Gender Hiring Quotas*

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(JOB MARKET PAPER)

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Abstract

Gender quotas have mostly targeted corporate boards and done little for the gender employment gap. A quota on the stock of employment would force firms to separate incumbent workers or hire beyond their needs. A quota on new hires avoids these costs, but firms can comply and then undo its effect through separations. We study a unique policy: Italian firms that win public contracts funded by the EU recovery plan must reserve for women at least 30 percent of the new hires needed for the project. Coverage depends on how contracts are funded, so similar contracts are awarded with and without the quota. Linking procurement records to employer-employee data and balance sheets, we compare winners of contracts with and without the quota in a matched difference-in-differences design. The quota raises female hires by 26 percent without changing whether firms hire. Female employment rises by 8.6 percent relative to comparable winners without the quota and keeps growing over time. Male and total employment do not change significantly, so the gender composition of the workforce shifts toward women. Female separations do not rise, and the women hired in the award year are still employed two years later. The new jobs are mostly white-collar but temporary and part-time, with no effect on wages. We find no detectable costs for firms. Hiring quotas can bring women into male-dominated firms, and firms keep the women they hire.

Keywords: Gender hiring quotas; Public procurement; Affirmative action; Labor demand; Female employment; Matched employer-employee data.

JEL Codes: H57, J16, J23, J63, J78.

*We are deeply grateful to Matteo Paradisi, Barbara Biasi, Stefano Gagliarducci, and Luigi Guiso for their guidance and support. We thank Emilio Calvano, Gaia Dossi, Matteo Escudé, Elisa Facchetti, Simon Jäger, Salvatore Lattanzio, Francesco Lippi, Claudio Michelacci, João Monteiro, Matthew Notowidigdo, Luigi Paciello, Facundo Piguillem, Kirill Ponomarev, Mounu Prem, Nina Roussille, Ayşegül Şahin, Silvia Sarpietro, Massimo Sbracia, Fabiano Schivardi, Vincenzo Scrutinio, Tommaso Sonno, Daniele Terlizzese, Giulia Tura, and Luana Zaccaria for helpful comments, and Carolina Bussotti for her collaboration. This project was carried out within the VisitPNRR program, run jointly by INPS and the Italian Ministry of Economy and Finance. The views expressed are those of the authors only. All errors are our own.

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1 Introduction

Despite decades of government efforts to close them, gender gaps persist both in employment and in top positions (Olivetti and Petrongolo, 2016; Blau and Kahn, 2017). In OECD countries, 72.5 percent of women are employed, against 87.7 percent of men.[1] Quotas have become one of the most widespread tools to close these gaps. Since Norway's law in 2003, 28 countries have adopted binding quotas for corporate boards.[2] These quotas have raised women's presence on boards, but they reach a few, highly selected women. They can narrow the employment gap only indirectly, through spillovers to women below the top, and studies find little such effect (Bertrand et al., 2019; Maida and Weber, 2022).

A separate policy would be a broader quota that applies to the whole workforce. Such a quota can regulate either the stock of workers or the flow of new hires. Regulating the stock is difficult: a firm below the target would have to separate incumbent workers or hire beyond its optimal size, and forcing either can be legally controversial. A quota on new hires avoids both problems. It leaves existing matches intact and moves the firm toward the target as it hires new workers. Its effect on employment, however, is not obvious. Firms can comply and then undo the quota through separations, or place the new hires in marginal jobs. Whether a hiring quota works therefore depends on three questions. First, do more female hires (the flow) translate into more female employment (the stock)? Second, in which jobs do firms place the women they hire? Finally, do quotas impose an excessive burden on firms?

We know little about the answers, for two reasons. First, governments find it hard to impose quotas on private employers, whose hiring they do not control. Second, where binding hiring targets exist, evaluating them requires data on every hire and separation in a firm, and a comparison group of similar firms not subject to the quota.[3] For these reasons, the evidence on gender quotas comes almost entirely from corporate boards and elected offices (Beaman et al., 2009; Baltrunaite et al., 2014; Bertrand et al., 2019; Maida and Weber, 2022).

We overcome these obstacles using a temporary gender hiring quota in Italy, which has one of the largest gender employment gaps in the EU.[4] Since 2021, firms that win public

[1] Employment rates refer to the population aged 25-54 in 2024 (OECD, 2024).

[2] We count only binding quotas, with a numerical target and sanctions, and exclude soft rules such as comply-or-explain requirements, governance-code recommendations, and non-binding affirmative-action goals. After Norway: Iceland, Italy, France, Belgium, India, Germany, Portugal, Austria, South Korea, Greece, Argentina, the Netherlands, the United Arab Emirates, Malaysia, Ecuador, Denmark, Spain, Finland, Croatia, Lithuania, Malta, Slovakia, Slovenia, Estonia, Ireland, Romania, and the United States (California, 2018). Directive (EU) 2022/2381 extends board quotas to large listed firms across the EU.

[3] Existing affirmative-action programs in employment either set non-binding hiring goals (Leonard, 1984; Holzer and Neumark, 2000; Miller, 2017; Amano-Patino et al., 2021) or, when binding, target narrow groups of workers, such as persons with disabilities (Szerman, 2022).

[4] In 2024, the employment rate of women aged 25-54 was 19.2 percentage points below that of men in

contracts funded by the National Recovery and Resilience Plan (PNRR)[5] must reserve for women at least 30 percent of the new hires needed for the project. The government thus imposes the quota on private firms using its purchasing power as a buyer rather than its authority as a regulator. Linking procurement records to employer-employee data, we compare winners of similar contracts with and without the quota. Our results deliver three main takeaways. First, the quota shifts the gender composition of the workforce toward women, and the shift persists. Second, the new jobs are mostly temporary, part-time, white-collar positions. Third, the quota imposes no detectable burden on firms.

Three features of the quota matter for our analysis.[6] First, firms know whether a contract carries the quota before deciding to bid, because contracting authorities specify it in the call for tenders.[7] Section 4 discusses how we address the resulting selection. Second, contracting authorities can waive the quota only with a formal justification, for instance because the contract is small or requires fewer than three hires. In practice, the quota applies in full to 30 percent of PNRR contracts, which account for 47 percent of their value, and is waived mostly in small contracts.[8] Third, after the award the quota binds only if the firm hires at least three workers for the project. It leaves the firm free to choose how many workers to hire and in which occupational categories, hours, and contract types. Firms can therefore comply by hiring more women or fewer men, or avoid the quota by staying below the three-hire threshold.

To our knowledge, this policy is unique: it is the first binding gender quota on the hiring of private firms that applies to all jobs and all levels of the workforce, while leaving firms full discretion over how to comply. It offers an ideal setting to answer our three questions. First, the quota binds. In the year before the award, the median firm in our matched sample employs 12 workers, one of whom is a woman, and hires three new workers, none of them women. Women account for only 9.8 percent of hires among all treated firms and 8.8 percent among matched treated firms, less than a third of the share the quota requires, and for 46 percent of firms complying would mean hiring a woman for the first time. Second, firms have strong incentives to comply, because the contract is worth on average 31.8 percent of their

Italy, twice the EU average of 9.6 points (Eurostat, 2024b).

[5]The PNRR is Italy's component of Next Generation EU, the EU's post-COVID-19 recovery program, running from 2021 to 2026. PNRR contracts accounted for 8.1 percent of the value of all public procurement in 2021-2025, before our sample restrictions, peaking at about 18 percent in 2023.

[6]The quota is set by Article 47 of Decree-Law 77/2021, which also sets an independent 30 percent quota on hires of workers under 36.

[7]Contracting authorities are the public entities that buy works, goods, and services, from ministries and regions to municipalities, health authorities, and publicly owned companies. They design the call for tenders, choose the award procedure, and select the winning firm.

[8]The quota is reduced in 2.1 percent of PNRR contracts and waived in 55.4 percent. The remaining 12.6 percent are unclassified. Section 2 describes the waiver rules.

annual sales.[9] Third, the quota lasts only as long as the contract, so we can test whether its effects persist once it expires. The setting is also a demanding test. Nearly three quarters of the firms operate in construction, where only 8.5 percent of workers are women[10] and physical strength may give men a productivity advantage (Galor and Weil, 1996; Pitt et al., 2012). If the quota works here, it is likely to work where women are easier to hire.

We build a new dataset that links employer-employee records to the universe of Italian public procurement contracts and to firm balance sheets. Because coverage depends mainly on how a contract is funded, not on what it buys, we can compare firms awarded contracts with the quota to firms awarded comparable contracts without it, using a matched difference-in-differences design. Both groups win a public contract, so the comparison isolates the effect of the quota from that of winning. We match contracts on type of work, procedure, timing, and value, and firms on sector, size, and pre-award female employment share.[11] The key assumption is that, without the quota, two similar firms winning similar contracts would have hired and retained women in the same way.

An important concern is that firms know before bidding whether a contract carries the quota, so firms reluctant to hire women may avoid covered contracts. Three pieces of evidence suggest that selection is limited. First, we do not observe pre-trends in hiring or employment between treated and control firms. Second, we match firms on their pre-award female employment share. Third, covered contracts are large relative to the sales of the firms that win them, so a firm is unlikely to give one up to avoid hiring a woman. We cannot rule out selection entirely. We have recently obtained data on all bidders, which will allow us to study selection directly. Section 4 discusses selection in detail.

Relative to matched control firms, female hires rise by 26 percent of their baseline average in the two years after the award. The quota does not change whether firms hire. In the award year, some firms that would have hired only men hire no one, while the additional women are hired by firms that also hire men. Small firms hire a woman instead of a man among the few workers they hire, while large firms hire women on top of men. Our estimates capture the effect of exposure to the quota, an intention-to-treat effect, rather than the effect of compliance, which we do not observe directly. They also abstract from general equilibrium effects, such as changes in the hiring of control firms.

More female hires translate into more female employment. After the award, female

[9] Computed for the 87.5 percent of eligible treated firms for which we observe balance sheets. The median is 5.5 percent.

[10] In 2024, women accounted for 8.5 percent of workers aged 25-64 employed in construction in Italy (Eurostat, 2024a).

[11] The unit of analysis is the firm-contract, and treatment is a firm's first award of a contract with the quota. Before the award, treated and control firms employ on average 20.7 and 20.9 workers, and their contracts have nearly identical values.

employment in treated firms rises by 8.6 percent of its average baseline level relative to control firms, about 0.2 additional women per firm, and the effect grows over time. Male employment does not change. Total employment rises, but not significantly, so the composition of the workforce shifts toward women. Across the 685 treated firms, the quota adds about 210 female hires over three years: a small number in absolute terms, but about one third more female hires per year than before.

Separations determine how much of the flow becomes stock. Female separations fall in the award year and do not rise afterwards, and the women hired in the award year are still employed by the firm two years later. This runs against the concern that firms comply and then undo the quota by separating the women they hire. Evidence from contracts that last less than a year points in the same direction, although it is imprecise.

The new jobs are mostly temporary white-collar positions. Only 25 percent of the women hired come from other private firms; the rest had no private-sector employee contract in the previous year. Most are hired into white-collar positions, but 81 percent of the new jobs are temporary and 64 percent part-time. We find no effect on wages.

The quota imposes no detectable costs on firms. Revenues, assets, and profit margins do not change, and labor productivity rises by 6.7 percent. Contracts with the quota are not completed more slowly than comparable PNRR contracts without it.

Two forces shape these responses. The first is the government's leverage as a buyer. Responses are stronger when the contract is large relative to the firm's wage bill, consistent with firms complying because they depend on public demand. The second is hiring frictions. Replacing a woman with a man requires a new hiring cost, plus a firing cost for longer contracts, and she accumulates skills specific to the firm. The quota thus creates a wedge between keeping the new hires and returning to the old workforce composition. Even when the jobs end, they may strengthen women's attachment to the labor market: most of these women did not come from other private firms, and work experience can make it easier to find the next job.

Taken together, our results indicate that a hiring quota attached to public contracts can shift the gender composition of private firms toward women at no detectable cost to them, and that firms keep the women they hire. The type of jobs created limits the gains, because most are temporary.

Contribution to the Literature Our paper contributes to three literatures. First, we add to the literature on gender quotas by moving the analysis from leadership positions to the workforce as a whole. Existing studies examine quotas for elected offices and corporate boards. These quotas can change beliefs about women in leadership (Beaman et al., 2009,

2012) and the quality of those selected (Baltrunaite et al., 2014). Their effects on firm performance are mixed: some studies find costs (Ahern and Dittmar, 2012; Matsa and Miller, 2013), others none (Eckbo et al., 2022; Ferrari et al., 2022). Because they move only a handful of positions, their effects on women's employment below the top are small (Bertrand et al., 2019; Maida and Weber, 2022). A hiring quota works through a different channel: it changes who enters the firm, at every level. We show that this channel raises female employment, mostly through temporary white-collar jobs, that firms keep the women they hire, and that firms bear no detectable costs, in contrast with the mixed evidence on board quotas.

Second, we contribute to the literature on affirmative action in employment. Existing programs fall into two groups. The first sets hiring goals for broad groups, as in the US federal contractor program, which ties affirmative action to public contracts but does not bind (Leonard, 1984; Holzer and Neumark, 2000; Kurtulus, 2016; Miller, 2017; Amano-Patino et al., 2021). The second binds, but applies to the stock of employment and to narrow groups, such as persons with disabilities (Szerman, 2022). We study a quota that is binding, applies to the flow of new hires, and targets half of the population. By following hires, separations, and the women who stay, we show how a flow quota turns into a stock.

Finally, we contribute to the literature on public procurement as a policy tool. Governments often use their purchasing power to favor particular suppliers, and existing work studies how these preferences affect who wins contracts, procurement costs, and competition (Marion, 2007, 2009; Krasnokutskaya and Seim, 2011; Athey et al., 2013; Rosa, 2024), and how winning a contract affects firm growth (Ferraz et al., 2015). Conditions on what winning firms do after the award have received less attention, and the main example, the US federal contractor program, sets non-binding goals. We show that a binding condition attached to public contracts can change the hiring of private employers at no detectable cost to firms, and that its effect grows with the firm's reliance on public demand.

The remainder of the paper is organized as follows. Section 2 describes the institutional setting, Section 3 the data, and Section 4 the empirical strategy. Section 5 presents the results: the effect on female hiring, the extensive, intensive, and compositional margins of the response, and firm performance. Section 6 studies heterogeneity and external validity. Section 7 concludes.

