Anticipation Effects of a Boardroom Gender Quota Law: Evidence from a Credible Threat in Sweden

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Implementation of boardroom quota laws has been evaluated previously. However, firms anticipate laws. We provide novel results on female board participation and board recruitment in Sweden due to a credible threat of a quota law. The threat caused a rapid increase in the share of female board members, an increased board diversity, a lower turnover rate for directors and a lower turnover for male CEOs in profitable firms. Interestingly, firm performance improved. Thus, we show it is possible to increase the share of women on boards without resorting to quotas and that anticipatory effects could be essential to the analysis.

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I. Introduction

Over the last decade, policymakers in Europe have focused on the relative underrepresentation of women on corporate boards, and numerous countries have considered or implemented gender boardroom quotas. The first quota law, adopted in Norway in December 2005, required public limited liability companies (ASAs) to increase female representation on their boards of directors to 40 percent within two years. The law increased female representation by approximately 20 percentage points for the typical firm (Matsa and Miller 2013). Other countries, including Spain, Belgium, France, Germany, Iceland, Italy, and the Netherlands, have subsequently implemented boardroom quotas (Eckbo, Nygaard and Thorburn 2021). Lately, EU-wide quotas have also been proposed. In Sweden, the policy debate has been intense as well. In 2002, Swedish Deputy Prime Minister Margareta Winberg threatened to impose a mandatory law if considerable improvements in boardroom representation were not achieved by listed companies within two years. Specifically, listed companies were asked to raise their share of female directors to at least 25%, an increase of about 20 percentage points.

Leveraging the Swedish experience, this paper provides the first rigorous estimates of a pure *anticipation effect* of a gender quota law and show that the effects are large in magnitude. Our main analysis uses a difference-in-differences design in which listed companies—the treatment group—were threatened by a quota law, whereas comparable non-listed firms—the control group—were not.

Our key results show that the threat caused listed firms to increase their share of female directors by 5-10 percentage points or an approximate 100-200 percent increase.[1] This new higher level of female board participation also persisted. Interestingly, the increase was also accompanied by a higher share of board members born outside of Stockholm and immigrants from non-EU countries. Furthermore, female director turnover decreased, which is in line with the evidence in Ferreira et al. (2017), but also male director turnover. These findings, along with anecdotal evidence, suggest that treated firms began to use new recruitment firms and practices to appoint board positions. This

[1] These results contrast findings from Spain. However, the Spanish reform differs since the government did not fully commit to the law and in practice made it voluntary to comply (de Cabo et al., 2019; Conde-Ruiz et al., 2019). Fedorets et al. (2019) also demonstrate that the share of women only increased on the part of boards in Germany where the quota was mandated, further suggesting that the threat of a quota need to be credible to be effective.

broadened board representation then directly affected firm management through a lower turnover rate for CEOs among the listed firms in the post period relative to the non-listed firms. Interestingly, these changes in CEO turnover rates only appear in firms with a pre-threat positive ROA, and among male CEOs.

Alongside with the dramatic changes of board composition, we also observe improved firm performance in terms of return on assets (ROA) in the exact same years. On average, ROA increased by approximately 3-5 percentage points among listed firms after the threat relative to the change in ROA among unlisted firms in the same period.[2] This result differs from other quasi-experimental studies evaluating gender quota law *introduction*.[3]

Finally, we offer suggestive evidence indicating that the increase in the board share is driven by increased competition for the most competent women.

Our results highlight the importance of the anticipatory effects of a law. Furthermore, the policy discussions on increasing female board participation have to date focused on quotas. However, we show that increasing the share of women on corporate boards without resorting to quotas is possible and that doing so is unlikely to negatively affect firm performance.

The remainder of the paper is outlined as follows. In section II, we discuss the related literature and document the background of the threat. In section III, we describe the methodology, data, and sampling. In section IV, we provide the results for board compositional effects and firm performance measures. In section V, we present additional robustness checks. In section VI, we conclude.

[2] For stock prices, we see an insignificant but positive coefficient around 7-8 percent around the time when the quota law plans were stated more concretely on the 22nd of October 2002, see Table A8, Panel B.

[3] See, for instance, Matsa and Miller (2013), Ahern and Dittmar (2012). However, more recent developing literature studying quota laws in other countries have sometimes found either zero or positive effects; see, for instance, Ferrari et al. (2021), Fedorets et al. (2019), or Eckbo et al. (2021).

II. Related Literature and Background

A. Related Literature

Several papers over the last decade have evaluated board quotas in different countries and contexts. However, no previous paper has studied the threat of the implementation of a quota or discussed anticipatory effects to any great degree. As Figure 1 in Bertrand et al. (2019) indicates, the increase in the share of females on boards began back in 2001 in Norway and continued until 2008. This is a sign of anticipatory effects, as the law was passed in late 2005 at first, with compliance required at the start of 2008. Anticipatory effects are well known as a direct threat to validity in a difference-in-differences setting if they are not properly accounted for.[4] For example, if a law was anticipated but not acknowledged by the econometrician, the estimated effect may even have the wrong sign. The importance of anticipatory effects might thus be crucial when evaluating board quotas.

Broadly speaking, the literature on board quotas has focused on outcomes related to organizational characteristics, such as board competence, and firm performance and market value. A recent study by Besley et al. (2017) studies quotas in party politics and provide a theoretical perspective. In a setting in which the competence of new candidates on a party ballot is positively related to the party's success but simultaneously threatens the power of the incumbent, the authors theoretically show that the incumbent trades off party success for the survival of power. Thus, a gender quota could lead to better candidates, as mediocre male candidates are replaced by better male and female candidates. The authors also find strong empirical support for the model and note explicitly that the model "could be applied, for example, to private organizations such as corporate boards" (pp. 2206).

The most notable paper related to board competence is Bertrand et al. (2019), which demonstrate that the women appointed to corporate boards after the Norwegian quota law were more competent, and that the earnings gap within boards decreased. Correspondingly, Ferreira et al. (2017) find greater stability of post-quota than pre-quota female appointments in France, indicating a more efficient market. Ferrari et al (2021) also find that a quota in Italy led to overall higher levels of

[4] See, for instance, Ashenfelter (1978), Angrist and Pischke (2009), or Malani and Reif (2015).

education of board members, while Baltrunaite et al. (2021) provide evidence for higher quality board members due to quotas in state-owned enterprises in Italy.[5]

In terms of firm performance, the ex-ante expected effect of a board quota is indeterminable. From a classic economics perspective, where agents are profit maximizing and have perfect information, it would be reasonable to conjecture that a quota law or a credible threat of a law should reduce profits, in particularly in the corporate sector where the competition pressure is high, which should limit suboptimal board composition. However, as pointed out in Besley et al. (2017), and as seen in the literature on board composition, a board quota may improve board competence. Moreover, as has been noted frequently in the literature, if male directors have a distaste for women board members and/or a preference for homogeneity, then diversity and independence could increase firm performance (Adams and Ferreira, 2009; Smith, 2014; Ferreira, 2015).[6] This diversity could be manifested in less permanent characteristics, such as levels of formal training and experience.[7] However, gender differences could also be more stable than these characteristics. Related to decision making are differences in preferences and attitudes such as differences in risk attitudes,[8] attitudes towards competition and negations.[9] Kim and Starks (2016) specifically demonstrate that women on corporate boards posses' expertise in other areas than their male counterparts, suggesting that a higher share of women on the board would increase expertise heterogeneity.

