Hiring Subsidies and Female Employment[⋕]

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Abstract

We study a hiring subsidy targeted at women with extended labor market interruptions in Italy to examine firms' short- and long-term hiring decisions following subsidy adoption and the career outcomes of the targeted group. Using comprehensive employer-employee data from social security records, we document that subsidized firms change their hiring pattern by increasing hiring of women from the target group, including mothers. Workers hired through the subsidy are also more likely to remain employed in the long term. We argue that the hiring subsidy operates as a mechanism allowing firms to learn about the productivity of disadvantaged groups.

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1 Introduction

Women remain underrepresented in labor markets around the world. The arrival of children is one of the primary drivers of persistent gender inequalities in the labor market as it often leads women to take career breaks (Adda et al., 2017; Angelov et al., 2016; Kleven, Landais, & Sogaard, 2019). While policy attention has largely focused on supply-side interventions such as parental leave and subsidized childcare,[1] far less consideration has been given to demand-side forces, particularly employers' perceptions and hiring behavior, which may also constrain women's labor market outcomes.

A major demand-side barrier to female labor force participation may stem from employers' reluctance to hire individuals with career interruptions, potentially due to perceptions of lower productivity (Arulampalam, 2001; Eriksson & Rooth, 2014; Kroft et al., 2013).[2] Sociological research shows that mothers are perceived as less competent and committed and face an even lower likelihood of hiring (Correll et al., 2007; Weisshaar, 2018). If firms are imperfectly informed about the productivity of female workers returning after career breaks, policies designed to stimulate employer demand for this group may have significant potential to boost women's overall labor market participation by encouraging firms to experiment with—and learn about—this pool of workers.

In this paper, we study a hiring subsidy aimed at increasing the hiring of women with lengthy labor market interruptions. Many OECD countries—including Italy, Spain, France, Germany, Japan—have implemented gender-targeted hiring subsidies to increase female employment and unlock underutilized human capital for growth. We focus on a hiring subsidy known as "Bonus Donne", enacted in Italy in 2013, which provided a large 50% temporary (12 to 18 months) cut to the employer's social security contributions on new female hires from non-employment. By lowering the cost of hiring, the hiring subsidy provides a direct incentive for firms to hire

[1] Many OECD countries have introduced supply-side policies to support mothers' return to work. However, these interventions have shown limited success in improving women's long-run labor force attachment and earnings (Albanesi et al., 2023; Olivetti & Petrongolo, 2008).

[2] Recent evidence shows that general human capital depreciation during unemployment spells is small (Cohen et al., 2023), suggesting that low perceived productivity—rather than actual skill loss—plays a central role in firms' hesitancy to hire workers with employment interruptions.

workers they might otherwise overlook (Pallais, 2014). Guided by a conceptual framework of employer learning under uncertainty about individual- and group-level productivity, we examine firms' short- and long-term hiring decisions following their adoption of the subsidy and the career outcomes of the targeted group. We find that firms utilizing the subsidy diversified their longer-run hiring patterns by increasing the recruitment of women with lengthy labor market interruptions, including mothers. Importantly, this diversification did not come at the expense of worker quality. We provide evidence that the hiring subsidy facilitated firm-level learning about the potential firm-specific productivity of this disadvantaged group. Finally, we show that the hiring subsidy significantly improved the long-term labor force participation of workers hired under the program.

The Italian context offers a particularly interesting setting to study policies aimed at improving female employment. First, gender differences in the labor market are especially stark: Italy has the lowest female employment rate among EU countries, at 57 percentage points, and the largest gender employment gap (Eurostat, 2025). Consequently, Italy possesses substantial untapped potential for economic growth through increasing female labor market participation (Carta et al., 2023a). Second, to characterize subsidized firms and workers and track them over time before and after subsidy adoption, we leverage rich matched employer-employee records from the Italian Social Security Institute (INPS), covering the universe of private-sector workers and firms in Italy from 2005 to 2019. A unique feature of the data is that it allows us to observe whether a worker is hired under the subsidy, enabling measurement of both individual and firm take-up of the subsidy—an aspect that is key for our analysis.

We first present a conceptual framework that connects a hiring subsidy to firm-level learning about the productivity of disadvantaged groups. The model builds on recent work showing that employers update their beliefs through direct experience with workers (e.g., Benson & Lepage, 2024; Lepage, 2024; Li et al., 2025). In our setting, firms are uncertain not only about individual productivity but also about group-level match quality—for instance, for women with long employment gaps—and form priors that guide their interview and hiring decisions. When sufficiently pessimistic priors deter interviews, firms may fall into a "learning trap" in which

those workers are not interviewed and thus not hired. A temporary hiring subsidy can shift incentives enough to induce experimentation with the disadvantage group, allowing firms to learn about the group's fit. The model implies that (i) exposure to the targeted group during the subsidy can lead to persistent changes in hiring, and (ii) these effects depend on the quality of the initial match.

To examine the short- and longer-run changes in firms' hiring composition after their first hiring of "Bonus Donne" workers, we combine a matching strategy with a staggered event-study design that compares "treated" ('Bonus Donne') firms to suitable control firms. Specifically, we match each hiring-subsidized firm to a comparable control firm based on a set of predetermined characteristics, including firm size, average wages, the share of female employees, and the number of hires in the periods preceding subsidy adoption. The matching approach allows us to construct a counterfactual hire for hiring-subsidized firms in the absence of the subsidy and to investigate changes in the hiring pattern and firm outcomes.

We find that subsidized firms permanently increase their hiring of women with lengthy labor market interruptions, including mothers, relative to control firms. These effects are sizable in the subsidy take-up year, but remain positive, though smaller, in subsequent years. The fact that firms hired these new female workers predominantly in less secure employment contracts suggests that they perceived them as riskier. However, despite having extended labor market interruptions, these new female hires do not negatively impact firm outcomes, such as productivity or profits. Robustness checks that restrict the sample to (eventually) subsidized firms and exploit only staggered treatment timing across them as the source of variation (leveraging Sun and Abraham, 2021) deliver a similar picture.

What explains why firms initially avoided hiring these workers—despite no evidence that they were less productive? According to our conceptual framework, firms may have perceived their productivity as too low—either because of limited past exposure to this group or stereotypes—and the hiring subsidy induces firms to experiment and hire from this group. If firms learn from interacting with workers, the policy's effect on future hiring decisions should then depend on the quality of the initial match. We test this idea by examining whether variation in firms' early

experiences with target-group hires under the "Bonus Donne" influenced subsequent hiring. Using a difference-in-differences framework, we compare "Bonus Donne" firms that took up the subsidy between 2013 and 2016 and initially hired a "good match" to those that hired a less good match. Consistent with the learning mechanisms, firms with better early matches were more likely to continue using the subsidy and hired more women with extended employment gaps and mothers in later years.

In the final part of the paper, we shift the focus to hiring subsidized workers and investigate the long-term employment effects of the hiring subsidy beyond its expiration. Leveraging our ability to follow subsidized workers through the economy, we conduct a matched worker-level analysis that builds on our firm level event-study design. We document positive retention and employment effects for hiring-subsidized workers that persist beyond the subsidy period, indicating that the Bonus Donne strengthened long-term labor market attachment—a key objective of hiring subsidies. Importantly, the program not only facilitated labor market re-entry for these female workers but also enhanced their labor market integration—both through continued employment at the hiring firm and by improving their prospects in the wider economy. At the same time, by comparing subsidized workers with observationally similar female hires at control firms, the design allows us to assess whether the tax cut was passed onto workers in the form of higher wages. We find no evidence of pass-through of the subsidy to workers, consistent with the firm-level evidence that Bonus Donne firms experienced a decline in labor costs and evidence provided by Rubolino (2025).

Our analysis suggests that gender-specific hiring subsidies can be an effective policy tool to promote labor market participation among groups of workers for which labor market re-entry is often difficult: by subsidising experimentation they can have a dynamic effect of shaping employer perception of groups and longer lasting effects on firm behaviour and hiring composition beyond policy duration.

This paper contributes to several strands of the literature. First, by examining firms' responses to a gender-specific hiring subsidy, we bring a new perspective to the literature on hiring subsidies.[3]

[3]While theoretical arguments for gender-based taxation exist (Alesina et al., 2011), empirical evidence remains limited.

Such policies have long been implemented in both the United States and Europe, primarily to boost employment among low-wage workers (Katz, 1996), or as a countercyclical tool during recessions (Neumark, 2013; Neumark & Grijalva, 2017). Earlier U.S. studies relied largely on regional or survey data and focused on aggregate outcomes (e.g., (Bishop, 1981; Neumark & Grijalva, 2017; Perloff & Wachter, 1979)), while evidence based on worker- or firm-level data remains scarce. Using French firm-level administrative data, Cahuc et al. (2019) document positive employment effects of a temporary low-wage hiring subsidy during the Great Recession, whereas Jain et al. (2025), drawing on administrative records from Wisconsin, find that the U.S. Work Opportunity Tax Credit largely operates as a transfer to firms rather than increasing employment or changing hiring behavior.[4] We argue—and provide— empirical evidence that hiring subsidies that lower the cost of experimentation serve as a learning tool about the productivity of disadvantaged groups, as argued by Lepage (2024), a channel that has received little attention in the subsidy literature to date. Our work thus complements contemporaneous research by Rubolino (2025) who focuses on the tax incidence of the Bonus Donne program, rent-sharing within firms, and the policy's fiscal implications, and estimates that most of the program's costs are ultimately recouped.

A related strand of work examines active labor market policies that embed subsidized work experiences. Evaluating a program in Colorado, Barham et al. (2023) analyze employment outcomes for participants and argue that the program facilitates employer learning. However, their analysis focuses worker-level data and does not follow firms over time, limiting their ability to study how employer behavior evolves. Moreover, their focus is on firm learning about the productivity of individual applicants based on direct experience. In contrast, we study learning from the firm's perspective but also emphasize its dynamic implications: how initial exposure to a disadvantaged group shapes beliefs about group-level productivity and affects future hiring behavior. We also evaluate outcomes for subsidized workers and find not only increased retention,

[4]There is a sizable literature, predominantly for the European context, on the effects of permanent reductions in employer social security contributions (e.g,Saez et al. (2012), Saez et al. (2019), Saez et al. (2021), Huttunen et al. (2013), Benzarti and Harju (2021a) and Benzarti and Harju (2021b), Bíró et al. (2024) and Ku et al. (2020). 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, and none of these studies address gender-specific aspects.

but also greater long-term labor market attachment—suggesting that these workers became more attractive to other firms. This contrasts with Barham et al. (2023), who find employment effects concentrated among workers retained by their host employer.

Second, our paper offers a complementary perspective to the literature on regulatory policies to increase labor market representation, such as gender quotas (e.g. Bertrand et al., 2018; Maida & Weber, 2022; C. Miller et al., 2022)) and affirmative action targeting minorities (e.g. Coate & Loury, 1993; Fryer & Loury, 2013; A. Miller & Segal, 2012; C. Miller, 2017).[5] While gender quotas are designed to increase the representation of women in top corporate positions, empirical evidence suggests that boardroom quotas have had limited short-run effects on female participation beyond the board itself (Bertrand et al., 2018; Maida & Weber, 2022). In contrast, the evidence on affirmative action—largely based on U.S. programs targeting racial minorities—shows that even temporary interventions can have persistent effects on workforce composition. We contribute to this literature by studying a policy that offers financial incentives to employers to hire disadvantaged workers. We document that exposure to the subsidy lead to persistent changes in firms' hiring patterns towards disadvantaged groups, which do not appear to come at the cost of efficiency. These findings are consistent with the subsidy facilitating experiential learning about the quality of disadvantaged groups.

Finally, this study relates to the aforementioned literature on experience effects and experiential learning. A growing body of work shows that hiring decisions are shaped by employers' personal experiences, particularly with underrepresented groups (e.g. Benson & Lepage, 2024; Lepage, 2024; Leung, 2018; Li et al., 2025). Stereotypes or inaccurate beliefs about minority applicants' productivity can impede learning (Bohren et al., 2019; Li et al., 2025), and negative experiences with minority hires may discourage future hiring from those groups that could correct biases (Benson & Lepage, 2024). Conversely, nudges that encourage firms to learn about minority workers or provide additional information about these groups can raise minority hiring.[6] Studying the role of hiring algorithms, Li et al. (2025) find that recruiters and algorithms underestimate the

[5] On the theoretical rationale for such policies, see Fryer and Loury (2013), who show that valuing diversity can enhance social welfare even when it entails short-run efficiency costs.

