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RFBerlin
ROCKWOOL Foundation Berlin –
Institute for the Economy
and the Future of Work

Gormannstrasse 22, 10119 Berlin
Tel: +49 (0) 151 143 444 67
E-mail: info@rfberlin.com
Web: www.rfberlin.com

Female-Targeted Hiring Subsidies, Firm Learning, and Women's Employment*

Mimosa Distefano, Lorenzo Incoronato, Anna Raute

August 2026

Abstract

Women often struggle to re-enter employment after career breaks, possibly because employers are uncertain about their productivity. We study whether hiring subsidies help firms overcome this uncertainty and hire from this group. Exploiting an Italian policy that temporarily cut payroll taxes for women hired from non-employment, we find that firms persistently hire more women with career breaks, including mothers, following subsidy adoption. Consistent with employer learning about target-group productivity, firms with better initial matches later hire more from this group. Subsidized workers also show stronger labor-market attachment. These findings suggest demand-side interventions can complement supply-side policies in addressing gender gaps.

JEL Codes: J16, J23, H25, D83

Keywords: gender employment gap; mothers; hiring subsidies; employer learning; firm hiring behavior.

*Distefano: London School of Economics, CEP, RFBerlin, email: m.distefano@lse.ac.uk; Incoronato: University of Naples Federico II, CSEF, CESifo, RFBerlin, email: lorenzo.incoronato@unina.it; Raute: Queen Mary University of London, CEPR, CESifo, RFBerlin, email: a.raute@qmul.ac.uk.

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1 Introduction

Women remain underrepresented in labor markets around the world. A key driver of persistent gender inequalities is the arrival of children, as it often leads women to take career breaks (Adda, Dustmann, & Stevens, 2017; Angelov, Johansson, & Lindahl, 2016; Kleven, Landais, & Søgaard, 2019). Beyond concerns about equity, gender disparities in employment can entail substantial losses in aggregate productivity (Hsieh et al., 2019). Policy efforts have largely focused on supply-side interventions, such as parental leave and subsidized childcare, which have had mixed success in strengthening women's long-run labor force attachment and earnings (Albanesi, Olivetti, & Petrongolo, 2023; Olivetti & Petrongolo, 2017).

In contrast, less attention has been given to demand-side forces, in particular employers' hiring decisions. Employers' reluctance to hire workers with career interruptions, especially when such interruptions are perceived as a signal of lower productivity, may constrain women's labor market opportunities (Arulampalam, 2001; Eriksson & Rooth, 2014; Kroft, Lange, & Notowidigdo, 2013).[1] These concerns may be particularly salient for mothers: sociological research shows that they are perceived as less competent and committed, and consequently face lower hiring probabilities (Correll, Benard, & Paik, 2007; Weisshaar, 2018). If firms are imperfectly informed about women returning after career breaks, or if gender stereotypes shape employers' beliefs about their productivity, policies that stimulate demand for this group may help increase women's labor market participation by encouraging firms to experiment with and learn about these workers.

This paper studies whether a hiring subsidy targeted at women with extended employment interruptions can improve their labor market outcomes by persistently affecting firm hiring decisions. We analyze the Italian "Bonus Donne" policy introduced in 2013, which provided a temporary and sizable reduction in employer social security contributions for hiring women from non-employment. Guided by a conceptual framework of employer learning, we study how firms adjust their hiring behavior after first using the subsidy. We find that firms using the subsidy

[1] Recent evidence suggests that general human capital depreciation during unemployment spells is small (Cohen, Johnston, & Lindner, 2025), implying that perceived productivity—rather than actual skill loss—may play a central role in firms' hesitancy to hire workers with employment interruptions.

expand their hiring of women with extended employment interruptions, including mothers, with no decline in firm performance. Central to our analysis, firms' early experiences with Bonus Donne hires shape their subsequent hiring decisions, consistent with employer learning. Subsidized workers, in turn, show lasting gains in labor market attachment.

Italy provides a well-suited setting for our analysis because gender differences in the labor market are particularly pronounced, with one of the lowest female employment rates and the largest gender employment gap in the European Union (Eurostat, 2025). As a result, raising female participation could potentially yield large economic gains (Carta et al., 2023). The Italian setting also offers several empirical advantages. We draw on rich matched employer-employee records from the Italian Social Security Institute (INPS), covering the universe of private-sector workers and firms. A key feature of the data is that we observe whether a worker is hired through the Bonus Donne subsidy, which enables us to measure both worker- and firm-level take-up and to track subsidized firms and workers before and after adoption.

We present a conceptual framework linking hiring subsidies to employer learning about a group of workers. The model builds on recent work showing that employers update their beliefs through direct experience with workers (e.g., Benson & Lepage, 2025; Lepage, 2024). In our setting, firms are uncertain about the productivity of workers with extended employment interruptions and may thus be reluctant to hire them. This uncertainty concerns both the productivity of specific applicants a firm considers, and more fundamentally, how well workers from this group match the firm—for instance, women returning after long career gaps. A central question is therefore whether direct experience leads a firm to update its beliefs about its match with the group. When such uncertainty discourages hiring, firms may fall into a "learning trap" in which workers from the target group are rarely hired, and firms have little opportunity to update their beliefs. A temporary hiring subsidy can shift incentives enough to induce experimentation with these workers, generating new information about the group-firm match. The framework implies that (*i*) hiring from the target group under the subsidy can lead to persistent changes in firms' hiring, and (*ii*) these effects depend on the quality of firms' initial matches with subsidized workers.

To examine the short- and long-run changes in firms' hiring patterns following their first subsidized hire under the Bonus Donne, we implement a staggered event-study design comparing treated firms—defined as those that hire at least one worker under the subsidy—to control firms that hire women in the same year, but not through the program. To account for differences between these two groups, we combine this approach with a matching strategy. Specifically, we pair each treated firm with an observationally similar control firm based on predetermined characteristics including firm size, average wages, the share of female employees, and the number of hires prior to subsidy adoption.

We find that treated firms increase their hiring of women with extended employment interruptions, including mothers, relative to control firms. In the year of first subsidy use, treated firms hire about 17% more women with long non-employment spells, though this contemporaneous increase partly reflects the fact that treatment is defined by first subsidy use. The result we emphasize is its persistence: three to five years later, they still hire roughly 3% more from this group than control firms. We observe a similarly positive and persistent increase in the hiring of mothers. These patterns are robust across alternative specifications that exploit only variation in the timing of subsidy use across treated firms and do not rely on a control group. Treated firms also grow in size and value added, with no evidence of a decline in value added per worker.

Why were firms reluctant to hire from this group before the subsidy? According to our conceptual framework, firms may be uncertain about the productivity of workers with extended employment interruptions and the hiring subsidy can induce them to experiment and learn about the group. If firms learn from interacting with workers, future hiring decisions should depend on the quality of the initial match. We test this implication by examining whether hiring evolves differently across firms that experience better versus worse initial matches with subsidized workers, where match fit is proxied by estimated Mincer wage residuals at the time of hiring. Consistent with employer learning, firms with better initial matches subsequently hire more women with extended employment interruptions and more mothers. This comparison also allows separating learning from a cost-based explanation: since both good- and bad-match firms hired subsidized workers, cheaper labor alone cannot explain the lasting increase in hiring from the target group.

Finally, we shift the focus to workers hired under the Bonus Donne and investigate their employment trajectories after hiring and beyond the expiration of the subsidy. Leveraging our ability to follow workers across firms, we implement a matched worker-level event-study design that compares women hired under the subsidy with observationally similar women hired in the same year at matched control firms. We document positive effects on employment probabilities for Bonus Donne workers that persist beyond the subsidy period. Even five years after hiring, subsidized workers are about 8 percentage points more likely to be employed than comparable women at control firms. Importantly, these gains reflect not only higher retention at the hiring firm, but also stronger attachment to the broader labor market, as many treated workers transition into jobs at other firms. We also find no evidence that the Bonus Donne was passed through to workers in the form of higher wages, consistent with Rubolino (2025).

This paper contributes to several strands of the literature. First, it offers a new perspective on hiring subsidies. Hiring subsidies have long been implemented in many countries to boost employment among low-wage workers (Katz, 1998), or as a countercyclical tool during recessions (Albanese, Cockx, & Dejemeppe, 2024; Neumark, 2013; Neumark & Grijalva, 2017). The hiring subsidy we analyze is instead targeted at women with extended employment interruptions, independently of their wage level or the business cycle. While earlier studies relied largely on regional or survey data and focused on aggregate outcomes (e.g., Bishop, 1981; Perloff & Wachter, 1979), recent work has provided micro-level evidence on the effects of hiring subsidies. Cahuc, Carcillo, and Le Barbanchon (2019) document positive employment effects of a French subsidy targeting low-wage workers, whereas Jain, Mommaerts, and Weaver (2026) draw on administrative records from Wisconsin to find that the U.S. Work Opportunity Tax Credit largely operated as a transfer to firms without increasing employment or hiring.[2] Kunze, Palczynska, and Magda (2026) document gender differences in the effectiveness of hiring subsidies for young unemployed workers in Poland, with higher probability of exiting unemployment for women

[2]There is a sizable literature on the effects of permanent reductions in payroll taxes (e.g., Benzarti & Harju, 2021a, 2021b; Bíró et al., 2024; Huttunen, Pirttilä, & Uusitalo, 2013; Ku, Schönberg, & Schreiner, 2020; Saez, Matsaganis, & Tsakloglou, 2012; Saez, Schoefer, & Seim, 2019, 2021). Unlike a cut to payroll taxation, which covers both new hires and incumbent workers and is typically applied automatically, hiring subsidies apply only to new hires and require active take-up. Therefore, findings from the payroll-tax literature are only partially informative about hiring subsidies. In addition, none of these studies address gender-specific aspects.

with pre-school age children. In related work, Rubolino (2025) studies the tax incidence of the Bonus Donne program and its effects on worker outcomes, including sorting across jobs and effects on incumbents. Our paper instead examines how hiring subsidies shape firms' recruitment decisions through an experiential learning mechanism. This channel, highlighted in recent work on employer learning (Lepage, 2024), has received little attention in the hiring subsidy literature. One exception is Barham, Cadena, and Turner (2023), who study a supported work program in Colorado and provide evidence consistent with employer learning about the match quality of individual workers. In contrast, we study a hiring subsidy targeted at women with extended employment interruptions and examine how initial exposure to subsidized hires affects subsequent hiring decisions for the group more broadly.

Second, our paper speaks to the literature on regulatory policies aimed at increasing labor market representation, such as gender quotas (e.g., Bertrand et al., 2018; Maida & Weber, 2022; Miller, Peck, & Seflek, 2022) and affirmative action policies targeting minorities (e.g., Coate & Loury, 1993; Fryer & Loury, 2013). While quotas and affirmative action policies rely on regulatory requirements to promote representation, hiring subsidies operate through financial incentives for employers to hire disadvantaged workers. Evidence on affirmative action—largely based on U.S. programs targeting racial minorities—shows that temporary interventions can have persistent effects on workforce composition (Miller & Segal, 2012; Miller, 2017), although recent evidence suggests that such effects may not generalize to other periods or settings (Amano-Patiño, Contractor, & Aramburu, 2025). Our analysis adds to this literature by studying a hiring subsidy and highlighting an experiential learning mechanism through which firms' initial exposure to the target group can shape subsequent hiring behavior.

Finally, this study relates to the literature on experiential learning in hiring. A growing body of work shows that employers' hiring decisions are shaped by their own experiences, particularly with underrepresented groups (e.g., Lepage, 2024; Leung, 2018). Stereotypes or inaccurate beliefs about worker productivity can impede learning (Bohren, Imas, & Rosenberg, 2019), and negative experiences may discourage future hiring from these groups, preventing the correction of biased beliefs (Bardhi, Guo, & Strulovici, 2025; Benson & Lepage, 2025).[3] Related work

[3] See Onuchic (forthcoming) for a broader discussion of "learning traps" in recent theories of discrimination, and

also emphasizes the role of experimentation in hiring under uncertainty (Li, Raymond, & Bergman, 2026). In our setting, hiring subsidies lower the short-run cost of experimentation, inducing firms to recruit from groups they might otherwise avoid and generating new information about match quality. We contribute to this literature by providing evidence that policy-induced experimentation leads to persistent changes in firms' hiring behavior toward the targeted group, consistent with learning about match quality.

The paper is organized as follows. Section 2 describes the institutional setting and the policy. Section 3 presents the data and provides descriptive statistics. Section 4 introduces our conceptual framework. Section 5 reports the empirical strategy, the main estimates and robustness checks, while Section 6 presents evidence on firm learning. Section 7 explores worker-level effects. The last section concludes.

2 Institutional Background

2.1 Gender Gaps in the Italian Labor Market

There has long been a substantial gender divide in Italy's labor market. Women's employment and labor force participation are low compared to other advanced economies: Italy has one of the lowest female employment rates among EU countries, at 57%, and the largest gender employment gap (Eurostat, 2025). Much of this gap emerges around childbirth: the child penalty alone explains 60% of the gender employment gap (Kleven, Landais, & Leite-Mariante, 2025). The employment drop following childbirth also implies substantial and persistent earnings losses for mothers (Casarico & Lattanzio, 2023). These gaps are reinforced by the institutional setting. Like other European countries, Italy exhibits a dual labor market, where incumbent workers on open-ended contracts ("insiders") enjoy a high degree of employment security, while new hires ("outsiders") are often offered temporary contracts with limited unemployment protection (Boeri, 2011). This segmentation has a strong gender dimension: women are over-represented among outsiders and experience more intermittent participation through more precarious contracts

Escudé et al. (2026) for a general model of statistical discrimination with inaccurate priors. Related experimental evidence shows that providing employers with information about worker performance or reducing perceived hiring risks increases hiring from disadvantaged groups (Cullen, Dobbie, & Hoffman, 2023).

(Carta et al., 2023). The historically strong employment protection for incumbent jobs in Italy, partly shaped by traditional male breadwinner norms, contributes to lower female employment rates (Giavazzi, Serafinelli, & Schiantarelli, 2013).[4]

2.2 The "Bonus Donne"

At the height of the European sovereign debt crisis, many European countries, including Italy, implemented structural reforms aimed at stimulating employment and growth. On June 28, 2012, the Italian government passed a comprehensive reform package (Law n. 92/2012, *Provisions on labor market reform from a growth perspective*). This legislation introduced various measures intended to create an inclusive and dynamic labor market, increase employment, and improve job stability and quality, with the aim of reducing unemployment in the long run.[5]

The hiring subsidy analyzed in this paper was one of the measures introduced as part of this reform package. It came into force on January 1, 2013 and was dubbed "Bonus Donne" ("Bonus Women") by the media. The low female employment rate was an important policy priority for the government at the time: *"Ensuring the full inclusion of women in all areas of working life is a matter that cannot be postponed. It is necessary to address issues concerning the reconciliation of work and family life [...] as well as to consider the introduction of preferential taxation for women"* (Interview with Prime Minister Monti, La Repubblica, 2011). The subsidy granted a temporary 50% reduction in employers' social security contributions for hiring non-employed women. Considering that employer-borne social security contributions account for about 20% of the wage in Italy, the Bonus Donne provided a roughly 10% cut to employers' labor costs for new hires, comparable in size to hiring subsidies in the French and U.S. contexts (Cahuc, Carcillo, & Le Barbanchon, 2019; Neumark & Grijalva, 2017).[6]

The subsidy varied along several margins, including contract type, eligibility duration, and worker characteristics. Firms received the subsidy for 12 months when hiring a worker on a

[4]Gender gaps in the Italian labor market are further exacerbated by less egalitarian gender norms (Campa, Casarico, & Profeta, 2011). Childcare availability in Italy remains relatively low, still falling short of the 33% European target especially in Southern regions (Istat, 2025).