2 Public Procurement and the Gender Hiring Quota

Public procurement is one of the largest channels through which governments buy from private firms. It accounts for 11.7 percent of GDP in Italy, in line with the OECD average

of 12.9 percent (OECD, 2026). In 2025, procurement procedures were issued in Italy for a total value of €309.7 billion (ANAC, 2026).[12]

The Italian procurement system is highly decentralized. The central government allocates public funds to projects that require works, services, and supplies through contracting authorities. They are public entities that include ministries, regions, municipalities, health agencies, and public companies, and are in charge of issuing and managing calls for tenders. Each authority defines the contract object, chooses the award procedure, sets the reserve price, and selects the winner.[13]

Winning firms bear all contract obligations for as long as the contract is in force.[14] Firms that breach these obligations face penalties and, in serious cases, termination of the contract. Serious breaches are recorded by the National Anti-Corruption Authority (ANAC) and can exclude firms from future calls. For firms that depend on public demand, a breach can therefore be seriously costly beyond the single contract, because it puts future contracts at risk.

Winning a contract can create a labor demand shock. A firm can meet the additional demand in two ways. It can reallocate its existing employees to the new project, drawing on the stock of workers it already employs. Or it can hire new workers, increasing the flow of hires. This choice matters for our analysis because the gender quota we study applies only to new hires.

The PNRR is Italy's share of NextGenerationEU, the EU's recovery program after the COVID-19 pandemic. Italy is the largest recipient of these funds in the EU: between 2021 and 2026, the plan channels €194.4 billion into the Italian economy, on average about €32 billion per year, or 1.5 percent of annual GDP.[15] About two thirds of the plan finances new projects and investments that come on top of ordinary public spending.[16] Much of this money reaches firms through public procurement, where PNRR contracts accounted for up to 18 percent of the total value, in the peak year 2023.

European Union rules allow governments to use public procurement to pursue social objectives. Contracting authorities can attach social and employment-related conditions to the performance of a contract, provided that these conditions are linked to the subject matter

[12]This amount refers to procedures issued, not awarded, so it does not measure the money that actually flowed into the economy in 2025.

[13]Legislative Decree 50/2016, replaced by Legislative Decree 36/2023 from July 2023.

[14]Firms can bid alone or as a temporary association of firms (*associazione temporanea di imprese*), which is treated as a single firm. In these cases, we keep only the lead firm (Section 3.1).

[15]Computed as €194.4 billion over six years, relative to average Italian GDP in 2023-2025 (ISTAT, 2026).

[16]Of the €191.5 billion in the original plan, €124.5 billion finance additional spending. The remaining €67 billion finance projects that were already planned and funded from national sources (Camera dei Deputati, 2022).

of the contract and announced in the call for tenders.[17] Italy used this room to attach a gender hiring quota to the contracts funded by its recovery plan. The Italian government later decided to extend the quota to all public contracts for works and services, starting in January 2025.[18] To our knowledge, no other country attaches a binding gender hiring quota to its public contracts.[19]

Article 47 of Decree-Law 77/2021 requires firms winning a covered contract to reserve for women at least 30 percent of the new hires needed to execute it.[20] A separate 30 percent requirement applies to workers younger than 36. The two requirements are independent and not mutually exclusive: for instance, a woman younger than 36 counts toward both, while a woman aged 36 or older counts only toward the gender requirement, and the firm must still meet the youth requirement. In this paper, we focus on the gender requirement.

The quota targets the flow of new hires, not the existing workforce. It does not prescribe occupational categories, wages, hours, or contract types. Firms therefore keep full discretion over how to hire and where to place the new workers, which allows us to study their response along the extensive, intensive, and compositional margins. Like all contractual obligations, the quota applies only for the duration of the contract. This allows us to test whether the shift in the gender composition of the workforce persists once the quota expires, within the few years after the award that our data cover.

Contracting authorities can waive or reduce the quota, but they are not required to do so, and any deviation requires an adequate and specific justification. Examples of full waivers include small direct awards, projects expected to require fewer than three hires, specialized qualifications, and emergencies. Waivers are in general hard to justify in labor-intensive service contracts, where labor costs account for at least half of the contract value.[21] A partial reduction is allowed where sectoral female employment is especially low.[22] In practice, waivers are not rare: across all PNRR contracts awarded in 2021-2024 in our data,

[17] Article 70 of Directive 2014/24/EU.

[18] Legislative Decree 209/2024, in force since December 31, 2024, amended Article 57 and Annex II.3 of the Public Contracts Code (Legislative Decree 36/2023). The extension excludes supplies and intellectual services.

[19] The closest example is Brazil, where federal contracts for continuous services with at least 25 workers must reserve 8 percent of positions for women who are victims of domestic violence (Decree 11,430/2023, implementing Law 14,133/2021). The Brazilian quota targets a selected group of women and sets a much lower target.

[20] Decree-Law 77/2021, converted into Law 108/2021. Implementing guidelines were adopted on December 7, 2021.

[21] The guidelines allow a waiver in these contracts when social clauses require the winning firm to rehire the workers of the previous contractor, to protect their employment stability.

[22] In two-digit sectors where women account for less than 25 percent of employment, the contracting authority can lower the quota to the sector's female employment share plus 5 percentage points (Guidelines adopted by Decree of the President of the Council of Ministers of December 7, 2021, Section 6).

29.9 percent carry the full quota, 2.1 percent a reduced quota, 55.4 percent a waiver, and 12.6 percent remain unclassified. However, waivers are concentrated in small contracts and account for only 18.0 percent of the value of PNRR contracts, while contracts with the full quota account for 46.7 percent.

The quota comes with possible penalties and reporting duties. Firms with 15 to 50 employees must report the gender composition of their workforce within six months. Failure to report excludes them from subsequent PNRR tenders for twelve months. Enforcement is nevertheless uncertain. We observe exposure to the quota, not compliance, and estimate the policy's effect as monitored and enforced in practice.

3 Data: Linking Contracts to Workers

Our data link Italian public contracts awarded from 2017 to 2025 to the full workforce of each winning firm. We combine four sources: administrative records on public contracts, information on which contracts carry the gender quota, matched employer-employee records, and firm balance sheets.

Firm-Level Administrative Data Matched employer-employee records cover every private-sector employment contract in Italy from 2017 to 2025. They come from the monthly contribution declarations that employers file with the Italian National Social Security Institute. For each job, we observe wages, weeks worked, full-time or part-time status, temporary or permanent contract, occupational category (such as blue-collar or white-collar), and the worker's age and gender. We construct employment, hires, separations, and their gender composition at the firm-year level. A hire is a worker employed by the firm in a year but not in the previous year. A hire comes from non-employment if the worker held no private-sector job in the previous year. This group may include unemployed, inactive, self-employed, or public-sector workers. The data do not record the contract on which a worker is employed. We observe all hires of a winning firm, but we cannot disentangle which of them work on each specific public contract.

Balance-sheet data from Orbis report sales, input costs, value added, profits, and total assets.

Public Procurement Data Procurement records cover the universe of public contracts awarded in Italy from 2017 to 2024. For each contract, they report the object, the prod-

uct classification (Common Procurement Vocabulary code, CPV),[23] the award procedure, the location, the value of the contract, the contracting authority, the funding source, and the winning firms. They are collected by the Italian National Anti-Corruption Authority (ANAC). These records identify only the winning firms, not all bidders. We have recently collected data on all bidders for the calls managed by two central purchasing bodies, Invitalia and Consip, which our next analyses will use.

Information on the gender quota comes from the monitoring system of PNRR projects, managed by the State General Accounting Department of the Ministry of Economy and Finance. For each PNRR call issued from 2021 to 2024, it reports whether the call applies the full quota, a reduced quota, or a waiver. We link the two sources through the tender unique identifier.

3.1 Sample and Descriptive Statistics

Sample Restrictions We focus on calls published between 2021 and 2024 and awarded through competitive procedures or direct awards, with a value of at least €15,000 and below €10 million. We keep only contracts whose winning firm we can identify. These contracts form our procurement sample, with 882,822 contracts. Among winning firms, we keep private firms with at least one baseline employee, and only the lead firm of contracts won by a temporary association of firms. We date each contract by its award date, or by the publication date of the call when the award date is missing or invalid. The unit is the firm-contract. Our firm-contract sample contains 214,073 firm-contracts won by 45,593 firms.

Our treated sample is a subset of all contracts with the quota. Between 2021 and 2024, 78,196 PNRR contracts carry the full quota. Of these, 20,367 satisfy our contract restrictions, and 9,558 firms appear among their winners. For each firm, we keep only its first full-quota contract, and we drop it if the firm fails the firm restrictions. This leaves 4,112 eligible treated firm-contracts. We call them eligible because they can enter the matching described in Section 4. The other 209,961 firm-contracts in this sample are potential controls. They are ordinary contracts and PNRR contracts with a complete waiver, won by firms that never win a full-quota contract. Table A1 in Appendix A reports each step. The quota became effective in 2022, so the first cohort of treated contracts is that of 2022. Because our employment data end in 2025, we observe firms for at most three years after the award in the 2022 cohort,

[23]The CPV is the European Union's standard classification of the works, goods, and services bought through public procurement. Its eight-digit codes are hierarchical: the first two digits identify broad divisions, and each further digit narrows the category down to individual products or works. For example, code 45000000 identifies construction work and code 45233140 identifies roadworks.

and for fewer years in later cohorts.

Descriptive Statistics Table 1 describes all contracts in our procurement sample. PNRR contracts represent 6.8 percent of these contracts and 7.3 percent of their total value. The median PNRR contract is worth €61,537, compared with €72,484 for an ordinary contract. Works account for 34.8 percent of PNRR contracts and 19.7 percent of ordinary contracts.

PNRR contracts are slightly shorter among completed contracts with observed duration. Their median duration is 4.6 months, compared with 5.5 months for ordinary contracts. Moreover, 84.5 percent last less than one year, compared with 78.9 percent of ordinary contracts. Duration is observed for 20.6 percent of PNRR contracts and 16.8 percent of ordinary contracts. Because it is observed only for completed contracts, observed duration understates true duration. This applies in particular to PNRR contracts, which are scheduled to run until 2026.

Table 1: Public Contracts and the Gender Hiring Quota

			PNRR, by quota status		
	Ordinary	PNRR	Full quota	Reduced quota	Waiver
Number of contracts	823,096	59,726	20,367	2,381	25,261
Share of contracts (%)	93.2	6.8	2.3	0.3	2.9
Relative share of contracts (%)			42.4	5.0	52.6
Total value (€ billion)	287.4	22.7	10.6	4.0	5.1
Share of total value (%)	92.7	7.3	3.4	1.3	1.6
Relative share of total value (%)			53.9	20.4	25.7
Median value (€)	72,484	61,537	95,000	1,029,000	47,780
Works (%)	19.7	34.8	31.1	92.4	42.2
Duration observed (%)	16.8	20.6	—	—	—
Median observed duration (months)	5.5	4.6	—	—	—
Observed duration below one year (%)	78.9	84.5	—	—	—

Notes: The table describes the procurement sample: public contracts with calls published between 2021 and 2024, awarded through competitive procedures or direct awards, with a value from €15,000 to below €10 million and an identified winning firm. No restriction on winning firms is applied, and the unit is the contract. Ordinary contracts are not funded by the PNRR. Among PNRR contracts, the full quota requires that at least 30 percent of new hires be women, a reduced quota sets a lower target, and a waiver exempts the contract. Shares of contracts and of value are computed over all contracts in the sample. Relative shares are computed over PNRR contracts with a classified quota status, and the three quota columns exclude 11,717 PNRR contracts with unclassified status. Works are contracts for construction or maintenance work, as opposed to services or supplies. Duration is the time between the start and the end of the contract and is observed only for completed contracts.

In the procurement sample, the full quota applies to 42.4 percent of PNRR contracts with a classified quota status and to 53.9 percent of their value. Compared with all PNRR contracts in Section 2, the procurement sample contains more full-quota contracts and fewer

waivers.[24] This suggests that our restrictions mainly exclude waived contracts of small value. In terms of value, the share of the full quota barely changes, at 46.9 against 46.7 percent. Full-quota contracts are also larger, with a median value of €95,000 against €47,780 for waived contracts.

Firms that win public contracts are larger than the average private firm and employ fewer women. Table 2 describes the firm-contract sample by type of contract won and compares it with all private employers.[25] We measure all characteristics in the year before the award, which we use as the baseline year. The median eligible treated firm employs 18 workers, against 22 for winners of ordinary contracts, 16 for winners of waived PNRR contracts, and 3 for the average private employer. Women account for 12.3 percent of employment in eligible treated firms and 9.8 percent of their hires, against 40.1 percent of both in the private sector. Winners of ordinary contracts employ more women, with 16.7 percent of employment and 18.3 percent of hires.

Hiring is common among winning firms. In the baseline year, 91.8 percent of eligible treated firms hire at least one worker and 68.8 percent hire at least three. These shares are similar among winners of contracts without the quota. The median treated firm hires five workers but no women. Female hires are less often temporary than male hires, but more often part-time and white-collar.

Eligible treated firms are frequent procurement participants. Between 2017 and 2020, the median treated firm won six public contracts, and 39.5 percent won at least one contract each year. The same firms often win contracts both with and without the quota. Among firms that win at least one PNRR contract with the full quota or with a waiver between 2021 and 2024, 18.9 percent win both.

Quota contracts are large relative to the firms that win them. For the median eligible treated firm, the first full-quota contract is worth 5.5 percent of baseline sales and 0.47 times the baseline wage bill. The averages are much higher, at 31.8 percent of sales and 2.5 times the wage bill, because a few contracts are very large relative to the firm. For the median potential control, the first contract is worth 3.4 percent of sales and 0.27 times the wage bill. Sales are observed for 87.5 percent of eligible treated firms.

Exposure of Treated Firms to the Quota One might expect the quota to bind mainly in construction, where many jobs are physically demanding and men may have a productivity

[24] Counting unclassified contracts in the denominator, as in Section 2, the full quota covers 34.1 percent of PNRR contracts, against 29.9 percent, and waivers 42.3 percent, against 55.4 percent.

