-cut eligibility across individuals within a given municipality-occupation-cohort-month cell, thus accounting for several time-varying policies, shocks, and secular trends across places and occupations. I find an increase in the employment probability of 1.4 percentage points (i.e., about 2.5 percent), which maps into a net-of-tax elasticity of 0.11. Second, I quantify the size of the pass-through of the tax windfall on workers' wages by exploiting the fact that tax-cut eligibility varies discontinuously by firm tenure: it applies for up to 12 months for temporary jobs, 18 for permanent jobs. I then implement a difference-in-differences empirical approach, comparing the evolution in wages before versus after crossing the firm-tenure cutoff, and across tax-cut eligible versus noneligible jobs. I provide clear evidence that *net* wages earned throughout the preferential tax scheme period are strikingly similar to those earned in noneligible jobs. By contrast, I find a clear dip in *gross* wages. This result provides striking evidence that employers do not adjust wages in response to the tax cut.

My first set of results suggests that employers pocket the tax cut and thus benefit from lower labor costs by hiring eligible female workers. The second part of the empirical analysis, therefore, focuses on how firms respond to the tax cut. Firms' response depends on two main channels. First, the tax cut generates a large cash flow to firms with higher subsidized hires. If firms are credit constrained, this cash windfall could lead firms to grow.³ A second channel is that the tax cut makes gender discrimination more costly. If gender prejudice created barriers to female employment, the tax cut can erode discrimination by "taxing" gender-prejudiced employers. A few papers have shown that firms with more female employees earn higher profits and survive longer (Hellerstein et al. 2002; Kawaguchi 2007; Weber and Zulehner 2014). A suitable explanation is that stereotypes distort beliefs and lead discriminatory firms to underperform in the market. By making gender biases relatively more costly, the tax-cut-induced increase in female employment can then improve business performance, particularly in more gender-biased firms.

Matching social security data with firm-level balance sheets, I implement a two-stage least squares (2SLS) strategy, leveraging the fact that the payroll tax cut generates firm-specific windfalls that are proportional to a firm's share of subsidized hires. I take advantage of persistent between-firm variation in the share of female workers before the reform, as done in previous studies (Cahuc et al. 2019; Saez et al. 2019). I provide three main results. First, I find that a 10 percentage point increase in the share of subsidized hires (which fairly approximates the impact of hiring an eligible worker in the median firm) significantly raises the firm's workforce by nearly 4.0 percent, to-

³Studies in the corporate finance literature (see, e.g., Fazzari et al. 1988) show that cash windfalls significantly affect firms' performance. Some recent studies have also shown that payroll taxes significantly affect firm-level outcomes (see Saez et al. 2019, Benzarti and Harju 2021a, and Benzarti and Harju 2021b).

tal production value by 1.7 percent, and value-added by 0.9 percent. Second, around half of the tax windfall is passed on to incumbent workers' wages. Third, I estimate a significant increase in firm profitability ratios: the return on assets increases by 0.4 percentage points, the return on equity by 1.2 percentage points, and the return on investment by 0.7 percentage points. These results appear robust to alternative specifications and survive the inclusion of many fixed effects.

To shed light on the mechanism behind these effects, I investigate whether the marginal impact is relatively stronger for firms that are more likely to discriminate. To this end, I match longitudinal administrative data on firm-level outcomes with a nationwide survey eliciting information on gender prejudices. Following previous studies (see, e.g., [Bertrand 2020](#)), I create a gender bias index as the share of workers that agrees with the view that "when jobs are scarce, men have more right to a job than women." Comparing the industry-specific 2SLS coefficient estimate with the gender bias index, I show that, for a given share of subsidized hires (and a given "mechanical" reduction in labor costs), the productivity gains are relatively larger in industries with more severe gender biases. On average, a 1.0 percentage point increase in the gender bias index raises the firm logged value-added by 0.005 coefficient estimate points. To put this estimate in perspective, this result suggests that the value-added impact of a marginal increase in the policy take-up rate on a stereotypically male industry such as the construction sector (gender bias index = 0.3) would be 6.7 percentage points larger than the same take-up rate increase in a stereotypically female industry such as the health sector (gender bias index = 0.13). I uncover similar patterns when looking at the profitability ratios, suggesting that gender-biased industries have much more to gain from the integration of female workers.

What explains the reason for the marginal increase in firm productivity and profitability being larger in gender-biased industries? I rule out two possible explanations. First, this fact cannot be explained by the possibility that gender-biased firms are relatively more likely to grow: the gender bias index is not significantly associated with a larger marginal impact on the firm workforce or production value. Second, the productivity increase does not stem from a more generous rent-sharing response of gender-biased firms with their incumbent workers. A residual explanation would be that the tax-cut-induced integration of female workers into gender-biased (and male-dominated) industries spurred productivity by reducing gender discrimination. This explanation would be in line with [Hsieh et al. \(2019\)](#), showing that declining discrimination against women and Blacks raised U.S. aggregate productivity.⁴

The final part of the paper presents a cost-benefit analysis of the policy. Accounting

⁴This finding is also corroborated by interpersonal contact theory, which predicts that mixing groups will break down stereotypes and encourage between-group collaboration. In a recent experimental study, [Dahl et al. \(2021\)](#) show that exposure of women to men in a traditionally male-dominated context—the military in Norway—changed attitudes about mixed-gender productivity, without harming male performance.

for the fiscal externalities triggered by the policies, I find that for every €1 of missing revenue to finance the tax scheme, government revenue increases by €0.73. This result suggests that the payroll tax cut on female hires has been quite effective, since the net cost per job created is about one-quarter of the budgetary cost. The tax cut thus turns out to be an effective policy to reduce the gender *employment* gap even in contexts where gender attitudes are still traditional, but it is an undesirable policy if policy-makers want to reduce the gender *wage* gap. A simple back-of-the-envelope calculation suggests that the gender employment gap in Italy would have been 2.0 percentage points larger (20.2 instead of 18.2) in the absence of the policy.

The main contribution of this paper is to offer the first empirical evidence on the effect of differentiating payroll taxes by gender. My findings relate to the literature on gender by providing empirical evidence that ad hoc tax policies can remove barriers to female employment in places and occupations that are particularly gender segregated. Despite a few studies that have investigated whether economic policies can successfully lead to a rise in the share of female employees in an industry (see, e.g., [Ashenfelter and Hannan \(1986\)](#) and [Black and Strahan \(2001\)](#) for the banking sector), there is no clear evidence about male-dominated places and occupations. These findings can be particularly relevant for countries considering further integration of women into male-dominated contexts.

The paper connects with studies estimating the incidence of payroll taxes. My result is consistent with a recent series of empirical works focusing on upper earners in Greece ([Saez et al. 2012](#)), young workers in Sweden ([Egebark and Kaunitz 2018](#), [Saez et al. 2019](#); [Saez et al. 2021](#)), low earners in France ([Cahuc et al. 2019](#)), and workers in Finland ([Benzarti and Harju 2021a](#); [Benzarti and Harju 2021b](#)). Furthermore, as 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 new insights into this literature. First, the richness of the data and the quasi-experimental variation generated by the Italian reform allow me to evaluate the impact of the payroll tax cut on a wide range of outcomes, using several complementary empirical approaches. Second, because of the pervasive presence of gender biases, gender-based tax instruments are potentially more critical than the evaluation of age-based tax instruments. By correcting for the mix of male and female employment, this study shows that a tax cut can reduce the gender employment gap and help increase the productivity and profitability of firms operating in gender-biased environments.

This study also speaks to the literature on the effects of gender discrimination on firms. This literature rests on the [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). 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 empirical literature contains little evidence on how changes in the cost of gender discrimination affect firm productivity and business performance.⁵

The rest of the paper proceeds as follows: Section 2 provides information on the Italian labor market, the preferential tax scheme for female hires, and describes the data. Section 3 presents the labor market effects of the payroll tax cut. Section 4 reports the effects of the payroll tax cut on various firm-level outcomes. A cost-benefit analysis and a discussion on the implications for gender inequalities are presented in Section 5. Section 6 concludes.

2 Institutional Background and Data

This section discusses the main institutional features of the Italian labor market and the payroll tax reform. Next, I will describe the data.

2.1 Institutional and Policy Background

2.1.1 Gender Labor Market Inequalities in Italy

According to the [OECD Family Database](#), Italy ranks low regarding female labor market outcomes: in 2018, the full-time-equivalent employment share of women was 40.3 percent, and the gender employment gap was 26.5 percentage points. Among European countries, only Greece performs worse than Italy. In terms of gender pay differences, Italy looks relatively better: the gender gap in median earnings of full-time employees was around 5 percentage points in 2018, against an average OECD value of slightly larger than 13 percent ([OECD Family Database](#); [Casarico and Lattanzio 2019](#)).

Female employment differs widely across occupations. To offer an international perspective, Appendix [Figure A1](#) provides a comparison between Italy and the United States for a variety of occupations. The figure shows that the proportion 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 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.⁶

Furthermore, there is striking heterogeneity in female employment across places.

⁵See [Altonji and Blank \(1999\)](#) and [Bertrand \(2011\)](#) for surveys on how discrimination affects labor market outcomes of women as well as other underprivileged workers.

⁶Appendix [A.1](#) offers a discussion and descriptive evidence on gender stereotypes and discrimination in Italy, based on a nationwide survey conducted by the Italian Institute of Statistics in 2011.

Appendix [Figure A2](#) depicts the municipality-level female employment share in 2012 (the year before the payroll tax cut). A clear north-south divide in female employment emerges from the figure. For instance, the gender employment gap is about 12 percentage points in cities in northern Italy such as Milan and Turin, but around 30 percentage points in southern Italian cities such as Naples and Palermo.

2.1.2 The Payroll Tax Cut for Female Hires

Similarly to most developed countries, payroll taxes in Italy contribute to covering a range of welfare benefits, such as unemployment insurance and maternity leave. According to the [OECD Tax Database](#), they accounted for 13.3 percent of the Italian GDP (or 31.3 percent of taxation) in 2018. The total payroll tax burden is relatively high, representing around one-third of employees' gross compensation.⁷ Appendix [Figure B1](#) depicts the evolution in payroll tax rates since 2005. The employer (normal) tax rate is marked by the blue circles in the left-hand-side graph; the right-hand-side graph depicts the employee payroll tax rate, shown separately for the bottom (red squares) and top tax rate (blue circles). Both the employer and employee tax rates have been quite stable over the past two decades.

Law 92/2012 introduced a permanent employer payroll tax cut for female hires, starting from January 2013. The law, known as "Fornero reform," aimed at tackling gender labor market inequalities and stimulating business activity. As shown by the red squares in the left-hand side graph of [Figure B1](#), the preferential payroll tax scheme reduced the employer's portion of the payroll tax by 50 percent, dropping it to 10.81 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 *non-mutually exclusive* dimensions. First, the eligibility criteria are relatively less binding in economically disadvantaged areas (named *disadvantaged municipalities* hereafter).⁸ Disadvantaged municipalities are classified as municipalities that are eligible for European Union structural funds (see [Figure A4](#) for a map of municipalities eligible for structural funds). Specifically, the payroll tax cut applies to female hires that spent at least six months in nonemployment status and are residents in disadvantaged municipalities. In the other municipalities, the minimum nonemployment duration requirement is 12 months for women older than 50, 24 months for those younger than 50.

Second, the eligibility criteria favor occupations with a greater gender imbalance. The minimum nonemployment duration requirement is set to six months for women

⁷Apart 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.

⁸Following 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 a payroll tax cut in poorer areas was to increase economic activity.

hired in occupations where the gender employment gap is at least 25 percent larger than the average employment gap (referred to as *male-dominated occupation* hereafter), 24 months for the other occupations. The Ministry of Labor published the list of occupations eligible for the preferential tax scheme annually, along with official occupation-specific statistics on the gender employment gap. Eligibility for year t is based on gender employment-gap estimates relative to year $t - 2$. Occupations are identified by the International Standard Classification of Occupations (ISCO) submajor group. I report series on the occupation-specific gender employment gap in Appendix [Table A1](#) (see Appendix [A.2](#) for details).⁹

The payroll tax cut is meant to be a hiring subsidy. It applies for up to 12 months for a temporary job opening, 18 months for a permanent job (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 limiting the possibility that employers could substitute noneligible workers for eligible ones. The reform was salient. In the public debate, politicians emphasized the opportunities 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](#)).

2.1.3 Other Contemporaneous Policy Changes

The Fornero reform also implemented other labor market changes. First, a salient pension reform was also enacted by the same government, which increased the statutory retirement age for both male and female workers (see [Bianchi et al. 2022](#)). Second, there were important changes in the unemployment protection system: ordinary unemployment insurance was replaced by a new instrument, named ASPI (Social Insurance for Employment), that provided higher benefits and a gradual increase in the maximum potential duration (see [Zurla 2022](#)). Finally, the 2015 labor market reform (the so-called Jobs Act) introduced two main policy changes: 1) a hiring subsidy for permanent jobs, and 2) a reduction in new hires' firing costs (see [Sestito and Viviano 2018](#)).

However, the eligibility criteria for these contemporaneous policy changes do not vary by gender, and none of these policies are directly aimed at spurring female employment or wages. Even in cases in which female labor market outcomes are rela-

⁹Furthermore, the legislature introduced an additional requirement based on the industry in which the employer operates. Earlier eligibility is also based on a similar cutoff rule. However, I do not find any significant rise in the take-up rate for industries benefiting from less-binding eligibility criteria. There are two potential explanations for this lack of response. First, the eligibility criteria in this case rests on *employer-specific* characteristics, while the other criteria depend on the 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. My baseline results, presented below, are not affected by dropping workers in eligible industries from the analysis.

tively more likely to be affected by these policy changes, the design of the payroll tax cut, which provides different eligibility criteria across places, birth cohorts, and occupations, allows for credible control of these other reforms.

2.1.4 Wage Setting and Unemployment Insurance in Italy

In Italy, wages are set by collective agreement at the 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 add an extra premium (a wage cushion) for workers.¹⁰ Therefore, the two-pillar Italian system can create considerable variation in wages across firms in the same job task and across workers within a firm (Guiso et al. 2005). For instance, Card et al. (2014) show that actual wages are above the wage floor for nearly all employees in Italy, and the median worker enjoys a wage premium of about 24 percent.

The Italian unemployment insurance (UI) system is similar to the other continental European systems in terms of its 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 their 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 threshold (yearly updated; it was 1,328,760 euros in 2019), but the amount is reduced by 3 percent after three months. The maximum UI potential duration also depends on work history: it is equal to half the number of weeks of work during the past four years, up to a cap of 24 months.

2.2 Data and Descriptive Evidence

2.2.1 Administrative Data

My primary data source is matched employer-employee data provided by the Italian Social Security Institute (INPS, *Istituto Nazionale di Previdenza Sociale*) through the VisitINPS program. The data cover the universe of Italian workers in the nonagricultural private sector. They include information on demographic 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 terminating the job contract (including whether the worker was hired with the preferential payroll tax scheme). Starting in 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 from 2005 up to December 2020, which is the latest available date.

¹⁰Wage floors can also differ across provinces, although this is not very common (Boeri et al. 2021).

The observation unit in the data is the job spell. Since a worker can be employed by 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.¹¹ Furthermore, I drop any duplicates based on observations that have the same information for a given employer-employee record in a given month.

In the paper, *gross wages* refers 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.¹² *Net wages* 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). In addition to regular wages and salaries, earnings also include bonuses, overtime pay, and any pay in arrears. Therefore, "earnings" in the Italian social security data represent a broad definition of cash employment income, which is used as the reference for computing the payroll tax burden and is also the standard reference for employer-employee compensation negotiations and decisions.

I collect information on workers' occupations from another data set (called *Comunicazioni Obbligatorie*) that collects six-digit-level information on the occupation of each spell (merged with the main data set through the scrambled worker identifier). This information is reported by the firm at the beginning of the job spell and updated in case of any change. Different from the main data set, this information is available until 2019. I also collect data on unemployment insurance benefit recipients. This data set (called *Prestazioni a Sostegno del Reddito*) is collected from various administrative records compiled by INPS and provides individual-level information on unemployment history.

To study the impact of the policy on firm performance, I access firms' balance-sheet data, provided by Cerved over the 2005–2018 period. I observe firm-level information on several financial outcomes, including the production value and value-added, among other measures. Using balance-sheet data, I compute standard measures of firm profitability, such as the return on assets (computed as the firm's net income over its average total assets), the return on equity (net income over shareholders' equity), and the return on investment (net income over investment costs).¹³ The data set also reports detailed geographic information, the industry, and the dates of "birth" or closure of the firm (if applicable).

¹¹Alternative methods, such as selecting observations with the highest number of days worked, have no impact in practice.

¹²More 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.

¹³To reduce the influence of outliers, I remove firm-year observations with unusually high values of value-added, production, and profitability ratios (defined as those in the top percentile of these variables).

2.2.2 Policy Take-Up and Descriptive Evidence

Appendix [Table B4](#) displays summary statistics of program recipients. On aggregate, 218,768 women have been hired at least once through the preferential payroll tax scheme. The representative recipient earns a daily full-time-equivalent gross salary of nearly 85 euros. Recipients are relatively young (average age is 38), hired for temporary work (67 percent), in blue-collar fields (61 percent), and in part-time jobs (57 percent). The characteristics of employers benefiting from the payroll tax cut are presented in Appendix [Table B5](#). They mostly operate in large, experienced firms (the average number of employees is 20; the average firm age is 8.6 years). The composition of the industrial sector reveals that most all sectors made use of the payroll tax cut, but with an overrepresentation of the wholesale and retail trade (around half of the sample), followed by accommodations and food service (19 percent) and manufacturing (16 percent).

[Figure 1](#) illustrates the trend in payroll tax cut take-up. Panel A shows a tenfold rise in the count of recipients, increasing from 7,312 in the first year of the policy to more than 70,000 in 2019 and 2020. Appendix [Figure B2](#) shows that the trend in the policy take-up looks fairly similar when I scale the recipient count by total female employment or total new female hires. It turns out that payroll-tax-cut recipients account for around 4 percent of total new female hires, which mostly include female hires that did not meet the eligibility criteria.¹⁴

Does the policy design affect the take-up? [Figure 1](#) allows me to investigate whether the discontinuities created by the eligibility criteria across municipalities (Panel B), age cohorts (Panel C), and occupations (Panel D) had any differential impact on the policy take-up. The pattern emerging from the figure suggests that the policy take-up is in accordance with the less-binding eligibility criteria granted to disadvantaged municipalities and older age cohorts, but not across occupations.

Panel B shows that the take-up is systematically larger in disadvantaged municipalities (red circles), compared to nondisadvantaged ones (blue diamonds). The graph in Panel C displays a large and discontinuous increase in the take-up at age 50 in nondisadvantaged municipalities, where the minimum nonemployment duration requirement discontinuously drops by 12 months. By contrast, there is only a small change at the same age cutoff in disadvantaged municipalities, where the eligibility criteria are unchanged. The less-binding eligibility criteria granted to male-dominated occupations do not translate, instead, into a higher policy take-up (Panel D). This is valid both cross-sectionally in a given year and in the overall take-up trend.

To offer prima facie evidence on whether employers passed some of the tax cut on workers' wages, [Figure 2](#) plots kernel density estimates of the distribution of gross

¹⁴Many hiring-subsidy policies present low take-up because of administrative application costs for employers and stigma costs for recipients (see [Katz 1998](#) and [Neumark 2013](#) for discussion). The take-up of the payroll tax cut is not likely to be confused with other preferential tax regimes, such as those provided by the 2015 Jobs Act, because the tax cut is not large.

wages (Panel A) and net wages (Panel B) of the preferential tax regime’s recipients. I depict these distributions for two job spells: 1) the job spell subject to the tax cut (red solid line), and 2) the previous (noneligible) job spell (blue dashed line). Two main findings emerge from this figure. First, the figure shows that tax incidence is on firms: the *gross* wage postreform distribution has shifted left, while the *net* wage distributions strongly overlap. Second, it shows that this tax-incidence result is homogeneously distributed along the wage distribution. This suggestive evidence implies that the policy could have been effective in raising demand for female workers, but not in increasing workers’ take-home wages.

3 Labor Market Effects of the Payroll Tax Cut

This section studies the labor market effects of the payroll tax cut on female hires. I first set up a simple conceptual framework that illustrates the expected impacts on workers’ employment and wages (section 3.1). I then provide empirical evidence on the impact of the policy using both aggregate (section 3.2) and microlevel (section 3.3) analyses.

3.1 Conceptual Framework

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 Hamermesh 1979; Fullerton and Metcalf 2002; Saez et al. 2019).

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. With the introduction of a tax cut for female hires, (eligible) female workers would be cheaper than male hires (or than noneligible female hires). A profit-maximizing firm should then hire more (eligible) female workers and lay off male workers (or noneligible female workers). With an upward-sloping labor supply, these employment effects bid up the wage of (eligible) female workers until the cost of the two groups is equalized. Therefore, in equilibrium, the standard labor market model predicts a wage increase for female workers equal to the tax cut.

Yet this standard tax incidence result might be questioned for several reasons in practice. For instance, institutional or discriminatory-based wage rigidity, as well as frictions in costs of recruiting and laying off workers, would make labor demand less than infinitely elastic and thus prevent wages from adjusting. In the presence of such wage frictions, employee-borne tax cuts would be more effective in raising take-home earnings, while employer-side tax cuts would be more effective at increasing employ-

ment. Therefore, studying payroll tax incidence and whether gender-based payroll taxes can be effective in curbing gender gaps is ultimately an empirical question. I look at the effect of the policy on employment and wages, which are the key outcomes for assessing tax incidence and gender gaps.

3.2 Aggregate Analysis

To identify the labor market effects of the payroll tax cut, I propose multiple empirical approaches. This choice is motivated by the possibility of shedding light on the effectiveness of payroll tax cuts 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 three empirical approaches. First, I implement an event-study approach to compare labor market outcomes between municipalities eligible for EU structural funds with those that are noneligible ([Section 3.2.1](#)). Second, I perform a triple difference approach, comparing outcomes over time, birth cohorts, and municipalities. This strategy rests on the fact that the minimum nonemployment duration requirement discontinuously drops by 12 months ([Section 3.2.2](#)) in nondisadvantaged municipalities for women older than 50. Third, I implement a regression discontinuity (RD) approach that exploits the cutoff rule determining earlier eligibility (6 months instead of 24) for male-dominated occupations ([Section 3.2.3](#)).

3.2.1 Cross-Municipality Analysis

I start by presenting an event-study approach that compares labor market outcomes between *disadvantaged* and *nondisadvantaged* municipalities. Ignoring, for now, any differential cross-occupation exposure and focusing on women younger than 50, the minimum nonemployment duration requirement differs by 18 months across these groups of municipalities (24 versus 6 months).