[5]Appendix A.1 provides additional detail on the policy package and the political context.

[6]Both employers and employees are subject to payroll taxes in Italy. Payroll tax rates have been stable since 2005 at about 20.6% for employers and about 10% for employees.

fixed-term contract, and for 18 months when hiring a worker on an open-ended contract or converting a fixed-term contract into an open-ended one within 12 months. The extension of the benefit period for open-ended contracts reflects the policymaker's explicit aim of promoting stable employment and labor market attachment among non-employed women.[7] The eligible target group consisted of individuals who had been out of employment for at least 24 months, regardless of whether they were registered as unemployed. The Bonus Donne also applied to new labor market entrants. The 24-month non-employment requirement was more lenient for some categories of workers.[8]

The legislation also imposed several requirements on firms, entailing non-trivial administrative costs. First, firms were required to submit a separate online claim to the Italian Social Security Institute for each subsidized hire, and employers' social security contributions were reimbursed ex post at the start of the following tax year. Second, the subsidy could be applied only if no other worker with a legally granted preferential hiring right was available for the position.[9] Finally, the subsidy was available only to firms that had not experienced a net decrease in employment—excluding worker resignations and retirements—during the 12 months prior to claiming the Bonus Donne. Net job growth requirements are common in hiring subsidies; the U.S. New Jobs Tax Credit, for instance, was an "incremental employment" subsidy (Katz, 1998). Such requirements often impose significant administrative burdens on firms and limit the pool of eligible users (Neumark, 2013).

These provisions, together with firms' general reluctance to hire workers targeted by such subsidies (Hamersma, 2003; Katz, 1998), help explain the relatively low take-up of the Bonus Donne. Using the data described in Section 3, we calculate that 3.8% of firms used the Bonus

[7]The subsidy applied to private-sector workers on both full-time and part-time contracts, but excluded apprenticeships and managerial positions. The total euro amount of the subsidy per worker was uncapped until 2022, which lies outside our analysis period.

[8]The non-employment requirement was reduced to 6 months for women hired in male-dominated occupations or sectors, or resident in municipalities eligible for EU Structural Funds. For women over the age of fifty, eligibility began after 12 months of registered unemployment, and for this group, the law required formal unemployment status. The subsidy also applied to men over fifty in this category. Few men were hired through it, however, and these hires were concentrated in specific, male-dominated industries such as construction and waste management. We exclude subsidized male hires from our analysis.

[9]In Italy, a worker previously employed under a fixed-term contract for more than six months has a preferential right to be rehired by the same firm within six months of the contract's end.

Donne to hire at least one woman between 2013 and 2019. Among all non-employed women hired, the share hired under the subsidy was initially low and increased over time, reaching 8% in 2019 (Appendix Figure A.1).[10] Reflecting growing interest from firms, the Bonus Donne has been retained and made more generous over time. Starting in 2022, the government has increased the subsidy to cover 100% of employers' social security contributions, although subject to a cap.

3 Data and Descriptive Evidence

This section describes the data used in the analysis and presents descriptive statistics on the workers and firms that received subsidies under the Bonus Donne.

3.1 Data Description and Sample Selection

We use administrative matched employer-employee data from the Italian Social Security Institute (INPS), which we access through the VisitINPS Scholars program. The data cover the universe of private-sector workers and firms subject to the Italian social security system, thus excluding the self-employed and public-sector employees. Several features of this dataset make it uniquely suited to our research questions.

Take-up. Crucially, and unlike in most other contexts, we observe subsidy take-up at both the individual and firm levels, as the data directly report whether a worker was hired under the Bonus Donne. This enables us to characterize hiring-subsidized workers and firms relative to the average Italian worker and firm. To the best of our knowledge, no other countrywide dataset used in the study of hiring subsidies allows simultaneous observation of both worker- and firm-level take-up, along with detailed information on the firm's workforce.[11]

[10] Another possible reason for low take-up was that few firms were initially aware of the subsidy. The Bonus Donne was introduced alongside more controversial and prominent policies that dominated public discourse (see Appendix A.1). Previous studies have also documented limited awareness of hiring subsidies—for example, only 34% of U.S. firms were aware of the U.S. New Jobs Tax Credit (Perloff & Wachter, 1979).

[11] For example, Cahuc, Carcillo, and Le Barbanchon (2019) use firm-level administrative data for France, but lack worker-level information. A recent exception is Jain, Mommaerts, and Weaver (2026), who use matched data for Wisconsin.

Workers. We observe workers' employment histories since labor market entry, along with their labor earnings and the number of weeks worked each year (in full-time equivalent), which allows us to compute a worker's full-time-equivalent weekly wage. The data also report information on the employment contract (part-time versus full-time, fixed-term versus open-ended), broad occupational category (apprenticeship, blue-collar, white-collar, and manager), and demographic characteristics such as age, gender, place and year of birth, year of labor market entry, and municipality of work.[12]

Defining mothers. Maternity leave spells are available in the INPS data starting in 2005, which enables us to identify births among women employed prior to childbirth, as is common in studies using administrative employer-employee data (e.g., Lalive et al., 2014; Schönberg & Ludsteck, 2014). Throughout our period of analysis, paid mandatory maternity leave in Italy lasts five months—remunerated at 80% of the salary—of which up to two can be taken before childbirth. Pregnant workers also benefit from employment protection. Voluntarily exiting the labor force without taking leave results in the loss of both job protection and maternity benefit entitlements. Consequently, nearly all women employed prior to childbirth take maternity leave.[13] We define mothers as women who experienced a maternity leave event in the previous four years, restricting attention to mothers with young children—a key group of interest for a policy targeting women's re-entry into the labor market.[14]

Firms. Worker-level data can be matched to firms using unique firm identifiers. The firm database reports additional information such as sector (up to 6-digit), location, and firm age. We also obtain balance sheet data from the Cerved Group databases, recording information on total costs, revenues, assets, value added, and profits. The Cerved data are available until 2018 for limited liability companies only and can be merged with the INPS data using a unique national

[12]Since 2010, more detailed information on occupations (based on the ISCO classification) is available for a subset of workers observed upon contract changes (e.g., layoffs, promotions). Due to the high incidence of missing values for this detailed occupation variable—especially during the early reform years—we do not use it in our analysis.

[13]Paid paternity leave in Italy is much shorter—a maximum of two to five working days during our analysis period. Parents could take additional leave, up to ten months, until their child is 12 years old, at a 30% replacement rate.

[14]Because our worker panel begins in 2008, this four-year window is slightly shorter in the first sample year and complete afterwards.

firm tax identifier.

Sample construction. We build a panel dataset consisting of one observation per worker per year between 2008 and 2019—approximately 160 million observations—which forms our worker sample and is used to construct workforce and hiring composition at the firm level.[15] We restrict the analysis to workers aged 18 to 67 and drop sectors that are only partially represented in the INPS data, such as agriculture, electricity and water supply, and international organizations. We drop the (very few) cases where earnings are negative or missing, and where the gender variable is missing. Finally, we exclude one-employee firms. After data cleaning and incorporating information on take-up, we calculate that about 180,000 women were hired through the Bonus Donne between 2013 (the year the subsidy was introduced) and 2019 (our last year of observation), and around 26,500 firms hired at least one female worker under the subsidy over the same period.[16]

3.2 Descriptive Statistics

Table 1 contrasts average characteristics of hiring-subsidized firms (Bonus Donne firms, Column 1), i.e., those that used the Bonus Donne at least once between 2013 and 2019, with those of the average Italian private-sector firm that did not use the subsidy (Column 2). Each variable is measured in 2012, the year before the introduction of the policy. Bonus Donne firms differ from the average Italian firm in several respects. They are larger (48 versus 18 employees on average; see also Appendix Table A.1 Panel (a) for details on the firm size distribution) and employ a higher share of women (57%) compared to the average firm in the data (44%). Bonus Donne firms also have a lower share of full-time contracts (63% versus 70% in other firms), consistent with greater use of part-time employment, and a higher proportion of female workers among their new hires (54% versus 42% in other firms). In addition, Bonus Donne firms hired more workers between 2010 and 2012 (both in absolute numbers and relative to firm size), suggesting differential growth trajectories. However, they pay about 3% lower wages, both on average and

[15] If a worker holds multiple jobs in a year, we select the job associated with the highest number of weeks worked; if these are equal, we keep the job with the highest weekly wage.

[16] We conclude our analysis in 2019, as Italy was severely impacted by the Covid-19 pandemic, which led to a nationwide lockdown in early 2020.

for female workers, and have lower average productivity (measured as value added per worker). These baseline differences, particularly in pre-policy hiring dynamics, motivate our matching approach: comparing each Bonus Donne firm to an observationally similar non-subsidized firm helps ensure that the two groups are on similar trajectories prior to adoption. Finally, Bonus Donne firms are more likely to be located in the South of Italy (Appendix Table A.1 Panel (b)) and to operate in relatively low-skill services such as wholesale and retail trade, hospitality, and administrative services (Appendix Table A.2). We will account for these regional and sectoral differences in robustness checks.

We then turn to subsidized female workers and compare them with the average female hire not hired under the subsidy between 2013 and 2019 (Columns 1 and 2 in Appendix Table A.3, respectively). Bonus Donne hires are, on average, older and earn slightly higher wages in both their current and previous jobs (if any) than the average female hire. While these differences may hint at positive selection, Bonus Donne workers have substantially longer employment interruptions, are slightly more likely to be mothers of young children (despite being older on average), and are more likely to be hired under a fixed-term contract, indicating weaker prior labor market attachment than the average female hire. Several of these characteristics—most notably the length of prior non-employment spells and motherhood status—feature prominently in our analysis of how firms adjust their hiring behavior following subsidy adoption.

4 Model

In this section, we outline a conceptual framework that clarifies how a one-time hiring subsidy may affect firm hiring decisions persistently, building on recent work on experience-based learning (Benson & Lepage, 2025; Lepage, 2024). Appendix A.4 provides more detail. Employers are uncertain about the firm-specific productivity of individual workers and take a worker's group membership into account when making hiring decisions; in addition, we assume that they are uncertain about the productivity of different groups at the firm. This uncertainty may be particularly pronounced for disadvantaged groups such as women with long non-employment spells—those targeted by the Bonus Donne—who are often perceived as "outsiders" to the labor market and for whom firms may have limited information (e.g., Benson, Board, & Meyer-ter-Vehn, 2024; Cornell & Welch, 1996; Lundberg & Startz, 1983). Through the model, we illustrate that a hiring subsidy can nudge firms to experiment by hiring from the targeted group and that such experimentation can generate learning about the group's productivity at the firm, potentially increasing hiring from that group in the long run.

A worker i from group g working in firm j has productivity

$$y_{igj} = \bar{y}_{gj} + \theta_{gj} + \theta_i,$$

where $\bar{y}_{gj}$ represents a known baseline fit of group g at firm j, θ_{gj} is an unknown group-firm component of productivity (i.e., match quality), and θ_i represents an unknown, idiosyncratic component of individual i. We assume that $\theta_{gj} \sim \mathcal{N}(\mu_{gj}, 1)$ and $\theta_i \sim \mathcal{N}(0, 1)$. Thus, in contrast to models of statistical discrimination (e.g., Aigner & Cain, 1977; Arrow, 1973; Phelps, 1972), where employers act on true group-level differentials, here the firm is uncertain about *both* the individual's productivity θ_i and the group-firm component of productivity θ_{gj}. These assumptions distinguish our framework by allowing firms not only to learn about individual productivity, but also about how well workers from a given group match with the firm.

The model spans two periods. In each period, firms may hire and thereby learn about the group.

The hiring process. The hiring process takes place in two stages. First, the firm decides whether to interview a candidate. Second, the firm decides whether to hire the candidate based on the signal observed at the interview. The firm observes only the worker's group membership g and decides whether to interview based on its prior belief about the expected productivity of group g at firm j (i.e., $\bar{y}_{gj}$ and μ_{gj}). Since interviews are costly (Kuhn & Yu, 2021), the firm interviews a candidate if the expected value of conducting the interview for workers from that group exceeds the interview cost, $V_g \geqslant \kappa > 0$. We define V_g below.

If the firm conducts the interview, it observes a noisy signal of worker i's productivity,

$$\tilde{y} = y_{igj} + \epsilon,$$

where $\epsilon \sim \mathcal{N}(0, \sigma^2)$. After observing $\tilde{y}$, the firm updates its belief about the worker's productivity, forming a posterior expectation $\hat{y}_{igj} = \mathbb{E}[y_{igj} \mid \tilde{y}]$.[17] We assume that the worker's wage w is proportional to their expected productivity at the firm, $w = \alpha \hat{y}_{igj}$, for some fixed $\alpha \in (0, 1)$. The firm's surplus from hiring the worker is $s = (1 - \alpha)\, \hat{y}_{igj}$, and the firm hires the worker if this surplus is non-negative.

The ex ante expected value of interviewing a candidate from group g is

$$V_g = \mathbb{E}_g \left[\max \left\{ 0, (1 - \alpha) \hat{y}_{igj} \right\} \right],$$

where the expectation is taken with respect to the firm's beliefs about productivity in group g, given its current information about group-firm fit ($\bar{y}_{gj}$ and μ_{gj}) and anticipated signal noise (σ^2). We assume that, in the absence of the subsidy, the firm's prior belief about the targeted group's productivity is sufficiently low that $V_g < \kappa$, creating a "learning trap" in which no interview takes place—and hence no updating or hiring.

Now consider a hiring subsidy that covers a proportion $\beta \in (0, 1)$ of the wage. This lowers the firm's effective wage cost to $(1 - \beta)\alpha \hat{y}_{igj}$, raising the firm's surplus from hiring the worker to $s = [(1 - \alpha) + \beta\alpha]\, \hat{y}_{igj}$. Accordingly, the ex ante expected value of interviewing a worker from group g increases to

$$V_g = \mathbb{E}_g \left[\max \left\{ 0, ((1 - \alpha) + \beta\alpha)\, \hat{y}_{igj} \right\} \right].$$

By raising V_g, the subsidy increases the expected return to interviewing candidates from the targeted group, encouraging firms to experiment by interviewing and hiring some workers from this group.[18]

Learning. Through the signal observed during the interview, the firm learns not only about the productivity of the individual worker, but also about the group-firm component of productivity, θ_{gj}. This learning is especially important in our context, as it connects the firm's initial interview, and any subsequent hiring decision induced by the subsidy, to the firm's future hiring decisions

[17] See Appendix A.4 for the full expression of the firm's updated beliefs, following standard Bayesian updating.

