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Abstract

The success of board quotas regulation in promoting gender balance hinges on whether its effect extends beyond the (few) firms and jobs directly targeted by law. So far, research has found no evidence of *vertical spillovers*, that is, indirect effects on workers within targeted firms. We study *horizontal spillovers,* i.e., the effects on boards of firms not directly targeted by the quotas. We examine the 2011 Italian law mandating gender quotas on boards of listed and state-controlled enterprises (target companies). We define "connected" firms as non-target companies that shared at least one board member with target companies prior to the reform. Employing a difference in differences design, we find that connected firms significantly increase the share of female board members post-reform compared to similar non-connected firms. Accounting for these horizontal spillovers, the effect of the reform on the number of female directors is at least twice as large as that computed for target firms alone, which amounts to approximately 2,500 additional women on boards. Our results suggest that the quotas law indirectly expanded the supply of candidates for directorship positions available to connected firms, rather than increasing their demand for gender diversity on the board. We show evidence that the spillover is largely due to information sharing between target and connected firms.

Keywords: Gender quotas laws, spillover effects, board interlocks

JEL classification: J24, J7, J78

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1 Introduction

"Listed companies have a particular economic importance, visibility and impact on the market as a whole. Such companies set standards for the wider economy and their practices can be expected to be followed by other types of companies" (Directive 2022/2381 of the European Parliament).

Board quotas in listed companies have become a standard tool to foster gender balance in the corporate world. An increasing number of countries has followed Norway, first to mandate gender quotas for boards of publicly listed companies in 2003.[1] Being mandated, most listed companies did indeed increase the share of female directors on boards. But legislators hoped that the impact of these reforms would reach beyond just the boardrooms of the companies involved. They aimed for these changes to support the advancement of other female employees within these firms and, more generally, to inspire younger women by providing role models. As the European Union 2022 directive states, "Enhancing women's participation in economic decision-making, on boards in particular, is expected to have a positive spill-over effect on women's employment in the companies concerned and throughout the whole economy". Contrary to policy makers expectations, research so far has failed to find evidence of these trickle down effects. As noted by Bertrand et al. [2019] in their study of the effects of the 2003 Norwegian quotas law: "Overall, seven years after the board quota policy fully came into effect, we conclude that it had very little discernible impact on women in business" (p. 191).

[1]The list of countries mandating board quotas include Norway (2003), Iceland (2010), Belgium (2011), France (2011), Israel (2011), Italy (2011), India (2013), Germany (2015), Austria (2017), Portugal (2018), Greece (2021), Korea (2021), the Netherlands (2021), Malaysia (2021) and Spain (2023). In 2022 the EU has adopted a directive establishing that no later than June 2026, all large listed companies in EU regulated markets will have to take measures to increase the presence of women in leadership positions. In the US, the only State that has mandated board quotas is California, which in 2018 required that publicly traded corporations headquartered in California included at least one woman on their boards by the end of 2019 and at least two (three) women on boards with five (at least six) members by the end of July 2021. The law was challenged and ruled unconstitutional in 2022.

Maida and Weber [2022] reach the same conclusion as Bertrand et al. [2019] for Italy, where the scope for enhancing female career prospects is presumably larger compared to Norway, due to Italy's more conservative gender attitudes. The lack of robust empirical evidence on positive externalities fuels the ongoing controversy over quota policies in the corporate world. [2]

To inform this debate, we investigate the presence of spillovers of mandated board quotas in a novel direction: we focus on *horizontal* rather than *vertical spillovers*. Specifically, we analyze firms that are not legally subject to the quota requirements, i.e., non-target firms, but are connected to target firms through board overlaps. Previous literature shows that board overlaps can signal the presence of interactions among firms, which typically involve information exchange across management teams (Cai and Sevilir [2012], Cabezon and Hoberg [2022], Gopalan et al. [2022], Geng et al. [2024]), and often facilitate the diffusion of corporate governance practices (Bouwman [2011]; Coles et al. [2020]). We point to this information sharing channel, along with the persistence of firms' interactions, as the likely vehicles for the spillover effects.

Using Italian data on the population of listed and non-listed joint-stock companies between 2003 and 2022, we examine the effects of the 2011 reform that required listed and state-controlled companies (the target firms) to have a minimum 20% share of women on board initially, gradually lifting it up to 40% after 2019. We use individual-level information on the identity of board members in target and non-target firms to detect companies' ties through directors that sit in more than one board. Employing a difference in differences design, we show that non-target companies that share at least

[2]For example, the evidence emerging from international studies was considered crucial by the Los Angeles Superior Court that, in 2022, struck down the board quotas law for companies headquartered in California, enacted just two years prior. The court's sentence notes that available academic studies fail to support the claims that the "use of a gender-based classification is necessary to boost California's economy [and] improve opportunities for women in the workplace" (*Crest v. Padilla*, Case No. 19STCV27561). Without a demonstrable "compelling state interest", the court concludes, board quotas constitute unlawful sex-based discrimination.

one director with target companies *before* the reform (labeled *connected*) experience a significant increase in female representation as compared to other non-target companies (labeled *non-connected*) after the introduction of board quotas. These results are robust to various matching techniques aimed at eliminating potential omitted variable biases. Importantly, we show that the effects mostly arise from connections with companies that were most affected by the reform, i.e., target companies with less than 20% women on board as of 2011. Connections through shared directors are distinct from connections through ownership: only a small fraction of connected firms have target companies among their shareholders and the results are unaffected when they are dropped from the sample. Additionally, our results do not depend on whether firms operate in the same geographical area and/or the same sector as target companies, implying that the effects of connections are not due to proximity.

We document the specific dynamic features of these positive horizontal spillovers. Female representation in boards of connected firms grows significantly faster than that in non-connected firms in the years following the reform. The differential effect in female share increases over time reaching a point estimate of 2.6% in 2022, approximately a 25% increase compared to the average share of women on boards of connected firms in 2011. This result implies that, between 2011 and 2022, connected firms bring on their boards about 1,800 additional women - an effect that is of the same order of magnitude as the direct effect of the reform on the boards of target firms, which we estimate to be between 1,500 and 2,600. We then study second-order spillover effects - from non-target connected to other non-target companies with which they share a director (i.e., connected to connected). We also find evidence of statistically significant second order spillovers effects, smaller in magnitude than first order effects but large enough to add over 700 women to the boards of non-target companies. Summing first and second order effects, the horizontal spillover is of at least the same magnitude as the direct effect of the reform - a sizable "side" effect of the board quotas law. Though hard to quantify due to lack of data on private boards outside of our setting, the scope for these spillover effects is potentially large in many economies as board overlaps are a very common feature of the corporate world.[3]

Positive spillovers between target and non-target connected companies are neither granted nor obvious in sign. In principle, they may be negative: listed companies, forced to comply with the gender quotas law, may poach female directors from the boards of non-target connected firms. Thus, our findings rule out that mandating quotas on target companies crowds out the presence of women on the boards of companies not subject to the law. Additionally, the evidence of positive horizontal spillovers marks a difference compared to the lack of evidence on vertical spillovers in previous studies (including in the context of the Italian reform by Maida and Weber [2022]). We show that mandating board quotas may promote women managerial careers through "contagion" across firms, a channel that is new to the academic literature but that, interestingly, policy makers seem to have in mind when setting quotas on listed companies.

We next investigate the mechanisms behind the positive spillover. One potential explanation is that connected firms tend to hire directors from target companies, for example to keep active relations with influential firms. Given that, after the reform, target companies have more women on board, the chances that connected companies hire a woman from their boards also go up, explaining the spillover. This "supply-side" composition effect accounts for approximately 27% of the total effect, implying that the goal of preserving strategic connections is a relevant spillover mechanism. However, our results indicate that it is not the only one. After the reform, connected firms significantly increase their propensity to hire women that do not hold positions in the boards of target firms. This suggests the presence of additional channels.

[3] For example, Ewens and Malenko [2024] document that a significant share of directors in U.S. VC-backed startups has current or past experience in public boards.

We examine the possibility that connected firms learn from the (costly) effort of target companies in their intensive search for qualified women to serve on their boards. Our data show that this search was conducted outside of the pool of current female board members, as target companies comply with the law largely by hiring women with no previous directorship positions in public boards rather than having existing female directors serving in a larger number of boards. Moreover, there is growing evidence that compliance with board quotas forced companies to adopt a more professional approach to board members selection (e.g., Wiersema and Mors [2016], Ferreira et al. [2017], Ferrari et al. [2022]). In particular, target firms had to rely more on professional head hunters when searching for board members, rather than focusing on candidates within the social network of current managers and directors.[4] This broader exploration into the market for potential directors (especially female) typically results in selected lists of suitable candidates in excess of the positions to fill, producing information that can be shared with connected firms. This ultimately expands the pool of candidates from which connected firms can draw when renewing their boards, leading to more female directors and less network-based hiring. The characteristics of newly appointed female board members in connected firms lend support to this mechanism. We find that they are more likely to be outsiders, to be foreigners and less likely to be born close to the headquarters of the company, suggesting that connected firms have access to a broader pool of candidates after the reform. They are also more professionally experienced and less likely to have previous ties with company directors. Interestingly, similar effects are also present among newly hired male directors in connected firms, suggesting that

[4]Knyazeva et al. [2013] show that companies rely heavily on the local labor market for potential directors when hiring new board members. In the related literature on executives hiring, Cziraki and Jenter [2022] and Yonker [2017] show that CEOs are hired mostly locally and from a small pool of candidates who are either insiders or executives who current directors have previously worked with. Similarly, Sauvagnat and Schivardi [2024] show that thick local labor markets for managers allow for smoother transitions to a new executive following the sudden death of a current one, alleviating the negative effects on firm performance.

the spillover originates in a more professional approach to board members selection, rather than a change in attitudes towards female directors. All in all, the quotas law broadened the pattern of search for skilled directors to a larger set that included more women, unearthing the managerial talent in the half of the population traditionally under-represented in company boards. This newly produced information spilled over to connected companies not targeted by the law.

Related Literature Our paper contributes to the literature that studies the effects of gender quotas in corporate boards. A set of papers focuses on vertical spillovers, that is, on the effects of board quotas on gender differences in lower ranks within the firm. Bertrand et al. [2019] study the introduction of a gender quota for board members in Norway while Maida and Weber [2022] do it for Italy. Neither paper finds evidence of vertical spillovers. Other papers examine firm performance in target firms, and largely confirm the negative effects documented by Adams and Ferreira [2009]'s seminal paper on gender diversity in the board. Ahern and Dittmar [2012] find that the Norwegian reform had a negative impact on firm valuation and operating performance.[5] Similar conclusions are reached by Matsa and Miller [2013], again for Norway, and by Hwang et al. [2018] for US firms headquartered in California after the 2018 reform. Taken together, these results offer a rather disappointing picture of gender quotas. These reforms do not appear to foster gender balance in the workforce and may impose significant costs in terms of corporate performance. Additionally, von Meyerinck et al. [2021] find that the negative effects on stock prices extend to firms not directly targeted by the California reform, i,e., listed firms headquartered outside of California, and suggest that the mechanism behind this horizontal spillover rests

[5] Eckbo et al. [2022] rivisit the evidence from Ahern and Dittmar [2012] accounting for contemporaneous cross-correlation of stock returns and find no significant stock market reactions to the quota policy announcements.

on markets' anticipation of similar quota laws being implemented in other U.S. states. Our findings focus on board composition in non-target companies and deliver a more optimistic picture in terms of labor outcomes. They indicate that the policy increases female representation even in the boards of companies for which the new law is not binding.

Two recent papers look for evidence of horizontal spillovers of the same reform we analyze, both focusing on the Italian banking sector. Bongiovanni et al. [2023] find that, after the reform, there is a change in the trend of female representation in non-listed banks. However, their analysis does not have a control group and therefore cannot account for a possible increase in the overall female representation in boards, independent from the law. Del Prete et al. [2022] find that unlisted banks belonging to a listed group did not record a differential change in female representation when compared to unlisted banks not belonging to a listed group. Their evidence suggests ownership linkages do not necessarily imply that target firms extend the application of the law to not non-target owned firms, at least in the banking sector. Our analysis studies spillovers through shared directors rather than ownership linkages among firms and uses a much richer dataset, comprising all incorporated companies in Italy, that allows us to implement a very robust empirical design.

Finally, our finding that mandatory board quotas improved the market for directors, especially female ones, is confirmed by other papers in the literature. Ferreira et al. [2017] show that, in France, "female directors hired after the quota are more independent, more experienced, more internationally diverse, and no less academically qualified than those hired before the quota." Bertrand et al. [2019] find that female directors appointed after the Norwegian reform were more qualified and better paid. Giannetti and Wang [2023] find that, in the US, female director appointments following heightened public attention to gender equality do not dilute board's skills. Again for the US, Gormley et al. [2023] show that the campaigns launched by large institutional investors in 2017 to increase board diversity were met by the selection of female candidates beyond managers' existing networks. Gertsberg et al. [2021] show that, after the introduction of board quotas in California, companies were able to hire female directors that shareholders largely approved of. Most relevant for this study, Ferrari et al. [2022] find that, after the implementation of quotas in Italy, education increases and age decreases on average for *all* board members, and conclude that the reform affected the recruiting process for the entire board.

The rest of the paper proceeds as follows. In Section 2 we discuss the Italian 2011 board quotas law and provide some institutional background. In Section 3 we describe the data, show evidence of compliance to the quotas laws by target firms, and define our core sample of connected non-target firms. In Section 4 we present the empirical model used to test for spillover and show the main results. In Section 5 we discuss possible mechanisms. Section 6 concludes.

2 Institutional Background: The Golfo-Mosca Law

In 2011 the Italian legislators modified the corporate governance laws with the explicit goal of addressing gender imbalances in the composition of managerial and supervising bodies of listed and state-controlled companies. Specifically, the law aimed at re-balancing access to to corporate jobs in favor of women, who are typically under-represented in directorship positions.

Different from otherwise similar quota-laws in other jurisdictions, the Italian Golfo-Mosca law (by the name of its sponsors) had temporary validity as it regulated the gender ratios of newly appointed board members only for the three consecutive board elections taking place in target companies *after* the enactment of the law.[6] The under-represented gender is assigned one-fifth of the board seats in the first board renewal, and one-third of the board seats in the following two. According to the Italian corporate law, the board comprises both a directors' board and an auditors' board, and the mandatory quotas apply to both sub-groups.

Board members can serve for a maximum period of three years after their (re)election. Therefore, in its initial formulation the law was effectively binding only for a limited amount of time (i.e., 12 years at most). However, in 2016 it became permanent for state-controlled companies, though its applicability after the transitory period was limited to the board of directors only (i.e., the board of auditors is not subject to quotas). For listed companies, the law was extended to three additional board renewals and the quota raised to 40% of board seats in 2019.