A credible threat could push the board to be more gender-neutral and generally diverse, thus causing firms to perform better. However, in any of these models proposing potentially positive effects of a quota, there must be some supply of competent women or of female candidates to

[5] Maida and Weber (2019) also demonstrate that the Italian reform had little spillover effects on women further down in the organization. However, Dalvit et al. (2021) use the French board quota and find decreased wage and employment gaps among senior executives and middle management, but not among workers further down in the organization.

[6] A related approach would be to assume that shareholders or directors have a bias when evaluating female competence. A quota may then ex ante reduce this bias, analogous to the findings in Beaman (2009).

[7] As discussed in Adams (2016), diversity could be either temporary or more of a permanent type. Differences such as female directors being likely to be younger than male directors (see, e.g., Adams & Ferreira, 2009 and Adams & Funk, 2012) or being outsiders of the "old boys club" could change over time.

[8] For example, it has been suggested that the Lehman Brothers crisis never would have occurred if the company were Lehman Sisters (Adams and Ragunathan, 2014). However, this argument ignores the selection into boards as pointed out and documented in Adams and Funk (2012), where they find that the selected female directors were less risk averse, invalidating the Lehman Sisters "hypothesis" with respect to risk aversion differences.

[9] See, e.g., the survey of the literature and empirical evidence in Bertrand (2011).

recruit who have different characteristics than male candidates.[10] Recent evidence in von Essen and Smith (2022) suggest that female candidates in particular often lack the right network to make it onto company boards.

One important difference from studying actual law introduction is that a threat need not be complied with. This point leads to a free-riding motive of allowing other listed firms to increase their share of female directors and still avoid a binding law. Although this force may be in play, other counteracting effects exist. First, the threat aimed at the *individual* board share and not only the listed firm's average board share. Indeed, in a subsequent law proposal in 2006, the minimum number of female directors was specified for a given board size. Second, a penalty is incurred for acting late because the best directors are more likely to be already occupied.

Thus, theoretically, we cannot determine which of the effects will prevail, and we would ideally like to randomize gender composition of corporate boards. The Norwegian law of quotas in 2005 has been used as an exogenous shock (Ahern and Dittmar, 2012 and Matsa and Miller, 2013).[11] Ahern and Dittmar (2012) use the pre-reform share of women on the boards of listed firms as the treatment assigning variable, as firms with a higher share of women prior to the reform had fewer seats to fill with women. Using this strategy, they find a large negative effect on firms' Tobin's Q ratio.[12] Matsa and Miller (2013) use a difference-in-differences design, in which a sample of non-listed limited liability firms act as the control group while listed firms act as the treated group, similar to our design. The authors find a negative effect on firm performance.[13] Conversely, Nygaard (2011) finds a positive effect of quotas on firm performance when evaluating reforms in Norway. However, the robustness of the results from these papers has been questioned (Ferreira,

[10] In most OECD countries, women have been more highly educated than men for many years. Related to the supply argument is the literature on compensation, in particular at the top level of organizations. See, for example, Bertrand and Hallock (2001) or Keloharju et al. (2022) for evidence using Swedish data.

[11] The Norwegian reform was implemented sequentially in practice. The first discussions began in 1999, and the first proposal was released in 2001 by the then center-left government. In 2002, the newly elected center-right government made statements both in support of and in defiance against a quota law, which ultimately resulted in a law being passed in late 2005. The law, in turn, gave the affected companies two years to comply.

[12] However, as discussed by Ferreira (2015), early adopters are unlikely to be similar in trends to their counterparts. When we replicate their first stage in our setting, that is, the pre-reform share of women in 2002 regressed on the share of women on the board in subsequent and earlier years, the parallel trend assumption is violated due to mean reversion. This finding is illustrated in Figure A1 in the Appendix. For the sake of completion, Figure A2 also shows similar estimates to their reduced form. If we replicate their reduced form but use ROA instead of Tobin's q as the outcome, we obtain estimates that are essentially zero.

[13] The authors choose post-2006 as the treatment period.

2015; Eckbo, Nygaard and Thorburn, 2021). When critically assessing the empirical design used in previous papers, Eckbo, Nygaard and Thorburn (2021) find a zero effect of the quota law on firm performance measures. One point made by Eckbo, Nygaard and Thorburn (2021) is that firms could anticipate the law after the political debate changed in February 2002. As Figure 1 in Bertrand et al. (2019) indicates, the increase in the share of females on boards began back in 2001 in Norway and continued until 2008, a sign of anticipatory effects. Papers evaluating reforms in Italy have tended to find zero or positive productivity or performance measures (Comi et al., 2020; Ferrari et al., 2021; Baltrunaite et al.; 2021), while papers covering the German reform tend to find zero effects (Fedorets et al., 2019).

Furthermore, there is a literature trying to explain the diverging productivity measures by focusing on board member characteristics. Comi et al. (2020) evaluates the quota reforms in France, Italy and Spain, concluding that only the Italian reform led to improvements in firm performance, which they link to improved board characteristics. Similarly, Gertsberg et al. (2021) find that the Californian quota law only led to lower share prices when favored male board members were substituted rather than the least-supported men.[14] Baltrunaite et al. (2021) link productivity increases in state owned enterprises in Italy to the quality of the board members.

Our study thus makes several contributions to the existing literature. We are the first to provide evidence of anticipation effects in this setting. The magnitude of the effects clearly points to the importance of acknowledging anticipatory behavior when analyzing firms' responses to quota laws in particular but also for any foreseen law in general. Thus, we contribute to the small but growing literature on anticipatory effects (Malani and Reif, 2015; Coglianese et al., 2017; Alpert, 2016). Second, the policy discussion so far has mainly been binary, either for or against quotas. Our paper shows that it is possible to increase female board participation and firm performance at the same time without resorting to actual binding quotas. Third, board diversity increases along several dimensions, suggesting spillover effects on other minorities. Fourth, board stability and firm performance improve as a consequence of the threat, making the previously unregulated market for board directors seem suboptimal, as also suggested in Ferreira et al. (2017). These results further

[14] The effect of the Californian law on stock prices is also evaluated in Hwang et al. (2018), von Meyerinck et al. (2019) and Greene et al. (2020).

indicate the importance of recruitment firms for filling vacancies for top positions in corporations. Finally, corporate boards matter for firm performance, primarily measured here as ROA. Our results are in line with the literature showing the importance of the board's appointment of a CEO. Here, we contribute primarily to the board director and CEO turnover literature (see, for instance, Jenter and Kanaan (2015), Jenter and Lewellen (2021), Weisbach (1988), Parrino (1997), Kaplan and Minton (2012), Huson et al. (2001), Denis et al. (2015), Cornelli and Karakaş (2015), Bushman et al. (2010), Peters et al. (2014), Fee and Hadlock (2004) and Bates et al. (2018)).