[18] Importantly, the firm's hiring rule—hiring all candidates with non-negative surplus—remains unchanged. The subsidy raises the expected return to interviewing, thereby shifting firm behavior at the interview margin.

toward the target group. The posterior expectation of θ_{gj} given the interview signal is:

$$\hat{\mu}_{gj} = \mathbb{E}\left[\theta_{gj}|\tilde{y}\right] = \frac{1}{2+\sigma^2}(\tilde{y} - \bar{y}_{gj}) + \frac{1+\sigma^2}{2+\sigma^2}\mu_{gj}. \quad (1)$$

The firm's belief about the group-firm component of productivity θ_{gj} increases when the "surprise" term $(\tilde{y} - \bar{y}_{gj})$ exceeds its prior mean μ_{gj}, and decreases when it is smaller. This learning process implies that a one-time subsidy can generate lasting effects. By nudging firms to consider—and ultimately hire—workers from a group they would otherwise avoid, the subsidy generates new information about group-firm match quality, potentially breaking the "learning trap" and leading firms to update their beliefs about this match quality. The extent of this updating depends on the realized signal: firms with better-than-expected initial experiences revise their beliefs more positively.

Second period. At the start of the second period, firms differ in what they have learned about their match quality with group g, denoted θ_{gj}. Some firms may have experienced no learning if they chose not to interview in the first period. For these firms, second-period hiring mirrors that of the first period, except for any policy changes (e.g., adjustments in the hiring subsidy) that alter the expected value of interviewing V_g.

In contrast, firms that interviewed a candidate in the first period observed a signal $\tilde{y}$ and updated their beliefs about θ_{gj} based on the "surprise" term $(\tilde{y} - \bar{y}_{gj})$. This learning shifts their posterior distribution of θ_{gj} to $\theta_{gj} \sim \mathcal{N}(\hat{\mu}_{gj}, \hat{\sigma}^2)$, where $\hat{\mu}_{gj}$ is given by Equation 1 and $\hat{\sigma}^2 < 1$. These updated beliefs affect second-period interview and hiring decisions. In particular, firms that received more favorable signals (i.e., higher realizations of $\tilde{y}$) in the first period are more likely to interview and hire workers from group g in the second period relative to firms that observed less favorable signals.

We assume that wages are set at the time of hiring, after the firm observes the signal about the worker's productivity. Under this assumption, the hiring wage, and particularly its deviations from predicted wages (wage residuals), reflects the information revealed by this signal and thus proxies for the firm's perceived match quality at the time of hire. This provides the conceptual foundation for our empirical interpretation of wage residuals, as discussed in Section 6.

Implications. Our framework highlights two mechanisms through which a hiring subsidy can affect firm hiring behavior.

1. The hiring subsidy can have persistent effects on firms' decisions to interview—and subsequently hire—from the targeted group if it helps resolve a "learning trap". By nudging firms to experiment with workers from the targeted group, the subsidy facilitates learning about the group-firm match quality.[19]

2. The long-term effect of the subsidy on firm hiring depends on the quality of initial matches: firms that experience higher-quality matches update their beliefs more positively and are more likely to continue hiring from the targeted group than firms whose initial matches are less favorable.

These mechanisms generate key empirical predictions that we test in the data. First, firms exposed to the subsidy should subsequently increase hiring from the targeted group. Second, we expect heterogeneous responses across firms depending on the quality of their initial matches with workers from the targeted group. In practice, very poor matches may not lead to hiring and are therefore not observed. Accordingly, our empirical comparison is between firms that experienced relatively stronger versus weaker matches among hired workers.

5 Firm Hiring Dynamics

We now examine whether firms' exposure to workers hired under the Bonus Donne—who come from long career breaks—leads to persistent changes in their hiring behavior. Do firms continue to hire women with employment interruptions, including recent mothers, after their initial subsidized hires, or is such exposure short-lived, with firms reverting to their previous hiring patterns?

[19]In our model, learning can generate persistent effects even if the subsidy is only temporary, consistent with empirical findings by Miller (2017) on temporary affirmative action policies.

5.1 Empirical Approach

Answering these questions requires constructing a counterfactual scenario: how would hiring have evolved had the firm not used the subsidy? As described in Section 3.2, Bonus Donne firms differ from the average Italian firm. These differences may correlate with subsequent hiring patterns, confounding a simple comparison between treated and other firms. To construct an appropriate control group, we combine a matching strategy with a staggered event-study design, comparing the evolution of outcomes between Bonus Donne ("treated") firms—where treatment is assigned from the year of the first subsidized hire onward—and observationally similar "control" firms that never use the subsidy.[20]

Firm matching. We narrow our focus to firms that are observed every year between 2008 and 2019.[21] We define subsidy adoption as the year of the first subsidized hire, and define treated firms as those that hire at least one female worker through the Bonus Donne between 2013 and 2019. We match each treated firm to a twin control firm using propensity score matching based on an extensive set of predetermined variables. Specifically, we fit a logit model that relates the probability of subsidy take-up to the following firm-level covariates in the three years before a treated firm's first subsidized hire: average (log) weekly wage, (log) total employment, female share of employment, and quartiles of the number of workers hired. Additionally, we require that control firms hire at least one female worker in the same calendar year in which their matched treated firm hires a worker through the Bonus Donne for the first time. These female hires at control firms serve to characterize the average female hire in the absence of subsidy use.[22]

[20] Our empirical strategy directly exploits take-up information and compares firms that use the subsidy to observationally similar firms that do not, rather than relying on the eligibility margins introduced by the policy (Footnote 8). These margins would exploit variation across, for example, broad (1-digit) sectors, or between areas subject to EU structural interventions and those that are not, before and after the introduction of the Bonus Donne in 2013. We do not pursue this, because these margins induce little variation in take-up, compare structurally different industries and regions, and coincide with other interventions such as the 2014-2020 EU funding cycle.

[21] By conditioning on firms operating over the entire period, we ensure that results do not reflect differential firm survival rates. In additional checks, we find no significant differences in survival rates between treated and matched control firms.

[22] We restrict control firms to those that hire at least one female worker in the initial period (the year their matched treated firm first uses the subsidy), so that treated and control firms are comparable at baseline in their female hiring. Control firms are unrestricted in subsequent years, so the comparison captures how female hiring evolves with and without subsidy use. Our results are robust—if anything, larger in magnitude—when we relax this restriction and consider control firms that hired workers of any gender in the initial period.

The resulting sample consists of 38,270 firms, half of which are treated (19,135—corresponding to a matching rate of approximately 72%). Table 2 shows the balancing properties of the matched sample. Treated and control firms are well matched on average wages, workforce size, and the share of female employees. Additional indicators of workforce composition that were not explicitly targeted in the matching procedure, such as the share of full-time or open-ended contracts, are also quite similar between the two groups, suggesting that the matching procedure yields well-balanced comparison firms.

Firm event-study design. We perform the following staggered event-study analysis on the matched sample of firms:

$$y_{jt} = \xi_j + \rho_t + \sum_{k=-4,\neq-1}^{5} \gamma_k \cdot \mathbb{1}[t = t_j^* + k] + \sum_{k=-4,\neq-1}^{5} \beta_k \cdot \mathbb{1}[t = t_j^* + k] \cdot T_j + e_{jt}, \qquad (2)$$

where y_{jt} denotes the outcome variable for firm j in calendar year t, T_j is a binary indicator taking value one for treated firms and zero for control firms, and $t_j^* \in \{2013, \ldots, 2019\}$ denotes the year in which treated firm j first hires a worker through the Bonus Donne (for a control firm, this corresponds to the year of the first subsidized hire at their matched treated firm). The specification includes firm fixed effects (ξ_j), which account for time-invariant firm-specific unobserved characteristics, and calendar year fixed effects (ρ_t), which account for aggregate shocks. The γ_k coefficients capture period-from-event effects common to both treated and control firms, while the β_k's, our coefficients of interest, capture the difference in outcomes between treated and control firms k years before or after the first subsidized hire, for k from -4 to 5. These effects are measured relative to the same difference in the reference period $k = -1$, which is normalized to zero. Standard errors are clustered at the firm level.

Because each treated firm is compared to a matched control firm observed over the same years and assigned the same reference point (the treated firm's adoption year), we mitigate potential challenges inherent in event-study models with staggered treatment timing.[23] When reporting

[23]Without controlling for period-from-event effects, Equation 2 would implicitly assume homogeneous treatment effects across early and late adopters, potentially distorting the estimates through negative weights. By incorporating a matched control group, we are able to estimate period-from-event effects and avoid these distortions—see also

our estimates, we refer to the coefficient β_0 as the immediate (event year) change in hiring in the year of subsidy adoption. This contemporaneous change partly reflects the fact that treatment is defined by the first subsidized hire. The coefficients for subsequent years $\beta_{k\geq 1}$ capture longer-run responses in firms' hiring patterns following subsidy adoption.

Threats to identification. The main identifying assumption is that hiring at treated and control firms—matched on key indicators of workforce composition and prior hiring—would have followed parallel trends in the absence of subsidy adoption. To assess the plausibility of this assumption, we examine the evolution of hiring outcomes in the years preceding adoption, as captured by the pre-event coefficients $\beta_{k<0}$. To corroborate our results further, we perform additional exercises that augment Equation 2 with province-by-year or (2-digit) industry-by-year effects, which account for industry-specific and local labor market shocks that could influence both subsidy take-up and hiring. In the most stringent specification, we introduce pair-by-year fixed effects that allow for pair-specific time shocks (where each pair consists of a treated firm and its matched control). As we will show, the estimates are robust to these additional controls.

A remaining concern is that unobserved factors may jointly influence subsidy adoption and firms' hiring dynamics in ways that are not fully captured by our design. To further mitigate concerns about selection into treatment, we conduct a final robustness check that excludes firms that never adopt the Bonus Donne and focuses solely on *treated* firms within our matched firm sample, estimating staggered event-study models that exploit variation in treatment timing among these firms. To address treatment effect heterogeneity and the resulting bias, we follow Sun and Abraham (2021) and use the last treatment cohort (2019) as the control group. We regard the previous matched design as our preferred specification, since it estimates period-from-event effects against a contemporaneous control group; the treated-only design serves as a complementary check on selection, which confirms the sign and persistence of our main results.

We acknowledge that our empirical approach may still be subject to threats, for instance, time-varying unobserved firm shocks that drive subsidy adoption. While these concerns cannot be fully ruled out, our matched event-study design, combined with the robustness checks above,

Baker, Larcker, and Wang (2022) and Cengiz et al. (2019).

suggests that the documented changes in firm hiring behavior are more likely reflecting subsidy adoption than pre-existing trends or selection into treatment.

Main outcome variables. Our analysis focuses on several outcomes derived from the data described in Section 3. To study the direct effects of subsidy take-up on firm hiring and labor costs, we examine two outcomes: the share of female new hires hired through the Bonus Donne, defined as subsidized female hires divided by total female hires in the year, and the average (log) gross weekly wage of female new hires, measured inclusive of employer social security contributions and thus reflecting the firm's labor cost per hire. To further characterize female new hires, we consider three dimensions: the average number of years of prior non-employment among the firm's female new hires; the (log) number of female new hires from long-term non-employment (i.e., a non-employment period of at least three years); and the (log) number of mothers among female hires (see Section 3 for a definition of mothers). Finally, we assess broader firm-level outcomes, including the (log) number of male new hires; (log) total firm employment; (log) firm value added; and (log) firm value added per (full-time equivalent) worker.[24]

5.2 Results

We begin by presenting the evolution of subsidized female hires in treated firms in Figure 1 Panel (a).[25] In the first year of subsidy use (the event year), 70% of new female hires in treated firms were hired through the subsidy. While subsidy use tends to be concentrated in the initial year, treated firms continue to use the subsidy to a lesser extent in subsequent years, with an average of 6% of new female hires subsidized.

This surge in subsidized hiring at treated firms is associated with a reduction in the average labor costs of new female hires. Figure 1 Panel (b) shows event-study coefficients and corresponding confidence intervals for the average labor cost of new female employees. Prior to the first use of the subsidy, female hires' labor costs at treated and control firms were on parallel trends. In the

[24]For female hires from long-term non-employment, mothers among female hires, and male hires, the log-outcomes are computed as $\log(y + 1)$ to capture both extensive and intensive margin adjustments.

[25]As control firms never use the subsidy, the graph simply plots the share of female workers hired through the subsidy as a percentage of new female hires for treated firms.

event year, labor costs for female new hires at treated firms decline by 2.6% compared to control firms.[26] This gap in labor costs shrinks over time, as estimates become noisier and much smaller.

We next shed light on the characteristics of these new female hires compared to the average female hire, both in the event year and in the long run. Figure 2 Panel (a) investigates the length of the prior non-employment spell of female new hires at treated and control firms, plotting event-study coefficients for the average duration (in years) of non-employment spells. While non-employment spell length of female new hires evolved similarly at treated and control firms in the pre-treatment years, we find a notable difference at the time of treatment. The average non-employment spell for female new hires in the event year was 1.5 years longer in treated firms than in control firms, representing a nearly 50% increase. This event-year difference partly reflects the subsidy's eligibility criterion, which targets women with long interruptions. Crucially, this effect persists beyond the event year, with treated firms continuing to hire women with longer labor market interruptions in subsequent periods.

Panels (b) and (c) further explore hiring patterns for two groups that may be facing the most significant barriers to re-enter the labor market—long-term non-employed women and mothers. For both outcomes, the pre-event coefficients confirm parallel trends between treated and control firms prior to the first subsidized hire. Treated firms increase their hiring of women with long non-employment spells (of at least three years) in the event year by about 17%. These effects persist, albeit to a lower extent, in the years following the first subsidized hire, with a coefficient of around 3%. The increase in hiring of recent mothers is of particular interest given our motivating context. Much of Italy's gender employment gap opens at childbirth (Kleven, Landais, & Leite-Mariante, 2025), and lengthy interruptions following childbearing can erode skills and hinder re-entry (Adda, Dustmann, & Stevens, 2017), so mothers are among the groups for whom demand-side barriers may bind most. Treated firms increase the number of recent mothers among new hires in the event year by 4%, and this effect persists at around 2% in subsequent periods (Panel (c)). The persistence suggests that demand-side incentives can durably draw

[26] Here, we do not control for differential selection among new hires. In Section 7, we examine worker-level outcomes while explicitly accounting for selection (on observables) and show that there is no evidence of wage pass-through: the temporary cut in employer social security contributions was fully retained by firms and not passed on to workers as higher wages.

mothers back into employment.

Taken together, the evidence from Figures 1 and 2 suggests that the Bonus Donne has led firms to broaden recruitment to the targeted group—women with extended employment interruptions. Viewed through the lens of our model, the subsidy increased the expected return to hiring from the group, making it more attractive for firms to hire workers they might otherwise have avoided, and who may have been perceived as a poorer match. These findings illustrate how policies designed to improve access to employment can have a lasting influence on firms' hiring patterns.

We complement the above evidence by examining additional firm-level outcomes, as reported in Figure 3. Panel (a) shows that the hiring of men evolved similarly at treated and control firms prior to the first subsidized hire. In the two to three subsequent periods, treated firms hired about 2% *more* men than control firms; the effect is no longer statistically distinguishable from zero from the fourth year onwards. Bonus Donne firms also grow more rapidly following subsidy adoption, reaching a long-run increase in firm size of about 6% (Panel (b)). This growth is consistent with treated firms expanding hiring along several margins, including the temporary rise in male hiring and, more durably, the persistent increase in hiring from the targeted group documented above. This steeper post-event growth path may also partly reflect lower labor costs for new hires, which allow firms to expand.[27]

Panels (c) and (d), based on balance-sheet data, report changes in value added and labor productivity, the latter measured as value added per full-time equivalent worker. Treated firms experience a long-run increase in value added of around 5%, but no change in productivity: the point estimates are close to zero, slightly negative in some years, and imprecisely estimated. We therefore find no evidence that hiring through the subsidy reduced labor productivity, even though treated firms hired workers coming from extended employment interruptions.