[6] Cullen et al. (2023) show that providing employers with information about workers with criminal records improves their willingness to hire.

value of learning about workers with less common characteristics, and that a screening algorithm that values exploration increases both diversity as well as quality. We add to this literature by providing theoretical and empirical evidence that hiring subsidies can serve as an effective policy tool to nudge firms toward experimentation and learning about minority groups.

The paper is organized as follows. Section 2 describes the institutional setting and the policy. Section 3 presents our data and provides descriptive statistics. Section 4 presents the empirical strategy. Section 5 reports the main estimates and robustness checks and Section 6 presents evidence on firm learning. Section 7 explores the longer-run changes on the worker level. 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 developed countries: Italy has the lowest female employment rate among EU countries at 57%, and the largest gender employment gap (Eurostat, 2024). Many mothers drop out of the labor force after having a child, with 60% of the gender employment gap explained by the child penalty (Kleven et al., 2024). The employment drop after childbirth also implies substantial and lasting earning losses for mothers (Casarico & Lattanzio, 2023). The historically strong employment protection for incumbent jobs in Italy—partly shaped by traditional male breadwinner norms (Cahuc et al., 2019)—has been shown to contribute to lower female employment rates (Giavazzi et al., 2013).[7] Like in other European countries, the Italian labor market has a dual structure, 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 support (Boeri, 2011). This leads to labor market segmentation, with women largely over-represented in outsider categories and suffering from more intermittent labor market participation due to hiring into

[7] Gender gaps in labor market outcomes in Italy are exacerbated by less egalitarian gender norms, especially in Southern regions (Campa et al., 2010).

more precarious contracts (Carta et al., 2023a).

2.2 The "Bonus Donne"

At the height of the European sovereign debt crisis, many European countries, including Italy, implemented structural reforms designed to stimulate employment and economic 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"*) that introduced various measures aimed at *"achieving an inclusive and dynamic labor market, capable of contributing to the creation of employment, in quantity and quality, to social and economic growth and to the permanent reduction of the unemployment rate"*.[8]

The hiring subsidy analyzed in this paper was among the policies introduced in this package. It came into force on January 1, 2013 and was dubbed "Bonus Donne" (which translates to "Bonus Women") by the media. Indeed, an important priority for the Prime Minister at the time was the persistently low rate of female employment: *"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"*.[9] The subsidy granted a 50% temporary cut to employers' social security contributions for the hiring of women not in employment. 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 the gross salary of new hires, comparable in size to hiring subsidies in the French (Cahuc et al., 2019) and US context (Neumark & Grijalva, 2017).[10]

Firms that take up the subsidy benefit from the tax cut for 12 months if a worker is hired on a fixed-term contract, and for 18 months if the worker is hired on an open-ended contract, or if a fixed-term contract is converted into an open-ended one within 12 months. The extension of the

[8] Appendix A.1 provides more detail on the policy package and the political setting.

[9] https://www.repubblica.it/politica/2011/11/17/news/monti_al_senato_per_la_fiducia_il_testo_integrale_del_discorso-25168289/

[10] 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.

benefit period for open-ended contracts suggests that the policymaker aimed to encourage labor market attachment among non-employed women. 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 of our analysis sample period). The eligible target group were women 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 requirement on non-employment duration was more lenient for some categories of workers.[11] [12]

The legislation also set out requirements for firms, which entailed non-trivial administrative costs. First, firms had to submit a separate online claim to the Italian Social Security Institute for each subsidized hire. Reimbursements of firms' social security contributions were made ex post, at the beginning of the following tax year. Second, the subsidy was restricted to firms that had not experienced a net decrease in employment—excluding worker resignations and retirements—over the 12 months prior to using the Bonus Donne (this requirement applied to the firm as a whole, not just to individual establishments). Finally, the subsidy could only be applied if no other worker with the right to preferential hiring could be hired.[13] Net job growth requirements for the application of hiring subsidies are not uncommon—for example, the US New Jobs Tax Credit was an "incremental employment" subsidy (Katz, 1996)—but often impose significant administrative costs on firms and limit the pool of eligible users (Neumark, 2013).

These provisions, coupled with the fact that the subsidy targeted non-employed women—a group that some firms may be more reluctant to hire (Hamersma, 2003; Katz, 1996)—help explain the relatively low take-up of the Bonus Donne. Using data described in the section, we calculate that

[11] The non-employment period was lowered to 6 months for female workers hired in male-dominated occupations or sectors, or resident in municipalities eligible for EU Structural Funds. Male-dominated occupations and sectors are identified each year by ministerial decrees (Appendix Tables A.1 and A.2). These are occupations and sectors where the gender employment gap is at least 25% larger than the average. There is little variation over time in the list of occupations and sectors identified as male-dominated. EU Structural Funds municipalities have been first identified in 2008, and updated in 2014. Most of these municipalities are located in Southern Italy.

[12] For women over the age of fifty, subsidy eligibility began after 12 months of registered unemployment. Only for this target group, the law requires formal unemployment status. In this case, the subsidy also applies to men over fifty. However, few men were hired through the subsidy, and concentrated in specific, male-dominated industries such as construction and waste management. Hiring-subsidized men are excluded from the analysis.

[13] In Italy, a worker employed under a fixed-term contract for more than 6 months is subject to preferential hiring by the firm within 6 months of the contract's end.

only 3.8% of firms made use of the Bonus Donne to hire at least one woman between 2013 and 2019. The share of non-employed women hired under the subsidy was initially quite low and has gradually increased over time, reaching 8% in 2019 (Appendix Figure A.1 shows take-up numbers). This pattern also suggests informational frictions, as firms may not have initially been aware of the subsidy.[14] Given the rising interest by firms, Italian governments have retained the Bonus Donne and strengthened its generosity over time. Since 2022, the subsidy has been enlarged to 100% of the employer's social security contributions, although subject to a total Euro cap.

3 Data and Descriptive Evidence

This Section describes the data we use in the paper and presents descriptive statistics, characterizing which workers and firms 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 encompass the universe of private-sector workers and firms covered by the social security system in Italy since 1975 (excluding self-employed and public sector employees). Various features of this dataset make it uniquely suited to our research questions.

Crucially, and unlike in most other contexts, we observe subsidy take-up at both the individual and firm level, 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 country-wide dataset employed in the study of hiring subsidies enables simultaneous observation of both individual and firm level take-up, along with detailed information on firm workforce.[15]

[14] The Bonus Donne was introduced alongside more controversial and prominent policies that dominated the public discourse (see Appendix A.1). Previous studies have also highlighted a lack of 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).

[15] For example, Cahuc et al. (2019) use firm-level administrative data for France, but lack worker-level information. A recent exception is Jain et al. (2025), which utilizes matched data for Wisconsin.

Workers. We observe workers' full employment histories since their 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 weekly wage.[16] 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 managers) and demographic characteristics such as age, sex, place and year of birth, year of labor market entry and municipality of work.[17] Maternity leave spells are available in the data starting in 2005, which enables us to identify childbearing of women employed prior to giving birth. 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 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.[18] We restrict our focus to mothers with young children—women who experienced a maternity leave event in the previous four years (our analysis sample begins in 2008). This is a key group of interest for a policy targeting women's re-entry into the labor market. However, since we can identify childbirth only among women who were employed prior to giving birth and we are unable to observe leave-taking events before 2005, our data underestimate the proportion of young mothers among new hires.

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 obtained 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 into the INPS data using a unique national tax firm identifier (*codice fiscale*).

[16]The "full-time equivalent weeks of work" variable exploits information on the number of hours worked in a month (available to INPS, but not to researchers).

[17]Since 2010, 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 share of missing values in the early years of its introduction—which coincide with the reform years—we exclude this variable from our main analysis.

[18]During our period of analysis, paid paternal leave was short with a maximum of two weeks. Parents could take additional leave, up to 6 months, until their child is 12 years old, with 30% replacement rate.

Sample construction. We build a panel dataset consisting of one observation per worker per year between 2008 and 2019—a total of roughly 160 million observations, which forms our worker sample and which we draw on to construct workforce and hiring composition at the firm level.[19] 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 additionally drop the (very few) cases where earnings are negative or missing and where the gender variable is missing. Last, we exclude one-employee firms. After data cleaning and incorporating information on take-up, we calculate that about 180,000 women have been 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.[20]

3.2 Descriptive Statistics

We now turn to the focus of our analysis: firms and workers covered by the Bonus Donne. Table 1 contrasts average characteristics of hiring-subsidized firms (Bonus Donne firms)—i.e., those that used the Bonus Donne at least once between 2013 and 2019—with those of the average Italian private-sector firm. Each variable is measured in 2012, the year before the introduction of the policy. Bonus Donne firms differ from the average Italian firm in multiple aspects. They are larger (48 vs. 18 employees on average—see also Table A.3 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%)—suggesting these may be more female-friendly firms. Indeed, Bonus Donne firms have a higher part-time share (37% versus 30% in other firms) 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. They, however, pay about 3% lower wages, both on average as well as for female workers, and have lower average productivity (measured as value-added per worker). Bonus Donne firms are more likely to be located in the South

[19] 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.

[20] 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.

of Italy—see Appendix Table A.3 Panel (b)—and operating in the service sector, such as in wholesale and retail trade, hospitality and administrative services (Appendix Table A.4).

We then turn to subsidized female workers and compare them with the average female hire between 2013 and 2019 (Appendix Table A.5). A total of 183,615 female workers have been hired with the subsidy during these years. Bonus Donne hires are on average older and earn a slightly higher wage in both their current and previous job (if any) compared to the average female hire. These differences may hint at positive selection among these hires. On the other hand, 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—suggesting that the subsidy is indeed used to hire among disadvantaged groups.

4 Model

In this section, we outline a conceptual framework that clarifies how a one-time hiring subsidy may affect firm hiring decisions persistently. Our model builds on recent work on experience-based learning (Benson & Lepage, 2024; Lepage, 2024; Li et al., 2025). 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 the groups to which workers belong. 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 which firms may have limited information (e.g., Benson et al., 2024; Cornell & Welch, 1996; Lundberg & Startz, 1983). Through the model, we show 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, potentially increasing hiring from that group in the long run.

We assume that a worker i from group j working in firm k has productivity $y_{ijk} = \bar{y}_{jk} + \theta_{jk} + \theta_i$, where $\bar{y}_{jk}$ represents a baseline fit of group j to the firm k (known to the firm), θ_{jk} is the

unknown group-firm-specific component, and θ_i represents an unknown and idiosyncratic quality of individual i. We assume that $\theta_{jk} \sim \mathcal{N}(\mu_{jk}, 1)$ 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 worker's true group differentials, we assume that the firm is uncertain *both* about the individual's productivity θ_i and the group's (firm specific) productivity θ_{jk}.

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

The hiring process. The hiring process takes place in two stages. First, the firm decides whether to interview a candidate. Before interviewing, the firm observes only the worker's group membership j, so the decision on whether to interview is based only on the firm prior belief about the expected productivity of group j to the firm k ($\bar{y}_{jk}$ and μ_{jk}). Since interviews are costly (Kuhn & Yu, 2021), the firm interviews a candidate if the expected value of conducting the interview exceeds the interview cost, $V_j \geqslant \kappa > 0$. We define V_j below.

If the firm conducts the interview, it observes a noisy signal about worker i's productivity, $\tilde{y} = y_{ijk} + \epsilon$, where $\epsilon \sim \mathcal{N}(0, \sigma^2)$. After observing $\tilde{y}$, the firm updates its belief about the worker's productivity, forming an expectation $\hat{y}_{ijk} = \mathbb{E}[y_{ijk} \mid \tilde{y}]$. We assume that the worker's wage w is proportional to their expected productivity at the firm, $w = \alpha \hat{y}_{ijk}$, for some fixed $\alpha \in (0, 1)$. So the firm's surplus from hiring the worker is $s = (1 - \alpha)\, \hat{y}_{ijk}$. The firm hires the worker if this value is larger than zero.

Given the noisy signal of the worker productivity and the wage, the firm decides to interview a worker from group j if $V_j = \mathbb{E}_j\left[\max\left\{0, (1 - \alpha)\, \hat{y}_{ijk}\right\}\right] \geqslant \kappa > 0$, where the expectation is taken with respect to the distribution of the workers' productivities in group j, $\hat{y}_{ijk}$, taking into account the employer's current information about group-firm fit ($\bar{y}_{jk}$ and μ_{jk}) and signal noise (σ^2). We assume that, in the absence of the subsidy, the firm's belief about the targeted group's productivity is sufficiently low that V_j is never larger than the cost of interviewing, creating a "learning trap" in which no interview—and hence no updating and no hiring—occurs.