B. Background of the threat

Listed companies in Sweden have a long history of male-dominated boardrooms. During the 1990s, the share of females on boards was steady at just below 5%. Then, in 2003, the share of females began to increase, tripling within 3 years. Anecdotally, this increase has been attributed to threats of a gender quota law made by the minister of gender equality, Margareta Winberg, during the second half of 2002. Winberg, a prominent feminist figure with a long history in the Social Democratic Party and the government, became the minister of gender equality in 1998. In our study, identification is linked to the timing of the threat, and therefore, it is crucial to describe the threats that were carried out over time. Figure 1 shows the number of printed newspaper articles in Sweden, a major channel used by policy makers to propose new policy ideas. The number of articles is based on a search that includes the minister's name and the terms "quota", "women", and "board".[15] In 1999, as depicted in Figure 1, Winberg began to discuss, although rarely, the role of boardroom quotas for women in listed companies. Previously, she had acknowledged that a female quota in the business world could be problematic since competencies might be scarce. In three articles in leading Swedish newspapers in 1999, Winberg stated that she was not hostile to a law but hoped instead to see voluntary improvements within 5 years. In the following years, gender quotas in boardrooms were absent from the debate, as depicted in Figure 1.

[15] Source: Mediaarkivet, a digital archive containing more than 700 printed newspapers. See http://www.retriever-info.com/sv/category/news-archive/. The search was "margareta winberg kvotering kvinnor styrelse".

[Insert Figure 1 Here]

In 2002, the temperature of the debate rose. During that year, the number of printed articles mentioning Winberg's name in combination with quotas, women and boards exploded. In June 2002, Winberg indicated in the leading business daily *Dagens Industri* that she was contemplating a quota law to increase the pressure on listed firms (*Dagens Industri* 2002-06-17). As a result, the debate became heated. Following Winberg's appointment as deputy prime minister in October, a series of articles intensified the tone and outlined the quota threat in detail. In an article in the *Dagens Industri*, she stated that "the threat is real," noting that if the listed companies were not making significant progress, "there will be a law" (*Dagens Industri* 2002-10-22). In another article in the leading daily paper *Svenska Dagbladet*, Winberg defined significant progress: the share of female directors must increase to 25% within two years. She noted that she had full support from Prime Minister Göran Persson and that a formal "Investigation Directive" was under way and would be ready by the spring. After that, a formal investigation could proceed. Winberg estimated that the law would be ready in 2004 or 2005. Thus, the magnitude of articles significantly increased, and the tone concerning a quota was sharpened at the end of 2002. Winberg's new political appointment, her well-known feminist ideology, combined with the backing of the prime minister, strengthened the credibility of the quota threat. For the first time in Sweden's history, the representation of women on the boards of listed companies began to rise consistently.

In this study, we set 2002 as the baseline year since we observe data annually. Thus, the red vertical line in Figure 1 denotes the year 2002. This choice is reasonable for two reasons: the explicit threats were laid out at the end of 2002, and shareholders appoint new directors at an annual meeting. Since the annual meeting typically occurs in the late spring, 2003 was the first year in which the companies had the opportunity to respond to the threat.[16]

The time series of the articles ends in 2003, the year Winberg resigned. However, the investigation of the law was established by the minister of justice, Thomas Bodström, in the summer of 2005, and in June 2006, a law proposal was finished. The proposal stated that listed firms (and government-controlled limited liability companies) should have at least 40% women on

[16]In the Appendix, Table A2 depicts the results if 2001 is set as the baseline year. The results do not differ substantially.

their boards by 2008; otherwise, a fine would be paid every time a new board was elected. The investigator argued that other limited liability companies should also not be subject to the law.[17] Thus, the law proposal was consistent with the content in the previous threats made towards listed limited liability firms.

In September 2006, the Social Democratic Party lost the election, and a new conservative-liberal government was formed. The new government was against the gender quota law proposal, and as depicted in Figure 1, the share of female representation halted for several years. In February 2010, both Anders Borg, the finance minister, and Per Schlingmann, the spin doctor and secretary of the leading party in the government "Nya Moderaterna," complained that progress toward female representation was too slow (it had been stable since the Social Democrats lost the election and the law proposal was rejected), again opening up the discussion of a law (*Dagens Industri,* 2010-02-02). However, at Nya Moderaterna's annual convention a year and a half later, party members reacted strongly and rejected any quota law (*Dagens Industri,* 2011-10-22).

Generally, the development of female representation on corporate boards responds to different threat levels. However, in this paper, we will focus on the first major threats at the end of 2002 and study their effects. From a causal point of view, everything else may be an endogenous response.

III. Methodology and Data

A. Methodology

Previous papers have pointed out that failing to take anticipatory effects into account can severely bias estimated effects of a law (Ashenfelter, 1978; Angrist and Pischke, 2009; Malani and Reif, 2015). Yet the previous literature on board quotas has seldomly discussed the importance of anticipatory effects, even though most of the proposed quotas have taken several years to implement typically. We thus begin our empirical analysis by focusing on the effect of the credible threat on the share of females on boards, which we refer to as the intended effect, and on other board composition characteristics, which we call unintended effects. After this, we proceed to

[17]See the investigation proposal "Könsfördelningeni bolagsstyrelser" (2006) for a full description.

determine how the threat affects firm performance and contrast this finding to previous findings of the effect of a law.

There are several reasons why failing to take anticipation of a law into account may cause biased estimates. For instance, if the pool of talented women is not endless, then one will expect the first new women to be hired on to boards to have the best credentials, while later recruits will probably have lower competence. Thus, if one fails to take the anticipation part of a law into account, it is possible to estimate a general decrease in the quality of the board members, although the actual effect might be zero or even positive. Similarly, one may conclude that firm performance has decreased, while the true effect is zero, if firms that act under the anticipation period are those that benefit from hiring women on to their board, for instance due to more competent women being available in their particular industry.

We will hence estimate reduced form effects of the threat of a quota law using a difference-in-difference methodology. Specifically, as only listed firms were covered by the threat, and the serious discussion of a law started in 2002, we start by estimating the following model:

$$(2) \qquad Y_{ct} = \alpha + \gamma Listed_c + \lambda\, Post_t + \delta(Listed_c * Post_t) + \mu_{nt} + \varepsilon_{ct}$$

where *Post* is a dummy taking the value of one for the period after 2002 and zero otherwise. Equivalently, *Listed* is a dummy for listed firms *c* in 2002, while Y_{ct} is one of our outcome variables of firm *c*. Finally, μ_{nt} is a full set of industry*year fixed effects, thus non-parametrically capturing all industry specific time trends. We present result with and without industry*year fixed effects.

The key identifying assumption is parallel trends of the outcome across treatment and control groups in absence of the treat. Under the parallel trends assumption, δ, the parameter of the interaction, will measure the causal effect of the threat of a quota law on our outcomes.