[27] In our main matched specification, treated and control firms follow similar size trajectories before adoption (Panel (b)), which makes selection on pre-existing growth paths a less likely explanation for the divergence.

5.3 Robustness Checks

We now present robustness checks, focusing on our main hiring outcomes (average non-employment spell, new hires from long-term non-employment, newly hired mothers, and firm size). Figure A.3 plots event-study coefficients and corresponding confidence intervals estimated according to Equation 2 after adding (*i*) province-by-year fixed effects, (*ii*) 2-digit industry-by-year fixed effects, and (*iii*) firm match-pair-by-year fixed effects. The first two specifications exploit variation in subsidy use within provinces or industries over time, while the third specification controls for time shocks specific to each matched firm pair, effectively exploiting within-pair variation over time. Event-study coefficients are remarkably similar across specifications and to the baseline estimates, suggesting that our results are not driven by differential trends at the province, industry, or matched-pair level.

Figure A.4 shows event-study coefficients estimated on a sample restricted to treated firms from the matched sample, using the last treatment cohort (2019) as the control group (Sun & Abraham, 2021). By comparing outcomes for firms that first use the Bonus Donne at different points in time, this analysis addresses the concern that treated firms differ from control firms in ways not captured by our matching approach. Our key findings are broadly confirmed in the treated-only sample, with the main result on hiring from the targeted group holding under flat pre-trends. Point estimates differ in magnitude from the baseline matched-sample estimates, being somewhat larger for most outcomes. For newly hired mothers and firm size, there is some upward movement in the pre-period, though small relative to the post-adoption effects. Taken together with the fixed-effects specifications above, these checks indicate that the observed changes in firms' hiring behavior are not primarily driven by unobserved selection into treatment.

6 Firm Learning

6.1 Empirical Approach

The previous section showed that Bonus Donne firms increased recruitment from the targeted group, with no evidence of a decline in labor productivity, and that these effects persist beyond the first year of subsidy use. Why did so few firms hire from this group before the subsidy, and why did a temporary subsidy lead to persistent changes in hiring behavior among firms that did use it? A possible explanation, developed in Section 4, is that firms believed women with long employment interruptions to be poor matches, leaving firms in a learning trap. By lowering the effective cost of recruiting from this group, the subsidy induces firms to experiment with these workers and learn about their match quality. As our model predicts, the adjustment in firms' beliefs depends on their initial experience with Bonus Donne hires: firms that receive more positive signals about match quality with the targeted group update their beliefs about the group upwards and, consequently, are more likely to hire from this group in the future.

To explore this learning mechanism empirically, we examine whether variation in firms' initial experiences with Bonus Donne hires affects their subsequent hiring behavior. We focus on treated firms that first use the subsidy between 2013 and 2016, which allows us to track their hiring patterns over time following the first subsidized hire. We ask whether firms that hired better matches in the year of their first subsidized hire are subsequently more likely to hire again from the target group, relative to other treated firms whose initial hires were perceived as poorer matches. To do so, we classify these early-treated firms based on the quality of their initial match with Bonus Donne hires, distinguishing between those whose first subsidized hire was a "good" versus a "bad" match—that is, a worker perceived to be more versus less productive at the firm.[28]

To construct a proxy for firm-match quality for each Bonus Donne hire, we estimate a Mincerian wage regression using individual-level data, relating (log) weekly wages to key observable worker characteristics (age, contract type, occupation, hiring cohort) and firm characteristics (location, 2-digit industry, total employment). The estimated residual—the portion of the hiring wage unexplained by these characteristics—serves as a proxy for the quality of the individual worker-firm match at the time of hiring, which firms use to update their beliefs about the group, in line with our conceptual framework in Section 4.[29] We estimate the Mincerian regression on

[28]As noted in Section 4, very poor matches are unlikely to result in hiring, so the observed contrast between stronger and weaker initial matches may understate the true dispersion in signals. Our heterogeneity estimate, therefore, likely understates the strength of experiential updating.

[29]Studies in the U.S. retail sector, where turnover is high, often use worker tenure or turnover as productivity measures (Autor & Scarborough, 2008; Benson & Lepage, 2025). Such ex-post measures are less suitable in the Italian context, where labor markets are less flexible and turnover is less frequent. Moreover, using tenure as a proxy

the sample of initial hires—women hired for the first time under the Bonus Donne between 2013 and 2016—restricting the sample to the years in which these hires occur.[30] We discretize the distribution of residuals and classify Bonus Donne hires below the median as bad matches and those above the median as good matches (results are robust to an alternative classification using the first and third terciles). Treated firms are then classified based on the quality of their initial (time 0) Bonus Donne hire(s): firms hiring an above-median worker are labeled "good-match" firms, while those hiring a below-median worker are labeled "bad-match" firms.[31]

The empirical analysis employs a difference-in-differences design comparing hiring patterns at bad-match and good-match firms in the years following the first subsidized hire. The year of first subsidy use—time 0, which is used to classify firms based on match quality—is thus excluded from the estimation sample. We estimate the following specification over the 2008-2019 period:

$$y_{jt} = \alpha + \gamma_1 \cdot Post + \gamma_2 \cdot Post \cdot Good_j + \xi_j + \epsilon_{jt} \tag{3}$$

where $Good_j$ is an indicator variable equal to one for good-match treated firms and zero for bad-match treated firms. The variable $Post$ takes value one for years 1 to 5 after the first subsidized hire, and zero for the years -4 to -1. Firm fixed effects (ξ_j) control for time-invariant differences across firms. The coefficient of interest, γ_2, captures the differential change in outcomes between good- and bad-match firms, averaged over the post period, relative to the pre-treatment baseline. By construction, this comparison excludes the event year (time 0), which coincides with the first subsidized hire. It instead isolates how firms' initial experiences with subsidized hires shape their subsequent hiring. Standard errors are clustered at the firm level.

In this specification, identification comes from differences between treated firms with good versus bad initial matches. It relies on the assumption that, absent the difference in initial match quality, the two groups would have followed parallel trends. To further corroborate this

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for worker productivity would mechanically lower subsequent hiring for firms with higher retention, introducing negative bias. Our approach instead leverages detailed administrative records on wages and worker characteristics to construct a cleaner proxy for firms' perceived match quality *at the time of hiring*, as reflected in entry wages. This measure aligns closely with the wage-setting assumption in our conceptual framework (Section 4).

[30] We run a Mincerian wage regression pooling all subsidized female hires at Bonus Donne firms between 2013 and 2016. In addition to the controls listed in the main text, the regression also includes year fixed effects.

[31] If a firm hires multiple Bonus Donne workers at time 0, it is classified based on the modal worker.

approach and relax the parallel-trends requirement, we extend the analysis by incorporating the matched control group described in Section 5. We estimate a triple-differences specification that compares: (*i*) good-match treated firms to their matched controls, (*ii*) bad-match treated firms to their matched controls, and (*iii*) the difference between these two effects.[32]

6.2 Results

Table 3 presents estimates of the γ_2 coefficient in Equation 3 for various firm outcomes. Hiring patterns following a firm's first subsidized hire depend on its initial experience with the program: good-match firms hire 2% more Bonus Donne workers in later periods than bad-match firms (Column 1). Relative to bad-match firms, good-match firms also increase their hiring of workers from the targeted group more broadly: their hiring of long-term non-employed women rises by 2.3%, and their hiring of mothers by 1.7%, in the post-period (Columns 2-3). Notably, the difference between good- and bad-match firms is comparable in magnitude to the overall post-event increase in hiring from these groups in our main treated-versus-control estimates (Figure 2 Panels (b)-(c)). This pattern is consistent with firms updating their beliefs about match quality for the targeted group, in line with the learning mechanism outlined in our conceptual framework. Finally, Column 4 shows no significant effect on total firm size. The increased hiring from the targeted group at good-match firms therefore does not simply reflect faster overall growth, but a shift toward hiring more from this group specifically.

A natural alternative to learning is that the subsidy simply gave firms access to cheaper labor, which they continued to exploit. A purely cost-based explanation, however, would not predict that the quality of a firm's initial match shapes its subsequent hiring: both good-match and bad-match firms hired subsidized workers and so faced the same cost incentive to continue hiring from the group. Hiring instead diverges with the quality of the initial match, with good-match firms hiring more, which points to firms updating their beliefs about the group rather than simply

[32] The estimated model is as follows, where the coefficient of interest γ_4 captures the triple-differences effect and indicator variables $Post$, $Good_j$, and T_j are defined in the main text:

$$y_{jt} = \alpha + \gamma_1 \cdot Post + \gamma_2 \cdot Post \cdot Good_j + \gamma_3 \cdot Post \cdot T_j + \gamma_4 \cdot Post \cdot T_j \cdot Good_j + \xi_j + \epsilon_{jt} \tag{4}$$

exploiting cheaper labor.

We provide two further robustness checks. First, our findings are robust to the cutoff used to classify firms: Appendix Table A.4 reports very similar estimates when good- and bad-match firms are defined by the third versus first tercile of the distribution of Mincer residuals, rather than above versus below the median. Second, Appendix Table A.5 confirms the baseline results under the triple-differences design, where good- and bad-match treated firms are also compared with their matched controls. Notably, we find no statistically significant differences in any of the outcomes between the controls matched to good- and bad-match treated firms (not reported). This is reassuring, as it suggests that the different hiring trajectories between good- and bad-match firms are unlikely to be driven by differential shocks, but instead reflect differences in their initial exposure to the targeted group.

We then present evidence that helps distinguish learning about the group from one leading alternative, the selection of generally more able workers. Appendix Table A.6 examines the wage residuals of good- and bad-match firms' subsequent female hires. Consistent with learning, good-match firms pay their later female hires higher entry wages than bad-match firms, relative to what observable characteristics predict (Column 1).[33] In our framework, this follows from firms raising their wage offers to subsequent female hires as they revise their beliefs about the group's productivity upward. These workers were not, however, better paid in their prior jobs, measured by the wage residual at their previous employer (Column 2), suggesting the difference reflects firms' revised assessment of the group rather than the selection of more able workers.[34]

Taken together, these results indicate that early positive experiences with workers from the targeted group increase firms' willingness to hire from this group in subsequent periods, consistent with the learning mechanism in our conceptual framework and, more broadly, with models of experiential learning in hiring (Benson & Lepage, 2025; Lepage, 2024; Leung, 2018).

[33] Since the specification includes firm fixed effects, this is a within-firm increase rather than a reflection of good-match firms being higher-paying overall.

[34] A broad improvement in screening ability (Miller, 2017) would instead predict higher prior-job residuals among their later hires, which we do not find.

7 Worker Outcomes

7.1 Empirical Approach

So far, we have focused on how firms responded to the Bonus Donne. This section turns to the workers themselves and examines their long-term labor market outcomes, asking whether subsidy exposure improves both retention at the hiring firm and broader labor market attachment relative to a comparable unsubsidized hire.

Worker matching. To examine worker-level effects, we build on the firm-level design by matching each Bonus Donne hire to a comparable control worker. We focus on female workers hired out of non-employment at treated and control firms, using the matched firm sample described in Section 5 and restricting attention to the first year in which treated firms hire a Bonus Donne worker (between 2013 and 2019). First, we identify subsidized hires at treated firms in the initial year of subsidy take-up. Each of these workers is matched to a newly hired female worker in the same year at matched control firms, with similar observable characteristics. The worker matching is based on a propensity-score logit model estimating the probability that a worker is hired via the subsidy, controlling for worker age, contract type (fixed-term vs. open-ended, part-time vs. full-time), broad occupational categories (manager, white-collar, blue-collar), and the length of the preceding non-employment spell. This approach compares the careers of observationally similar female workers—of similar age, hired after comparable non-employment spells, into the same occupation and contract type, and by firms on similar trajectories—who differ only in whether they were hired with or without the subsidy. Crucially, our administrative data allow us to follow workers not only at the hiring firm but also at subsequent employers.

Our identifying assumption is that, conditional on the observable characteristics used in matching, whether a worker was hired with or without the subsidy is unrelated to unobserved determinants of their later labor market outcomes. Because take-up is a firm-level decision, there is limited scope for selecting individual workers on unobservables. Our worker-level analysis relates to Rubolino (2025), who also studies worker-level effects of the Bonus Donne. We complement this study with a matched design explicitly distinguishing retention at the hiring firm from broader labor market attachment.[35]

Our matched sample consists of 13,336 Bonus Donne workers and an equal number of matched controls. Table A.7 shows that the two groups are well-balanced on observable characteristics.

Worker event-study design. As in the firm analysis, we estimate a staggered event-study design on the matched sample of new hires:

$$y_{it} = \phi_i + \rho_t + \sum_{k=-4,\neq-1}^{5} \gamma_k \cdot \mathbb{1}[t = t_i^* + k] + \sum_{k=-4,\neq-1}^{5} \beta_k \cdot \mathbb{1}[t = t_i^* + k] \cdot T_i + e_{it}, \tag{5}$$

where y_{it} denotes the outcome variable for worker i in year t, T_i is a treatment indicator equal to one for Bonus Donne workers and zero for the matched control group, and $t_i^* \in \{2013, \ldots, 2019\}$ is the year treated worker i was hired under the Bonus Donne (it is also the hiring year of their matched control). The specification controls for time-invariant, worker-specific unobserved characteristics through worker fixed effects ϕ_i, for aggregate shocks through year fixed effects ρ_t, and for period-from-event effects through the γ_k coefficients. The coefficients of interest are the β_k's, denoting the difference in outcomes between treated and control workers relative to the year before being hired (β_{-1} normalized to zero). They identify the effect of being hired under the subsidy on workers' subsequent labor market outcomes, under a parallel-trends assumption: absent the subsidy, Bonus Donne hires would have followed trajectories similar to those of their matched controls. Standard errors are clustered at the worker level.

Our outcomes of interest are wages and employment trajectories, both at the original hiring firm and in the broader labor market. We address two central questions. First, by accounting for worker selection, we directly assess whether the hiring subsidy is passed through to workers in the form of higher wages or retained by firms. Second, by tracking workers for up to five years after hiring, we examine whether the Bonus Donne improved long-term labor market attachment even after the subsidy expires—a key goal of hiring subsidies. Because the subsidy is extended

[35] Our design differs: Rubolino (2025) exploits variation in the timing of subsidy take-up, using a within-worker comparison of treated to not-yet-treated individuals, whereas we compare each subsidized hire to an observationally similar worker hired without the subsidy in the same year. Our matched design provides a contemporaneous counterfactual of comparable unsubsidized hires and does not rely on the timing of take-up.

in duration when a worker is hired under, or converted to, a permanent contract, it creates an incentive for firms to retain subsidized hires and may also enhance workers' attractiveness to other employers through accumulated experience.

7.2 Results

Our primary wage outcome is weekly wages excluding employer social security contributions, the relevant measure for assessing whether the reduction in payroll taxes was passed through to workers in the form of higher pay. Figure 4, Panel (a), shows event-study coefficients for net wages, conditional on employment (wages are coded as missing when the worker is not employed). Prior to hiring, treated and control workers exhibit parallel pre-trends. At time 0, the year of hiring, we find no significant divergence in net wages, and this holds in subsequent periods. This finding indicates no pass-through of the hiring subsidy to workers in the form of higher wages, consistent with Rubolino (2025) and Jain, Mommaerts, and Weaver (2026).