With a hiring subsidy that covers a proportion $\beta \in (0, 1)$ of the wage, the firm effectively

pays $w = (1-\beta)\alpha\hat{y}_{ijk}$, so that the firm's surplus from hiring the worker becomes $s = [(1-\alpha)+\beta\alpha]\,\hat{y}_{ijk}$, with the ex-ante expected value of interviewing a worker from group j increasing to $V_j = \mathbb{E}_j\left[\max\left\{0, ((1-\alpha)+\beta\alpha)\,\hat{y}_{ijk}\right\}\right]$. By raising V_j, the subsidy increases the expected return from interviewing from the targeted group, encouraging firms to experiment and hire some workers from this group.[21]

Learning. Through the signal observed during the interview, the firm learns not only about the individual worker productivity, but also about the match quality between the group and the firm, θ_{jk}. This learning is particularly important in our context, as it links the firm's initial interview, and subsequent hiring, decisions (induced by the subsidy) to the firm's behavior in the second period toward the targeted group. The posterior expectation of θ_{jk} given the interview signal is:

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

The firm's belief about the group's productivity θ_{jk} increases when the "surprise" term $(\tilde{y} - \bar{y}_{jk})$ exceeds its prior mean μ_{jk}, and decreases when it is smaller. This learning process implies that a one-time subsidy can have effects beyond the initial period. By inducing firms to interview workers from a group they would otherwise avoid, the subsidy generates information about group-firm match quality, potentially breaking the "learning trap" and leading to a persistent change in beliefs about the group's productivity. The direction and magnitude of this updating depend on the realized signal, as firms may learn differently based on their initial experiences.

Second period. Going into the second period, we classify firms in terms of what they have learned about their fit to population j, θ_{jk}. First, a firm may have had *no learning*, if they chose not to interview in the first period. For this firm, the second-period hiring is exactly the same as first-period hiring, except for any policy changes (e.g., adjustments in the hiring subsidy) that alter the expected value of interviewing V_j. Alternatively, a firm that did interview a candidate in the first period observed a signal $\tilde{y}$, and therefore updated their beliefs about θ_{jk} based on the

[21] Note that the subsidy does not affect the firm's decision to hire—since the firm hires all candidates with non-negative expected productivity—but it raises the expected value of interviewing, and thereby increases the likelihood that firms will interview workers from the targeted group.

"surprise" term $(\tilde{y} - \bar{y}_{jk})$. This learning affects their hiring in the second period by changing the posterior distribution of θ_{jk}: $\theta_{jk} \sim \mathcal{N}(\hat{\mu}_{jk}, \hat{\sigma}^2)$, where $\hat{\mu}_{jk}$ is given by Equation (1) and $\hat{\sigma} < 1$.

This change in the distribution of θ_{jk} impacts firms' decisions to interview and hire in the second period. In particular, firms that observed higher realizations of $\tilde{y}$ in the first period will tend to interview and hire more workers from group j in the second period than firms that saw lower realizations. In this framework, we also assume that wages are set at the time of hiring, after the firm observes the signal about the worker's productivity. Hence the wage—and in particular wage residuals—in our empirical application can be interpreted as a measure of the perceived quality of the firm-worker match at the time of hiring. We discuss this point again 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 resolves a "learning trap". By inducing firms to experiment with workers from the group, it facilitates learning about the fit of such workers to the firm.[22]

2. The long-term effect of the subsidy on firm hiring depends on the quality of the initial matches formed: firms that experience high-productivity matches update their beliefs upward and are more likely to continue hiring from the group, relative to firms that experience lower-productivity matches.

These mechanisms generate key empirical predictions, which we test in the empirical analysis below. First, firms that take up the subsidy should exhibit changes in their hiring toward the targeted population. Second, we expect heterogeneous responses depending on the quality of firms' initial matches with workers from the targeted group. In practice, we may not observe extremely poor matches, since such cases do not result in hiring; instead, we will test for differences between firms that experienced relatively good versus less good initial matches.

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

5 Firm Hiring Decisions

As described in Section 3, workers hired under the subsidy come from lengthy employment interruptions, indicating that they belong to relatively disadvantaged groups in the labor market. According to our model, firms that take up the subsidy are induced to experiment with these workers, overcoming the "learning trap" and potentially fostering learning about their firm-match productivity. In this section, we examine whether such experimentation leads to persistent changes in firms' hiring patterns. Do firms continue to hire long-term non-employed women after experimenting through the Bonus Donne, or is such exposure short-lived, with firms reverting to their previous hiring patterns?

5.1 Empirical approach

To answer these questions, we examine how hiring at Bonus Donne firms changes during and after the first exposure to subsidized hires. Doing so 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: they tend to be larger, more female-friendly, and possibly on a steeper growth trajectory. These differences may correlate with subsequent hiring patterns and lead to spurious results. To define an appropriate control group and approximate how hiring would have evolved in the absence of the subsidy, we combine a matching strategy with a staggered event-study design, where we compare the evolution of outcomes between Bonus Donne ("treated") firms and observationally similar "control" firms.[23]

Firm matching. We narrow our focus to firms that are observed every year between 2008 and 2019.[24] We define treated firms as those that hire at least one female worker through the

[23] Our empirical strategy directly exploits take-up information and compares firms that use the subsidy to those that do not, rather than take advantage of the margins introduced by the policy (Footnotes 11-12). Exploiting those margins would leverage variation across large (1-digit) sectors, or between municipalities that are subject to structural interventions from the EU versus those that do not, before and after the introduction of the Bonus Donne in 2013. These margins not only give rise to endogeneity concerns, but also suffer from limited "first stages", as they induce little variation in Bonus Donne take-up—which, as documented in Section 3, is low, particularly in the first year. Given the lack of a first stage, in our empirical design, we use the information on subsidy adoption and compare treated to untreated firms, accounting for observable characteristics through a matching strategy.

[24] 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.

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 pre-determined variables. Specifically, we fit a logit model that relates treatment probability (an indicator variable taking value of one for firms that use the subsidy and zero for other firms) to the following firm-level covariates in the three years before a treated firm's first adoption of the subsidy: average (log) weekly wage, (log) total employment, female share of employment and quartiles of the number of workers hired.[25] Additionally, we impose that control firms hire at least one female worker in the same year as their matched treated firm hires through the Bonus Donne for the first time. Female workers hired by the control firm in the initial period identify the average female hire in the absence of subsidy adoption.[26]

The resulting sample consists of 38,270 firms, half of which are treated (19,135—a matching rate of about 72%).[27] Table 2 shows the balancing properties of the sample. Treated and control firms are well-matched on average wages, workforce size and share of female employees. Additional indicators of workforce composition that have not explicitly been targeted for matching, such as the share of full-time or open-ended contracts, are also quite similar between the two groups.

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

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

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 treated firm j used the Bonus Donne for the first time (for a control firm, this is the

[25] The matching is performed without replacement and with a 0.05 caliper for the propensity score.

[26] We restrict control firms to those that hire at least one female worker in order to construct a comparable female counterfactual hire (e.g., when examining mothers, a male control hire would never be a valid comparison) and to improve overall comparability between treated and control firms. Our results are robust—and, if anything, larger in magnitude—when we relax this restriction and allow control firms that hired workers of any gender in the initial period.

[27] The cohort distribution for treated firms (hence for controls, too) is as follows: 2,498 in 2013, 5,372 in 2014, 1,816 in 2015, 3,170 in 2016, 8,524 in 2017, 8,710 in 2018 and 8,180 in 2019.

year of treatment for its matched treated firm). The specification controls for firm-specific unobserved characteristics (ξ_j), aggregate shocks (ρ_t), as well as for cohort-specific effects (the γ_k coefficients). The coefficients of interest are the β_k's, denoting the difference in outcomes between treated and control firms k years before or after treatment relative to the same difference in period $k = -1$, which is normalized to zero. Standard errors are clustered at the firm level.

By comparing outcomes for firms not only in the same calendar year but also in the same period relative to subsidy adoption, we mitigate potential challenges inherent in event-study models with staggered treatment timing (Sun & Abraham, 2021).[28] When reporting our estimates, we refer to the coefficient β_0 as the immediate (enactment) change in hiring in the initial take-up year, while coefficients for subsequent years capture medium-run changes in firm's hiring patterns following the initial experimentation with the subsidy.

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 the Bonus Donne adoption, as captured by the pre-event coefficients $\beta_{k<0}$. To corroborate our results further, we perform additional exercises where we augment the specification in Equation 2 by including either province-by-year or (2-digit) industry-by-year effects—to account for industry-specific and local labor market shocks that could influence both subsidy uptake 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 remain robust to these additional controls.

A remaining concern is that unobserved factors might jointly influence subsidy adoption and firms' hiring dynamics in ways that may not be fully captured by our extensive set of covariates. To eliminate concerns about selection into treatment, we conduct a final robustness check that

[28]Without controlling for period-from-event effects, Equation 2 would implicitly assume homogeneous treatment effects between early and late adopters, potentially distorting the estimates through negative weights, which can undermine pre-trends tests. By incorporating a matched control group, we are able to estimate period effects and avoid these distortions—see also Baker et al. (2022) and Cengiz et al. (2019).

excludes firms that never adopt the Bonus Donne and focuses solely on *treated* firms, estimating staggered event-study models that exploit variation in treatment timing among these firms only. To address issues about possible treatment effect heterogeneity and the resulting bias, we follow the method proposed by Sun (2022) and use the last treatment cohort (2019) as the control group. Our results are robust to this alternative strategy, which strengthens our confidence that the observed changes in firms' hiring behavior following subsidy take-up are not driven by unobserved selection into treatment.

While our empirical specifications and robustness tests yield consistent results, we acknowledge that this approach might still be subject to potential threats—for instance, time-varying, unobserved firm shocks driving subsidy adoption. However, we stress that our key goal with this analysis is not to causally identify the impact of the hiring subsidy *per se*, but rather to understand how a change in the composition of firm hires, *brought about by the hiring subsidy*, affects subsequent firm behavior. In this context, we therefore view the firm-level evidence—corroborated by several robustness tests—as a stepping stone toward understanding how hiring subsidies enable firms to experiment with the targeted group and foster learning about their potential productivity, as highlighted in the theoretical section.

Main outcome variables. Our analysis concentrates on several outcomes derived from the data described in Section 3. To study the direct effects of the hiring subsidy take-up on firm hires and labor costs, we examine two outcomes: the share of female new hires at the firm hired through the Bonus Donne (computed as the number of subsidized female hires as a share of total female hires each year) and the average (log) gross weekly wage of female new hires at the firm. To further characterize female new hires, we consider four dimensions: the average number of previous non-employment years of female new hires at the firm; the (log) number of female new hires from long-term non-employment (i.e., non-employment period of at least three years before being hired); the (log) number of mothers among female new hires (see Section 3 for a definition of mothers); and the share of female new hires on an open-ended (as opposed to a fixed-term) contract. Finally, we assess broader firm-level outcomes, including the male share of new hires at the firm (male new hires over total new hires); the female share of employment at the firm

(stock of female workers as a share of total firm employment); (log) total firm employment; (log) firm value added; and (log) firm value added per worker.

5.2 Results

We now turn to event-study results from the estimation of Equation 2 on the matched sample of treated and control firms. We begin by presenting the evolution of subsidized female hires in treated firms in Figure 1 Panel (a).[29] In the first year of adoption of the Bonus Donne (event time 0), 70% of new female hires in treated firms were hired through the subsidy. While adoption tends to be concentrated in the initial year, treated firms continue using the subsidy to a lower extent in subsequent years—an average of 6% of subsidized hires among new female hires.

This surge in subsidized female hiring among treated firms is associated with a reduction in the average wage (gross of employer social security contributions) paid to new female hires. Figure 1 Panel (b) shows event-study coefficients and corresponding confidence intervals for the average hiring wage of new female employees. Prior to the introduction of the subsidy, treated and control firms paid comparable wages to female hires. In period 0, when treated firms use the Bonus Donne, they pay wages that are 2.6% lower than those of control firms, suggesting that these firms lower their labor costs for new hires.[30] This wage gap appears to persist over time, though estimates become noisier and smaller in magnitude.