This exercise is similar in spirit to the graphical evidence presented in Panel B of [Figure 1](#), comparing the take-up across municipalities, but a formal event-study approach is valuable for many reasons. In particular, I can go beyond the mechanical employment effects suggested by the divergence in take-up rates across municipalities. The event-study approach allows me to estimate the policy effect that incorporates several margins of behavioral responses, including intensive margin responses, job duration, firm responses (e.g., substitution between eligible and noneligible workers), and other general equilibrium effects. I can look at these effects separately by gender, which is key for discussing the gender inequality implications of the policy. From a statistical point of view, the event-study approach allows me to test whether the two groups of municipalities were on similar trends before the reform and to investigate the dynamics of both employment and wage changes. If, for instance, wage effects take time to

materialize, the event-study approach allows me to scrutinize the timing of the adjustment.

I first collapse wages and workers' count by municipality and year; then I run event-study specifications as the following:

$$\log(y_{m,t}) = \sum_{j \neq 2012} \beta_j \cdot 1(m \in \text{Disadvantaged}) \cdot 1(t = t_j) + \gamma_m + \delta_{t,r(m)} + u_{m,t}, \quad (1)$$

where $y_{m,t}$ is a labor market outcome observed in municipality m at year t . The interaction between a dummy for disadvantaged municipalities and years, $1(m \in \text{Disadvantaged}) \cdot 1(t = t_j)$, omits the year before the reform (denoted by $j = 2012$), so that the event-study coefficients β_j can be interpreted as the treatment effect at year t relative to the year before the reform. In the absence of differential preexisting trends across the two groups of municipalities, $\beta_j = 0$ all $j < 2012$. Identification of the β_j coefficients rests on the assumption that observations from disadvantaged municipalities can be used as a counterfactual for observations from nondisadvantaged municipalities. Since trends in labor market outcomes are likely to differ geographically, I augment Equation (1) by including macro region-year fixed effects, $\delta_{t,r(m)}$.¹⁵ Municipality fixed effects, γ_m , account 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 the net-of-payroll tax elasticity, ϵ , by running a two-stage least squares (2SLS) regression specification of the following form:

$$\log(y_{m,t}) = \epsilon \cdot \log(1 - \tau_{m,t}) + \gamma_m + \delta_{t,r(m)} + u_{m,t}, \quad (2)$$

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 disadvantaged municipalities and a postreform-period dummy, $1(m \in \text{Disadvantaged}) \cdot 1(t \in \text{Post})$. The payroll tax rate, $\tau_{m,t}$, is the preferential tax rate for disadvantaged municipalities at time $t \geq 2013$ and the regular tax rate for nondisadvantaged municipalities at any t and for disadvantaged municipalities at $t < 2013$. The elasticity estimate, ϵ , is the Wald ratio of the DiD of the log of workers count or net wages ("reduced form") to the DiD of the log net-of-tax rate ("first stage").

Figure 3 displays the β_j event-study coefficient estimates, computed from Equation (1). The top panels show employment effects; the bottom panels depict net wage effects. Each figure also displays the corresponding average treatment effect on eligible

¹⁵Specifically, I interact year dummies with dummies for the following three macroregions: 1) Northeast, 2) Northwest, and 3, Center-South. Central and southern Italy are jointly considered, as the treatment does not present enough variation across municipalities in southern Italy. Likewise, other finest-level interactions, such as province- or region-year fixed effects, are not feasible, since there are only a few cases in which the treatment varies across municipalities within a given region or province.

municipalities, computed as the difference-in-differences (DiD) coefficient estimate on the postreform and treatment group interaction and the net-of-payroll tax elasticity.

The figure provides three main findings. First, there is compelling evidence of employment responses: female employment increases by 3.3 percent, on average, in disadvantaged municipalities compared to nondisadvantaged municipalities. Employment growth built up over the first four years and then stabilized. This effect translates into a net-of-payroll tax elasticity of 0.259. The gradual buildup of the treatment effect mirrors the evolution in the take-up rate across municipalities. Second, there is no significant impact on female wages: the event-study coefficient estimates depict a clear pattern of zero estimates over the entire postreform period. Third, the female employment increase did not come at the expense of male employment: Panel B shows that male employment did not significantly respond to the reform. Taken together, these results suggest that payroll tax incidence is mostly on firms.

In Appendix [Figure B3](#), I use the event-study approach to investigate the impact of the policy on other labor market outcomes, such as job-spell duration, new hires (i.e., workers entering the labor market for the first time), and days of work, separately by gender. The results illustrated in this figure lead to three main findings. First, the lack of substitution between female and male workers is also confirmed when looking at the average duration of a job spell. Second, some of the female employment increase comes from women starting their first regular employment spell: I estimate an increase of 4.4 percent in the count of new female hires in disadvantaged municipalities over the postreform period. Finally, the female employment increase indicates an actual increase in aggregate days of work, rather than reflecting some substitution between part-time versus full-time jobs. I detect an increase of around 3.1 percent in days of work supplied by female workers over the postreform period in disadvantaged municipalities, while the impact for men is negative (−0.8 percent), but only marginally significant.

To assess the welfare implications of the policy, another key margin is whether the tax cut reduces the duration of unemployment insurance benefits. Since the tax cut does not directly alter the compensation for unemployment, it is not a priori obvious whether the tax cut would affect UI benefits duration. From an employer's perspective, the lack of incidence on workers' wages would imply an increase in demand for (eligible) unemployed women. 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 met, and to leave welfare and find a job only once UI expires. In this case, given that the maximum benefit duration is larger than the nonemployment-duration cutoff determining eligibility for the payroll tax cut, we should expect limited, if any, effects.

I investigate this question in Appendix [Figure B4](#), which depicts the event-study co-

efficient estimates obtained by running Equation (1) on two outcomes: 1) UI payments (in euros), and 2) UI duration (in days). The figure provides clear evidence that the tax cut reduces UI benefits payment, thanks to a significant reduction in the time spent on welfare. After the tax-cut inception, women in disadvantaged municipalities spend around 30 days less (6.2 percent) on welfare compared to those located in nondisadvantaged municipalities. This effect translates into a UI benefit savings of about 747 euros per worker in each postreform year in disadvantaged municipalities. This result implies that the payroll tax cut reduced the fiscal externalities of unemployment benefits: by receiving benefits for a shorter period of time, women release resources that can be invested in other public budget items—offsetting, at least in part, the revenue losses from the tax cut.

3.2.2 Cross-Cohort Analysis

A second empirical approach consists in comparing labor market outcomes over time by birth cohort and municipality. Namely, I exploit the fact that the minimum nonemployment duration requirement drops from 24 to 12 months as a woman turns 50 in nondisadvantaged municipalities. These sources of variation allow me to implement a triple-difference approach that compares labor market outcomes across three differences: 1) women younger versus older than 50, 2) women before and after the policy implementation, and 3) women in nondisadvantaged versus disadvantaged municipalities. This strategy accounts not only for the fact that labor market outcomes can systematically differ across municipalities, but also for any possible factors or policy changes that would lead labor market outcomes to discontinuously change at age 50.¹⁶

I report the results of this exercise in [Figure 4](#), which depicts the difference in employment (top panel) and net wages (bottom panel) between the two groups of municipalities and by birth cohort, focusing on workers between the ages of 46 and 53. Each line in the graphs refers to a single postreform year, and each outcome is normalized with respect to 2012. The vertical axis displays the outcome difference across municipalities by age and year relative to 2012. The figure also reports the triple-difference coefficient estimates and net-of-payroll tax elasticity estimates using a model that includes cohort fixed effects, age fixed effects, and municipality fixed effects.

The figure provides evidence in line with the results emerging from the cross-municipality approach: there is a gradual and persistent increase in female employment, but no effect on take-home wages. Female employment increases by around 2 percent over the postreform period, which translates into a net-of-payroll tax elasticity of 0.14. By contrast, I do not uncover any significant effect on male outcomes.

The triple-difference approach rests on the assumption of parallel trends over the prereform period. I validate this assumption in [Appendix Figure B5](#), which depicts

¹⁶One example is differences in potential UI benefits duration (see, e.g., [D’Ambrosio and Scrutinio 2022](#)).

pretrends in each outcome variable since 2005. The graphs show the absence of any preexisting discontinuity at age 50. In Appendix [Figure B6](#), I use the triple-difference approach to investigate the impact of the policy on job-spell duration, new hires, and days of work, separately by gender. The results are again similar to those estimated using the cross-municipality approach, and they point to a significant increase in aggregate days of work supplied by female workers (+1.4 percent). Unsurprisingly, the impact on new hires is not statistically significant, since it is the sample composed of older individuals.

3.2.3 Cross-Occupation Analysis

The third approach exploits the cutoff rule favoring male-dominated occupations: namely, among women younger than 50 and living in nondisadvantaged municipalities, the minimum nonemployment duration requirement drops 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.

I implement a regression discontinuity (RD) design to leverage this source of cross-occupation variation in exposure to the payroll tax cut. This strategy has the advantage of evaluating the effectiveness of a payroll tax cut in favoring the entry of female workers into male-dominated occupations, where there could be entry barriers due to gender biases. Following [Gelman and Imbens \(2019\)](#), I run local linear regressions within a given bandwidth of the treatment cutoff, controlling for the running variable (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. \(2014\)](#). Formally, I run regressions as the following:

$$\Delta y_{o,t} = \beta \cdot 1(Gap_{o,t-2} \geq C_{t-2}) + \gamma \cdot (Gap_{o,t-2} - C_{t-2}) + \delta \cdot (Gap_{t-2} - C_{t-2}) \cdot 1(Gap_{o,t-2} \geq C_{t-2}) + \Delta u_{o,t}, \quad (3)$$

where the outcome variable, $\Delta y_{o,t}$, is the first difference in a labor market outcome variable in occupation o at year t .¹⁷ $1(Gap_{o,t-2} \geq C_{t-2})$ is an indicator for payroll-tax-cut eligibility after 6 months of nonemployment (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 outcome variable of interest. Finally,

¹⁷Given that the running variable is a *year-varying* function of the gender employment gap, I can only identify short-time (year-to-year) variation in the outcome variable. 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—could become treated in a successive year.

$\Delta u_{o,t}$ is the error term. Standard errors are clustered by occupation.¹⁸

Figure 5 presents the RD graphs, along with the RD coefficient estimates and standard errors. The top panel depicts the variation in the female-male employment ratio; the bottom panel, the variation in the female-male wage ratio. The left-hand-side graphs focus on nondisadvantaged municipalities, where the eligibility criteria create a discontinuity across occupations; the right-hand-side graphs focus on disadvantaged municipalities, where eligibility does not vary by occupation, and thus provide a placebo test. The vertical axis is the first difference in the outcome variable of interest. The horizontal axis is the distance from the cutoff (i.e., $1.25 \times$ average gender employment gap defined as $t = -2$). Each scatter point in the graphs is the sample average in the outcome variable over intervals of two cutoff-point bins. The red diamonds depict scatter points relative to “male-dominated” occupations; the blue circles relative to “non-male-dominated” occupations.

Three main remarks emerge from this figure. First, the graph in Panel A shows a discontinuity in the share of female workers in male-dominated occupations operating in nondisadvantaged municipalities. I estimate a β coefficient of 0.021, suggesting that the share of female workers in male-dominated occupations grows by 2.1 percentage points each year. Panel B presents no significant effect across occupations operating in disadvantaged municipalities. This result suggests that the payroll tax cut was effective in curbing the gender employment gap among male-dominated occupations.

Yet the graph also shows that this is a local effect: the impact is not clear among occupations that are located further away from the cutoff (and that started with an extremely large gender employment gap). Namely, mapping the scatter points with the associated occupations (see Table A1), the positive effect is mostly located among occupations such as business and administration professionals, fixed machinery operators in the food industry, and stationary plant and machine operators, but the effect on highly male-dominated occupations, such as science and engineering professionals or metalworkers, is limited.

Finally, the cross-occupation analysis confirms the cross-municipality and cross-cohort evidence on tax incidence: the tax cut does not have any significant bearing on net wages. Panel C provides no evidence of a discontinuous change in female wages in male-dominated occupations. Figure B7 presents the RD graphs on job duration, new hires, and days of work. The graphical evidence suggests a positive impact on female days of work (in line with the increase in female employment), whereas my estimates yield no statistically significant effect on the other outcomes.

The main implication emerging from this analysis is that an employer-borne payroll tax cut is effective in tackling the gender employment gap only among male-

¹⁸The RD estimate rests on the assumption that there is no manipulation in the running variable. Figure B8 shows that the density of the gender employment gap is smooth around the cutoff, as would be expected in a valid RD design; the McCrary discontinuity estimate is 0.452 (0.579).

dominated occupations. Yet in occupations that are *extremely* male-dominated, a payroll tax cut is perhaps not the best policy to tackle gender inequalities, either for the pervasive presence of gender-identity norms (Akerlof and Kranton 2000) or for structural (“non-family-friendly”) characteristics of these occupations (Goldin 2014; Olivetti and Petrongolo 2016).

3.3 Microlevel Analysis

While the aggregate analyses show transparent evidence that the tax cut raises female employment without affecting net wages, it is useful to develop an individual-level microeconomic framework to estimate the size of the labor market responses.

I propose two regression models to leverage microlevel variations in payroll tax-cut eligibility. First, to quantify the size of the pass-through on workers’ wages, I exploit the fact that a payroll tax cut’s eligibility varies discontinuously by firm tenure: it applies for up to 12 months for temporary jobs, 18 months for permanent jobs. I perform a difference-in-differences (DiD) approach, comparing the evolution in wages before versus after crossing the firm’s tenure cutoff, and across payroll tax-cut-eligible versus noneligible jobs.

Second, I create an individual-specific treatment that can incorporate all the sources of payroll tax-cut eligibility presented above. By combining the period of nonemployment from two consecutive job spells with the factors determining earlier eligibility (i.e., municipality of residence, age, and occupation), I can leverage within-individual over-time variation in eligibility. This approach allows me to create variation in eligibility even across women in a given municipality-occupation-age-month cell.

There are two key advantages of these microlevel analyses. First, the definition of treated and control groups in the aggregate analyses is based on differences in *exposure* to the payroll tax cut. As eligibility varies along several nonmutually exclusive dimensions, it is likely that the treatment definition contained measurement errors. The microlevel analysis overcomes this issue by exploiting variation in actual eligibility. Second, the microlevel analyses account for the fact that treatment effects can be contaminated by any policies or shocks that differentially hit municipalities, cohorts, or occupations with differential exposure to the payroll tax cut. Namely, it allows me to focus on a panel of individuals and to include a plethora of fixed effects, including individual, municipality-by-year, occupation-by-year, and cohort-by-municipality fixed effects.

3.3.1 Estimating Tax Incidence From Variation in Eligibility by Firm Tenure

In this section, I present a microlevel approach, exploiting the fact that the payroll tax cut is time limited. As the duration cutoff depends on the type of job (i.e., permanent versus temporary), I first center each time in the data set 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 firm-tenure months) from crossing the duration cutoff. I then collapse net and gross wages into firm tenure bins centered at the duration cutoff.

I compare the evolution in gross and net wages across (eligible and noneligible) jobs, and before versus after crossing the cutoff determining tax-cut eligibility. In this way, I can evaluate tax incidence by assessing whether there is a discontinuous change in gross or net wages as the preferential tax scheme expires in the job for which the tax cut applies, compared to noneligible jobs. I define a “noneligible” job as the (worker-specific) previous job before the reform.¹⁹ If tax incidence is on firms, I should see a discontinuity in gross wages, but no changes in net wages.

Figure 6 presents the results. The top panel shows the evolution in *gross wages*, while the bottom panel depicts *net wages*. The horizontal axis displays the evolution in these two variables over (normalized) firm tenure. For each panel, the figure reports series for the job that started with the preferential payroll tax scheme (red circles) and the previous (noneligible) job spell (blue squares). The figure also reports the DiD coefficient estimate obtained from regressions of the following form:

$$\log(y_{i,t,j}) = \beta \cdot 1(t < C) \cdot 1(j \in Eligible) + \gamma_{i,t} + \delta_j + u_{i,t,j} \quad (4)$$

where $y_{i,t,j}$ represents wages, gross or net of the employer payroll tax, of worker i at her t^{th} firm-tenure month in 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 eligible for the payroll tax cut, $1(j \in Eligible)$. One caveat is that the payroll tax scheme can affect job duration and distort the job duration distribution across eligible and noneligible jobs (although the aggregate analyses show that this effect was limited).²⁰ This issue is alleviated by using individual-firm tenure 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-firm-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 wages earned across eligible and noneligible jobs. These fixed effects account for individual-specific unobservables and any other factors, including job-specific characteristics, that might induce wages to change over time within a given job. The coeffi-

¹⁹This identification strategy implies that I can only focus on women hired with the preferential payroll tax scheme that have been employed before, who represent 77.8 percent of the subsidized hires.

²⁰A potential effect is bunching responses at the firm tenure cutoff. Because the tax cut creates a “notch” in the budget constraint of employers (that is, a discontinuity in the choice set of labor cost versus job duration), employers can minimize labor costs by offering job contracts ending exactly when the tax cut expires. I analyze bunching responses in the previous working-paper version of this study (Rubolino 2022). Although statistically significant, I find only small evidence of excess bunching at the tenure cutoff.

cient of interest, β , measures the percentage change in wages due to the tax cut. Finally, $u_{i,t,j}$ is the error term. I cluster the standard errors at the individual level.

The figure provides clear evidence that tax incidence is on firms. The top panel shows a clear discontinuity in gross wages at the cutoff for the job that started with the preferential tax scheme, while the series relative to the previous job spell is continuous. By contrast, the bottom panel displays a continuous series for both of the two jobs. The DiD coefficient estimates suggest that, on average, gross wages drop by around 8.3 percent, while net wages grow by 0.8 percent, but this increase was not statistically significant at the usual confidence intervals. This result provides clear evidence that employers do not adjust wages in response to the payroll tax cut.

Appendix B.1 tests the robustness of this result and also tests for heterogeneous responses. In particular, I estimate the “pass-through” of the tax cut to workers’ wages on selected samples of the payroll tax-cut recipient population, such as those that changed employer or workplace municipality, and heterogeneity by skill level, industry, and employer gender. These heterogeneity analyses allow me to test for several potential explanations for the tax incidence result, including the fact that wages do not change because of the presence of implicit contracts whereby firms offer the same wages as the previous job. Despite differences in magnitude, I find that the lion’s share of the payroll tax cut is always held by firms.

3.3.2 Employment Responses From Variation in Eligibility by Nonemployment Duration

To estimate microlevel employment responses, I draw on monthly data to create an individual-specific treatment. By combining the period of nonemployment (in months) from two consecutive job spells with the factors determining earlier eligibility (i.e., municipality of residence, age, and occupation), I create a dummy variable, $D_{i,t}$, equal to 1 as individual i meets the minimum nonemployment duration requirement at time (month) t ; 0 otherwise. I then compare within-individual variation in employment status and payroll tax-cut eligibility by running DiD equations of the following form:

$$y_{i,t} = \alpha \cdot D_{i,t} + \beta \cdot D_{i,t} \cdot 1(t \in Post) + \gamma_i + \delta_t + u_{i,t}, \quad (5)$$

where $y_{i,t}$ defines employment: it is equal to 1 if individual i works a positive number of days during a month t ; 0 otherwise. The treatment status is given by the interaction between a dummy for the postreform period, $1(t \in Post)$, and a dummy for payroll tax-cut eligibility, $D_{i,t}$.²¹ Individual fixed effects, γ_i , account for any time-invariant individual-specific factors. Month fixed effects, δ_t , account for any month-level shocks

²¹Specifically, $D_{i,t}$ is set to 0 in each month t for which an individual i is not observed in social security data. This approach has the limitation of not covering possible transitions toward public employment or self-employment. Therefore, the dependent variable will only capture employment in the private sector that is available in the social security archives.

or policy changes. The coefficient of interest, β , computes the percentage change in the probability of entering employment for workers eligible for the tax cut during the postreform period. Finally, $u_{i,t}$ is an error term. Following the three-group criteria determining eligibility, I use three-way clustered standard errors by municipality, birth cohort, and occupation.

The top panel in [Table 2](#) shows the β coefficient estimates from variants of Equation (5), while the bottom panel presents the effects in elasticity terms by regressing employment on the net-of-payroll tax rate, instrumented by the interactions between the eligibility dummy and the postreform period dummy. I start with a simple model containing individual fixed effects and time fixed effects. I estimate an employment increase of 5.4 percentage points over the postreform 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: 1) municipality of residence, 2) occupation, and 3) age group (above or below 50). These interactions reduce the chances that unknown shocks or policies that differentially affect women of 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 being employed 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 include interactions between municipality, age group, and occupation fixed effects. Coefficients are precisely estimated and are not sensitive to the inclusion of these fixed effects. Finally, in column 7, the individual fixed effects interact with the occupation fixed effects. This interaction accounts for the potential sorting of eligible individuals into male-dominated occupations, which are subject to less-binding eligibility criteria. I find that sorting explains little, if any, of the estimated impact.

3.4 Taking Stock

Overall, the broad picture emerging from different identification strategies is that the reform increases female employment, but that it does not raise take-home wages. However, there are some differences in coefficient magnitude across empirical approaches. In particular, the aggregate analyses suggest a slightly larger impact on female employment compared to the microanalysis: the cross-municipality analysis suggests an elasticity of around 0.26 and the cross-cohort elasticity is 0.14, while the microelasticity estimate is 0.11. This divergence between micro- and macroelasticity estimates is not a novel occurrence in economics (see, e.g., [Chetty et al. 2013](#)), and it can be rationalized by several factors. For instance, the aggregate analyses incorporate externalities to other noneligible female workers, both intensive and extensive margin responses, and changes in job duration, while the microanalysis only measures the extensive margin

response of treated women.

These effects suggest that payroll tax incidence is on firms. Although it is at odds with the received wisdom in public economics, my results fit in with a recent stream of the empirical literature ([Saez et al. 2012](#); [Egebark and Kaunitz 2018](#); [Cahuc et al. 2019](#); [Saez et al. 2019](#); [Benzarti and Harju 2021a](#); [Benzarti and Harju 2021b](#); [Saez et al. 2021](#)). These results provide the first evidence of the potential for gender-based payroll taxes to curb gender inequalities. The increase in female employment and the absence of any crowding-out effect on male employment maps into a reduction in the gender employment gap, which I will discuss in Section 5. The increase in female labor demand in gender-imbalanced places and occupations suggests that gender stereotypes and prejudices are not sticky, but rather that they are malleable and can be shaped through ad hoc policies. By making gender discrimination more costly, the payroll tax cut can thus tackle discrimination by “taxing” gender-prejudiced employers, thus leading them to increase their demand for female labor.

4 Firm-Level Evidence

The previous analyses have provided clear evidence that the incidence of the tax cut is on firms: employers pocket the tax cut and benefit from lower labor costs by hiring (eligible) female workers. The second part of the empirical analysis, therefore, focuses on how firms respond to the tax cut.