We next examine employment trajectories. One year post-hire, subsidized workers are approximately 10 percentage points more likely to be retained by their initial employer (Figure 4, Panel (b), dashed lines)—consistent with the longer job durations documented by Rubolino (2025). While the retention gap narrows over time, it does not disappear by year two, when the subsidy expires: even five years later, subsidized hires remain nearly 5 percentage points more likely to be employed at their original firm compared to control workers, a substantial effect relative to the overall retention rate of 20%. The findings suggest that firms do not displace workers once the subsidy expires, but tend to retain them over the medium term.

Figure 4, Panel (b) (solid lines), shows that treated workers are 8 percentage points more likely to be employed *at any firm* than control workers one year after hire, with the effect remaining stable in subsequent years. Decomposing this effect, about half reflects higher retention at the original firm, while the remainder comes from treated workers transitioning to new employers. This broader labor market attachment, operating partly through mobility to other firms rather than retention alone, suggests the subsidy raises workers' employability beyond the direct retention incentive, plausibly through accumulated experience or improved signals of productivity. These broader employment gains contrast with earlier evidence from temporary work programs, where employment gains were largely confined to the host firm (Barham, Cadena, & Turner, 2023).

8 Conclusion

Increasing women's labor market participation is a key policy priority in many countries, especially for women who face difficulties re-entering employment after career breaks. While most policy interventions have focused on supply-side measures such as parental leave or childcare provision, less attention has been paid to the role of employer demand in shaping women's employment opportunities. This paper studies whether a hiring subsidy targeted at women with extended employment interruptions in Italy can improve their labor market outcomes by encouraging firms to hire from this group.

Leveraging administrative employer-employee data that record firms' take-up of the subsidy and allow us to track both firms and workers over time, we document several key findings. First, firms using the subsidy broaden their hiring by recruiting more women with extended employment interruptions, including mothers. While the increase is largest in the year of first subsidy use, the key result is that the hiring response persists beyond the take-up year: treated firms continue to recruit more women from the targeted group, and more mothers, relative to matched control firms. These changes in hiring are not accompanied by a decline in firm-level labor productivity.

Second, we present evidence suggesting that firms learn from their initial matches with subsidized workers. Firms with better initial matches hire more women with extended employment interruptions, including mothers, in subsequent years. This pattern is consistent with the employer learning mechanism formalized in our conceptual framework: by lowering the cost of experimentation, hiring subsidies generate information about match quality and can shift employers' beliefs about how productive workers with extended employment interruptions are at the firm, thereby shaping subsequent hiring decisions.

Third, women hired under the subsidy experience persistent employment gains. Even five years after hiring, subsidized workers remain more likely to be employed than comparable women hired at control firms. These gains reflect both higher retention at the original hiring firm and stronger attachment to the broader labor market. Given evidence that career breaks contribute to persistent employment and earnings penalties (Adda, Dustmann, & Stevens, 2017; Kleven, Landais, & Søgaard, 2019), these findings suggest that hiring subsidies can help women with extended employment interruptions re-enter employment and strengthen their subsequent labor market attachment. Finally, we find no evidence that the subsidy was passed through to workers in the form of higher wages, consistent with firms capturing much of the reduction in labor costs.

These findings highlight the role of demand-side interventions in reducing gender inequalities in the labor market. Our results suggest that hiring subsidies can help address informational frictions that arise when firms are uncertain about the productivity of workers with extended employment interruptions, and may be particularly effective when targeted at groups facing such hiring barriers. The lack of evidence for a decline in firm-level labor productivity alleviates concerns that broadening hiring toward underrepresented groups necessarily comes at the expense of firm performance.

Taken together, these findings suggest that the effects of a time-limited reduction in hiring costs can extend beyond the subsidized spell. Our main contribution is to point to employer learning as a mechanism behind this persistence. Consistent with this channel, firms that drew better initial matches subsequently hire more from the group, a pattern not readily explained by cheaper labor or differential firm trajectories. As our evidence suggests, the subsidy's lasting effect operates through the information it generates about a group firms had rarely hired. This relates to evidence from transitory affirmative action policies showing that firms' hiring behavior can remain affected even after the policy incentive is removed, for instance through investments in screening capital (Miller, 2017). In our setting, the evidence points to a related but distinct channel: by generating information about match quality, the subsidy shifts firms' beliefs about the group and shapes their future hiring. More broadly, our findings point to a setting where hiring subsidies may be particularly effective: groups that firms hire less frequently because of uncertainty about their productivity, where the scope for learning is greatest. By prompting firms to experiment with such workers, these subsidies may generate lasting changes in hiring.

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Tables and Figures

Tables

Table 1: Firm Summary Statistics: Bonus Donne versus Non-User Firms

	(1) Bonus Donne Firms	(2) Other Private-Sector Firms
Number of employees	48.17	17.96
	(910.89)	(263.47)
Female share (employment)	0.57	0.44
	(0.33)	(0.36)
Share full-time contract (employment)	0.63	0.70
	(0.34)	(0.34)
Share open-ended contract (employment)	0.80	0.83
	(0.27)	(0.28)
Female share (new hires)	0.54	0.42
	(0.40)	(0.41)
Number of new hires 2010-12	12.32	3.47
	(350.99)	(75.42)
Mean ln weekly wage	5.84	5.87
	(0.34)	(0.41)
Mean ln weekly wage (women)	5.82	5.85
	(0.36)	(0.46)
Value added per worker	35.10	40.42
	(32.97)	(58.21)
Number of firms	26,497	660,764

Notes: The table compares average characteristics of firms that used the Bonus Donne to hire at least one woman between 2013 and 2019 ("Bonus Donne" firms, Column 1) with all other private-sector firms that never used the subsidy (Column 2). All variables are measured in 2012, the year before the subsidy was introduced. Number of employees denotes the firm's headcount; Female share, the share of full-time contracts, and share on open-ended contracts are computed over the firm's workforce; Female share (new hires) is computed over the firm's hires in the year; Number of new hires 2010–12 is the firm's average annual number of new hires over that period; Mean ln weekly wage (overall and for women) is the natural logarithm of the weekly wage averaged across the firm's workers; Value added per worker is firm value added divided by the number of employees (headcount). Standard deviations are reported in parentheses. *Source:* Administrative matched employer-employee records from the Italian Social Security Institute (INPS). Value added per worker is computed on the Cerved balance-sheet data.

Table 2: Firm Balance: Treated versus Matched Control Firms

	(1) Treated firms	(2) Control firms
Mean ln weekly wage	5.94	5.94
	(0.28)	(0.37)
Mean ln weekly wage (women)	5.92	5.91
	(0.30)	(0.40)
Number of employees	44.89	43.83
	(512.53)	(594.65)
Female share (employment)	0.55	0.55
	(0.32)	(0.32)
Share full-time contract (employment)	0.60	0.63
	(0.35)	(0.34)
Share open-ended contract (employment)	0.83	0.77
	(0.24)	(0.30)
Number of firms	19,135	19,135

Notes: The table reports mean characteristics of treated (Column 1) and matched control (Column 2) firms in the matched sample of 38,270 firms used for the firm-level analysis. All characteristics are measured in the three years before the treated firm's first subsidized hire. Treated firms used the Bonus Donne at least once between 2013 and 2019; control firms are observationally similar firms that hired at least one woman in the same calendar year but never used the subsidy. Matching is by propensity-score matching on average (log) weekly wage, (log) total employment, female employment share, and quartiles of the number of hires as described in Section 5. The matching is performed without replacement and with a 0.05 caliper for the propensity score. Mean ln weekly wage is the natural logarithm of the weekly wage averaged across the firm's workers. Standard deviations are reported in parentheses. *Source*: Administrative matched employer-employee records from the Italian Social Security Institute (INPS).

Table 3: Good-Match versus Bad-Match Firms

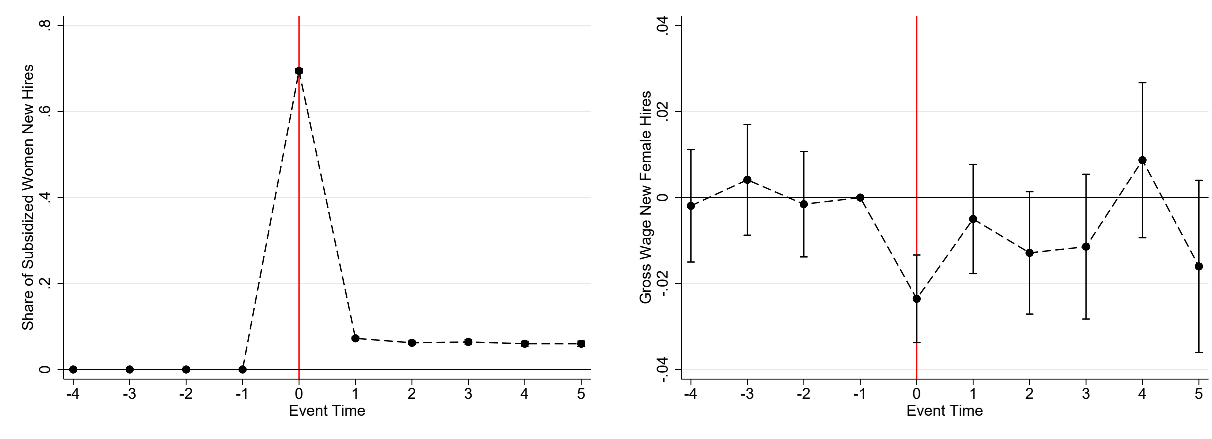
	(1) Ln Subsidized Female Hires	(2) Ln New Hires From Long- Term Non-Employment	(3) Ln Mothers New Hires	(4) Ln Total Firm Size
Estimate	0.0211**	0.0226***	0.0171***	0.0055
	(0.0094)	(0.0075)	(0.0064)	(0.0121)
Observations	70,224	70,224	70,224	70,224
R-squared	0.342	0.721	0.646	0.924

Notes: The table reports the estimated γ_2 coefficient from Equation 3: the differential post-adoption change in hiring between treated firms whose initial Bonus Donne hire was a better versus worse match (good- versus bad-match firms). The sample consists of treated firms that first used the subsidy between 2013 and 2016. A firm is classified as good- (bad-) match if its modal initial Bonus Donne hire has above- (below-) median match productivity, proxied by the residual from a Mincer wage regression estimated on initial subsidized hires (see Section 6). The specification includes firm fixed effects; *Post* equals one in event years 1–5 and zero in years -4 to -1, so the take-up year (event time 0) is excluded. We report estimates for the following outcomes: the natural logarithm of (1+) the number of women hired under the Bonus Donne (Column 1); the natural logarithm of (1+) the number of female new hires from long-term ($\geq$ 3 years) non-employment (Column 2); the natural logarithm of (1+) the number of newly hired mothers, defined as women who took maternity leave in the previous four years (Column 3); and the natural logarithm of total firm employment (Column 4). Standard errors, clustered at the firm level, are reported in parentheses. *, **, and *** denote statistical significance at the 10, 5, and 1 percent level, respectively. *Source:* Administrative matched employer-employee records from the Italian Social Security Institute (INPS).

Figures

Figure 1: Share and Cost of Female Workers Hired Through the Bonus Donne

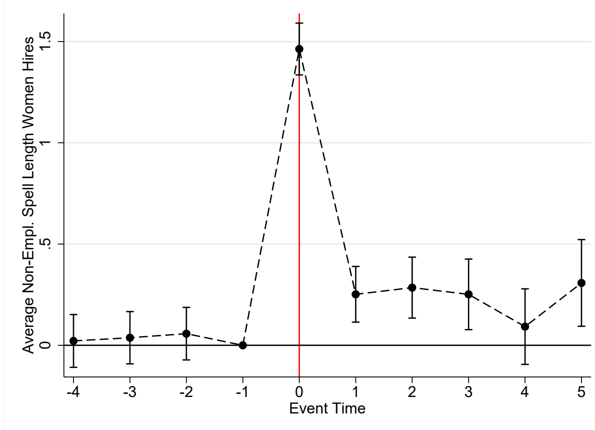
(a) Share Hired Under Bonus Donne (b) Mean Log Weekly Wages, Gross of Payroll Taxes



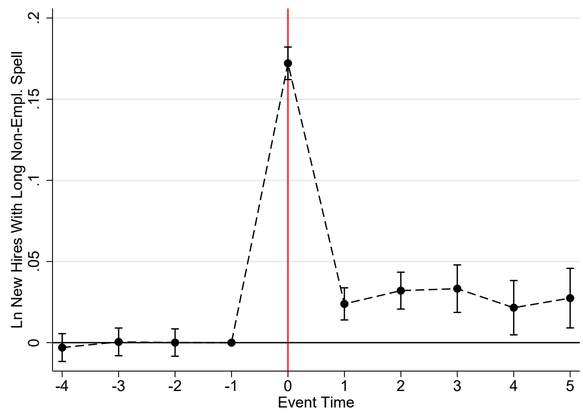
Notes: The figure plots the share of new female hires made through the Bonus Donne (Panel a) and the average labor cost of new female hires (Panel b), for treated firms and their matched controls. Event time is measured in years relative to a firm's first subsidized hire (event time 0, red vertical line). Panel (a) plots, by event time, the average share of new female hires made through the Bonus Donne among treated firms; this share is zero by construction for control firms. Panel (b) plots the estimated event-study coefficients β_k from Equation 2 for the average (log) gross weekly wage of new female hires, measured inclusive of employer social security contributions and thus reflecting the firm's labor cost per hire; the coefficients are differences between treated and control firms relative to event time -1 (normalized to zero). The specification includes firm fixed effects, calendar-year fixed effects, and event-time fixed effects. The estimation sample comprises treated ("Bonus Donne") firms that hired at least one woman through the subsidy between 2013 and 2019 and observationally similar matched control firms that hired at least one woman in the same calendar year but never used the subsidy, matched on baseline firm characteristics measured before the first subsidized hire (38,270 firms in total: 19,135 treated and 19,135 matched controls; see Section 5 and Table 2). In Panel (b), 95% confidence intervals, based on standard errors clustered at the firm level, are shown as vertical bars. *Source:* Administrative matched employer-employee records from the Italian Social Security Institute (INPS), 2008–2019.

