Changing the Board Game: Horizontal Spillovers of Gender Quotas

Changing the Board Game: Horizontal Spillovers of Gender Quotas*

Luigi Guiso† Fabiano Schivardi‡ Luana Zaccaria§

March 2026

Abstract

We examine the effects of mandatory board gender quotas on unregulated firms that are connected to regulated ones via interlocking directorates. After the introduction of quotas, connected firms significantly increase their share of female directors relative to similar unconnected firms. The spillover effects are substantial—at least as large as the direct effects on regulated firms, challenging previous claims that quotas have no broader impact on women in business. Our results suggest that quotas indirectly broaden the supply of candidates for connected firms, along dimensions that include, but are not limited to, gender.

Keywords: Gender quotas laws, spillover effects, board interlocks

JEL classification: J24, J7, J78

*We are grateful to Daniel Ferreira, Ariadna Jou, Kai Li, Alexandra Niessen-Ruenzi, Elena Pikulina, Mounu Prem, Merih Sevilir, Denis Sosyura and to seminar participants at the Bank of Italy, WU Vienna, Goethe University, UBC, Queen Mary University of London, University of Essex, the 5th Annual Boca-ECGI Corporate Finance and Governance Conference, EWFC 2025, the 2024 MIPP Workshop in Policy Evaluation, and the 13th IBEO Workshop for helpful discussions and suggestions. Fabiano Schivardi thanks the ERC for financial support (ERC grant 835201).

†EIEF and CEPR.

‡LUISS, EIEF and CEPR. Corresponding author. Address: Luiss University, Viale Pola, 12 00198 Roma Italy, fschivardi@luiss.it.

§LUISS, EIEF and CEPR.

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 on improving the gender balance among directors of listed companies).

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] To comply with quotas mandates, most listed companies increased the proportion of female directors on their boards. Regulators, however, hoped for the impact of such reforms to reach beyond the boardrooms of the companies subject to the new regulation. They aimed to support the advancement of other female employees within regulated firms and, more generally, to inspire younger women by providing role models. These goals are explicit in the European Union Directive 2381 of 2022, which 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 policymakers' 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). Maida & Weber (2022) reach the same conclusion 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

[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.

externalities fuels the ongoing controversy over quota policies in the corporate world.[2]

To inform this debate, we investigate spillovers of mandated board quotas along a novel dimension: we focus on *horizontal* rather than *vertical spillovers*. Specifically, we analyze firms that are not legally subject to a quota requirement, i.e., non-target firms, but are connected to target firms through board overlaps in the years immediately prior to the reform. Previous literature shows that board overlaps can signal the presence of interactions among firms, which typically involve information exchange across management teams (Cai & Sevilir (2012), Cabezon & 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 study 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 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 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 bias. Importantly, we show that the effects mostly arise from connections with companies that were most affected by the reform, i.e., target companies with fewer than 20% women on board as of 2011. Interestingly, connections through shared directors are distinct from connections through ownership: only a small fraction of connected firms has target companies among their shareholders and the results are unaffected when they are dropped from the sample. Our results do not depend on whether firms operate in

[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.

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. While on similar trends before the reform, female representation in boards of connected firms grows significantly faster than that in non-connected firms in the years following it. 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 added approximately 1,800 women to their boards — an effect that is close in size to 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. Importantly, 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 & 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

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

on listed companies, as the opening quote in this section suggests.

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 21% of the total effect, implying that the goal of preserving strategic connections is a relevant spillover mechanism, albeit 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.

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. Specifically, we build on the growing evidence that compliance with board quotas forced companies to adopt a more professional approach to board members selection (see Wiersema & Mors (2016), Ferreira et al. (2017), Ferrari et al. (2022), Ferri et al. (2023)), for example by relying more on professional head hunters rather than focusing on candidates within the social network of current managers and directors.[4] This is consistent with the framework proposed by Matveenko & Mikhalishchev (2021), where rationally-inattentive hiring managers with limited information or wrong priors on candidates from minority groups tend to reject them *a priori*. The introduction of quotas generates an incentive to acquire information on such candidates, increasing the overall information production, particularly about minority candidates. Our data show that indeed target companies comply with the law largely by expanding their search and hiring women with no previous directorship positions on public boards. 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

[4]Knyazeva et al. (2013) show that companies rely heavily on the local labor market for potential directors when hiring new board members. Using a sample of 9,801 director appointments during 2003-2014, Cai et al. (2022) show that the vast majority of new directors have professional ties to incumbent boards. In the related literature on executives hiring, Cziraki & 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 whom current directors have previously worked with. Similarly, Sauvagnat & 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 pool of qualified 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 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 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 & Weber (2022) do it for Italy. Neither paper finds evidence of vertical spillovers.[5] Other papers examine firm performance in target firms, and largely confirm the negative effects documented by Adams & Ferreira (2009)'s seminal paper on gender diversity in the board. Ahern & Dittmar (2012) find that the Norwegian reform had a negative impact on firm valuation and operating performance.[6] Similar conclusions are reached by Matsa & 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,

[5]Also Matsa & Miller (2025) show that, after the introduction of quotas in nine European countries, "women's representation among CEOs or other senior executives hardly changed" (p. 529)

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

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 this is due to 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, indicating that the policy increases female representation even in the boards of companies for which it is not binding. Importantly, the spillovers effect we document also have implications for the identification strategies used in earlier evaluations of board gender quotas. Most existing studies rely, implicitly or explicitly, on a stable unit treatment value assumption (SUTVA), under which the outcomes of non-targeted firms are unaffected by the policy and therefore provide a valid counterfactual for targeted firms. If this assumption does not hold, standard treated-versus-control comparisons may not recover the full effect of the reform.

Closely related to our paper, Del Prete et al. (2024) focus on Italian banks to examine horizontal spillovers of the same reform we analyze, and 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.[7] Their evidence suggests ownership linkages do not necessarily imply that target firms extend the application of the law to non-target owned firms, at least in the banking sector. Different from this study, we focus on spillovers transmitted through shared directors—far more common than ownership. Our study leverages a comprehensive dataset covering all joint stock companies — and their directors— in Italy, which enables a robust identification strategy and allows us to distinguish our channel versus other spillover mechanisms (e.g., ownership links, similarity or proximity, anticipation of regulatory extensions).

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

[7] An earlier study by Bongiovanni et al. (2023) finds that, after the reform, there is a change in the trend of female representation in non-listed banks, suggesting the presence of horizontal spillovers. However, the authors openly acknowledge the impossibility of making any causal claim in their setting "due to the absence of a suitable control group" (p.1864), which prevents them from using quasi-experimental empirical designs, such as difference-in-differences methods. Moreover, a concurrent reform prohibiting interlocking directorates in the banking sector substantially reshaped the composition of bank boards (Barone et al. (2025)).

hired before the quota." Bertrand et al. (2019) find that female directors appointed after the Norwegian reform were more qualified and better paid. Giannetti & 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 in target firms, and conclude that the reform affected the recruiting process for the entire board. Building on this evidence, we show that the changes in the hiring practices that occurred in companies subject to the quota requirements also affected connected firms, and are at the heart of the spillover mechanism.

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 corporate jobs in favor of women, who are typically under-represented in directorship positions.

Unlike otherwise similar quota laws, the Italian Golfo-Mosca law (named after 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.[8] 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 Italian corporate law, the board comprises both a directors' board and an auditors' board, and the mandatory quotas apply to both subgroups.

Board members can serve for a maximum period of three years after their election or re-election. Therefore, in its initial formulation the law was effectively binding only for a limited period of time (i.e., 12 years at most). However, in 2016 it became permanent for state-controlled companies, though its applicability after the transitional period was limited to the board of directors only (i.e., the board of auditors was 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.

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

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

funds, as well as detailed information on company owners and board members. We obtained annual data from 2003 to 2022 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 quota law.[9] For each firm-year observation in the sample, we retrieve financial statements and firm demographics (i.e., headquarters location, industry classification, year of founding), 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 home 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 on 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 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 in which the largest shareholder is a public entity (e.g., the ministry of finance, state development banks, municipalities).[10] We refer to listed companies as those firms that have shares listed on the main Italian stock exchange, and we include in this group the (few) companies that are both listed and state-controlled, as the stricter quota requirements for listed firms apply in this case. We refer to state-controlled and listed firms jointly as *target* firms (i.e., firms that are

[9]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 a 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.

[10]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/)

required to comply with the quota law) and to the other companies as *non-target* ones.[11]

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 year in which the law was enacted, 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 points (from 8% to 11%) in the 6 pre-reform years, and by approximately 20 percentage points in the 10 post-reform years. 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 started increasing after 2012 reaching over 2 members at the end of the sample period.

The figures above suggest that, roughly speaking, each listed (state-controlled) firm hired

[11] Companies listed on the "growth" segment of the Milan stock exchange dedicated to SMEs ("Euronext Growth Milan") are not included in the target group since they are exempted from certain governance requirements, including gender quotas.

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

Before investigating the spillover effects of the quota law, it is useful to provide an estimate of the direct effects on target firms, which will serve as reference to assess the relative magnitude of our main results. There are at least two ways to quantify these effects. 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 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 and board size of firms subject to quotas, the reform had the potential to generate 2,610 new board positions for women (+223% with respect to 2011). Though only a rough estimate, we interpret this figure as the upper bound for the direct effects of the reform, and we will later on compare it with our (most conservative) estimates of the indirect (i.e., spillover) effects on non-target firms.

3.3 Core Sample: Connected vs Non-Connected Firms

Our core sample comprises the universe of non-target joint-stocks non-listed companies not currently undergoing liquidation or restructuring, totaling approximately 26,000 firms per year. Connected firms within this sample are identified by examining board overlaps in the years preceding the implementation of the 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 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. Among these, 45% are connected with state-controlled firms, 29% are connected with listed firms, and the rest are connected with both. These proportions reflect the larger average number of state-controlled companies as compared to listed firms in the period 2009-2011 (1,100 and 200 respectively), but also the smaller average size of their boards (7.9 versus 13.6).

Figure 5, Panel (a), shows the evolution of the share of female board members for connected and non-connected firms. 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 from 17% to 19%). In other words, it appears as if the female board share in connected firms moves to a different, steeper trend after the reform, while in non-connected firms it continues on the same path as before.