[25] The private-sector benchmark includes every private employer with at least one employee in the employer-employee data. It pools the years 2020 to 2023, the baseline years of the 2021 to 2024 cohorts, and applies no minimum firm size. A hire is a worker who appears in a firm in a given year but was not employed there in the previous year. Female shares are ratios of totals across all firm-years.

Table 2: Baseline Characteristics of Winning Firms and Private Employers

	Potential controls		Eligible treated	
	Ordinary	PNRR waiver	PNRR full quota	Private sector
Firm-contracts	203,317	6,644	4,112	—
Firms	40,752	3,954	4,112	—
Median employment	22	16	18	3
Median hires	5	4	5	—
Median female hires	0	0	0	—
Any hire (%)	91.1	90.8	91.8	—
At least three hires (%)	69.2	63.7	68.8	—
Female employment share (%)	16.7	13.7	12.3	40.1
Female share of hires (%)	18.3	11.7	9.8	40.1
Female temporary hires (%)	38.8	30.1	35.7	—
Male temporary hires (%)	45.5	46.3	58.1	—
Female part-time hires (%)	18.0	13.5	21.8	—
Male part-time hires (%)	7.9	4.2	4.9	—
Female white-collar hires (%)	64.5	90.5	89.4	—
Male white-collar hires (%)	29.8	34.3	24.1	—

Notes: The table describes the firm-contract sample in the year before the award, the baseline year, and compares it with all private employers. Columns 1 and 2 cover potential controls: firm-contracts for ordinary contracts and for PNRR contracts with a complete waiver, won by firms that never win a contract with the full quota. A firm can appear in both columns. Column 3 covers eligible treated firm-contracts, each firm's first PNRR contract with the full quota. The unit is the firm-contract, so a firm that wins several contracts enters once per contract. Column 4 covers all private employers in the employer-employee data, pooling the years 2020 to 2023, with no minimum size. A hire is a worker employed by the firm in a year but not in the previous year. Shares of employment and hires are ratios of totals across firm-contracts in columns 1 to 3 and across firm-years in column 4. Temporary, part-time, and white-collar shares are computed separately over female and male hires.

advantage. Among eligible treated firms, however, the quota binds in every sector. Figure 1 breaks down the 4,112 eligible treated firm-contracts by sector.[26] For each sector, it reports the mean female share and its interquartile range, from the 25th to the 75th percentile across firms. Construction accounts for 59.5 percent of eligible treated firm-contracts, and for 70.9 percent when we include multi-sector firms with construction among their sections. In construction, women make up about 8 percent of employees in the average firm. Other sectors employ more women, but not enough to meet the quota. Even in trade and ICT, the sectors with the most women, the mean female share of employment is below 20 percent, and in every sector the 75th percentile is below 30 percent.

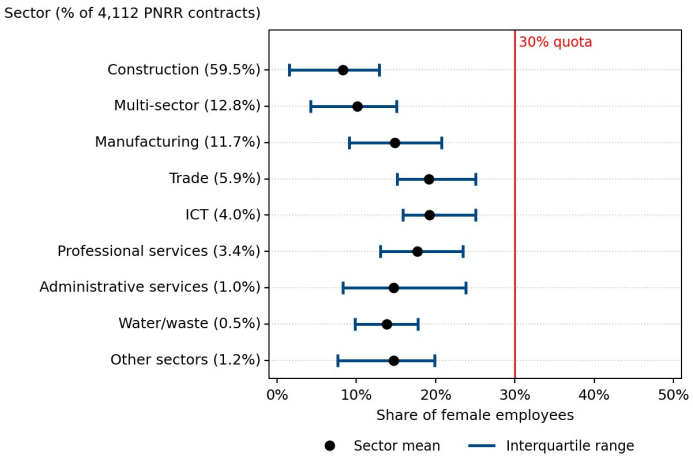


Figure 1: Exposure to the Quota by Sector

Notes: The figure reports, for the 4,112 eligible treated firm-contracts, the sector mean and interquartile range of the female share of employment in the baseline year. The share of each sector in eligible treated firm-contracts is in parentheses. The vertical line marks the 30 percent quota.

4 Empirical Strategy

Identification and Selection The main empirical challenge is selection into contracts with the quota. Contracting authorities decide quota status before publishing the call, and firms observe it before bidding. Firms that find it easier to hire women may bid more often for calls with the quota, and firms that find it harder may avoid them. A simple comparison of firms with and without the quota would therefore mix the effect of the quota with differences

[26] We define sectors at the one-letter level of the Italian classification of economic activities (ATECO 2007), which corresponds to the sections of the European NACE Rev. 2 classification. The sector comes from the firm registry of the employer-employee data. A firm is multi-sector when the registry records activities in more than one section between 2017 and 2025, either at the same time or over the period.

in firms, sectors, authorities, and projects.

We partially address selection by matching highly comparable firms that win nearly identical contracts, with and without the quota. All firms in the comparison win a public contract and face a similar demand shock. We match them on the contract and on the firm characteristics most likely to drive selection: sector, size, and female employment share in the year before the award. The control firm wins a nearly identical contract at about the same time, so it shows how the treated firm would have hired had its contract carried no quota. Our identifying assumption is that, without the quota, the treated firm would have hired and retained women as its matched control did. Under this assumption, within each matched pair, whether the contract carries the quota is as good as random.

Two pieces of evidence support this assumption. First, treated and control firms follow the same paths before the award: the pre-award event-study coefficients are flat and close to zero for all main outcomes. Second, we view selection as a second-order concern, because quota contracts are large relative to the firms that win them. Among eligible treated firms, the first full-quota contract is worth 31.8 percent of annual sales on average. Giving up such a contract to avoid hiring women would be costly and hardly rational. Consistent with this, 18.9 percent of the firms that win a PNRR contract with the full quota or with a waiver win both. In the matched sample, contracts weigh less, at 6.2 percent of annual sales on average and 2.6 percent at the median. Section 6 therefore shows how the effects vary with the importance of the contract for the firm, separating the firms with the least room to avoid the quota. Selection on unobserved characteristics may still remain. We have recently collected data on all bidders in a subsample of calls, both with and without the quota. Comparing winners with runners-up in both types of calls will separate the effect of the quota from that of winning a contract and will also fully control for selection.

Treatment and Comparison Groups Eligible treated and potential control firm-contracts are defined in Section 3.1. Each treated firm enters the analysis once, with its first contract carrying the full quota. Firms that ever win a contract with the full quota never serve as controls, so a treated firm cannot appear among the controls, either before or after its own award. Each treated firm-contract is compared only with its matched control, and each pair has its own timeline. The award year is event year zero, and the year before the award is the baseline year. For control firm-contracts, event time is defined by their own award date, which falls within six months of the treated award. The two award dates can fall in different calendar years, so event time and calendar time need not coincide within a pair. Every comparison is therefore between a treated firm and a never-treated control that won a contract at about the same time. We estimate the intention-to-treat effect of this first exposure to

the quota. After it, both treated and control firms may win other public contracts without the quota, and treated firms may also win further contracts with the quota. Our estimates include the response to these later contracts.

Matching on Contracts and Firms We match each treated firm-contract to one control firm-contract in two steps. Table A2 in Appendix A summarizes the criteria. First, contracts must have the same object and award procedure, be awarded within six months of each other, and share a CPV code. We search hierarchically, first at four digits, then at three, and then at two, moving to a coarser level only when no control is found.[27] Contract values must differ by less than 10 log points.

Second, following the procedure of Jäger et al. (2024), we match firms on the baseline values of outcomes that we study later, employment and the female employment share. Firms must operate in the same set of one-letter sectors. Log baseline employment must differ by less than 0.30, and the baseline female employment share by less than 10 percentage points. Among candidates, we select the nearest control using the standardized Euclidean distance on these two variables. Finally, we keep only pairs whose contract values differ by at most €1,500. Matching is with replacement. The same control firm-contract can be matched to several treated firm-contracts, and the same firm can enter as a control through several contracts. We account for this repetition in the standard errors.

We do not match on location, although female employment varies widely across Italian regions. We make this choice for three reasons. First, our criteria are already demanding, and adding location would leave very few pairs. Second, it is not clear which location matters. A contract can be carried out far from the firm's headquarters, so the location of the contract and that of the firm need not coincide. Third, matching firms in the same local labor market would make treated and control firms compete for the same pool of women. Treated firms could then hire women who would otherwise work for controls, which would bias our estimates (Section 4.2).

The Matched Sample These criteria are strict, and most treated firm-contracts find no match. The procedure yields 685 matched pairs, or 16.7 percent of eligible treated firm-contracts. We call them the matched sample. We accept a low match rate by design. In exchange, each treated firm is compared with a firm that wins a contract of the same type, in the same period, and of almost the same value. Differences in hiring after the award can then be attributed to the quota rather than to the contract.

[27] In the matched sample, 73 percent of pairs share a four-digit CPV code, 13 percent a three-digit code, and 14 percent a two-digit code.

Among the 685 matched controls, 519 are ordinary contracts and 166 are PNRR contracts with a complete waiver. They correspond to 665 distinct firm-contracts won by 642 firms. Table 3 shows that treated and control firms are nearly identical before the award. The average firm employs about 21 workers, of whom 2.6 are women, and wins a contract worth about €92,000. For most matched firms, the quota binds on hiring. In 610 of the 685 pairs, or 89 percent, women made up less than 30 percent of the treated firm's hires in the three years before the award.

The matched sample is a particular subgroup of treated firms. Compared with all eligible treated firms, matched firms are smaller, with median employment of 12 workers against 18, and they win smaller contracts, with a median value of about €48,000 against €141,000. They are also more concentrated in construction. Single-sector construction firms account for 70 percent of matched treated firms, against 59 percent of all eligible treated firms, and multi-sector firms are rarely matched. The baseline female employment share is about 11 percent in both groups. Matching therefore makes our setting an even more demanding test. In construction, few women work and physical strength may give men a productivity advantage, so hiring women is harder. Our estimates are thus likely a lower bound of the effect of the quota where women are easier to hire. Section 6 shows how the effects vary with firm size and contract importance.

Table 3: Baseline balance between all treated firms, matched treated firms, and matched controls

	All treated		Matched treated		Matched controls	
Variable	Mean	Median	Mean	Median	Mean	Median
Panel A: Variables used in matching						
Female employment share	0.11	0.10	0.11	0.11	0.11	0.11
Total employment (log)	3.10	2.89	2.63	2.48	2.63	2.48
Industry	0.13		0.13		0.13	
Services	0.16		0.13		0.13	
Construction	0.59		0.70		0.70	
Agriculture	0.00		0.00		0.00	
Multisector	0.12		0.04		0.04	
Procurement amount	836,793.47	140,667.52	92,790.81	47,638.07	92,799.21	47,607.00
Panel B: Main baseline outcomes						
Total employment	43.63	18.00	21.39	12.00	21.51	12.00
Female employment	5.39	2.00	2.63	1.00	2.60	1.00
Male employment	38.24	17.00	18.75	11.00	18.91	11.00
Blue-collar employment	25.17	12.00	13.73	9.00	14.32	9.00
White-collar employment	18.46	4.00	7.66	3.00	7.19	2.00
Weeks worked	1,684.72	653.00	839.24	463.00	834.05	468.00
Wage bill	1,207,789.97	367,138.50	556,132.69	251,897.00	526,392.38	252,418.00
White-collar employment share	0.32	0.22	0.27	0.20	0.25	0.18
Female white-collar employment share	0.10	0.09	0.11	0.10	0.10	0.10
Total hires	11.09	5.00	4.95	3.00	5.12	3.00
Female hires	1.08	0.00	0.44	0.00	0.45	0.00
Average weekly wage	576.79	524.18	545.12	508.44	546.84	510.56
Average weekly wage female	598.14	543.95	561.14	528.10	564.49	524.83
Average weekly wage (log)	6.25	6.23	6.21	6.20	6.22	6.20
Gender wage gap (%)	-1.44	-0.59	-1.94	-1.74	-2.79	-1.98
N. observations	4,112		685		685	
N. firm-events	4,112		685		665	
N. unique firms	4,112		685		642	

Notes: Statistics are measured in the baseline year. "All treated" reports eligible treated firm-events before matching; "Matched treated" and "Matched controls" report the matched firm-events used in the estimation. A control firm matched to different procurement events enters once for each event, possibly with different baseline years. Panel A reports the firm-level matching variables and Panel B selected baseline outcomes. The female share of white-collar employment is computed for firms with at least one white-collar employee, and the average female weekly wage for firms with at least one female employee. The gender wage gap is the log difference in firm-level FTE weekly wages between men and women, read as the percentage by which men's wages exceed women's, and is computed for firms with at least one worker of each gender.

4.1 Estimating Equation

We estimate a difference-in-differences model within matched pairs. For firm i, contract c, pair p, and calendar year t:

$$Y_{icpt} = \beta \left(\text{Treated}_{icp} \times \text{Post}_{ict}\right) + \alpha_{ic} + \lambda_{pt} + \varepsilon_{icpt}. \tag{1}$$

Treated_{icp} equals one for the treated firm-contract of pair p. Post_{ict} equals one from the calendar year in which contract c is awarded. The interaction therefore switches on for the treated firm-contract from its award year onward.

Firm-by-contract fixed effects, α_{ic}, absorb time-invariant differences between firm-contracts. Pair-by-year fixed effects, λ_{pt}, absorb any shock that hits both firms of a pair in the same calendar year. We use them instead of pair fixed effects because the two awards in a pair can fall in different calendar years (Section 4). With pair-by-year effects, the treated firm and its control are always compared in the same calendar year, whatever their event time.

The coefficient β measures the average differential change in the outcomes of treated firm-contracts after the award, relative to their matched control firm-contracts. Each treated firm-contract is compared only with its own control, never with other treated firms. The design is therefore a stacked difference-in-differences. It avoids comparisons between early- and late-treated firms, which can bias two-way fixed effects estimates when treatment timing is staggered (Callaway and Sant'Anna, 2021). We cluster standard errors at the firm level, because matching is with replacement and the same firm can serve as a control in several pairs and through several contracts.

We also estimate:

$$Y_{icpt} = \sum_{k \neq -1} \beta_k \left(\text{Treated}_{icp} \times \mathbf{1}[t - E_{ic} = k]\right) + \alpha_{ic} + \lambda_{pt} + \varepsilon_{icpt}, \tag{2}$$

where E_{ic} is the award year of contract c and the year before the award is the reference period. We report coefficients from three years before to two years after the award. Earlier and later years are binned into endpoint coefficients, which we do not report. Pre-award coefficients test for differential trends. They also test for anticipation. Firms learn the quota status when they bid, before the award, yet treated and control firms follow the same paths before the award for all main outcomes.