We next shed light on the characteristics of these new female hires compared to the average female hire, both in the enactment period as well as in the medium 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 the non-employment spells. While non-employment spell lengths 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

[29] 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.

[30] Here, we do not control for potential (differential) selection among new hires. In Section 7, we examine worker-level outcomes while explicitly accounting for selection and show that there is no evidence of pass-through of the hiring subsidy: the temporary cut in employer social security contributions was fully retained by firms and not passed on to workers as higher wages. This lack of pass-through aligns with the results of Rubolino, 2023 and suggests that the subsidy effectively reduced the cost of labor for firms.

non-employment spell for female new hires in period 0 was 1.5 years longer in treated firms compared to control firms, representing a nearly 50% increase. Crucially, this effect persists beyond the subsidy take-up year, as treated firms continue to hire women with longer labor market interruptions in subsequent periods.[31]

Panels (b) and (c) further explore hiring patterns for two groups that are potentially 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 Bonus Donne take-up. Treated firms increase their hiring of female new hires with long non-employment spells (of at least three years) at time 0 by over 15%. Again, these effects persist—albeit to a lower extent—in the years following subsidy adoption. Panel (c) shows that treated firms also increase the number of recent mothers among new hires upon subsidy take-up. The number of newly hired mothers rises by 4% in the event year at treated firms, followed by positive and significant increases also in the ensuing periods. Finally, Panel (d) examines the type of contracts offered to new female hires, focusing on stable open-ended contracts. Before treatment, the share of women hired on such contracts evolved similarly across treated and control firms. In the first year of subsidy adoption, however, treated firms reduced this share by 9 percentage points (approximately 22%) relative to control firms. This decline persisted beyond the subsidy take-up year, with a 3 percentage-point reduction in subsequent periods. This pattern suggests a degree of caution on the part of treated firms: while they increased their recruitment of women with longer non-employment spells and mothers, they simultaneously shifted towards offering less secure employment contracts.

Taken together, the evidence from Figures 1 and 2 suggests that the Bonus Donne has led firms to broaden recruitment to the targeted group. Viewed through the lenses of our model, lower expected interview costs have enabled firms to take risks they might have otherwise avoided and hire these workers, typically viewed as a possible bad match. At the same time, this shift in hiring was accompanied by a reduction in the share of open-ended contracts offered, indicating a more cautious approach in the types of employment terms extended to these workers. These

[31] We estimate similar effects when examining the average non-employment spell among all hires (including men), suggesting that this finding is largely driven by new female hires.

findings indicate that policies designed to improve access to employment can influence hiring patterns.

We complement the above evidence by examining the evolution other firm outcomes, as reported in Figure 3. Panel (a) shows that treated and control firms hired men in similar proportions prior to the subsidy take-up year, but treated firms hired 3.3 percentage points fewer men in the take-up year (a 12% decrease). This effect is short-lived, as the difference in the share of males among new hires between treated and control firms quickly reverts to baseline. This temporary shift is partly explained by the fact that treated firms grow more rapidly starting from the take-up year, ultimately reaching a long-term increase in firm size of about 6% as showed in Panel (b). Finally, as reported in Panel (c), treated firms experience a long-run increase in value added of around 5%, while productivity per worker remains unchanged (Panel (d)). This steeper post-take-up growth path of Bonus Donne firms might be at least partially attributed to the lower labor costs of new hires—as firms might use the tax windfall to expand. Importantly, the absence of any decline in productivity per worker indicates that women hired through the subsidy did not adversely affect firm performance, despite potential labor market interruptions prior to hiring. These results imply that subsidized female hires are at least as productive as the average hire.

5.3 Robustness checks

We now show robustness checks, focusing on a selection of outcomes (average non-employment spell, new hires from long-term non-employment, mother new hires and firm size).[32] Figure A.3 plots event study coefficients and corresponding standard errors estimated according to Equation 2 with the additional inclusion of i) province-by-year fixed effects, ii) 2-digits industry-by-year fixed effects, and iii) firm match-pair-by-year fixed effects. The first two specifications leverage variation in the adoption of the subsidy over time between treated and control firms within the same province or 2-digit industry. The third specification controls for common time trends specific to each treated and control matched pair. Event study coefficients estimated after the inclusion of these fixed effects are remarkably similar to each other and to the baseline estimates,

[32] So far, we have examined these outcomes only as part of our robustness checks. Once we enter the peer-review phase, we will be able to run the full set of outcomes, as we don't have data access without peer review due to the data owner's rules.

suggesting that our results are not driven by differential trends at the province, industry or match-pair level.

Figure A.4 shows event-study coefficients estimated over a restricted sample including only treated firms, and using the last treatment cohort (2019) as control group (Sun, 2022; Sun & Abraham, 2021). By comparing outcomes for firms that adopt the Bonus Donne in different periods, this analysis addresses potential selection concerns that firms taking up the subsidy differ from control firms in ways unaccounted for by our matching approach. While point estimates differ in magnitude compared to the baseline, matched-sample approach—being, in fact, larger for most outcomes, although with evidence of some pre-trends—our key findings are confirmed in the treated-only estimation sample. This strengthens our confidence that the observed changes in firms' hiring behavior following subsidy take-up are not fundamentally driven by unobserved selection into subsidy adoption.

6 Firm Learning

In this Section, we turn to the second key prediction of our model, and analyze how firms' hiring behavior varies with the quality of their initial subsidized hires through learning.

6.1 Empirical approach

The previous Section has documented how Bonus Donne firms increased recruitment from the targeted group. Importantly, such hiring was not accompanied by a decline in firm labor productivity, which raises a key question: if these workers are of comparable quality, why were they not hired before the subsidy? A possible explanation we introduce in Section 4 is that firms believe women with long employment interruptions to be a poor match, leaving them in a learning trap in which they never interact with this group of workers. By lowering the cost of interviewing from this group, the subsidy encouraged firms to experiment with these workers and learn about their fit, which subsequently informed new hiring decisions. As our model predicts such adjustment in firm beliefs depends on the initial experience with subsidized hires: a firm that receives a more positive signal about the firm-match-productivity of the target group will

update upwards its beliefs about the target group's firm-match-productivity and, consequently, hire more from this group in the future.

To explore this learning mechanism empirically, we examine whether variation in firms' initial experiences with target-group hires under the Bonus Donne affects their subsequent hiring behavior. We focus on treated firms that adopted the subsidy early (i.e, treatment cohorts 2013 to 2016—a total of 6,428 firms) to track their hiring patterns over time following initial adoption. We ask whether firms that hired better matches in the initial period of Bonus Donne take-up were subsequently more likely to hire again from the target group, relative to 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 from the pool of subsidized workers and distinguish between those hiring a "good" versus a "bad" match, that is, a more versus less productive worker for the firm.

To construct a proxy for the firm-match-productivity of each subsidized hire, we use individual-level data to estimate a Mincerian wage regression that relates (log) weekly wages to key observable characteristics of the worker (age, contract type, occupation, hiring cohort) and the firm (location, 2-digit industry, total employment). The estimated residual—the portion of the hiring wage unexplained by these characteristics—is intended to capture firm-match-specific productivity with the hiring firm, as anticipated in our conceptual framework in Section 4.[33] Notably, the Mincerian regression is estimated on the sample of initial hires: women hired for the first time under the Bonus Donne between 2013 and 2016, and only for the years in which the hiring occurs.[34] We then discretize the distribution of residuals and classify subsidized

[33]Studies in the U.S. retail sector, where turnover is high, often use worker tenure or turnover decisions as productivity measures (Autor & Scarborough, 2008; Benson & Lepage, 2024). Such ex-post measures would be problematic in the Italian context, where labor markets are less flexible and turnover less frequent. Using tenure as a proxy would mechanically lower subsequent subsidy take-up and 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 of how firms perceive a worker's productivity *at the point of hiring*. Furthermore, this empirical measure derived from entry-level wages also corresponds more closely to the perceived productivity of a given worker by the firm when hired, which aligns more closely with the wage set in our theoretical framework (Section 4).

[34]We run a Mincer regression pooling together all subsidized female hires at firms using the Bonus Donne between 2013 and 2016. In addition to the controls listed in the main text, the regression also controls for year fixed effects.

hires below the group median as bad matches and those above the median as good matches.[35] Treated firms are then classified based on the quality of their initial (time 0) subsidized hire(s): firms hiring an above-median worker are labeled as "good-match" firms, while those hiring a below-median worker as "bad-match" firms.[36]

The empirical analysis employs a difference-in-differences framework comparing hiring patterns at bad-match versus good-match firms in the years after the initial subsidy take-up. The year of subsidy adoption—time 0, used to classify firms based on match quality—is hence excluded from the analysis sample. We estimate the following specification:

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

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 year of subsidy adoption, and value 0 for the years -4 to -1. Firm fixed effects (ξ_j) control for time-invariant differences across firms, such as workforce composition or firm policies. The coefficient of interest, γ_2, captures the differential change in outcomes between good- and bad-match firms, averaged over post-treatment periods 1 to 5—thus excluding the large initial takeup (time-0) effects documented in Section 5. Standard errors are clustered at the firm level.

This design relies on parallel trends between good- and bad-match treated firms. To further corroborate it, we augment the specification—which is based on a comparison *within* the group of treated firms—by incorporating the matched control group described in Section 5. Namely, 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 $iii)$ finally compares these two differences to each other. This more demanding specification allows for differential pre-trends among good- and bad-match treated firms, to the extent that they are accounted for by the inclusion of the matched control group.[37]

[35]The empirical results outlined below holds with an alternative classification using the first and third tercile.
[36]If a firm hires multiple subsidized workers at time 0, it is classified based on the modal worker.
[37]The estimated model is as below, where the coefficient of interest γ_4 captures the triple differences effect and

6.2 Results

Table 3 presents estimates of the γ_2 coefficient in Equation 3 for various firm outcomes. Our results provide strong evidence that hiring patterns following a firm's first use of the Bonus Donne depend on the initial experience with Bonus Donne hires. Good-match firms, which hired a better match through the Bonus Donne, hire 2% more subsidized workers in later periods compared to bad-match firms (Column 1). This effect is sizable when compared to a roughly 6% average increase in subsidized hiring in treated firms in post-enactment periods (Figure 1 Panel (a)). Good-match firms also increase their hiring of workers from the targeted group, as hiring of long-term non-employed women and of mothers rises by 2.3% and 1.7%, respectively (Columns 2-3)—accounting for a large part of the post-enactment increase in hiring from these groups at baseline (Figure 2 Panels (b)-(c)). This finding is consistent with a higher perceived group-specific match by the firm and aligns with the firm learning process outlined in our conceptual framework. Finally, Column 4 shows no significant effect on total firm size, suggesting that the increase in hiring from the targeted group is not driven by differential growth between good- and bad-match firms.

We provide robustness tests for these results. First, Appendix Table A.6 confirms our findings when good- and bad-match firms are defined based on whether their (modal) Bonus Donne hire lies in the first or third tercile of the distribution of Mincer residuals, as opposed to above versus below the median. Second, Appendix Table A.7 confirms results under the triple differences design, where good- and bad-draw treated firms are also compared with their matched control. In particular, the lack of a statistically significant difference in any of the outcomes between the control firms (not reported) is a reassuring finding. It suggests that the different hiring trajectories between good- and bad-match firms are unlikely to be driven by differential shocks and indeed result from differences in their initial exposure. Overall, the findings suggest that early positive experience with disadvantaged hires improves firms' willingness to hire from these groups in the future, consistent with our theoretical framework (Section 4) and models of

indicator variables $Post$, $Good_j$ and T_j are defined above:

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

experiential learning (Bardhi et al., 2023; Lepage, 2021; Leung, 2018).

7 Worker Effects

7.1 Empirical approach

So far, we have focused on how firms responded to the Bonus Donne. This final Section complements the above results by shifting the focus to the workers themselves, investigating the long-term labor market outcomes for individuals hired under the subsidy.

Worker matching. To examine worker-level effects, we build on the firm-level design by matching each subsidized hire to a comparable control worker. We focus on female workers hired from non-employment at both treated and control firms, using the matched firm sample described in Section 5 and restrict the analysis to the first year treated firms adopt the subsidy (between 2013 and 2019). For treated firms, we identify female workers hired through the Bonus Donne in that initial year of subsidy uptake. Each of these workers is matched to a newly hired female worker in the same year at the matched control firm, who is similar in various observables before being hired. The matching is based on a propensity-score logit model for the probability that a worker is hired through the subsidy, and 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 apprentice), and the length of the preceding non-employment spell before the hiring year. This approach compares the careers of similar female workers—those of the same age, hired after the same duration of non-employment, 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 any subsequent employer. Compared to the firm-level analysis, this design requires less strong assumptions for internal validity, as a firm's decision to use the subsidy when hiring a worker is likely to be exogenous to workers' choices. This, in turn, enables us to isolate the effect of being hired under the subsidy on a workers' career trajectories.