Table 1, columns 1 and 2, shows the main characteristics of connected and non-connected firms. Connected firms have larger and busier boards (measured by the average number of board seats per board member). This is, of course, to be expected, as it is more likely to observe a "connection" with a different company (target or not) when we consider a larger number of individuals (i.e., a larger board), and more so if these individuals hold multiple director positions ("busy" directors). Connected firms also have 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 their shareholders, and, to a lesser extent, to own shares in listed firms. It is worth emphasizing, however, 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 own 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 match treated (i.e., connected) firms to non-connected firms with high estimated probability of treatment, and later employ this matched sample for our analysis (see Section 4). 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 2009 to 2011. We then select as controls non-connected firms with propensity scores higher than the median score in the connected sample. Our choice of explanatory variables in (1) is based on the fact that, as noted earlier, firms are significantly more likely to qualify as "connected" if they have a large and/or busy board. This mechanical relationship originates from the firm's choice of board size and composition, which is arguably endogenous. For example, firms may choose to hire directors from target companies, thus establishing a connection, in order to rely on their contacts and advice (see Field et al. (2013)). Our matching procedure selects as controls firms that, like those in the connected sample, endogenously choose boards with a high share of "busy" directors. In this way, we identify firms that similarly value the potential benefits that "networked" directors may provide, for instance through their advisory role. This helps reduce the unobservable differences between treated and controls that may influence our outcome of interest. Indeed, Figure 5, Panel (b), shows that, differently from the dynamics depicted in Panel (a) for the full sample, in the matched sample trends in female board representation between treated and control firms before the reform are parallel, as the observations nearly overlap in every year before 2012. This validates our empirical strategy, suggesting that the connection with a target firm *per se* does not affect within-firm variations in the share of female board members over time other than through the exposure to the quotas reform. Column 3 of Table 1 illustrates other characteristics of matched non-connected firms. The matching algorithm significantly reduces differences along most dimensions (see column 4). Noticeably, the differences between the treated and the control group in board and firm size, i.e., the largest imbalances in firm characteristics in the full sample, drop significantly in the matched sample, as shown by the standardized difference measures. This is not surprising since larger (and more complex) firms tend to have larger boards, and board size increases the propensity scores computed with our matching algorithm. [12] Importantly, the share of female board members in matched firms (14.97%) is lower than that in other non-connected firms (16.29%) and significantly closer to that in treated firms (14.10%), as measured in the years before initiating a connection.[13]

[12]This homogeneity in firm size between the treated and the matched-control group further increases confidence in our identification strategy, as firm size may correlate with female leadership. This is true in our data, where, before the quota reform, firms in the bottom fifth percentile of the assets distribution had almost double the share of women on board compared to firms in the top fifth percentile (15% versus 8%). But this pattern also appears to be a broader phenomenon. For example, it has been estimated that, in the U.S., while one in four businesses with revenue of $1 million or more are owned and led by women, only 7.2% of private companies worth over $1 billion (and only 7.8% of S&P500 companies) have a female CEO (See the 2024 Women CEOs in America Report published by the Women Business Collaborative association, retrieved online on April 2025: https://wbcollaborative.org/women-ceo-report/the-report/2024-executive-summary/).

[13]The average share of female board members in connected firms is 12.48% when we measure it after connections are activated. This is a consequence of our definition of treatment. When firms establish a new connection with target companies, the connecting director—almost always drawn from a predominantly male

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 for spillover effects, 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

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

where $F\_Share_{i,t}$ is the share of female members in the board of non-target firm i in calendar year $t \in [2006, 2022]$ expressed in percentage points, α_i are firm fixed effects, γ_t represent year dummies, and $\mathbb{1}_{\{s=t\}}$ is a dummy equal to one for $s = t$. We use 2011, the year before the Golfo-Mosca law was implemented, as the base year. Parameters β_t trace the pattern of the difference in the dynamics of the women 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 percentage point larger increase in female representation between 2006 and 2011. This negative pre-trend reverses its course after the reform. Beginning in 2012, connected firms display a significantly steeper trend. By the end of the sample period, the share of female board members increases by 2.6% more than that of non-connected firms compared to 2011 (i.e., $\hat{\beta}_{2022} = 2.6$). Hence, the post-2011 dynamics in

pool (see Figures 1 and 2)—typically replaces an incumbent non-executive director, a position where women are more frequently represented. This reshuffling mechanically tilts the new board composition towards a lower female share as compared to otherwise similar non-connected firms.

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), where 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 the number and busyness of directors, and other correlated characteristics, e.g., firm size. In other words, the path towards gender balance in corporate boards was particularly slow in relatively larger and more complex firms before the reform.

Our results suggest that the quotas 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 2 revisits the evidence in Figure 6 by examining different outcomes in the full sample (Panel 2a) and in the matched sample (Panel 2b) with the following specification

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

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

In column (1) we consider $Y_{i,t} = \Delta Share_{i,t} \equiv (F\_Share_{i,t} - F\_Share_{i,t-1})$, i.e., the year on year variation in the share of female board members in firm i. In column (2) we set $Y_{i,t} = F\_Share_{i,t}$ - the board female share. Notice that the specification in column (1) ensures that the estimated coefficient $\hat{\beta}$ can be interpreted as an average change in *trend* in the post reform period, rather than an increase in the *level* of female representation, as in column (2), which could be due to the continuation of pre-existing trends. In this sense, the specification in column (1) is the most robust to potential violations of the no pre-trends assumption, and the closest in spirit to the estimation exercise plotted in Figure 6. For this reason, we use $\Delta Share_{i,t}$ as the main outcome for the robustness tests presented in Section (4.4). In column (3) 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 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) of Panel (a) 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. This corresponds to a 1 percentage point higher level of female share in the post-reform period (column 2). The result in column (3) reveals that $F\_Share_{i,t}$ increases not only in the intensive margin (as per the coefficient estimate in column 2) 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. One way to meet the quota requirement would be to reduce the number of male directors while leaving the number of female directors unchanged. This would lead to a smaller board, with a larger share of women, and it would require no substantial changes to the recruitment process of directors. Instead, columns (4) and (5) (in both panel a and b) show that the number of female directors increases more than the number of male directors. In fact, the larger share of women is achieved despite a small increase in board size (as implied by the fact that the sum of the coefficients in (4) and (5) is larger than zero). This should not be interpreted as a claim of positive causal effects of quotas on board size (though we cannot exclude it), rather as further evidence in support of the view that the reform affects the board composition of connected firms.

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 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.[14] 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.

The presence of second-order spillovers affects the interpretation of our main results, as some control firms may themselves have been affected by the reform through indirect connections. As a result, the coefficient estimates in equations (2) and (3) should be interpreted as lower bounds of the overall effects of the reform on non-target firms. An accurate assessment of total spillovers should therefore include both the main (first-order) spillovers and the second-order spillovers. This is the approach we take in the next sub-section, where we quantify total spillover effects. Notice, however, that only a fraction of the control sample – namely firms connected to connected firms – is indirectly affected, and that second-order spillovers are considerably smaller in magnitude than the first-order effects. This suggests that the violation of SUTVA in this context is likely to be mild. Consistent with this interpretation, in Section A of the Online Appendix we construct formal tests following Boehmer et al. (2020), which indicate that indirect effects are positive but small and not statistically significantly different from zero. We also show that dropping indirectly-connected firms from the control sample has negligible effects on our preferred estimates.

Overall Spillover Effect We summarize the total horizontal spillover summing first and second order effects. Such effects can be approximated as follows

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

where $\Delta F_{2011-2022}$ is the additional number of female board members due to the spillover effect of the reform, k is the degree of the connection (k =[direct, indirect]),$\hat{\beta}^k{}_{2022}$ is es-

[14] To avoid 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 appointments of board members in 2009 to 2011.

timated with equation 2, $\#Boards_{k,2011}$ is the number of k-connected firms in 2011, and $\overline{BoardSize}_{k,2011}$ is the average size of k-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% of the size of the estimates for the direct effects of the law on female representation in target firms provided in Section 3.2.1.

4.2 Persistence, Timing and Intensity of Connections

The results above show that board overlaps with target firms in 2009-2011 are strongly associated with an increase in female representation after 2012 and onward. As we explained in the previous section, in our definition of connected firms we use board overlaps before the reform to avoid running into endogeneity problems. If the spillover mechanism hinges on board connections, however, our results should be stronger when we compare treated firms that remain connected for a significant portion of the post-reform period with "never-connected" control firms. To verify that this is indeed the case, we split the sample of treated firms in three groups, based on the share of post-reform years when connections with target companies are still active (100%, between 50% and 100%, less than 50%). We then estimate equation (2) comparing each of these three sub-groups with "never-connected" control firms. The results of this exercise are illustrated in Figure 8, for both the full and the matched samples (panels (a) and (b) respectively). Interestingly, when we consider treated firms that kept their connections active for the entire post-reform period, the final point estimates ($\hat{\beta}_{2022}$) are almost three times larger in magnitude as compared to our main specification (6.6 vs 2.6 in the main sample, 6 vs 2.1 in the matched sample). These estimates become progressively smaller for the samples with shorter connection periods. In other words, spillovers are proportional to the persistence of connections.

In a similar spirit, we next explore the effects of ongoing (rather than past) connections. 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 $Post_t$. Because Italian corporate law requires that board renewals occur every 3 years, 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 3, 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 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 $MeanTotConnection_i = \frac{\sum_t(\sum_b n_{b,i,t})}{3}$, where n is the number of current seats in the board of target firms held by board member b in firm i at time $t \in [2009, 2011]$. 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 4 and 5 of Table 3 we show the coefficient 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. The effect of connections increases significantly with intensity. 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 sample distribution - amplifies the baseline effect by approximately 0.08% (0.15%).

These results are consistent with a mechanism based on information sharing between target and connected firms. Having persistence or ongoing connections or connections with multiple target companies augments and diversifies the sources of information available to connected firms, thus amplifying the spillover effects.

4.3 Exposure to the Reform

As we explain in Section 2, not all target firms are 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 firms, the quota of $\frac{1}{3}$ becomes 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 in 2016. 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 2016. This is indeed what we observe. Figure 9 shows coefficient estimates for $\hat{\beta}_t$ from equation 2 computed separately for firms connected to listed companies and those connected exclusively to state controlled entities, dropping from the control sample firms connected to state and listed companies respectively. Coefficients for the first group are larger after the reform but diverge significantly 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.