4.2 Interpreting the Estimates

What the Estimates Measure The coefficient β in equation (1) measures how hiring and employment change when a firm wins its first contract with the full quota, relative to a similar firm that wins a nearly identical contract without the quota. Each β_k in equation (2) measures the same change k years after the award. Three features follow from this definition and apply to both. First, β is net of winning a contract. Both firms receive the same demand shock, so β isolates the quota. Second, β is an intention-to-treat effect of exposure to the quota, including the response to any later contracts. As mentioned in Section 3, we cannot observe in our data which hires work on the contract. We therefore cannot verify whether firms comply with the quota. This has two implications. If some firms do not comply, β understates the effect of a fully enforced quota. And because we measure all hires of the firm, β captures the net effect on its workforce, including any reallocation of women and men between the contract and the firm's other activities. Third, β is an average over matched treated firms, a particular subgroup described in Section 4.

What the Estimates Do Not Capture The estimates do not capture three other effects of the quota. First, they do not capture how the quota changes which firms bid for and win public contracts, since we condition on winning. Second, they do not capture general equilibrium effects. If the quota raises the demand for women across many firms at once, it may change wages or the allocation of women across firms and sectors. Our estimates describe the response of a single firm, not the aggregate effect of extending the quota. Third, they do not measure the effect of a permanent quota, because the obligation lasts only as long as the contract.

Spillovers on Control Firms Treated and control firms may compete for the same workers. If treated firms hire women who would otherwise work for controls, our estimates overstate the effect of the quota. Two features limit this concern. First, we do not match on location, so treated and control firms rarely draw on the same local pool of women (Section 4). Second, the effect is small relative to local labor markets. Across the 685 treated firms, the quota adds about 210 female hires over the three years after the award, which is unlikely to change the hiring of control firms.

The Youth Quota Covered firms must also reserve 30 percent of new hires for workers under 36. Our estimates therefore capture the response to both requirements together. Further analysis of the characteristics of the women hired is needed to assess how the two requirements interact.

5 How Firms Respond to a Hiring Quota

A firm that wins a contract with the quota can respond in several ways. It can avoid the quota by keeping its hiring below three workers. It can substitute women for men among the workers it would have hired anyway, or expand its workforce by hiring women on top of men. It can keep the new hires or let them go once the contract ends, and it can place them in different jobs and on different terms. Each of these responses may raise or lower firm performance.

We organize the results along these responses. We first verify that the quota raises female hiring. We then study three margins. The extensive margin asks whether firms hire at all, and whether they hire any woman or any man. It captures avoidance and shows whether the adjustment falls on women or on men. The intensive margin, how many women firms employ and keep, separates substitution from expansion and measures persistence. The compositional margin describes which women firms hire, in which jobs, and on what terms. We conclude with firm performance. All estimates compare treated firms with their matched controls and should be read as described in Section 4.2.

5.1 First Stage: Female Hiring

The quota raises female hiring (Figure 2). Treated firms hire 0.11 more women per year than controls, an increase of 25.9 percent over a baseline of 0.44 (panel a). Scaled by baseline employment, the female hiring rate rises by 0.7 percentage points, or 35.8 percent of its baseline value of 2.0 percent (panel b). Both female hires and the female hiring rate rise mainly in the award year and the following year. The larger proportional effect on the scaled measure indicates that, relative to firm size, the increase is larger in smaller firms, where each additional woman is a larger share of the workforce. Neither measure, however, accounts for how often firms hire. The quota applies to new hires, so it is more demanding for firms that hire few workers each year. As mentioned in Section 3, we cannot observe which hires work on the contract. Still, the rise in female hires relative to matched controls, both in levels and relative to firm size, indicates that firms respond to the quota.

An increase of 0.11 women per firm may look small, but the average includes many firms that hire no women in a given year, either because they hire only men or because they hire no one at all. In the baseline year, 13 percent of treated firms hire no workers, and the median treated firm hires no women. Against a mean of 0.44, a gain of 0.11 is large. Appendix Figure A2 shows that, among firms that hire at least one woman after the award, treated firms hire about half a woman more per year than controls in the award year and the year after. This comparison conditions on an outcome after the award, so it is descriptive rather

than causal. It suggests that the average effect combines many firms that hire no one, or no women, with a larger response among those that do.

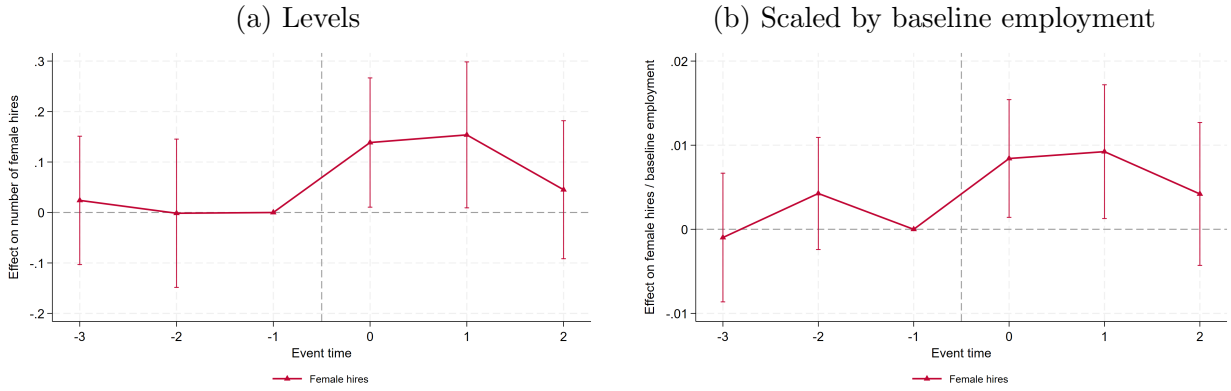


Figure 2: Female Hires: Levels and Scaled

Notes: The figure shows the effect of the quota on female hiring. The figure reports estimates of β_k from equation (2), which compares 685 treated firm-contracts with their matched controls and includes firm-by-contract and pair-by-year fixed effects. Event time 0 is the award year, and the year before the award is the reference period. Earlier and later years are binned into endpoint coefficients, which are not reported. The estimate two years after the award uses only cohorts observed for that long. Vertical bars are 95 percent confidence intervals based on standard errors clustered at the firm level. In panel (a), the outcome is the number of female hires in the year. In panel (b), it is female hires divided by the firm's total employment in the baseline year. A hire is a worker employed by the firm in a year but not in the previous year.

5.2 Extensive Margin: Whether Firms Hire

The first decision is whether to hire at all, and whom. Once the contract is awarded, the firm must comply with the quota only if it hires at least three workers for the project. It can respond in three ways, each with a different trace on the extensive margin. It can hire less, executing the project with its existing workers. The probability of hiring any worker then falls, and the fall comes from men. It can hire women on top of the men it would have hired anyway. The probability of hiring at least one woman then rises, and the probability of hiring at least one man does not change. Or it can hire a woman instead of a man. The probability of hiring at least one woman then rises, and the probability of hiring at least one man falls. Ignoring the quota is risky, because a firm that does not meet it faces penalties, the termination of the contract, and exclusion from future calls (Section 2).

Which response prevails is not obvious ex ante. Hiring less is costly when the contract is large relative to the firm, because workers must be reassigned from other tasks and execution may slow down. Hiring women on top of men is more likely for firms that need more workers to execute the contract. Hiring a woman instead of a man is easiest for firms that hire few

workers each year, where a single hire can meet the quota. We return to the role of contract importance in Section 6.

On average, the quota does not change whether firms hire. In the award year, the probability of hiring any worker falls by 5.1 percentage points from a baseline of 87 percent, but it returns to zero in the following year (Figure 3, panel a). Over the post-award years, the average effect is −1.3 percentage points and is not significant.

The dip in the award year comes from men. In that year, the probability of hiring at least one man falls by 5.5 percentage points, while the probability of hiring at least one woman rises by 3.3 points, though not significantly (panel b). Fewer firms hire any man, but the number of men hired does not fall (Appendix Figure A1), and the number of women hired rises (Section 5.1). In the award year, firms that stop hiring men hire no one, and the additional women are hired by firms that also hire men.

The decomposition of the probability of hiring by gender shows this. A firm hires at least one worker if it hires at least one man or at least one woman, so

$$\Pr(\text{any worker}) = \Pr(\text{any man}) + \Pr(\text{any woman}) - \Pr(\text{both}),$$

where "both" denotes firms that hire at least one man and at least one woman. The same identity holds for the effects of the quota. In the award year, 5.5 percentage points fewer firms hire a man. Of these, 5.1 points hire no one, which is the fall in the probability of any hire, and only 0.4 points hire only women. The 3.3-point rise in the probability of hiring a woman therefore comes almost entirely from firms that also hire men, whose share rises by 2.9 points. These firms hire women on top of men, or replace one of several men with a woman. Firms that hire only women barely change. These are net changes in the share of firms in each group, computed from the three estimates, and they carry no standard errors.

The responses differ by firm size (Figure 4). Small firms, with at most 9 employees in the baseline year, hire few workers each year, so a single hire can meet the quota. After the award, they are 4.3 percentage points more likely to hire at least one woman and 2.2 points less likely to hire at least one man, though the second estimate is not significant. This is the pattern expected when firms hire a woman instead of a man. Mid-sized firms, with 10 to 17 employees, are 3.3 points less likely to hire any worker, though not significantly. Large firms, with more than 17 employees, show no change. They hire many workers each year and can meet the quota by hiring women on top of men.

Each response has a different implication for employment, which we study next. If firms hire women on top of men, female employment should rise and male employment should not change. If they hire a woman instead of a man, female employment should rise and male

employment should grow less than it otherwise would. If they hire less, employment should change only if separations rise.

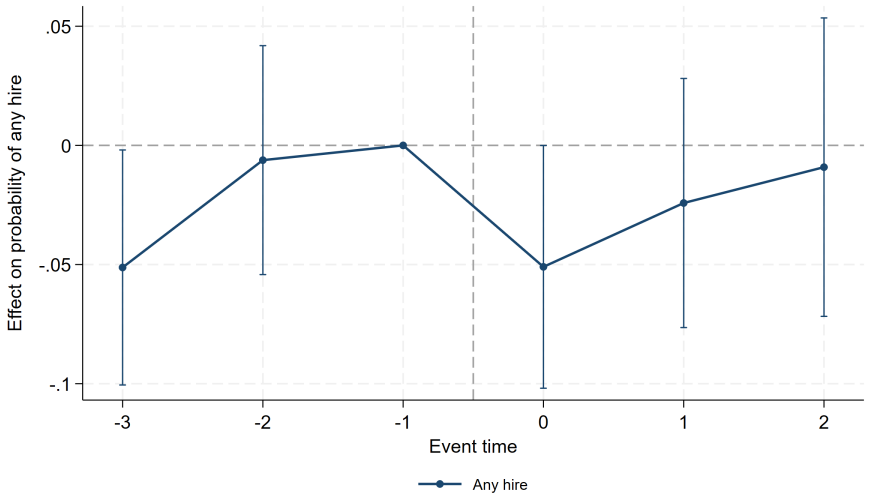
5.3 Intensive Margin: How Many Women Firms Employ

More female hires raise female employment only if the women stay. We ask two questions. First, do the women hired under the quota enter the workforce of these firms? Second, does the flow of hires turn into a stock, or do firms separate the women once the contract ends? The responses on the extensive margin have different implications here. If firms hire women on top of men, female employment rises and male employment does not change. If they hire a woman instead of a man, female employment rises and male employment grows less than it otherwise would. If firms comply only while the contract runs, female separations rise when it ends and female employment returns to its baseline level.

Female Employment The women hired under the quota enter the workforce. We measure the employment of each gender relative to the firm's total employment in the baseline year, and divide it by the average baseline share of that gender among treated firms. This double normalization gives each firm the same weight and turns coefficients into proportional changes relative to the average treated firm, so effects on women and men are comparable (Appendix C). After the award, female employment in treated firms rises by 8.6 percent of its average baseline level relative to controls (s.e. 3.9, Figure 5). The effect grows over time, from 8.8 percent in the award year to 12.0 percent one year later and 12.9 percent two years later. It corresponds to 0.95 percentage points of baseline employment, or about 0.2 additional women in a firm of 21 workers. Male employment does not change (-0.3 percent, s.e. 2.1), and total employment rises by a non-significant 0.7 percent. The workforce therefore shifts toward women, in line with firms hiring women on top of men. Before the award, treated and control firms follow similar paths. The shift also shows up in hours of work. The female share of weeks worked in the firm, computed each year over that year's total, rises by 1.5 percentage points over a baseline of 13.3 percent, and the rise grows over time (Appendix C, Figure A5).

From Flow to Stock: Separations and Persistence The flow of new hires becomes stock because firms keep the women they hire. Hires and separations in year t refer to the same transition, between $t-1$ and t, and we scale both by the firm's total employment in the baseline year. In the award year, female separations fall by 0.5 percentage points of baseline employment (Figure 6, panel b). In the following years they do not rise. Net female hiring, hires minus separations, rises by 1.4 percentage points of baseline employment in the award

(a) Any worker



(b) By gender

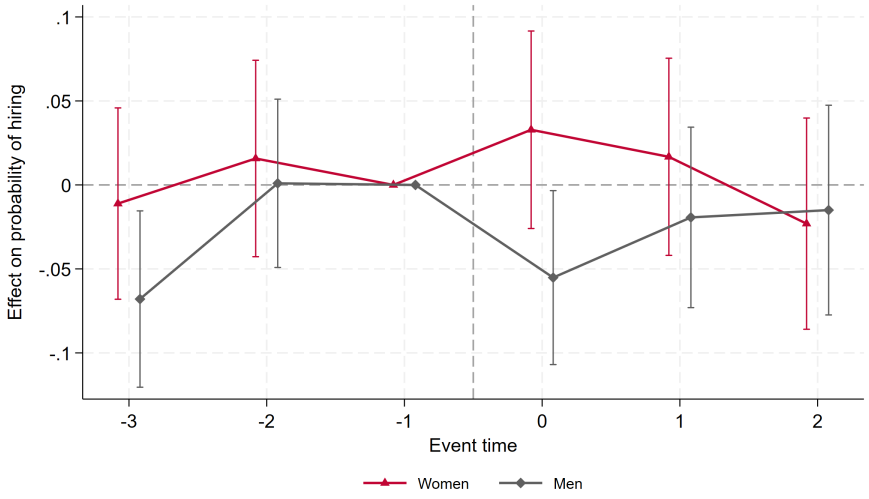


Figure 3: Probability of Hiring

Notes: The figure shows the effect of the quota on the probability of hiring. The figure reports estimates of β_k from equation (2), which compares 685 treated firm-contracts with their matched controls and includes firm-by-contract and pair-by-year fixed effects. Event time 0 is the award year, and the year before the award is the reference period. Earlier and later years are binned into endpoint coefficients, which are not reported. The estimate two years after the award uses only cohorts observed for that long. Vertical bars are 95 percent confidence intervals based on standard errors clustered at the firm level. In panel (a), the outcome is an indicator equal to one if the firm hires at least one worker in the year. In panel (b), the outcomes are indicators equal to one if the firm hires at least one woman (red) or at least one man (gray) in the year.