Our final matched sample consists of 13,336 hiring-subsidized female workers, and an equal

number of matched control female workers. Appendix Table A.8 confirms that the two groups share very similar characteristics.

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

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

where y_{it} denotes the outcome variable for worker i in calendar 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 has been hired through the Bonus Donne (for a control worker, this is the year of treatment for its matched treated worker). 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 time-to-treatment 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 treatment (β_{-1} normalized to zero). They identify the causal effect of being hired under the subsidy on workers' careers under a parallel trends assumption: in the counterfactual scenario where Bonus Donne hires would have been hired without the subsidy, the difference in career trajectories between Bonus Donne and matched workers would have remained constant. Standard errors are clustered at the level of the worker.

Our outcomes of interest for the worker-level analysis are wages and employment trajectories both at the original hiring firm and in the broader labor market. By comparing a Bonus Donne hire with an observationally similar worker who was not hired through the subsidy, the design allows us to address two central questions. First, by accounting for worker selection, we can directly assess whether the temporary payroll tax cut is passed through to workers in the form of higher wages, or whether firms retain it. Second, by tracking workers' for up to five years after they are hired, we can assess whether the Bonus Donne improved long-term labor market attachment even after its expiry—a central goal of hiring subsidies. Because the subsidy is

extended in duration when a worker is hired on, or converted to, a permanent contract, it creates an incentive for firms to retain subsidized hires who turn out to be good matches and may also enhance workers' attractiveness to other employers through accumulated experience.

7.2 Results

Figure 4, Panel (a), shows event-study coefficients for weekly wages net of employer social security contributions, that is, workers' take-home pay. This outcome is measured conditional on employment (i.e., wages are set to missing if the worker is not in employment) and was not targeted in our matching algorithm. Before being hired, the net wages of treated and control workers followed parallel trends. At time 0, the year treated workers were hired through the Bonus Donne and matched control workers were also hired, we observe no divergence in take-home wages and no statistically significant difference in subsequent periods. This result provides direct evidence that there is no pass-through of the hiring subsidy to workers in the form of higher wages, in line with Rubolino (2023) and Jain et al. (2025). Coefficients turn negative in subsequent periods, although they are never statistically significant.

Lower wage growth for Bonus Donne hires may be due to differential attrition in the labor market between treated and control workers.To investigate this, we examine how being hired through the subsidy affects workers' employment trajectories. Figure 4 Panel (b) shows that one year post-hire, subsidized workers are about 10 percentage points more likely to be retained by their initial employer (red circles). This likely reflects the financial incentives offered to firms by the Bonus Donne rules (Section 2.2). While the retention gap narrows over time, it does not sharply decline by year 2—when the subsidy expires—nor does it disappear in the medium run: even five years later, subsidized hires remain nearly 5 percentage points more likely to be employed at their original firm compared to control workers. The findings suggest that firms do not simply churn workers after the subsidy expires but keep them on in the medium run.

We also investigate effects on workers' employment at any firm. Overall labor market attachment might diverge following subsidized hire not only as a result of direct incentives for firms to retain workers, but also from indirect effects—for example, longer initial employment spells of

Bonus Donne hires may improve their skills or signal higher productivity to other employers (see Barham et al., 2023). As shown in Figure 4 Panel (b) (blue circles), treated workers are 8 percentage points more likely to be employed than control peers one year after hire, with the effect persisting in the medium run. While roughly half of this difference is explained by higher retention at the original firm, the remainder reflects a greater likelihood of treated workers transitioning into jobs at other firms.

These findings suggest that hiring subsidies can effectively enhance labor market attachment for the targeted group—not just through immediate job retention, but also by improving long-term employment prospects.

8 Conclusion

Increasing labor market participation of women is a key policy priority in many countries around the world. Among the various labor market interventions undertaken by governments, hiring subsidies for female workers have not received much attention in the economic literature. This paper studies a hiring subsidy implemented in Italy—the Bonus Donne—explicitly targeting female workers out of employment. We combine a matched difference-in-differences estimation with an event-study approach to investigate the effects of the subsidy on firm hiring decisions. Our findings show that the composition of new hires at hiring-subsidized firms differs from that at control firms in several ways following the adoption of the subsidy. These firms increase their hiring of women with long labor market interruptions, including mothers. Despite coming from non-employment, subsidized female hires appear to be of the same quality as the average firm hire. Women hired through the Bonus Donne are more likely to remain employed at the firm in the medium run. Additionally, firms benefit from the subsidy and expand by increasing employment and value-added following its adoption.

Our analysis suggests that gender-specific hiring subsidies can be an effective policy tool to promote female labor market participation and attachment, especially among typically disadvantaged groups of workers, such as mothers, for which labor market re-entry is often difficult. We also document how firms—that may be uncertain about the productivity of these

worker categories—use the subsidy as a learning tool. Namely, we show that firms hiring a high-firm-match worker through the subsidy tend to hire even more subsidized workers, long-term non-employed women (including mothers) in subsequent years, compared to firms hiring a low-match-firm worker.

Last, our worker level findings suggest that hiring subsidies can effectively enhance labor market attachment for the targeted group—not just through immediate job retention, but also by improving long-term employment prospects. This stands in contrast with the results of Barham et al. (2023), who find that medium-run employment effects of a subsidized work program in Colorado were entirely concentrated among participants who transitioned into unsubsidized employment at their host-site employer with no overall employment effect. In our case, the impact of the Bonus Donne appears to operate beyond retention at the initial hiring firm, indicating that temporary hiring subsidies may foster sustained labor market integration.

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Tables and Figures

Tables

	(1) Bonus Donne Firm	(2) Average Italian Firm
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 mean values for firms that have used the Bonus Donne for at least one female hire (Column (1)) versus other Italian firms that have never made use of the subsidy (Column (2)). Data as of 2012. Standard deviations in parentheses.

Table 1: Firm Descriptive Statistics

	(1) Treated firms	(2) Control firms
Targeted for matching		
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)
Not targeted for matching		
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 shows mean values for treated (Column (1)) and control (Column (2)) firms in our matched sample (matching algorithm described in Section 5). Treated firms are those that have used the Bonus Donne. Descriptive statistics are computed in the period before the adoption of the subsidy. Standard deviations in parentheses.

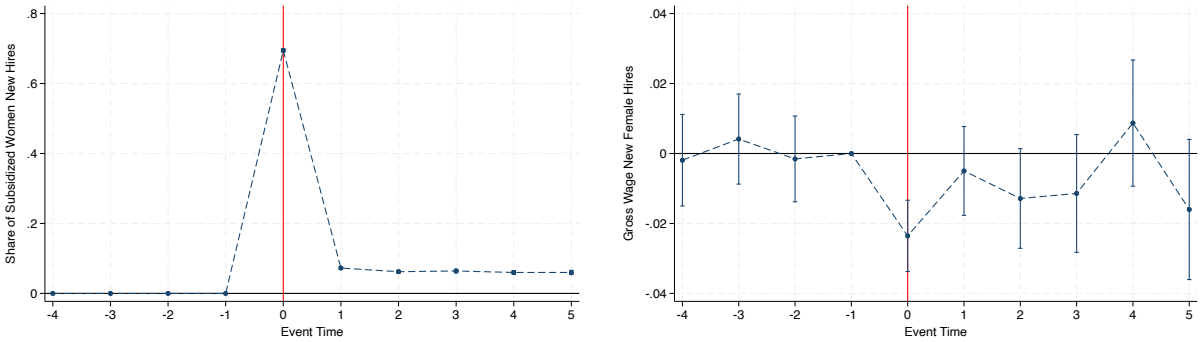
Table 2: Balancing Table, Firms

	(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: Estimated γ_2 coefficient from Equation 3, denoting the differential effect between treated firms hiring a more versus less productive worker through the Bonus Donne (good-match versus bad-match firms). Good (bad) match firms are those where the modal Bonus Donne hire has above (below) median match productivity. Match productivity computed as the residual of a Mincer wage regression, see Section 6 for details. The coefficient is estimated in the post-treatment periods 1 to 5, thus excluding the treatment take-up period. Long-term non-employment defined as non-employment period of at least 3 years. Standard errors clustered at the firm level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Good-Match versus Bad-Match Firms

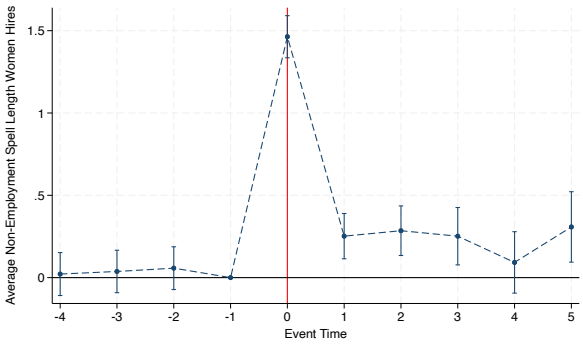
Figures



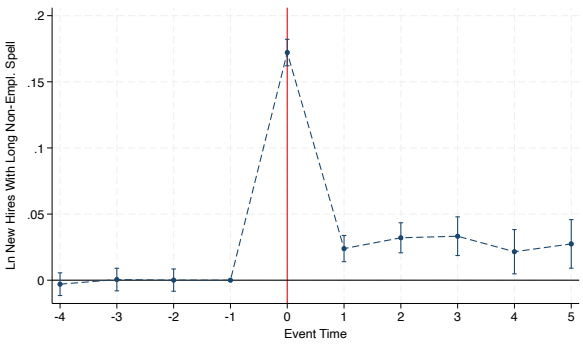
(a) Share hired under Bonus Donne (b) Mean Gross Wages

Notes: Coefficient estimates from Equation 2 denoting differential effects for treated firms using the Bonus Donne versus matched control firms, before and after subsidy adoption (event time = 0). Panel (a): The outcome are female workers hired through the subsidy as a share of new female hires; Panel (b): The outcome is the wage (gross of social security contributions) for new female hires at the firm each period. The red vertical line denotes the period when treated firms hire at least one worker through the Bonus Donne for the first time. The solid vertical lines denote 95% confidence intervals. Standard errors clustered at the firm level.

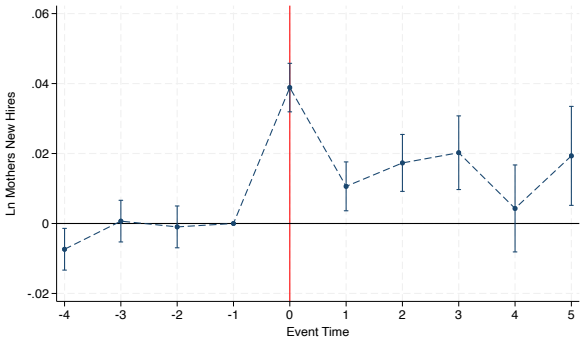
Figure 1: Share and Cost of Female Workers Hired Through the Bonus Donne