Next, we exploit a second dimension of heterogeneity. As of 2011, target firms displayed significant differences in female representation on boards. Approximately 40% of them had no women, and 50% had less than 10% female share. However, for 20% of the target firms sample, the share of women was large enough to satisfy the quota requirement for the first board renewal (20%). These firms were 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 $Exposed_i$ ($NotExposed_i$), which takes value 1 if firm i is connected to a target firm with less (more) than 20% women on board in 2011.[15] We estimate the following regression

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

Figure 10 displays the estimated coefficients $\hat{\beta}_t$ and $\hat{\theta}_t$. For the *Exposed* group, the total post-reform estimated differential increase in female representation is 2.9% (i.e., $\hat{\beta}_{2022} = 2.9$), marginally larger than our baseline estimate. No significant effect can be detected for the *NotExposed* group (i.e., $\hat{\theta}_{t>2011}$ is not statistically different from zero). Notice that this specification does not suffer from potential issues related to unobservable differences between connected and non-connected firms, since both *Exposed* and *NotExposed* firms are connected to target companies. Indeed, our estimates show overlapping pre-trends for these two groups.

Both exercises illustrated in this section show that the effects we document are stronger for firms connected to companies that were more affected by the reform. Similar to the evidence we illustrated in the previous section, this heterogeneity is consistent with a mechanism based on information sharing (on which we elaborate later in Section 5), while it is less compatible with other explanations that do not depend on firm-specific exposure, such as reputation concerns or the anticipation of an extension of quota requirements to non-listed, non-state-controlled firms.

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 these information exchanges 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.

Alternative Spillover Channels A first possibility is that connected firms, which are generally larger (see Table 1), are more likely to seek public listing in the immediate future,

[15] In case of multiple connections we consider the maximum female share among the boards of target companies.

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 geographical proximity rather than firm board 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 4, where Panel 4a and Panel 4b 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 2 for comparison. The estimate 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 occurrence, 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 estimates are not affected by the inclusion of these controls. Moreover, the coefficients of the two proximity variables are not statistically distinguishable from zero, 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 5. In column (1), we repeat the same exercise as in column (1) of Table 2, 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, although common ownership amplifies spillovers for a small subset of connected firms, its overall contribution to the effects of the board quota reform remains relatively limited.

Alternative Matching Algorithms In Table 6 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 11. 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

To summarize, the results presented 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 may stem from two distinct—though not mutually exclusive—mechanisms, which we examine in this section.

The first mechanism relies on the idea that the reform may have affected the *supply* of candidates that connected firms face when appointing new directors. This channel can be both direct and indirect. The direct-supply channel hinges on 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 the boards of 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, by contrast, stems from spillovers of new recruiting processes in target companies. Previous research shows that, to comply with quota mandates, shareholders of targeted companies reduce their reliance on personal networks and 'professionalize' the executive search process—for example, by engaging consultants to identify candidates shortlists (see, for example, Wiersema & Mors (2016), Ferreira et al. (2017), and Ferri et al. (2023)).[16] In Italy, several large companies, together with law firms and HR specialists, started collaborating to offer training, mentoring and networking opportunities for current or potential female directors.[17]We conjecture that these changes in search technology may

[16] Matveenko & Mikhalishchev (2021) develop a model in which a rationally inattentive manager selects board members from different groups (men and women). The manager holds a prior on candidate quality, with lower precision for the minority group, and can choose to acquire additional information. They show that quotas incentivize information acquisition: while an unrestricted manager may bypass screening by selecting only from the majority group, a manager subject to a quota always chooses to acquire information on minority candidates.

[17]For example, the "InTheBoardRoom" project, established in 2012, promotes women's inclusion in boards through training courses and networking events. As of 2025, over 600 individuals participated in the various editions of the training program.See https://intheboardroom.it/, retrieved on 2nd July 2025.

spill over to connected firms as target firms share information on newly selected candidates for directorship positions, while hiring a subset of them. These candidates' pools become 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, additional 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 may reflect reputation concerns or a shift in corporate culture triggered by direct exposure to a more equitable recruitment process.

Before presenting evidence of the mechanisms, it is useful to define the two pools of candidates from which connected and non-connected firms can draw when appointing new directors. The first is the set of individuals currently seated on the board of target firms, labeled *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, labeled *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. To do this, we move our analysis from the board level to the individual director level.

In-target Pool. To investigate the direct-supply channel, we focus on newly appointed board members and decompose the overall increase in the propensity to appoint women into in-target and out-target components. We estimate the following model:

$$Y_{j,i,t} = \beta_1 Post_t \times Connected_i + \alpha_i + \gamma_t + \varepsilon_{j,i,t} \tag{4}$$

where $Y_{j,i,t}$ is a characteristic of board member j appointed in firm i between year $t-2$ and year t, and α_i and γ_t are firm and year fixed effects.

The results are presented in Table 7, columns (1) and (2). In column (1) we estimate the probability of new director j being female, i.e., $Y_{j,i,t} = F_{j,i,t}$, where $F_{j,i,t} = 1$ if j is female and zero otherwise. Consequently, coefficient $\hat{\beta}_1$ provides an estimate of the differential probability of hiring a woman in connected firms after the reform. As expected, this coefficient is positive–equal to 0.038– and highly significant, confirming our previous firm-level results (see Table 2, column 5). In column (2) we estimate the joint probability of new director j being female *and* currently seating on the board of a target firm, i.e., $Y_{j,i,t} = F_{j,i,t} \cdot InTarget_{j,i,t}$, where $InTarget_{j,i,t}$ takes value 1 if j currently seats in the board of a target company, and zero otherwise. 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 21%, as quantified by the ratio of the coefficients $\hat{\beta}_1$ in column (2) over that in column (1).

We further explore the result in column (2), by noticing that the joint probability of new director j being female *and* currently seating in the board of a target firm can be expressed as follows

$$Pr\left(F_{j,i,t} \cdot InTarget_{j,i,t} = 1\right) = Pr\left(InTarget_{j,i,t} = 1\right) \times Pr\left(F_{j,i,t} = 1 | InTarget_{j,i,t} = 1\right)$$

implying that differential changes in the left hand side must reflect a combination of changes in the likelihood of connections, in-target pool composition, and preferences (for a formal derivation of this decomposition and its mapping into our empirical strategy see Section B of the Online Appendix). Intuitively, if connected firms hire more from the in-target pool even after the reform (i.e., connections are persistent), the larger share of women among new hires in connected firms can mechanically reflect the increasing female representation in this pool mandated by the quota laws ("composition" effect). Additionally, connected firms may also change preferences with respect to women as a consequence of the exposure to the reform ("preference" effect). While persistence affects the likelihood of connections ($Pr\left(InTarget_{j,i,t} = 1\right)$), composition and preferences affect the probability of hiring a female candidate, *conditional* on picking from the in-target pool ($Pr\left(F_{j,i,t} = 1 | InTarget_{j,i,t} = 1\right)$).

First, we examine whether connections are persistent. To do so we estimate the following model

$$InTarget_{j,i,t} = \beta_1 Post_t \times Connected_i + \beta_2 Connected_i + \gamma_t + \varepsilon_{j,i,t}$$

Our estimates, presented in column (3), show that indeed connected firm are more likely to hire in-target candidates throughout the sample period ($\hat{\beta}_2 = +15\%$), although this difference becomes less pronounced after the reform ($\hat{\beta}_1 = -9\%$). This is to be expected since, by definition, non-connected firms do not employ in-target directors in the three years before the reform. Next, we examine the relative contribution of the "composition" versus "preference" effects. We do so by restricting the sample to in-target board members only and estimating the following model

$$F_{j,i,t} = \beta_1 Post_t \times Connected_i + \beta_2 Post_t + \alpha_i + \varepsilon_{j,i,t}$$

Notice that, once we condition on $InTarget_{j,i,t} = 1$, the composition effect is common to both connected and non-connected firms and it is captured by the estimate $\hat{\beta}_2$ (alongside, potentially, equal changes in preferences across the two groups), while differential changes in preference are captured by the estimate $\hat{\beta}_1$. The results are presented in column (4) and show that $\hat{\beta}_2$ is large (13%) and significant, implying that the probability of hiring a woman from the in-target pool increases dramatically after the reform for both connected and non-connected firms. Notice that this coefficient approximately corresponds to the average increase in the share of female directors in target boards after the reform, implying that it largely captures the composition effect. Importantly, the coefficient of the interaction term $Post \times Connected$, $\hat{\beta}_1$, is small (+2%) and not statistically different from zero. Therefore, we do not find support for the conjecture that connected firms experience a differential change in preferences for women.

To summarize, the results in Table 7 show that approximately 21% of the increase in the share of new female hires in connected firms comes from direct-supply effects, totally driven by composition. Within the in-target pool, we find no evidence for the demand-driven mechanism, i.e., changes in preferences for female candidates.

Out-target Pool. We now examine out-target newly appointed board members to investigate to what extent the remaining 79% of the effect is explained by indirect-supply or demand effects. As mentioned earlier, the pool of out-target candidates is not fully observable and, to the best of our knowledge, systematic information on the recruiting processes for directors in private firms is non-existent. However, the characteristics of out-target new directors may shed light on the underlying mechanism. In particular, the indirect-supply mechanism predicts that directors hired after the reform in connected firms should be 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, assuming that a more professionalized search process also affects the recruitment of male directors.

We test these conjectures by examining the characteristics of newly appointed out-target directors—both male and female. In Table 8 we estimate the following model

$$Y_{j,i,t} = \beta_1 Post_t \times Connected_i + \beta_2 Post_t \times Connected_i \times F_j + \beta_3 Post_t \times F_j + \beta_4 F_j + \alpha_i + \gamma_t + \varepsilon_{j,i,t}$$

In column (1) the dependent variable is a dummy equal to 1 if individual j is an outsider, i.e., with no link to the company other than through their seat in the board. The estimates of parameters β_1 and β_2 suggest that new male board members in connected firms are relatively more likely than women to be insiders (e.g., executives or shareholders) after the reform. In other words, as expected, the replacement of male with female board members mostly occurs among outside directors. In columns (2) to (6) we restrict the sample to outsiders, to examine how their characteristics change. The results in column (2) show that neither female nor male new hires are more likely to be foreign born, while new hires of both genders are marginally younger (column (3)). Men hired by connected firms after the reform are also less busy than women (column (4)).

We next examine whether new hires are at their first-ever board appointment. Consistent with the idea that the supply of directors expands primarily by including women who are new to the profession, newly hired women in connected firms after the reform are more likely to be at their first board appointment (column (5)). We find no significant effect for men.