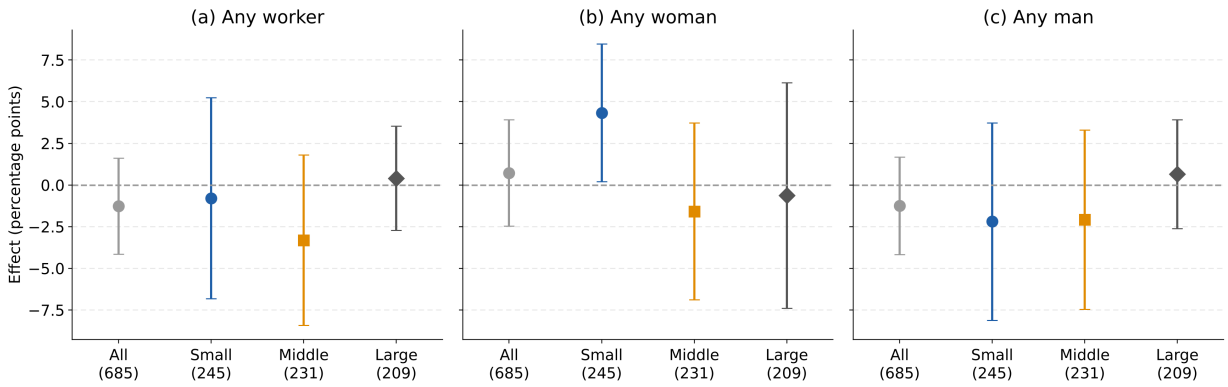


Figure 4: Probability of Hiring by Firm Size

Notes: The figure shows how the effect of the quota on the probability of hiring varies with firm size. Each dot is the post-award difference-in-differences estimate from equation (1), in percentage points, for all matched firms and separately for small (at most 9 employees), middle (10 to 17), and large (more than 17) firms, based on baseline employment. The number of matched pairs is in parentheses. The outcomes are indicators equal to one if the firm hires at least one worker (panel a), at least one woman (panel b), or at least one man (panel c) in the year. Vertical bars are 95 percent confidence intervals based on standard errors clustered at the firm level.

year. The women hired in that year also stay. Female hires still employed by the firm two years later rise by 0.8 percentage points of baseline employment, as much as female hires themselves. In levels, female separations rise by 0.16 two years after the award (panel a). This rise is not visible once separations are scaled by baseline employment.

One concern is that many contracts may still be running at the end of our data, so the women hired may be separated only once the contract ends. Contracts that end within a year help interpret the persistence we find. If firms undid the quota, female separations should rise in the year after these contracts end. Among the 92 pairs whose treated contract lasts at most one year and is observed for a full year after it starts, female separations do not rise in the two years after the award (Appendix Figure A4).[28] The estimates are imprecise, and female employment in these pairs is back at its baseline level one year after the award, so this evidence is suggestive.

5.4 Compositional Margin: Which Women, in Which Jobs, and on What Terms

The quota fixes only the share of women among new hires. It leaves firms free to choose which women to hire, in which jobs, and on what terms (Section 2). Firms can use this

[28]Of the 685 treated contracts, 621 report a planned duration, but only 425 report a start date and 274 a recorded end date. Results are similar when we replace the planned duration with the historical median duration of contracts with the same two-digit CPV code, which yields 100 pairs.

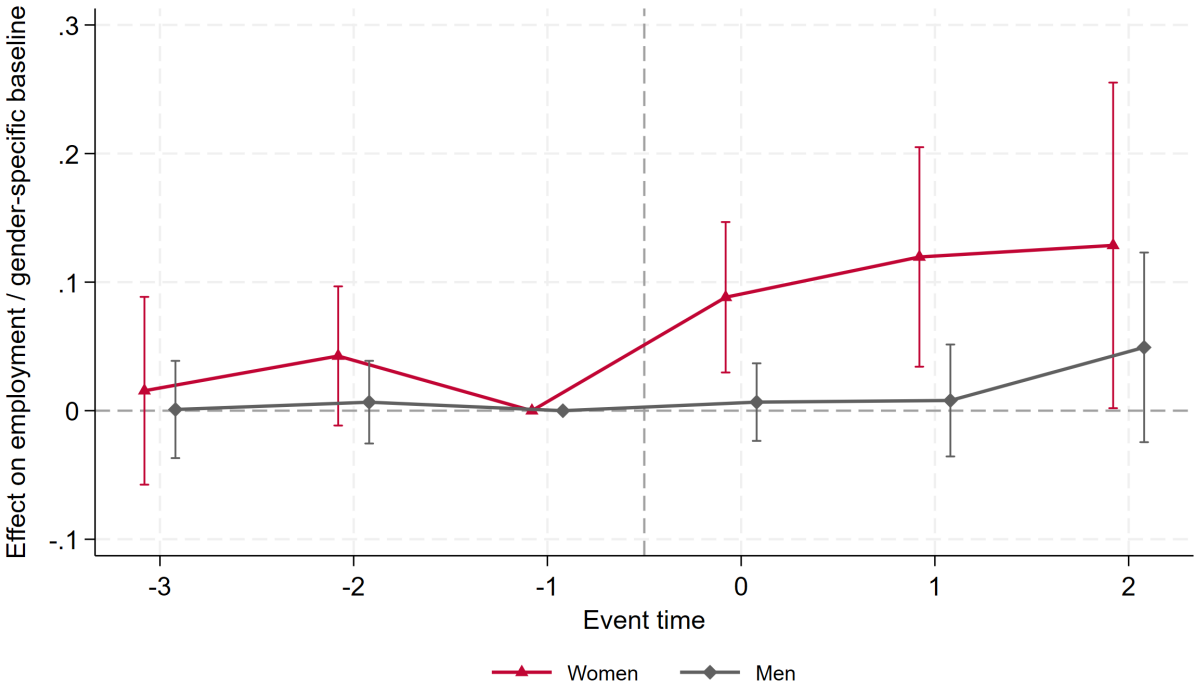


Figure 5: Employment by Gender

Notes: The figure shows the effect of the quota on employment by gender. The figure reports estimates of β_k from equation (2), which compares 685 treated firm-contracts with their matched controls and includes firm-by-contract and pair-by-year fixed effects. Event time 0 is the award year, and the year before the award is the reference period. Earlier and later years are binned into endpoint coefficients, which are not reported. The estimate two years after the award uses only cohorts observed for that long. Vertical bars are 95 percent confidence intervals based on standard errors clustered at the firm level. The outcomes are female employment (red) and male employment (gray), each divided by the firm's total employment in the baseline year and by the average baseline share of that gender across treated firms (Appendix C). Coefficients are proportional changes relative to the average baseline level.

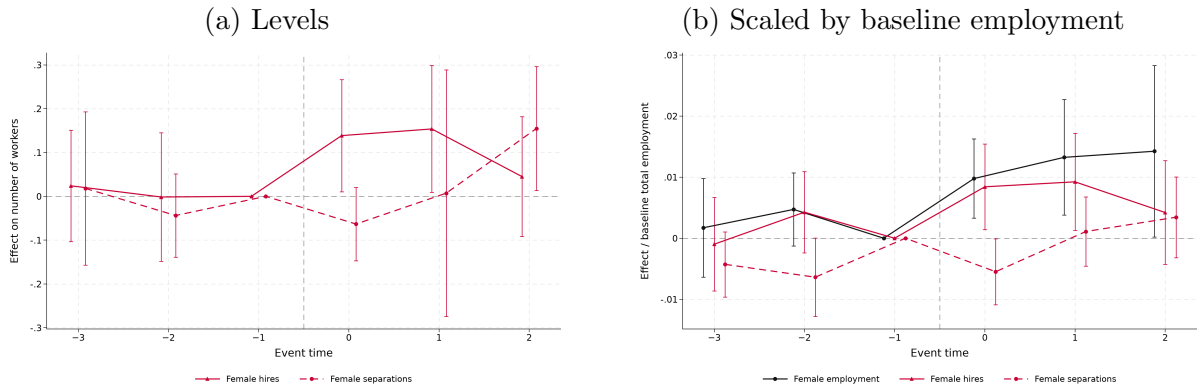


Figure 6: Female Hires and Separations: Levels and Scaled

Notes: The figure shows how female hires translate into female employment once separations are taken into account. The figure reports estimates of β_k from equation (2), which compares 685 treated firm-contracts with their matched controls and includes firm-by-contract and pair-by-year fixed effects. Event time 0 is the award year, and the year before the award is the reference period. Earlier and later years are binned into endpoint coefficients, which are not reported. The estimate two years after the award uses only cohorts observed for that long. Vertical bars are 95 percent confidence intervals based on standard errors clustered at the firm level. In panel (a), the outcomes are the numbers of female hires (solid line) and female separations (dashed line). In panel (b), the outcomes are female employment, female hires, and female separations, each divided by the firm's total employment in the baseline year. A hire is a woman employed by the firm in year t but not in year $t-1$. A separation is a woman employed by the firm in year $t-1$ but not in year t.

discretion to comply at the lowest cost. They can hire women into the jobs where women are easiest to find, such as office and administrative roles, rather than into the core jobs of the contract, such as manual work on a construction site. They can also offer temporary or part-time contracts, which are cheaper to end once the contract is over, or pay lower wages. Alternatively, firms may treat the women they hire like any other worker and place them in the jobs the contract requires. The composition of new female hires therefore shows how firms comply. We study where the women come from, which occupations they enter, and their contracts and wages. These results describe which women firms hire, but they do not identify the effect of the quota on the women themselves. Most of the additional women were not employed in the private sector in the year before, so we do not observe them before they are hired and cannot build a counterfactual for them. We therefore cannot estimate how the quota changes their employment, earnings, or careers at the worker level. The estimates in this section compare the composition of hires across treated and control firms, and we read them as descriptive evidence on how firms comply. We are developing an alternative strategy to estimate the effects on the women hired.

Origin and Occupation of New Hires Only 25 percent of the additional women were employed in the private sector in the year before. Hires from non-employment account

for 26.8 percentage points of the 35.8 percent increase in the female hiring rate, and hires from other private firms for 9.0 points. The remaining 75 percent therefore come from non-employment: they may have been out of the labor force, unemployed, self-employed, or employed in the public sector, which we cannot distinguish. Before the award, 50.8 percent of female hires in matched treated firms came from non-employment, so the quota tilts hiring further toward women outside private employment.

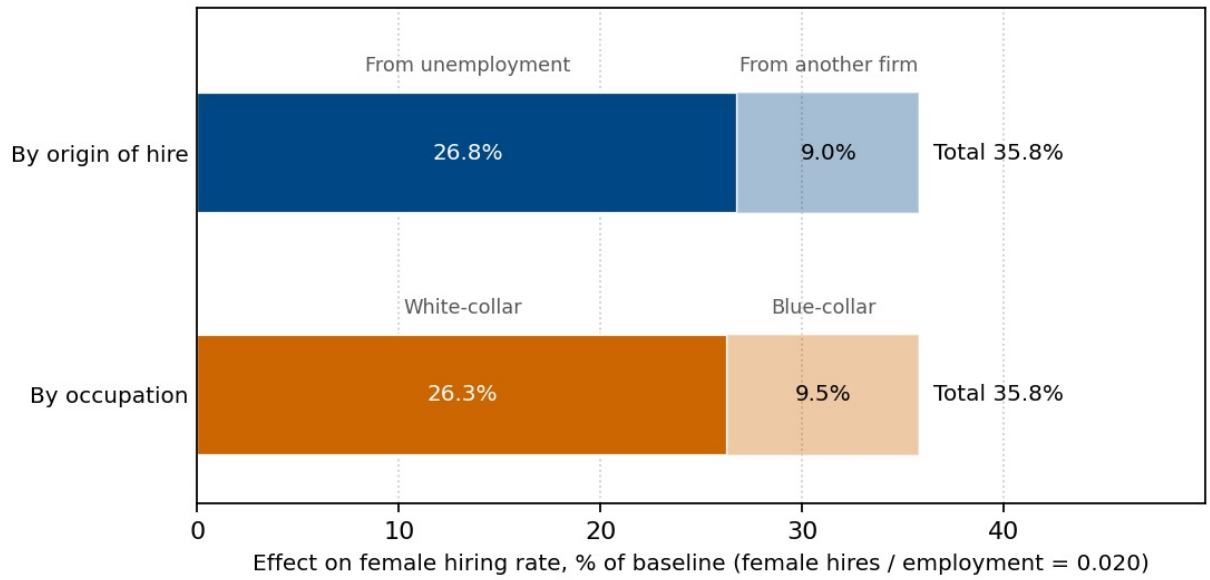


Figure 7: Composition of Additional Female Hires

Notes: The figure decomposes the effect of the quota on the female hiring rate, female hires divided by baseline total employment, by the origin and the occupation of the new hires. The total bar is the post-award difference-in-differences estimate from equation (1), expressed as a percentage of the baseline female hiring rate of 2.0 percent. Each segment is the estimate for the corresponding subgroup of hires. By origin, hires from non-employment are women with no private-sector job in the previous year, and hires from another firm are women employed by another private firm. By occupation, hires are split into white-collar and blue-collar positions.