In the case of listed companies, enforcement of the Golfo-Mosca law falls under the responsibility of the Italian securities markets oversight agency (CONSOB). Non-compliant companies receive an initial warning; if they fail to comply, they face a pecuniary sanction ranging from €100,000 to €1 million, and the election that resulted in a non-compliant board is declared void.

For state-controlled companies, compliance supervision with the Golfo-Mosca law is overseen by the executive branch of the government, specifically either the prime minister or the ministry for equal opportunities. While non-compliant state-controlled companies do not face fines, their boards are declared void if they fail to adhere to the prescribed quotas.

[6] The law was enacted on August 12th 2011 for listed companies and on February 12th 2012 for state-controlled companies.

3 Data Sources and Descriptive Statistics

In this section, we describe the data sources, document compliance in target firms and describe the core sample of firms that are not subject to the quota law requirements.

3.1 Data sources

Our main data is the firm register administered by the local chambers of commerce where all Italian incorporated companies must register and deposit their balance sheets and flow of funds, as well as detailed information on company owners and board members. Annual data from 2003 to 2022 are obtained from Cerved, a private data provider. We limit our sample to joint stock companies and focus on data from 2006 onwards, five years before the quotas law.[7] For each firm-year observation in the sample, we retrieve financial statements and firm demographics (e.g. headquarters location, industry classification, year of foundation etc), board composition, and ownership information. In particular, for every company we observe the identity (i.e., the social security number) of each board member, their gender, date and place of birth, current address, role within the board (e.g., president, director, auditor, etc.) and any additional direct relationship with the company (e.g., director general, shareholder, CFO, etc.). We classify as outsiders those board members that have no link to the company other than through their seat in the board. Thus, our definition of outsider excludes executive board members that are employees, shareholders, and creditors (and their representatives), but does not perfectly overlap with the legal definition of independent board member as we do

[7] In Italy, there are two forms of incorporation: *Società per Azioni* (joint stock company) and *Società a Responsabilità Limitata* (limited liability company). The joint stock company is the typical legal form of corporations, and all listed companies must be joint stock companies. Usually, when firms grow, they also become joint stock companies. *Società a Responsabilità Limitata* is an hybrid between incorporated and non incorporated legal forms. They enjoy limited liability, but in terms of management they are more similar to non incorporated companies, where there is no separation between owners and managers. As such, their boards are typically made up directly of owners. We therefore exclude them from the analysis.

not observe all possible ties between individuals and the firm (e.g., supplier-customer relationships). Ownership information includes the unique social security number of each shareholder, whether individuals or other legal entities, and their ownership share. For individual shareholders we observe their demographics (gender, date and place of birth, current address), while for other legal entities we observe all information included in the firm register provided that they are registered in Italy.

Using information on ownership, we identify state-controlled firms as firms where the largest shareholder is a public entity (e.g., the ministry of finance, state development banks, municipalities).[8] We refer to listed companies as those firms that have shares listed on the main Italian stock exchange. We exclude from this definition companies listed on the "growth" segment as they are exempt from corporate governance requirements and are not subject to board quotas. We refer to state-controlled and listed firms jointly as *target* firms (i.e., firms that are required to comply with the quota law) and to the other companies as n*on-target* ones.

3.2 Compliance in Target Firms

Both listed and state-controlled Italian companies promptly complied with the Golfo-Mosca law. Figure 1 shows the share of female board members in target companies between 2006 and 2022. Following an initial moderate upward trajectory, the trend shows a clear discontinuity in level and slope right after the enactment of the law. For listed companies, the share of female board members increased by 2 percentage points, from 5% to 7%, in the 6 years between 2006 and 2011, jumped to 12% in 2012, the first year the law was implemented, and then proceeded to raise up to over 40% in 2022. For state-controlled firms, the share of female board members increased by 3 percentage

[8]To correctly identify public entities we use the official list provided by the ministry of finance (see https://www.dt.mef.gov.it/it/attivita_istituzionali/partecipazioni_pubbliche/)

points (from 8% to 11%) in the 6 pre-reform years, and by approximately 20 percentage points in the 10 post-reform. In light of the speed of the changes illustrated in Figure 1, it appears reasonable to conclude that such a sizable shift in gender composition within the boards of target companies could have not occurred in the absence of a regulatory intervention.

Importantly, target firms achieved compliance by hiring *new* women as directors, rather than decreasing the size of the board to inflate the female share or appointing the same woman on multiple boards (a phenomenon often referred to as "golden skirts"). Figure 2 shows the average number of male and female board members in listed (Figure 2a) and state-controlled (Figure 2b) companies over time. For listed firms, despite a small and smooth downward trend in the average size of the board (from 14 to 13 members over a 17 year period), the drastic change in female representation is due to the significant increase in the average number of female board members (from less than 1 in 2011 to over 5 in 2022). For state-controlled firms, the size of the board shrank from 10 to 7 members over time due to regulatory changes in the maximum number of board members allowed. However, the average number of female board members remained constant (approximately 1 woman) before the reform, and progressively increased to over 2 members after 2012.

The figures above suggest that, roughly speaking, each listed (state-controlled) firm hired 4 (1) additional women over a period of 10 years in order to comply with the new quotas. Figures 3 and 4 show that this rise in female share is not due to an increase in multiple appointments. Said differently, compliance is achieved by expanding the pool of female board members (extensive margin) rather than drawing more intensively from the existing pool. To see this, we first examine new entrants in the pool of board members defined as individuals who appear in the board member registry for the first time in a particular year. Figure 3 shows that the share of women among new entrants increases drastically after the reform, especially for listed firms (left panel), i.e., those with more quota-vacancies to fill, but also in state-controlled firms (right panel). Second, we show that the average number of appointments (where we take into account appointments both in target and non-target firms) of female board members does not change following the reform (Figure 4). Overall, this evidence suggests that Italian companies responded to the mandatory gender quota requirements by replacing (some) male board members with female ones, and by tapping into an ample pool of women that did not previously access board-level jobs.

3.2.1 Measuring the Direct Effects of the Reform

There are at least three ways to quantify the direct effect of the quota law on female representation in the boards of target firms. The first is to simply compute the difference in the number of board positions filled by women between 2011 and 2022. This difference amounts to 1,540 positions, implying an increase of +132% with respect to 2011. This figure, however, does not account for two important issues. The first is that the number of state-controlled firms and the average size of their boards decreased substantially over that time period due to the recent government efforts to rationalize state holdings in Italian companies. Second, as shown in Figure 1, female representation was growing in target firms even before the reform, albeit at a much slower pace. To correct these biases, one can compute a simple alternative estimate that accounts for both exogenous changes in the number of target firms and pre-trends. The change in female representation directly attributable to the reform can be roughly estimated by subtracting from the overall increase in female share between 2011 and 2022 (34% and 20% for listed and state controlled companies, respectively) that of the pre-reform period (3.3% and 5.8%, after adjusting for the different time length). These calculations yield pre-trend adjusted growth rates of 31% and 14%. After multiplying the adjusted growth rates by the number of boards and average board size of listed and state controlled firms *as of* 2011, we obtain an effect of 2,610 additional women on target companies boards. In other words, absent changes in the number of firms subject to quotas, the reform had the potential to generate 2,610 new board positions for women (+223% with respect to 2011).

3.3 Core Sample: Connected vs Non-Connected Firms

Our core sample comprises non-target firms not currently undergoing liquidation or restructuring, totaling approximately 26,000 joint-stocks non-listed companies per year. Connected firms within this sample are identified by examining board overlaps in the years preceding the implementations of reform. Specifically, we define the variable $Connected_i$ as a dummy variable that equals 1 if firm i shared at least one board member with a target firm in the years 2009, 2010, or 2011. We use the three years prior to the reform as our reference point in defining the treatment variable ($Connected_i$) to avoid running into potential endogeneity issues. For example, firms may seek a connection with target firms *after* the reform precisely to have access to female directors, who become more common in target firms with the introduction of quotas. Approximately 35% of firms in the core sample are classified as connected.

Figure 5 shows female representation in the boards of connected and non-connected firms over time, both in average shares (panel 5a) and in average differences with respect to 2011 shares (panel 5b).The patterns that emerge are strikingly different between the two types. Non-connected firms increase their share of female board members smoothly between 2006 (when the share was around 15%) and 2022 (when it reaches 19%). This 4% advancement in female representation is spread evenly across the 17 years in our sample. The trend for connected firms instead is somewhat flatter for the years prior to the reform, and it increases sharply afterwards, with the share of female board members rising from 13% to 19% between 2012 and 2022 (over the same period, non-connected went form 17% to 19%). In other words, it appears as if the female board share in connected firms move to a different, steeper trend after the reform, despite not being directly subject to it, while in non-connected firms continues on the same path as before.

While this divergence seems to originate in pre-reform connections with target companies, as we will argue more rigourosly in the next sections, it may also be explained by other correlated factors, e.g., firm size. We examine the main characteristics of connected and non-connected firms in Table 1. As expected, connected firms have larger and busier boards, larger book values of assets and are more likely located in the North of the country, i.e., the area with larger population and denser economic activity. There are no sizable differences instead in the industry they operate in (manufacturing for about 54 percent in both groups). Connected firms are also more likely to have a target company among its shareholders, and, to a lesser extent, to own shares in listed firms. But it is worth stressing that ownership links only very partially explain connections. Only 7% (3%) of connected firms are owned by listed (state-controlled) companies, and only 1% of them owns shares in listed companies. For comparison, 1% (0%) of non-connected firms are owned by listed (state-controlled) companies, and (almost) none of them holds shares of listed or state-controlled companies.

To alleviate concerns over possible confounders, we build a matched sample of treated (i.e., connected) firms and non-connected firms with high estimated probability of treatment. In particular, we use the following cross-section logit specification to compute propensity scores in the pre-reform period for our $Connected_i$ variable:

$$Pr\left(Connected_i = 1\right) = f\left(\gamma X_i + \varepsilon_i\right) \tag{1}$$

where γX_i includes the average board size and the average number of appointments of board members in years 2009 to 2011. We select non-connected firms with propensity scores higher than the connected sample median and include them in our matched sample, along with connected firms. Table 2 illustrates the main characteristics of connected and matched non-connected firms. The matching algorithm significantly reduces differences between the two groups, most noticeably in terms of firm size as measured by board size and assets. In the next section we present our main results based on both the full and the matched sample. In Section 4.4 we show the results of robustness tests using alternative matching techniques.

4 Spillover Effects of Mandatory Quotas

In this section we first illustrate the main results and then perform some robustness analysis.

4.1 Main Results

To test the hypothesis of quota spillover effects through connections with target firms, we examine the difference in the trends of female shares in the boards of connected and non-connected firms in the core sample. Specifically, we estimate the following two-way fixed effects difference in differences model

$$F\_Share_{i,t} = \sum_{s \neq 2011} \beta_t 1_{\{s=t\}} Connected_i + \delta X_{i,t} + \alpha_i + \gamma_t + \varepsilon_{i,t} \tag{2}$$

where $F\_Share_{i,t}$ is the share of female members in the board of non-target firm i in calendar year $t \in [2006, 2022]$, $X_{i,t}$ is a vector of firm-level time-varying controls for board size and average number of appointments of the firm's board members, α_i are firm fixed effects, γ_t represent year dummies and $1_{\{s=t\}}$ is a dummy equal to one for $s = t$. We use 2011, the year before the Golfo-Mosca started to be implemented, as the base year. Parameters β_t trace the pattern of the difference in the dynamic of share between connected and non-connected firms in the 5 years before and the 11 years after the reform.

Figure 6 displays the estimated coefficients $\hat{\beta}_t$ using the full sample (Panel 6a) and the matched sample (Panel 6b). The results in Panel 6a show that non-connected firms were increasing their share of female board members at a somewhat faster pace than connected firms before the reform, leading to a 0.5% larger growth in female representation between 2006 and 2011. This negative pre-trend *inverts* its course after the reform. Beginning in 2012, connected firms display a significantly steeper trend, and, by the end of the sample period, the share of female board members increases by 3% more than non-connected firms compared to 2011 (i.e., $\hat{\beta}_{2022} = 2.6\%$). Hence, the post 2011 dynamics in the estimated $\hat{\beta}_t$ do not arise as the continuation of a pre-existing trend, which, if anything, was heading in the opposite direction. We obtain similar results when we use our matched sample (see Panel 6b). The total post-reform estimated differential increase in female representation is 2.4% (i.e., $\hat{\beta}_{2022} = 2.4\%$). Interestingly, in this case the pre-trends are flat - all estimated coefficients $\hat{\beta}_{t<2011}$ are not significantly different from zero - suggesting that, prior to the reform, the widening gap between connected and non-connected firms (i.e., the negative pre-trend) was likely related to differences in firm size. In other words, the path towards gender balance in corporate boards was particularly slow in large firms before the reform.

Our results suggest that the quota reform had a strong causal impact on the pace of gender rebalancing within the boards of connected firms, implying a substantial horizontal spillover from target to non-target firms. Table 3 revisits the evidence in Figure 6a by examining different outcomes in the full sample (Panel 3a) and in the matched sample (Panel 3b) with the following specification

$$Y_{i,t} = \beta Post_t \cdot Connected_i + \gamma X_{i,t} + \alpha_i + \gamma_t + \varepsilon_{i,t} \tag{3}$$

where $Post_t = 1$ if $t \geq 2012$ and zero otherwise.

In column (1) we consider $Y_{i,t} = \Delta Share_{i,t} \equiv (F\_Share_{i,t} - F\_Share_{i,t-1}) * 100$, i.e., the year on year variation in the share of female board members in firm i. In column (2) we set $Y_{i,t} = 1$ if $F\_Share_{i,t} > 0$ and zero otherwise, i.e., we examine the probability of observing at least 1 female board member in firm i at time t. In column (3) we set $Y_{i,t} = F\_Share_{i,t}$- the board female share. In columns (4) and (5) we examine year on year changes in the number of male ($\Delta\, M.BM$) and female ($\Delta\, F.BM$) board members respectively.

The positive and significant estimate of coefficient $\hat{\beta}$ in column (1) confirms the results in Figure 6 and suggests that in the 10 years after the reform the presence of women in the boards of connected firms increases by approximately 0.3 percentage points more per year on average as compared to non-connected firms. The result in column (2) reveals that $F\_Share_{i,t}$ increases not only in the intensive margin (see column (3)) but also in the extensive margin, i.e., we observe a positive effect of the reform on the probability of hiring a female board member in male-only (connected) boards. Finally, columns (4) and (5) show that connected firms are more likely to hire new women than men after the reform.