4.1 How Does the Payroll Tax Cut Affect Firm Outcomes?

The response of firms to the tax cut depends on two main channels. First, the tax cut generates a large cash flow to firms with a higher share of subsidized hires. On average, my tax incidence results imply that a firm would reduce labor costs by around 1,907 euros per year by hiring an eligible worker (i.e., 50 percent of her employer payroll tax burden and around 11.5 percent of her total salary). If firms are credit constrained, this cash windfall would lead firms to grow. Studies in the corporate finance literature (see, e.g., [Fazzari et al. 1988](#)) have shown that cash windfalls significantly affect firms’ performance. Some recent studies have shown that payroll taxes significantly affect firm-level outcomes. For instance, [Saez et al. \(2019\)](#) provide clear evidence that the tax windfall generated by the payroll tax cut for young workers in Sweden significantly improved business performance. Likewise, [Benzarti and Harju \(2021b\)](#) find that payroll taxes affect firm-level production and input factor choices, while [Benzarti and Harju \(2021a\)](#) show that payroll tax cuts make firms more resilient during downturns by relaxing liquidity constraints.

A second channel is that the tax cut makes gender discrimination more costly. Since the seminal work by [Becker \(1957\)](#) on labor market discrimination, researchers have

assumed that discrimination is the product of personal prejudice. If gender prejudices prevented the integration of female workers, the tax cut would reduce discrimination by “taxing” gender-prejudiced employers. 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 distort beliefs and lead discriminatory firms to underperform in the market. By increasing the cost of gender discrimination, the tax-induced increase in female employment can improve business performance, particularly in more gender-biased firms, which used to underperform in the market.

4.2 Empirical Strategy and “First-Stage” Relationship

To study firm-level responses, I exploit the persistent between-firm variation in the share of the female workforce before the reform, as done in previous studies.²² This strategy takes advantage of a simple empirical fact: there is a robust and positive association between the firm-level prereform share of the female workforce and the policy take-up rate (i.e., the firm-level workforce share of subsidized hires).

Figure 7 compares the firm-level preexisting share of the female workforce (horizontal axis), computed over the full prereform period, with the take-up rate (vertical axis), averaged over the whole postreform period. Each panel in the figure depicts this relationship in 100 equal-size bins and shows the line of best fit. Panel A depicts the raw correlation; it shows that a 10 percentage point increase in the prereform share of the female workforce increases the take-up rate by 2.85 percentage points. In Panel B, I depict the within-region association, plotting the residuals from an OLS model with region fixed effects. The relationship remains strong. Panel C shows that the slope is similar when exploiting within-industry variation, thus accounting for any systematic cross-industry heterogeneity. Finally, Panel D presents the residuals from an OLS model with both industry and region fixed effects. The graph shows that this relationship holds even across firms operating in the same region and industry.

I implement a two-stage least squares (2SLS) model, where various firm-level outcomes are regressed on the (instrumented) share of subsidized hires. Specifically, for each firm i in year t , I run systems of equations of the following form:

$$TakeUpRate_{i,t} = \alpha \cdot (ShareFemalePre_i \cdot Post_t) + \eta_i + \rho_t + v_{i,t}; \quad (6)$$

$$y_{i,t} = \beta \cdot \widehat{TakeUpRate}_{i,t} + \gamma_i + \delta_t + u_{i,t}. \quad (7)$$

where the outcome variable, $y_{i,t}$, is measured for firm i at year t . The treatment

²²In their study of a hiring subsidy for low-wage workers in France, Cahuc et al. (2019) propose a 2SLS strategy that uses the prereform share of low-wage workers as an instrument for the share of subsidized hires. Similarly, in the context of the Swedish payroll tax cut on young workers, Saez et al. (2019) take advantage of variation across firms in the share of young workers before the reform.

variable of interest, $TakeUpRate_{i,t}$ is the firm-level share of subsidized hires. The “first-stage” equation regresses the take-up rate on the interaction between the prereform share of the female workforce (computed over the whole prereform period within a firm) and a dummy for the postreform period, $ShareFemalePre_i \cdot Post_t$. Since I define $ShareFemalePre_i$ for multiple years, the instrument is robust to potential mean-reversion bias, which could occur if the definition had been based on the firm portion of the female workforce only in the year before the reform. η_i and γ_i are firm fixed effects, while ρ_t and δ_t are year fixed effects, which account for any time-varying shocks and firm-specific time-invariant characteristics. Finally, $v_{i,t}$ and $u_{i,t}$ are random error terms. Because outcomes are likely to be correlated within a firm, I account for any dependence between observations within a firm by clustering all regression results at the firm level.

The 2SLS estimator, β , calculates the local average treatment effect (LATE) of the policy take-up rate on the outcome variable of interest. β yields the causal effect of the policy on financial outcomes under three main assumptions. First, the ex-ante share of the female workforce and its interaction with the postreform dummy significantly predict the take-up rate. I validate the relevance condition of my instrument by examining the “first-stage” relationship. Column 1 of [Table 3](#) shows that a 1 percentage point increase in the firm-level prereform share of the female workforce increases the take-up rate by 0.267 percentage points. This result implies a strong first-stage relationship.

Second, the interpretation of the β estimate as a weighted average of compliers’ treatment effects rests on the monotonicity assumption: the take-up rate linearly increases by prereform share of the female workforce. A simple check to assess whether the monotonicity condition is satisfied can be performed by examining whether there is a monotonic relationship between the take-up rate and the instrument. The scatterplots in [Figure 7](#) show a clear linear relationship, suggesting that the monotonicity condition is likely to be satisfied in this setup.

Finally, there could be other (non-tax-cut related) year-varying shocks that are correlated with the prereform share of female employment. I assuage these concerns in two ways. First, I control for several additional fixed effects, including firm size, region, and industry. Second, I propose an event-study approach that compares firm outcomes by prereform share of the female workforce. These alternative specifications lend credence to the fact that the effects I uncover here are indeed due to the tax cut.

4.3 The Effect of the Payroll Tax Cut on Firm-Level Outcomes

This section presents the effects of the tax cut on several firm-level outcomes. In benchmarking the treatment effect, I express the coefficient estimate in terms of a 10 percentage point increase in the take-up rate. This fairly approximates the treatment effect generated by hiring an eligible worker in the median firm, which has a workforce of

around nine or ten employees.

4.3.1 Firm-Level Employment Effects, Business Activity, and Workers' Wages

Table 3 presents the 2SLS coefficient estimates of the take-up rate (multiplied by 10) on various firm-level outcomes. I start by examining firm-level employment effects: the impact of the policy on the firm-level (log of) employees' count (column 2). The coefficient estimate is positive and statistically significant: a 10 percentage point increase in the take-up rate would increase the firm-level workforce by nearly 4 percent. Given a prereform average of 16 workers, this effect suggests that a 10 percentage point increase in the firm-level share of subsidized hires would boost firm-level employment by around 0.64 workers each year over the postreform period. This result further confirms that the tax cut stimulated employment.

In column 3, I look at the firm's total production value (logged). The estimate suggests that a 10 percentage point increase in the take-up rate raises production value by around 1.7 percent. This result suggests that the reduction in labor costs driven by the tax cut was successful in boosting business activity. In column 4, I estimate the effect on firm logged value-added. The 2SLS result suggests that the payroll tax cut increases firm productivity, with precisely estimated coefficients. On average, an increase of 10 percentage points in the take-up rate raises firm value-added by about 0.9 percent.

Next, I study whether firms passed on some of the tax cut to (noneligible) workers through higher net wages (logged). This exercise allows me to study whether there is rent sharing of the tax windfall with incumbent workers. The coefficient estimate presented in column (4) implies that a 10 percentage point increase in the take-up rate raises workers' wages by around 0.2 percent. Given an average prereform net wage of 25,280 euros, this estimate suggests that each incumbent worker benefits, on average, from a 56.8 euro wage increase per year. A simple back-of-the-envelope calculation suggests that rent sharing with incumbent workers accounts for around 48 percent of the total payroll tax-cut windfall.²³

4.3.2 Firm-Level Profitability

In columns 6–8, I present the effect of the payroll tax cut on three firm-level profitability ratios. First, I focus on returns on assets (ROAs), computed by scaling the firm net income by its average total assets. This index measures how well a company is generating profits from its total assets. I estimate that a 10 percentage point increase in the take-up rate raises the ROA by nearly 0.4 percentage points, which accounts for around 10 percent of the average prereform ROA.

²³This is computed by subtracting from the firm-level tax windfall (1,907 euros) the average net wage increase for incumbent workers (multiplied by the average workforce) = $56.8 * 16 = 908.8$ euros. A fully fledged incidence assessment of the tax windfall on incumbent workers is beyond the scope of this paper.

Second, I compute the firm-level return on equity (ROE), calculated by dividing the firm net income by its shareholders' equity. The higher the ROE, the better a firm is at converting its equity financing into profits. I find that a 10 percentage point increase in the take-up rate raises the ROE by about 1.2 percentage points. This is a large impact, accounting for around one-third of the prereform ROE.

Finally, I study the impact on the return on investment (ROI), which is firm net income divided by the total annual investment costs. I find a positive effect of the payroll tax cut on the ROI (by around 0.7 percentage points), but it is only marginally statistically significant at the usual confidence intervals.

Taken together, the firm-level analysis has shown that the tax windfall generated by the payroll tax cut improves business performance and firm profitability. Firms with a higher take-up rate enjoy higher , both on their assets and from shareholders' equity.

4.4 The Role of Gender Discrimination

The seminal theory of Gary Becker on discrimination supposes that gender-biased employers prefer to hire male workers even if their wages exceed those of women with a similar level of productivity (Becker 1957). To indulge their preferences, discriminatory employers would then face higher labor costs compared to nondiscriminatory employers, harming their profits. Consistent with these predictions, previous empirical research has shown that discriminatory employers hire more men (Ashenfelter and Hannan 1986), favor men in terms of wages and promotions (Black and Strahan 2001), and have shorter survival rates (Weber and Zulehner 2014). In a recent correspondence experiment, Kline et al. (2022) show that discriminatory behaviors among large U.S. employers are negatively correlated with firm profitability.

The payroll tax cut on female hires can exacerbate these firm-level financial distortions by making gender discrimination more costly. If employers then respond to the tax incentive by hiring female workers, the policy take-up marginal impact on firm outcomes can be relatively larger among firms with more severe gender biases. The rationale goes as follows: If gender biases created barriers to female employment in some firms, then gender-biased firms would be systematically less productive and profitable than nondiscriminatory firms. After the policy, there will be an even larger loss for firms preferring male to (similar but cheaper) female hires, leading them to readjust their hiring choice in favor of more female hires. Then, for a given take-up rate (and a given "mechanical" reduction in labor costs due to the tax windfall), the marginal impact on firm-level outcomes, such as profitability and productivity, would be relatively larger among gender-biased firms.

4.4.1 Measuring Gender Biases

The measurement of gender biases is the main challenge to test this prediction. Ideally, one would like to observe gender biases at the firm level, but this turns out to be quite challenging. Instead, I focus on measuring gender biases at the industry level, which is a strong predictor of discriminatory behaviors. For instance, [Kline et al. \(2022\)](#) finds that the two-digit industry explains roughly half of the cross-firm variation in racial and gender gaps in employer response to a fictitious résumé in the U.S.

I elicit industry-level gender biases using a nationwide survey on discriminatory behaviors in Italian society, conducted by the Italian National Institute of Statistics in 2011 (called *Indagine sulle discriminazioni in base al genere, all'orientamento sessuale, all'appartenenza etnica*). Following previous studies (see, e.g., [Bertrand 2020](#)), I proxy beliefs about gender roles as the share of workers that agrees with the view that “when jobs are scarce, men have more of a right to a job than women.” The data allow me to compute this index (hereafter “gender bias index”) for each two-digit industry. There is a large variation in the gender bias index across industries, ranging from more than 45 percent in the mining and agricultural sector to around 10 percent among professionals and the real estate sector. Gender biases are strongly associated with the share of the female workforce in an industry (see [Figure A3](#)), with a correlation coefficient of -0.46 .²⁴

4.4.2 Marginal Effects by Gender Bias

I estimate the marginal impact of the policy take-up by the gender bias index. [Figure 8](#) relates the industry-level 2SLS coefficient estimate (vertical axis), measuring the impact of a 10 percentage point increase in the (predicted) take-up rate, with the gender bias index (horizontal axis). Each scatter point in the graphs refers to a two-digit industry. The figure also reports the line of best fit and the estimated slope.

The figure provides two main findings. First, there is a positive association between the productivity gains generated by the policy and the gender bias index. Panel C shows a positive and statistically significant relationship between the 2SLS coefficient estimate on firm value-added and the gender bias index. The slope estimate suggests that, for a given level of take-up rate, a 1 percentage point increase in the gender bias index would lead to a 0.00475 coefficient estimate increase in the firm logged value-added. To put this estimate in perspective, this result suggests that the value-added impact of a marginal increase in the policy take-up rate on a stereotypically male industry such as the construction sector (gender bias index = 0.3) would be $14 * 0.00475 = 0.067$ points larger (6.7 percentage points) than the same take-up rate increase in a stereotypically female industry such as the health sector (gender bias index = 0.16).

²⁴Similar patterns emerge when focusing on other questions that have been proposed to measure gender attitudes, such as “being a housewife is just as fulfilling as working for pay,” or when collecting responses only from male workers.

What explains the fact that the marginal increase in firm productivity is larger in gender-biased industries? The results depicted in Panels A, B, and D allow me to rule out two possible explanations. First, this fact cannot be explained by the possibility that gender-biased firms are more likely to hire more workers or to expand their production: the slope estimate is not statistically significant, neither when looking at firm workforce nor at firm production value. Second, the productivity increase does not stem from a more generous rent-sharing response of gender-biased firms with their incumbent workers: Panel D shows a fairly flat relationship between the 2SLS coefficient estimate on incumbent net wages and the gender bias index.

A residual explanation would be that the tax cut-induced integration of female workers into gender-biased (and male-dominated) industries spurred productivity by reducing gender discrimination. This channel would be in line with some recent studies. Using a structural Roy model, [Hsieh et al. \(2019\)](#) point out that declining discrimination against women and Blacks raised U.S. aggregate productivity. These findings are corroborated by interpersonal contact theory ([Allport 1954](#); [Pettigrew and Tropp 2006](#)). This theory predicts that mixing groups will break down stereotypes and encourage between-group collaboration. In a recent experimental study, [Dahl et al. \(2021\)](#) show that exposure of women to men in a traditionally male-dominated context—the military in Norway—changed attitudes about mixed-gender productivity, without harming male performance.

A second finding emerging from this exercise is that the policy-induced profitability growth was relatively larger in gender-biased industries. Panels E through G show positive slopes, although with some noise in the case of the ROE and ROI measures. This result is in line with predictions from the classical [Becker \(1957\)](#) model. Gender-biased industries have much more to gain from the integration of female workers. By breaking down gender stereotypes, the tax-induced increase in female employment improves profits, particularly where gender biases are more severe.

4.5 Robustness Checks and Alternative Specifications

4.5.1 Other Time-Varying Policies and Shocks

The 2SLS identification strategy rests on the assumption that there are no other time-varying shocks or policies that differentially hit firms by share of female employment. To test for these potential issues, I augment the baseline 2SLS model by interacting the year fixed effects with several other fixed effects. I present the results in Appendix [Table B6](#). First, I include year-region fixed effects, which allow me to account for any differential trends across regions and absorb any region-specific shocks or policy changes (including the fact that the eligibility criteria vary across places). Column 2 shows that the coefficient estimates remain unchanged. In columns 3–5, I account for any underlying trends and shocks across firms based on their legal form, size, and indus-

try. The 2SLS estimates remain fairly similar and statistically significant at the usual confidence intervals. An exception is the coefficient estimates for the ROI and ROE outcomes, which become less precise when accounting for the full set of fixed effects' interactions.

4.5.2 Event-Study Estimates

Another threat to identification is that there could be other time-varying shocks or policies that differentially hit firms, depending on their prereform gender composition. To assuage these concerns, I implement an event-study approach that compares the evolution in firm-level outcomes for two groups of firms: 1) firms in the top quartile of the prereform female workforce distribution ("highly exposed" firms), and 2) firms in the third quartile ("fairly highly exposed" firms). As the prereform female workforce is a strong predictor of the policy take-up rate, I expect firm outcomes to be relatively more affected in "highly exposed" firms compared to "fairly highly exposed" firms. The main identifying assumption is that firm-level outcomes of firms with a higher prereform fraction of the female workforce would have followed the same trend as the outcomes of firms with a relatively lower prereform fraction of the female workforce.

Figure B9 presents the event-study coefficient estimates and 95 percent confidence intervals on the various firm-level outcomes, along with coefficient estimates and firm-level standard errors computed from a difference-in-differences model with firm and year fixed effects.²⁵ The graphs show that firm outcomes were evolving in a parallel fashion over the years leading to the reform. The graphs then show a clear and gradual increase after policy inception. These results provide reassuring evidence that the tax cut accounts for the effect I uncovered here. The gradual increase in the firm-level workforce is also consistent with the previous results from the aggregate analysis, showing that employment effects built up over time. However, the effect on the ROI is not clear, which is also consistent with the baseline 2SLS analysis showing a marginally significant effect.

4.5.3 Firm Survival

The tax cut can also affect firm survival: firms that face a larger relative cost in sticking with gender discrimination would be more likely to perish. If this is a likely behavioral response to the policy, then my estimates would provide a lower bound, since they would not account for the extensive margin effect of the tax cut on firm exit (or

²⁵Namely, the event-study coefficient estimates are computed from regressions of the following form:

$$\log(y_{i,t}) = \sum_{j \neq 2012} \beta_j \cdot 1(i \in \text{HighlyExposedFirms}) \cdot 1(t = t_j) + \gamma_i + \delta_t + u_{i,t}, \quad (8)$$

where $y_{m,t}$ is an outcome variable in firm i at year t . The treatment is given by interacting the dummy for "highly exposed firms" and years, $1(i \in \text{HighlyExposedFirms}) \cdot 1(t = t_j)$.

entry). I test whether my baseline estimates differ when I focus on a balanced panel of firms that span the period covered in the analysis. The latter sample allows me to uncover exclusively intensive margin responses, thus offering indirect evidence on the importance of extensive margin responses to explain the overall effect. I then run the 2SLS model on this balanced panel of firms. Appendix [Table B7](#) shows that the coefficient estimates obtained from the balanced sample are fairly similar to those calculated from the baseline (unbalanced) sample. This result suggests that the tax cut has limited, if any, effect on firm survival and/or entry.

5 Cost-Benefit Analysis and Gender Inequalities

The first part of this section presents a simple cost-benefit analysis that allows me to estimate the net costs of the policy. Based on the empirical results, the second part discusses the effect of differentiating payroll taxes by gender on gender employment and pay gap.

5.1 Cost-Benefit Analysis

This section evaluates the effect of the payroll tax cut for female hires on the government's net budget. [Hendren \(2016\)](#) shows that estimating a policy's impact on revenue is a sufficient statistic for welfare analysis when the envelope theorem holds and individual utilities are not affected by the small behavioral responses induced by the policy change. Whether the policy net cost is more or less than the budgetary cost depends on the fiscal externalities triggered by the policy.

I perform a simple back-of-the-envelope calculation to evaluate the net costs of the policy. The details of this calculation are described in [Appendix B.2](#). I account for four sources of fiscal externalities. First, the positive employment effects and the limited crowding-out effect on male employment would increase income and payroll tax revenue. Second, the positive effect on business growth would increase value-added taxes. Third, the reduction in time spent on welfare would reduce government spending to finance UI benefits. Fourth, the increase in incumbent workers' net wages raises payroll and income tax payments. Overall, these fiscal externalities will dampen the mechanical negative effect of missing payroll tax revenue.

I find that the policy budgetary cost is €1,907 per recipient. In total, the policy costs €22,265,113 in terms of missing employer payroll tax revenue. For each recipient, the positive employment effect generates €102 in income tax revenue, €144 in payroll tax revenue, €73 in value-added tax, €747 in spending savings to finance UI benefits, and €323 in income and payroll tax revenue from the incumbent workers' wage increase. Therefore, each recipient brings €1,389, which maps to a 73 percent self-financing rate. In other words, the preferential payroll tax scheme's net cost is 27 percent of the bud-

getary cost. This estimate suggests that payroll tax cuts are quite effective once fiscal externalities are accounted for.²⁶

5.2 The Impact of the Policy on Gender Inequalities

Each identification strategy proposed in this study has yielded the same result: the payroll tax cut increased female employment, but it did not raise the net wages of directly treated workers, nor did it affect male employment. Therefore, differentiating employer-borne payroll taxes by gender can be effective in reducing the gender *employment* gap, but it would leave the gender *wage* gap unchanged.

Figure 9 offers a graphical illustration of how the policy contributes to reducing the gender employment gap in Italy. The gender employment gap is calculated as the difference (in percentage points) between male and female employment rates, using series provided by the Italian Institute of Statistics (ISTAT) over the 2005–2020 period. The figure compares the evolution in the observed gender employment gap (solid line) with the counterfactual gender employment gap (dashed line) that would have been observed in the absence of the policy. The counterfactual gender gap is computed by scaling the observed gender gap by the female employment increase generated by the payroll tax cut.²⁷

The figure shows that the payroll tax cut on female hires absorbs a nonnegligible portion of the gender employment gap. For instance, the gender employment gap would have been 2 percentage points larger in the absence of the tax cut in 2020 (20.2 versus 18.2). However, the policy is not sufficient to permanently close the gender employment gap: a substantial portion of the gap still remains. This result suggests that the policy is effective in reducing the gender employment gap but is not sufficient, at least over the medium run, to entirely close the gap.

6 Conclusions

The labor market participation of women has spectacularly increased in recent decades. However, women's 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 through policy. This paper provides the first empirical evidence on the labor market impacts of differentiating payroll taxes by gender. Starting in 2013,

²⁶This result is in line with the assessment proposed by Cahuc et al. (2019). In their evaluation of a hiring credit for low-wage workers in France, Cahuc et al. (2019) find that the gross cost per job created is equal to 25 percent of the cost of a job. When the savings generated by job creation are considered, the net cost of the hiring credit per job created is equal to zero.

²⁷To compute the average employment effect, I use the most conservative microlevel estimate (see column 7 of Table 2). Since this is a static estimate, I transform the average employment effect into annual effects by normalizing the total effect by the year-specific employment increase yielded by the event-study estimates (see Figure 3).

the payroll tax rate paid by Italian employers for female hires was reduced by 50 percent. 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 and firm performance.

I find that employer-specific payroll tax cuts are sticky. Under such nonstandard tax incidence results, reducing employer payroll taxes increases employment, while not affecting net wages. In particular, the preferential tax scheme promotes the integration of women into traditionally gender-segregated places and occupations, without crowding out male employment. This result implies that employer-specific payroll tax cuts can reduce the gender *employment* gap even in contexts where gender attitudes are still traditional, but it is an undesirable policy if policymakers want to reduce the gender *wage* gap. Furthermore, I show that the payroll tax cut improves firms' performance and profitability by promoting the integration of female workers in traditionally male-dominated firms. This result suggests that governments have the power to reduce gender discrimination and spur business profitability by "taxing" gender-prejudiced employers.