Contract Terms The new jobs are mostly temporary white-collar positions. White-collar hires account for 26.3 percentage points of the increase and blue-collar hires for 9.5 points. Temporary contracts account for 81 percent of additional female hires and part-time contracts for 64 percent, well above the baseline shares of 41.6 and 33.9 percent among female hires in matched treated firms.

Wages Firms do not pay the women they hire less. The log average weekly wage of female employees does not change after the award (0.002, s.e. 0.011), and neither does that of male employees (−0.003, s.e. 0.008, Figure 9). The gender wage gap within the firm is also

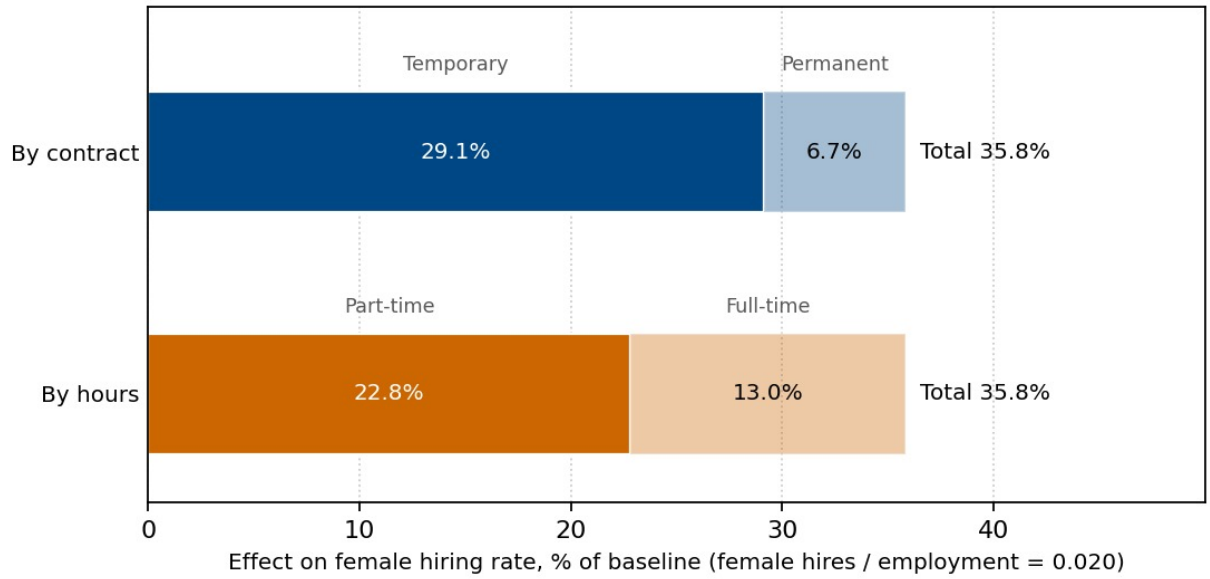


Figure 8: Terms of Employment of Additional Female Hires

Notes: The figure decomposes the effect of the quota on the female hiring rate, female hires divided by baseline total employment, by the terms of the new contracts. The total bar is the post-award difference-in-differences estimate from equation (1), expressed as a percentage of the baseline female hiring rate of 2.0 percent. Each segment is the estimate for the corresponding subgroup of hires: temporary and permanent contracts, and part-time and full-time contracts.

unchanged (0.003, s.e. 0.012). The same holds for white-collar employees, the jobs where firms could most easily place women (Appendix Figure A3, panel b). For new hires, the wage of men does not change (−0.008, s.e. 0.012), while the wage of new female hires rises by 10.7 log points, significant only at the 10 percent level (s.e. 0.062, panel a). This estimate is fragile. It uses only firm-years with at least one female hire, a sample that changes from year to year and that the quota itself affects, and it shows a large negative coefficient three years before the award. Among white-collar new hires, wages do not change for either gender (panel c). Firms therefore comply without lowering the pay of the women they hire.

5.5 Firm Performance

Productivity, Revenues, and Profits The quota imposes no detectable costs on firms. Revenues, total assets, and profit margins do not change, and labor productivity, measured as value added per employee reported in the balance sheet, rises by 6.7 percent. Because many additional women work part-time, a per-employee measure, if anything, understates productivity per hour worked.

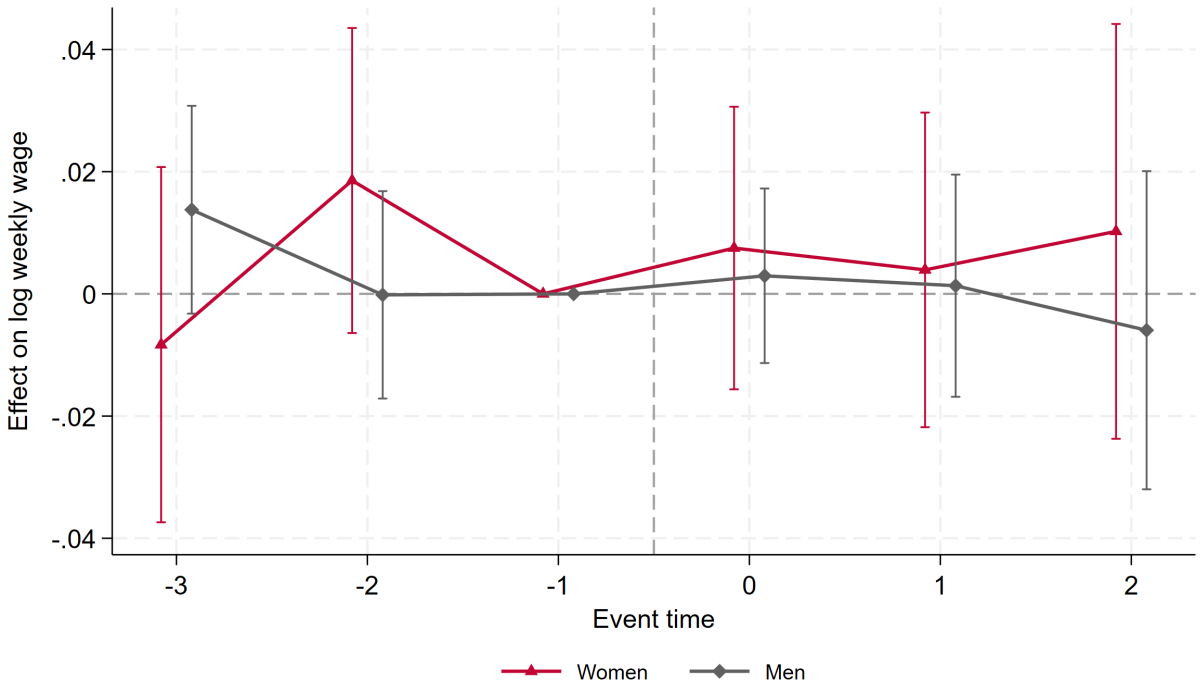


Figure 9: Weekly Wages by Gender

Notes: The figure shows the effect of the quota on weekly wages by gender. The figure reports estimates of β_k from equation (2), which compares 685 treated firm-contracts with their matched controls and includes firm-by-contract and pair-by-year fixed effects. Event time 0 is the award year, and the year before the award is the reference period. Earlier and later years are binned into endpoint coefficients, which are not reported. The estimate two years after the award uses only cohorts observed for that long. Vertical bars are 95 percent confidence intervals based on standard errors clustered at the firm level. The outcomes are the log average weekly wage of female employees (red) and of male employees (gray) in the firm.

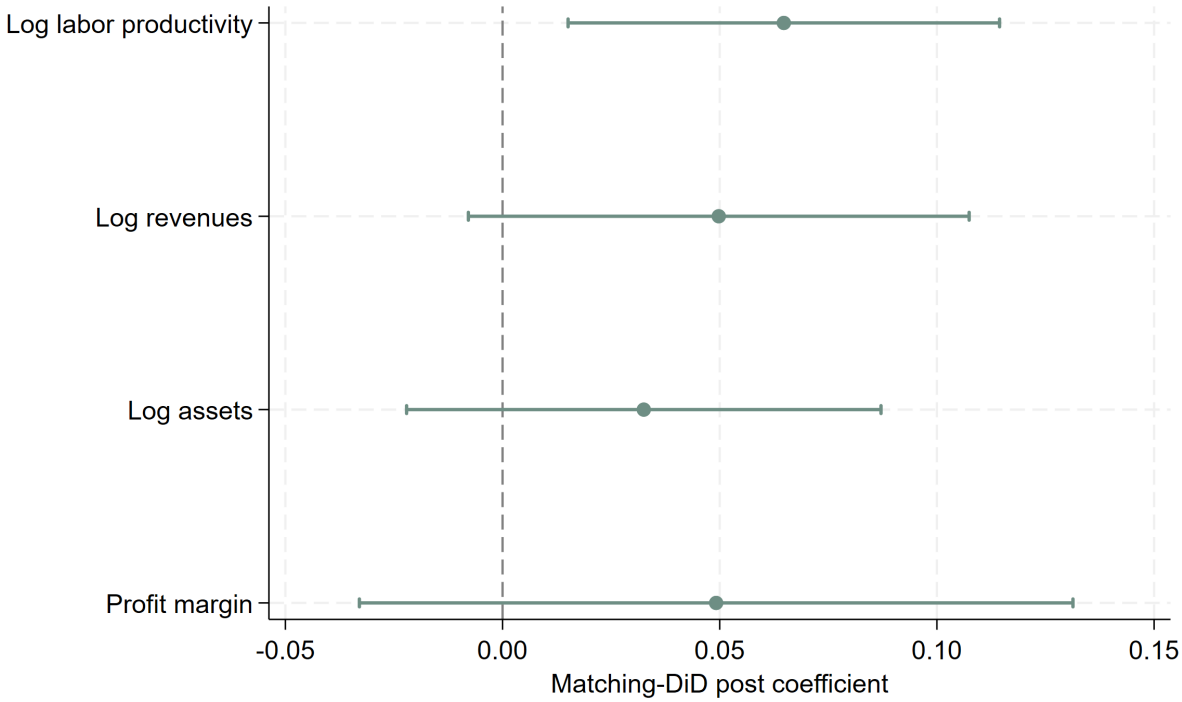


Figure 10: Firm Performance

Notes: The figure shows the effect of the quota on firm performance. Estimates of β from equation (1), which compares treated firm-contracts with their matched controls and includes firm-by-contract and pair-by-year fixed effects. Horizontal bars are 95 percent confidence intervals based on standard errors clustered at the firm level. Outcomes come from balance sheets and are available for matched firms with balance-sheet data. Labor productivity is value added per employee. Profit margin is profits over revenues.

Contract Execution Balance sheets are a blunt measure of how the quota affects firms. Firms complete the contract and get paid in any case, and many win other contracts in the meantime, so a cost of complying with the quota is hard to detect in revenues or profits. Balance sheets are also available only for a subset of matched firms, mostly the larger ones, where the quota matters less. The time a firm takes to complete the contract is a more direct, operational measure of performance. If hiring women under the quota disrupts production, treated contracts should take longer to complete than their matched controls.

Treated contracts are not completed more slowly than comparable PNRR contracts (Table 4). We compare each treated contract with its matched control, using two models described in Appendix D. Against all matched controls, treated contracts are completed more slowly. The hazard of completion is 29 percent lower (column 1, Panel A), and among completed contracts, treated contracts last 44 days longer (column 1, Panel B). Controlling for planned duration barely changes these estimates (column 2). This gap, however, reflects PNRR contracts rather than the quota. When we restrict to pairs in which the control contract is also PNRR-funded, the hazard ratio is 1.07 (s.e. 0.20) and the difference in realized duration is 7 days (s.e. 26), neither significant (columns 3 and 4). PNRR contracts carry their own rules on funding, reporting, and deadlines, and these, not the quota, appear to slow execution.

These estimates rest on small samples. Completion is observed for 304 matched pairs in the hazard model, and the realized duration of both contracts for only 128 pairs, 28 of them with a PNRR control. Read together with the balance-sheet results, which cover a different and also limited sample, they provide no evidence that the quota harms firm performance. We are developing an alternative empirical strategy that would extend the sample on which we observe performance.

6 Heterogeneity and External Validity

Contract Importance Responses are concentrated where the contract matters most. We measure importance as contract value relative to the treated firm's average annual wage bill in the two years before the award. Among firms for which the contract is more important, the probability of any hire falls and the female employment share rises. Among the others, effects are not distinguishable from zero.