Second order spillovers Taken together, the evidence so far shows that the 2011 board quotas reform had a significant horizontal spillover on firms *directly* connected to companies targeted by the reform through shared directors. Next, we examine second-order spillover effects, that is, spillovers to other non-target companies that are *indirectly* connected to target companies because they share a director with connected companies. We define the variable $Connected2_i$ as a dummy that takes value 1 if firm i had at least 1 board member in common with a *connected* firm in years 2009, 2010, or 2011. We remove observations where $Connected_i = 1$ from the core sample and we estimate equation 2 using our second-degree connection variable as treatment. The results are presented in Figure 7a, and suggest the presence of second-order spillover effects, though milder in magnitude than the first-order ones, as one would expect. In particular, while there is no significant difference in trends between second-degree-connected and non-connected firms before the reform, second order connected firms set on a steeper trend after 2011, reaching a point estimate of 1.4%, significant at the 1% confidence level, in 2022.

In Figure 7b we repeat the same exercise using a matched sample of second-degree connected firms and non-connected firms.[9] Though in this sample the process appears to evolve at a slower pace and estimates are more noisy, the final point estimate is close in magnitude to that computed above ($\hat{\beta}_{2022} = 1\%$) and highly significant.

Overall spillover effect We summarize the total horizontal spillover summing first and second oder effects. Such effects can be approximated as follows

$$\Delta F_{2011-2022} = \sum_k \hat{\beta^k}_{2022} * \#Boards_{2011} * \overline{BoardSize}_{2011}$$

where $\Delta F_{2011-2022}$ is the additional number of female board members due to the spillover effect of the reform, k is the estimated spillover's order [first, second],$\hat{\beta^k}_{2022}$ is estimated

[9] To avoid large excessive imbalances between the second-order connected firms (the vast majority of the sample) and non- connected firms, here we use 3-nearest neighbors propensity score matching. Specifically, we use the following cross-section logit specification to compute propensity scores in the pre-reform period for our $Connected2_i$ variable

$$Pr\left(Connected2_i = 1\right) = f\left(\gamma X_i + \varepsilon_i\right)$$

where γX_i includes the average values of board size and number of appointements of board members in years 2009 to 2011.

with equation 2, $\#Boards_{2011}$ is the number of boards in connected firms in 2011, and $\overline{BoardSize}_{2011}$ is the average size of connected firms' boards in 2011. To provide the most conservative estimates, we use the lowest value of $\hat{\beta^k}_{2022}$ between the full and the matched sample. The total increment of women on boards generated by the reform's spillovers is approximately 2,550. This is 98% to 166% as large as the estimates for the direct effects of the law on female representation in target firms provided in Section 3.2.1.

4.2 Exposure to the Reform

As we explain in Section 2, target firms are not equally affected by the reform. First, enforcement is arguably stricter for listed companies, as violations are punished with substantial pecuniary fines. Second, starting from 2016 and only for state controlled firm, the quota of $\frac{1}{3}$ becames permanent but it only applies to directors, exonerating from the requirement the auditors, who usually represent 25%-30% of the board. Thus, quotas are less stringent for state controlled firms starting from 2016 and onward. Additionally, an amendment to the law in 2019 increased the quota for listed firms to 40%. We expect spillover effects to reflect these differences. In particular, firms that in the pre-reform period were connected through board overlaps with listed firms should display a larger increase in the share of female board members than firms connected to state controlled firms, particularly after year 2016. This is indeed what we observe. Figure 8 shows coefficient estimates for $\hat{\beta}_t$ from equation (2) computed separately for firms connected to listed companies and those connected to state controlled entities. Coefficients for the first group are larger after the reform but diverge in a statistically significant manner from the second group only starting from 2016. By the end of the sample period, $\hat{\beta}_{2022}$ is equal to 3.5% (1.6%) for firms connected to listed (state controlled) companies.

It is also important to consider that, as of 2011, some of the target firms, both listed and state controlled, had already a share of female board members that was large enough to satisfy the quota requirement for the first board renewal (20%). These firms were presumably drawing directors from a more gender balanced pool to begin with, and to meet the quotas they simply had to either retain the same directors or keep hiring from the same pool of candidates as before. The rest of the target firms, instead, faced a much larger shortfall, that had to be addressed with more aggressive changes to their current recruiting practices. Consequently, we conjecture that the spillovers documented above mostly involve non-target firms connected to target firms that, at the time of the enactment of the law, had less than 20% female board members. To identify this heterogeneity in treatment, we define the dummy variable $Exposed_i$, which takes value 1 if any of the board members in connected firm i sat in the board of a target firm with a share of women less than 20% in 2011. We estimate the following model

$$F\_Share_{i,t} = \sum_{s \neq 2011} \beta_t 1_{\{s=t\}} Exposed_i + \delta X_{i,t} + \alpha_i + \gamma_t + \varepsilon_{i,t}$$

on the sample of connected firms. Figure 9 displays the estimated coefficients $\hat{\beta}_t$, and shows a total post-reform estimated differential increase in female representation of 2.8% (i.e., $\hat{\beta}_{2022} = 2.8\%$), marginally larger than our baseline estimate.

Notice that, by restricting the sample to connected firms only, we reduce the number of control firms to approximately 12% of the connected sample. At the same time, however, we increase the comparability between the control and the treated group. This is evident from the absence of pre-trends in the estimates, and suggests that the spillovers cannot be attributed to unobservable differences between connected and non-connected firms, but rather originate precisely from the (indirect) exposure to the quota reform.

4.3 Timing and Intensity of Connections

The results above show that board overlaps with target firms in the years 2009-2011 are strongly associated with an increase in female representation after 2012 and onward. More generally, however, our results should hold among all firms with ongoing (rather than past) connections. To verify that this is indeed the case, we define the variable $Current\_C_{i,t}$ which takes value of 1 if firm i is currently connected to a target firm, and regress the variable $F\_Share_{i,t+s}$ on $Current\_C_{i,t}$ and its interaction with $Post_t$. We consider up to three lead values for the dependent variable, i.e. $s \in [1, 3]$, and control for board size, average number of appointments of the firm's board members, firm and year fixed effects. The coefficient estimates presented in Table 4, columns 1 to 3, show that connections active before the reform are associated with a smaller share of female board members in the following three years. Importantly, this relationship flips sign after the reform, implying that firms that share a board member with target companies after the reform are more likely to have a more gender balanced board in the following three years.

Next, we examine whether the timing of the connections with target firms plays a role in our analysis. To do so we define an additional variable, $Connected - 06 - 08_i$, which takes value 1 if firm i shared at least one board member with a target firm in the years 2006, 2007, or 2008, that is 3 to 6 years before the reform. As one would expect, this measure is highly correlated (70%) with our $Connected_i$ indicator as connections are fairly persistent over time, and approximately 88% of firms connected in 2009-2011 shared a board member with target firms in the previous three years. Column 1 of Table 4 shows the coefficient estimate for the interaction terms $Post_t \times Connected - 06 - 08_i$, where the outcome variable ($\Delta Share_{i,t}$) and the controls are the same as in equation 3. We restrict the sample to connected firms only. Our results show that the estimated base line effects (0.32%, see Table 3) are mostly due to recent connections, that is connections established in the three years prior to the reform. In particular, the point estimate for $Post_t \times Connected - 06 - 08_i$ is not statistically different from zero, i.e., connected firms with more remote connections do not behave differently from those with recent connections only.

Finally, we investigate whether the intensity of connections affects our results by constructing two additional measures. The first, $Mean\ Connection_i$, is the average number of shared board members per year in the period 2009-2011. That is $Mean\ Connection_i = \frac{\sum_t(\sum_b c_{b,i,t})}{3}$, where c is a dummy that takes the value 1 if board member b in firm i at time $t \in [2009, 2011]$ seats in the board of a target firm. For example, if we observe *one* shared board member in *only one* of the years between 2009 and 2011, $Mean\ Connection_i$ equals $\frac{1}{3}$. If we observe *one* shared board member in *each* of the years between 2009 and 2011, $Mean\ Connection_i$ equals 1. The second, $Mean\ Tot\ Connection_i$, is the average number of positions held in target companies by shared board members per year in the period 2009-2011. That is $Mean\,Tot\,Connection_i = \frac{\sum_t(\sum_b n_{b,i,t})}{3}$, where n is the number of current seats in board of target firms held by board member b in firm i at time $t \in [2009, 2011]$. Here, if we observe one shared board member in only one of the years between 2009 and 2011 but this individual sits in the board of n target firms, $Mean\ Tot\ Connection_i$ equals $\frac{n}{3}$. In columns 3 and 4 of Table 4 we show the coefficients estimates for the interactions terms $Post_t \times Mean\,Connection_i$ and $Post_t \times Mean\,Tot\,Connection_i$ in the restricted sample of connected firms only. Our results show that the effect of connections increases significantly with intensity. In particular, an increase in the $Mean\,Connection_i$ ($Mean\,Tot\,Connection_i$) measure from 0.67 to 2 (from 0.67 to 3)- i.e., from the 25th to the 75th percentile of its distribution - amplifies the baseline effect by approximately 0.08% (0.08%).

4.4 Robustness

We measure connections by tracking board interlocks between target and non-target firms before the reform, i.e., we treat common directors as an observable signal of strategic interactions among firms. Such interactions typically involve information exchange across management teams, and we point to this information sharing as the likely vehicle for the spillover effects (see Section 5). However, firms that share directors with target firms may be more likely to hire female directors after the reform for different reasons.

For example, connected firms, which are generally larger in size (see Table 1), are more likely to seek public listing in the immediate future, and therefore may be acting preemptively to comply with the new legislation. Additionally, non-target firms may be more likely connected to target firms because of proximity, e.g., if they are located in the same geographical area. If the law generates an increase in the supply of female candidates in areas where most target firms are located (i.e., the North of Italy), our results may be due to time-varying geographic factors rather than firm connections. Similarly, connections to target companies may be more likely for firms that operate in the same area and in the same sector, and the reform may have affected local labor markets differently in different sectors.

We test these alternative hypotheses in Table 5, where Panel 5a and Panel 5b show estimation results for equation 3 using the full and the matched sample respectively. The outcome variable is $\Delta Share_{i,t}$, and column 1 reports the baseline results from Table 3 for comparison. The estimates for the coefficient of the $Post_t \times Connected_i$ interaction term in equation 3 does not change significantly when we exclude from the sample firms that went public after the reform (column (2)). This is not surprising, since IPOs are a rare event, and only 1% of connected firms go public in our sample period. In columns (3) and (4) we control for two measures of proximity to target firms and interact them with $Post_t \times Connected_i$. The first, $Prox1_{i,t}$ is the number of target firms in year t that are located in the same province as firm i, divided by the total number of firms in the province. The second, $Prox2_{i,t}$ is the number of target firms in year t that are located in the same province and operate in the same sector as firm i, divided by the total number of firms in the province. Our main results are not affected by the inclusion of these controls, suggesting that proximity to target firms is not a relevant confounding factor.

It is also possible that direct ownership links with a target firm are driving our results. As one would expect, in our data firms with common shareholders are more likely to have overlapping boards, and, consequently, connected firms are more likely to either own shares of target firms or have target firms among their shareholders. Previous research shows that information exchanges often occur across commonly owned firms, generating spillovers, e.g., in innovation (Antón et al. [2024]; González-Uribe [2020]; Lindsey [2008]), as well as collusive behaviors (Azar et al. [2018]). Thus, the effects that we document may be entirely attributable to - or reinforced by - common ownership. We explore this possibility by estimating the effects of ownership links on $\Delta Share_{i,t}$ using different variations of the model in equation 3. The results are presented in Table 6. In column (1), we repeat the same exercise as in column (1) of Table 3, but we restrict the sample to only firms with no ownership links at time t , i.e., to firms that do not own shares of target firms nor have target firms among their shareholders. The coefficient estimate for the interaction term $Post_t \times Connected_i$ is barely affected, suggesting that our main results are not explained by ownership. Additionally, we define the the dummy variable $Ownership_i$ which takes value 1 if firm i had ownership links with a target firm in the three years before the reform, and we interact it with $Post_t \times Connected_i$ both in the full sample (column 2) and in the matched sample (column 3). The coefficient estimates for this triple interaction term are positive, implying that ownership may reinforce the main effects, but not significant, due to the low number of ownership links even among connected firms. Finally, in column (4) we examine the effects of the interaction $Post_t \times Ownership_i$ in the sample of connected firms only. Our coefficient estimates imply that, among connected firms, those with ownership links with target companies (approximately 7%) have a significantly larger increase in female representation on the board. Thus, while common ownership may play a role, its contribution to the overall spillover effects of the board quota reform is relatively marginal.

In Table 7 we estimate equation 3 with $\Delta Share_{i,t}$ as the outcome variable using alternative matching algorithms. We compute propensity scores in the pre-reform period for our $Connected_i$ variable using the same cross-section logit specification as in equation 1. In column (1) we show results from our base matched sample regression for comparison. In column (2) we use all firms, treated and controls, with propensity scores above the core sample median. In columns 3 and 4 we apply the same methodology, but we include average assets and sales, as well as indicators for whether the firm is located in the North and operates in the manufacturing industry as additional controls in equation 1 (full specification). Finally, in columns 5 and 6 we use 3-nearest neighbors propensity score matching. Probability scores are computed using the base and the full specification of equation 1 in column 5 and 6 respectively. The corresponding dynamic coefficients are illustrated in Figure 10. The coefficient for the interaction term $Post_t \times Connected_i$ is highly significant in all specifications and ranges between 0.27 and 0.40. The final cumulative differential effect ($\hat{\beta}_{2022}$) ranges approximately between 2% and 3%. Our baseline result ($\hat{\beta}_{2022} = 2.6\%$) sits approximately in the middle of this interval.

5 Mechanisms

The results in Section 4 show that, after 2011, connected firms progressively increase female representation in their boards by hiring relatively more women than men as compared to the years prior to the reform. These effects can originate from two different - though not necessarily mutually exclusive - mechanisms.

The first relies on the idea that the reform may have affected the *supply* of candidates that connected firms face when hiring new directors. This channel can be both direct and indirect. The direct-supply channel originates from the persistence of firm connections. Firms that were connected to target companies through board overlaps *before* the reform may want to preserve these connections even *after* the reform, and to do so they may continue hiring board members from target firms. As the reform forces the gender composition of target firms' boards to become more balanced, connected firms drawing from this set of candidates are more likely to hire women on their boards. The indirect-supply channel stems from spillovers of new recruiting processes in target companies. Previous research documents that, in order to comply with quota mandates, shareholders of target companies rely less on network-based hiring practices and "professionalize" executive search, for example by relying on consultants for candidates' selection (see for example Wiersema and Mors [2016] and Ferreira et al. [2017]). This change in search technology may spill over to connected firms as target firms share information on new sets of professionally selected candidates for directorship positions. These candidates' pools are more gender balanced – as a consequence of the quota requirements – and less likely to overlap with the firm's existing network – due to professionalization of the selection process. In other words, the quota law may have indirectly shocked the market for directors relevant for connected firms, while leaving the one for non-connected firms relatively unchanged.

The second, alternative mechanism hinges on an increase in the *demand* for gender diversity in the board. After observing the implementation of the reform in target companies, connected firms may adjust their preferences and deliberately target women in their selection of new directors and board members. This can be due to reputation concerns, or because the direct exposure to a new, more equitable approach to board members recruitment induces a change in "corporate culture".