In column (6) we further restrict the sample to new hires with prior board experience, and we use a proxy for past interactions with current board members, $Contacts_{j,i,t}$, as the outcome variable. $Contacts_{j,i,t}$ is computed as the number of firm i 's current board members with whom individual j has served over the previous 3 years.[18] While we can only track interactions occurring within boards, we can observe whether two individuals previously served together 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

[18]This measure is similar to that of first-degree connection between new and incumbent board members in Cai et al. (2022), which is defined as a dummy variable that takes value 1 when both have worked (in either director or executive capacity) at the same company during an overlapped period of time.

of individual j's "membership" in firm i's network. Our results show that $Contacts_{j,i,t}$ decreases significantly for all new hires in connected firms after the reform, with a stronger effect among women.

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.

Unfortunately, differently from the in-target case, we cannot observe candidates that were not ultimately selected. This implies that we cannot directly study demand effects for the out-target pool. Notice, however, that if the demand-based mechanism cannot explain the spillover effect when firms hire from the in-target pool, as the results in Table 7 suggest, it would only explain the remainder of the effect under the strong assumption that the firm's preferences regarding the gender of board members vary depending on the pool from which it hires.

Finally, 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 costs when searching for board directors.

Board Characteristics. The total effect of the reform on board member characteristics depends both on shifts in the gender composition and in within-gender variation in individual attributes, which we have analyzed separately so far. To capture the overall effect, we complement the individual-level analysis with a board-level approach, estimating the following specification:

$$Y_{i,t} = \beta Post_t \times Connected_i + \gamma G_{i,t} + \alpha_i + \gamma_t + \varepsilon_{i,t} \tag{5}$$

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 9 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 individual-level analysis, we find 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 increases by approximately 8% and 7% as compared to their base levels of 14% and 7% respectively.

Finally, in Section C of the Online Appendix we explore the effects of the spillovers on firm's performance by means of a TWFE difference-in-differences estimation where outcomes are measured in terms of sales growth, profitability, and investment rate.[19] The previous literature focused on target firms, finding evidence for negative effects of gender quotas on performance (Adams & Ferreira (2009), Matsa & Miller (2013), von Meyerinck et al. (2021)). We also uncover limited evidence of negative effects, but the results are not robust, suggesting small–if any– effects on performance. We attribute this difference to the fact that, in our setting, the increase in female representation stems from a voluntary firm decision rather than a legal mandate: when choosing a woman, firms are likely to do so based on perceived managerial value instead of in response to regulatory compliance.

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 regulated firms. After the reform, connected firms increased the share of female board members significantly more than comparable non-connected firms.

[19] 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 $(PPE_t - PPE_{t-1} + Depreciation_t)/PPE_{t-1}$, where PPE is the value of tangible capital in property, plants and equipment. Final values are 2% winsorized.

By pushing the literature beyond its current focus on vertical spillovers (or lack thereof), our findings counter the prevailing pessimism about the effectiveness of board quota laws, and illustrate the potential mechanisms underlying positive horizontal spillovers. Our results suggest that changes in the relevant pool of candidates, rather than a shift in preferences for gender diversity, drive the increase in female representation on the boards of connected firms.

We find that the spillovers occur through two channels. First, to maintain strategic relationships, connected firms hire directors from the pool of *current* board members in target firms. As this pool becomes more gender-balanced due to the quota law, so does the set of new hires in connected firms. This mechanism accounts for approximately 21% of the differential increase in female new hires on connected firms' boards. The second channel, driven by information-sharing, explains the remaining 79%. As target firms collect information on *potential* director 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 not constrained by regulatory requirements, suggesting that firms find it beneficial to broaden their search and improve their chances of finding qualified candidates.

Overall, these findings suggest that the reform acted as a shock to the labor market for board members, redirecting the search towards segments of the population - women and network outsiders - that were traditionally overlooked. As a result, new candidates are now part of the pool from which both target companies and connected firms 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. Once qualified women gain experience as directors and enter the lists maintained by professional headhunters, their chances of being hired again in the future likely increase, irrespective of the presence of mandatory quotas. While one could argue that a reversal might occur due to strong gender biases in corporate environments, particularly in managerial roles, our results speak against this possibility. Connected firms adapt smoothly to the changing gender composition of the candidates' pool - despite not being legally required to do so - rather than resisting it.

References

Adams, R. B. & Ferreira, D. (2009), 'Women in the boardroom and their impact on governance and performance', *Journal of financial economics* **94**(2), 291–309.

Ahern, K. R. & Dittmar, A. K. (2012), 'The changing of the boards: The impact on firm valuation of mandated female board representation', *The Quarterly Journal of Economics* **127**(1), 137–197.

Antón, M., Ederer, F., Giné, M. & Schmalz, M. C. (2024), Innovation: The bright side of common ownership?, Technical report, National Bureau of Economic Research.

Azar, J., Schmalz, M. C. & Tecu, I. (2018), 'Anticompetitive effects of common ownership', *The Journal of Finance* **73**(4), 1513–1565.

Barone, G., Schivardi, F. & Sette, E. (2025), 'Interlocking directorates and competition in banking', *The Journal of Finance* **80**(4), 1963–2016.

Bertrand, M., Black, S. E., Jensen, S. & Lleras-Muney, A. (2019), 'Breaking the glass ceiling? The effect of board quotas on female labour market outcomes in Norway', *The Review of Economic Studies* **86**(1), 191–239.

Boehmer, E., Jones, C. M. & Zhang, X. (2020), 'Potential pilot problems: Treatment spillovers in financial regulatory experiments', *Journal of Financial Economics* **135**(1), 68–87.

Bongiovanni, A., De Vincentiis, P., Guariglia, A., Isaia, E. & Rossi, M. (2023), 'Do board gender quotas generate horizontal spillovers?', *British Journal of Management* **34**(4), 1851–1868.

Bouwman, C. H. (2011), 'Corporate governance propagation through overlapping directors', *The Review of Financial Studies* **24**(7), 2358–2394.

Cabezon, F. & Hoberg, G. (2022), Leaky director networks and innovation herding. Available at SSRN 4158977.

Cai, J., Nguyen, T. & Walkling, R. (2022), 'Director appointments: It is who you know', *The Review of Financial Studies* **35**(4), 1933–1982.

Cai, Y. & Sevilir, M. (2012), 'Board connections and M&A transactions', *Journal of Financial Economics* **103**(2), 327–349.

Coles, J. L., Daniel, N. D., Durrani, F. & Naveen, L. (2020), Director connections and board advising. Available at SSRN 2002250.

Cziraki, P. & Jenter, D. (2022), The market for CEOs. European Corporate Governance Institute–Finance Working Paper.

Del Prete, S., Papini, G. & Tonello, M. (2024), 'Gender quotas, board diversity and spillover effects. Evidence from Italian banks', *Journal of Economic Behavior Organization* **221**, 148–173.

Eckbo, B. E., Nygaard, K. & Thorburn, K. S. (2022), 'Valuation effects of Norway's board gender-quota law revisited', *Management Science* **68**(6), 4112–4134.

Ewens, M. & Malenko, N. (2024), Board dynamics over the startup life cycle. ECGI Working Paper.

Ferrari, G., Ferraro, V., Profeta, P. & Pronzato, C. (2022), 'Do board gender quotas matter? Selection, performance, and stock market effects', *Management Science* **68**(8), 5618–5643.

Ferreira, D., Ginglinger, E., Laguna, M.-A. & Skalli, Y. (2017), Closing the gap: Board gender quotas and hiring practices. ECGI-Finance Working Paper No 520.

Ferri, F., Ou, Z. & Wang, Y. (2023), Searching for directors. Available at SSRN 4572326.

Field, L., Lowry, M. & Mkrtchyan, A. (2013), 'Are busy boards detrimental?', *Journal of Financial Economics* **109**(1), 63–82.

Geng, H., Hau, H., Michaely, R. & Nguyen, B. (2024), Does board overlap promote coordination between firms? Available at SSRN 3887280.

Gertsberg, M., Mollerstrom, J. & Pagel, M. (2021), Gender quotas and support for women in board elections. National Bureau of Economic Research.

Giannetti, M. & Wang, T. Y. (2023), 'Public attention to gender equality and board gender diversity', *Journal of Financial and Quantitative Analysis* **58**(2), 485–511.

González-Uribe, J. (2020), 'Exchanges of innovation resources inside venture capital portfolios', *Journal of Financial Economics* **135**(1), 144–168.

Gopalan, R., Li, R. & Zaldokas, A. (2022), Do board connections between product market peers impede competition? Available at SSRN 4053853.

Gormley, T. A., Gupta, V. K., Matsa, D. A., Mortal, S. C. & Yang, L. (2023), 'The big three and board gender diversity: The effectiveness of shareholder voice', *Journal of Financial Economics* **149**(2), 323–348.

Hwang, S., Shivdasani, A. & Simintzi, E. (2018), 'Mandating women on boards: Evidence from the United States', *Kenan Institute of Private Enterprise Research Paper* (18-34).

Knyazeva, A., Knyazeva, D. & Masulis, R. W. (2013), 'The supply of corporate directors and board independence', *The Review of Financial Studies* **26**(6), 1561–1605.

Lindsey, L. (2008), 'Blurring firm boundaries: The role of venture capital in strategic alliances', *The Journal of Finance* **63**(3), 1137–1168.

Maida, A. & Weber, A. (2022), 'Female leadership and gender gap within firms: Evidence from an Italian board reform', *IlR Review* **75**(2), 488–515.

Matsa, D. A. & Miller, A. R. (2013), 'A female style in corporate leadership? Evidence from quotas', *American Economic Journal: Applied Economics* **5**(3), 136–169.

Matsa, D. A. & Miller, A. R. (2025), 'Did board gender quotas break the glass ceiling in Europe?', *AEA Papers and Proceedings* **115**, 529–535.

Matveenko, A. & Mikhalishchev, S. (2021), 'Attentional role of quota implementation', *Journal of Economic Theory* **198**, 105356.

Sauvagnat, J. & Schivardi, F. (2024), 'Are executives in short supply? Evidence from death events', *Review of Economic Studies* **91**(1), 519–559.

von Meyerinck, F., Niessen-Ruenzi, A., Schmid, M. & Davidoff Solomon, S. (2021), As California goes, so goes the nation? Board gender quotas and shareholders' distaste of government interventions. European Corporate Governance Institute–Finance Working Paper.

Wiersema, M. & Mors, M. L. (2016), 'What board directors really think of gender quotas', *Harvard Business Review* **14**(November), 2–6.

Yonker, S. E. (2017), 'Geography and the market for CEOs', *Management Science* **63**(3), 609–630.

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.