Figure 2: Changes in Firm Hiring Patterns Towards the Target Group

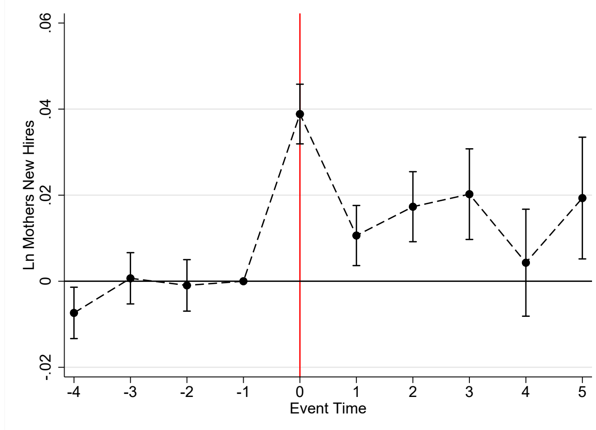
(a) Previous Non-Employment Period



(b) Female Hires with Long Non-Employment Spells

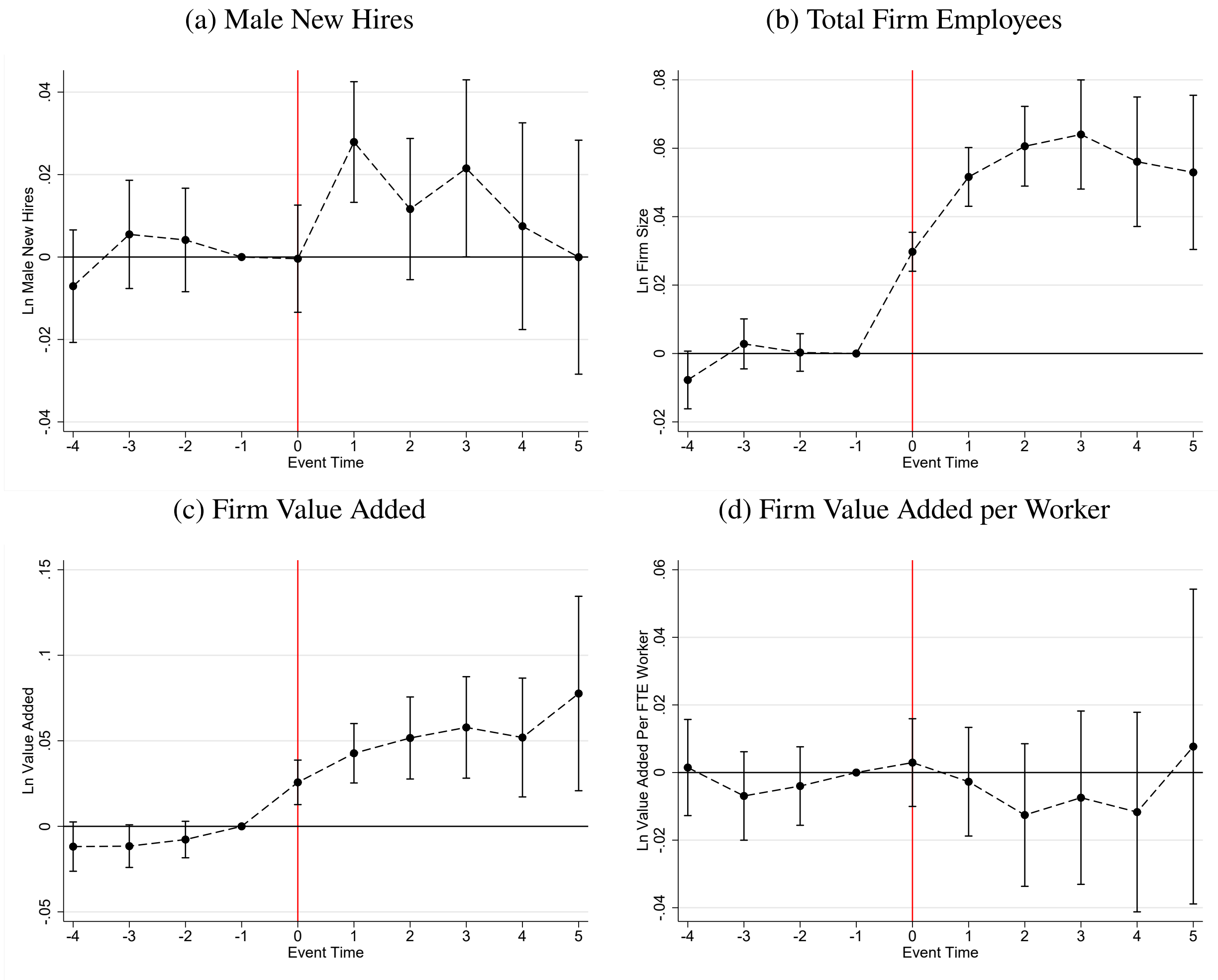


(c) Newly Hired Mothers



Notes: The figure plots the estimated event-study coefficients β_k from Equation 2, tracing the difference in outcomes between treated ("Bonus Donne") and matched control firms before and after subsidy adoption. Each β_k is measured relative to this difference in the year before the first subsidized hire (event time -1, normalized to zero); event time 0 (red vertical line) is the year of the first subsidized hire. Outcomes: Panel (a), the average length of prior non-employment (in years) among new female hires; Panel (b), the natural logarithm of (1+) the number of new female hires from long-term ($\geq$ 3 years) non-employment; Panel (c), the natural logarithm of (1+) the number of newly hired mothers, defined as women who took maternity leave in the previous four years. The specification includes firm, calendar-year, and event-time fixed effects. The estimation sample comprises treated firms that hired at least one woman through the subsidy between 2013 and 2019 and observationally similar matched control firms that hired at least one woman in the same year but never used the subsidy, matched on baseline firm characteristics measured before the first subsidized hire (38,270 firms in total: 19,135 treated and 19,135 matched controls; see Section 5 and Table 2). Vertical bars denote 95% confidence intervals based on standard errors clustered at the firm level. *Source:* Administrative matched employer-employee records from the Italian Social Security Institute (INPS), 2008–2019.

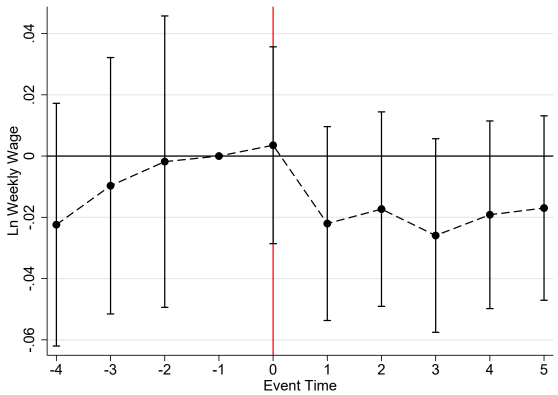
Figure 3: Firm Growth and Productivity



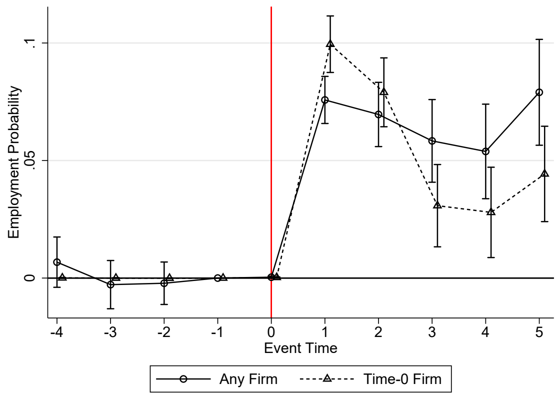
Notes: The figure plots the estimated event-study coefficients β_k from Equation 2, tracing the difference in outcomes between treated ("Bonus Donne") and matched control firms before and after subsidy adoption. Each β_k is measured relative to this difference in the year before the first subsidized hire (event time -1, normalized to zero); event time 0 (red vertical line) is the year of the first subsidized hire. Outcomes: Panel (a), the natural logarithm of (1+) the number of male new hires; Panel (b), the natural logarithm of total firm employment; Panel (c), the natural logarithm of firm value added; Panel (d), the natural logarithm of firm value added per full-time-equivalent worker. The value-added outcomes in Panels (c)–(d) are available through 2018 for limited-liability companies, and are therefore estimated on the corresponding subsample. The specification includes firm, calendar-year, and event-time fixed effects. The estimation sample comprises treated firms that hired at least one woman through the subsidy between 2013 and 2019 and observationally similar matched control firms that hired at least one woman in the same year but never used the subsidy, matched on baseline firm characteristics measured before the first subsidized hire (38,270 firms in total: 19,135 treated and 19,135 matched controls; see Section 5 and Table 2). Vertical bars denote 95% confidence intervals based on standard errors clustered at the firm level. *Source:* Administrative matched employer-employee records from the Italian Social Security Institute (INPS), 2008–2019; value-added data are from Cerved balance sheets.

Figure 4: Worker-Level Outcomes: Net Wages and Employment

(a) Net Wages



(b) Employment Probabilities



Notes: The figure plots the estimated event-study coefficients β_k from Equation 5, tracing the difference in outcomes between women hired through the Bonus Donne and matched control workers in the years before and after the hiring year. Each β_k is measured relative to this difference in the year before hiring (event time -1, normalized to zero); event time 0 (red vertical line) is the hiring year. Panel (a): the outcome is the (log) net weekly wage—the worker's wage excluding employer social security contributions—measured conditional on employment (coded as missing in non-employment years) and not used in the matching. Panel (b): the outcome is an indicator for being employed at any firm (solid lines) and at the hiring (time-0) firm (dashed lines). The specification includes worker, calendar-year, and event-time fixed effects. The sample comprises 13,336 women hired under the Bonus Donne in the first year their firm used the subsidy (2013–2019) and an equal number of matched control women hired the same year at matched control firms. Workers are matched by propensity-score matching on age, contract type (fixed-term vs. open-ended, part-time vs. full-time), broad occupational category (manager, white-collar, blue-collar), and the length of the preceding non-employment spell (see Section 7 and Appendix Table A.7). Vertical bars denote 95% confidence intervals based on standard errors clustered at the worker level. *Source:* Administrative matched employer-employee records from the Italian Social Security Institute (INPS), 2008–2019.

Appendices

A.1 Policy Background and Political Setting

Following the global financial crisis, many European countries were struck by the sovereign debt crisis in the early 2010s. These included Italy, where turmoil in financial markets led to the fall of the incumbent government in November 2011. A technocratic government led by Prime Minister Mario Monti came to power with the aim of implementing structural reforms to bring public debt back under control and promote economic growth. These reforms were implemented within a short period, due to pressures from international investors and supranational institutions. Two key reforms were introduced six months apart: a pension reform, approved on December 22, 2011, and a labor market reform, approved on June 28, 2012 (Law 92/2012, which also introduced the Bonus Donne). The former created new pension rules to ease pressure on public debt.[36] The latter had four major components: (i) it eased employment protection legislation for workers on an open-ended contract by relaxing rules on economic dismissal; (ii) it made temporary and atypical contracts marginally more stringent;[37] (iii) it expanded the coverage of the unemployment insurance scheme; and (iv) it introduced the hiring subsidy which is the focus of our paper—the Bonus Donne. Importantly for our empirical design, this last measure was the only subsidy targeted at firms and at the hiring of women.

There was strikingly little news coverage of the Bonus Donne. In contrast, most media attention focused on the pension reform and on the easing of the employment protection legislation. These policy items were highly controversial in the public debate, as either worsening pension conditions for all cohorts or allegedly undermining a fundamental worker protection (Article 18 of the Workers' Statute) by allowing the firing of workers for economic reasons. Google Trends searches clearly illustrate that these policies attracted far more public attention than the Bonus Donne. Figure A.2 Panel (a) shows the evolution of Google searches for the item "Bonus

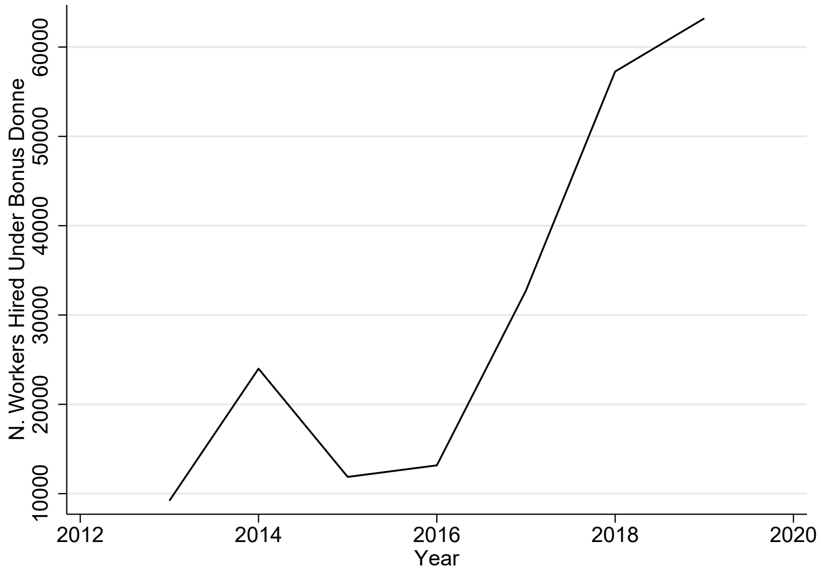
[36] The reform introduced an increase in retirement age and a transition from a defined benefit to a defined contribution scheme for older cohorts.

[37] The law decreased the maximum number of years during which a worker could be employed at a firm under a fixed-term contract. It also increased the minimum time span before a fixed-term worker could be re-hired by the same employer under a fixed-term contract.

Donne" over time. Google searches were very few in 2013 (when the subsidy came into force) and remained low through the first months of 2014. Searches eventually increased and reached a peak in 2015, then remained relatively stable at lower levels from 2016 onwards. Public interest in this hiring subsidy was significantly lower than for the other, more controversial policies discussed above, as illustrated in Panels (b) and (c) of Figure A.2. Panel (b) compares Google searches for the item "Bonus Donne" (pink line) to searches for "Pensioni Fornero", the pension reform (blue line). During the announcement and parliamentary discussion of the pension reform in 2012, Google searches for this item soared, whereas searches for "Bonus Donne" were very few. This gap is even more striking when Google searches for "Bonus Donne" are juxtaposed with those for "Article 18" in Panel (c). Such low coverage—hence low public awareness—of the subsidy may be among the reasons for its low take-up, as discussed in Section 2.

A.2 Appendix Figures

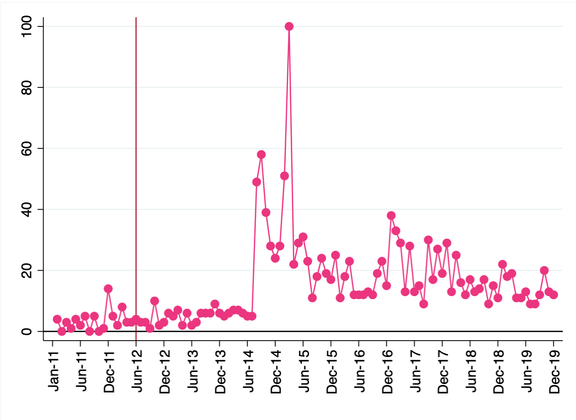
Figure A.1: Subsidy Take-Up Over Time



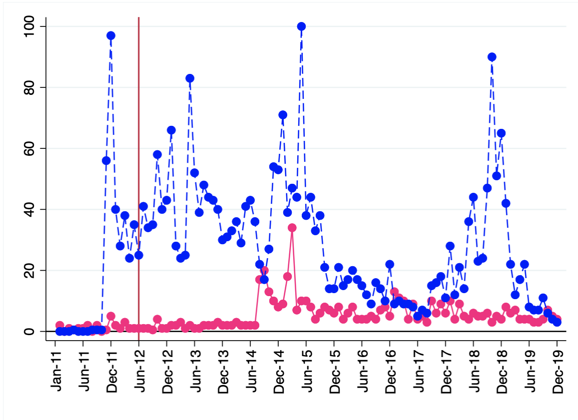
Notes: The figure plots the total number of workers hired each year under the Bonus Donne subsidy, 2013–2019, including men older than 50, for whom the same reduction in employer social security contributions applied. The main analyses in the paper exclude these men and focus on women with extended employment interruptions (see Section 2.2). *Source:* Administrative matched employer-employee records from the Italian Social Security Institute (INPS), covering the universe of private-sector firms and workers.