Before showing evidence of the mechanisms, it is useful to define the two pools of candidates from which connected and non-connected firms can draw when hiring new directors. The first is the set of individuals currently seating in the board of target firms, labelled *in-target* candidates. Importantly, this pool is perfectly observable to us, as we have complete information on the board members of listed and state-controlled companies. The second is the set of all other candidates to board members, labelled *out-target* candidates. This set is not fully observable as we only have information on individuals who are eventually hired as board members. We will proceed by investigating the relevance of supply and demand mechanisms focusing on these two pools - *in-target* and *out-targed* - and moving our analysis to the individual director level.

In-target Pool. To fix ideas, let us consider a newly hired board member j in company of type c in period p. Firm type c equals 1 if the firm is connected (and zero otherwise), and $p = 1$ ($p = 0$) denotes all the years after (before) the reform. Let us further define the indicators F_j, which takes value 1 if board member j is female, and $InTarget_j$, which takes value 1 if j currently seats in the board of a target company (we suppress the firm and time subscripts for ease of exposition). From our main results, we know that the probability of hiring women after the reform increases more in connected firms than in non-connected ones, that is

$$\Delta_{c,p} P\left(F_j = 1\right) \equiv \left[P_{1,1}\left(F_j = 1\right) - P_{1,0}\left(F_j = 1\right)\right] - \left[P_{0,1}\left(F_j = 1\right) - P_{0,0}\left(F_j = 1\right)\right] > 0 \tag{4}$$

where $P_{c,p}(x)$ indicates the probability of event x occurring in firm of type c at time p. For brevity, we label the left hand side of equation (4) as $\Delta_{c,p}P(F_j = 1)$, where $\Delta_{c,p}$ indicates the first derivative with respect to type $c \in \{0,1\}$ and period $p \in \{0,1\}$. Thus, $\Delta_{c,p}P(F_j = 1)$ measures the spillover effect, that is, the incremental effect of the reform on the probability of hiring a woman in connected firms relatively to other non-target companies.

Recall that we defined direct-supply effects as the contribution of in-target candidates to the overall increase in female representation in connected boards. Therefore, for this mechanism to be relevant, at least part of the overall differential effect in (4) should be due to hiring women from the in-target pool. Since $\Delta_{c,p}P(F_j = 1) = \Delta_{c,p}P(F_j = 1, InTarget_j = 1) + \Delta_{c,p}P(F_j = 1, InTarget_j = 0)$, in the presence of direct supply-effects we should observe

$$\Delta_{c,p}P(F_j = 1, InTarget_j = 1) > 0 \tag{5}$$

with the ratio $\Delta_{c,p}P(F_j = 1, InTarget_j = 1)/\Delta_{c,p}P(F_j = 1)$ indicating the relative contribution of in-target candidates to the overall increase in female hiring. Crucially, since we observe the entire pool of in-target candidates, we can also make testable predictions regarding whether the demand mechanism plays a role. In particular, if connected firms do not change their preferences for female directors after the reform, the probability of hiring women *conditional* on drawing from the in-target pool should not differ between connected and non-connected firms, that is

$$\Delta_{c,p}P(F_j = 1 | InTarget_j = 1) = 0 \tag{6}$$

Finally, if both equations (5) and (6) above hold, then the differential effect in (5) can be fully explained by a composition and a persistence component. The composition component is the increase in the share of women in the in-target pool in the post-reform period. This is obviously directly induced by the reform requirements and, in principle, it is common to both connected and non-connected firms provided that they hire in-target candidates. The persistence component is due to the fact that connected firms may be more likely to hire in-target candidates. We include the derivation of this decomposition result in Appendix A.

In Table 8 we test whether equations (4), (5), and (6) hold in our data. Specifically, we focus on newly hired board members and estimate the following model

$$Y_{j,i,t} = \beta_1 Post_t * Connected_i + \beta_2 Connected_i + \beta_3 Post_t + \gamma X_{i,t} + \delta G_{j,t} + \varepsilon_{j,i,t} \quad (7)$$

where $Y_{j,i,t}$ is an outcome measured at the level of individual j hired as a board member in firm i between year $t-2$ and year t. We include board size and average number of appointments of board members as time-varying firm-level controls, as well as a dummy variable that takes value 1 if the firm is located in the North and a dummy that takes value 1 if the firm operates in the manufacturing sector. We also control for individual j's age, tenure within firm i (which ranges between 0 and 2 for new hires), and include the dummy variable $Native_{j,i}$ that takes value 1 if individual j's place of birth is less than 100Km distant from the location of firm i.

In column (1) we estimate the probability of new director j being female, i.e., $Y_{j,i,t} = F_{j,i,t}$. Consequently, coefficient $\hat{\beta}_1$ provides an estimate of the differential probability of hiring a woman in connected firms after the reform, i.e., $\Delta_{c,p} P\left(F_j = 1\right)$ in equation (4). As expected, this coefficient is positive and significant (with an increment of 2.6%), confirming our previous results at the firm level (see Table 3 , column 5). In column (2) we estimate the probability of new director j being female and currently seating in the board of a target firm, i.e., $Y_{j,i,t} = F_{j,i,t} \times InTarget_{j,i,t}$. In this case,

$\hat{\beta}_1$ measures the differential probability $\Delta_{c,p} P\left(F_j = 1, InTarget_j = 1\right)$ in equation (5). Our results support the hypothesis of direct supply-effects, that is, in-target candidates contribute to the overall increase in female hiring. This contribution is approximately 27%, as quantified by the ratio of the coefficients $\hat{\beta}_1$ in column (2) over that in column (1). In column (3) we restrict the sample to in-target board members only and consider the outcome $F_{j,i,t}$. Therefore, here $\hat{\beta}_1$ measures the differential probability $\Delta_{c,p} P\left(F_j = 1 | InTarget_j = 1\right)$ in equation (6). Our estimates show that this is not statistically different from zero, implying that, conditional on hiring an in-target board member, connected firms do not change their preferences for women relatively to non-connected firms after the reform. Interestingly, we also find that $\hat{\beta}_2$ is not statistically different from zero, implying that, both before and after the reform, the propensity to hire women within the in-target pool does not depend on firm's past connections. Thus, the increase in female representation in the in-target component seems purely related to supply factors, i.e., composition and persistence. Indeed, in this sample the coefficient estimate $\hat{\beta}_3$ for the variable $Post_t$ is positive, significant, and approximately four times larger than that in column (1), reflecting the effect that the reform had on the gender composition of in-target candidates.

We explore the persistence factor in column (4) using the dummy variable $InTarget_j$ as the outcome. Our results confirm the conjecture that connected firms are more likely to hire in-target candidates than non-connected firms ($\hat{\beta}_2 = +14\%$). However, this difference in more pronounced before the reform than after ($\hat{\beta}_1 = -8\%$), implying that the composition effect is large enough to compensate the drop in persistence, resulting in an overall increase in the probability of hiring female in-target candidates.

To summarize, the results in Table 8 suggest that approximately 27% of the increase in the share of new female hires in connected firms comes from direct-supply effects. Within the in-target pool, we find no evidence for the demand driven mechanism, i.e., changes in preferences for female candidates. Next, we examine out-target newly hired board members to investigate whether indirect-supply or demand explain the remaining 73% of the effect. Notice, however, that if a change in preferences for hiring women cannot explain the spillover effect when firms hire from in-target pool, than it cannot explain the remainder of the effect under the (mild) assumption that preferences should be invariant to the pool from which the firm is hiring.

Out-target Pool. As mentioned earlier, the pool of out-target candidates is not fully observable. However, the characteristics of out-target new directors may point to the relevant mechanism. In particular, the indirect-supply mechanism predicts that directors hired after the reform in connected firms are more diverse, not just in terms of gender but also regarding their background and connections with the rest of the board. Moreover, differently from the demand mechanism, the indirect-supply effect should operate on both the male and the female population of new directors. Consistent with this channel, we show that not only connected firms are more likely to hire new female directors after the reform (as per the results in Table 8), but also that newly appointed directors, both male and female, are hired drawing from a broader geographical area, tend to have fewer prior connections with older board members, and are more likely to be firm outsiders. These results are presented in Tables 9a and 9b where we regress various individual level outcomes on the explanatory variable of interest ($Post_t \times Connected_i$), firm-level covariates (board size and average number of appointments), and individual j's tenure in firm i at time t. We use the sample of out-target newly hired female and male board members respectively. In column (1) the outcome variable is a dummy that takes value 1 if individual j is an outsider, and zero otherwise. The positive and significant coefficient estimates for the interaction variable $Post_t \times Connected_i$ suggest that new board members in connected firms are less likely to be insiders (e.g., executives or shareholders) after the reform, and this is true for both female and male board members, though the effect is stronger in the female sample. In columns (2) to (6) we restrict the sample to outsiders, to examine how their characteristics change. The results in columns (2) and (3) show that female new hires are more likely to be foreign born and less likely to be native, i.e., born within a 100Km radius from the firm location. The effects on the male sample are stronger when we consider the first indicator (foreign born) but opposite for the second (native). New female hires are marginally older (column (4)) and are busier (column (5)) in connected firms after the reform, which, in line with previous research, can be interpreted as evidence for better quality and/or experience. We find an opposite effect on age for male new hires, i.e., they are younger, but we find similar effects on busyness. Next, we examine whether new hires in firm j are also new entrants in the profession in general, i.e., if they are at their first appointment ever as board members. Somewhat surprisingly, while the coefficient on $Post_t \times Connected_i$ is not different from zero in the female sample, it is positive and significant in the male sample, that is, new male hires in connected firms after the reform are more likely to be at their first board experience (column (6)). In column (7) we further restrict the sample to new hires with past board experience and we use a proxy for past interactions with current board members, $Contacts_{j,i,t}$, as the outcome of interest. Specifically, $Contacts_{j,i,t}$ is computed by counting how many of firm i 's current board members individual j has worked with over the past 3 years. Of course, we can only track work experience within boards, but we can observe whether two individuals met as board members in a company different from i in the past or whether they currently sit together in a different board. Thus, $Contacts_{j,i,t}$ can be interpreted as a measure of individual j's "membership" in firm i's network. Our results show that $Contacts_{j,i,t}$ decrease for new hires in connected firms after the reform, and that the effect is three-fold stronger among women.

Board Characteristics. Finally we explore changes in board characteristics using the following firm-level specification

$$Y_{i,t} = \beta Post_t \cdot Connected_i + \gamma G_{i,t} + \alpha_i + \gamma_t + \varepsilon_{i,t} \tag{8}$$

where $G_{i,t}$ is a vector of firm-level time-varying controls, α_i are firm fixed effects, and γ_t represent year dummies. In Table 10 we consider several different outcome variables, in particular: board busyness, i.e., the average number of current appointments of board members in firm i and year t (column 1); the average distance between the place of birth of each board member and the location of firm i (column 2); the average tenure, i.e., years of service in firm i's board (column 3); a dummy variable that takes value of 1 if any of the executive board members are female (column 4); a dummy variable that takes value of 1 if the firm's CEO is female (column 5). As in the indiviual-level analysis, our results suggest that board characteristics change significantly in connected firms after the reform. Specifically, board members become on average less busy and their geographical origins are more dispersed. There is more turnover in the board, as average tenure decreases for both female and male board members. This evidence supports the view that the reform triggered a change in the hiring practices of firms connected to target companies. Interestingly, the probability of observing female executives and a female CEO increase by approximately 8% and 7% as compared to their base levels of 14% and 7% respectively.

Overall, this evidence shows that connected firms expand the pool of candidates for directorship positions beyond their pre-existing network after the reform, and it suggests that information sharing between target and connected firms, combined with the push towards professionalization induced by the reform, may have lowered the executive search costs for connected firms.

Interestingly, neither of the two mechanisms documented above hinges on changes in corporate culture. This is different from previous research on vertical trickle-down effects, which argues that mandatory gender rebalancing in the board may lead to career improvements for all women in target firms due to the attenuation of gender bias in promotions and the adoption of women-friendly corporate policies (e.g., childcare facilities on premises, flexible working hours, etc.). Horizontal spillovers appear to be mostly driven by economic incentives, i.e., the preservation of valuable connections and lower search costs.

5.1 Firm Performance

In this final section, we explore the effects of the spillovers on firm's performance by means of a TWFE difference-in-difference estimation where outcomes are measured in terms of sales growth, profitability, and investment rate.[10] We premise the discussion of the results with three important caveats. First, the overall effects on board composition in connected firms, though significant, are not large in magnitudes and are spread over time, as one would expect from voluntary rather than mandatory adjustments. This suggests a modest, if even discernable, potential impact on firm performance. Second, we document spillover effects not only in the gender composition of the board but also along other dimensions (e.g., directors' connections, busyness, tenure). It is therefore difficult to disentangle the effects of the larger presence of women on board from those of other changes in directors' characteristics. Lastly, and very importantly, the timing of the reform coincides with the height of the sovereign-debt crisis in Europe, which hit the Italian economy with particular intensity. It is possible that connected and non-connected firms were affected differently by the economic downturn, and had a different

[10] Sales growth is defined as $log(Sales_t) - log(Sales_{t-1})$, profitability is defined as $EBITDA_t/Assets_t$, investment rate is defined as $(PPE_t - PPE_{t-1} + Depreciation_t)/PPE_{t-1}$. Final values are 2% winsorized.

recovery path in the following years. Since this may significantly change the interpretation of the results of a difference-in-difference exercise on firm performance, extra care is required when choosing the control sample to attenuate the problem of confounding factors. To this end, we restrict the analysis to the period 2009-2019, and, in addition to the control group defined in our baseline and propensity-score matched sample specifications presented in the previous sections, we use an alternative control group which comprises non-connected firms matched to the connected sample by macro-sector (manufacturing vs non.-manufacturing) and average sales growth in the three years prior to the reform. This allows us to compare connected firms with non-connected firms that were similarly impacted by the crisis. To further support a causal reading of our findings, we make use of the results in Section 4.2 showing that spillovers are stronger for firms connected to listed companies. Thus, in each of these three specifications, we add the interaction term $Post_t \times Conn.\, to\, Listed_i$ where $Conn.\, to\, Listed_i$ is a dummy variable that takes value 1 if firm i is connected to a listed firm. In a similar spirit, we estimate an additional specification where we replace the term $Post_t \times Connected_i$ with the term $Post_t \times Exposed_i$, where $Exposed_i$ is a dummy variable that takes value 1 if firm i was connected to a target company with less than 20% female board members in the three years prior to the reform. In other words, we remove from the treated group firms that, while connected, were not significantly affected by the quota reform.

The results are presented in Table 11. Panel (a) and (b) show a negative effect on sales growth and a mildly positive effect on profitability for firms connected to listed companies. These effects, however, are only statistically significant when we use the matched control samples (columns 2 and 3). The results on investment rates, instead, seem to indicate an increase for connected firms in the post reform period, which is robust across all specifications and has a magnitude ranging between 3% and 7% (Panel c).