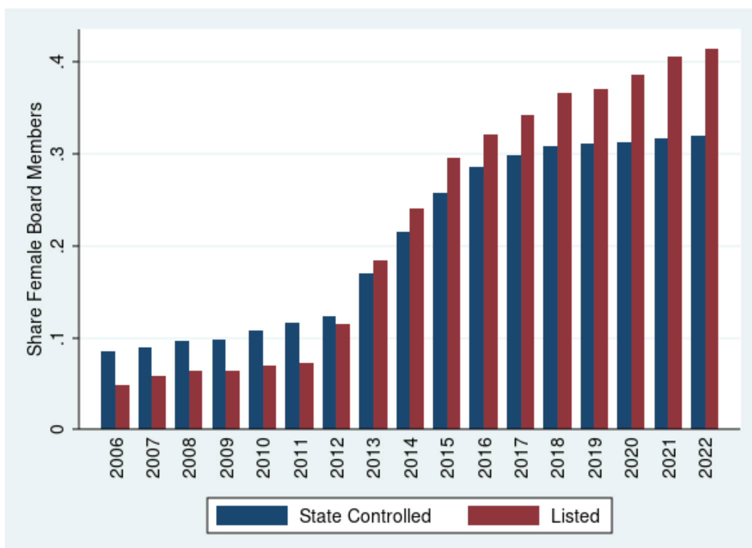
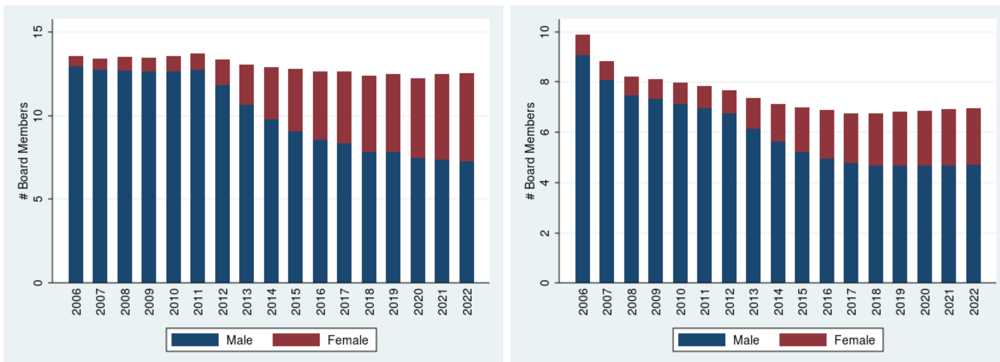


Figure 2: Board Composition in Target Firms

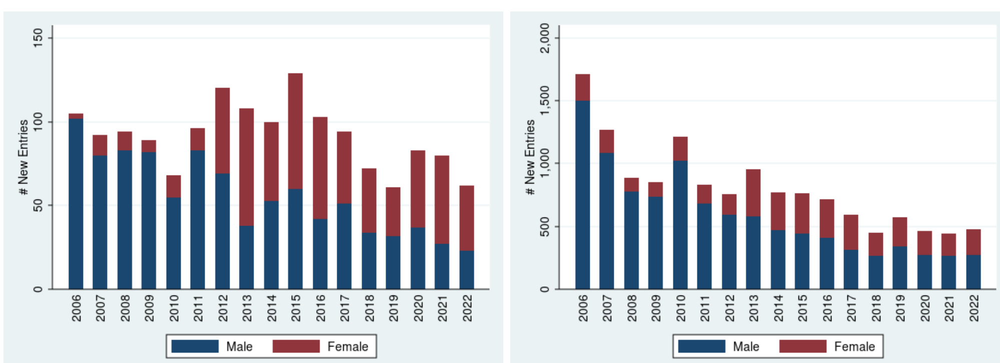
This figure plots the average number of male and female board members over time in listed firms (Panel a) and state controlled Firms (Panel b).



(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 new board 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.

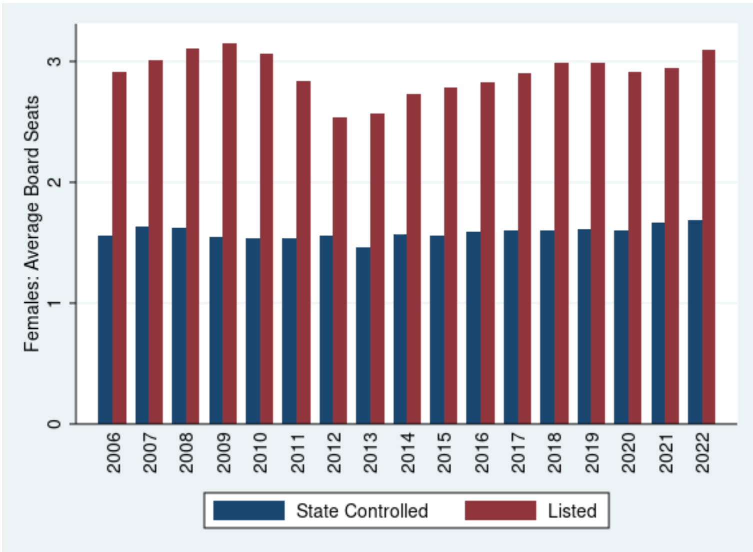
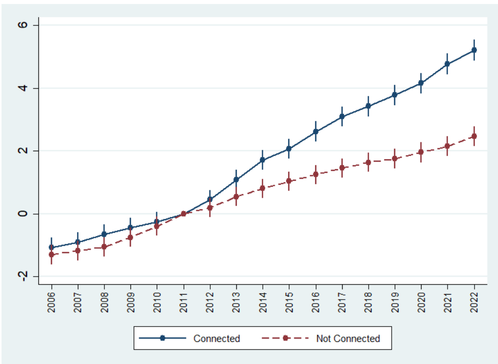
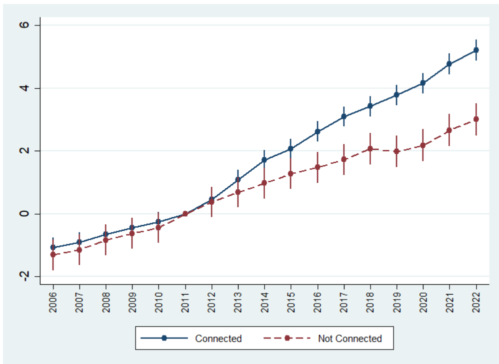


Figure 5: Share of Female Board Members: Connected vs Non-connected Firms

This figure plots the share of female board members over time for firms in the core sample (i.e., non-target, ongoing concern firms) in average percentage points differences with respect to 2011 shares. 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) includes all firms in the core sample. Panel (b) includes all connected firms and matched non-connected firms.



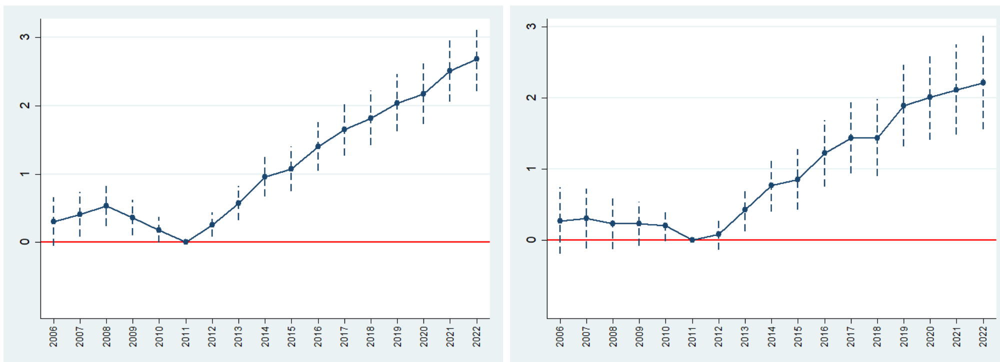
(a) Full Sample



(b) Matched Sample

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: $F\_Share_{i,t} = \sum_{s\neq 2011} \beta_t \mathbb{1}_{\{s=t\}} Connected_i + \alpha_i + \gamma_t + \varepsilon_{i,t}$, where $F\_Share_{i,t}$ is the share of female members in the board of non-target firm i in calendar year $t \in [2006, 2022]$, α_i are firm fixed effects, γ_t represent year dummies and $\mathbb{1}_{\{s=t\}}$ is a dummy equal to one for $s = t$. $F\_Share_{i,t}$ is in percentage points. 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 are clustered at the firm level. The dashed vertical bars represent 95% confidence intervals.



(a) **(b)**

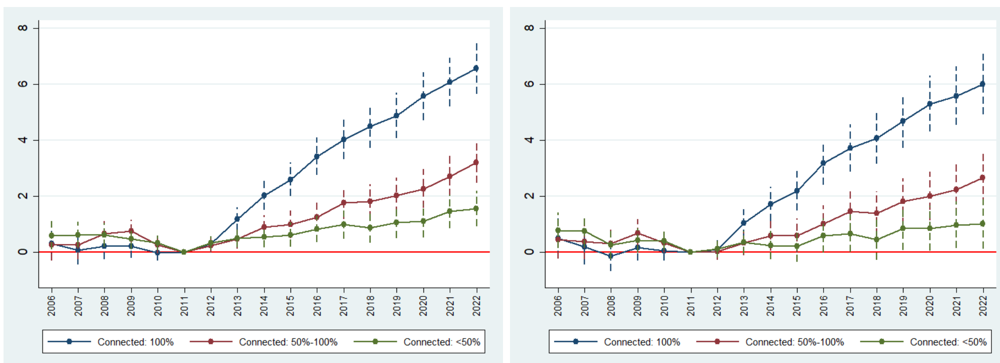
Figure 7: Women on Boards: Second-degree Spillover Effects

This figure shows the estimated coefficients $\hat{\beta}_t$ for the interaction terms in the following specification $F\_Share_{i,t} = \sum_{s\neq 2011} \beta_t \mathbb{1}_{\{s=t\}} Connected2_i + \alpha_i + \gamma_t + \varepsilon_{i,t}$. $F\_Share_{i,t}$ is the share of female members in the board of firm i in calendar year $t \in [2006, 2022]$, α_i represents firm fixed effects, γ_t represent year dummies and $\mathbb{1}_{\{s=t\}}$ 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. $F\_Share_{i,t}$ is in percentage points. 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. Standard errors are clustered at the firm level. The dashed vertical bars represent 95% confidence intervals.