Figure A.2: Google Searches, Bonus Donne Versus Contemporaneous Reforms

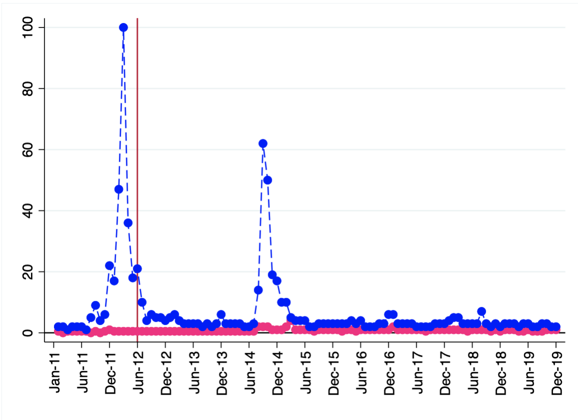
(a) Google Searches for Item "Bonus Donne"



(b) Google Searches for Item "Bonus Donne" (pink) and "Pensioni Fornero" (blue)

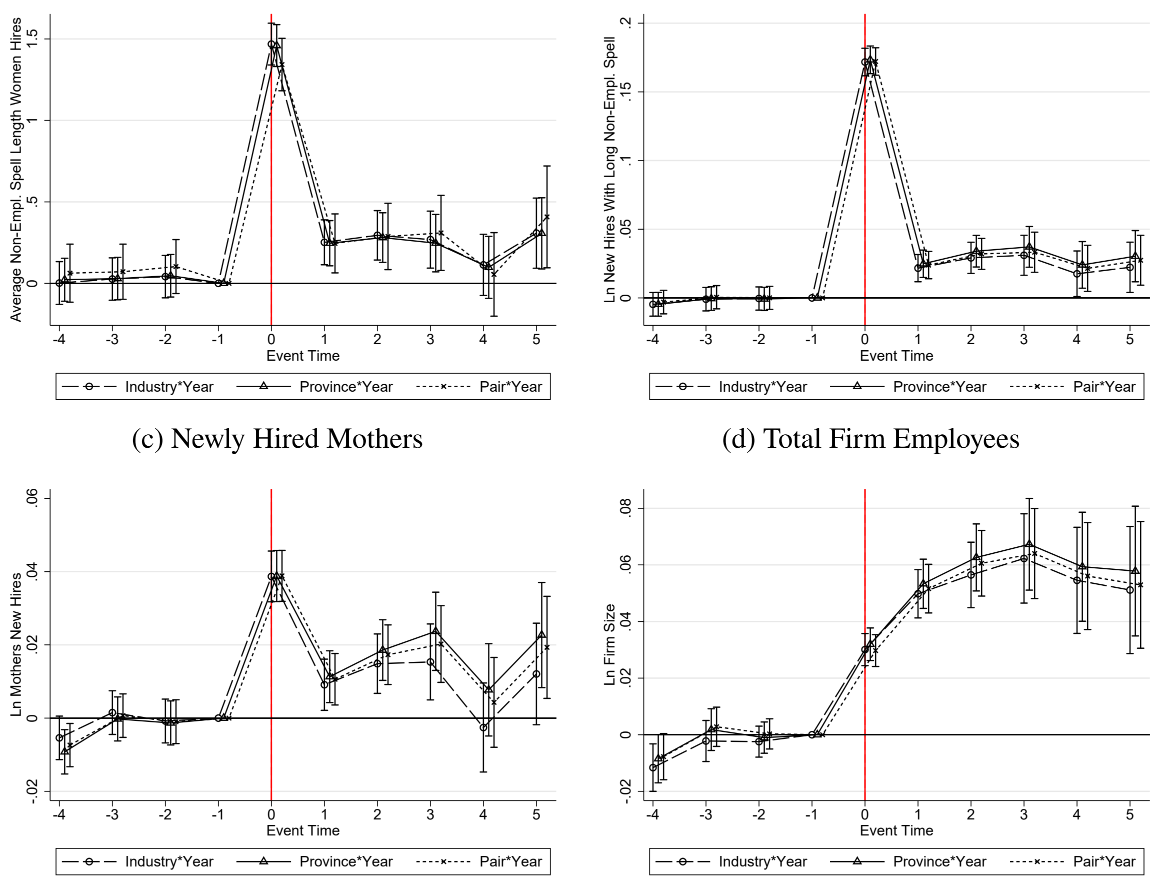


(c) Google Searches for Item "Bonus Donne" (pink) and "Article 18" (blue)



Notes: Monthly Google search intensity between January 2011 and December 2019. The vertical axis reports each term's monthly search volume relative to its own maximum over the period, normalized to 100. The vertical line marks the month the reform package introducing the Bonus Donne (Law 92/2012) was approved. Panel (a) plots searches for the term "Bonus Donne". Panel (b) compares "Bonus Donne" (pink) with "Pensioni Fornero" (blue), the pension reform enacted in the same 2012 reform package. Panel (c) compares "Bonus Donne" (pink) with "Article 18" (blue), referring to the contemporaneous easing of employment-protection rules on dismissals. The series illustrate that the Bonus Donne attracted far less public attention than the other, more controversial measures introduced at the same time (see Appendix A.1). *Source:* Google Trends.

Figure A.3: Robustness: Firm Hiring Patterns, Additional Fixed Effects



(a) Previous Non-Employment Period

(b) Female Hires with Long Non-Employment Spells

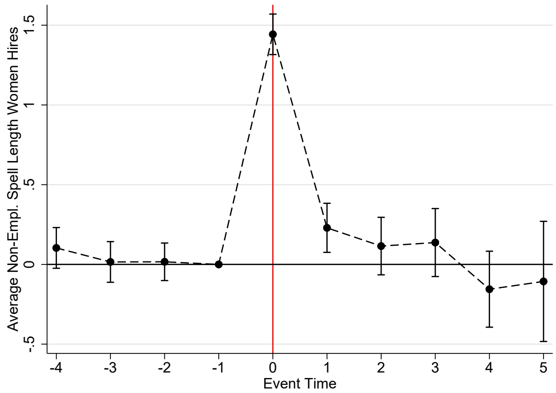
(c) Newly Hired Mothers

(d) Total Firm Employees

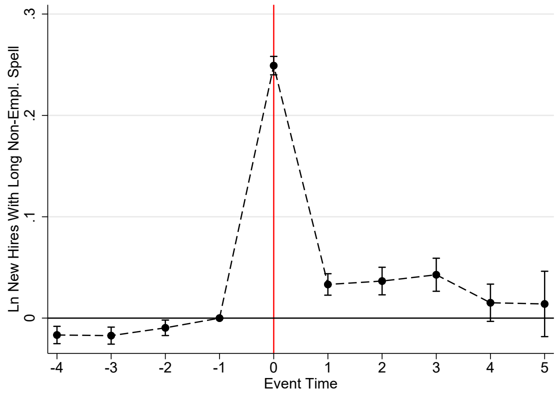
Notes: The figure reproduces the baseline firm event-study of Equation 2 (treated vs. matched control firms; event time = 0, red vertical line) while augmenting the specification with additional high-dimensional fixed effects: 2-digit-industry-by-year (long-dashed lines), province-by-year (solid lines), or matched treated–control firm-pair-by-year fixed effects (short-dashed lines). Outcomes: Panel (a), the average length of prior non-employment (in years) among new female hires; Panel (b), the natural logarithm of (1+) the number of new female hires from long-term ($\geq$ 3 years) non-employment; Panel (c), the natural logarithm of (1+) the number of newly hired mothers, defined as women who took maternity leave in the previous four years; Panel (d), the natural logarithm of total firm employment. All specifications also include firm, calendar-year, and event-time fixed effects; coefficients are differences between treated and control firms relative to event time -1 (normalized to zero). The estimation sample is the matched sample of 38,270 firms, described in Section 5 and Table 2. Vertical bars denote 95% confidence intervals based on standard errors clustered at the firm level. *Source:* Administrative matched employer-employee records from the Italian Social Security Institute (INPS), 2008–2019.

Figure A.4: Robustness: Firm Hiring Patterns, Treated-Only Staggered Design

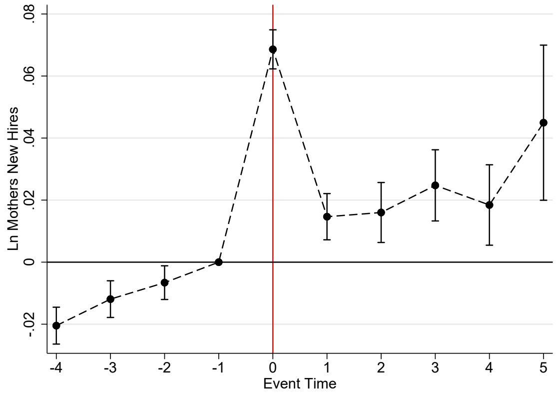
(a) Previous Non-Employment Period



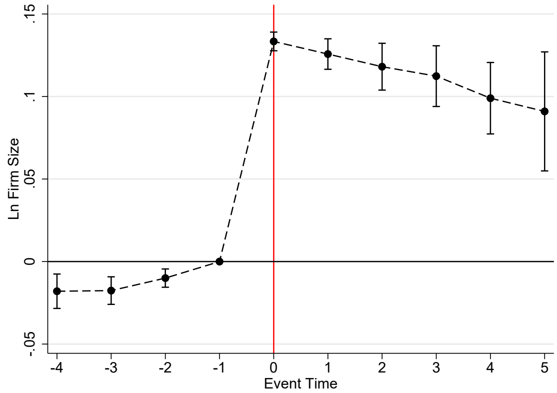
(b) Female Hires with Long Non-Employment Spells



(c) Newly Hired Mothers



(d) Total Firm Employees



Notes: The figure reports a robustness check exploiting only the staggered timing of subsidy adoption across treated firms, estimated with the interaction-weighted estimator of Sun and Abraham (2021) (Stata command *eventstudyinteract*), which is robust to treatment-effect heterogeneity across adoption cohorts. The estimation sample includes treated ("Bonus Donne") firms only, using the last adoption cohort (2019) as the control group. Coefficients capture the dynamic differences in outcomes before and after a firm's first subsidized hire (event time = 0, red vertical line), relative to event time −1 (normalized to zero). Outcomes: Panel (a), the average length of prior non-employment (in years) among new female hires; Panel (b), the natural logarithm of (1+) the number of new female hires from long-term (≥ 3 years) non-employment; Panel (c), the natural logarithm of (1+) the number of newly hired mothers, defined as women who took maternity leave in the previous four years; Panel (d), the natural logarithm of total firm employment. The estimation sample includes the 19,135 treated firms in the matched sample, described in Section 5 and Table 2. Vertical bars denote 95% confidence intervals based on standard errors clustered at the firm level. *Source:* Administrative matched employer-employee records from the Italian Social Security Institute (INPS), 2008–2019.

A.3 Appendix Tables

Table A.1: Additional Firm Descriptive Statistics

	(1) Bonus Donne Firms	(2) Other Private-Sector Firms
	Panel (a): Firm size distribution	
N. Employees	Firm shares	
2-4	0.31	0.43
5-15	0.44	0.41
16-50	0.18	0.12
51-250	0.06	0.03
$\geq$ 251	0.01	0.00
	Panel (b): Geographic distribution	
Area	Firm shares	
Center	0.21	0.22
Islands	0.14	0.09
North-east	0.14	0.21
North-west	0.17	0.28
South	0.34	0.20
Number of firms	26,497	660,764

Notes: The table reports the distribution of firms by size bin (Panel (a)) and by macro-region (Panel (b)), comparing firms that used the Bonus Donne to hire at least one woman between 2013 and 2019 (Column 1) with all other private-sector firms that never used the subsidy (Column 2). Entries are shares of firms within each column (each panel sums to one), averaged over 2013–2019. Size bins are based on the number of employees (one-employee firms are excluded from the sample; see Section 3). *Source:* Administrative matched employer-employee records from the Italian Social Security Institute (INPS).

Table A.2: Firm Breakdown by Sector

	(1) Bonus Donne Firms	(2) Other Private-Sector Firms
Mining and quarrying	0.04	0.14
Manufacturing	19.60	21.15
Construction	4.54	12.21
Wholesale and retail trade; repair of motor vehicles	21.75	19.56
Transportation and storage	2.38	4.11
Accommodation and food service activities	20.84	18.81
Information and communication	2.62	2.24
Financial and insurance activities	1.10	1.30
Real estate activities	0.54	0.52
Professional, scientific and technical activities	4.70	4.71
Administrative and support service activities	7.14	5.24
Education	2.33	1.20
Human health and social work activities	5.86	3.02
Arts, entertainment and recreation	1.15	1.05
Other service activities	5.41	4.73

Notes: The table reports the sectoral distribution of firms by 1-digit industry, comparing firms that used the Bonus Donne to hire at least one woman between 2013 and 2019 (Column 1) with all other private-sector firms that never used the subsidy (Column 2). Entries are percentages of firms within each column (each column sums to 100), averaged over 2013–2019. *Source:* Administrative matched employer-employee records from the Italian Social Security Institute (INPS).

Table A.3: Worker Descriptive Statistics

	(1) Bonus Donne Hires	(2) Non-Bonus Donne Hires
Mean age	38.55	35.19
Mean ln weekly wage last job	5.76	5.75
Mean ln weekly wage	5.76	5.69
Average non-empl. spell (years)	6.08	1.74
Mother	0.07	0.06
Hired with open-ended contract	0.27	0.43
Hired with full-time contract	0.31	0.45
Blue-collar	0.61	0.57
White-collar	0.39	0.36
Number of workers	183,615	10,043,297

Notes: The table compares female workers hired under the Bonus Donne (Column 1) with other female new hires (not under the Bonus Donne) over 2013–2019 (Column 2). All statistics are computed in the year of hire. Mean ln weekly wage is the natural logarithm of the weekly wage in the year of hire; Mean ln weekly wage last job is the natural logarithm of the weekly wage in the final year of the most recent previous job, if any; Average non-employment spell is the number of years since the end of the most recent job spell, if any; Mother is an indicator for having taken maternity leave in the previous four years; Hired with open-ended contract, Hired with full-time contract, Blue-collar, and White-collar describe the job in the year of hire (apprentices and managers are omitted). Number of workers reports the sample size in each column. *Source:* Administrative matched employer-employee records from the Italian Social Security Institute (INPS).

Table A.4: Good-Match versus Bad-Match Firms, Terciles

	(1) Ln Subsidized Female Hires	(2) Ln New Hires From Long-Term Non-Employment	(3) Ln Mothers New Hires	(4) Ln Total Firm Size
Estimate	0.0291**	0.0208**	0.0180**	0.0108
	(0.0128)	(0.0095)	(0.0082)	(0.0152)
Observations	46,002	46,002	46,002	46,002
R-squared	0.356	0.742	0.664	0.925

Notes: The table reproduces the good- versus bad-match comparison of Table 3, reporting the estimated γ_2 coefficient from Equation 3, but defining match quality by terciles rather than the median: good- (bad-) match firms are those whose modal initial Bonus Donne hire lies in the top (bottom) tercile of match productivity, proxied by the Mincer wage residual (see Section 6); middle-tercile firms are dropped. The sample consists of treated firms that first used the subsidy between 2013 and 2016. The specification includes firm fixed effects; $Post$ equals one in event years 1–5 and zero in years −4 to −1, so the take-up year (event time 0) is excluded. The outcomes are: the natural logarithm of (1+) the number of women hired under the Bonus Donne (Column 1); the natural logarithm of (1+) the number of female new hires from long-term (≥ 3 years) non-employment (Column 2); the natural logarithm of (1+) the number of newly hired mothers defined as women who took maternity leave in the previous four years (Column 3); and the natural logarithm of total firm employment (Column 4). Standard errors, clustered at the firm level, are reported in parentheses. *, **, and *** denote statistical significance at the 10, 5, and 1 percent level, respectively.
Source: Administrative matched employer-employee records from the Italian Social Security Institute (INPS).