References

- Akerlof, G. A. (1978). The Economics of "Tagging" as Applied to the Optimal Income Tax, Welfare Programs, and Manpower Planning. *American Economic Review*, 68(1):8–19. [1](#)
- Akerlof, G. A. and Kranton, R. (2000). Economics and Identity. *Quarterly Journal of Economics*, 115(3):715–753. [19](#)
- Alesina, A., Ichino, A., and Karabarbounis, L. (2011). Gender-Based Taxation and the Division of Family Chores. *American Economic Journal: Economic Policy*, 3(2):1–40. [1](#)
- Allport, G. W. (1954). *The Nature of Prejudice*. Addison-Wesley, Cambridge / Reading, MA. [29](#)
- Altonji, J. G. and Blank, R. (1999). Race and Gender in the Labor Market. In Elviesier, editor, *Handbook of Labor Economics*, Vol. 3, page 3143–3259. [6](#)
- Ashenfelter, O. and Hannan, T. (1986). Sex Discrimination and Product Market Competition: The Case of the Banking Industry. *The Quarterly Journal of Economics*, 101(1):149. [5](#), [27](#)
- Becker, G. S. (1957). *The Economics of Discrimination*. University of Chicago Press, Chicago. [1](#), [5](#), [23](#), [27](#), [29](#)
- Benzarti, Y. and Harju, J. (2021a). Can Payroll Tax Cuts Help Firms During Recessions? *Journal of Public Economics*, (forthcoming). [3](#), [5](#), [23](#)
- Benzarti, Y. and Harju, J. (2021b). Using Payroll Tax Variation to Unpack the Black Box of Firm-Level Production. *Journal of the European Economic Association*, (forthcoming). [3](#), [5](#), [23](#)

- Bertrand, M. (2011). New Perspectives on Gender. In Ashenfelter, O. and Card, D., editors, *Handbook of Labor Economics*, chapter 17, pages 1543–1590. North Holland: Elsevier Science, vol.4b edition. [6](#)
- Bertrand, M. (2020). Gender in the Twenty-First Century. *AEA Papers and Proceedings*, 110:1–24. [4](#), [28](#)
- Bertrand, M., Black, S. E., Jensen, S., and Lleras-Muney, A. (2019). Breaking the Glass Ceiling? The Effect of Board Quotas on Female Labour Market Outcomes in Norway. *The Review of Economic Studies*, 86(1):191–239. [1](#)
- Bianchi, N., Bovini, G., Li, J., Paradisi, M., and Powell, M. (2022). Career Spillovers in Internal Labor Markets. *The Review of Economic Studies*, (forthcoming). [8](#)
- Black, S. E. and Strahan, P. E. (2001). The Division of Spoils: Rent-Sharing and Discrimination in a Regulated Industry. *American Economic Review*, 91(4):814–831. [5](#), [27](#)
- Blau, F. D. and Kahn, L. M. (2003). Understanding International Differences in the Gender Pay Gap. *Journal of Labor Economics*, 21(1):106–144. [1](#)
- Blundell, R. and Macurdy, T. (1999). Labor Supply: A Review of Alternative Approaches. In Ashenfelter, O. and Card, D., editors, *Handbook of Labor Economics*, pages 1559–1695. North Holland: Elsevier Science, Amsterdam. [1](#)
- Boeri, T., Ichino, A., Moretti, E., and Posch, J. (2021). Wages Equalization and Regional Misallocation: Evidence from Italian and German Provinces. *Journal of the European Economic Association*, (forthcoming). [9](#)
- Bozio, A., Breda, T., and Grenet, J. (2019). Does Tax-Benefit Linkage Matter for the Incidence of Social Security Contributions? *IZA DP No. 12502*. [5](#)
- Cahuc, P., Carcillo, S., and Le Barbanchon, T. (2019). The Effectiveness of Hiring Credits. *The Review of Economic Studies*, 86(2):593–626. [2](#), [3](#), [5](#), [23](#), [24](#), [32](#)
- Calonico, S., Cattaneo, M. D., and Titiunik, R. (2014). Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs. *Econometrica*, 82(6):2295–2326. [17](#)
- Card, D., Devicienti, F., and Maida, A. (2014). Rent-sharing, Holdup, and Wages: Evidence from Matched Panel Data. *The Review of Economic Studies*, 81(1):84–111. [9](#)
- Card, D. and Hyslop, D. R. (2005). Estimating the Effects of a Time-Limited Earnings Subsidy for Welfare-Leavers. *Econometrica*, 73(6):1723–1770. [15](#)
- Card, D. E., Colella, F., and Lalive, R. (2021). Gender Preferences in Job Vacancies and Workplace Gender Diversity. *SSRN Electronic Journal*. [1](#)
- Casarico, A. and Lattanzio, S. (2019). What Firms Do: Gender Inequality in Linked Employer-Employee Data. *WorkINPS paper n. 24*. [6](#)
- Chetty, R., Guren, A., Manoli, D., and Weber, A. (2013). Does Indivisible Labor Explain the Difference between Micro and Macro Elasticities? A Meta-Analysis of Extensive Margin Elasticities. *NBER Macroeconomics Annual*, 27(1):1–56. [22](#)

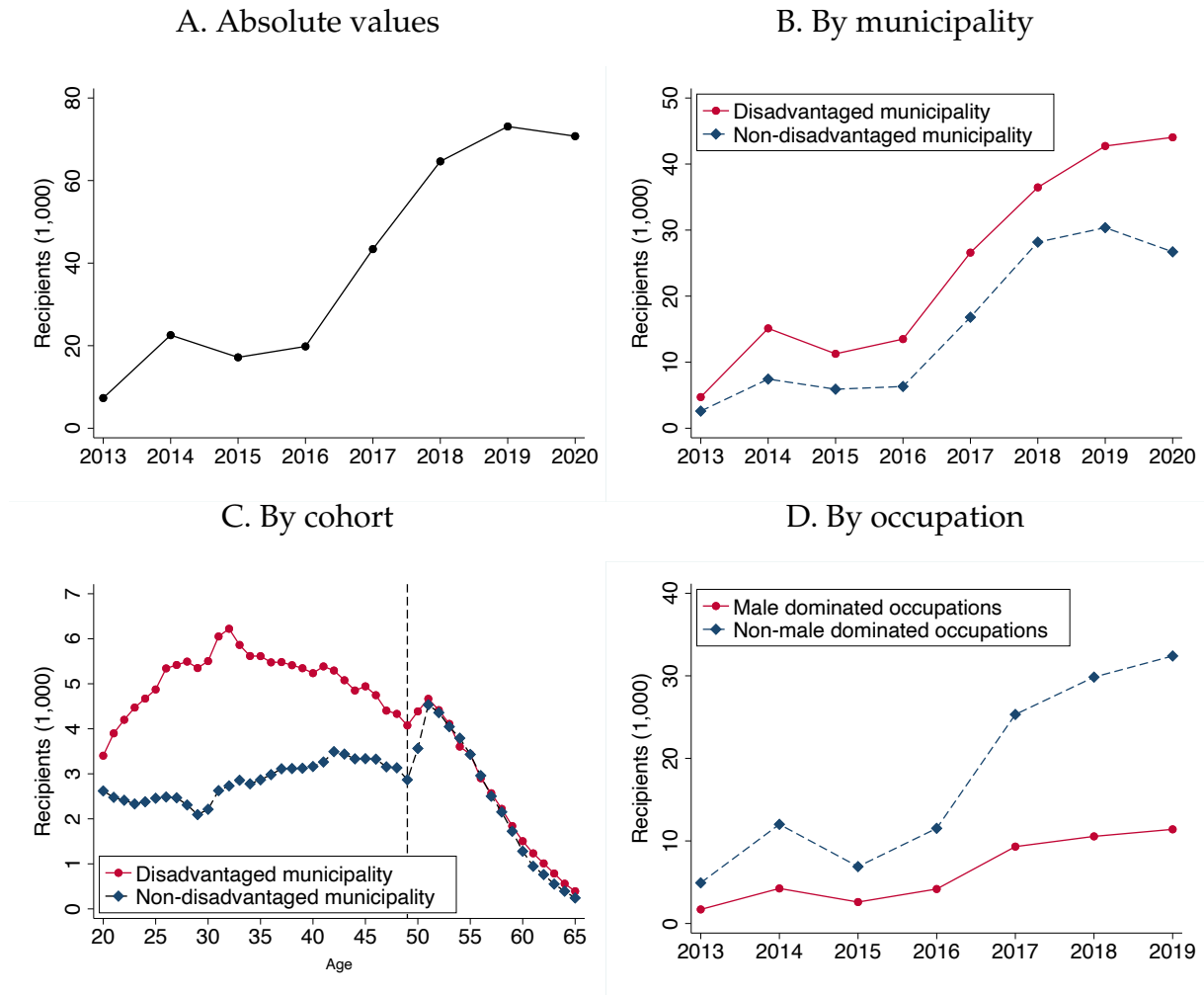
- Dahl, G., Kotsadam, A., and Rooth, D.-O. (2021). Does Integration Change Gender Attitudes? The Effect of Randomly Assigning Women to Traditionally Male Teams. *Quarterly Journal of Economics*, 136(2):987–1030. [4](#), [29](#)
- Dahl, G. B., Løken, K. V., Mogstad, M., and Salvanes, K. V. (2016). What Is The Case For Paid Maternity Leave? *Review of Economics and Statistics*, 98(4):655–670. [1](#)
- D’Ambrosio, A. and Scrutinio, V. (2022). For a Few Euro More: Benefit Generosity and the Optimal Path of Unemployment Benefits. *CEPR Discussion Paper No. 1835*. [16](#)
- De Vincenzi, R. and De Blasio, G. (2020). La Disoccupazione Amministrativa: Prestazioni di Sostegno al Reddito, Copertura e Persistenza. *WorkINPS paper n. 29*. [9](#)
- Egebark, J. and Kaunitz, N. (2018). Payroll Taxes and Youth Labor Demand. *Labour Economics*, 55:163–177. [5](#), [23](#)
- Fazzari, S. M., Hubbard, R. G., Petersen, B. C., Blinder, A. S., and Poterba, J. M. (1988). Financing Constraints and Corporate Investment. *Brookings Papers on Economic Activity*, 1988(1):141. [3](#), [23](#)
- Fullerton, D. and Metcalf, G. E. (2002). Tax Incidence. In Auerbach, A. and Feldstein, M., editors, *Handbook of Public Economics*, chapter 26, pages 1787–1872. Elsevier, Amsterdam. [1](#), [12](#)
- Gayle, G.-L. and Shephard, A. (2019). Optimal Taxation, Marriage, Home Production, and Family Labor Supply. *Econometrica*, 87(1):291–326. [1](#)
- Gelman, A. and Imbens, G. (2019). Why High-Order Polynomials Should Not Be Used in Regression Discontinuity Designs. *Journal of Business & Economic Statistics*, 37(3):447–456. [17](#)
- Goldin, C. (2006). The Quiet Revolution That Transformed Women’s Employment, Education, and Family. *American Economic Review*, 96(2):1–21. [1](#)
- Goldin, C. (2014). A Grand Gender Convergence: Its Last Chapter. *American Economic Review*, 104(4):1091–1191. [1](#), [19](#)
- Guiso, L., Pistaferri, L., and Schivardi, F. (2005). Insurance Within the Firm. *Journal of Political Economy*, 113(5):1054–1087. [9](#)
- Hamermesh, D. S. (1979). New Estimates of the Incidence of the Payroll Tax. *Southern Economic Journal*, 45(4):1208. [1](#), [12](#)
- Hellerstein, J. K., Neumark, D., and Troske, K. R. (2002). Market Forces and Sex Discrimination. *Journal of Human Resources*, 37(2):353–380. [3](#), [5](#), [24](#)
- Hendren, N. (2016). The Policy Elasticity. *Tax Policy and the Economy*, 30(1):51–89. [31](#)
- Hsieh, C.-T., Hurst, E., Jones, C. I., and Klenow, P. J. (2019). The Allocation of Talent and U.S. Economic Growth. *Econometrica*, 87(5):1439–1474. [4](#), [29](#)
- Katz, L. F. (1998). Wage Subsidies for the Disadvantaged. In Freeman, R. B. and Gottschalk, P., editors, *Generating Jobs*, pages 21–53. Russell Sage Foundation, New York. [2](#), [11](#)

- Kawaguchi, D. (2007). A Market Test for Sex Discrimination: Evidence from Japanese Firm-Level Panel Data. *International Journal of Industrial Organization*, 25(3):441–460. [3](#), [5](#), [24](#)
- Keane, M. P. (2011). Labor Supply and Taxes: A Survey. *Journal of Economic Literature*, 49(4):961–1075. [1](#)
- Kleven, H., Landais, C., and Søgaard, J. E. (2019). Children and Gender Inequality: Evidence from Denmark. *American Economic Journal: Applied Economics*, 11(4):181–209. [1](#)
- Kleven, H., Posch, J., Landais, C., Steinhauer, J., and Zweimuller, A. (2020). Do Family Policies Reduce Gender Inequality? Evidence from 60 Years of Policy Experimentation. *Working Paper*. [1](#)
- Kleven, H. J., Kreiner, C., and Saez, E. (2009). The Optimal Income Taxation of Couples. *Econometrica*, 77(2):537–560. [1](#)
- Kline, P. and Moretti, E. (2014). People, Places, and Public Policy: Some Simple Welfare Economics of Local Economic Development Programs. *Annual Review of Economics*, 6(1):629–662. [7](#)
- Kline, P., Rose, E. K., and Walters, C. R. (2022). Systemic Discrimination Among Large U.S. Employers. *Quarterly Journal of Economics*, 137:963–203. [1](#), [27](#), [28](#)
- Neumark, D. (2013). Spurring Job Creation in Response to Severe Recessions: Reconsidering Hiring Credits. *Journal of Policy Analysis and Management*, 32(1):142–171. [11](#)
- Neumark, D. and Simpson, H. (2015). Place-Based Policies. In *Handbook of Regional and Urban Economics*, pages 1197–1287. [7](#)
- OECD (2010). Employment Outlook, Chapter 1. OECD Publisher, Paris. [2](#)
- Olivetti, C. and Petrongolo, B. (2016). The Evolution of Gender Gaps in Industrialized Countries. *Annual Review of Economics*, 8(1):405–434. [1](#), [19](#)
- Pettigrew, T. F. and Tropp, L. R. (2006). A Meta-Analytic Test of Intergroup Contact Theory. *Journal of Personality and Social Psychology*, 90(5):751–783. [29](#)
- Rubolino, E. (2022). Taxing the Gender Gap: Labor Market Effects of a Payroll Tax Cut for Women in Italy. *CESifo Working Paper No. 9671*. [20](#)
- Saez, E., Matsaganis, M., and Tsakloglou, P. (2012). Earnings Determination and Taxes: Evidence From a Cohort-Based Payroll Tax Reform in Greece. *The Quarterly Journal of Economics*, 127(1):493–533. [5](#), [23](#)
- Saez, E., Schoefer, B., and Seim, D. (2019). Payroll Taxes, Firm Behavior, and Rent Sharing: Evidence from a Young Workers’ Tax Cut in Sweden. *American Economic Review*, 109(5):1717–1763. [1](#), [3](#), [5](#), [12](#), [23](#), [24](#)
- Saez, E., Schoefer, B., and Seim, D. (2021). Hysteresis from Employer Subsidies. *Journal of Public Economics*, (forthcoming). [5](#), [23](#)
- Sestito, P. and Viviano, E. (2018). Firing Costs and Firm Hiring: Evidence from an Italian Reform. *Economic Policy*, 33(93):101–130. [8](#)

Weber, A. and Zulehner, C. (2014). Competition and Gender Prejudice: Are Discriminatory Employers Doomed to Fail? *Journal of the European Economic Association*, 12(2):492–521. [1](#), [3](#), [6](#), [24](#), [27](#)

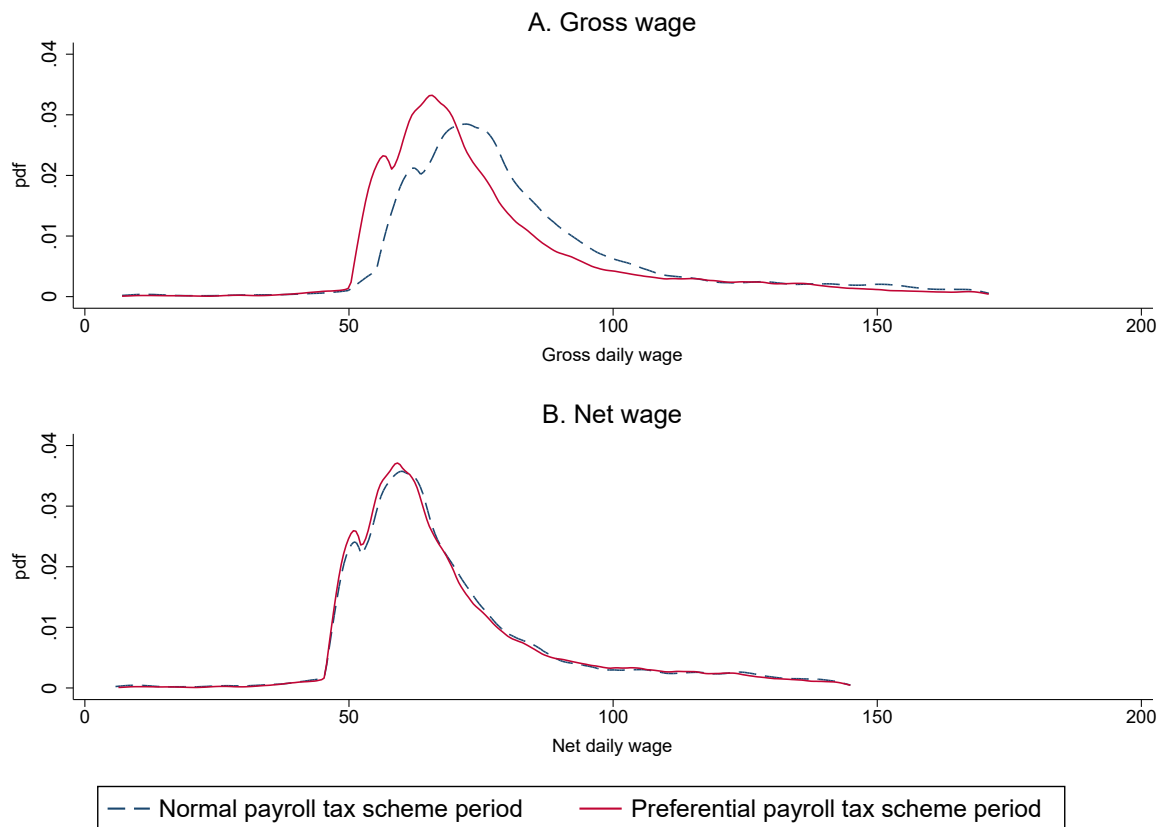
Zurla, V. (2022). How Should We Design Parental Leave Policies? Evidence From Two Reforms in Italy. *Job Market Paper, Brown University*. [8](#)

Figure 1: Policy Take-Up



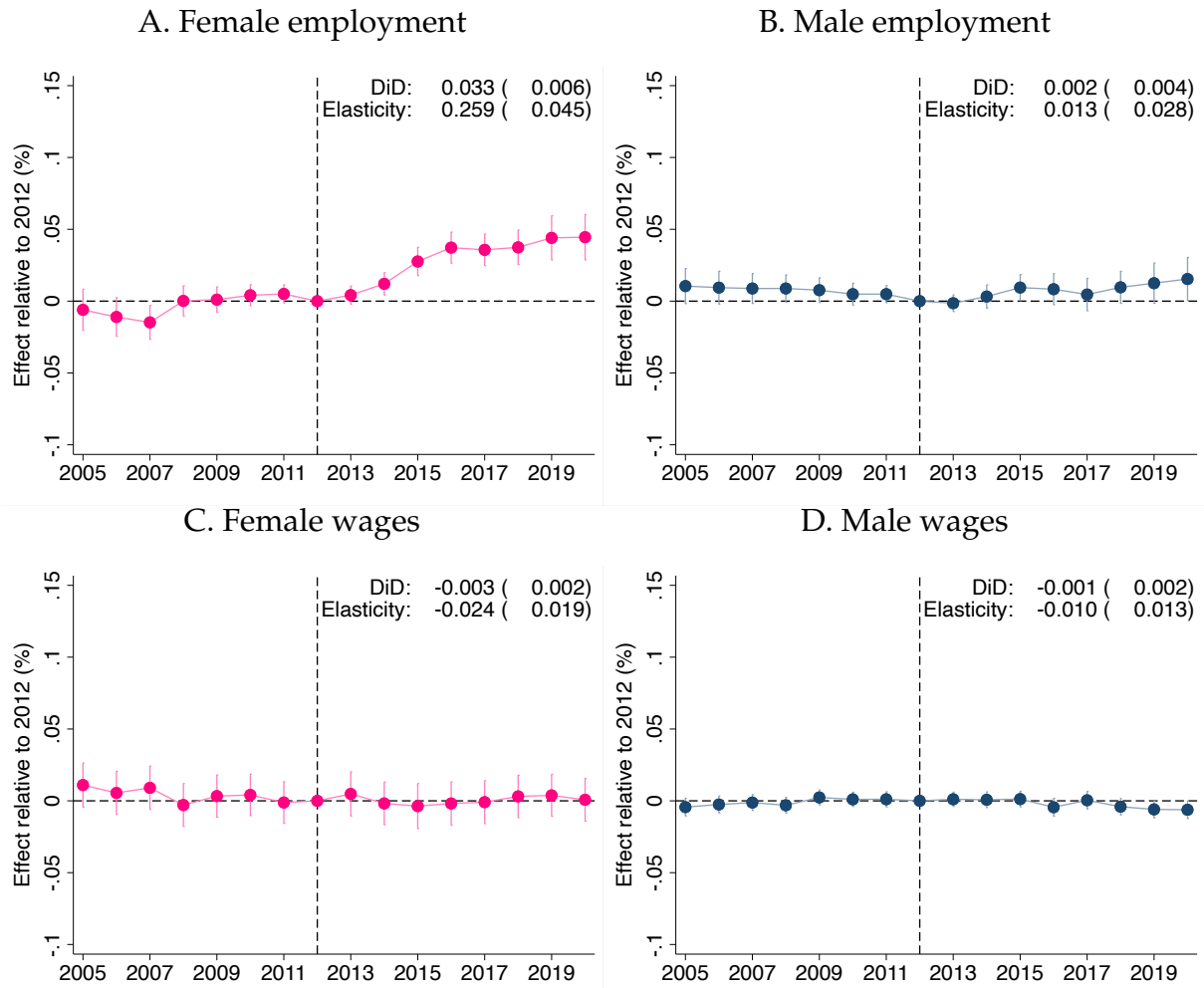
NOTE: This figure shows the trends in payroll-tax-cut recipients (thousands of female hires) since policy implementation (January 2013) up to 2020. Panel A shows absolute values. Panel B reports trends for municipalities eligible for EU structural funds (“disadvantaged municipalities”) versus municipalities that are noneligible for EU structural funds (“nondisadvantaged municipalities”), with the former being subject to less binding eligibility criteria. Panel C displays the policy take-up by age and municipalities. The dashed vertical line refers to a discontinuous change in eligibility criteria: women older than 50 are subject to less binding eligibility criteria in nondisadvantaged municipalities. Panel D reports policy take-up by occupation: following the law’s definition, “male-dominated occupations” are those where the gender employment gap is larger than 25 percent of the mean; “non-male-dominated occupations” refer to the other occupations. Data are from matched employer-employee data on the universe of Italian nonagricultural private-sector workers.