Firm Size Responses differ by firm size (Figure 12). Small firms, with at most 9 employees in the baseline year, account for most of the effect on women. Their female hiring rate rises by 1.5 percentage points of baseline employment, mostly through women hired from non-

Table 4: The Gender Hiring Quota and Contract Completion Time

	All matched controls		PNRR controls only	
	(1)	(2)	(3)	(4)
Panel A. Cox proportional-hazards model				
Treated contract: hazard ratio	0.714***	0.734***	1.069	1.032
	(0.064)	(0.068)	(0.196)	(0.206)
Observations	608	602	156	156
Matched pairs	304	301	78	78
Pair strata	✓	✓	✓	✓
Planned-duration control		✓		✓
PNRR-only controls			✓	✓
Panel B. OLS: realized duration in days				
Treated contract	43.688***	38.288**	6.679	7.407
	(14.455)	(14.860)	(26.098)	(25.390)
Observations	256	256	56	56
Matched pairs	128	128	28	28
Pair fixed effects	✓	✓	✓	✓
Planned-duration control		✓		✓
PNRR-only controls			✓	✓

Notes: The table compares the completion time of treated contracts with that of their matched controls. Panel A reports hazard ratios from Cox proportional-hazards models stratified by matched pair. A hazard ratio below one indicates a lower instantaneous probability of completion and therefore slower completion. Panel B reports OLS coefficients, measured in realized contract days, with matched-pair fixed effects, for pairs in which both contracts are completed. Planned duration is measured using the preferred contract-specific ex-ante duration. Columns (3) and (4) retain only matched pairs in which the control contract is also PNRR-funded. Appendix D describes the samples and models. Standard errors clustered at the firm level are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

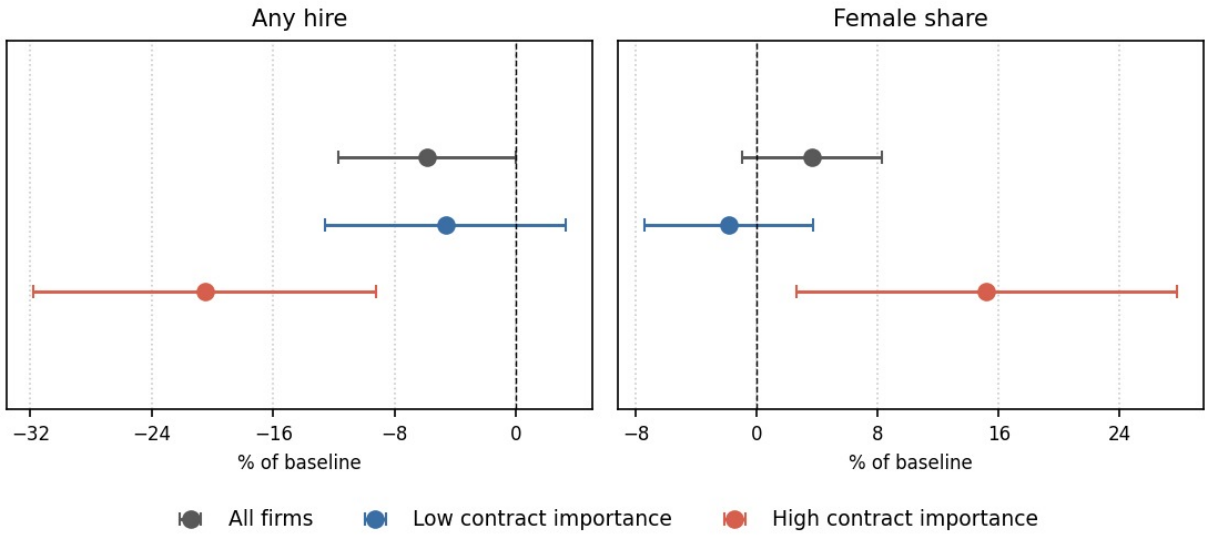


Figure 11: Heterogeneity by Contract Importance

Notes: The figure shows how the effect of the quota varies with the importance of the contract for the firm. Each dot is the post-award difference-in-differences estimate from equation (1), expressed as a percentage of the baseline mean of the outcome, for all matched firms (gray) and separately for firms with low (blue) and high (red) contract importance. Contract importance is the contract value relative to the treated firm's average annual wage bill in the two years before the award. The outcomes are an indicator for hiring any worker and the female employment share. Horizontal bars are 95 percent confidence intervals based on standard errors clustered at the firm level.

employment, and their female employment rises by 26 percent of its baseline level. Male employment does not fall in these firms. Mid-sized firms, with 10 to 17 employees, show no effect on women. Their probability of hiring any worker falls, though not significantly. In large firms, with more than 17 employees, female employment rises by 10 percent and the female share of weeks worked by 1.3 percentage points, while the probability of hiring does not change. Wages and separations do not respond in any group.

External Validity Our estimates come from smaller firms winning smaller contracts, mostly in construction (Section 4). The heterogeneity results indicate how far they extend to the larger firms and contracts that we cannot match.

7 Conclusions

We study a gender quota on new hires attached to public contracts in Italy. Firms exposed to the quota hire more women than comparable firms winning similar contracts without it. The quota does not change whether firms hire. In the award year, some firms that would have hired only men hire no one, while the additional women are hired by firms that also

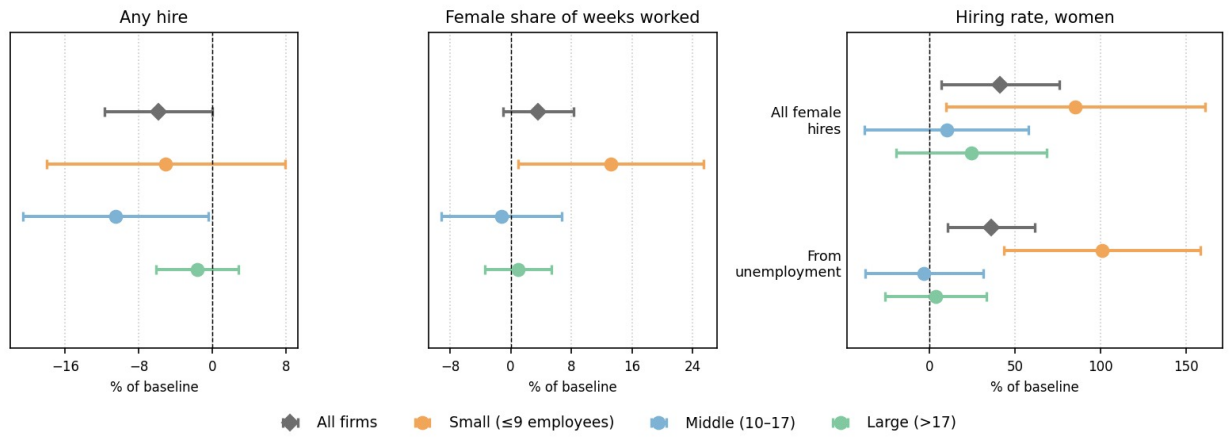


Figure 12: Heterogeneity by Firm Size

Notes: The figure shows how the effect of the quota varies with firm size. Each dot is the post-award difference-in-differences estimate from equation (1), expressed as a percentage of the baseline mean of the outcome, for all matched firms (gray) and separately for small (at most 9 employees), middle (10 to 17), and large (more than 17) firms, based on baseline employment. The outcomes are an indicator for hiring any worker, the female share of weeks worked, and the female hiring rate, for all female hires and for hires from non-employment. Horizontal bars are 95 percent confidence intervals based on standard errors clustered at the firm level.

hire men. Female employment rises by 8.6 percent of its baseline level and keeps growing over time, while male and total employment do not change significantly. Firms keep the women they hire: female separations do not rise, and the women hired in the award year are still employed two years later. Only 25 percent of the additional women were employed in the private sector in the year before. Firms do not pay them less, and we find no detectable costs for firms.

The effect is large relative to these firms but small in absolute terms. Across the 685 treated firms, the quota adds about 210 female hires over the three years after the award, about one third more female hires per year than before. At its peak, it adds about 225 women to employment, roughly 10 percent more women but only one percent of these firms' workforce. Because matched firms are small and win small contracts, these figures are a lower bound for the firms and contracts the quota covers.

Three lessons follow. First, binding conditions attached to public contracts can change the hiring of private employers. Governments rarely regulate whom private firms hire, but as buyers they can attach conditions to the contracts they award, and firms accept them when the contract matters. The reach of this lever grows with firms' reliance on public demand. Second, a quota on the flow of hires can change the stock of employment. Firms did not undo the quota while it was in force, and the women hired in the award year were still employed two years later. Evidence from contracts shorter than a year is too imprecise to tell whether this persistence survives the end of the obligation. Third, even short jobs may strengthen

women's attachment to the labor market. Most of the additional women did not come from other private firms, so the quota may have drawn women into private employment rather than moving them across firms.

Our results come with caveats. We estimate the effect of exposure to the quota as enforced in practice, not the effect of full compliance, and we abstract from general equilibrium effects. The treatment combines the quota with a large public contract, so the effects may differ for firms that face a quota without a demand shock. The matched sample consists mostly of small construction firms, where women are hard to hire. This makes the setting a demanding test, but the magnitudes may not carry over to sectors with a larger supply of female workers. Finally, firms know before bidding whether a contract carries the quota, and we cannot yet rule out selection entirely.

Several questions remain open. Data on all bidders will allow us to study selection into covered contracts directly. Linking the women hired under the quota to their later careers will show whether these jobs lead to stable employment, and data on public employment will separate women drawn from outside the labor force from those moving out of the public sector. A closer look at contract duration, and at the 2022 cohort that we observe for longer, will clarify how long the obligation binds and whether firms keep the women once it ends. Finally, scaling our estimates to the Italian economy would show how much a hiring quota of this kind could narrow the gender employment gap.

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A Matching and Sample

Table A1: Construction of the Treated Sample

Step	Sample restriction	Unit	Number
(1)	PNRR contracts with the full quota, calls 2021–2024	Contracts	78,196
(2)	Competitive or direct award, €15,000 to below €10 million, identified winner	Contracts	20,367
(3)	Winning firms, all members of temporary associations	Firms	9,558
(4)	One winner per contract: single firm or lead firm	Firm-contracts	
(5)	Valid date, contract object, procedure, and CPV code	Firm-contracts	
(6)	First full-quota contract of each firm	Firm-contracts	
(7)	Firm in employer-employee data in the baseline year	Firm-contracts	
(8)	At least three employees in the baseline year	Firm-contracts	
	Eligible treated firm-contracts	Firm-contracts	4,112

Notes: The table reports each step from all PNRR contracts with the full quota to the eligible treated firm-contracts used in the matching. The unit column indicates whether each row counts contracts, firms, or firm-contracts. Step (6) is applied before steps (7) and (8), so a firm whose first full-quota contract fails them leaves the sample. Steps (4) to (8) together lead from the 9,558 winning firms to the 4,112 eligible treated firm-contracts.

Table A2: Matching Criteria

Criterion	**Rule**
A. Candidate contracts	
Calls	Published 2021–2024; competitive procedure or direct award; value from €15,000 to below €10 million; winning firm identified
Treated	The firm's first PNRR contract with the full quota
Potential controls	Ordinary contracts and PNRR contracts with a complete waiver, won by firms that never win a full-quota contract
B. Contracts: exact match and caliper	
Object	Same object: works, services, or supplies
Procedure	Same procedure group: open; restricted; other competitive procedures (competitive negotiation, competitive dialogue, innovation partnership); e-procurement and mini-competitions within framework agreements; framework agreements; direct awards. Direct orders under framework agreements count as direct awards
Timing	Awarded within six months of each other
CPV code	Same first four digits of the main CPV code; if no control is found, same first three; if none, same first two
Value	Absolute log difference of at most 0.10, about 10 percent
C. Firms in the baseline year: exact match and caliper	
Sector	Same set of one-letter ATECO sections; multi-sector firms are matched only to firms with the same sections
Size	Absolute difference in log employment of at most 0.30
Female share	Absolute difference in the female employment share of at most 10 percentage points
Same firm	A firm cannot serve as its own control
D. Selection among candidates	
Distance	Euclidean distance on log employment and female employment share, each divided by its standard deviation in the sample
Ties	Smaller difference in log value, then smaller difference in timing
Controls	One control per treated firm-contract, with replacement
E. After matching	
Value	Keep pairs whose contract values differ by at most €1,500

Notes: The table lists the criteria used to match each eligible treated firm-contract to one control firm-contract (Section 4). Panel A defines the candidate contracts. The criteria in panels B and C must all hold for a control to be a candidate. Panel D selects one control among the candidates, and panel E is applied to the resulting pairs. Firm characteristics are measured in the year before the award.

Table A3: Matched and All Treated Firms

	Matched treated	All treated
Employment (median)	12	18
Female share of employment	0.111	0.109
Contract value (median, euros)	47,638	140,668
Construction share	0.734	0.709
Firm-contracts	685	4,112

Notes: The table compares the 685 matched treated firm-contracts with all 4,112 eligible treated firm-contracts. Firm characteristics are measured in the year before the award. Contract value is the value of the treated firm's first contract with the full quota. The female share of employment is the average across firms. The construction share includes multi-sector firms with construction among their sections.

B Additional Results

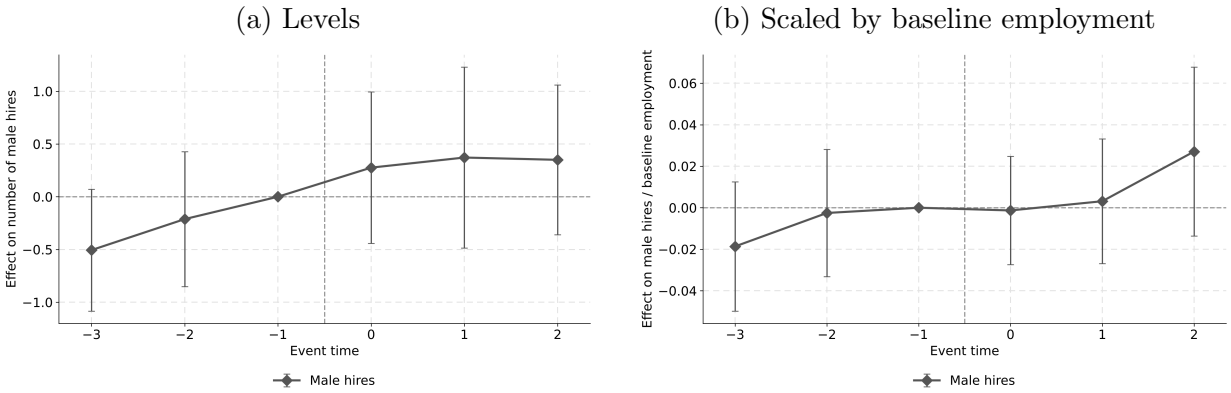


Figure A1: Male Hires: Levels and Scaled

Notes: The figure shows the effect of the quota on male hiring. It is the counterpart of Figure 2 for men. The figure reports estimates of β_k from equation (2), which compares 685 treated firm-contracts with their matched controls and includes firm-by-contract and pair-by-year fixed effects. Event time 0 is the award year, and the year before the award is the reference period. Earlier and later years are binned into endpoint coefficients, which are not reported. The estimate two years after the award uses only cohorts observed for that long. Vertical bars are 95 percent confidence intervals based on standard errors clustered at the firm level. In panel (a), the outcome is the number of male hires in the year. In panel (b), it is male hires divided by the firm's total employment in the baseline year. A hire is a worker employed by the firm in a year but not in the previous year.