(a) **(b)**

Figure 8: Share of Female Board Members: Persistence of Connections

This figure shows the estimated coefficients $\hat{\beta}_t$ for the interaction terms in the following specification: $F\_Share_{i,t} = \sum_{s \neq 2011} \beta_t \mathbb{1}_{\{s=t\}} Connected_i + \alpha_i + \gamma_t + \varepsilon_{i,t}$, where $F\_Share_{i,t}$ is the share of female members in the board of non-target firm i in calendar year $t \in [2006, 2022]$, α_i are firm fixed effects, γ_t represent year dummies and $\mathbb{1}_{\{s=t\}}$ is a dummy equal to one for $s = t$. $F\_Share_{i,t}$ is in percentage points. We restrict controls to non-connected firms that never initiate a connection in the post-2012 period. Treated firms are connected firms that keep active connections for 100% of the post-reform period (Connected: 100%), between 50% and 100% (Connected: 50%-100%), and less than 50% (Connected: <50%). In Panel (a) all firms satisfying these restrictions in the core sample are included. In Panel (b) all firms satisfying these restrictions in the matched sample are included. Standard errors are clustered at the firm level. The dashed vertical bars represent 95% confidence intervals.



(a) Full Sample **(b)** Matched Sample

Figure 9: 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: $F\_Share_{i,t} = \sum_{s \neq 2011} \beta_t \mathbb{1}_{\{s=t\}} Connected_i + \alpha_i + \gamma_t + \varepsilon_{i,t}$, where $F\_Share_{i,t}$ is the share of female members in the board of non-target firm i in calendar year $t \in [2006, 2022]$, α_i are firm fixed effects, γ_t represent year dummies and $\mathbb{1}_{\{s=t\}}$ 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. $F\_Share_{i,t}$ is in percentage points. Standard errors are clustered at the firm level. The dashed vertical bars represent 95% confidence intervals.

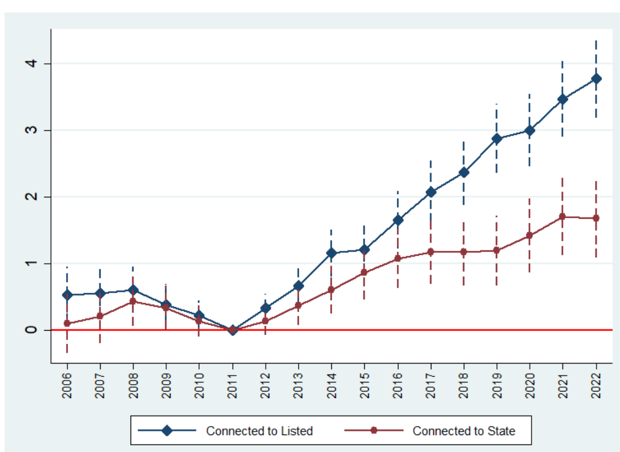


Figure 10: Women on Boards: Heterogeneous Exposure

This figure shows the estimated coefficients $\hat{\beta}_t$ and $\hat{\theta}_t$ for the interaction terms in the following specification: $F\_Share_{i,t} = \sum_{s\neq 2011} \beta_t \mathbb{1}_{\{s=t\}} Exposed_i + \sum_{s\neq 2011} \theta_t \mathbb{1}_{\{s=t\}} NotExposed_i + \alpha_i + \gamma_t + \varepsilon_{i,t}$, where $F\_Share_{i,t}$ is the share of female members in the board of non-target firm i in calendar year $t \in [2006, 2022]$, α_i are firm fixed effects, γ_t represent year dummies and $\mathbb{1}_{\{s=t\}}$ is a dummy equal to one for $s = t$. $Exposed_i$ ($NotExposed_i$) takes value 1 if firm i is connected to a target firm with less (more) than 20% women on board in 2011. $F\_Share_{i,t}$ is in percentage points. Standard errors are clustered at the firm level. The dashed vertical bars represent 95% confidence intervals.

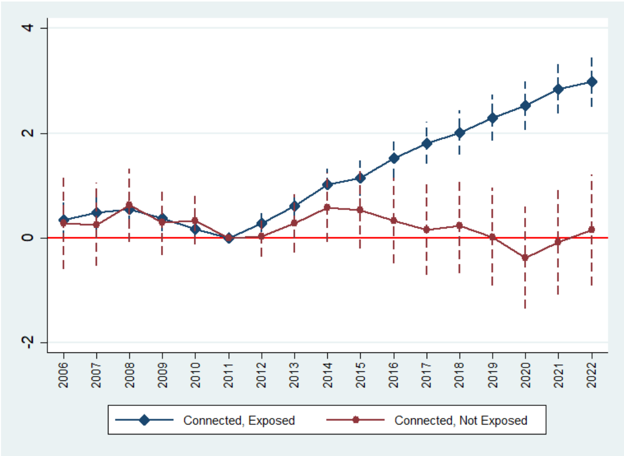


Figure 11: Women on Boards: Matched Samples

This figure shows the estimated coefficients $\hat{\beta}_t$ for the interaction terms in the following specification: $F\_Share_{i,t} = \sum_{s\neq 2011} \beta_t \mathbb{1}_{\{s=t\}} Connected_i + \alpha_i + \gamma_t + \varepsilon_{i,t}$, where $F\_Share_{i,t}$ is the share of female members in the board of non-target firm i in calendar year $t \in [2006, 2022]$, α_i are firm fixed effects, γ_t represent year dummies and $\mathbb{1}_{\{s=t\}}$ 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 pre-reform assets (in logs), as well as indicators for whether the firm is located in the North and operates in the manufacturing industry as additional controls in the logit regression (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. $\text{Share}_{i,t}$ is in percentage points. Standard errors are clustered at the firm level. The dashed vertical bars represent 95% confidence intervals.

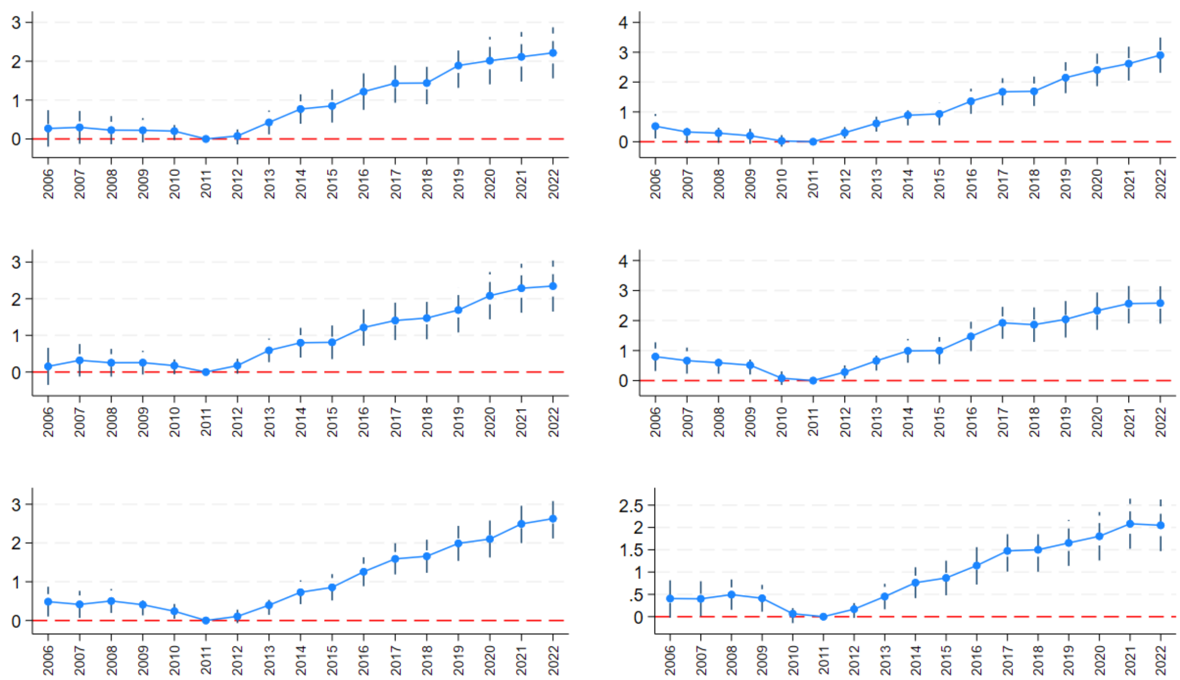


Table 1: Descriptive Statistics of Connected, Non-connected, and Matched Firms

This table displays means and standard deviations for selected variables in the three groups of connected (column 1), non-connected (column 2), and non-connected matched (3) firms. Matched non-connected firms have estimated propensity scores higher than the connected sample median. Column (4) shows standardized 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. Share of Female before 2012 is the share of female board members before the reform.

	(1)		(2)		(3)		4	
	Connected		Non-connected		Non-connected Matched		St. Differences	
	mean	sd	mean	sd	mean	sd	(1)-(2)	(1)-(3)
Board Size	7.26	2.68	5.96	2.24	7.91	2.32	0.52	-0.26
Avg Busyness	3.50	2.27	2.89	2.19	4.15	2.81	0.28	-0.25
lnAssets	10.00	1.55	9.40	1.41	9.98	1.29	0.41	0.02
North	0.75	0.43	0.68	0.47	0.79	0.40	0.01	0.17
Manuf	0.58	0.49	0.57	0.49	0.49	0.50	0.15	-0.10
Any Listed Owner	0.07	0.26	0.01	0.11	0.01	0.11	0.31	0.30
Any State Owner	0.03	0.16	0.00	0.07	0.01	0.09	0.18	0.15
Ownership in Listed	0.01	0.08	0.00	0.05	0.00	0.05	0.07	0.07
Ownership in State	0.00	0.02	0.00	0.01	0.00	0.00	0.02	0.04
Share Female 2006-2011	14.10	16.82	16.29	18.36	14.97	14.57	-0.12	-0.05
Observations	156,141	.	282,679	.	50,884	.	.	.