Given the large amount of disagreement in the evaluations of the Norwegian reform and the conflicting evidence from other quotas on firm performance, we provide a battery of specification tests in this paper. We start with a simple difference-in-difference specification without industry time trends. We then introduce firm and year fixed effects to control for all time-invariant firm characteristics and any general annual shock. Next, we address compositional bias by including industry-year fixed effects and thus non-parametrically control for industry-level specific time

trends.[18] Because this model will control for the most flexible time trends in all specifications, it will be our preferred model. In addition, it is straightforward to introduce two specification tests for parallel trends, as discussed by Angrist and Pischke (2009). First, we could add the leads of the variable of interest. If the parallel trends assumption holds, the coefficient should be both close to zero and statistically insignificant. This specification makes it possible to test whether there are any "pre-treatment" and dynamic effects. If the empirical approach is credible (parallel trends holds), then there should be no pre-treatment effects.

Furthermore, we could add a linear trend interacting with $Listed$ to the specification, and if the parallel trend assumption holds true and there are no dynamic effects, then the effect should remain stable (Roth 2018). However, since the election of board members often occurs at the annual meeting in the late spring, we could expect the effects to be smaller in 2003, implying that the coefficients will likely shrink slightly when we include the linear trends.

We also construct a synthetic control group by a weighted average of industries to match the pre-trends of listed companies according to the method of synthetic control developed in Abadie et al. (2010). As in any difference-in-differences setting, however, other factors could still be affecting the treated and control groups differently around the time of the event in 2002. Thus, we provide additional robustness checks in section V, including limiting the event window and results using foreign listed firms as the control group rather than non-listed Swedish firms and demonstrate the impact when the Social Democrats return to power after eight years in 2014. In summary, our results are robust to these checks.

Moreover, we acknowledge that the estimations of the standard errors are problematic in our study because the treatment only changes once for one group, as discussed by Bertrand, Duflo and Mullainathan (2004), Donald and Lang (2007), and Conley and Taber (2011). Regarding the standard errors, we begin by clustering them at the industry level, thus acknowledging not only firm correlated shocks but also industry shocks. Compared to the related literature, this is a conservative treatment of the standard errors. However, since the treatment varies only once at the

[18] In Appendix, Table A5, we also estimate our main model in which we leave out one industry at a time. This model is motivated by the fact that the year 2003, the first year of the treatment, is three years after the burst of the dot-com bubble and one could be worried that certain industries, such as IT or telecom, will drive our results. Fortunately, our results are robust when leaving out one industry at a time as well as when we control for industry trends.

control-treatment group level, it might not be conservative enough. Here, we follow the Pettersson-Lidbom and Thoursie (2013) application of the results in Donald and Lang (2007) and address the clustering problem as discussed in Moulton (1986) by aggregation into a time series and apply the Newey-West estimator with one lag as a robustness check.[19]

Lastly, in our main specifications, we consider a few restrictions on the data, as discussed below. For the sake of transparency, the sensitiveness of the results to these restrictions can also be found in the Appendix.

B. Data

Our main data consist of two datasets that have been merged. The first is composed of all, except financial, limited liability firms' final accounts and key figures over the 1998-2012 period.[20] To these data, we add information on all individual board members in limited liability firms and the years during which they were on the board. These data contain information for the 1998-2012 period.[21] Specifically, we take all board members who are on the board at some point during the given year and then compute the average share of women and other characteristics on the board based on these numbers. All data come from the Swedish Companies Registration Office. The office keeps track of, for example, the financial statement items and number of employees. By Swedish law, each firm must file this information within 6 months after the end of a fiscal year.

From a causal point of view, anything occurring after the threat and onwards could be endogenous, including delisting. Any restriction on data before the threat is non-problematic since it is based on pre-treatment characteristics. All restrictions made below will therefore be based on

[19] In Appendix Table A2, we also use two lags, which makes no substantial difference.

[20] Some firms do, however, produce two or even three accounts during one calendar year. To avoid weighting these firms more heavily, we identify their final accounts by the observation with the highest turnover in each year. Since the turnover increases over the fiscal year, this should leave us with the final accounts only. Notably, not all variables and measures exist for all firms in our sample.

[21] The data on boards contain information for more years than 1998-2012; however, it is censored from both the top and the bottom outside the range of 1998-2012. There are no dates assigned for those that start on a board prior to 1993 or who quit after 2012. Likewise, those quitting a board prior to 1988 or after 2012 have no date recorded. Since the data on the final accounts begins in 1998, the censoring prior to 1993 does not matter. Similarly, since both the board and final accounts data end in 2012, any censoring after that point is irrelevant to this study.

characteristics in 2002. In the Appendix, we will relax our restrictions, one by one, to verify and disclose the robustness of our results. The results are found in the Appendix, Table A2.

We begin with the sampling restriction wherein we limit our analysis to all firms that were active in 2002. A non-active firm is a firm in which there is no intent to operate a normal business. Furthermore, we define the treatment status based on whether a firm is listed or not in 2002, which means that we can use the number of firms as an indicator of compositional bias due to delisting. Thus, we show how the number of firms in the treatment group varies over time in Figure 2. Since we condition on the firms being listed in 2002, it must follow that there are (weakly) fewer firms before and after 2002. Clearly, attrition in the treatment group after 2002 might be an outcome causing survival bias when examining firm performance measures. If we find that the quota threat caused listed firms to perform better, we are worried that the worst-performing listed firms have exited. Figure 2 below shows the number of listed firms conditioned on their existence in 2002. We notice first that there is no substantial attrition until the financial crisis in 2009. Thus, the threat does not seem to have caused a large outflow of firms from the listed group.

[Insert Figure 2 Here]

Since non-listed firms may have a board size of 1, we limit our analysis to firms with a board size of at least 5 directors in 2002 for the firms to be more comparable. Furthermore, we consider only ordinary board members as part of the board, and thus, we exclude deputy directors and the like.[22] However, our results are not very sensitive when including these members. While a number of other reasonable restrictions could be made, our main analysis will hinge on these restrictions. However, in Appendix Table A3, we show the results for other plausible restrictions, including restrictions on the minimum amount of capital in the firm and the number of employees.[23] These different restrictions do not drive the results.

[22] We do not include labor union representatives.

[23] A public firm might have more than 200 stock owners and should have at least 500,000 SEK (approximately 60,000 USD) in share capital, whereas private limited liability firms may have as little as 50,000 SEK. Before 2005, this amount was doubly as high at 100,000 SEK. Moreover, public firms need a board size of 3, whereas private firms suffice with 1 member.

Finally, we determine the gender of the board members through their personal identification number for all Swedish residents. Using personal numbers, we obtain exact gender information for 95.72% of the data.[24] For non-Swedish residents, however, we rely on board members' first name. To do this, we use the list of all names given to more than 10 boys or girls born in the previous year (2014) from Statistics Sweden, dropping all duplicates between the genders, and then defining the gender of the board member by checking their first name against this list. This process increased the hit rate to 98.15%. If we could not determine the gender of a board member after this process, the board member's gender was coded as missing. Thus, we end up with final account data for the universe of limited liability firms in 2002 (except financial firms and after said restrictions) for the 1998-2012 period, along with information on the boards' gender composition and other board characteristics, such as the share of board members with an immigration background and the members' average age.