Figure 2: Gross and Net Wages of Payroll Tax Cut Recipients



NOTE: This figure focuses on the preferential payroll tax regime's recipients and plots kernel density estimates of the distribution of gross wages (Panel A) and net wages (Panel B) for the job, subject to the preferential payroll tax scheme (red solid line) versus the previous (noneligible) job spell (blue dashed line). *Gross wages* are defined as daily (full-time-equivalent) wage earnings gross of employer's payroll taxes. *Net wages* are daily (full-time-equivalent) wage earnings net of employer's payroll taxes. For graphical purposes, I drop observations in the top 5 percent (they are included in the main analysis).

Figure 3: Cross-Municipality Analysis



NOTE: This figure depicts the event-study coefficient estimates showing the impact of the payroll tax cut for female hires on 1) log of female employment (Panel A), 2) log of male employment (Panel B), 3) log of female net wages (Panel C), and 4) log of male net wages (Panel D). The figure plots coefficient estimates and the 95 percent confidence intervals obtained from Equation (1): 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 (2012). Standard errors are clustered by municipality. The figure also reports the difference-in-differences coefficient estimates and elasticity estimates, computed by running Equation (2). Data are from matched employer-employee data on the universe of Italian nonagricultural private-sector workers, collapsed at the municipality-year level.

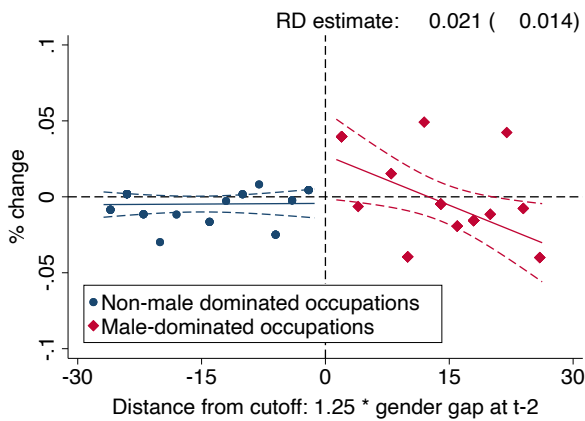
Figure 4: Cross-Cohort Analysis



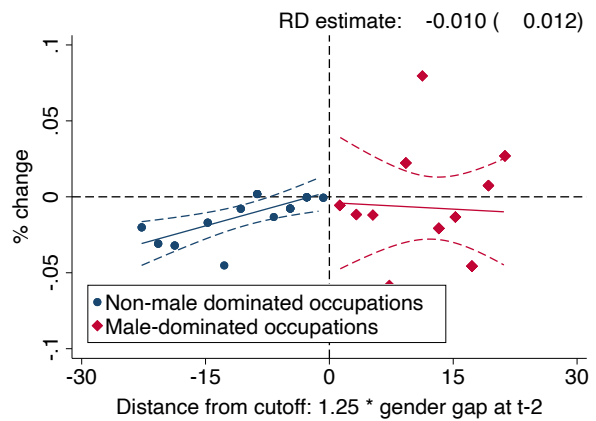
NOTE: The figure depicts the difference in employment (top two panels) and net wages (bottom two panels) between municipalities and by birth cohort, focusing on workers aged 46–53. Each line in the graphs refers to a single postreform year, and each outcome is normalized with respect to 2012. The vertical axis displays the difference across municipalities in workers' count (top panel) or net wages (bottom panel) by age and year relative to 2012. The figure also reports the triple different coefficient estimates and elasticity estimates, using cohort fixed effects, year fixed effects, and birth cohort fixed effects. Data are from matched employer-employee data over the 2005–2020 period on the universe of Italian nonagricultural private-sector workers, collapsed at the municipality/birth cohort/year level.

Figure 5: Cross-Occupation Analysis

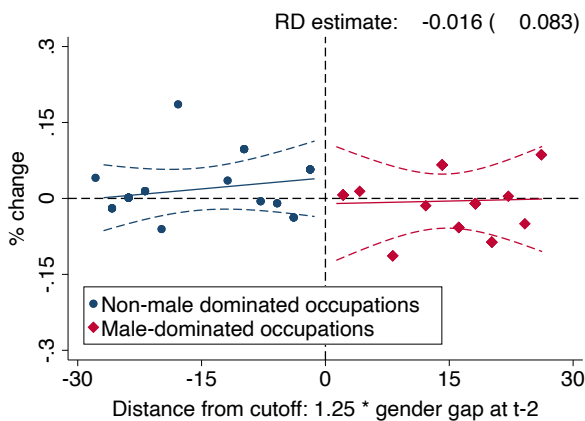
A. Female/male employment in nondisadvantaged municipality



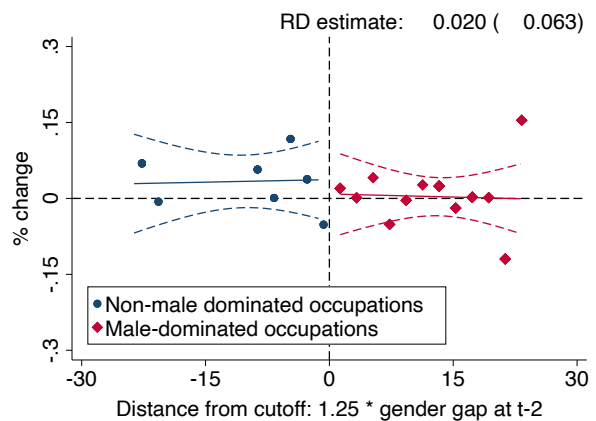
B. Female/male employment in disadvantaged municipality



C. Female/male wages in nondisadvantaged municipality

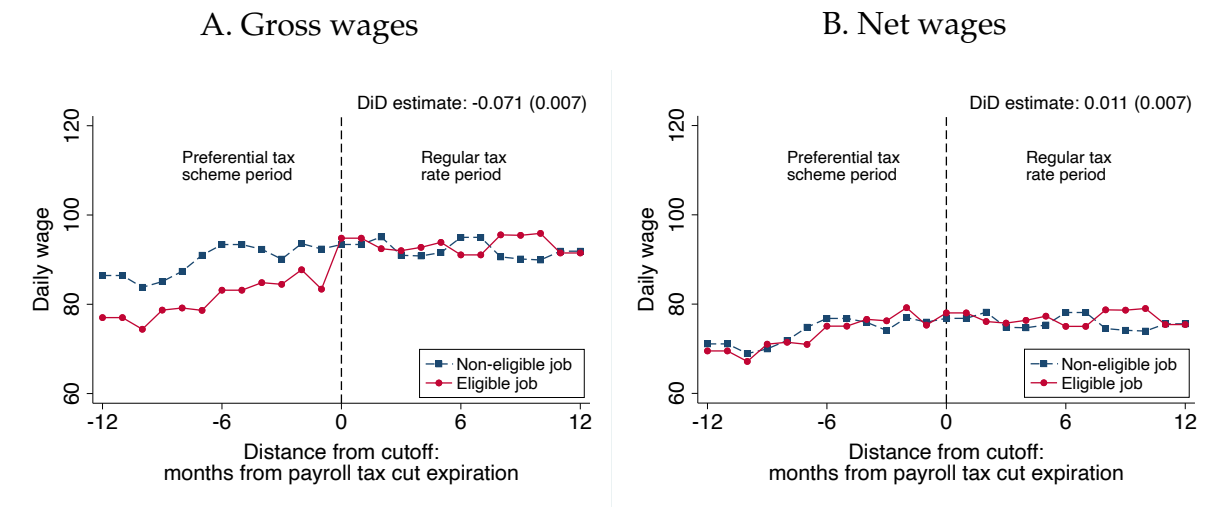


D. Female/male wages in disadvantaged municipality



NOTE: This figure presents the RD graphs, along with β coefficient estimates and standard errors computed from Equation (3). The top panel depicts the variation in the female-male employment ratio; the bottom panel in the female-male wage ratio. The left-hand-side graphs focus on nondisadvantaged municipalities, where the eligibility criteria create a discontinuity across occupations; the right-hand-side graphs on disadvantaged municipalities, where the minimum nonemployment duration requirement remains unchanged and would provide a placebo test. The vertical axis is the first-difference in the outcome variable of interest, with positive effects indicating an increase in the outcome among female workers. The horizontal axis is the distance from the cutoff (i.e., $1.25 \times$ average gender employment gap defined at $t = -2$). Each scatter point in the graphs is the sample average in the outcome variable over intervals of two-cutoff-point bins. The red diamonds depict scatter points relative to male-dominated occupations; the blue circles relative to non-male-dominated occupations. Data are from matched employer-employee data, matched with the *Comunicazioni Obbligatorie* data set over the 2005–2019 period on the universe of Italian nonagricultural private-sector workers, collapsed at the municipality group/occupation/year level.

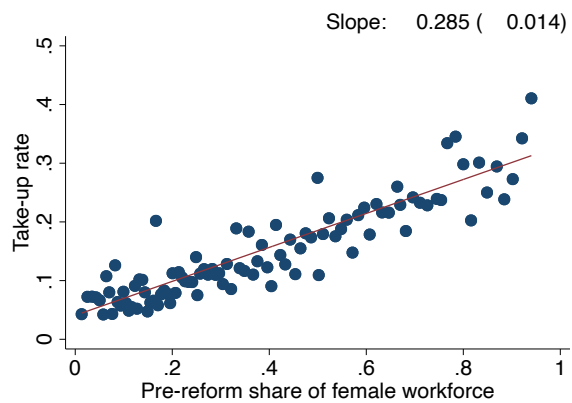
Figure 6: Gross and Net Wages around Firm Tenure Discontinuity



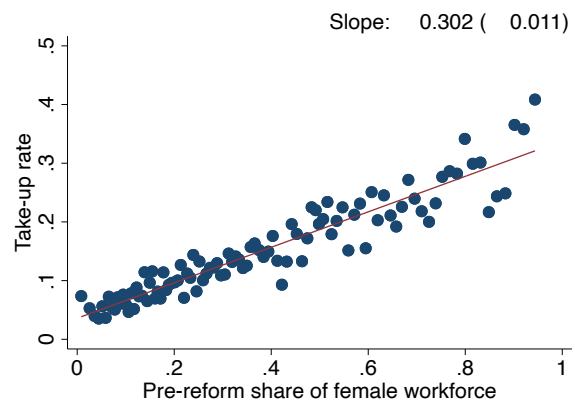
NOTE: The figure displays monthly gross (of employer payroll tax) wages in the top panel; monthly net wages in the bottom panel. Blue squares refer to the previous (noneligible) job spell; red circles to the job-eligible for the preferential payroll tax scheme. The horizontal dashed line defines the duration cutoff determining eligibility for the payroll tax cut. The figure also reports DiD coefficient estimate obtained from Equation (4) and standard errors clustered by individual. Data are from matched employer-employee data over the 2005–2020 period on the sample of subsidized hires that had a job spell before the reform.

Figure 7: Take-Up Rate and Prereform Workforce Composition

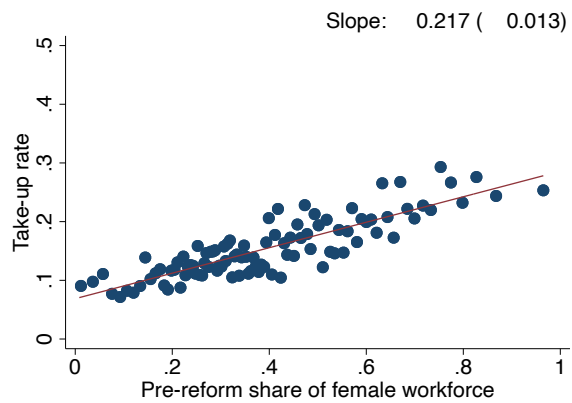
A. Raw correlation



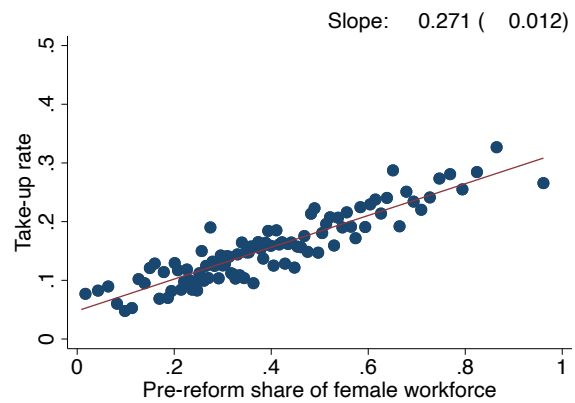
B. Within-region correlation



C. Within-industry correlation

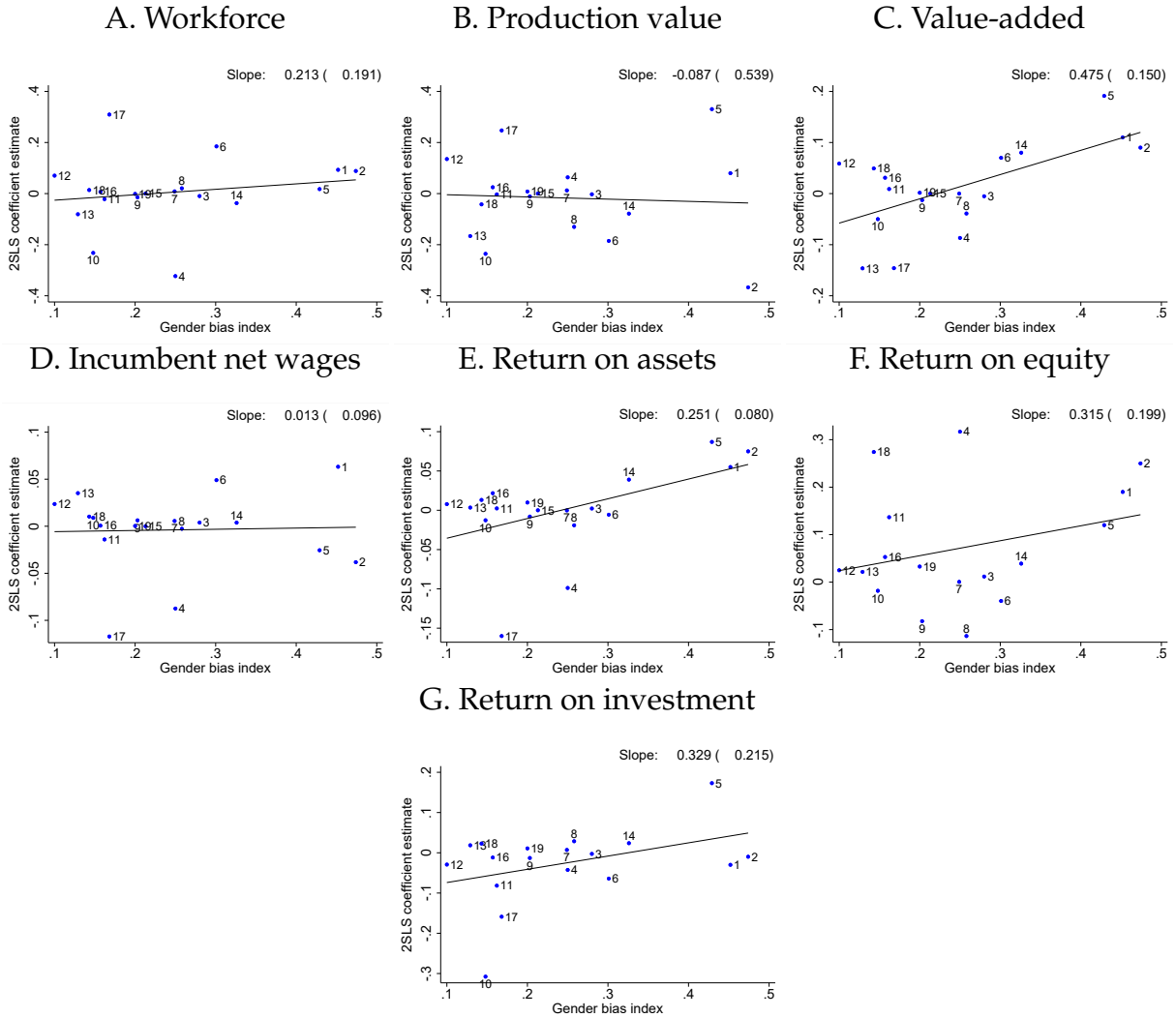


D. Within-region-industry correlation



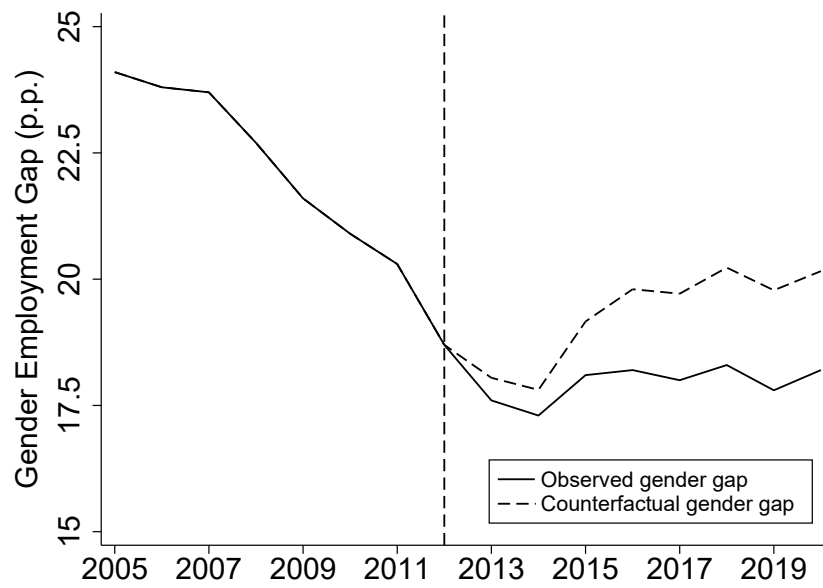
NOTE: This figure compares the firm-level policy take-up rate with the prereform share of the female workforce. The take-up rate is computed as the firm-level workforce share of subsidized hires, averaged over the postreform period. Each panel depicts this relationship in 100 equal-size bins and shows the line of best fit. Panel A illustrates the unconditional correlation; Panel B depicts the residuals from OLS regressions with region fixed effects; Panel C shows the residuals from OLS regressions with industry fixed effects; and Panel D the residuals from OLS regressions with both region and industry fixed effects. Each figure also presents the estimated slope.

Figure 8: Marginal Effects by Preexisting Gender Bias



NOTE: This figure shows the marginal impact by preexisting gender biases across industries. It relates the industry-level 2SLS coefficient estimate with a gender bias index, computed as 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 refer to the following industries (2-digit group): 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; 19 = others. Each graph also reports the slope estimate and robust standard error in parentheses.

Figure 9: The Gender Employment Gap in Italy, 2005–2020



NOTE: This figure compares the observed gender employment gap (solid line) with the counterfactual gender employment gap (dashed line) that would have been observed in the absence of the payroll tax cut. The gender employment gap is retrieved from the Italian Institute of Statistics (ISTAT), which is calculated from labor force survey data over the 2005–2020 period. The counterfactual gender gap is computed by scaling the observed gender gap by the female employment increase generated by the payroll tax cut. The dashed vertical line refers to the year before the payroll tax cut on female hires was implemented.

Table 1: Eligibility Criteria for the Payroll Tax Cut

	Nonemployment duration (months)	
	Age < 50	Age ≥ 50
A. Geographical requirement		
<i>Disadvantaged municipality:</i> worker resides in a municipality eligible for EU structural funds	6	6
<i>Nondisadvantaged municipality:</i> worker resides in a municipality noneligible for EU structural funds	24	12
B. Occupational requirement		
<i>Male-dominated occupation:</i> worker is hired in an occupation with gender employment gap ≥ 1.25 mean gap	6	6
<i>Non-male-dominated occupation:</i> worker is 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 female hires. The eligibility criteria were designed to provide much stronger work incentives along two *nonmutually 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 six months in nonemployment status and are residents in disadvantaged areas. In all the other municipalities, the minimum nonemployment 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 nonemployment duration requirement is set to six months for women hired in male-dominated occupations, defined as occupations where the gender employment gap is at least 25 percent larger than the average employment gap.

Table 2: Micro-Level Evidence of Employment Effects

	<i>Outcome: 1(woman i is employed at t)</i>						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Employment effect							
$D_{i,t} \cdot 1(t \in Post)$	0.054*** (0.002)	0.034*** (0.002)	0.015*** (0.002)	0.016*** (0.001)	0.015*** (0.001)	0.015*** (0.001)	0.014*** (0.001)
B. Elasticity estimate							
$\log(1 - \tau_{i,t})$	0.413*** (0.012)	0.268*** (0.008)	0.122*** (0.005)	0.123*** (0.005)	0.117*** (0.004)	0.116*** (0.004)	0.107*** (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
Muni \times year FE	No	Yes	Yes	Yes	Yes	Yes	Yes
Occ \times year FE	No	No	Yes	Yes	Yes	Yes	Yes
Age \times year FE	No	No	No	Yes	Yes	Yes	Yes
Muni \times age FE	No	No	No	No	Yes	Yes	Yes
Muni \times occ FE	No	No	No	No	Yes	Yes	Yes
Age \times occ FE	No	No	No	No	Yes	Yes	Yes
Muni \times age \times occ FE	No	No	No	No	No	Yes	Yes
Ind \times 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 employment in the top panel; the bottom panel reports net-of-payroll tax elasticity estimates. The outcome variable is a dummy equal to 1 if a woman 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). Three-way clustered standard errors by municipality, birth cohort, and occupation are in parentheses. Data are from matched employer-employee data on the universe of Italian nonagricultural private-sector workers.