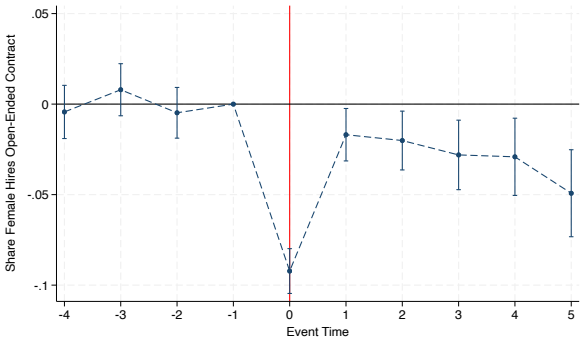
(a) Previous Non-Employment Period



(b) Female Hires with Long Non-Employment Spells



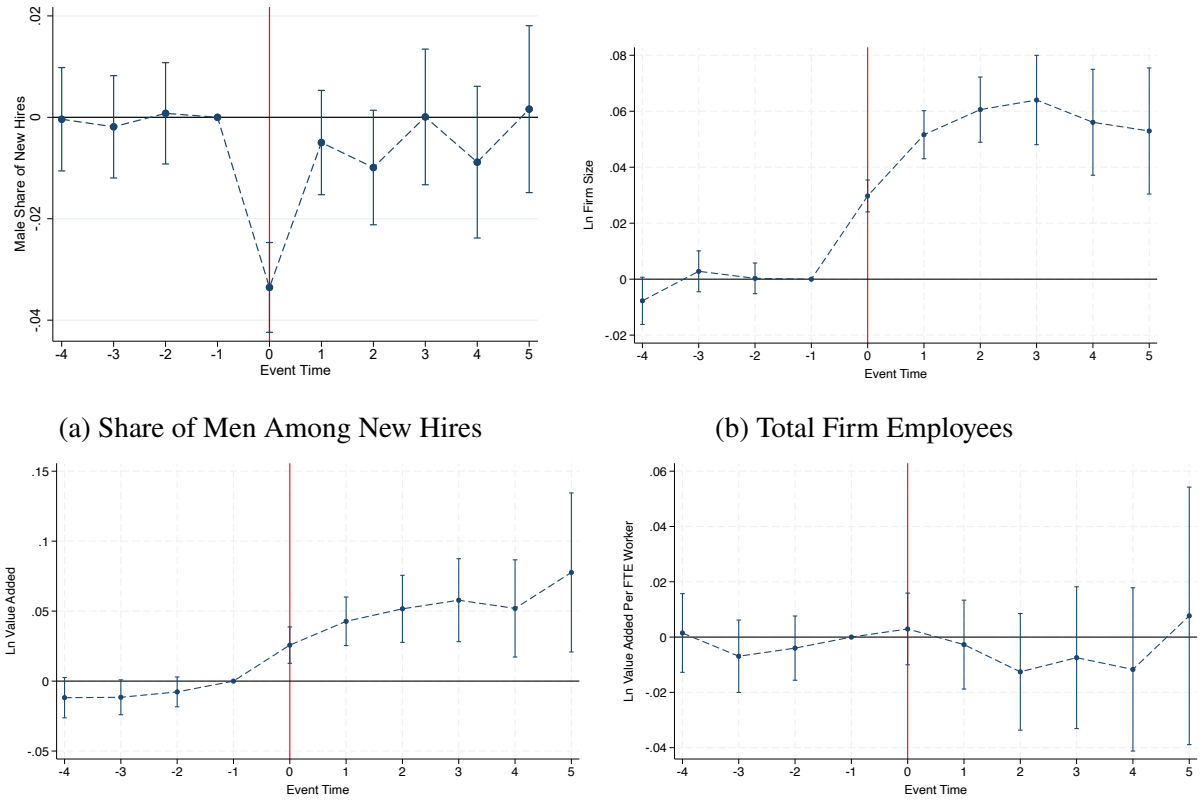
(c) Mothers



(d) Female Hires Under an Open-Ended Contract

Notes: Coefficient estimates from Equation 2 denoting differential effects for treated firms using the Bonus Donne versus matched control firms, before and after subsidy adoption (event time = 0). Panel (a): The outcome is the average length of non-employment (in years) for new female hires; Panel (b): The outcome is the natural logarithm of the number of female new hires from long-term ($\geq$ 3 years) non-employment; Panel (c): The outcome is the natural logarithm of the number of newly hired mothers at the firm (mothers defined as workers that had maternity leave in the previous four years); Panel (d): The outcome is the share of female hires under an open-ended contract. The red vertical line denotes the period when treated firms hire at least one worker through the Bonus Donne for the first time. The solid vertical lines denote 95% confidence intervals. Standard errors clustered at the firm level.

Figure 2: Effects on Firm Hiring Patterns Towards Target Group



(a) Share of Men Among New Hires

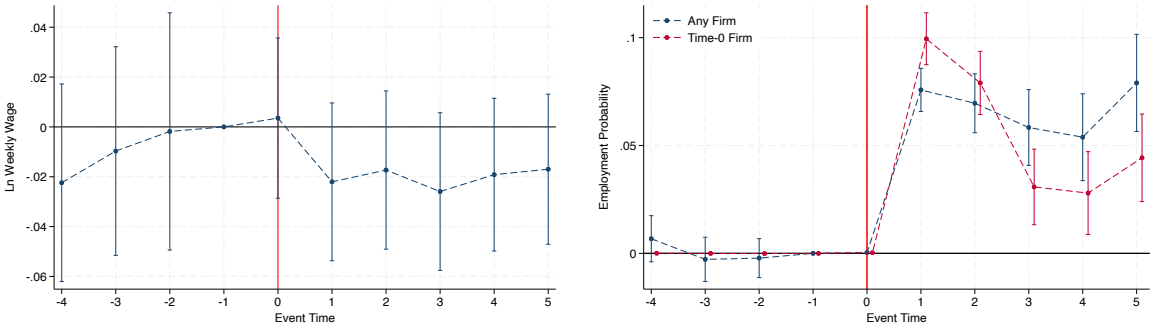
(b) Total Firm Employees

(c) Firm Value Added

(d) Firm Value Added per Worker

Notes: Coefficient estimates from Equation 2 denoting differential effects for treated firms using the Bonus Donne versus matched control firms, before and after subsidy adoption (event time = 0). Panel (a): The outcome is the share of men among new hires; Panel (b): The outcome is the natural logarithm of the total number of employees at the firm; Panel (c): The outcome is firm value Added; Panel (d): The outcome is firm value added per worker. Coefficient estimates β_τ and γ_τ from Equation 2. The red vertical line denotes the period when treated firms hire at least one worker through the Bonus Donne for the first time. The solid vertical lines denote 95% confidence intervals. Standard errors clustered at the firm level.

Figure 3: Other Firm Outcomes



(a) Net Wages (b) Employment Probabilities

Notes: Coefficient estimates from Equation 5 denoting differential effects for treated workers hired through the Bonus Donne versus matched control workers, before and after the hiring year (event time = 0). Treated and control workers are obtained from the matched sample of firms used for the firm-level analysis, see Section 8 for details. Panel (a): The outcome is the natural logarithm of the worker's net weekly wage (missing if the worker is non-employed); Panel (b): The outcome is worker's employment probability at any firm (blue circles) and at the time-0 firm (red circles). The red vertical line denotes the period when treated workers are hired through the Bonus Donne. Vertical solid lines denote 95% confidence intervals. Standard errors clustered at the worker level.

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

Appendices

A.1 Policy Background and Political Setting

Following the Great 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 into power with the goal of implementing structural reforms to bring public debt back under control and promote economic growth. These reforms were implemented in short time, due to pressures from international investors and supranational institutions. Two key labor market 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.[38] The latter had four major components: *i*) it eased employment protection legislation for workers under an open-ended contract by relaxing rules on economic dismissal (a very controversial policy decision); *ii*) it made temporary and atypical contracts marginally more stringent;[39] *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, the latter was the only subsidy targeted to firms and to the hiring of women.

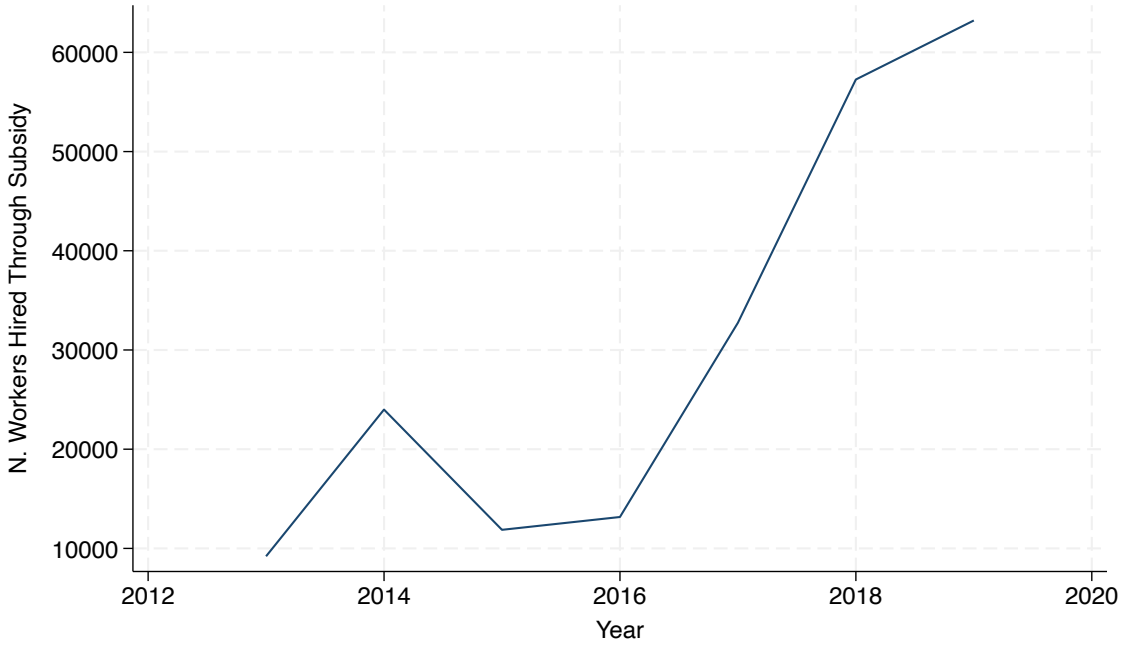
There was strikingly little news coverage of the Bonus Donne. In contrast, most media attention went to the pension reform and to the easing of the employment protection legislation. These policy items were highly controversial in the public debate, as either worsening pension conditions for all cohorts or allegedly going against some of the principles laid out by the Italian constitution (Article 18) by allowing the firing of workers for economic reasons. Google Trends searches clearly illustrate that these policies attracted far more media coverage than the Bonus Donne.

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

[39]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.

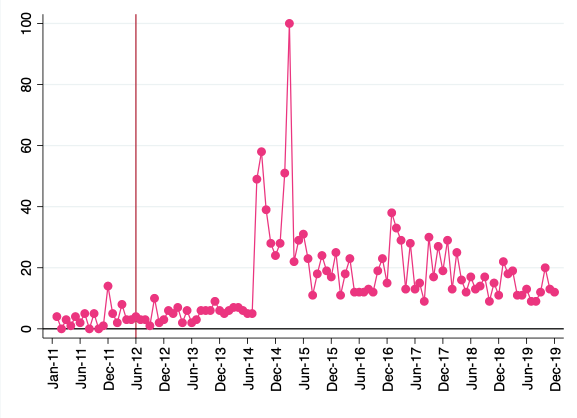
Figure A.2 Panel (a) shows the evolution of Google searches for the item “Bonus Donne” over time. Google searches were very few in 2013 (when the subsidy came into force) and remained low through the first months of 2014. Searches eventually increased and reached a peak in 2015, then remained relatively stable at lower levels from 2016 onwards. Public interest towards 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 reached their maximum, whereas searches for ”Bonus Donne” were very small. This gap is even more striking when Google searches of ”Bonus Donne” are juxtaposed with those for “Article 18” in Panel (c). Such low coverage—hence public knowledge—of the subsidy may be among the reasons for its low take-up, as discussed in Section 3.

A.2 Appendix Figures

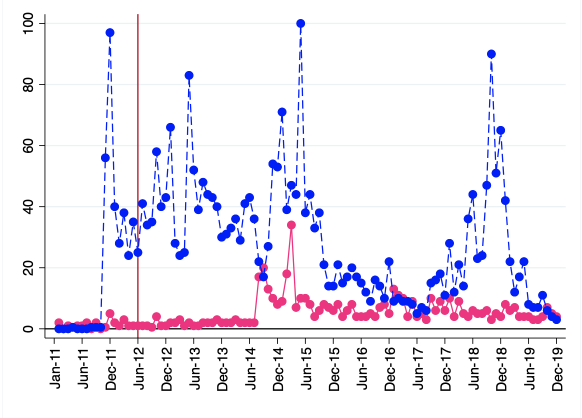


Notes: Number of workers hired every year with the Bonus Donne subsidy, 2013-2019.