Moreover, since a firm can belong to a group of firms, we focus our analysis on the group's final accounts for the accounting data and the parent firm for the board data. If the firm is not part of a group, then we study this sole firm. As an additional sanity check, we present the results when we include only firms that are organized as groups in 2002, which again does not significantly alter our results. As is standard in the literature, we winsorize all financial variables at the 1% and 99% levels. Thus, we cap all values above the 99th percentile and below the 1st percentile to the values at the 99th and 1st percentiles, respectively. This procedure is conducted separately for the listed and non-listed firms. The results after alternative levels of winsorizing can be found in Table A4, and it is reassuring that point estimates are unaffected by winsorizing levels as only the precision changes. The summary statistics for all firms after the process of winsorizing are presented in Table 1. Panel A shows the statistics for all firms, while panel B does the same for only groups. First, the average share of female directors is approximately 14% for the period. Second, one can note that the mean of operating profits/assets is close to zero. Turning to Panel B, where we use only groups, we can see that the share of women on average was 13% over the period, while operating

[24]A regression using only those in which the gender is identified from the personal number can be found in Table A6, column 1. The results are again robust.

profits/assets were approximately 0.02. For the sample in panel A, we have approximately 165,000 observations, while the same number for the group-only sample is approximately 35,000.

[Insert Table 1 Here]

IV. Results

A. Firm Governance Composition: Graphical Evidence

We begin by graphically inspecting the time series of the share of female board directors in Figure 3, panel A. Interestingly, in the years before the quota threat, we see a slightly upward and parallel trend in both listed and non-listed firms, although non-listed firms have a higher share of female directors than listed firms. After the threat, there is an extraordinary increase for listed firms, whereas the non-listed firms remain on the same approximate trend. Credibly, after the rejection of the law in 2006, parallel trends emerged once again. The effects are the mildest in the first year, showing some dynamic effects before stabilizing around 2006. Panel B shows the coefficients of annual treatment effects from our preferred model, as discussed by Angrist and Pischke (2009). The estimates suggest small and partly non-significant effects before the threat, with sharply increasing effects in the first few years after the threat, which then appear to flatten out around 2006. Although the estimates are close to zero before the threat, there may be weak evidence of an increase in the share of female board members before the threat. Therefore, testing whether the effect survives when including linear treatment and control group trends will be of interest.[25] However, the overall pattern is consistent with a causal interpretation of the effects. The effect size is approximately 8 percentage points. In Panel C we construct a synthetic control group to listed firms by a weighted average of industries where we match on pre-treatment levels of female board participation. Following the advice in Abadie et al. (2010), we match the dependent variables in

[25] Spillover effects from listed to non-listed firms may also exist. Panel A in Figure 3 shows possible weak evidence of an increase in the share of women for the non-listed firms in the post period. The spillover could result from the positive experience of the listed firms from increasing the share of women on the board. However, any such spillovers are limited because the share of women in non-listed firms is at a much higher level to begin with, as is evident from panel A. In any case, if such a spillover effect exists, it would bias our estimates downwards.

1998, 2000 and 2002.[26] Again, we observe a small increase for both groups in the pre-threat period, but no difference between the groups; that is, parallel trends hold for this specification. Then, starting in 2003, we observe a sharp divergence between listed and non-listed firms. Thus, we observe a similar pattern with no large differences in pre-treatment trends and a large increase in the post period regardless of whether we use the raw time series, our preferred regression model or a synthetic control framework.

[Insert Figure 3 Here]

B. Firm Governance Composition: Main Regression Results

In Table 2, we present our main results for the share of women on boards, beginning with estimating the basic DID model outlined in equation (2), in Column 1. The threat of quotas caused the share of females to increase by approximately 8.7 percentage points, an increase of approximately 150%. In Column 2, we then use firm and year fixed effects instead of the *Listed* and *Post* indicators, which leaves the estimates more or less unchanged. Our preferred model in Column 3, where we add industry-flexible time trends, does not alter the results either, thereby strengthening the indication that our found effect is not driven by underlying industry-specific trends. In Column 4, separate linear trends per treatment group are added. Thus, our identification strategy no longer hinges on the parallel trend assumption; instead, the necessary assumption is that any underlying difference in trends is not nonlinear. Since Figure 3 indicates a slightly upward trend, it is not surprising that the estimate changes. However, it remains significant and large at approximately 4 percentage points. Notably, if the first-year reaction is the mildest due to dynamic effects, which has been suggested since directors are appointed in the late spring, then part of the "true" effect is controlled away when adding linear trends. Lastly, in Column 5, we present the results from estimating equation (4), i.e., using collapsed data and a time series of 15 observations

[26] To implement Abadie et al. (2010), we collapse the data into the treatment group (in other words, all listed firms) and the remaining companies into industries, leaving us with 57 time series, where one is the treatment group and the other 56 are the remaining companies in their respective industries. To these data, we then apply the synthetic control method as in Abadie et al. (2010), where the control group is a weighted combination of the industries without the listed firms. As matching variables, we simply use the values of the dependent variable in 1998, 2000 and 2002. The exact resulting estimates of the effect can be found in Table A1 in the Appendix.

to address the Moulton and serial correlation problems when estimating the standard errors. Although the standard errors double in size, the effect remains significant.

[Insert Table 2 Here]

In Table 3, we proceed to look at other measures of corporate governance and board diversity. Column 1 in panel A shows that the threat causes the share of directors being born in Stockholm to decrease by 2.3 percentage points, while Column 2 displays that non-EU immigrants increase their share by approximately 2 percentage points. Both of these numbers indicate that the directors of the board are now recruited from a larger network. We conclude that this result is consistent with the fact that the listed firms began to use new recruitment firms and practices to find more female board directors and then also received the benefit of an expanded network of potential new directors. This also makes sense, as resent evidence in von Essen and Smith (2022) suggest that most women are outside of the usual recruitment network to corporate boards. Hence, other groups typically outside of this network also benefit from the quota threat.

The share of EU immigrants does not change significantly. However, taken at face value, the effects size is similar to the share of non-EU board members. Moreover, the average age of the board decreases by slightly less than a year, as shown in column 4. Furthermore, there is no significant change in the share of directors who have a CEO background, measured as being recorded as CEOs in our data for the 1998-2000 period. However, the log of the number of boards on which a director sits is significant at the 5 percent level, where the coefficient implies that the average board member sits on 0.06 more boards in total. Furthermore, both the board member turnover rate and the CEO turnover rate decreases, implying a better matching of directors to boards and a lower tendency to fire CEOs.