Table 3: Take-Up Rate and Firm-Level Outcomes

	Take-up rate (1)	log of workforce (2)	log of prod (3)	log of VA (4)	log of wages (5)	ROA (6)	ROE (7)	ROI (8)
$ShareFemale_{i,t} \cdot Post_t$	0.267*** (0.010)							
$TakeUpRate_{i,t}$ (x 10)		0.040*** (0.002)	0.017*** (0.002)	0.009*** (0.002)	0.002*** (0.000)	0.004*** (0.001)	0.012** (0.005)	0.007* (0.004)
Observations				2,900,773				
# of firms				248,002				
Firm and year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-stat	703.093	-	-	-	-	-	-	-

NOTE: This table studies the firm-level effects of the payroll tax cut on female hires. Column 1 reports the “first-stage” relationship between the firm-level prereform share of the female workforce and the take-up rate. Column 2 shows 2SLS impacts of the (predicted) take-up rate (multiplied by 10) on the following firm-level outcomes: log of workforce (column 2), log of production value (column 3), log of value-added (column 4), log of (noneligible) incumbent workers’ average net wage (column 5), return on assets (column 6), return on equity (column 7), and return on investment (column 8). Each specification includes firm fixed effects and year fixed effects. Standard errors are clustered at the firm level in parentheses.

Online Appendix

A Policy Background

A.1 Gender Biases and Discrimination in Italy

This section offers descriptive evidence on gender stereotypes and discrimination in Italy.

To what extent do gender stereotypes affect occupational choices? [Figure A3](#) compares the share of female workers in an industry (vertical axis) with the share of workers agreeing with the statement, “When jobs are scarce, men have more rights 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*). 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 correlated with gender stereotypes. There are two main interpretations for this result. On the one hand, it might reflect a “stereotype threat”: gender stereotypes deter women from entering male-biased industries. This can also be the result of choices made before entering the labor market, such as when enrolling in college. On the other hand, this correlation can be attributed to discrimination being more pronounced in industries with more conservative gender beliefs. Italy aimed at promoting female employment in male-biased occupations by granting weaker payroll-tax-cut eligibility criteria to male-dominated occupations. The reform thus allows me to test whether gender stereotypes and prejudices are malleable and can be shaped through policy.

Gender stereotypes are also more intense in poorer regions. For example, less than 20 percent of the population located in northern Italy agree with the statement, “When jobs are scarce, men have more of a right to a job than women,” as opposed to around one-third of southern Italians. A similar pattern emerges when considering questions that emphasize the notion of a natural difference by sex. Around 42 percent of the respondents in the south of Italy strongly or mildly agree that “it is not natural that a male worker has a female supervisor,” while the share of respondents that agree are not quite half as many 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 are more conservative.

Gender differences in job-searching behavior also arise. For instance, 32 percent of women state that they have “renounced searching for a job because of household tasks, such as meal preparation, house cleaning, and grocery shopping, at least once during her life,” against almost 14 percent for men. Furthermore, around two-thirds of

Figure A1: Gender Employment Gap, Italy versus the U.S.



NOTE: This figure compares the occupation-specific share of female workers in Italy (vertical axis) with those in the U.S. (horizontal axis). Italian estimates are collected from documents published by the Ministry of Labor, relying on labor force survey data (see Appendix A.2 for details). Occupations are identified by the International Standard Classification of Occupations (ISCO) submajor group. U.S. 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 more than 300 occupations. The estimates are limited to occupations with at least 100 observations. The choice of how to link the U.S.-Italy occupation classification has been dictated by the denomination of each occupation (using a semantic criterion). Female employment share estimates refer to the latest available data (2018 for Italy and 2019 for the U.S.).

working women report that they are “feeling overwhelmed by household duties and are considering resigning from their jobs.”

A.2 Definition of Male-Dominated Occupations

The legislator defines occupations following the standard International Standard Classification of Occupations (ISCO). Specifically, occupations are grouped in 37 2-digit ISCO groups, which is the so-called “submajor” 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 the Italian labor force survey.

A weakness of the data published in official public documents is that they report

exclusively information relative to eligible occupations—that is, those for which the gender employment gap is larger than 25 percent of the national average. To account for this issue, I use the same source of data to compute the same statistics for noneligible 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.

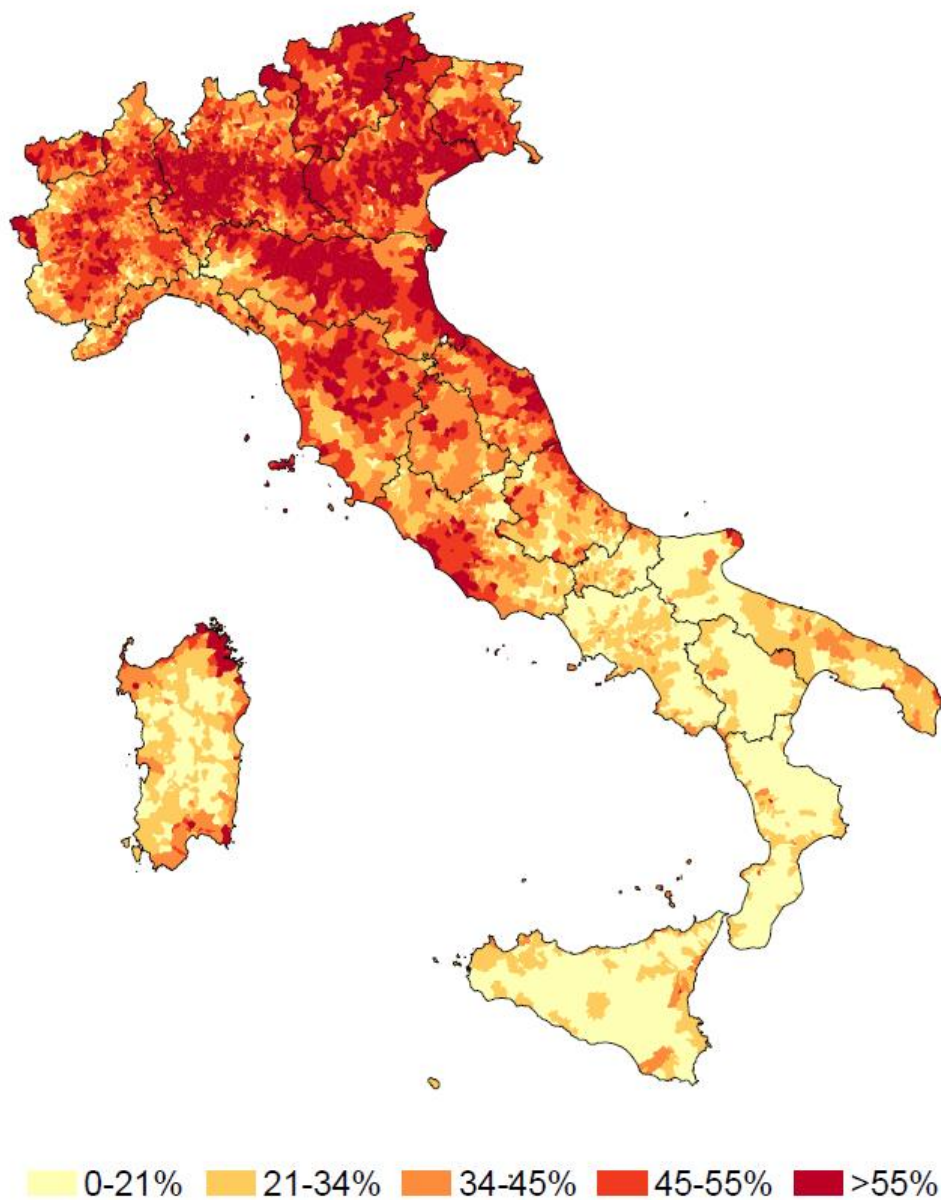
[Table A1](#) reports series on the occupation-specific gender employment gap, along with information on the cutoff value determining eligibility for the lower payroll tax rate.

Table A1: Gender Employment Gap By Occupation

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 service 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 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 service 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 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	Noncommissioned 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

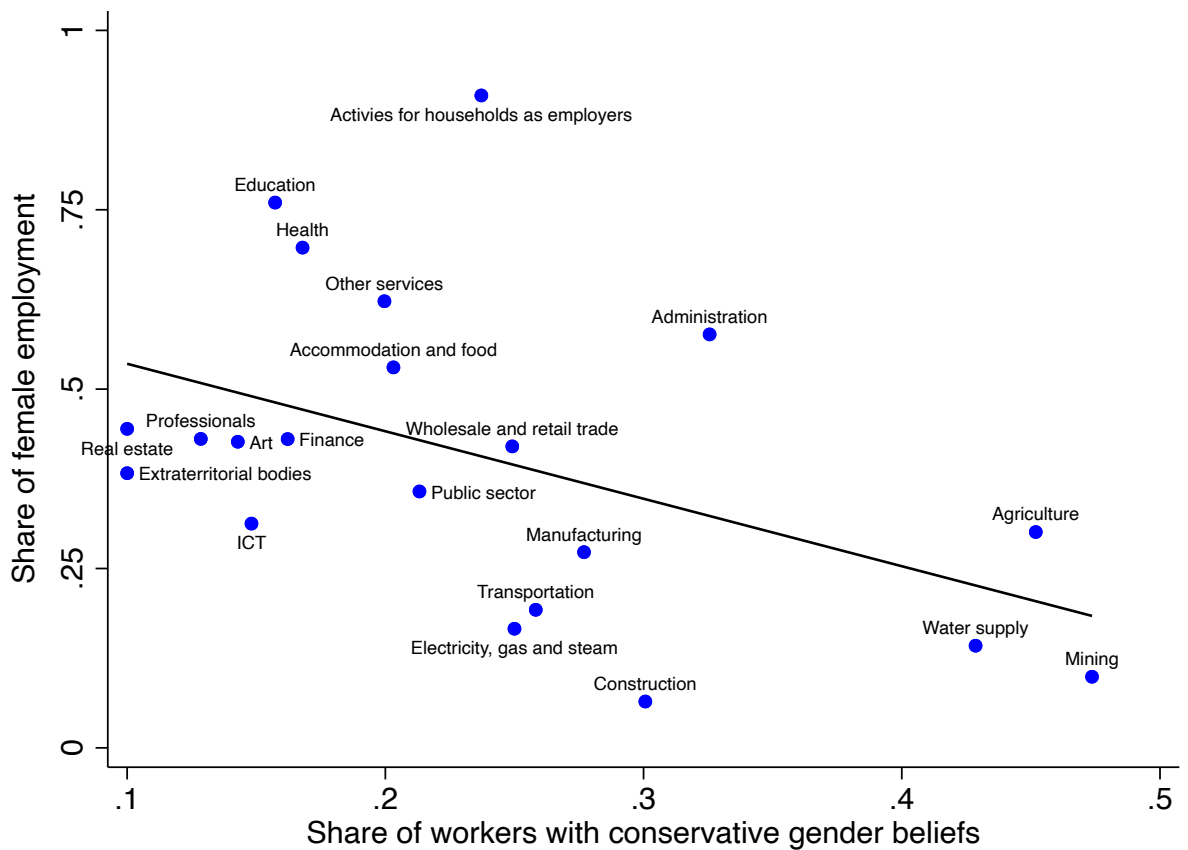
NOTE: This table reports the gender employment gap in each occupation (identified by the CP2011, i.e., the ISCO-08 submajor group) over the 2011–2018 period. In the last two rows, the table shows the average gender employment gap and the cutoff defining eligibility for the preferential payroll tax scheme. Occupations where the gender employment gap is larger than the cutoff value as 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 A2: Prereform Share of Female Employment



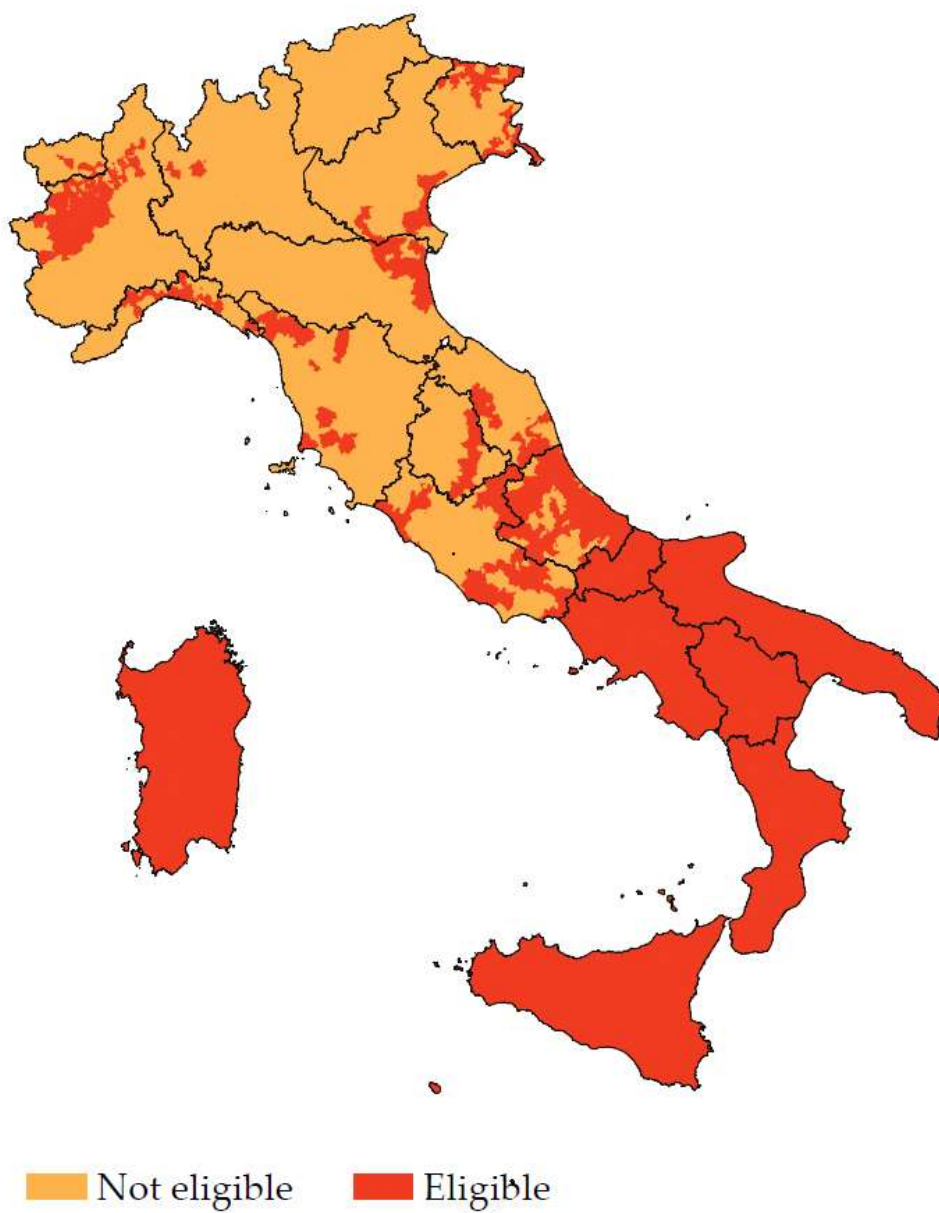
NOTE: This graph depicts the female employment share (share of female employees over 25–65 female population) over the period before the Fornero reform (2005–2012). Black lines refer to regional boundaries.

Figure A3: Female Participation Rates and Gender Beliefs across Industries



NOTE: The figure compares the 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 beliefs 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*.

Figure A4: Eligibility for European Union Structural Funds



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

B Additional Analyses and Results

B.1 Microlevel Analysis by Firm Tenure

Table B1 shows the β estimates and standard errors obtained by running regressions as in Equation (4). The results confirm the evidence presented in Figure 6: on average, gross wages drop by around 8.3 percent, while net wages grow by 0.8 percent. I also report tax incidence as the fraction of the payroll tax cut that benefits the employer—called “pass-through to firms” in the table—and computed as the gross wage coefficient divided by the gross wage coefficient net of the net wage coefficient. 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 recipient population. The table presents several robustness checks that confirm that tax incidence is mostly on firms. I start by studying whether tax incidence varies over the (prereform) wage distribution, which would capture, at least in part, heterogeneous effects by skill level. 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.

Table B1: Payroll Tax Incidence

	Full sample (1)	Below median (2)	Above median (3)	New emp. (4)	New occ. (5)	New muni. (6)	Female emp. (7)
A. Outcome: log of gross wages							
$1(t \leq C)$ $\times 1(j \in \text{Eligible})$	-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: log of net wages							
$1(t \leq C)$ $\times 1(j \in \text{Eligible})$	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)
Individuals	201,353	101,018	90,335	88,289	80,146	67,429	49,125
Ind. \times tenure FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Job spell 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 studies the incidence of the payroll tax cut for female hires. 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-firm tenure fixed effects and job fixed effects. Panel A (B) shows the results on wages gross (net) of the employer portion of the payroll tax rate. Pass-through to firms is defined as the fraction of payroll tax that benefited the firm. In columns 2–7, the analysis is based on the following subsamples: workers having wages below or above the median (based on prereform wages); workers that changed employers; workers that are hired in a new occupation; workers employed in a different municipality; workers hired by a female employer. Standard errors are in parentheses clustered at the individual level.

Another possibility is that wages do not change because of the presence of implicit contracts whereby firms offer the same wage of the previous job. In column (4), I focus exclusively on workers that changed employers, who, by definition, are not affected by implicit wage contracts. 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 into the labor market. I select workers (of both sexes) 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-differences analysis comparing men’s versus 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:

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

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 . β is the coefficient of interest: it measures the percentage change in wages earned by female workers during the postreform period. I also account for female worker fixed effects, $\gamma_{s(i)}$, to allow for any (permanent) difference in wages by sex (that is, the gender pay gap at career start). δ_t includes year dummies. $u_{i,t}$ are firm-level clustered standard errors.

The β coefficient estimates are presented in [Table B2](#). I start from a simple model with sex and year fixed effects. Column (2) includes municipality-year fixed effects, which allows me to account for any local labor market shocks and policies. In column 3, I add firm-year fixed effects so as to exploit within-firm variation and to control for any firm-specific economic shocks. Finally, columns 4 and 5 interact with job type and contract characteristics (i.e., permanent vs. temporary contract; full-time vs. part-time job) with year fixed effects.

This analysis presents results in line with the baseline results presented in [Table B1](#). I find that at least 92 percent of the payroll tax cut remains in the firm. This implies that new female hires over the postreform 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 they are willing to increase the geographical sphere of their job search or to look more extensively for finding a better match with an industry that would be more specialized in their job task. Columns 5–6 show that this is not the case: each specification presents a 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

Table B2: Payroll Tax Incidence on New Hires

	(1)	(2)	(3)	(4)	(5)
A. Outcome: Gross wage					
$1(i \in Female)$ $\times 1(t \in Post)$	-0.095*** (0.003)	-0.097*** (0.003)	-0.090*** (0.002)	-0.085*** (0.002)	-0.085*** (0.002)
B. Outcome: Net wage					
$1(i \in Female)$ $\times 1(t \in Post)$	-0.003 (0.003)	-0.005 (0.003)	0.002 (0.002)	0.007*** (0.002)	0.007*** (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
Gender 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-gender FE	No	No	No	No	Yes
Job type-gender 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 with a simple model with gender and year fixed effects. Column 2 includes municipality-year fixed effects, which allow me to account for any local labor market shocks and policies. In column 3, I add firm-year fixed effects, so as 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. temporary 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 benefits the firm. The sample includes all the workers younger than 35 entering for the first time into the labor market over the 2005–2019 period.

with the payroll tax cut—because they are influenced by the gender of their employer. *Ceteris paribus*, the transition from a male to a female employer might help overcome 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 B3, I run Equation (4) 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 tax incidence is on firms, at least over the medium run.

Table B3: Payroll Tax Incidence by Year of Job Signing

	Year of job signing:				
	2013 (1)	2014 (2)	2015 (3)	2016 (4)	2017 (5)
A. Outcome: Monthly gross wage					
$1(t \leq C)$ $\times 1(j \in \text{Eligible})$	-0.077*** (0.013)	-0.065*** (0.011)	-0.082*** (0.015)	-0.164*** (0.042)	-0.112 (0.200)
B. Outcome: Monthly net wage					
$1(t \leq C)$ $\times 1(j \in \text{Eligible})$	0.005 (0.013)	0.018 (0.011)	-0.001 (0.015)	-0.082 (0.052)	-0.029 (0.200)
Individuals	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 was applied. Each specification includes individual-firm tenure fixed effects and job fixed effects. The coefficient estimate thus presents the percentage 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 benefits the firm. Standard errors are in parentheses clustered at the individual level.

B.2 Back-of-the-Envelope Computations on Program's Fiscal Externalities

Building on the estimates presented in this paper, this section calculates the indirect effect of the policy on income tax revenue, payroll tax revenue, value-added tax revenue, and spending on unemployment insurance benefits. These back-of-the-envelope calculations help to understand whether the policy's net cost is more or less than the budgetary cost. In computing back-of-the-envelope calculations, I assume the following characteristics of a representative worker and firm benefiting from the preferential payroll tax scheme:

- Payroll tax-cut recipients (N) = 218,768 (source: [Table B4](#))
- Employment effect of recipients (ϵ_l) = + 2.5% (source: column 7 in [Table 2](#))
- Gross yearly salary of eligible workers (y) = €17,300 (source: [Table B4](#))
- Average income tax rate ($T(y)$) = $[(15,000 \cdot 0.23) + ((y - 15,000) \cdot 0.27)]/y = 0.235$
- Employer regular payroll tax rate (τ_e^{reg}) = 0.225

- Employer preferential payroll tax rate (τ_e^{pref}) = 0.11025 (source: [Figure B1](#))
- Employee payroll tax rate (τ_w) = 0.10805
- Value-added tax rate (τ_v) = 0.22
- Unemployment insurance benefits reduction (UI) = €747 (source: [Figure B4](#))
- Marginal effect on firm value-added (ϵ_V) = +0.9% (source: column 4 in [Table 3](#))
- Value-added per worker (V) = €36,904 (source: balance sheets data)
- Marginal effect on incumbent workers' wages (ϵ_{inc}) = +0.2% (source: column 5 in [Table 3](#))
- Gross yearly salary of noneligible incumbent workers (y^{inc}): €31,220 (source: matched employer-employee data set)
- Incumbent workers of firms with a positive take-up rate (N_{inc}) = 1,385,298 (source: [Table B5](#))
- Average income tax rate of noneligible incumbent workers $T(y^{inc})$: $[(15,000 \cdot 0.23) + ((28,000 - 15,000) \cdot 0.27) + ((y^{inc} - 28,000) \cdot 0.38)]/y = 0.235 = 0.262$

B.2.1 Budgetary Costs

The preferential payroll tax scheme on female hires introduced an employer-borne 50 percent payroll tax cut. The employer payroll tax rate thus dropped from 0.225 to 0.1125 percent. Budgetary costs (B) are computed by multiplying the payroll tax rate difference between the regimes with the tax base (gross yearly salary) of each recipient:

$$B = \sum_{i=1}^N y_i \cdot (\tau_e^{reg} - \tau_e^{pref}) = 417,261,676. \quad (C1)$$

It suggests a cost of €1,907 for each worker hired through the preferential payroll tax scheme in terms of missing employer payroll tax revenue.