6 Conclusions

The results in this paper strongly suggest that Italy's board quotas law caused substantial *horizontal* spillover effects to firms that, while not directly subject to the new regulation, had strategic connections with target firms, i.e., listed and state-controlled companies. After the reform, connected firms increased the share of female board members significantly more than comparable non-connected firms.

Our evidence overturns the pessimistic findings on the lack of *vertical* spillovers, on which the literature has focused so far. Our results are consistent with changes in the relevant pool of candidates for board positions available to connected firms, rather than a change in preferences for gender diversity by these firms. In particular, we show that the increase in female representation occurs via two channels. First, to preserve the strategic relationships, connected firms hire some of their directors choosing from the set of *current* board members in target firms. When this set becomes more gender-balanced as per the requirements of the quota law, so does the set of new hires in connected firms. This mechanism explains roughly 27% of the differential increase in female new hires on the boards of connected firms. The second channel, based on information-sharing, explains the remaining 73%. As target firms collect information on *potential* board member candidates to comply with the law, this information spills over to non-target connected firms, who then start hiring new board members from a more diverse pool of candidates outside of their existing network. The adoption of these new recruiting policies is voluntary, i.e., not constrained by regulatory requirements, suggesting that firms find it beneficial to broaden their search and improve their chances of finding qualified candidates.

Taken all together, these findings show that the reform acted as a shock to the labor market for board members, redirecting search towards segments of the population - women, as well as network outsiders- that were traditionally disregarded. Thus, new candidates join the pool from which not only target companies but also connected ones select their directors. Interestingly, this suggests that the effects of quotas on female representation in corporate boards may be permanent - i.e. survive even if the quotas stopped being mandatory. After qualified women gain experience as directors and enter the network of candidates for these jobs, their chance of being hired again in the future likely increases, irrespective of the presence of mandatory quotas. Of course it could be argued that a reversal may occur in the presence of strong gender biases in the corporate world, especially with respect to managerial jobs. Our results speak against this conjecture. Connected firms smoothly adapt to the changing gender composition of the candidates' pool - despite not being mandated by law - rather than resisting it.

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Figure 1: Share of Female Board Members: Target Firms

This figure plots the share of female board members over time. The sample includes only firms targeted by the Golfo-Mosca Law (quota law), namely listed and state-controlled firms.

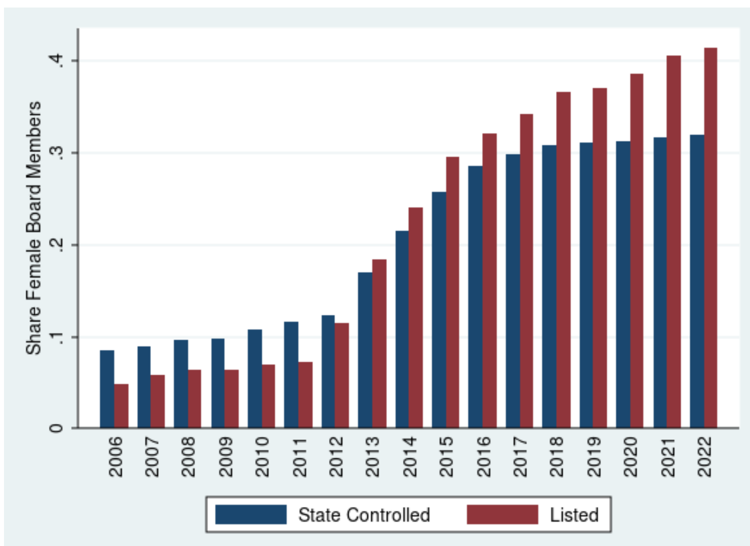
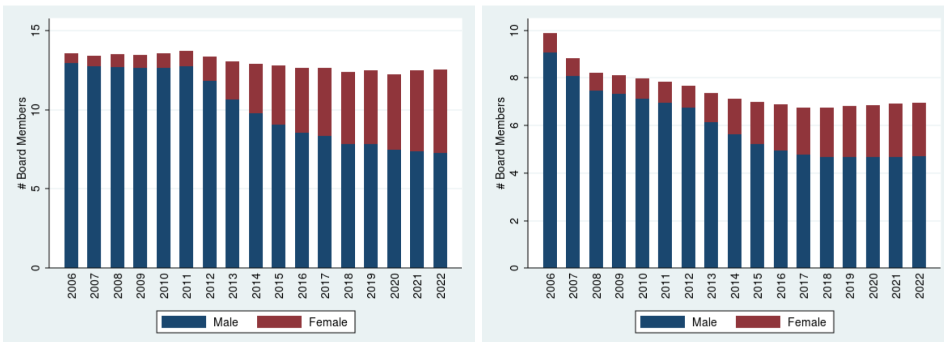


Figure 2: Board Composition in Target Firms

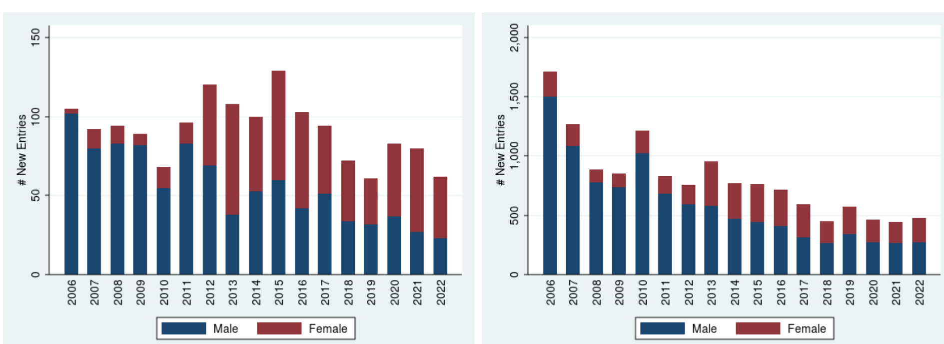
This figure plots the average number of male and female board members over time in listed firms (Panel a) and state controlled Firms (Panel b).



(a) Listed **(b)** State Controlled

Figure 3: Gender Composition of New Entrants in Boards of Target Firms

This figure plots the average number of male and female board new entrants over time. New entrants are defined as individuals who appear for the first time in our records as board members in listed firms (Panel a) and state controlled Firms (Panel b).



(a) Listed **(b)** State Controlled

Figure 4: Busyness of Female Board Members

This figure plots the average number of board appointments for female board members in listed and state controlled firms. We consider the total number of board seats that the focal board member has both in target and in non-target firms.

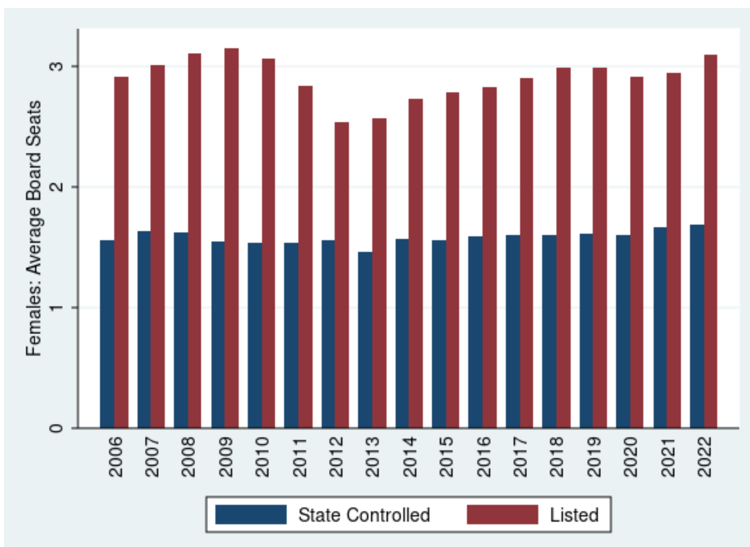
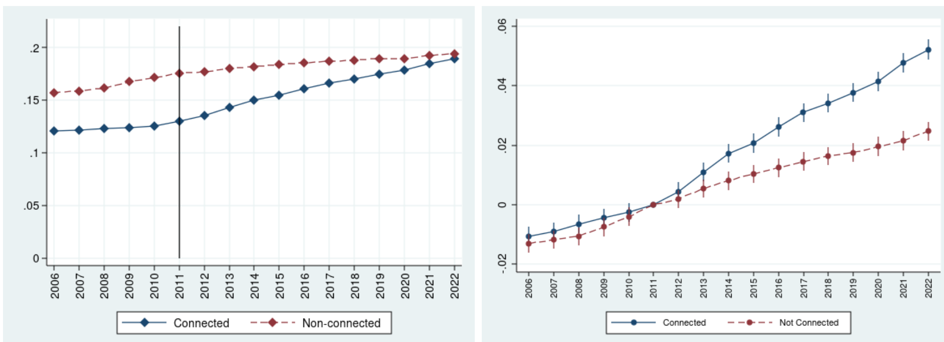


Figure 5: Share of Female Board Members: Connected vs Non-connected Firms

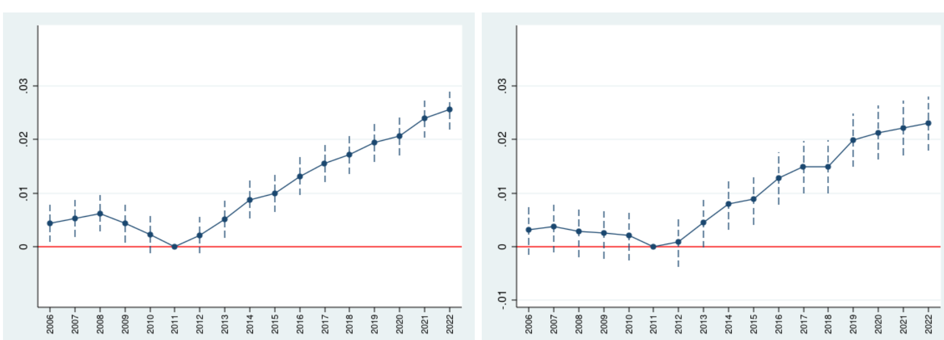
This figure plots the share of female board members over time for firms in the core sample (i.e., non-target, ongoing concern firms). Connected firms are defined as firms that in at least one of the three years before the reform had at least one board member in common with a target firm. Panel (a) shows average shares. Panel (b) shows average differences with respect to 2011 shares.



(a) (b)

Figure 6: Women on Boards: Connected vs Non-connected Firms

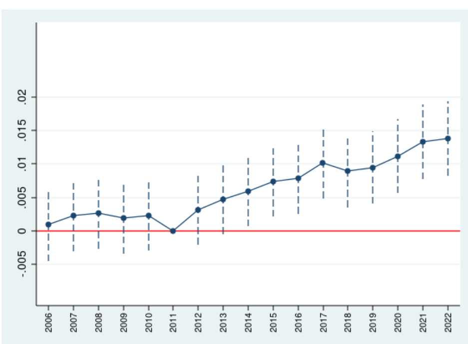
This figure shows the estimated coefficients $\hat{\beta}_t$ for the interaction terms in the following specification: $F\_Share_{i,t} = \sum_{s \neq 2011} \beta_t 1_{\{s=t\}} Connected_i + \delta X_{i,t} + \alpha_i + \gamma_t + \varepsilon_{i,t}$, where $F\_Share_{i,t}$ is the share of female members in the board of non-target firm i in calendar year $t \in [2006, 2022]$, $X_{i,t}$ is a vector of firm-level time-varying controls for board size and average number of appointments of the firm's board members, α_i are firm fixed effects, γ_t represent year dummies and $1_{\{s=t\}}$ is a dummy equal to one for $s = t$. In Panel (a) all firms in the core sample are included. In Panel (b) the sample includes all connected firms plus matched non-connected firms. The dashed vertical bars represent 95% confidence intervals.



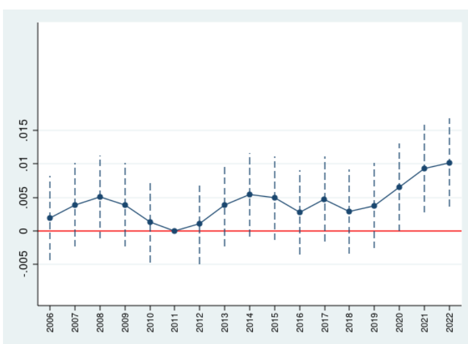
(a) (b)

Figure 7: Women on Boards: Second-degree Spillover Effects

This figure shows the estimated coefficients $\hat{\beta}_t$ for the interaction terms in the following specification $F\_Share_{i,t} = \sum_{s \neq 2011} \beta_t 1_{\{s=t\}} Connected2_i + \delta X_{i,t} + \alpha_i + \gamma_t + \varepsilon_{i,t}$. $F\_Share_{i,t}$ is the share of female members in the board of firm i in calendar year $t \in [2006, 2022]$,$X_{i,t}$ are firm-level time-varying controls for board size and average number of appointments of board members, α_i represents firm fixed effects, γ_t represent year dummies and $1_{\{s=t\}}$ is a dummy equal to one for $s = t$. $Connected2_i$ is a dummy that takes value 1 if firm i had at least 1 board member in common with a connected firm in years 2009, 2010, or 2011. We remove observations where $Connected_i = 1$ from the sample. In Panel (a) all firms in the core sample are included. In Panel (b) the sample includes all connected firms plus matched non-connected firms. The dashed vertical bars represent 95% confidence intervals.



(a)



(b)

Figure 8: Women on Boards: Connections to Listed vs. State Controlled

This figure shows the estimated coefficients $\hat{\beta}_t$ for the interaction terms in the following specification: $F\_Share_{i,t} = \sum_{s \neq 2011} \beta_t 1_{\{s=t\}} Connected_i + \delta X_{i,t} + \alpha_i + \gamma_t + \varepsilon_{i,t}$, where $F\_Share_{i,t}$ is the share of female members in the board of non-target firm i in calendar year $t \in [2006, 2022]$, $X_{i,t}$ is a vector of firm-level time-varying controls for board size and average number of appointments of the firm's board members, α_i are firm fixed effects, γ_t represent year dummies and $1_{\{s=t\}}$ is a dummy equal to one for $s = t$. The coefficients of interest are computed separately for firms connected to listed companies and those connected to state controlled entities. The dashed vertical bars represent 95% confidence intervals.