Table 2: Female Representation in Boards of Connected Firms

This table shows coefficient estimates for the following specification $Y_{i,t} = \beta Post_t \times Connected_i + \alpha_i + \gamma_t + \varepsilon_{i,t}$ 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. α_i and γ_t represent firm and year fixed effects. In column (1) the outcome variable is $Y_{i,t} = \Delta Share_{i,t} \equiv (F\_Share_{i,t} - F\_Share_{i,t-1})$, i.e., the year on year variation in the share of female board members in firm i. In column (2) the outcome variable is $Y_{i,t} = F\_Share_{i,t}$. In column (3) the outcome variable is $Y_{i,t} = 1$ if $F\_Share_{i,t} > 0$ and zero otherwise. 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 are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

(a) Full Sample

	(1) Δ Share F.	(2) Share F. BM	(3) Any F. BM	(4) Δ M.BM	(5) Δ F.BM
Post X Connected	0.3455***	1.1659***	0.0190***	-0.0937***	0.0212***
	(0.0461)	(0.1555)	(0.0048)	(0.0064)	(0.0028)
Firm and Year FE	yes	yes	yes	yes	yes
Observations	428049	438773	438820	428140	428140
Firms	38419	39600	39600	38424	38424
R-Squared	0.0002	0.0205	0.0227	0.0028	0.0002
Mean Dep. Var.	0.27	16.76	0.62	0.02	0.03

(b) Matched Sample

	(1) Δ Share F.	(2) Share F. BM	(3) Any F. BM	(4) Δ M.BM	(5) Δ F.BM
Post X Connected	0.2711***	1.0248***	0.0514***	0.0273***	0.0411***
	(0.0611)	(0.2104)	(0.0071)	(0.0098)	(0.0047)
Firm and Year FE	yes	yes	yes	yes	yes
Observations	204849	206944	206950	204862	204862
Firms	15266	15338	15338	15266	15266
R-Squared	0.0005	0.0400	0.0229	0.0062	0.0006
Mean Dep. Var.	0.33	15.29	0.63	-0.03	0.03

Table 3: Timing and Intensity of Firms Connections

Columns (1) to (3) show coefficient estimates for the following specification $Share_{i,t+s} = \beta Post_t \times Current\_C_i + \alpha_i + \gamma_t + \varepsilon_{i,t}$ 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. α_i and γ_t represent firm and year fixed effects. Columns (4) and (5) show coefficient estimates for the following specification $\Delta Share_{i,t} = \beta Post_t \times C_i + \alpha_i + \gamma_t + \varepsilon_{i,t}$ where $Post_t = 1$ if $t \geq 2012$ and zero otherwise, C_i is a measure of 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. $Mean\ Connection_i = \sum_t \left(\sum_b c_{b,i,t}\right)/3$, 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) and (5). Standard errors are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	(1) Share F. (t+1)	(2) Share F. (t+2)	(3) Share F. (t+3)	(4) Δ Share F.	(5) Δ Share F.
Post X Current_C	1.6929***	1.6230***	1.5353***		
	(0.1397)	(0.1402)	(0.1419)		
Current_C	-1.3057***	-1.2281***	-1.1413***		
	(0.1251)	(0.1228)	(0.1216)		
Post X Mean Connection				0.0665***	
				(0.0114)	
Post X Mean Tot. Connection					0.0389***
					(0.0058)
Other Controls	yes	yes	yes	yes	yes
Firm and Year FE	yes	yes	yes	yes	yes
Observations	374666	339130	305744	154451	154451
Firms	37379	35340	32460	11849	11849
R-Squared	0.0214	0.0211	0.0201	0.0008	0.0008
Mean Dep. Var.	17.07	17.29	17.52	0.36	0.36

Table 4: Female Representation in Boards of Connected Firms: Robustness

This table shows coefficient estimates for the following specification $\Delta Share_{i,t} = \beta Post_t \times Connected_i + \alpha_i + \gamma_t + \varepsilon_{i,t}$ where $Post_t = 1$ if $t \geq 2012$ and zero otherwise and $Connected_i = 1$ if firm i shared at least one board member with a target firm in any of the three years before the reform. α_i and γ_t represent firm and year fixed effects. In column 1 we reproduce 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 are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

(a) Full Sample

	(1)	(2) Never Listed	(3) Geography	(4) Sector
Post X Connected	0.3455***	0.3396***	0.3064***	0.3615***
	(0.0461)	(0.0461)	(0.0762)	(0.0477)
Post X Connected X Prox1			1.1676	
			(1.7509)	
Post X Connected X Prox2				-1.6872
				(2.1727)
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	0.0002	0.0002	0.0002	0.0002
Mean Dep. Var.	0.27	0.27	0.27	0.27

(b) Matched Sample

	(1)	(2) Never Listed	(3) Geography	(4) Sector
Post X Connected	0.2711***	0.2678***	0.2605***	0.3040***
	(0.0611)	(0.0612)	(0.0982)	(0.0636)
Post X Connected X Prox1			0.4585	
			(2.3845)	
Post X Connected X Prox2				-6.5006*
				(3.7063)
Other Controls	yes	yes	yes	yes
Firm and Year FE	yes	yes	yes	yes
Observations	204849	203760	204849	204849
Firms	15266	15116	15266	15266
R-Squared	0.0005	0.0005	0.0006	0.0006
Mean Dep. Var.	0.33	0.33	0.33	0.33

Table 5: Female Representation and Ownership Links

This table shows coefficient estimates for the following specification $\Delta Share_{i,t} = \beta Post_t \times Connected_i + \alpha_i + \gamma_t + \varepsilon_{i,t}$ 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 $Ownership_i$ takes value 1 if firm i had ownership links with a target firm in the three years before the reform. 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 are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	(1) No Ownership Links	(2) Full Sample	(3) Matched Sample	(4) Connected Firms
Post X Connected	0.2885***	0.2874***	0.2237***	
	(0.0478)	(0.0476)	(0.0626)	
Post X Connected X Ownership		0.0976	-0.1884	
		(0.2776)	(0.3688)	
Post X Ownership		0.5123**	0.7970**	0.6090***
		(0.2564)	(0.3531)	(0.1063)
Other Controls	yes	yes	yes	yes
Firm and Year FE	yes	yes	yes	yes
Observations	408100	400943	204849	154451
Firms	37129	30150	15266	11849
R-Squared	0.0002	0.0003	0.0007	0.0008
Mean Dep. Var.	0.25	0.28	0.33	0.36

Table 6: Female Representation in Boards of Connected Firms: Alternative Matching

This table shows coefficient estimates for the following specification $\Delta Share_{i,t} = \beta Post_t \times Connected_i + \alpha_i + \gamma_t + \varepsilon_{i,t}$ where $Post_t = 1$ if $t \geq 2012$ and zero otherwise, and $Connected_i = 1$ if firm i shared at least one board member with a target firm in any of the three years before the reform. 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 pre-reform average assets (in logs), 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 are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	(1)	(2)	(3)	(4)	(5)	(6)
Post X Connected	0.2711***	0.4086***	0.2607***	0.4062***	0.3758***	0.3139***
	(0.0611)	(0.0539)	(0.0639)	(0.0621)	(0.0493)	(0.0548)
Firm and Year FE	yes	yes	yes	yes	yes	yes
Observations	204849	191081	196629	140793	287468	220600
Firms	15266	13242	14631	9470	19920	14903
R-Squared	0.0005	0.0006	0.0006	0.0007	0.0004	0.0004
Mean Dep. Var.	0.33	0.31	0.34	0.28	0.31	0.27

Table 7: Female Representation: Newly Hired Board Members

This table shows coefficient estimates for linear probability models with individual outcomes $Y_{j,i,t}$, 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) $Y_{j,i,t} = 1$ if individual j seats in the board of a target company and zero otherwise. In column (4) 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. Firm-level clustered errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	(1) Female=1	(2) Female=1 + in Target=1	(3) in Target=1	(4) Female=1 (in Target)
Post X Connected	0.0377***	0.0081***	-0.0868***	0.0199
	(0.0035)	(0.0010)	(0.0025)	(0.0314)
Post				0.1345***
				(0.0308)
Connected			0.1511***	
			(0.0020)	
Firm FE	yes	yes	no	yes
Year FE	yes	yes	yes	no
Observations	734271	734271	734271	60785
R-Squared	0.004	0.003	0.038	0.036
Mean Dep. Var.	0.17	0.01	0.08	0.14

Table 8: Out-Target New Hires in Connected Firms

This table shows coefficient estimates for linear regressions of individual characteristics $Y_{j,i,t}$, where individual j is a board member in firm i hired between year $t-2$ and year t. $Post_t = 1$ 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, $F_j = 1$ if individual j is female. *Other Controls* include the interaction term $Post_t \times F_j$ and the dummy F_j. 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 5 we further restrict the sample to outsiders. *Foreign* is a dummy variable that takes value 1 if individual j is foreign born. *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 6 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. *Prob>F* indicates the significance level of the test that the sum of the coefficients for $Post_t \times Connected_i$ and $Post_t \times Connected_i \times F_j$ is equal to zero. Clustered errors at firm level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	(1) Outsider	(2) Foreign	(3) Age	(4) Current App.	(5) First App.	(6) Contacts
Post X Connected	-0.0268***	0.0047	-0.4091***	-0.1216**	0.0019	-0.3116***
	(0.0045)	(0.0032)	(0.1225)	(0.0527)	(0.0027)	(0.0419)
Post X Connected X F	0.0179***	0.0004	-0.1206	0.1817***	0.0102***	-0.0918***
	(0.0063)	(0.0040)	(0.1600)	(0.0584)	(0.0035)	(0.0320)
Other Controls	yes	yes	yes	yes	yes	yes
Firm and Year FE	yes	yes	yes	yes	yes	yes
Observations	673486	485377	485375	485377	485377	427847
Prob>F	0.180	0.229	0.002	0.352	0.003	0.000
R-Squared	0.003	0.001	0.039	0.012	0.004	0.004
Mean Dep. Var.	0.72	0.08	49.56	4.06	0.12	4.69

Table 9: 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 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 are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	(1)	(2)	(3)	(4)	(5)
	Busyness	Distance	Tenure	Any F exec	F CEO
Post X Connected	-0.2891***	3.1528***	-0.5396***	0.0078**	0.0013
	(0.0191)	(1.1435)	(0.0924)	(0.0034)	(0.0031)
logAssets	0.0608***	0.8880***	-0.0323	-0.0009	0.0007
	(0.0056)	(0.3176)	(0.0219)	(0.0009)	(0.0006)
logSales	-0.0123***	-0.4370**	0.0120	0.0008*	0.0008***
	(0.0038)	(0.2139)	(0.0163)	(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	0.0245	0.0020	0.4166	0.0006	0.0031
Mean Dep. Var	2.76	119.92	9.77	0.14	0.07