B.2.2 Income Tax Revenue

The effect of the policy on income tax revenue (T^y) is given by the positive employment effect on directly treated workers and the absence of any significant crowding-out effect on male employment. The most conservative estimate from [Table 2](#) implies an employment effect of 2.5 percent. Applying the income tax schedule to the representative payroll tax-cut hire, I calculate an average tax burden of €4,071. This corresponds to an average income-tax rate of 23.53 percent. Income-tax revenue is computed as

$$T^y = \epsilon_l \sum_{i=1}^N y_i \cdot T(y_i) = 22,265,113. \quad (C2)$$

This estimate implies an increased income-tax collection of €102 per recipient. The fiscal externality stemming from income-tax revenue corresponds to 5.3 percent of the total cost of the policy.

B.2.3 Payroll Tax Revenue

Employment effects would also bring extra revenue from employer and employee payroll taxes. Payroll tax revenue is computed by the following equation:

$$T^p = \epsilon_l \sum_{i=1}^N y_i \cdot (\tau_e^{reg} + \tau_w) = 31,512,245. \quad (C3)$$

These revenues correspond to €144 per recipient, accounting for 7.6 percent of the gross policy cost.

B.2.4 Value-Added Tax Revenue

The firm-level analysis has shown that the policy-induced increase in female employment raised firm value-added. Column 4 in [Table 3](#) shows that, on average, the marginal effect of the policy take-up on firm value-added is around 0.9 percent. To compute the expected effect of the policy on value-added tax revenue, I sum the value-added revenue that an extra worker would bring among all the new subsidized hires:

$$T^v = \sum_{i=1}^N \underbrace{(V \cdot \epsilon_V)}_{\text{marginal VA increase}} \tau_v = 15,985,360. \quad (C4)$$

The value-added increase would yield around €73 per recipient of extra value-added tax revenue. This corresponds to around 3.8 percent of the budgetary costs.

B.2.5 Incumbent Workers' Income and Payroll Tax Revenue

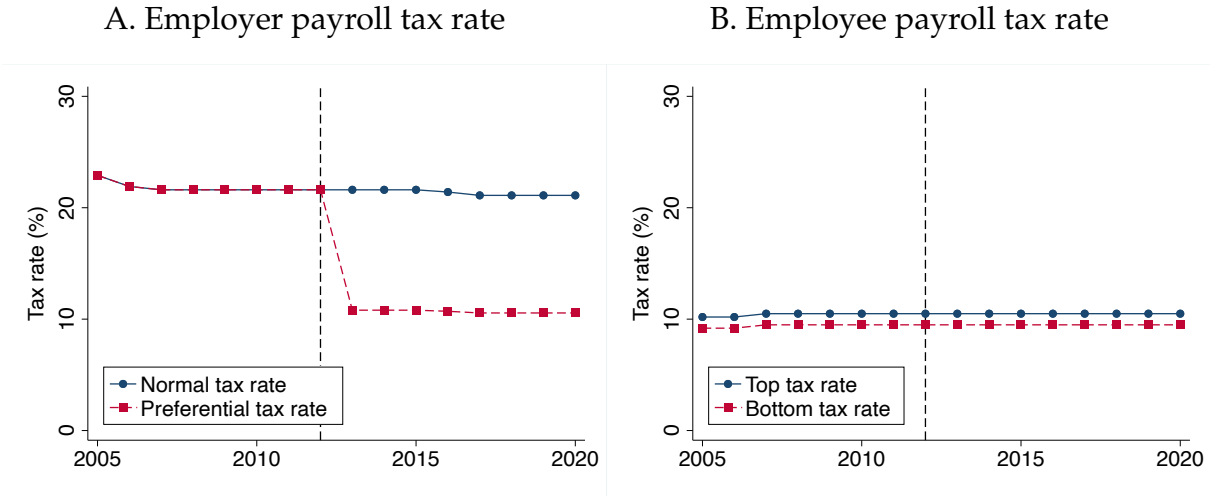
The effect of the policy on (noneligible) incumbent workers' income and payroll tax revenue (T_{inc}^y) is given by the positive effect of the policy on incumbent workers' wages. On average, column 5 in [Table 3](#) suggests that a marginal increase in the firm take-up rate raises incumbent workers' net wages by 0.2 percent. Applying the income tax schedule to a representative incumbent worker, I compute an average tax burden of €8,180. This corresponds to an average income tax rate of 26.2 percent. The total effect on income and payroll tax is computed as

$$T_{inc}^y = \sum_{i=1}^{N_{inc}} \underbrace{(y^{inc} \cdot \epsilon_{inc})}_{\text{marginal incumbent wages' increase}} \cdot (T(y^{inc}) + \tau_e^{reg} + \tau_w) = 71,061,023 \quad (C5)$$

This estimate implies an increase in income tax collection of €323 per payroll tax-cut hire, accounting for around 17.1 percent of the total gross cost.

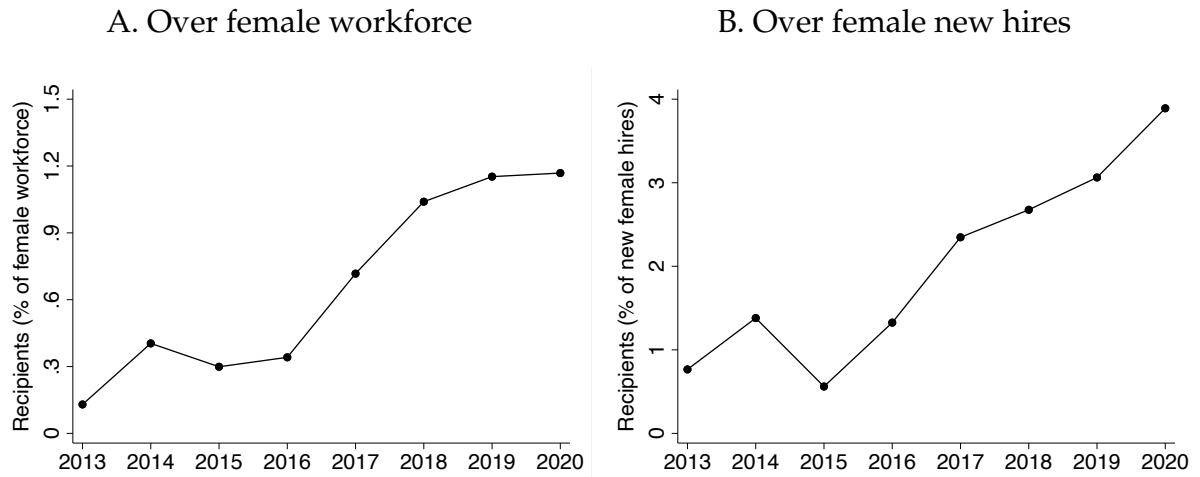
B.3 Additional Tables and Figures

Figure B1: Payroll Tax Rates in Italy



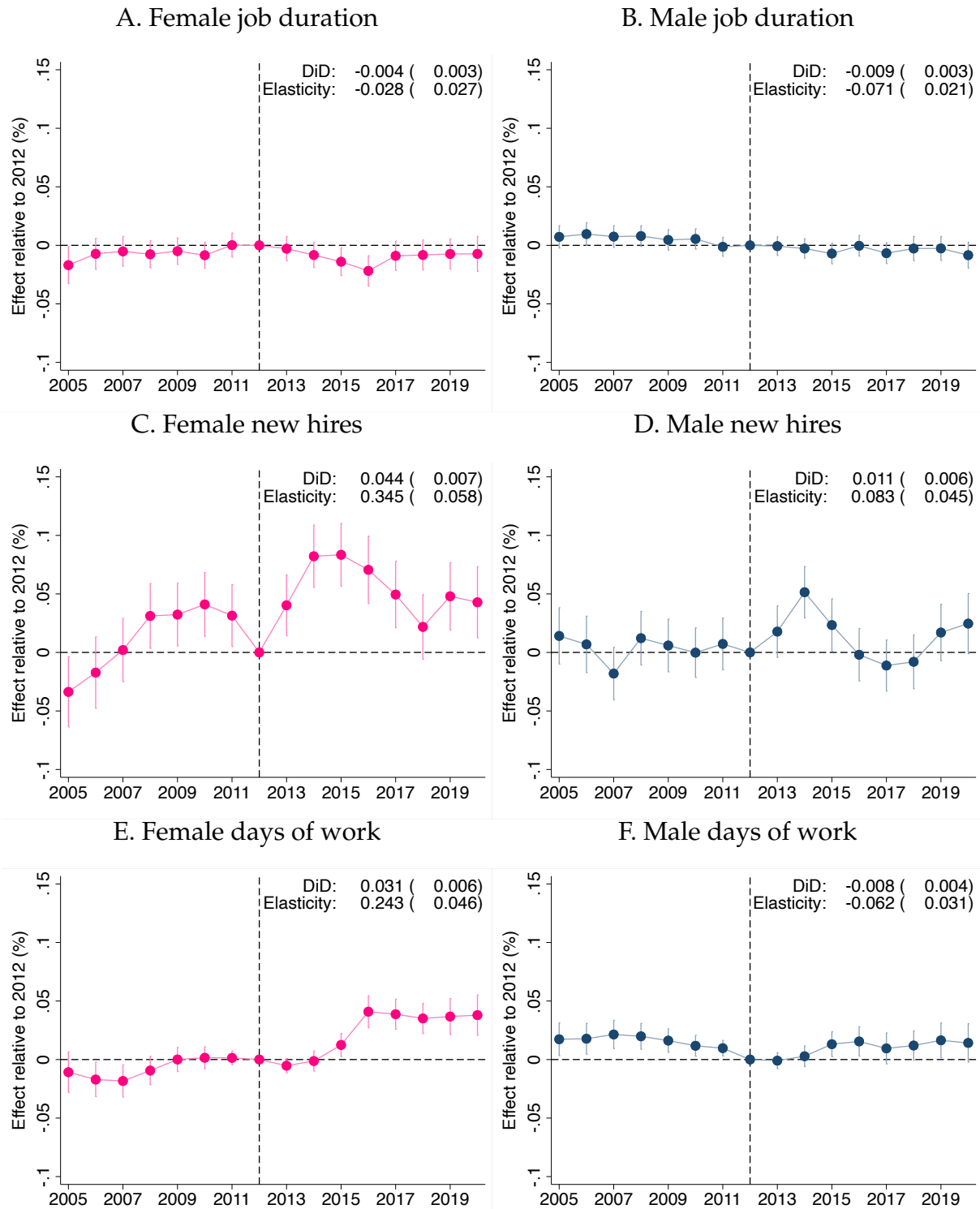
NOTE: The figure displays the evolution in the average payroll tax rate since 2000. The employer (normal) tax rate is marked by the blue circles in the left-hand-side graph, while the preferential payroll tax-scheme series is marked by the red squares. The right-hand-side panel depicts the employee payroll tax-rate series, shown separately for the bottom (red squares) and top tax rate (blue circles) applying to earnings above 47,143 euros. The vertical dashed line refers to the year before the payroll tax cut for female hires.

Figure B2: Take-Up Rate



NOTE: This figure shows the trends in payroll tax-cut recipients since policy implementation (January 2013) up to 2020. Panel A shows the trend in subsidized hires as a share of the total female workforce covered in the social security archive. Panel B displays the trend as a share of new female hires in a given year. Data are from matched employer-employee data on the universe of Italian nonagricultural private sector workers.

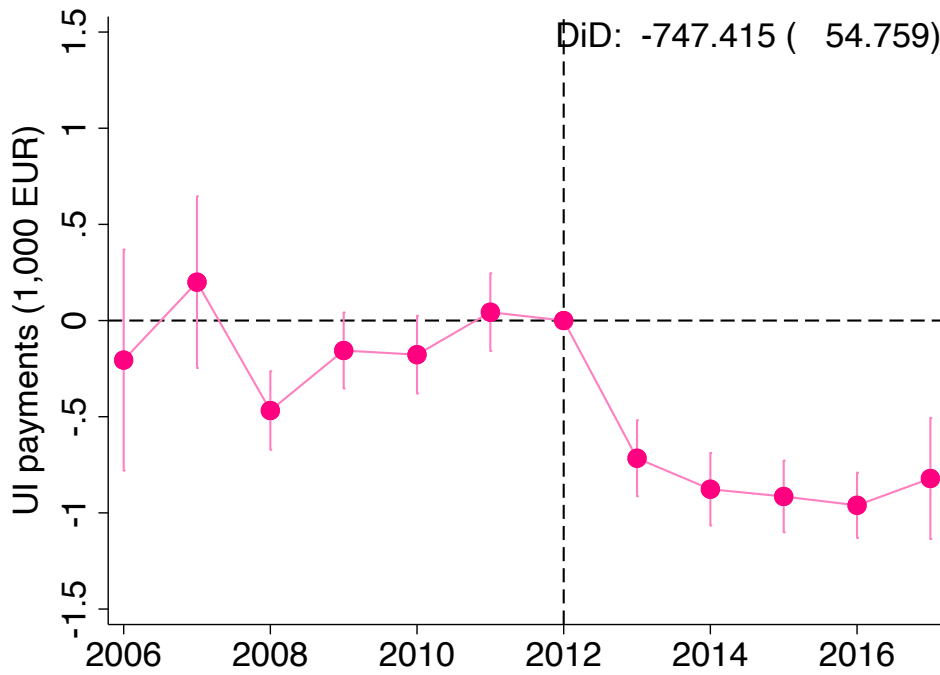
Figure B3: Cross-Municipality Analysis - Other Outcomes



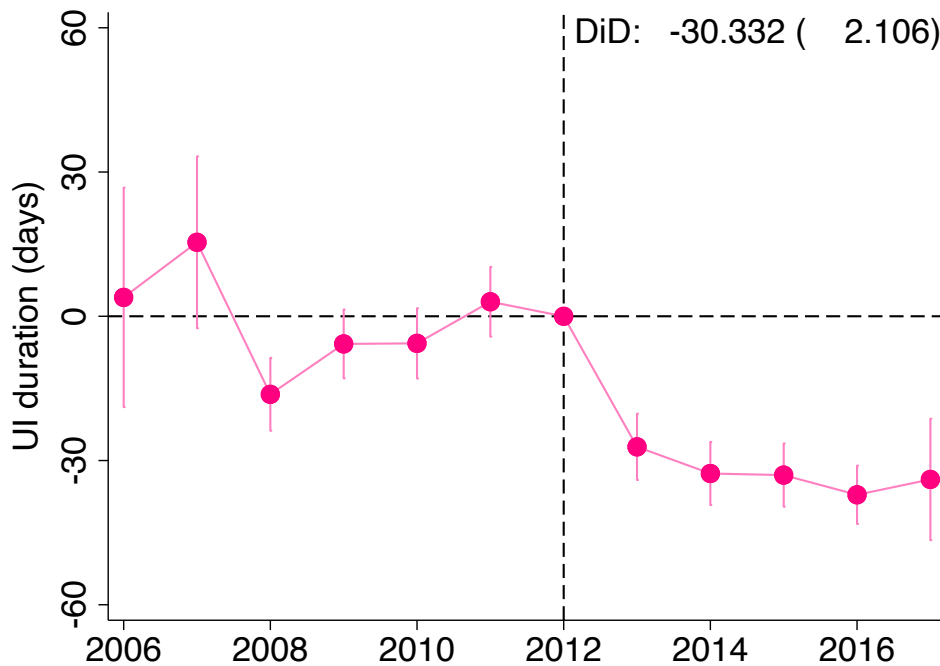
NOTE: This figure depicts the event-study coefficient estimates showing the impact of the payroll tax cut for female hires on 1) log of job duration of female workers (Panel A), 2) log of job duration of male workers (Panel B), 3) log of female new hires (Panel C), 4) log of male new hires (Panel D), 5) log of days of work of female workers (Panel E), and 6) log of days of work of male workers (Panel F). The figure plots coefficient estimates and the 95 percent confidence intervals obtained from Equation (1): 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 (2012). Standard errors are clustered by municipality. The figure also reports the difference-in-differences coefficient estimates. Data are from matched employer-employee data over the 2005–2020 period on the universe of Italian nonagricultural private-sector workers, collapsed at the municipality-year level.

Figure B4: Cross-Municipality Analysis—Unemployment Insurance

A. Unemployment insurance payments



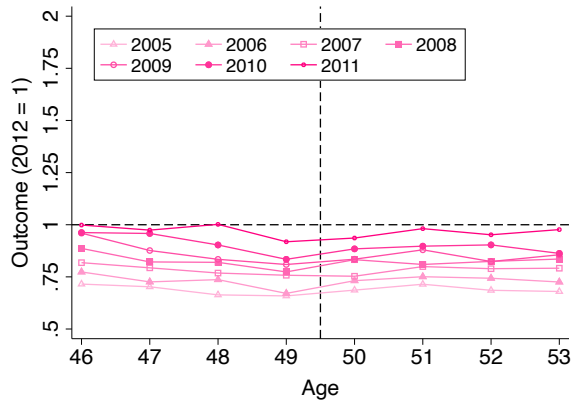
B. Unemployment insurance duration



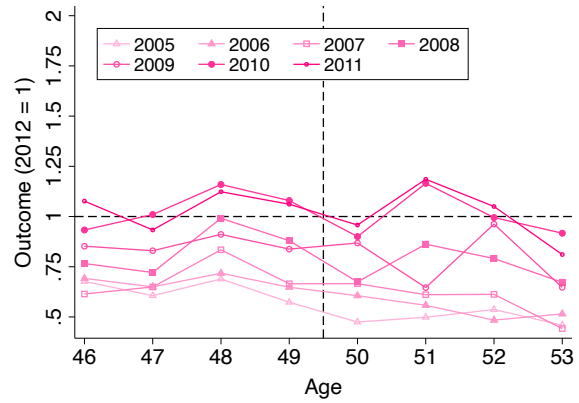
NOTE: This figure depicts the event-study coefficient estimates showing the impact of the payroll tax cut for female hires on 1) log of UI benefits payments (euros) and 2) log of UI benefits duration (number of days). The figure plots coefficient estimates and the 95 percent confidence intervals obtained from Equation (1): 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 (2012). Standard errors are clustered by municipality. The figure also reports the difference-in-differences coefficient estimates. Each year in the graphs refers to the year when a job was terminated and UI benefits payment started. Data are from UI benefit recipients over the 2006–2017 period on the universe of Italian nonagricultural private-sector workers, collapsed at the municipality-year level.

Figure B5: Cross-Cohort Analysis—Prereform Years

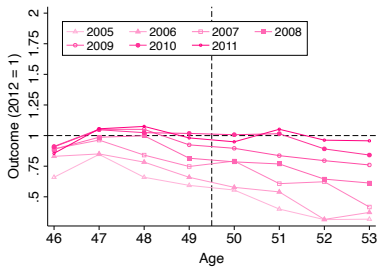
A. Employment



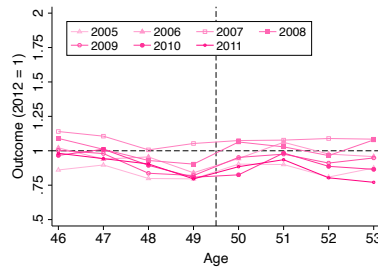
B. Wages



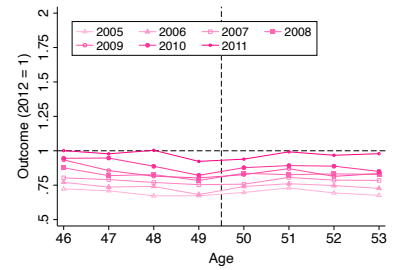
C. Job duration



D. New hires



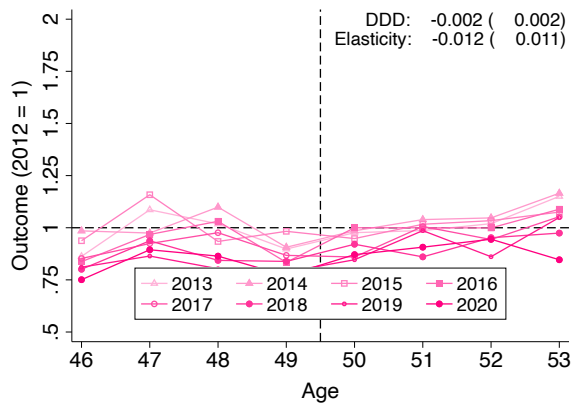
E. Days of work



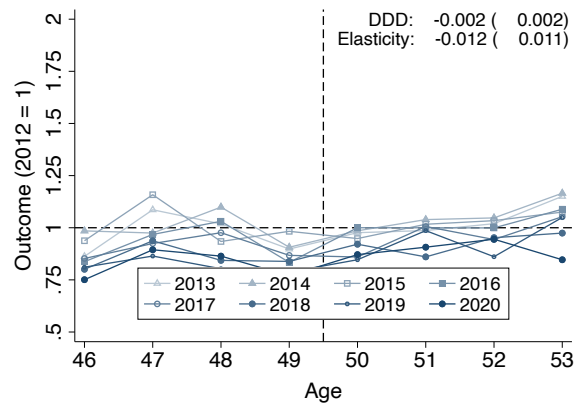
NOTE: The figure depicts the difference in employment, net wages, job duration, new hires count, and days of work of female workers between municipalities and by birth cohort, focusing on workers aged 46–53. Each line in the graphs refers to a single prereform year, and each outcome is normalized with respect to 2012. The vertical axis displays the difference across municipalities in each outcome by age and year relative to 2012. Data are from matched employer-employee data over the 2005–2020 period on the universe of Italian nonagricultural private-sector workers, collapsed at the municipality/birth cohort/year level.