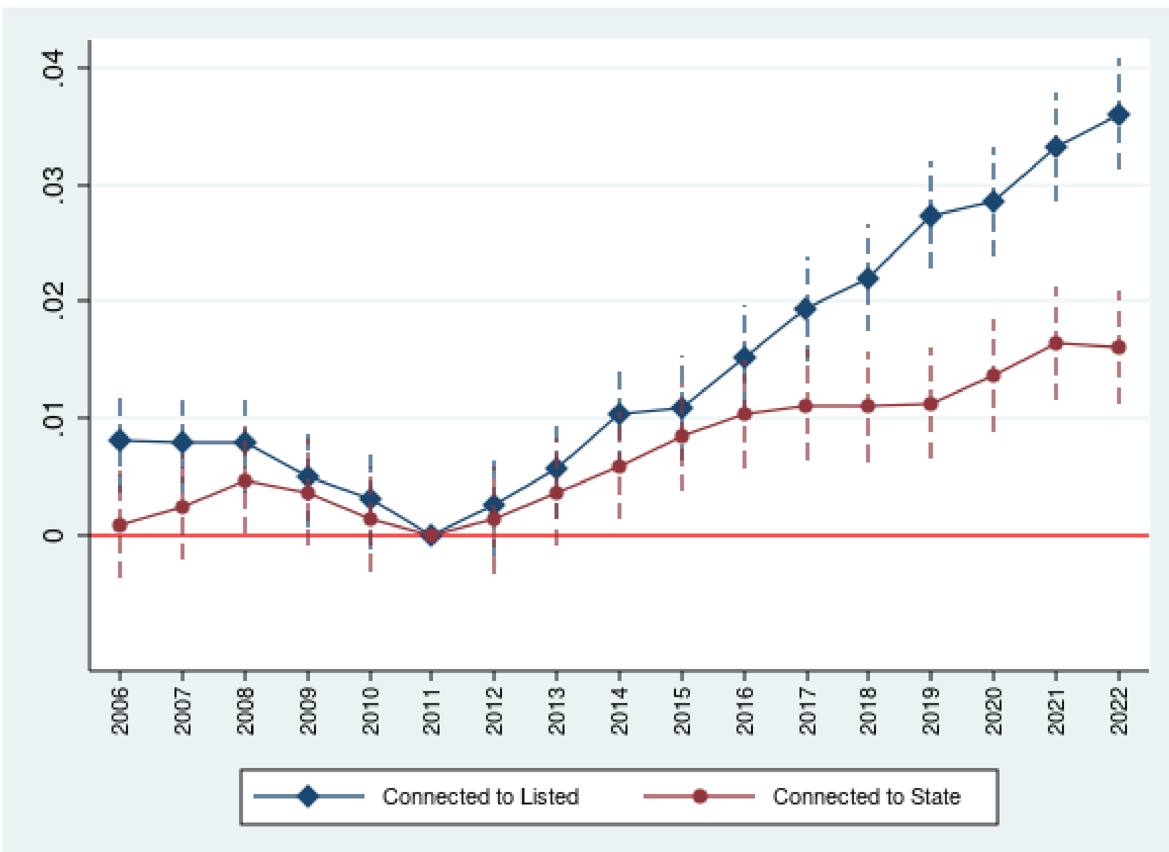


Figure 9: Women on Boards: Heterogeneous Exposure

This figure shows the estimated coefficients $\hat{\beta}_t$ for the interaction terms in the following specification: $F\_Share_{i,t} = \sum_{s \neq 2011} \beta_t 1_{\{s=t\}} Exposed_i + \delta X_{i,t} + \alpha_i + \gamma_t + \varepsilon_{i,t}$, where $F\_Share_{i,t}$ is the share of female members in the board of non-target firm i in calendar year $t \in [2006, 2022]$, $X_{i,t}$ is a vector of firm-level time-varying controls for board size and average number of appointments of the firm's board members, α_i are firm fixed effects, γ_t represent year dummies and $1_{\{s=t\}}$ is a dummy equal to one for $s = t$. $Exposed_i$ takes value 1 if any of the board members in connected firm i sat in the board of a target firm with a share of women less than 20% in 2011. The sample includes connected firms only. The dashed vertical bars represent 95% confidence intervals.

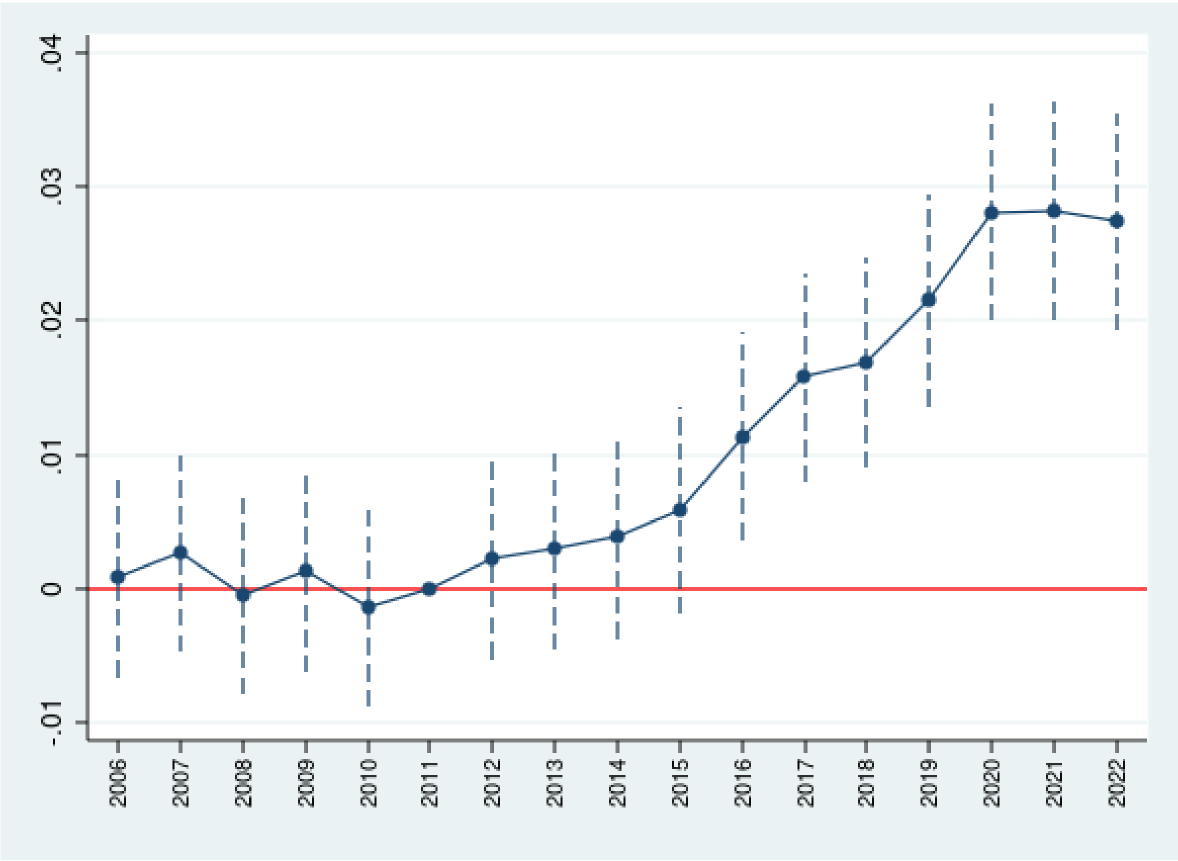


Figure 10: Women on Boards: Matched Samples

This figure shows the estimated coefficients $\hat{\beta}_t$ for the interaction terms in the following specification: $F\_Share_{i,t} = \sum_{s \neq 2011} \beta_t 1_{\{s=t\}} Connected_i + \delta X_{i,t} + \alpha_i + \gamma_t + \varepsilon_{i,t}$, where $F\_Share_{i,t}$ is the share of female members in the board of non-target firm i in calendar year $t \in [2006, 2022]$, $X_{i,t}$ is a vector of firm-level time-varying controls for board size and the average number of appointments of the firm's board members, α_i are firm fixed effects, γ_t represent year dummies and $1_{\{s=t\}}$ is a dummy equal to one for $s = t$. The six panels correspond to different matching techniques. Propensity scores in the pre-reform period are computed using the following cross-section logit specification $Pr\,(Connected_i = 1) = f\,(\gamma X_i + \varepsilon_i)$ where X_i includes the average values of board size and number of appointments of board members in years 2009 to 2011 (parsimonious specification). In the upper-left panel we use all treated units (i.e., $Connected_i = 1$) plus control firms with propensity scores higher than the connected sample median. In the upper-right panel we use all firms, treated and controls, with propensity scores above the core sample median. In the middle panels we apply the same methodology, but we include average assets and sales, as well as indicators for whether the firm is located in the North and operates in the manufacturing industry as additional controls in the previous equation (full specification). Finally, in the bottom panels we use 3-nearest neighbors propensity score matching. Probability scores are computed using the parsimonious and the full specification in the bottom-right and bottom-left respectively. The dashed vertical bars represent 95% confidence intervals.

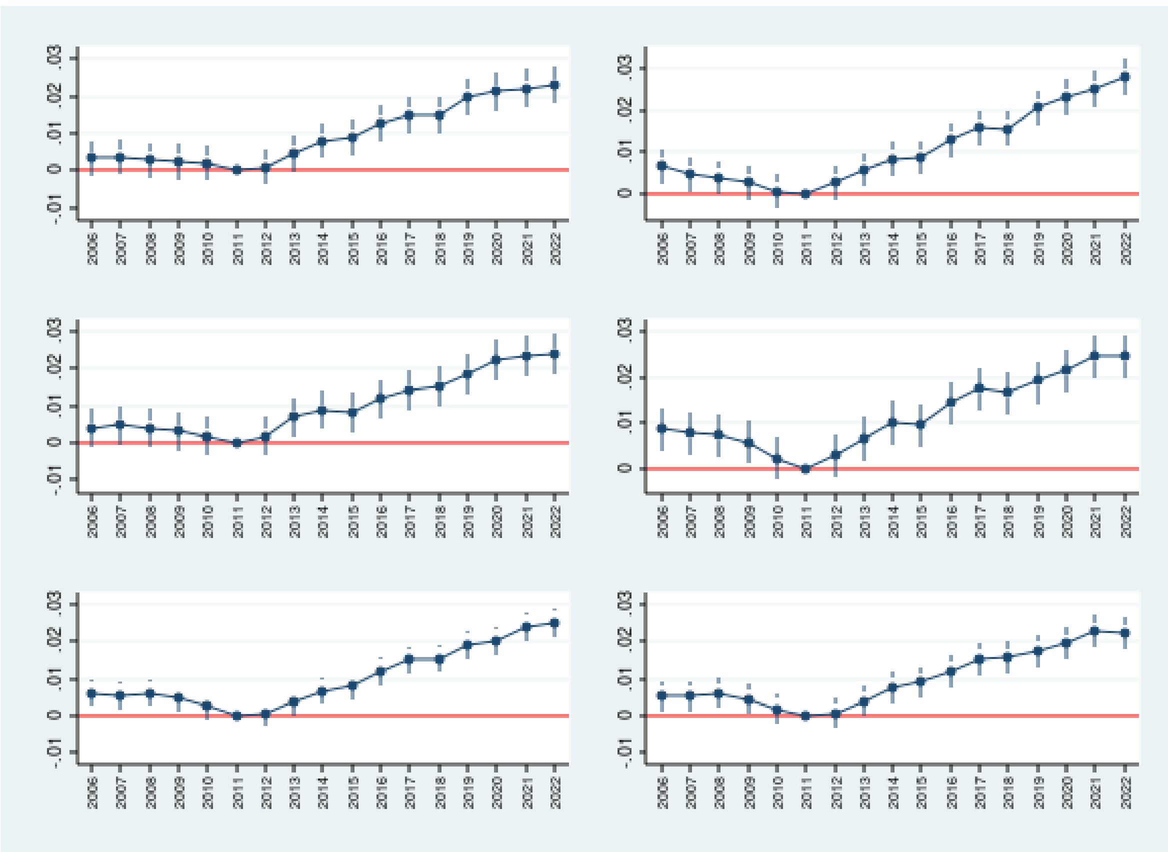


Table 1: Descriptive Statistics of Connected and Non-connected Firms

This table displays means and standard deviations for selected variables in the two groups of non-connected (column 1) and connected (column 2) firms. Column 3 shows the results of a t test for differences in means. *Board Size* is the number of board members. *Avg. Busyness* is the average number of board seats per board members. *Any Listed Owner* is a dummy variable that takes value 1 if the firm has a listed company as one of its shareholders. *Any State Owner* is a dummy variable that takes value 1 if the firm has a state-controlled company as one of its shareholders. *Ownership in Listed (State)* is a dummy variable that takes value 1 if the firm owns shares in a listed (state-controlled) company. *Manuf* is a dummy variable that takes value 1 if the firm operates in the manufacturing industry. *lnAssets* is the log of firm's total balance sheet assets (in thousands).*North* is a dummy variable that takes value 1 if the firm is located in the North of Italy. t values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

	(1)		(2)		(3)	
	Non Connected		Connected		Diff. in Means	
	mean	sd	mean	sd	b	t
Board Size	5.96	2.24	7.26	2.68	-1.29***	(-162.15)
Avg. Busyness	2.89	2.19	3.50	2.27	-0.61***	(-86.85)
Any Listed Owner	0.01	0.11	0.07	0.26	-0.06***	(-89.43)
Any State Owner	0.00	0.07	0.03	0.16	-0.02***	(-53.34)
Ownership in Listed	0.00	0.05	0.01	0.08	-0.00***	(-20.64)
Ownership in State	0.00	0.01	0.00	0.02	-0.00***	(-6.98)
Manuf	0.53	0.50	0.54	0.50	-0.01**	(-2.68)
lnAssets	9.40	1.41	10.00	1.55	-0.60***	(-102.63)
North	0.68	0.47	0.75	0.43	-0.07***	(-49.65)
Observations	282679		156141		438820	

Table 2: Descriptive Statistics of Connected and Non-connected Firms: Matched Sample

This table displays means and standard deviations for selected variables in the two groups of matched non-connected (column 1) and connected (column 2) firms. Matched non-connected firms have estimated propensity scores higher than the connected sample median. Column 3 shows the results of a t test for differences in means. *Board Size* is the number of board members. *Avg. Busyness* is the average number of board seats per board members. *Any Listed Owner* is a dummy variable that takes value 1 if the firm has a listed company as one of its shareholders. *Any State Owner* is a dummy variable that takes value 1 if the firm has a state-controlled company as one of its shareholders. *Ownership in Listed (State)* is a dummy variable that takes value 1 if the firm owns shares in a listed (state-controlled) company. *Manuf* is a dummy variable that takes value 1 if the firm operates in the manufacturing industry. *lnAssets* is the log of firm's total balance sheet assets (in thousands).*North* is a dummy variable that takes value 1 if the firm is located in the North of Italy. t values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