Changing the Board Game: Horizontal Spillovers of Gender Quotas

Online Appendix

A SUTVA Robustness Tests

Following Boehmer et al. (2020), we estimate the direct and indirect effect coefficients through the standard difference-in-differences specification

$$\Delta Share_{i,t} = \beta_1 Post_t \times Connected_i + \beta_2 Post_t + \beta_3 Connected_i + \theta X_{i,t} + \epsilon_{i,t} \tag{6}$$

where $\Delta Share_{i,t}$ is the change of female share on the board of firm i between $t-1$ and t. As in Boehmer et al. (2020) we include time-varying firm controls and year effects. Specifically, $X_{i,t}$ includes firm size (measured as log of assets) and the average share of female directors at time t in target firms. The coefficient estimates for β_1 and β_2 measure the direct and indirect effects of the reform, respectively. In particular, β_2 captures any trend common to all non-target firms, including potential second-order spillover effects of the reform, while β_1 captures any additional effect on the treated (connected) group. Table A1 shows the results of this estimation using the specification in equation (6) (column 1) and an alternative one with firm fixed effects (column 2). The coefficient estimates for β_2 are positive but not statistically distinguishable from zero in both specifications, suggesting that (most) control firms, i.e., non-connected firms, were not significantly affected by the reform. We repeat the same exercise in columns (3) and (4) dropping "indirectly-connected" firms from the control sample. If the SUTVA assumption is violated due to second-order spillovers we should observe a significant increase in $\hat{\beta}_1$ and a decrease of $\hat{\beta}_2$. Instead, the results are mixed: $\hat{\beta}_1$ increases by 0.03 in column (3) but decreases by 0.02 in column (4), while $\hat{\beta}_2$ decreases by 0.03 in column (3) and remains roughly unchanged in column (4). Finally, in column (5) we replicate our main analysis from equation (3) but we drop "indirectly-connected" firms from the control sample. We obtain a coefficient estimate for $\hat{\beta}_1$equal to 0.37, which is less than 0.03 larger than our main result. Thus, the latter may underestimate the effect of the reform on connected firms by at most 0.3 percentage points over ten years. The small impact of this adjustment is not surprising, since indirectly-connected firms represent only a small fraction of the controls and the size of second-order spillovers is limited (as per Figures 7a and 7b).

B Probability Decomposition

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

$$\Delta_{c,p}\pi\left(F_j = 1\right) \equiv \left[\pi_{1,1}\left(F_j = 1\right) - \pi_{1,0}\left(F_j = 1\right)\right] - \left[\pi_{0,1}\left(F_j = 1\right) - \pi_{0,0}\left(F_j = 1\right)\right] > 0 \quad (7)$$

where $\pi_{c,p}\left(x\right)$ 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 7 as $\Delta_{c,p}\pi\left(F_j = 1\right)$, where $\Delta_{c,p}$ indicates the second order difference with respect to type $c \in \{0, 1\}$ and period $p \in \{0, 1\}$. Thus, $\Delta_{c,p}\pi\left(F_j = 1\right)$ measures the spillover effect, that is, the incremental effect of the reform on the probability of hiring a woman in connected firms relative 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 (7) should be due to hiring women from the in-target pool. Since $\Delta_{c,p}\pi\left(F_j = 1\right) = \Delta_{c,p}\pi\left(F_j = 1, InTarget_j = 1\right) + \Delta_{c,p}\pi\left(F_j = 1, InTarget_j = 0\right)$, in the presence of direct supply-effects we should observe

$$\Delta_{c,p}\pi\left(F_j = 1, InTarget_j = 1\right) > 0. \quad (8)$$

Crucially, since we observe the entire pool of in-target candidates, we can also make testable predictions regarding the mechanisms at play. To do so, first notice that, for each type c and in each period p, the probability of hiring a female candidate who currently seats in the board of a target company is

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

We can model the second component in the LHS, i.e., the probability of hiring a female board member *conditional* on drawing from the in-target pool, as

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

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

$$\begin{aligned}
\Delta_{c,p}\pi\left(F_j = 1|InTarget_j = 1\right) =& \Delta_{p|c=1}\pi\left(F_j = 1|InTarget_j = 1\right) - \Delta_{p|c=0}\pi\left(F_j = 1|InTarget_j = 1\right) \\
=& \left[(S_1 - S_0) + (b_{1,1} - b_{1,0})\right] - \left[(S_1 - S_0) + (b_{0,1} - b_{0,0})\right] \\
=& \Delta_p S - \Delta_p S + \Delta_p b_1 - \Delta_p b_0 \\
=& \Delta_p b_1 - \Delta_p b_0
\end{aligned}$$

where Δ_p represents first difference with respect to period p. In other words, conditional on picking an in-target candidate, any incremental effect of the reform on connected firms is due to *relative* changes in preferences.

Finally, we can rewrite equation (8) as follows

$$\begin{aligned}
\Delta_{c,p}\pi\left(F_j = 1, InTarget_j = 1\right) =& \left(\Delta_p S + \Delta_p b_1\right)\left[\pi_{1,1}\left(InTarget_j = 1\right)\right] - \left(\Delta_p S + \Delta_p b_0\right)\left[\pi_{0,1}\left(InTarget_j = 1\right)\right] \\
&+ \left(S_0 + b_{1,0}\right)\left[\Delta_p \pi_1\left(InTarget_j = 1\right)\right] - \left(S_0 + b_{0,0}\right)\left[\Delta_p \pi_0\left(InTarget_j = 1\right)\right] \\
&> 0
\end{aligned} \tag{9}$$

where $\Delta_p S > 0$ represents the change in female share in target boards.

In Table 7, columns (1) and (2) we show that equations (7) and (8) hold in the data, i.e., part of our main findings can be attributable to direct-supply effects. In column (3) we show that

$$\pi_{1,1}\left(InTarget_j = 1\right) > \pi_{0,1}\left(InTarget_j = 1\right) \tag{10}$$

and

$$\Delta_p \pi_1 \left(InTarget_j = 1 \right) < \Delta_p \pi_0 \left(InTarget_j = 1 \right) \tag{11}$$

while in column (4) we show that

$$\Delta_p b_1 = \Delta_p b_0 \tag{12}$$

The results above, suggest that equation (8) holds in the data because connected firms hire more in-target directors in the post-reform period (see equation 10), triggering the "composition" effect (as $\Delta_p S > 0$) and, potentially, a common shift in preferences (see equation 12). These forces more than compensate the drop in persistence (see equation 11), which contributes negatively to the expression in (9) under the assumption that connected firms did not have a larger preference for women with respect to non-connected firms prior to the reform. Indeed, our estimates show that

$$\Delta_{c,p} \pi \left(F_j = 1, InTarget_j = 1 \right) \simeq \left(\Delta_p S + \Delta_p b \right) \left[\pi_{1,1} \left(InTarget_j = 1 \right) - \pi_{0,1} \left(InTarget_j = 1 \right) \right]$$

where the first term on the LHS is the coefficient estimate for *Post* in column (4) and the second component is the sum of the coefficient estimates for *Connected* and *Post*×*Connected* in column (3).

C Firm Performance

In this section, we explore the effects of the spillovers on firms' performance by means of a TWFE difference-in-differences estimation where outcomes are measured in terms of sales growth, profitability, and investment rate.[20] We begin 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 magnitude and are spread over time, as one might expect from voluntary rather than mandatory adjustments. This suggests a modest, if even discernible, 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

[20]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 $(PPE_t - PPE_{t-1} + Depreciation_t)/PPE_{t-1}$, where PPE is the value of tangible capital in property, plants and equipment. Final values are 2% winsorized.

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 recovery path in the following years. Since this may significantly affect the interpretation of the results in a difference-in-differences analysis of firm performance, extra care must be taken when selecting the control sample to minimize the impact of confounding factors. To this end, 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 great recession.

The results are presented in Table A2 and suggest a negative effect on sales growth (statistically significant only when we use the matched samples in panel b and c) but no material impact on firm profitability and investment rate.

Table A1: SUTVA Robustness Tests

This table shows coefficient estimates for a regression with outcome $\Delta Share_{i,t} \equiv (F\_Share_{i,t} - F\_Share_{i,t-1})$, i.e., the year on year variation in the share of female board members in firm i. $Post_t = 1$ 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. *Other Controls* include firm size (measured as log of assets) and the average share of female directors at time t in target firms. In columns (3), (4) and (5) we exclude indirectly-connected firms from the control sample. Standard errors are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	(1)	(2)	(3)	(4)	(5)
Post X Connected	0.2816***	0.3167***	0.3104***	0.2980***	0.3747***
	(0.0501)	(0.0543)	(0.0937)	(0.0998)	(0.0888)
Post	0.0745	0.0518	0.0489	0.0550	
	(0.0553)	(0.0563)	(0.1112)	(0.1149)	
Connected	-0.0721*		-0.0497		
	(0.0382)		(0.0715)		
Other Controls	yes	yes	yes	yes	no
Firm FE	no	yes	no	yes	yes
Year FE	no	no	no	no	yes
Observations	290506	290506	145476	145476	215955
R-Squared	0.0002	0.0002	0.0005	0.0003	0.0004
Mean Dep. Var.	0.26	0.26	0.27	0.27	0.29

Table A2: Outcomes: Firm Performance

This table shows coefficient estimates for a linear model of sales growth , profitability, and investment rates. The model specification is: $Y_{i,t} = \beta Post_t \times Connected_i + \sigma logAssets + \delta BoardSize + \alpha_i + \gamma_t + \varepsilon_{i,t}$ 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. α_i and γ_t represent firm and year fixed effects. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

(a) Full Sample

	(1) Sales Growth	(2) ROA	(3) Investment Rate
Post X Connected	-0.0051	-0.0014*	0.0126
	(0.0036)	(0.0009)	(0.0086)
Other Controls	yes	yes	yes
Firm and Year FE	yes	yes	yes
Observations	280142	290432	273136
Firms	26615	27211	25841
R-Squared	0.081	0.038	0.029
Mean Dep. Var.	0.04	0.02	0.37

(b) Matched Sample (Probability Score)

	(1) Sales Growth	(2) ROA	(3) Investment Rate
Post X Connected	-0.0137**	-0.0002	-0.0049
	(0.0054)	(0.0013)	(0.0129)
Other Controls	yes	yes	yes
Firm and Year FE	yes	yes	yes
Observations	130820	134717	127511
Firms	11048	11198	10783
R-Squared	0.089	0.037	0.032
Mean Dep. Var.	0.03	0.02	0.34

(c) Matched Sample (Sales Growth 2008-2011)

	(1) Sales Growth	(2) ROA	(3) Investment Rate
Post X Connected	-0.0103***	-0.0011	0.0099
	(0.0038)	(0.0009)	(0.0091)
Other Controls	yes	yes	yes
Firm and Year FE	yes	yes	yes
Observations	212555	217399	208584
Firms	16770	16899	16521
R-Squared	0.093	0.035	0.032
Mean Dep. Var.	0.04	0.02	0.35