Panels B and C in turn look at the same outcomes but focus only on female or male board directors. It is interesting to note that although female directors seem to primarily drive the increase in the share of directors born outside of Stockholm and the average lower age, the higher share of immigrants from non-EU countries is driven by male directors, suggesting that male board members began to face more competition. Furthermore, both male and female board members have a lower turnover rate after the threat, suggesting that the matching of directors is improved. This final result is in line with the findings in Ferreira et al. (2017), who also suggest that firms in France began to use recruitment firms as a consequence of the quota law. Male board directors also sit on

significantly more boards. This finding, along with the improved matching of male directors, suggests that more able male board directors begin to sit on more boards after the threat, while less able board directors are replaced by the more abled men, non-EU immigrants and women. Finally, we can see that both male and female CEOs seem to experience a similar decrease in turnover based on coefficient size, although we lack conventional significance in both instances.

In sum, the threat thus not only seems to have the intended effect of increasing the share of women on the boards, but boards also seem to function better among the treated firms after the reform, with a lower turnover rate and greater board diversity that is not driven solely by the increase in female directors. Furthermore, the new board composition seems to directly affect the appointed CEOs, as the turnover rate decreases.

[Insert Table 3 Here]

One relevant question is whether this increase in female board representation is uniform across different types of firms or if, for instance, larger or business-to-consumer (B2C) firms are more likely to respond to the threat. For example, some firms are more responsive to good press or customer sentiments. In Table A9 of the Appendix, we show that no large heterogeneity exists in responses to the threat when it comes to firm performance, firm size, whether the firm is in a business-to-business (B2B) or B2C industry, or whether the firm already has at least one woman on the board. The fact that the response is so uniform suggests that it is driven by increased competition for the best women. Specifically, if the best women are the first to be recruited, then you would want to get these on to your board as fast as possible, before some other company take all the available talent. Conversely, if board members do not matter, or if talent is abundant, then certain types of companies could afford to procrastinate its recruitment of women until the law was passed.

In Figures 4 and 5, we offer suggestive evidence that this is the case. Figure 4, panel A shows that the share of women born in Stockholm remains relatively stable at first after the reform. However, after a few years, the share seems to gradually decrease, suggesting that the talent pool is running thin in Stockholm and, thus, companies are using recruitment firms to look elsewhere to find more competent women. Interestingly, in Figure 4, panel B, we do not view as strong a pattern for men. Second, in figure 5, panel A, we observe the share of women on the board who are in between 40-50 years, i.e. a group that both has had time for accumulating experience and is more

likely to be beyond the years with small children. Here, we expect that the share of women 40-50 years first increases as an increasing number of competent women are recruited, only to decrease again as aging also occurs for this cohort. Indeed, this is exactly what we observe for women; in figure 5, panel B, we observe no such similar pattern for male board directors.

[Insert Figure 4 Here]

[Insert Figure 5 Here]

The effect of lower CEO turnover on firm performance is not obvious. If the lower turnover rate is caused by the fact that good performing CEOs are less often replaced, we expect a positive effect on firm performance. In contrast, if the lower CEO turnover rate is caused by poor decisions due to the new board composition, causing for instance a lower willingness to fire underperforming CEOs, we expect a negative effect on firm performance. Finally, if the new CEOs are as able as the previous ones on average, or if CEOs have little impact on firm performance, we expect no effect. Thus, we also more closely inspect how turnover rates differ depending on firm performance in the pre-treatment period.

This difference is illustrated in Table 4, panel A for board members and panel B for CEOs. Columns 1, 3, and 5 show firms with a negative average ROA during the pre-period, and columns 2, 4, and 6 show firms with an average positive ROA during the pre-period. From this table, we note that the lower turnover rate for female board members in Table 3 stems primarily from firms with a negative ROA during the pre-period. This observation suggests that these firms had more dysfunctional recruitments, in particular of women, before the threat. For male board members, the pattern is more uniform. We also note that the decreased CEO turnover in Table 3 stems from firms with a positive average pre-ROA, in particular for male CEOs.[27] Thus, CEOs that performed well had a higher tendency to stay, suggesting that firm performance could increase as a consequence of the threat.

We now turn to firm performance.

[27] The sample is relatively small for female CEOs, making it hard to draw any strong conclusions for this group.

[Insert Table 4 Here]

C. Results for Firm Performance: Graphical Evidence

We now turn to our main firm performance measure, operating profits divided by total assets (ROA), as used in Matsa and Miller (2013).[28] Figure 6, panel A shows a rather similar downward trend between listed and non-listed firms until 2002. The sharp decrease in ROA due to the burst of the dot-com bubble in 2000 is visible for both groups. The dot-come bubble decline also pedagogically illustrates the point of having a control group. However, there is also a slight tendency for profits to decline more for the listed groups between 2000 and 2001, potentially indicating a mild Ashenfelter's dip. When analyzing the annual treatment effects in panel B, however, the dip does not seem to significantly influence the results. We also note that the Lehman Brothers crisis in 2008 also yielded a sharp decline in profits and that the decrease is again somewhat larger for listed firms. It is therefore reassuring that we do not see a pattern in which the listed firms see some years of faster growth rates of ROA after the Lehman Brothers crisis. Thus, the estimated effects for the threats in the period from 2003 onwards are unlikely to merely be a convergence effect driven by the dot-com bubble in 2000. Profits over assets increase by approximately 0.04-0.05 among listed firms after the threat relative to the change in profits in unlisted firms in the same period.

[Insert Figure 6 Here]

Moreover, an interesting and important correspondence occurs between Figures 3 and 6. Both outcomes appear to be parallel before the threat. Then, a large reaction for the listed group occurs

[28] Ahern and Dittmar (2012) use Tobin's Q as their measure of firm performance. To compute this metric, however, one needs the market value of the firm, which we cannot observe for the non-listed firms. We thus focus on the other commonly used firm performance measures that are available both for our treatment and control groups. In Tables A7 and A8 in the Appendix, we provide more short-run estimates on stock-market prices between Swedish and non-Swedish listed firms on the Stockholm Stock Exchange.

until 2005-2006, when the law was rejected, for both the share of females and profits over assets before stabilization occurred and parallel trends emerged once again.

Lastly, in panel C, to address any remaining concerns about similar trends or to determine whether the effect might be driven by Ashenfelter's dip, we perform a robustness check using a synthetic control group approach once more. Again, we match on the dependent variable in 1998, 2000 and 2002. The effect size is now approximately 4 percentage points for ROA (see Table A1 in Appendix). Thus, concerns about pre-trends or dips are not critical to our results, nor, as suggested by panel C, that our results are driven by functional form assumptions.

D. Results for Firm Performance: Main Regression Results

Turning to our main regressions in Table 5 on firm performance and ROA, we see in general that using the financial statements for all firms (panel A) yields somewhat larger estimates compared to using only the groups in our sample (panel B). In summary, profits over assets increase by approximately 0.04–0.06 among listed firms after the threat relative to the change in profits in unlisted firms in the same period. Again, the first column runs our basic difference-in-differences model, while the second controls for firm and year fixed effects, the third includes our preferred model with industry×year fixed effects, and the fourth includes a linear trend. The final column again collapses the data to a time series and uses Newey-West standard errors with one lag. Overall, the results remain stable over all specifications, with a slight decrease in the coefficient when we include the industry×year fixed effects.