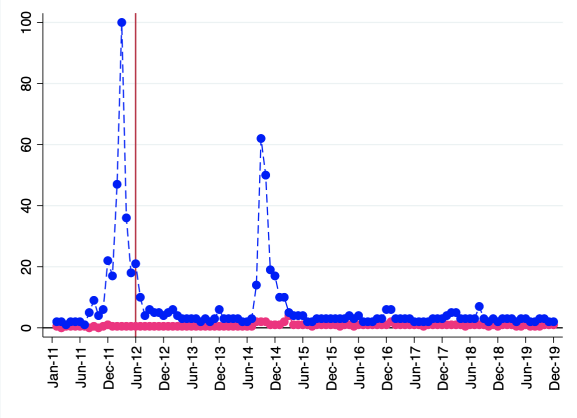
Figure A.1: Subsidy Take-Up Over Time



(a) Google Searches for Item "Bonus Donne"



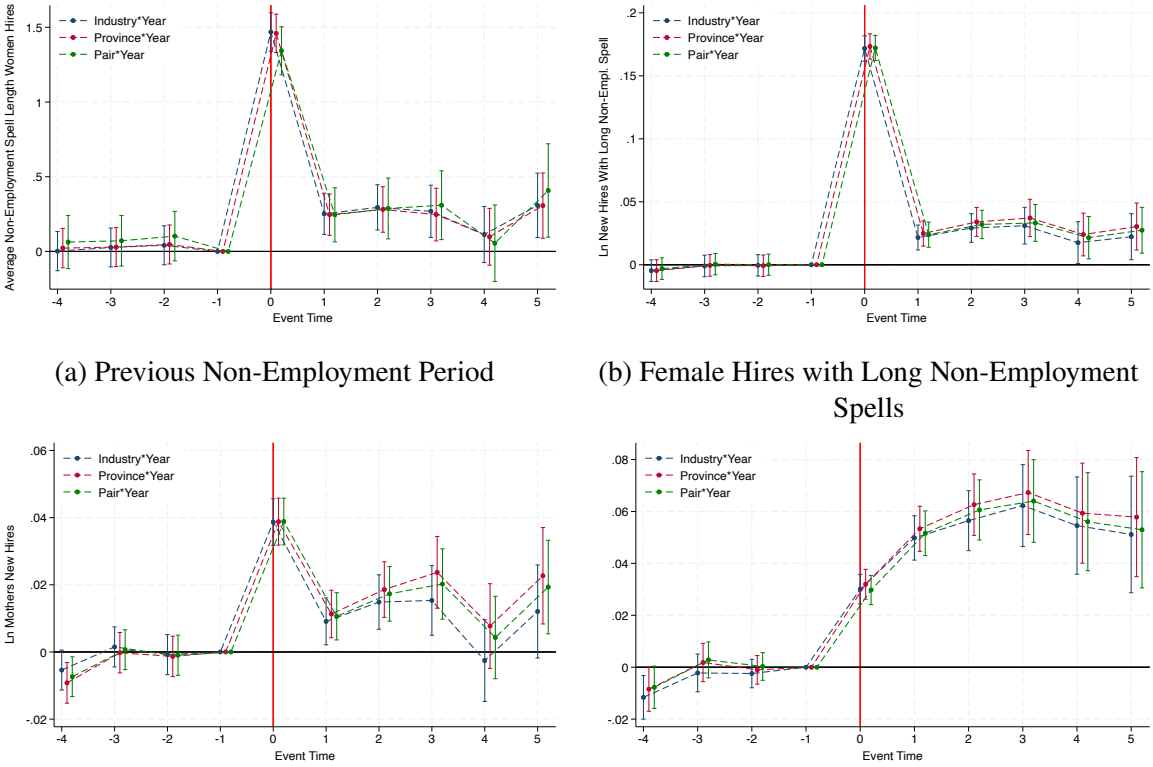
(b) Google Searches for Item "Bonus Donne" (pink) and "Pensioni Fornero" (blue)



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

Notes: Google searches between January 2011 and December 2019. The vertical axis reports the monthly number of Google searches relative to the highest point in the period, normalized to 100. The vertical line denotes the month of approval of the Bonus Donne.

Figure A.2: Google Searches



(a) Previous Non-Employment Period

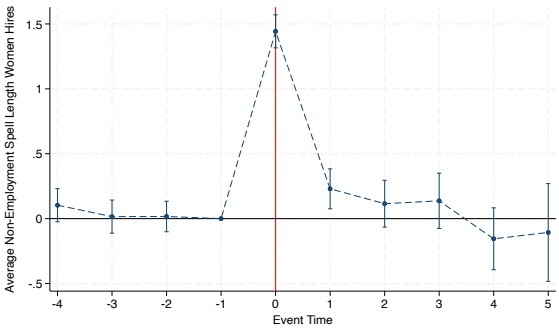
(b) Female Hires with Long Non-Employment Spells

(c) Mothers

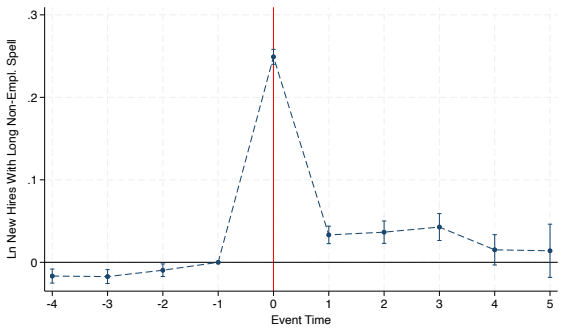
(d) Total Firm Employees

Notes: Coefficient estimates from Equation 2 denoting differential effects for treated firms using the Bonus Donne versus matched control firms, before and after subsidy adoption (event time = 0). In each specification, the model is augmented with either 2-digit industry*year fixed effects (blue circles), province*year fixed effects (red circles) or matched treated-control firm pair*year fixed effects (green circles). Panel (a): The outcome is the average length of non-employment (in years) for new female hires; Panel (b): The outcome is the natural logarithm of the number of female new hires from long-term (≥ 3 years) non-employment; Panel (c): The outcome is the natural logarithm of the number of newly hired mothers at the firm (mothers defined as workers that had maternity leave in the previous four years); Panel (d): The outcome is the natural logarithm of the total number of employees at the firm. The red vertical line denotes the period when treated firms hire at least one worker through the Bonus Donne for the first time. The solid vertical lines denote 95% confidence intervals. Standard errors clustered at the firm level.

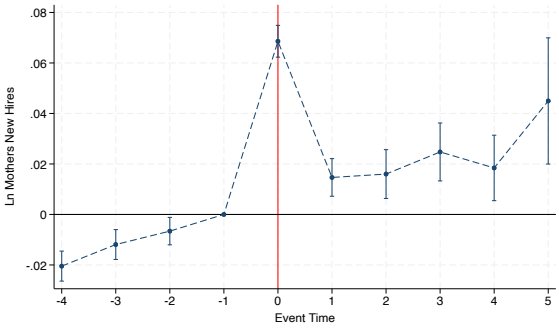
Figure A.3: Robustness: Fixed Effects



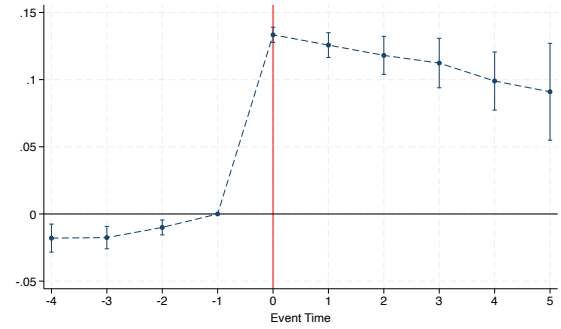
(a) Previous Non-Employment Period



(b) Female Hires with Long Non-Employment Spells



(c) Mothers



(d) Total Firm Employees

Notes: Robustness analysis exploiting staggered treatment timing within treated firms. Event studies estimated using the *eventstudyinteract* command in Stata (Sun, 2022). Coefficient estimates from Equation 2 denoting differential effects for treated firms using the Bonus Donne, before and after subsidy adoption (event time = 0). The estimation sample only considers subsidized firms and uses the last treatment cohort (2019) as control group. Panel (a): The outcome is the average length of non-employment (in years) for new female hires; Panel (b): The outcome is the natural logarithm of the number of female new hires from long-term ($\geq$ 3 years) non-employment; Panel (c): The outcome is the natural logarithm of the number of newly hired mothers at the firm (mothers defined as workers that had maternity leave in the previous four years); Panel (d): The outcome is the natural logarithm of the total number of employees at the firm. The red vertical line denotes the period when treated firms hire at least one worker through the Bonus Donne for the first time. The solid vertical lines denote 95% confidence intervals. Standard errors clustered at the firm level.

Figure A.4: Robustness: Within-Treated Analysis

A.3 Appendix Tables

Sector code	Sector	Male-dominated
A	Agriculture, Forestry and Fishing	Y
B	Mining and Quarrying	Y
C	Manufacturing	Y
D	Electricity, gas, steam and air conditioning supply	–
E	Water supply, sewerage, waste management and remediation activities	Y
F	Construction	Y
G	Wholesale and retail trade; repair of motor vehicles and motorcycles	–
H	Transportation and storage	Y
I	Accommodation and food service activities	–
J	Information and communication	Y
K	Financial and insurance activities	–
L	Real estate activities	–
M	Professional, scientific and technical activities	–
N	Administrative and support service activities	–
O	Public administration and defence; compulsory social security	Y
P	Education	–
Q	Human health and social work activities	–
R	Arts, entertainment and recreation	–
S	Other service activities	–
T-U	Other (Households as Employers and Extraterritorial Organizations	–

Notes: The table reports sectors identified as male-dominated ("Y") and those not ("–") over the years 2013-2019. The sector code follows the 1-digit NACE Rev.2 industry classification. Male-dominated sectors have a gender employment gap 25% larger than Italy's average gender employment gap. They are identified by yearly Ministerial Decrees.

Table A.1: Male-Dominated Sectors

Code	Occupation	Male-dominated
92	Sergeants, superintendents and marshals of the armed forces	2013-2019
74	Drivers of vehicles, mobile and lifting machinery	2013-2019
93	Troops of the armed forces	2013-2019
62	Craft and skilled metalworkers and installers and maintainers of electrical and electronic equipment	2013-2019
91	Officers of the armed forces	2013-2019
61	Craft and skilled laborers in the mining, construction and building maintenance industry	2013-2019
64	Farmers and skilled workers in agriculture, forestry, animal husbandry, fishing and hunting	2013-2019
31	Technical professions in the fields of science, engineering and production	2013-2019
12	Entrepreneurs, directors and managers of large companies	2013-2019
22	Engineers, architects and related professions	2013-2019
71	Industrial plant operators	2013-2019
84	Unskilled professions in manufacturing, mining and construction	2013-2019
21	Specialists in mathematical, computer, chemical, physical and natural sciences	2013-2019
13	Entrepreneurs and managers of small businesses	2013-2019
73	Stationary machinery operators in agriculture and the food industry	2013-2019
63	Craftsmen and skilled workers in precision mechanics, artistic handicrafts, printing and related trades	2013-2019
83	Unskilled occupations in agriculture, green maintenance, animal husbandry, forestry and fishing	2013-2019
72	Semi-skilled workers in stationary series processing machinery and assembly workers	2013-2019
11	Legislative and government bodies, public administration, judiciary, health, education, research, organizations	2013;2015-2019
24	Health specialists	2013-2017
65	Craftsmen and skilled workers in food, wood, textiles, clothing, leather, entertainment	2013-2019
25	Specialists in humanities, social sciences, arts and management	2016
81	Unskilled professions in trade and services	2017-2019

Notes: The table reports occupations identified as male-dominated and the years while they were identified as such in the period 2013-2019. The occupation code follows the Italian CP2011 classification. Male-dominated occupations have a gender employment gap 25% larger than Italy's average gender employment gap. They are identified by yearly Ministerial Decrees.

Table A.2: Male-Dominated Occupations

	(1) Bonus Donne Firm	(2) Average Italian Firm
	Panel (a): Firm size distribution	
N. Employees	Firm shares	
1-4	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 shows a breakdown of firms by size bins (Panel a) and macro-area (Panel b), comparing firms that have used the Bonus Donne for at least one female hire (Column (1)) to other Italian firms that have never made use of the subsidy (Column (2)). Data for 2013-2019.

Table A.3: Additional Firm Descriptive Statistics

	(1) Bonus Donne Firm	(2) Average Italian Firm
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 shows a breakdown of firms by sector, comparing firms that have used the Bonus Donne for at least one female hire (Column (1)) to other Italian firms that have never made use of the subsidy (Column (2)). Data for 2013-2019.

Table A.4: Firm Breakdown by Sector

	(1) Bonus Donne Hire	(2) Average Female Hire
Mean age	38.55	35.19
Mean log weekly wage last job	5.76	5.75
Mean log 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 through Bonus Donne (Column (1)) to other female new hires from non-employment and other jobs (Column (2)) between 2013 and 2019).