Table A.5: Good-Match versus Bad-Match Firms, Triple Differences

	(1) Ln Subsidized Female Hires	(2) Ln New Hires From Long-Term Non-Employment	(3) Ln Mothers New Hires	(4) Ln Total Firm Size
Estimate	0.0211**	0.0284***	0.0197**	–0.0089
	(0.0094)	(0.0099)	(0.0080)	(0.0165)
Observations	140,448	140,448	140,448	140,448
R-squared	0.353	0.671	0.591	0.929

Notes: The table reports the estimated γ_4 (triple-difference) coefficient from Equation 4, which compares the good- versus bad-match hiring differential at treated firms to the corresponding differential among their matched control firms. As in Table 3, good- (bad-) match firms are those whose modal initial Bonus Donne hire has above- (below-) median match productivity, proxied by the Mincer wage residual (see Section 6). The sample consists of treated firms that first used the subsidy between 2013 and 2016, together with their matched control firms. The specification includes firm fixed effects; *Post* equals one in event years 1–5 and zero in years −4 to −1, so the take-up year (event time 0) is excluded. The outcomes are: the natural logarithm of (1+) the number of women hired under the Bonus Donne (Column 1); the natural logarithm of (1+) the number of female new hires from long-term ($\geq$ 3 years) non-employment (Column 2); the natural logarithm of (1+) the number of newly hired mothers defined as women who took maternity leave in the previous four years (Column 3); and the natural logarithm of total firm employment (Column 4). Standard errors, clustered at the firm level, are reported in parentheses. *, **, and *** denote statistical significance at the 10, 5, and 1 percent level, respectively. *Source:* Administrative matched employer-employee records from the Italian Social Security Institute (INPS).

Table A.6: Good-Match versus Bad-Match Firms, Current and Past Wage Residuals

	(1) Average percentile residual wage at time of hiring	(2) Average percentile residual wage of past job
Estimate	1.0522**	-0.0553
	(0.4702)	(0.5143)
Observations	39,232	34,106
R-squared	0.505	0.238

Notes: The table reports the good- versus bad-match firm comparison with the outcome being the firm's average percentile wage residual among its new female hires in each year (the outcome residual). Wage residuals are obtained from a Mincer regression of log wages on worker observables, estimated over all female new hires, and each worker's residual is expressed as its percentile in the residual distribution. This is a different Mincer regression from the one estimated on Bonus Donne hires only, which is used to classify good- and bad-match firms (Section 6). Column (1) uses the residual from the worker's wage at the hiring firm; Column (2) uses the residual from the worker's most recent prior job. The table reports the estimated γ_2 coefficient from Equation 3: the differential post-adoption change in the average wage residual of new female hires between treated firms whose initial Bonus Donne hire was a better versus worse match (good- versus bad-match firms). The sample consists of treated firms that first used the subsidy between 2013 and 2016. A firm is classified as good- (bad-) match if its modal initial Bonus Donne hire has above- (below-) median match productivity, proxied by the residual from a Mincer wage regression estimated on initial subsidized hires (see Section 6). The specification includes firm fixed effects; $Post$ equals one in event years 1–5 and zero in years -4 to -1, so the take-up year (event time 0) is excluded. Standard errors, clustered at the firm level, are reported in parentheses. *, **, and *** denote statistical significance at the 10, 5, and 1 percent level, respectively. *Source:* Administrative matched employer-employee records from the Italian Social Security Institute (INPS).

Table A.7: Worker Balance: Treated versus Matched Control Workers

	(1) Treated workers	(2) Control workers
Mean age	37.91	38.17
Hired with open-ended contract	0.23	0.23
Hired with full-time contract	0.37	0.34
Non-employment years	4.01	3.92
Apprentice	0.00	0.00
Blue-collar	0.53	0.54
White-collar	0.47	0.45
Manager	0.00	0.00
Number of workers	13,336	13,336

Notes: The table reports mean characteristics of treated (Column 1) and matched control (Column 2) workers in the matched worker-level sample, measured in the hiring year. Non-employment years is the number of years the worker had spent in non-employment before being hired. The sample consists of female workers hired from non-employment at treated and control firms in the first year a treated firm adopts the subsidy (2013–2019); each woman hired through the Bonus Donne at a treated firm is matched to a woman hired the same year at a matched control firm. Matching is by a propensity-score logit for the probability of being hired through the subsidy, controlling for age, contract type (fixed-term vs. open-ended, part-time vs. full-time), broad occupational category (manager, white-collar, blue-collar), and the length of the preceding non-employment spell (see Section 7). *Source:* Administrative matched employer-employee records from the Italian Social Security Institute (INPS).

A.4 Model Details

In this appendix, we introduce a framework that illustrates the connection between the hiring subsidy, firms' hiring decisions, and the learning process that follows when firms hire from a group they would otherwise avoid. This framework clarifies the following points:

1. The policy can have lasting effects on firms' decisions to hire from the targeted group, if it resolves a "learning trap" by exposing firms to workers from that group, thereby inducing firms to learn about the group-firm match quality.

2. If firms learn from interacting with workers from the targeted group, the effect of the policy on firms' future hiring decisions depends on the "quality" of the worker the firm interacts with. In particular, firms that interview (and potentially hire) "good matches" are more likely to interview and hire workers from the targeted group in the future, relative to firms that interview/hire "bad matches".

Our model considers two periods in which firms can potentially hire workers. The hiring process is as follows: first, the firm chooses whether to interview a candidate (based only on the worker's group membership, which informs the firm's prior beliefs about expected productivity); second, if the firm chooses to conduct a costly interview, it observes an additional signal about that worker's productivity, and decides whether to hire them. The expected value of interviewing a worker is affected by the policy, since the subsidy covers part of the worker's wage; hence, the policy affects interviewing and hiring decisions.

Through the signal observed at an interview, the firm learns not only about the worker, but also about the group-firm component of productivity. This learning channel implies that first-period interview decisions can have lasting effects by influencing subsequent decisions to interview and eventually hire workers from that group in the second period.

In this model, we also posit that wages are set at the time of hiring, after the firm observes the signal of the worker's productivity. Within this framework, the wage (and in particular wage residuals) reflects the firm's perceived quality of the match at the time of hire.

A.4.1 Environment

A worker i from group g working in firm j has productivity

$$y_{igj} = \bar{y}_{gj} + \theta_{gj} + \theta_i,$$

where $\bar{y}_{gj}$ is a known baseline fit of a person from group g at firm j; θ_{gj} represents the unknown group-firm component of productivity; and θ_i represents an unknown idiosyncratic component of individual i. We assume that $\theta_{gj} \sim \mathcal{N}(\mu_{gj}, 1)$ and $\theta_i \sim \mathcal{N}(0, 1)$, and that they are mutually independent.[38]

Interview decision. The first step of the hiring process is an interview decision for the firm. At this stage, the firm observes only the worker's group membership g and decides to interview them if the expected value of the interview (and subsequent hiring decisions) exceeds some fixed interviewing cost $\kappa > 0$. We denote the expected value of conducting the interview by V_g, an endogenous object that we will describe further below, which depends on features of the learning environment, such as the firm's prior belief about the group-firm component of productivity, μ_{gj}. Firm j chooses to interview the candidate from group g if $V_g \geqslant \kappa$.

Hiring decision. Upon interviewing worker i, the firm observes a signal of the worker's productivity, given by

$$\tilde{y} = y_{igj} + \epsilon,$$

where $\epsilon \sim \mathcal{N}(0, \sigma^2)$. After observing $\tilde{y}$, the firm forms a belief about y_{igj}:

$$\hat{y}_{igj} = \mathbb{E}\left[y_{igj}|\tilde{y}\right] = \frac{2}{2+\sigma^2}\tilde{y} + \frac{\sigma^2}{2+\sigma^2}(\bar{y}_{gj} + \mu_{gj}) = (\bar{y}_{gj} + \mu_{gj}) + \frac{2}{2+\sigma^2}\left[\tilde{y} - (\bar{y}_{gj} + \mu_{gj})\right]. \tag{A1}$$

The firm's payoff from hiring the worker is $\hat{y}_{igj} - w$, where w represents the worker's wage. We posit that the worker's wage (if hired) is proportional to their expected productivity at the firm, so that $w = \alpha\hat{y}_{igj}$, for some fixed $\alpha \in (0, 1)$. Considering a hiring subsidy that pays

[38] We normalize the variances of θ_{gj} and θ_i for simplicity. Allowing for different variances would affect the weights used in Bayesian updating, but would not impact our results qualitatively.

a proportion $\beta \in (0, 1)$ of the worker's wage, the firm's surplus from hiring the worker is $s = [(1 - \alpha) + \beta\alpha]\,\hat{y}_{igj}$. The firm hires the worker if the surplus is non-negative.

We can write the value of interviewing a worker from group g (the group subject to the subsidy) as

$$V_g = \mathbb{E}_g \left[\max\left\{0, ((1 - \alpha) + \beta\alpha)\,\hat{y}_{igj}\right\}\right],$$

where the expectation is taken with respect to the distribution of $\hat{y}_{igj}$ in group g, given μ_{gj} and σ^2. Note that the hiring subsidy policy does not affect the firm's decision to hire, since the firm always hires workers with $\hat{y}_{igj} \geqslant 0$, but raises V_g, thereby incentivizing firms to interview workers and, through this channel, increase hiring from group g.

Learning. Apart from learning about individual workers' productivity, the firm also learns about the group-firm component of productivity θ_{gj} from observing $\tilde{y}$. This learning is important in our context as it connects first-period hiring decisions to subsequent hiring behavior toward the same group. Specifically:

$$\hat{\mu}_{gj} = \mathbb{E}\left[\theta_{gj}|\tilde{y}\right] = \frac{1}{2 + \sigma^2}(\tilde{y} - \bar{y}_{gj}) + \frac{1 + \sigma^2}{2 + \sigma^2}\mu_{gj}. \tag{A2}$$

The firm's belief about θ_{gj} updates upwards if the "surprise" $(\tilde{y} - \bar{y}_{gj})$ is larger than μ_{gj}, and downwards if this "surprise" is smaller than μ_{gj}.

Second-period hiring. Going into the second period, firms differ in what they have learned about θ_{gj}, the group-firm component of productivity. First, a firm may have had *no learning*, which happens if the firm chose not to interview. In that case, second-period hiring mirrors first-period hiring, absent any policy change that might alter the expected value of interviewing V_g (for example, a change in the subsidy rate).

Firms that interviewed a candidate in the first period observed a signal $\tilde{y}$, and therefore learned about θ_{gj} via the "surprise" $(\tilde{y} - \bar{y}_{gj})$. This learning affects hiring in the second period by changing the distribution of θ_{gj}, so that

$$\theta_{gj} \sim \mathcal{N}(\hat{\mu}_{gj}, \hat{\sigma}^2),$$

where $\hat{\sigma}^2 < 1$ (the closed-form expression of $\hat{\sigma}^2$ follows from standard Bayesian updating). This change in the distribution of θ_{gj} affects firms' decisions to interview and hire in the second period. In particular, firms that receive more favorable signals (i.e., higher realizations of $\tilde{y}$) in the first period are more likely to interview workers from group g in the second period, and, as a result, more likely to hire from this group, relative to firms that receive less favorable signals.

A.4.2 Empirical Implications

This framework highlights a possible long-term effect of the subsidy policy: it can resolve a *learning trap* that prevents firms from hiring from a given group—in our application, women with extended employment interruptions. This occurs in three stages:

Stage 1. In "period 0," before the hiring subsidy is introduced, we assume the firm's beliefs about θ_{gj} are given by $\theta_{gj} \sim \mathcal{N}(\mu_{gj}, 1)$, with μ_{gj} small enough that

$$V_g = \mathbb{E}_g \left[\max\left\{0, (1-\alpha)\hat{y}_{igj}\right\}\right] < \kappa.$$

(To see that such a small μ_{gj} exists, note that $\hat{y}_{igj}$ is increasing in μ_{gj}, and V_g is an increasing function of $\hat{y}_{igj}$.) Because the value V_g of interviewing a candidate from group g is so small, such candidates are never interviewed. Consequently, no learning occurs, and, in the absence of any policy, candidates from group g are never hired (even in later periods).[39]

Stage 2. In "period 1," a hiring subsidy policy $\beta > 0$ is introduced, where β is large enough that

$$V_g = \mathbb{E}_g \left[\max\left\{0, ((1-\alpha) + \beta\alpha)\, \hat{y}_{igj}\right\}\right] > \kappa.$$

[39] The assumption of a sufficiently low prior mean is one simple way to generate $V_g < \kappa$. The value of interviewing may also be lower if screening signals are less precise for the targeted group, even holding prior mean productivity fixed. Provided these signals remain informative, the subsidy can still induce experimentation and learning about group-firm match quality, although belief updating, and hence the potential persistence in hiring, will be weaker.

With the introduction of this policy, interviewing a candidate from group g becomes profitable. In that period, the firm interviews a candidate and updates its beliefs about θ_{gj}, as described in the learning environment above.

Stage 3. In "period 2," the subsidy policy is removed, so that $\beta = 0$. However, firms' beliefs about θ_{gj} have been updated due to the signals observed during interviews in the previous period. Some of these firms received signals that were "bad surprises," leading them to continue not interviewing, and therefore not hiring, workers from group g, in the absence of the subsidy. Other firms observed sufficiently positive surprises, leading to higher values of $\hat{\mu}_{gj}$ and, consequently, to interviewing and potentially hiring candidates from group g in period 2. On average, the policy can generate persistent increases in hiring of workers in group g, even after the subsidy has been removed. While this stage is not directly testable, since the policy is not removed in our setting, we do document persistent changes in firms' hiring behavior after their initial use of the subsidy. This stage therefore illustrates the mechanism through which temporary subsidies can generate such persistence.

These predictions are consistent with our empirical findings in Section 5.

"Good matches" versus "bad matches" in firm learning. An immediate implication of Equation A2, which describes the learning that occurs after the firm interviews a worker in group g, is that firms that observe more positive "surprises" $(\tilde{y} - \bar{y}_{gj})$ form higher beliefs $\hat{\mu}_{gj}$, and, through an increased propensity to interview candidates from this group, are consequently more likely to hire workers from group g in the next round of hiring.

We test this empirical prediction of the learning model in Section 6, where we define "good" and "bad" matches as workers with high and low realizations of the surprise term. Note that the value of the surprise term $(\tilde{y} - \bar{y}_{gj})$ is positively related to the expected productivity $\hat{y}_{igj}$, which determines the wage offered by the firm. In our empirical analysis, we therefore proxy this surprise term using the Mincerian wage residual of the worker at the time of hiring.