[Insert Table 5 Here]

In summary, we observe an increase in firm performance as a consequence of the quota threat. This finding thus strengthens our conclusion from section IV above that the threat caused the listed firms to begin to use new recruitment practices, which lead to better director-board matching and kept competent CEOs in the company to a larger degree. Interestingly, these findings are in line with the results in Besley et al. (2017), but since we study a corporate rather than a political setting,

we can also conclude that the quota threat seems to lead to a more efficient organization through the effect on firm performance.[29]

E. Results for Firm Performance: Additional and Robustness Results

In Table 6, we use our preferred specification model to investigate a few additional firm measures. Since operating profits include depreciation and amortization, we also show the effect for the EBITDA/assets outcome in Column 1. Again, our estimate is statistically significant, with EBITDA/assets increasing by approximately 0.038 among listed firms after the threat relative to the change in profits among unlisted firms during the same period. When considering only total revenue/assets, we again obtain a positive estimate, although less precisely estimated. Interestingly, labor costs/assets decrease by approximately 0.037 among listed firms after the threat relative to the change in labor costs/assets in unlisted firms during the same period. This finding, along with our estimates for ROA above, contrasts with that of Matsa and Miller (2013). Due to the accounting identity, an increase in profits must reflect some mixture of an increase in revenues and/or a decrease in costs. Although estimated with low precision, revenues seem to increase a little, and labor costs decrease. Two alternative outcomes, operating profits per employee and value added per employee, are presented in Columns 4 and 5. The results show the same sign as our other firm performance measure but are imprecisely estimated. Column 6 shows a positive but insignificant increase in the logged number employed, while Column 7 shows that the logged number of board members increases by approximately 0.22. A back of the envelope calculation suggests that boards expand by approximately one board member due to the quota threat. Thus, this finding again illustrates how the gender quota threat affects numerous potential channels that can affect firm performance.

[Insert Table 6 Here]

[29] These results are also in-line with more recent evidence in Baltrunaite et al. (2021), although they focus on state owned enterprises.

There could still be other major factors affecting listed companies differently than non-listed companies around 2002-2003. In any difference-in-differences setting with one policy change and two groups, particularly with annual data, this is a major concern, and ultimately, it is not testable. However, some sanity checks can be made. First, we identified two other potential drivers. Ferreira (2015) notes the changed Norwegian Code of Practice for Corporate Governance and changed accounting rules (Norway adopted IFRS accounting rules in 2005). Since Sweden also implemented both of these practices in 2005, we provide estimation results from a shorter window, namely, 1998-2004, which can be found in Table A6, Columns 2 and 3. Our results are similar for this shorter period, which makes it less likely that these two changes are drivers of the results.

Second, we provide an alternative control group: firms that are listed on the Stockholm Stock Exchange but registered abroad. Firms registered abroad follow the law of their country of register.[30] Moreover, this approach also allows for market-based outcomes in terms of stock prices. Unfortunately, we do not observe the industry of foreign firms, which means that we cannot include industry trends. Thus, we include only firm and year fixed effects in these regressions. Again, the results for female board composition and ROA are similar, as shown in Appendix Table A7, Columns 1 and 2, respectively. This again makes it likely that the threat of quotas is the cause. Furthermore, Column 3 shows the results for stock market prices in 2002, with a positive but not statistically significant effect.[31] Table A8 also shows estimates for the stock prices with varying window sizes around the initial threat on June 17, 2002, and October 22 when the threat was made more concrete. Around June 17, we observe close to zero or slightly positive coefficients, but around October 22, we do actually observe quite large positive coefficients of around 0.07-0.08 increases in Swedish companies' stock prices. The data is too noisy to detect any significant effects however. Figure A3 provides the time series for stock prices of both Swedish and foreign firms on the Stockholm Stock Exchange in 2002, with two red lines indicating June 17 and October 22.

Finally, Figure A4 also demonstrate what happens to the share of women on Swedish firms' boards and ROA once the Social Democrats returned to power in 2014, after eight years of rule by

[30] This approach follows the same idea in Eckbo et al. (2021).
[31] Regression analysis based on daily market data; company and date fixed effects are included.

a right-wing coalition with no plans to implement a quota law.[32] Again, we see an increase in the share of women on listed firms' boards compared to non-listed firms, and what looks like a corresponding increase in ROA. Thus, the threat still seems credible even after eight years out of office.

In sum, both ROA and the share of females on the board seem to be positively affected by the threat, and we see indications of positive effects on stock market prices, though the later estimates are too noisy to be significant.

VI. Conclusion

Gender quotas on corporate boards have recently received increased attention in the literature. The first quota law was adopted in Norway in December 2005, with other European countries following a few years later. This paper, however, uses a credible threat of gender quotas aimed at listed firms. We find that the threat caused a substantial and rapid increase in the female board share in firms listed on the Stockholm Stock Exchange. The effect size was approximately 5-10 percentage points, or a 100-200 percent increase. Thus, the anticipation effects of the quota law were large, consistent with a credible threat. Interestingly, this increase was accompanied by other changes in board composition, such as a lower turnover rate for board directors and for CEOs in profitable firms. The evidence suggests that the affected firms started to use (new) recruitment firms and networks, which led to more women and immigrants on the boards, which also became more stable and with fewer competent CEOs fired. This effect was then followed by an increase in measures of firm performance in the same years. We can generally reject effect sizes that are smaller than 0.005 measured as operating profits/total assets; on average, profits over assets increased by approximately 0.03-0.05 among listed firms after the threat relative to the change in profits in unlisted firms. This finding suggests that firm performance is related to both board competence and CEO competence. Moreover, labor costs decreased, in contrast to the previous evidence.

[32] Unfortunately, we do not have data for most of our outcomes after 2012 in our data, and thus we can only study the share of women and ROA after 2012.

Interestingly, the Swedish example shows it was possible to increase the share of women on corporate boards without resorting to actual quotas. Although we attempt to make substantial progress with respect to the implementation of the method, we cannot rule out the possibility that, in comparison to those of Norwegian studies, our conflicting results are due to differences across countries and reforms. In particular, although the Swedish quota threat was converted into a law proposal, it was never implemented due to a new government. Second, the threat increased female representation from approximately 5 to approximately 15 percent. This result was far from the level of 40 percent that was the intended goal in Norway.