Table A.5: Worker Descriptive Statistics

	(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: Estimated γ_4 coefficient from Equation 4, denoting the differential effect between treated firms hiring a more versus less productive worker through the Bonus Donne (good-match versus bad-match firms). Good (bad) match firms are those where the modal Bonus Donne hire is in the first (third) tercile of match productivity. Match productivity computed as the residual of a Mincer wage regression, see Section 6 for details. The estimation sample includes also the matched control firm for each treated firm. The coefficient is estimated in the post-treatment periods 1 to 5, thus excluding the treatment take-up period. Long-term non-employment defined as non-employment period of at least 3 years. Standard errors clustered at the firm level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.6: 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	0.0211**	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: Estimated γ_4 coefficient from Equation 4, denoting the differential effect between treated firms hiring a more versus less productive worker through the Bonus Donne (good-match versus bad-match firms). Good (bad) match firms are those where the modal Bonus Donne hire has above (below) median match productivity. Match productivity computed as the residual of a Mincer wage regression, see Section 6 for details. The estimation sample includes also the matched control firm for each treated firm. The coefficient is estimated in the post-treatment periods 1 to 5, thus excluding the treatment take-up period. Long-term non-employment defined as non-employment period of at least 3 years. Standard errors clustered at the firm level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.7: Good-Match versus Bad-Match Firms, Triple Differences

	(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 shows mean values for treated (Column (1)) and control (Column (2)) workers in our matched sample, where control workers are obtained in the matched sample of control firms (matching algorithm described in Section 7). Descriptive statistics are computed in the period of the adoption of the subsidy.

Table A.8: Balancing Table, Worker-Level Analysis

A.4 Model

In this section, we introduce a framework that clarifies the connection between the introduced policy, the hiring decision made by a given firm, and the learning process that ensues after the firm chooses to hire from a population they typically avoid. This framework clarifies the following points referred to informally in our main analysis:

1. The policy can have lasting effects on firms' decisions to hire from the targeted population, if it resolves a "learning trap" by exposing firms to workers of the targeted population, thereby inducing firms to learn about the fit of such workers to the firm.

2. In a model in which firms learn from interacting with workers of the targeted population — for instance, by choosing to interview them — the effect of the policy on the firms' future hiring decisions depends on the "quality" of the worker the firm interacts with. In particular, firms who interview (and maybe hire) "good match" workers are more likely to interview and hire workers from the targeted population in the future, relative to firms who interview/hire "bad match" workers.

Our model sketch considers two periods in which firms can potentially hire workers. The hiring process is as follows: first, the firm chooses whether or not to interview a particular candidate (based only on their perception of that worker's quality, which is signaled by their population membership); second, if the firm chooses to conduct a costly interview, they observe an additional signal about that worker's quality, and choose whether or not to hire them. The cost of hiring a worker is affected by the policy (since the policy covers part of the worker's salary); and consequently the policy has effects both on the hiring decision, and on the interviewing decision made by the firm.

Through the signal observed at an interview, the firm learns about the worker, but also about the average quality of workers who belong to that population. Through this learning channel, the firm's interview decision in the first period has lasting effects, influencing their decision to interview and/or hire workers from that population in the second period.

In this proposed model, we also posit that wages are set at the time of hiring, after the firm

observes the additional signal of the worker's quality. From this framework, we understand that the wage (and in particular wage residuals) can be interpreted as the firm's perceptions of a worker's quality, at the time of hiring.

A.5 Environment

A worker i from population j working in firm k has productivity

$$y_{ijk} = \bar{y}_{jk} + \theta_{jk} + \theta_i,$$

where $\bar{y}_{jk}$ is a component known to the firm, describing a baseline value of the fit of a person from population j to the firm k; θ_{jk} represents the unknown part of the fit of population j to firm k; and θ_i represents an unknown and idiosyncratic quality of individual i. We assume that $\theta_{jk} \sim \mathcal{N}(\mu_{jk}, 1)$ and $\theta_i \sim \mathcal{N}(0, 1)$.

A.5.1 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 population membership j and decides to interview them if the expected value yielded by the interview (and hiring decisions that follow it) exceeds some fixed interviewing cost $\kappa > 0$. We denote the expected value of conducting the interview by V_j, an endogenous object that we will describe further below, which depends on features of the learning environment, such as the expected fit of population j to firm k, μ_{jk}. Firm k chooses to interview the candidate from population j if $V_j \geqslant \kappa$.

A.5.2 Hiring Decision

If the firm decides to interview worker i, they see a signal of i's productivity, given by

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

where $\epsilon \sim \mathcal{N}(0, \sigma^2)$. After seeing signal $\tilde{y}$, the firm forms a belief about y_{ijk}, with

$$\hat{y}_{ijk} = \mathbb{E}\left[y_{ijk}|\tilde{y}\right] = \frac{2}{2+\sigma^2}\tilde{y} + \frac{\sigma^2}{2+\sigma^2}(\bar{y}_{jk}+\mu_{jk}) = (\bar{y}_{jk}+\mu_{jk}) + \frac{2}{2+\sigma^2}\left[\tilde{y} - (\bar{y}_{jk}+\mu_{jk})\right]. \quad (6)$$

The firm's payoff from hiring the worker is $\hat{y}_{ijk} - w$, where w represents the worker's wages or other hiring costs. We posit that the worker's wage (if hired) is proportional to their expected productivity at the firm, so that $w = \alpha\hat{y}_{ijk}$, for some fixed $\alpha \in (0, 1)$. Considering the government's policy that pays a proportion $\beta \in (0, 1)$ of the worker's salary, the firm's surplus from hiring the worker is $s = [(1-\alpha) + \beta\alpha]\,\hat{y}_{ijk}$. The firm hires the worker if this value is larger than 0.

Considering this hiring policy, we can write the value of interviewing a worker from population j as

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

where the expectation is taken with respect to the distribution of $\hat{y}_{ijk}$ in population j, given μ_{jk} and σ^2. Note that the policy does not affect the firm's decision to hire — since the firm always hires workers with $\hat{y}_{ijk} \geqslant 0$ — but affects the value of V_j, and incentivizes firms to interview workers from population j.

A.5.3 Learning

Apart from estimating the worker's productivity, the firm also learns about θ_{jk} from the observation $\tilde{y}$. This learning is important in our context as it connects hiring decisions in the initial period to those in the later period. Specifically, we have

$$\hat{\mu}_{jk} = \mathbb{E}\left[\theta_{jk}|\tilde{y}\right] = \frac{1}{2+\sigma^2}(\tilde{y} - \bar{y}_{jk}) + \frac{1+\sigma^2}{2+\sigma^2}\mu_{jk}. \quad (7)$$

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

A.5.4 Second-Period Hiring

Going into the second period, let's classify firms in terms of what they have learned about their fit to population j, θ_{jk}. First, a firm may have had *no learning*, which happens if the firm chose not to interview. In that case, the second-period hiring is exactly the same as first-period hiring, modulo changes in policy that might affect the expected value V_j of interviewing (for example, a change in the subsidy).

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

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

where $\hat{\sigma} < 1$ (we can calculate the closed form of $\hat{\sigma}$ from the appropriate Bayesian updating). This change in the distribution of θ_{jk} impacts firms' decisions to interview and hire in the second period. In particular, firms with higher realizations of $\tilde{y}$ in the first period should interview more, and hire more, in the second period, relative to firms who saw lower realizations of $\tilde{y}$ in the first period.

A.6 Empirical Implications

A.6.1 Long-term effects of the policy

Our first use of this framework is to understand one possible long-term effect of the subsidy policy, that it can resolve a *learning trap* that permanently prevents firms from hiring individuals from a given population — in the case of our application, women who have been out of the labor market. We understand the effect of the policy on unlocking the learning trap in three stages:

Stage 1. In "period 0," before a subsidy policy is instituted, we suppose the firm's belief is that

$\theta_{jk} \sim \mathcal{N}(\mu_{jk}, 1)$, where μ_{jk} is small enough that

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

(To see that such a small μ_{jk} exists, remember that the distribution of $\hat{y}_{ijk}$ is increasing in μ_{jk}, and V_j is an increasing function of $\hat{y}_{ijk}$.) Because the value V_j of interviewing a candidate from population j is so small, such candidates are never interviewed. Consequently, no learning occurs, and, in the absence of any policy, candidates from population j are never hired (even in later periods).

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

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

With the introduction of this policy, interviewing a candidate from population j becomes profitable to the firm. In that period, the firm interviews a candidate, and learns more about the distribution of θ_{jk}, as described in the learning environment above.

Stage 3. In "period 3," the subsidy policy is removed, so that $\beta = 0$. However, firms' beliefs about θ_{jk} have been updated due to their signal observations during interviews in the previous period. Some of these firms received signals that were "bad surprises," leading them to continue not hiring (or interviewing) agents from population j, in the absence of the subsidy policy. Some firms, however, observe "sufficiently good surprises," which lead to $\hat{\mu}_{jk}$ that is large enough, so that they interview (and potentially hire) candidates from population j in period 3. On average, when starting from the "learning trap" environment in stage 0, the policy has the long-term effect of increasing hiring of agents in population j, even after it has been retracted.

These dynamics and long-term effects of the policy are consistent with our observation in section 5, that firms that are exposed to the subsidy policy see long-term changes in their decisions to

hire from the population of women who have been out of the labor market.

A.6.2 "Good draws" versus "bad draws" in firm learning

An immediate implication of equation (7), which describes the learning that ensues after the firm chooses to interview someone in population j, is that firms who observed more positive "surprises" $\tilde{y} - \bar{y}_{jk}$ also formed higher beliefs $\hat{\mu}_j k$, and are consequently more likely to interview (and hire) workers from population j in the next round of hiring.

This empirical prediction of the learning model is exactly what we test in section 6, where we define "good draws" as being agents who yielded high surprises to the firm — note that the value of the surprise term $\tilde{y} - \bar{y}_{jk}$ is positively related with the estimate $\hat{y}_{ijk}$, which determines the wage proposed by the firm. In our empirical analysis, we measure this surprise term therefore via the Mincerian wage residual for the worker that was encountered by the firm. In turn, we define as "bad draws" the agents who yielded low "surprises" to the firm. Indeed, our analysis shows that future hiring outcomes (from the relevant population of women who are out of the labor force), are more positive for firms who encountered "good draws" in the first hiring period, compared to those who encountered "bad draws."

Our model permits more detailed observations regarding the comparison between first period and second period, which we do not validate empirically at this stage. The first observation regards updating for firms who did not hire in the first period; the second observation speaks of firms who interviewed, saw "very good" signal realizations, and decided to hire; the third and fourth observations speak of firms who marginally decided to hire, those that interviewed and saw realizations that are "just good enough" to justify a hiring decision.

Observation 1. *Conditional on interviewing and not hiring, firms are less optimistic about θ_{jk} in the second period, relative to the first period:*

$$\mathbb{E}\left[(\tilde{y} - \bar{y}_j k) - \mu_{jk} | \hat{y}_{ijk} < 0\right] < 0.$$

Observation 2. *There exists a threshold ψ such that if a firm observes a signal $\tilde{y} \geqslant \psi$ in an*

interview, they hire and are more optimistic in the first period, relative to the second period:

$$\tilde{y} \geqslant \psi \Rightarrow \hat{y}_{ijk} \geqslant 0 \text{ and } \hat{\mu}_{jk} \geqslant \mu_{jk}.$$

Observation 3. *Suppose* $0 > \bar{y}_{jk} + \mu_{jk}$. *Then every firm who hires a worker is more optimistic in the second period, relative to the first period:*

$$\hat{y}_{ijk} = t \Rightarrow \tilde{y} - (\bar{y}_{jk} + \mu_{jk}) > 0 \Rightarrow \hat{\mu}_{jk} > \mu_{jk}.$$

This assumption about $\bar{y}_{jk} + \mu_{jk}$ *also implies that the firm hires an agent from population* j *with probability less than* $1/2$.

Observation 4. *Suppose* $0 < \bar{y}_{jk} + \mu_{jk}$. *Then the marginal firm who chooses to hire a worker is less optimistic in the second period, relative to the first period:*

$$\hat{y}_{ijk} = 0 \Rightarrow \tilde{y} - (\bar{y}_{jk} + \mu_{jk}) < 0 \Rightarrow \hat{\mu}_{jk} < \mu_{jk}.$$

This assumption about $\bar{y}_{jk} + \mu_{jk}$ *also implies that the firm hires an agent from population* j *with probability more than* $1/2$.