	(1)		(2)		(3)	
	Non Connected		Connected		Diff. in Means	
	mean	sd	mean	sd	b	t
Board Size	7.87	2.35	7.24	2.75	0.63***	(54.52)
Avg. Busyness	4.22	2.90	3.57	2.36	0.65***	(49.22)
Any Listed Owner	0.01	0.11	0.07	0.26	-0.06***	(-80.66)
Any State Owner	0.01	0.09	0.03	0.16	-0.02***	(-37.19)
Ownership in Listed	0.00	0.05	0.01	0.08	-0.00***	(-15.39)
Ownership in State	0.00	0.00	0.00	0.03	-0.00***	(-11.09)
Manuf	0.45	0.50	0.53	0.50	-0.08***	(-29.04)
lnAssets	9.92	1.30	9.93	1.56	-0.02*	(-2.29)
North	0.80	0.40	0.75	0.43	0.04***	(22.61)
Observations	58825		182047		240872	

Table 3: Female Representation in Boards of Connected Firms

This table shows coefficient estimates for the following specification $Y_{i,t} = \beta Post_t \times Connected_i + \gamma X_{i,t} + \alpha_i + \gamma_t + \varepsilon_{i,t}$ where $Post_t = 1$ if $t \geq 2012$ and zero otherwise, $Connected_i = 1$ if firm i shared at least one board member with a target firm in any of the three years before the reform, and $X_{i,t}$ are firm-level time-varying controls for board size and average number of appointments of board members. α_i and γ_t represent firm and year fixed effects. In column (1) the outcome variable is $Y_{i,t} = \Delta Share_{i,t} \equiv (F\_Share_{i,t} - F\_Share_{i,t-1}) * 100$, i.e., the year on year variation in the share of female board members in firm i. In column (2) the outcome variable is $Y_{i,t} = 1$ if $F\_Share_{i,t} > 0$ and zero otherwise. In column (3) the outcome variable is $Y_{i,t} = F\_Share_{i,t}$. In columns (4) and (5) the outcome variable is the change in the number of male and female board members respectively. In Panel (a) all firms in the core sample are included. In Panel (b) the sample includes all connected firms plus matched non-connected firms. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

(a)

	(1) Δ Share F.	(2) Any F. BM	(3) Share F. BM	(4) Δ M.BM	(5) Δ F.BM
Post X Connected	0.3243***	0.0383***	0.0099***	0.0173***	0.0375***
	(0.0533)	(0.0021)	(0.0007)	(0.0063)	(0.0031)
Board Size	0.1237***	0.0544***	0.0033***	0.2464***	0.0485***
	(0.0099)	(0.0004)	(0.0001)	(0.0012)	(0.0006)
Board Avg. Interlocks	-0.1933***	-0.0129***	-0.0078***	0.0028**	-0.0135***
	(0.0115)	(0.0004)	(0.0001)	(0.0014)	(0.0007)
Firm and Year FE	yes	yes	yes	yes	yes
Observations	428049	438820	438773	428140	428140
Firms	38419	39600	39600	38424	38424
R-Squared	0.001	0.073	0.029	0.105	0.019
Mean Dep. Var.	0.27	0.62	0.17	0.02	0.03

(b)

	(1) Δ Share F.	(2) Any F. BM	(3) Share F. BM	(4) Δ M.BM	(5) Δ F.BM
Post X Connected	0.2763***	0.0445***	0.0105***	-0.0266**	0.0337***
	(0.0704)	(0.0033)	(0.0009)	(0.0105)	(0.0050)
Board Size	0.0424***	0.0379***	0.0014***	0.2599***	0.0407***
	(0.0116)	(0.0005)	(0.0002)	(0.0017)	(0.0008)
Board Avg. Interlocks	-0.1903***	-0.0146***	-0.0066***	0.0141***	-0.0123***
	(0.0129)	(0.0006)	(0.0002)	(0.0019)	(0.0009)
Firm and Year FE	yes	yes	yes	yes	yes
Observations	204911	207025	207019	204924	204924
Firms	15279	15351	15351	15279	15279
R-Squared	0.002	0.052	0.048	0.112	0.015
Mean Dep. Var.	0.33	0.63	0.15	-0.03	0.03

Table 4: Timing and Intensity of Firms Connections

Columns (1) to (3) show coefficient estimates for the following specification $Share_{i,t+s} = \beta Post_t \times Current\_C_i + \gamma X_{i,t} + \alpha_i + \gamma_t + \varepsilon_{i,t}$ where $Post_t = 1$ if $t \geq 2012$ and zero otherwise, $Current\_C_i$ equals 1 if firm i is connected to a target firm at time t, $X_{i,t}$ are firm-level time-varying controls for board size and average number of appointments of board members. α_i and γ_t represent firm and year fixed effects. Columns (4) to (6) show coefficient estimates for the following specification $\Delta Share_{i,t} = \beta Post_t \times C_i + \gamma X_{i,t} + \alpha_i + \gamma_t + \varepsilon_{i,t}$ where $Post_t = 1$ if $t \geq 2012$ and zero otherwise, C_i is a measure of longevity or intensity of connections with target firms, and $X_{i,t}$ are firm-level time-varying controls for board size and average number of appointments of board members. α_i and γ_t represent firm and year fixed effects. $Connected - 06 - 08_i$ takes the value 1 if firm i shared at least one board member with a target firm in the years 2006, 2007, or 2008. $Mean\,Connection_i = \sum_t \left(\sum_b c_{b,i,t}\right)/3$, where c is a dummy that takes the value 1 if board member b in firm i at time $t \in [2009, 2011]$ seats in the board of a target firm. $Mean\,Tot\,Connection_i = \sum_t \left(\sum_b n_{b,i,t}\right)/3$, where n is the number of current seats in the board of target firms held by board member b in firm i at time $t \in [2009, 2011]$. The sample includes all firms in the core sample in columns (1) to (3) and connected firms only in columns (4) to (6). Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	(1) Share F. (t+1)	(2) Share F. (t+2)	(3) Share F. (t+3)	(4) Δ Share F.	(5) Δ Share F.	(6) Δ Share F.
Post X Current_C	0.0157***	0.0152***	0.0145***			
	(0.0008)	(0.0008)	(0.0008)			
Current_C	-0.0100***	-0.0099***	-0.0095***			
	(0.0007)	(0.0007)	(0.0007)			
Post X Connected_06-09				-0.0696		
				(0.1093)		
Post X Mean Connection					0.0627***	
					(0.0123)	
Post X Mean Tot. Connection						0.0354***
						(0.0074)
Other Controls	yes	yes	yes	yes	yes	yes
Firm and Year FE	yes	yes	yes	yes	yes	yes
Observations	374666	339130	305744	151990	154451	154451
Firms	37379	35340	32460	11553	11849	11849
R-Squared	0.027	0.024	0.021	0.002	0.002	0.002
Mean Dep. Var.	0.17	0.17	0.18	0.35	0.36	0.36

Table 5: Female Representation in Boards of Connected Firms: Robustness

This table shows coefficient estimates for the following specification $\Delta Share_{i,t} = \beta Post_t \times Connected_i + \gamma X_{i,t} + \alpha_i + \gamma_t + \varepsilon_{i,t}$ where $Post_t = 1$ if $t \geq 2012$ and zero otherwise, $Connected_i = 1$ if firm i shared at least one board member with a target firm in any of the three years before the reform, and $X_{i,t}$ are firm-level time-varying controls for board size and average number of appointments of board members. α_i and γ_t represent firm and year fixed effects. In column 1 we reprepose the results from our main specification. In column 2 we restrict the sample to firms in the core sample that stayed private after the reform. In column 3 we restrict the sample to firms with no listed company among its shareholders. $Prox1_{i,t}$ is the number of target firms in year t located in the same province as firm i, divided by the total number of firms in the province. $Prox2_{i,t}$ is the number of target firms in year t located in the same province and operating in the same sector as firm i, divided by the total number of firms in the province. In Panel (a) all firms in the core sample are included. In Panel (b) the sample includes all connected firms plus matched non-connected firms. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

(a)

	(1)	(2) Never Listed	(3) Geography	(4) Sector
Post X Connected	0.3243***	0.3186***	0.2727***	0.3399***
	(0.0533)	(0.0534)	(0.0852)	(0.0551)
Post X Connected X Prox1			1.4839	
			(1.8595)	
Post X Connected X Prox2				-1.5240
				(2.5188)
Other Controls	yes	yes	yes	yes
Firm and Year FE	yes	yes	yes	yes
Observations	428049	426768	428049	428049
Firms	38419	38239	38419	38419
R-Squared	0.001	0.001	0.001	0.001
Mean Dep. Var.	0.27	0.27	0.27	0.27

(b)

	(1)	(2) Never Listed	(3) Geography	(4) Sector	(5) column5
Post X Connected	0.2763***	0.2729***	0.2273***	0.2771**	0.3100***
	(0.0704)	(0.0705)	(0.0718)	(0.1164)	(0.0731)
Post X Connected X Prox1				-0.0053	
				(2.8574)	
Post X Connected X Prox2					-6.5267
					(3.9786)
Other Controls	yes	yes	yes	yes	yes
Firm and Year FE	yes	yes	yes	yes	yes
Observations	204911	203819	188389	204911	204911
Firms	15279	15128	14491	15279	15279
R-Squared	0.002	0.002	0.002	0.002	0.002
Mean Dep. Var.	0.33	0.33	0.30	0.33	0.33

Table 6: Female Representation and Ownership Links

This table shows coefficient estimates for the following specification $\Delta Share_{i,t} = \beta Post_t \times Connected_i + \gamma X_{i,t} + \alpha_i + \gamma_t + \varepsilon_{i,t}$ where $Post_t = 1$ if $t \geq 2012$ and zero otherwise, $Connected_i = 1$ if firm i shared at least one board member with a target firm in any of the three years before the reform, $Ownership_i$ takes value 1 if firm i had ownership links with a target firm in the three years before the reform, and $X_{i,t}$ are firm-level time-varying controls for board size and average number of appointments of board members. α_i and γ_t represent firm and year fixed effects. In column (1) we restrict the sample to only firms with no ownership links at time t , i.e., to firms that do not own shares of target firms nor have target firms among their shareholders. In columns (2) and (3) we use the full sample and the matched sample respectively. In column (4) we restrict the sample to connected firms only. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	(1) No Ownership Links	(2) Full Sample	(3) Matched Sample	(4) Connected Firms
Post X Connected	0.2637***	0.2537***	0.2271***	
	(0.0555)	(0.0537)	(0.0720)	
Post X Connected X Ownership		0.1005	-0.2513	
		(0.3766)	(0.4654)	
Post X Ownership		0.5905*	0.8809*	0.6138***
		(0.3524)	(0.4506)	(0.1210)
Other Controls	yes	yes	yes	yes
Firm and Year FE	yes	yes	yes	yes
Observations	408100	400943	204911	154451
Firms	37129	30150	15279	11849
R-Squared	0.001	0.002	0.002	0.002
Mean Dep. Var.	0.25	0.28	0.33	0.36

Table 7: Female Representation in Boards of Connected Firms: Alternative Matching

This table shows coefficient estimates for the following specification $\Delta Share_{i,t} = \beta Post_t \times Connected_i + \gamma X_{i,t} + \alpha_i + \gamma_t + \varepsilon_{i,t}$ where $Post_t = 1$ if $t \geq 2012$ and zero otherwise, $Connected_i = 1$ if firm i shared at least one board member with a target firm in any of the three years before the reform, and $X_{i,t}$ are firm-level time-varying controls for board size and average number of appointments of board members. α_i and γ_t represent firm and year fixed effects. Observations are selected using different matching techniques. Propensity scores in the pre-reform period are computed using the following cross-section logit specification $Pr\left(Connected_i = 1\right) = f\left(\gamma X_i + \varepsilon_i\right)$ where X_i includes the average values of board size and number of appointments of board members in years 2009 to 2011 (parsimonious specification). In column (1) we use all treated units (i.e., $Connected_i = 1$) plus control firms with propensity scores higher than the connected sample median. In column (2) we use all firms, treated and controls, with propensity scores above the core sample median. In columns 3 and 4 we apply the same methodology, but we include average assets and sales, as well as indicators for whether the firm is located in the North and operates in the manufacturing industry as additional controls in the previous equation (full specification). Finally, in columns 5 and 6 we use 3-nearest neighbors propensity score matching. Probability scores are computed using the parsimonious and the full specification in column 5 and 6 respectively. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	(1)	(2)	(3)	(4)	(5)	(6)
Post X Connected	0.2763***	0.3705***	0.2894***	0.4030***	0.3467***	0.3489***
	(0.0704)	(0.0585)	(0.0757)	(0.0679)	(0.0529)	(0.0596)
Board Size	0.0424***	0.0382***	0.0409***	0.0763***	0.0758***	0.0914***
	(0.0116)	(0.0113)	(0.0119)	(0.0137)	(0.0107)	(0.0126)
Board Avg. Interlocks	-0.1903***	-0.1837***	-0.1900***	-0.1291***	-0.2034***	-0.1804***
	(0.0129)	(0.0123)	(0.0136)	(0.0155)	(0.0121)	(0.0147)
Firm and Year FE	yes	yes	yes	yes	yes	yes
Observations	204911	191081	195526	138078	287291	217446
Firms	15279	13242	14571	9278	19907	14624
R-Squared	0.002	0.002	0.002	0.002	0.002	0.001
Mean Dep. Var.	0.33	0.31	0.34	0.28	0.31	0.27

Table 8: Female Representation: Newly Hired Board Members

This table shows coefficient estimates for the following linear probability model $Y_{j,i,t} = \beta_1 Post_t * Connected_i + \beta_2 Connected_i + \beta_3 Post_t + \gamma X_{i,t} + \delta G_{j,t} + \varepsilon_{j,i,t}$, where individual j is a board member in firm i hired between year $t-2$ and year t . In column (1) $Y_{j,i,t} = 1$ if j female and zero otherwise. In column (2) $Y_{j,i,t} = 1$ if j is female and seats in the board of a target firm. In column (3) we restrict the sample to individuals currently seating in boards of target firms, and $Y_{j,i,t} = 1$ if j female and zero otherwise. In column (4) $Y_{j,i,t} = 1$ if individual j seats in the board of a target company and zero otherwise. $X_{i,t}$ and $G_{j,t}$ are firm-level and individual-level time-varying controls, including board size, average busyness of the board, individual tenure in firm i, and the native dummy. Firm-level clustered errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	(1)	(2)	(3)	(4)
	Female=1	Female=1 (in Target=1)	Female=1 (in Target)	in Target=1
Post X Connected	0.0264***	0.0069***	0.0116	-0.0775***
	(0.0036)	(0.0011)	(0.0108)	(0.0031)
Connected	-0.0240***	0.0071***	-0.0038	0.1387***
	(0.0027)	(0.0005)	(0.0068)	(0.0025)
Post	0.0356***	0.0094***	0.1422***	0.0269***
	(0.0025)	(0.0004)	(0.0088)	(0.0013)
Individual Controls	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Observations	464709	464709	39442	464709
R-Squared	0.036	0.007	0.076	0.053
Mean Dep. Var.	0.17	0.01	0.14	0.08