Figure B6: Cross-Cohort Analysis—Other Outcomes

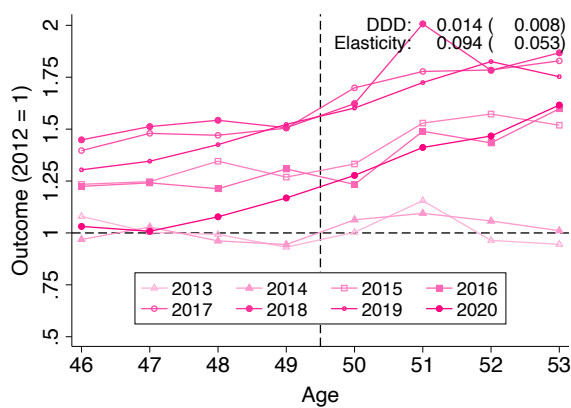
A. Female job duration



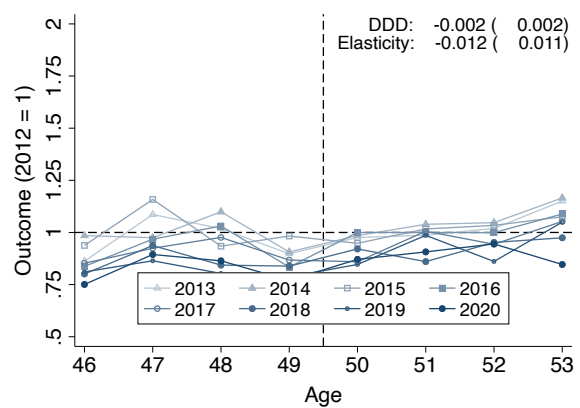
B. Male job duration



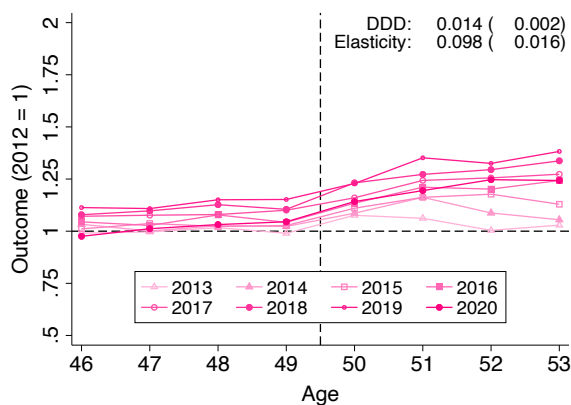
C. Female new hires



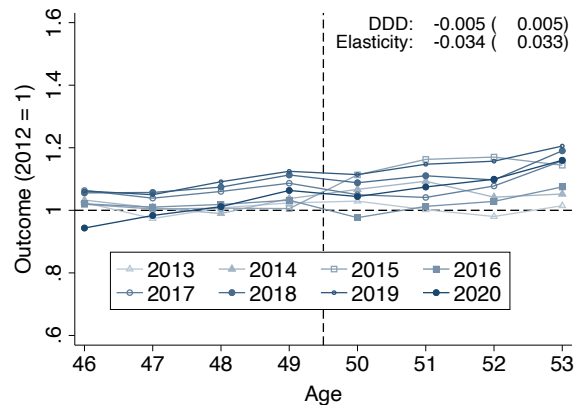
D. Male new hires



E. Female days of work



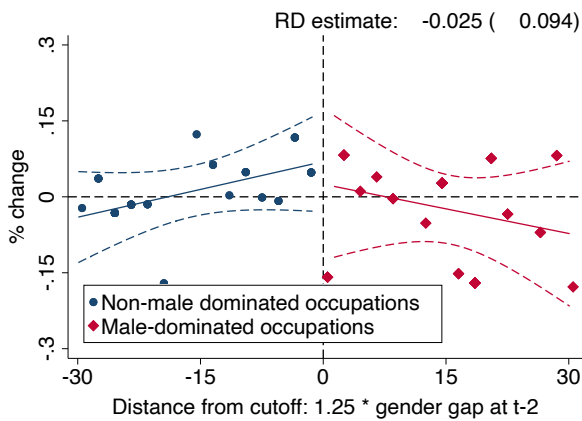
F. Male days of work



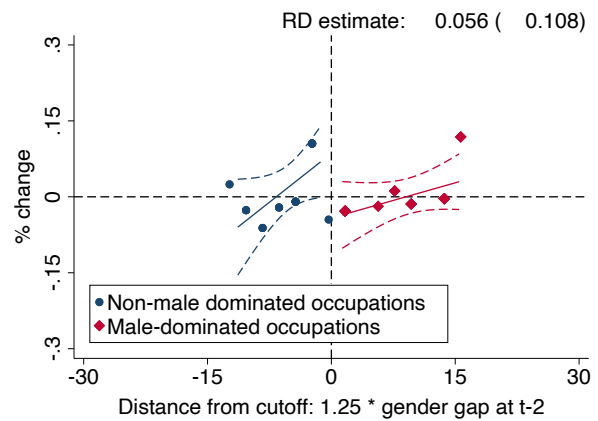
NOTE: The figure depicts the difference in job duration (top panel), new hires count (middle panel), and days of work (bottom panel) between municipalities and by birth cohort, focusing on workers aged 46–53. Each line in the graphs refers to a single postreform year, and each outcome is normalized with respect to 2012. The vertical axis displays the difference across municipalities in each outcome variable by age and year relative to 2012. Each graph also reports the triple different coefficient estimate and elasticity estimate using cohort fixed effects, year fixed effects, and birth-cohort fixed effects. Data are from matched employer-employee data over the 2005–2020 period on the universe of Italian nonagricultural private-sector workers, collapsed at the municipality/birth cohort/year level.

Figure B7: Cross-Occupation Analysis

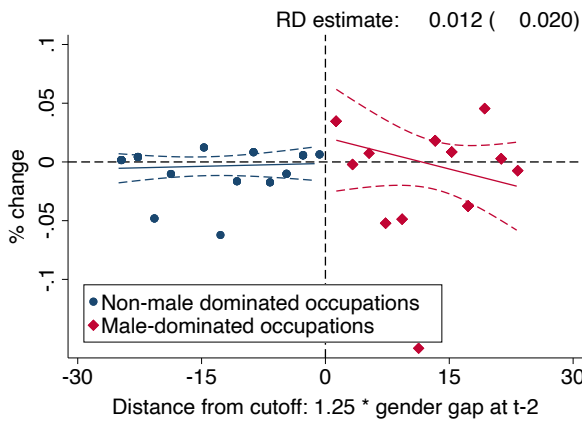
A. Female/male job duration in nondisadvantaged municipality



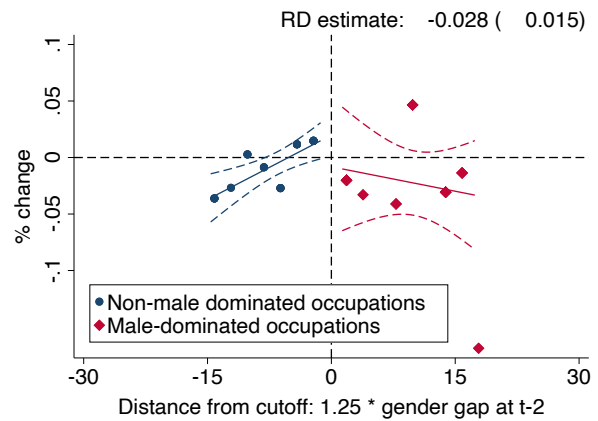
B. Female/male job duration in disadvantaged municipality



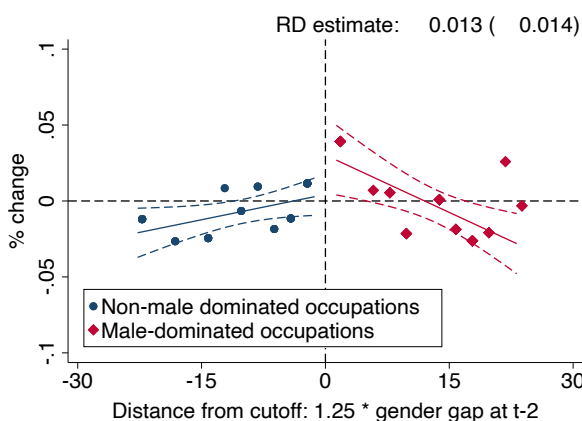
C. Female/male new hires in nondisadvantaged municipality



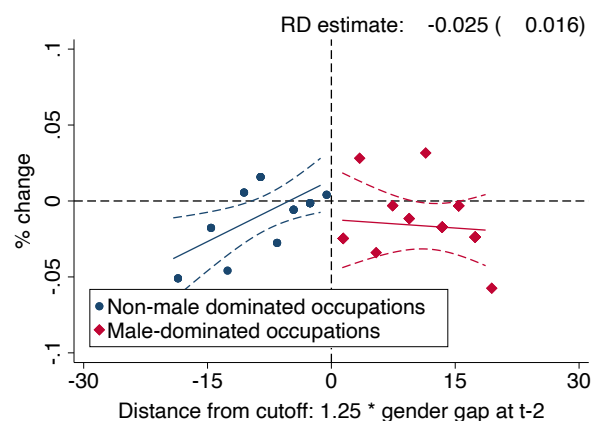
D. Female/male new hires in disadvantaged municipality



E. Female/male days of work in nondisadvantaged municipality

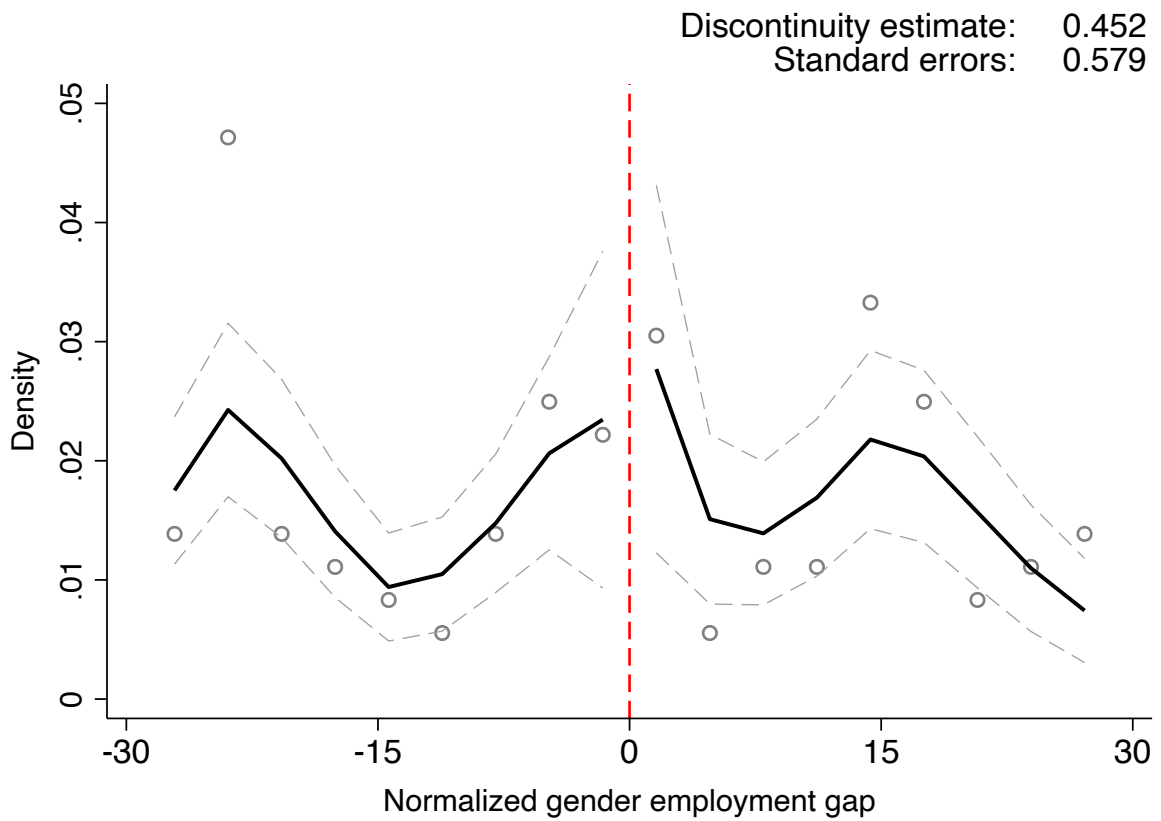


F. Female/male days of work in disadvantaged municipality



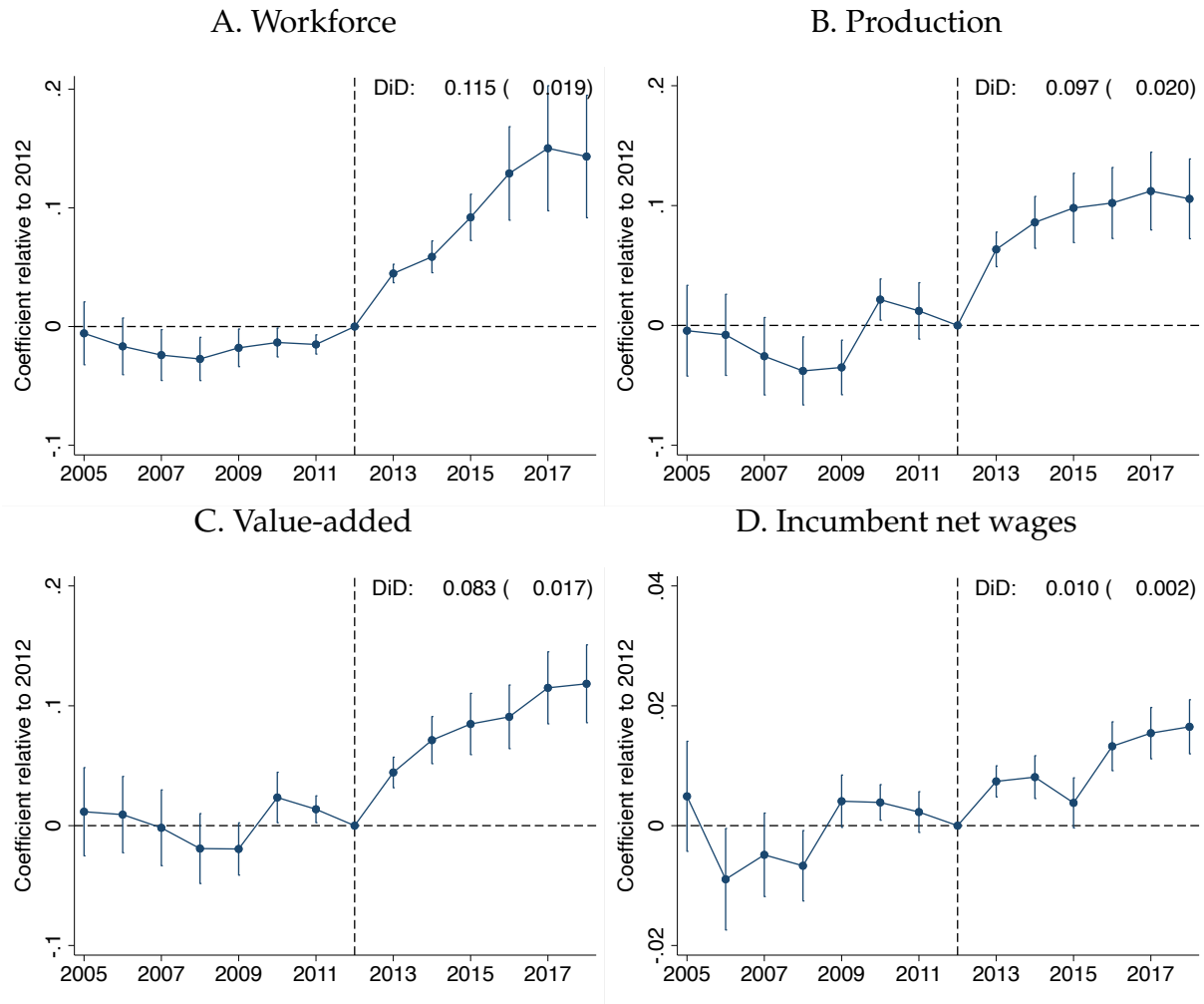
NOTE: This figure presents the RD graphs, along with β coefficient estimates and standard errors computed from Equation (3). The top two panels depict the variation in the female-male job duration ratio; the middle two panels the variation in the female-male new hires ratio; and the bottom two panels the variation in the female-male days-of-work ratio. The left-hand-side graphs focus on nondisadvantaged municipalities, where the eligibility criteria create a discontinuity across occupations; the right-hand-side graphs on disadvantaged municipalities, where the minimum nonemployment duration requirement remains unchanged.

Figure B8: Density of the Running Variable



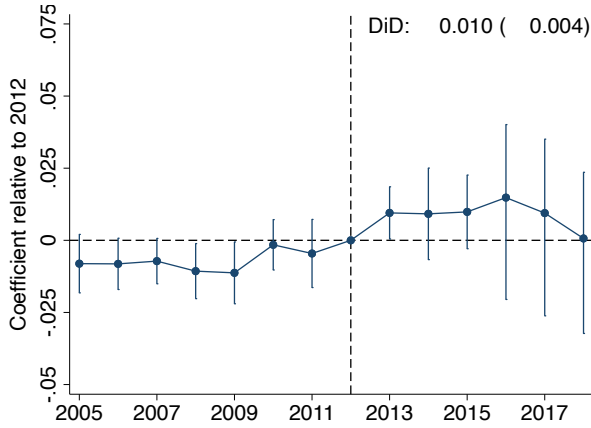
NOTE: The figure shows the distribution of the (normalized) occupation-specific gender employment gap around the eligibility cutoff defining a stricter exposure to the payroll tax cut (red vertical line) in occupations where the normalized gender employment gap is between -30 and 30 percent. Circles 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. The discontinuity estimate (standard errors) is -0.452 (0.579).

Figure B9: Event-Study Estimates on Firm-Level Outcomes

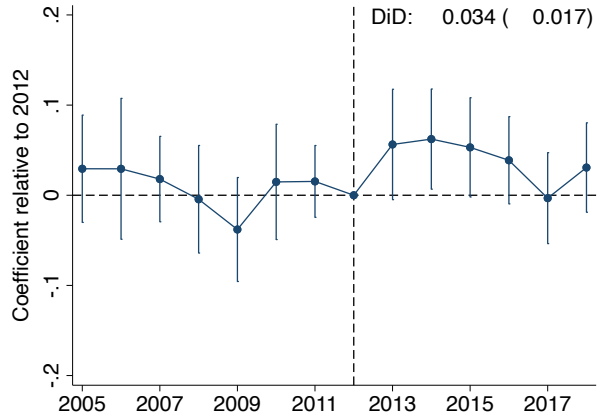


NOTE: Continues on next page.

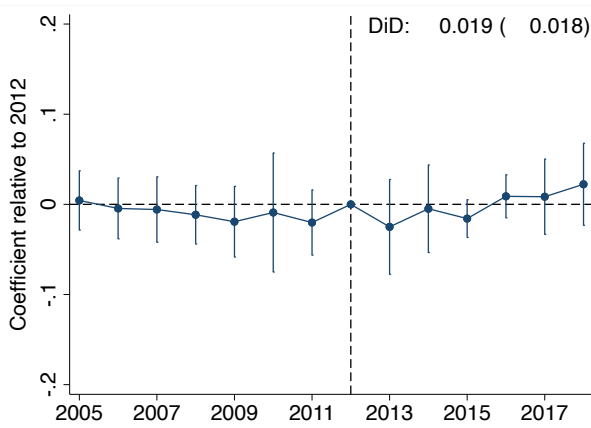
E. Return on assets



F. Return on equity



G. Return on investment



NOTE: This figure presents event-study coefficient estimates and 95 percent confidence intervals on firm-level outcomes, along with coefficient estimates and firm-level standard errors computed from a difference-in-differences model with firm and year fixed effects. The sample is composed of two groups: 1) firms in the top quartile of the prereform female workforce distribution (“highly exposed” firms: the treated group in the event-study regressions) and 2) firms in the third quartile (“fairly highly exposed” firms: the control group in the event-study regressions).

Table B4: Summary Statistics, Employees

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

NOTE: This table presents summary statistics of payroll tax-cut recipients.

Table B5: Summary Statistics, Employers

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

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

Table B6: Take-Up Rate and Firm-Level Outcomes

	(1)	(2)	(3)	(4)	(5)
Outcome: take-up rate; Specification: OLS—first stage					
$ShareFemalePre_i \cdot Post_t$	0.267*** (0.010)	0.291*** (0.010)	0.284*** (0.010)	0.284*** (0.010)	0.281*** (0.011)
F-stat	703.093	798.766	756.342	756.211	605.626
Outcome: log(production value); Specification: 2SLS					
$TakeUpRate_{i,t}$ (x10)	0.017*** (0.002)	0.019*** (0.001)	0.014*** (0.001)	0.014*** (0.001)	0.007*** (0.002)
Outcome: log(workforce); Specification: 2SLS					
$TakeUpRate_{i,t}$ (x10)	0.040*** (0.002)	0.037*** (0.002)	0.035*** (0.002)	0.035*** (0.002)	0.015*** (0.001)
Outcome: log(incumbent wages); Specification: 2SLS					
$TakeUpRate_{i,t}$ (x10)	0.002*** (0.000)	0.002*** (0.000)	0.002*** (0.000)	0.002*** (0.000)	0.006*** (0.001)
Outcome: log(value-added); Specification: 2SLS					
$TakeUpRate_{i,t}$ (x10)	0.009*** (0.002)	0.011*** (0.001)	0.006*** (0.001)	0.006*** (0.001)	0.013*** (0.002)
Outcome: return on assets; Specification: 2SLS					
$TakeUpRate_{i,t}$ (x10)	0.004*** (0.001)	0.004*** (0.001)	0.003*** (0.001)	0.003*** (0.001)	0.003*** (0.001)
Outcome: return on equity; Specification: 2SLS					
$TakeUpRate_{i,t}$ (x10)	0.012*** (0.005)	0.011*** (0.005)	0.012*** (0.004)	0.011** (0.004)	0.004 (0.006)
Outcome: return on investment; Specification: 2SLS					
$TakeUpRate_{i,t}$ (x10)	0.007* (0.004)	0.007* (0.004)	0.008 (0.006)	0.007 (0.005)	0.002 (0.005)
Observations			2,900,773		
# of firms			248,002		
Firm and year FE	Yes	Yes	Yes	Yes	Yes
Region-year FE	No	Yes	Yes	Yes	Yes
Legal form-year FE	No	No	Yes	Yes	Yes
Size-year FE	No	No	No	Yes	Yes
Industry-year FE	No	No	No	No	Yes

NOTE: This table lays out the firm-level effects of the payroll tax cut on female hires. In each panel, the first column reports estimates from the baseline model with firm and year fixed effects. In columns 2–5, I cumulatively add region-year fixed effects, legal form-year fixed effects; firm size-year fixed effects; and industry-year fixed effects. Standard errors are clustered at the firm level in parentheses.

Table B7: Take-Up Rate and Firm-Level Outcomes Using a Balanced Panel

	Take-up rate (1)	log of workforce (2)	log of prod (3)	log of VA (4)	log of wages (5)	ROA (6)	ROE (7)	ROI (8)
$ShareFemale_{i,t} \cdot Post_t$	0.289*** (0.010)							
$TakeUpRate_{i,t}$		0.065*** (0.005)	0.008*** (0.001)	0.004*** (0.000)	0.001*** (0.000)	0.005*** (0.002)	0.008** (0.003)	0.006** (0.003)
Observations				1,060,164				
# of firms				75,726				
Firm and year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-stat	774.852	-	-	-	-	-	-	-

NOTE: This table studies the firm-level effects of the payroll tax cut on female hires using a balanced panel of firms. The sample is composed of firms that span the entire period covered in the analysis (2005–2018). Column 1 reports the “first-stage” relationship between the firm-level prereform share of the female workforce and the take-up rate. Column 2 shows 2SLS impacts of the (predicted) log of the take-up rate (multiplied by 10) on the following firm-level outcomes: log of workforce (column 2), log of production value (column 3), log of value-added (column 4), log of (noneligible) incumbent workers’ average net wage (column 5), return on assets (column 6), return on equity (column 7), and return on investment (column 8). Each specification includes firm fixed effects and year fixed effects. Standard errors are clustered at the firm level in parentheses.