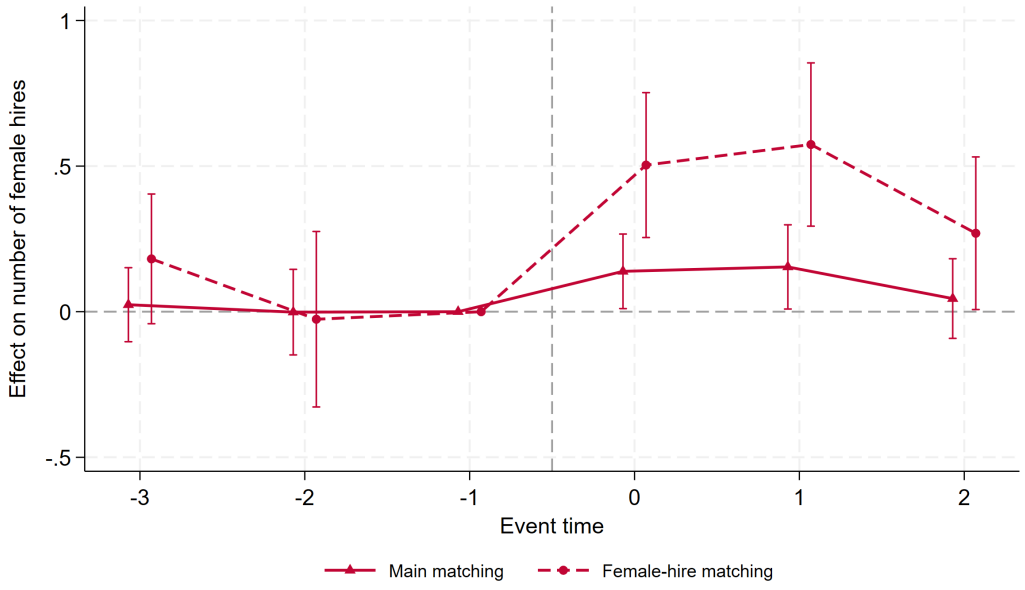
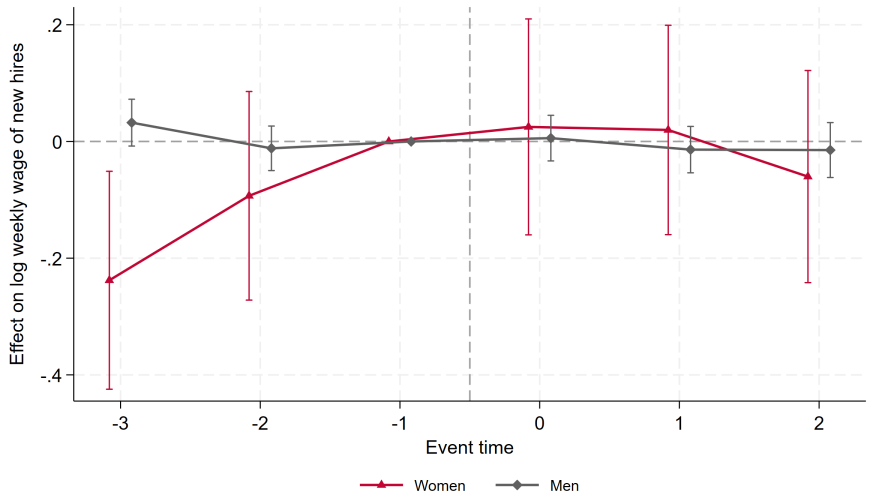


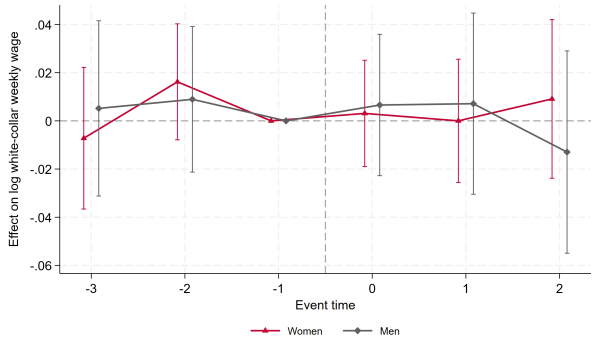
Figure A2: Female Hires Among Firms That Hire Women After the Award

Notes: The figure reports estimates of β_k from equation (2), where the outcome is the number of female hires in the year. The solid line uses all 685 matched pairs, as in Figure 2. The dashed line uses only the 316 pairs in which the treated firm hires at least one woman in the years after the award. Event time 0 is the award year, and the year before the award is the reference period. Vertical bars are 95 percent confidence intervals based on standard errors clustered at the firm level. Because the restriction depends on an outcome after the award, the dashed line is descriptive and does not identify a causal effect.

(a) New hires



(b) White-collar employees



(c) White-collar new hires

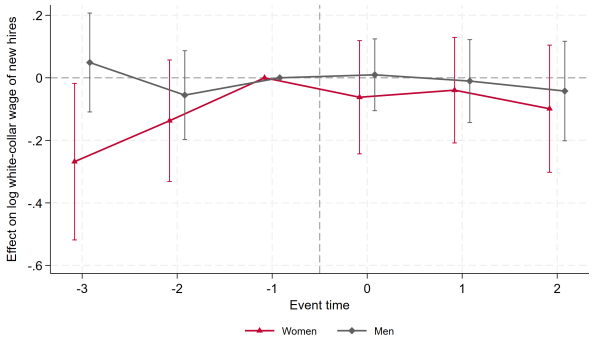


Figure A3: Weekly Wages of New Hires and White-Collar Workers by Gender

Notes: The figure reports estimates of β_k from equation (2), which compares 685 treated firm-contracts with their matched controls and includes firm-by-contract and pair-by-year fixed effects. Event time 0 is the award year, and the year before the award is the reference period. Earlier and later years are binned into endpoint coefficients, which are not reported. Vertical bars are 95 percent confidence intervals based on standard errors clustered at the firm level. The outcomes are the log average weekly full-time-equivalent wage of women (red) and men (gray) among new hires (panel a), white-collar employees (panel b), and white-collar new hires (panel c). Each outcome is defined only for firm-years with at least one worker in the corresponding group, so the sample changes from year to year and is not a balanced panel.

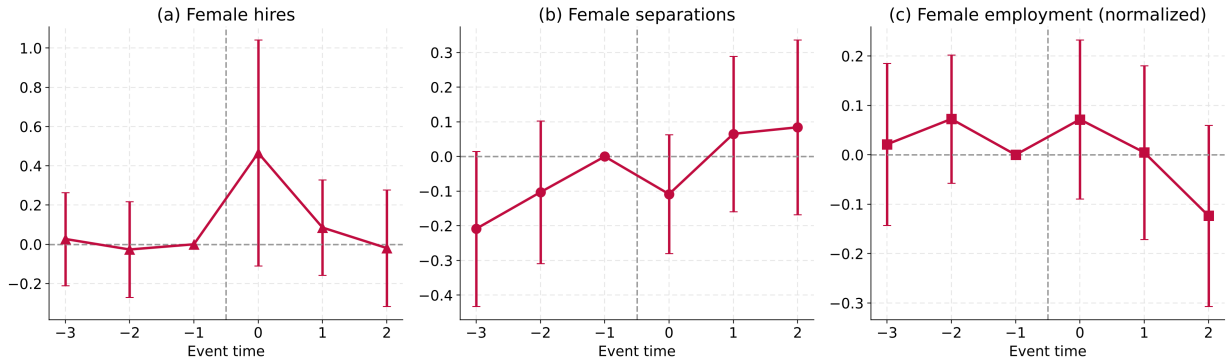


Figure A4: Contracts Shorter than One Year

Notes: The figure reports estimates of β_k from equation (2) for the 92 matched pairs in which the treated contract has a planned duration of at most 365 days and a start date that leaves a full year of follow-up within our data (Section 5.3). Event time 0 is the award year, and the year before the award is the reference period. Vertical bars are 95 percent confidence intervals based on standard errors clustered at the firm level. The outcomes are the number of female hires (panel a), the number of female separations (panel b), and female employment with the double normalization of Appendix C (panel c).

C Employment Outcomes and the Female Share

Employment, Hires, and Separations Employment of a group in year t is the number of its workers employed by the firm at any time during the year. A hire is a worker employed by the firm in year t but not in year $t-1$. A separation is a worker employed by the firm in year $t-1$ but not in year t. Hires and separations in year t thus refer to the same transition, and their difference equals the change in employment between $t-1$ and t. Flow outcomes divide hires and separations by $N_{i,-1}$, the firm's total employment in the baseline year.

Double Normalization For firm i in year t, let F_{it} and M_{it} denote female and male employment. We define

$$Y_{it}^{F} = \frac{F_{it}/N_{i,-1}}{\overline{s}_{-1}^{F}}, \qquad Y_{it}^{M} = \frac{M_{it}/N_{i,-1}}{\overline{s}_{-1}^{M}}, \qquad \overline{s}_{-1}^{F} = \frac{1}{n_T}\sum_{i \in T} \frac{F_{i,-1}}{N_{i,-1}},$$

where T is the set of n_T treated firm-contracts and $\overline{s}_{-1}^{M} = 1 - \overline{s}_{-1}^{F}$. In the matched sample, $\overline{s}_{-1}^{F} = 0.111$. The first normalization, by $N_{i,-1}$, removes differences in firm size: each firm enters with the same weight, and changes are measured relative to its initial workforce. The second normalization, by $\overline{s}_{-1}^{F}$, is a single constant computed on treated firms and applied to treated and control firms alike. It does not change the sign or the significance of the estimates. It only rescales them, so that β^F is the change in female employment as a share of the average baseline female employment share. For the average treated firm, $Y_{i,-1}^{F} = Y_{i,-1}^{M} = 1$.

Female Shares with a Moving Denominator We also report the female share of weeks worked, computed each year over that year's total. It is the number of weeks worked by women in the firm in year t, divided by the number of weeks worked by all its workers in the same year. Unlike Y_{it}^{F} and Y_{it}^{M}, whose denominator is fixed in the baseline year, this share has a denominator that changes every year. It rises both when female employment grows and when male employment falls, so it cannot tell the two apart. It also weights each worker by the time spent in the firm during the year, so women hired late in the year or for a short time count less. Coefficients are changes in percentage points. The female share of weeks worked rises by 1.5 percentage points after the award (s.e. 0.5), from a baseline of 13.3 percent, and the effect grows over time (Figure A5).

From the Coefficients to the Female Share The female share of employment is

$$s_{it} = \frac{F_{it}}{F_{it} + M_{it}} = \frac{\overline{s}_{-1}^{F} Y_{it}^{F}}{\overline{s}_{-1}^{F} Y_{it}^{F} + \overline{s}_{-1}^{M} Y_{it}^{M}}.$$

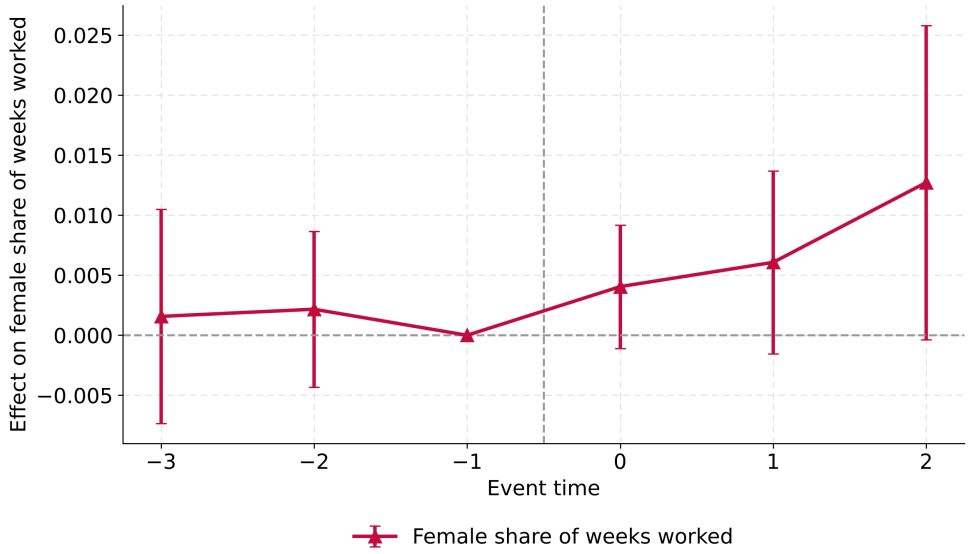


Figure A5: Female Share of Weeks Worked

Notes: The figure reports estimates of β_k from equation (2), which compares 685 treated firm-contracts with their matched controls and includes firm-by-contract and pair-by-year fixed effects. Event time 0 is the award year, and the year before the award is the reference period. Earlier and later years are binned into endpoint coefficients, which are not reported. The estimate two years after the award uses only cohorts observed for that long. Vertical bars are 95 percent confidence intervals based on standard errors clustered at the firm level. The outcome is the number of weeks worked by women divided by the number of weeks worked by all workers in the same year.

We linearize this expression around the average treated firm in the baseline year, where $Y^F = Y^M = 1$. This assumes that the firm's baseline gender shares equal the sample averages and that the changes are small. Under these assumptions,

$$\Delta s \approx \overline{s}^F_{-1}\overline{s}^M_{-1}\left(\beta^F - \beta^M\right).$$

The female share rises when the effect on normalized female employment exceeds the effect on normalized male employment. With $\beta^F = 0.086$, $\beta^M = -0.003$, and $\overline{s}^F_{-1} = 0.111$, the implied increase in the female share is about 0.9 percentage points. The approximation is less accurate for firms whose baseline female share is far from the average, and for large changes.

D Contract Completion Time

We compare the completion time of each treated contract with that of its matched control. The analysis is at the contract level, not at the firm-year level.

Cox Model Our preferred specification is a survival model that includes both completed and ongoing contracts. Time starts on the recorded starting date, completion is the event, and contracts not yet completed are right-censored at the latest data extraction date. We estimate

$$h_{cp}(s) = h_{0p}(s) \exp\left(\beta Q_{cp} + \gamma D_{cp}^{plan}\right),$$

where $h_{cp}(s)$ is the probability that contract c in pair p is completed at duration s, given that it is still ongoing, Q_{cp} equals one for the contract with the quota, and D_{cp}^{plan} is planned duration, omitted in the unadjusted specification. The model is stratified by matched pair, so β compares the two contracts within each pair. We report the hazard ratio $\exp(\beta)$. A value below one means slower completion. It is not a percentage change in duration, and its interpretation relies on the ratio being constant over the life of the contract.

OLS Model As a complement, we regress realized duration in days, D_{cp}^{actual}, on Q_{cp} with pair fixed effects, with and without planned duration, for pairs in which both contracts are completed. The coefficient is the average within-pair difference in days. Unlike the Cox model, it drops pairs with an ongoing contract and may underrepresent longer projects. In both models, standard errors are clustered at the firm level. Table 4 reports the estimates.