Table 9: Out-Target New Hires in Connected Firms

This table shows coefficient estimates for the following linear model $Y_{j,i,t} = \beta_1 Post_t * Connected_i + \beta_2 Connected_i + \beta_3 Post_t + \gamma X_{i,t} + \delta X_{j,t} + \varepsilon_{j,i,t}$, where $Post_t = 1$ if $t \geq 2012$ and zero otherwise, $Connected_i = 1$ if firm i shared at least one board member with a target firm in any of the three years before the reform, and $X_{j,t}$ and $X_{i,t}$ are individual-level (tenure) and firm-level (board size and average number of appointments) time-varying controls. We restrict the sample to out-target newly hired board members. In column 1 $Y_{j,i,t} = 1$ if individual j hired as a board member in firm i between year $t-2$ and year t is an outsider, and zero otherwise. In columns 2 to 4 we further restrict the sample to outsiders. *Foreign* is a dummy variable that takes value 1 if individual j is foreign born. *Native* is a dummy variable that takes value 1 if individual j within a 100km radius from the location of firm i. *Age* and *Current App.* are individual j's age in years and number of current board seats respectively. *First App.* is a dummy variable that takes value 1 if individual j is at her first appointment as board member. In column 5 we restrict the sample to new hires who are outsiders and have past board experience. $Contacts_{j,i,t}$ is computed by counting how many of firm i 's current board members individual j has worked with over the past 3 years. Clustered errors at firm level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

(a) Female

	(1) Outsider	(2) Foreign	(3) Native	(4) Age	(5) Current App.	(6) First App.	(7) Contacts
Post X Connected	0.0221***	0.0128***	-0.0094	0.1395	0.2915***	-0.0007	-0.1042***
	(0.0055)	(0.0034)	(0.0072)	(0.1437)	(0.0442)	(0.0049)	(0.0342)
Connected	0.0091**	-0.0000	-0.0455***	0.2448**	-0.3962***	0.0010	-0.0676**
	(0.0045)	(0.0027)	(0.0059)	(0.1175)	(0.0352)	(0.0040)	(0.0273)
Post	0.0252***	0.0106***	-0.0513***	3.2613***	0.0697***	-0.0214***	-0.0516***
	(0.0033)	(0.0019)	(0.0042)	(0.0879)	(0.0246)	(0.0029)	(0.0168)
Tenure	-0.1172***	-0.0065***	0.0200***	0.6886***	0.1506***	-0.2126***	1.8863***
	(0.0010)	(0.0009)	(0.0019)	(0.0394)	(0.0118)	(0.0016)	(0.0129)
Observations	114546	84055	84055	84055	84055	84055	70420
R-Squared	0.099	0.011	0.025	0.034	0.219	0.257	0.590
Mean Dep. Var.	0.73	0.06	0.63	45.39	2.96	0.16	4.56

(b) Male

	(1) Outsider	(2) Foreign	(3) Native	(4) Age	(5) Current App.	(6) First App.	(7) Contacts
Post X Connected	0.0043*	0.0184***	0.0118***	-0.7328***	0.3461***	0.0088***	-0.0343**
	(0.0023)	(0.0018)	(0.0033)	(0.0710)	(0.0299)	(0.0019)	(0.0161)
Connected	0.0094***	-0.0075***	-0.0463***	0.9988***	-0.4706***	-0.0107***	-0.1006***
	(0.0018)	(0.0014)	(0.0026)	(0.0567)	(0.0240)	(0.0015)	(0.0125)
Post	0.0195***	-0.0040***	-0.0361***	2.8828***	0.0304*	-0.0221***	-0.0430***
	(0.0015)	(0.0011)	(0.0021)	(0.0465)	(0.0184)	(0.0012)	(0.0085)
Tenure	-0.1351***	-0.0088***	0.0174***	0.8485***	0.2265***	-0.1424***	2.0310***
	(0.0012)	(0.0005)	(0.0009)	(0.0193)	(0.0081)	(0.0006)	(0.0064)
Observations	550885	395242	395242	395240	395242	395242	351420
R-Squared	0.159	0.011	0.021	0.025	0.206	0.172	0.586
Mean Dep. Var.	0.72	0.08	0.55	50.38	4.25	0.11	4.71

Table 10: Outcomes: Board Characteristics

This table shows coefficient estimates for the following specification $Y_{i,t} = \beta Post_t \times Connected_i + \gamma G_{i,t} + \alpha_i + \gamma_t + \varepsilon_{i,t}$ where $Post_t = 1$ if $t \geq 2012$ and zero otherwise, $Connected_i = 1$ if firm i shared at least one board member with a target firm in any of the three years before the reform, and $G_{i,t}$ are firm-level time-varying controls for board size and log of assets. α_i and γ_t represent firm and year fixed effects. In column 1 the outcome variable is board busyness, i.e., the average number of current appointments of board members in firm i and year t. In column 2 the outcome variable is the average distance between the place of birth of each board member and the location of firm i. In column 3 the outcome variable is average tenure, i.e., years of service in firm i's board. In column 4 the outcome variable is a dummy variable that takes value of 1 if any of the executive board members are female. In column 5 the outcome variable is a dummy variable that takes value of 1 if the firm's CEO is female. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	(1) Busyness	(2) Distance	(3) Tenure	(4) Any F exec	(5) F CEO
Post X Connected	-0.2783***	3.6936***	-0.6540***	0.0115***	0.0051***
	(0.0079)	(0.5191)	(0.0617)	(0.0023)	(0.0014)
logAssets	0.0417***	0.1259	0.0176	-0.0020***	-0.0005
	(0.0021)	(0.1414)	(0.0167)	(0.0006)	(0.0004)
Board Size	0.0285***	1.4163***	-0.4576***	0.0092***	0.0094***
	(0.0015)	(0.1021)	(0.0133)	(0.0004)	(0.0003)
Firm and Year FE	yes	yes	yes	yes	yes
Observations	290977	286499	112333	290977	290977
Firms	27217	26864	18976	27217	27217
R-Squared	0.025	0.003	0.424	0.002	0.008
Mean Dep. Var.	2.76	119.92	9.77	0.14	0.07

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 11: Outcomes: Firm Performance

This table shows coefficient estimates for a linear model of sales growth (panel a), profitability (panel b), and investments (panel c). In columns (1) to (3) the model specification is: $Y_{i,t} = \beta Post_t \times Connected_i + \theta Post_t \times Conn.toListed_i + \sigma logAssets + \delta BoardSize + \alpha_i + \gamma_t + \varepsilon_{i,t}$ where $Post_t = 1$ if $t \geq 2012$ and zero otherwise, $Connected_i = 1$ if firm i shared at least one board member with a target firm in any of the three years before the reform, and $Conn.toListed_i = 1$ if firm i shared at least one board member with a listed firm in any of the three years before the reform. α_i and γ_t represent firm and year fixed effects. In column (4) the model specification is: $Y_{i,t} = \beta Post_t \times Exposed_i + \sigma logAssets + \delta BoardSize + \alpha_i + \gamma_t + \varepsilon_{i,t}$ where $Exposed_i$ is a dummy variable that takes value 1 if firm i was connected to a target company with more than 20% female board members in the three years prior to the reform. Matched control firms in columns (2) and (3). Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

(a) Sales Growth

	(1) Core	(2) Match g	(3) PS Match	(4) Core
Post X Connected	-0.0057	-0.0138**	-0.0181**	
	(0.0056)	(0.0057)	(0.0073)	
Post X Conn. to Listed	0.0057	0.0061	0.0074	
	(0.0073)	(0.0073)	(0.0073)	
Post X Exposed				-0.0031
				(0.0046)
Other Controls	yes	yes	yes	yes
Firm and Year FE	yes	yes	yes	yes
Observations	204642	162254	99593	203459
Firms	22514	16604	10861	22494
R-Squared	0.082	0.090	0.086	0.083
Mean Dep. Var.	0.03	0.02	0.02	0.03

(b) Profitability

	(1) Core	(2) Match g	(3) PS Match	(4) Core
Post X Connected	-0.0014	-0.0013	-0.0020	
	(0.0010)	(0.0011)	(0.0014)	
Post X Conn. to Listed	0.0025	0.0025*	0.0027*	
	(0.0015)	(0.0015)	(0.0015)	
Post X Exposed				0.0002
				(0.0010)
Other Controls	yes	yes	yes	yes
Firm and Year FE	yes	yes	yes	yes
Observations	210746	165709	102515	209527
Firms	22819	16734	11009	22803
R-Squared	0.038	0.035	0.036	0.038
Mean Dep. Var.	0.02	0.02	0.01	0.02

(c) Investment Rate

	(1) Core	(2) Match g	(3) PS Match	(4) Core
Post X Connected	-0.0014	-0.0099	-0.0170	
	(0.0121)	(0.0125)	(0.0167)	
Post X Conn. to Listed	0.0675***	0.0682***	0.0687***	
	(0.0159)	(0.0159)	(0.0159)	
Post X Exposed				0.0289***
				(0.0105)
Other Controls	yes	yes	yes	yes
Firm and Year FE	yes	yes	yes	yes
Observations	199815	159105	96991	198681
Firms	22001	16346	10592	21981
R-Squared	0.039	0.040	0.040	0.039
Mean Dep. Var.	0.35	0.34	0.33	0.35

A Probability Decomposition

We model the probability of hiring a female board member *conditional* on drawing from the in-target pool as

$$P_{c,p}\left(F_j = 1|InTarget_j = 1\right) = S_p + b_{c,p}$$

where S_p is the share of women in the in-target pool, which is independent of firm type c, and $b_{c,p} \leq 1 - S_p$ is a type and time specific preference for female candidates. When $b_{c,p} = 0$, firms of type c at time p have no bias in favor or against women, and the probability above is equivalent to a random draw from the in-target pool.

From the expression above, it follows that

$$\begin{aligned}
\Delta_{c,p}P\left(F_j = 1|InTarget_j = 1\right) =& \Delta_{p|c=1}P\left(F_j = 1|InTarget_j = 1\right) - \Delta_{p|c=0}P\left(F_j = 1|InTarget_j = 1\right) \\
=& \left[(S_1 - S_0) + (b_{1,1} - b_{1,0})\right] - \left[(S_1 - S_0) + (b_{0,1} - b_{0,0})\right] \\
=& \left[b_{1,1} - b_{1,0}\right] - \left[b_{0,1} - b_{0,0}\right] \\
=& \Delta_{c,p}b_{c,p}
\end{aligned}$$

that is, any incremental effect of the reform on connected firms is due to *relative* changes in preferences. Notice that, if equation (6) holds, then $\Delta_{c,p}b_{c,p} = 0$, implying that we can reject the hypothesis of relative changes preferences. Moreover, under the same assumption, and since

$$\Delta_{p|c=1}P\left(F_j = 1|InTarget_j = 1\right) = \Delta_{p|c=0}P\left(F_j = 1|InTarget_j = 1\right)$$

we have that

$$\Delta_{p|c}P\left(F_j = 1|InTarget_j = 1\right) = \Delta_p\left(S_p + b_{c,p}\right) \tag{9}$$

that is, conditional on hiring from the in-target pool, connected and non-connected firms display the same incremental probability of hiring women after the reform, which depends on changes in the gender composition of the pool ($\Delta_p S_p$) and, possibly, on *identical* changes in preferences ($\Delta_p b_{c,p}$).

Building on the result in (9), we can examine the factors affecting the overall probability of hiring a woman who belongs to the in-target pool. Since

$$P_{c,p}\left(F_j =, InTarget_j = 1\right) = P_{c,p}\left(InTarget_j = 1\right) * P_{c,p}\left(F_j = 1 | InTarget_j = 1\right)$$

we can write the change between pre and post reform periods conditional on type c as

$$\begin{aligned}\Delta_{p|c} P\left(F_j =, InTarget_j = 1\right) =& \Delta_{p|c} P\left(InTarget_j = 1\right) * P_{c,0}\left(F_j = 1 | InTarget_j = 1\right) \\ &+ P_{c,0}\left(InTarget_j = 1\right) * \Delta_{p|c} P\left(F_j = 1 | InTarget_j = 1\right)\end{aligned}$$

and the overall differential change as

$$\Delta_{c,p} P\left(F_j =, InTarget_j = 1\right) = \Delta_{p|c=1} P\left(F_j =, InTarget_j = 1\right) - \Delta_{p|c=0} P\left(F_j =, InTarget_j = 1\right)$$

Using equation (9), we have that

$$\begin{aligned}\Delta_{c,p} P\left(F_j =, InTarget_j = 1\right) =& S_0\left[\Delta_{c,p} P\left(InTarget_j = 1\right)\right] \\ &+ \left[b_{1,0} \Delta_{p|c=1} P\left(InTarget_j = 1\right) - b_{0,0} \Delta_{p|c=0} P\left(InTarget_j = 1\right)\right] \\ &+ \Delta_p\left(S_p + b_{c,p}\right)\left[P_{1,0}\left(InTarget_j = 1\right) - P_{0,0}\left(InTarget_j = 1\right)\right]\end{aligned}$$

Under the additional assumptions that

$$b_{c,p} = b \tag{10}$$

that is, preferences are type and time invariant, and

$$P_{1,0}\left(InTarget_j = 1\right) - P_{0,0}\left(InTarget_j = 1\right) > 0 \tag{11}$$

that is, connected firms are more likely than non-connected firms to hire in-target candidates before the reform, we have that

$$\begin{aligned} \Delta_{c,p} P\left(F_j =, InTarget_j = 1\right) = & \underbrace{\Delta_{c,p} P\left(InTarget_j = 1\right)\left(S_0 + b\right)}_{Persistence} \\ & + \underbrace{\Delta_p\left(S_p\right)\left[P_{1,0}\left(InTarget_j = 1\right) - P_{0,0}\left(InTarget_j = 1\right)\right]}_{Composition} \end{aligned} \tag{12}$$

Equation (12) implies that changes in probability over time and across firm types depend on relative changes in the propensity to hire from the in-target pool (i.e., the persistence factor) and on changes in the share of women among in-target candidates (i.e., the composition factor). Both the effects of persistence and composition are (weakly) positive, since $(S_0 + b) = P_{c,0}\left(F_j = 1 | InTarget_j = 1\right) \geq 0$ and by the assumption in (11).

Finally, notice that the assumptions in (10) and (11) can be verified empirically. We do so in Table 8. In particular, column (3) shows that $P_{c,p}\left(F_j = 1 | InTarget_j = 1\right)$ does not depend on firm type c (i.e., on the variable $Connected_i$ in the regression) neither before nor after the reform, suggesting that connected and non-connected firms have similar preferences throughout the sample period. Thus, the identity in (10) holds in the data. In column (4) we show that connected firms are more likely to hire in-target candidates than non-connected firms, and particularly so before the reform. That is, $P_{1,0}\left(InTarget_j = 1\right) > P_{0,0}\left(InTarget_j = 1\right)$, as in equation (11).