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FIGURES

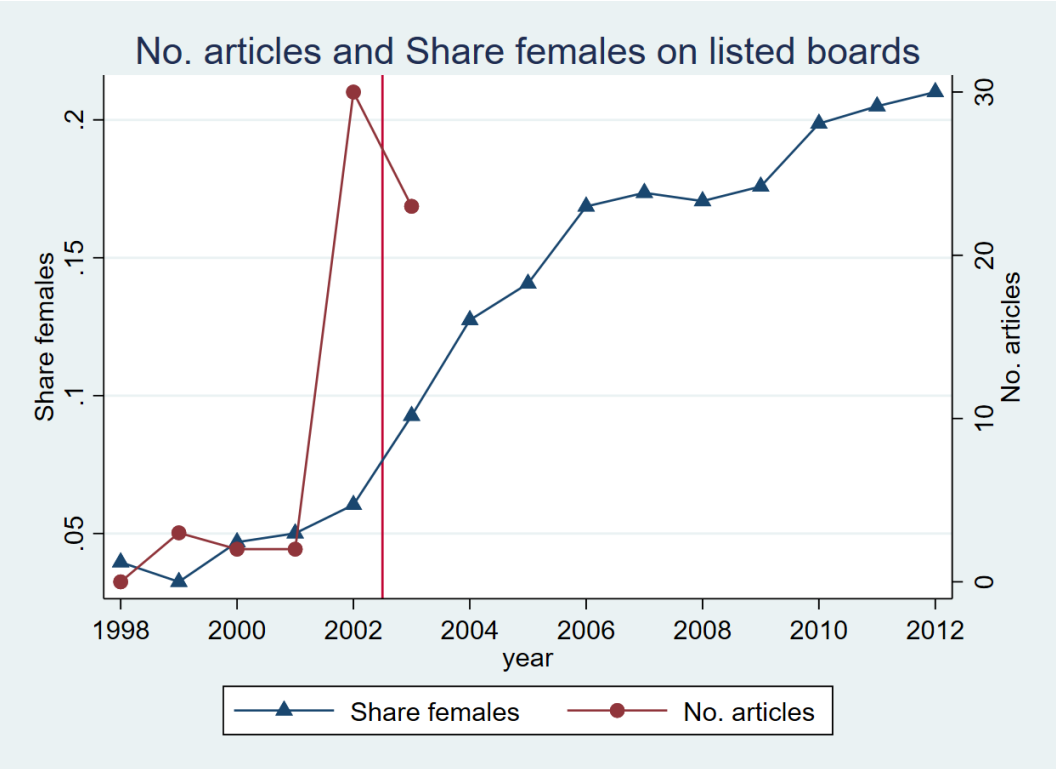


FIGURE 1. SHARE OF FEMALE REPRESENTATION ON THE BOARDS OF LISTED FIRMS AND THE ANNUAL NUMBER OF PRINTED ARTICLES IN THE SWEDISH PRESS FROM 1998 TO 2003

Note: The listed firms only include those listed on the Stockholm Stock Exchange in 2002 with at least 5 regular members on the board in 2002. The number of news articles is based on a search in Mediearkivet that includes the name of the deputy prime minister, Margareta Winberg, and the terms “quota”, “women”, and “board”.

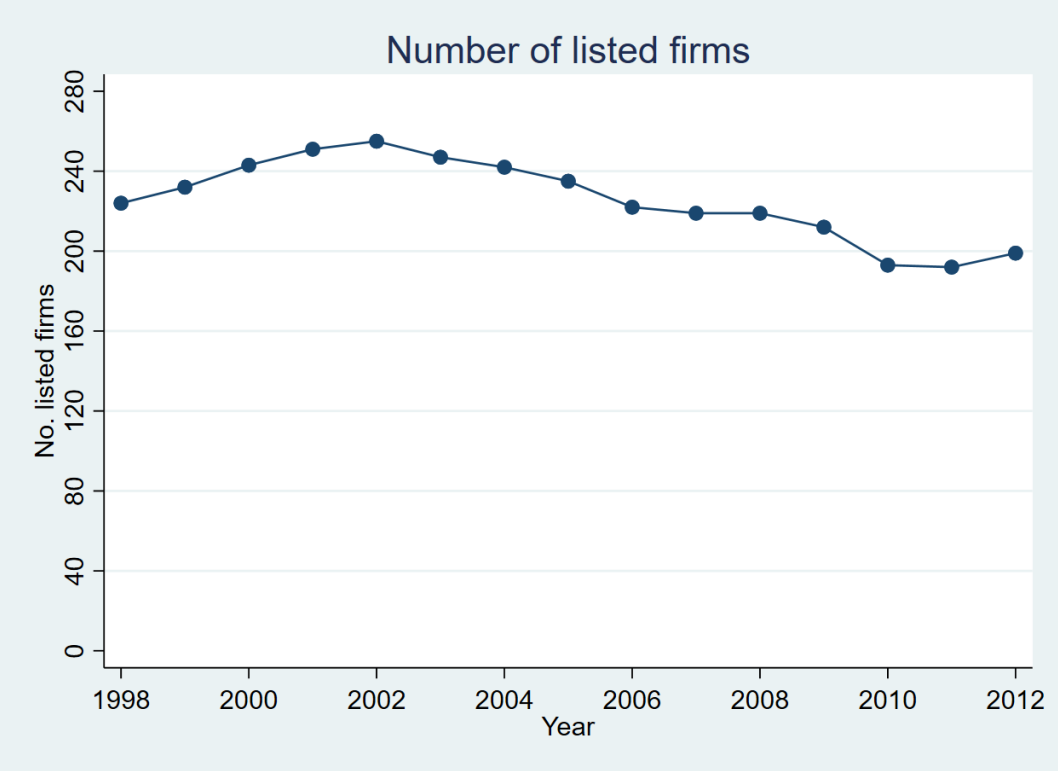
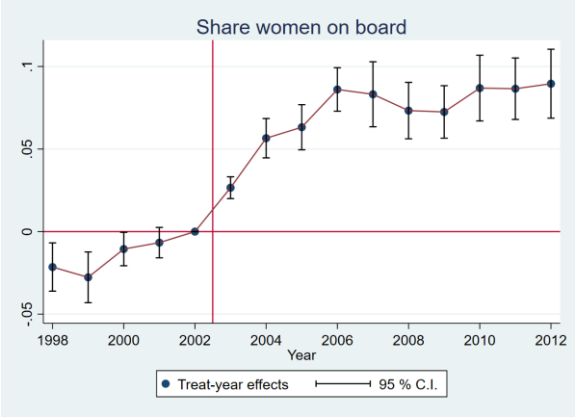


FIGURE 2. NUMBER OF LISTED FIRMS OVER TIME ON THE STOCKHOLM STOCK EXCHANGE

Note: The figure shows the number of firms listed at the Stockholm Stock Exchange in our data. The listed firms only include those listed on the Stockholm Stock Exchange in 2002 with at least 5 regular members on the board in 2002.

Panel A: Time series

Panel B: Annual treatment effects



Panel C: Synthetic control

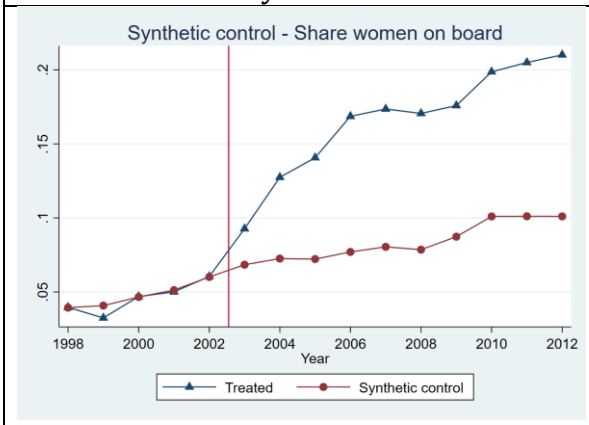
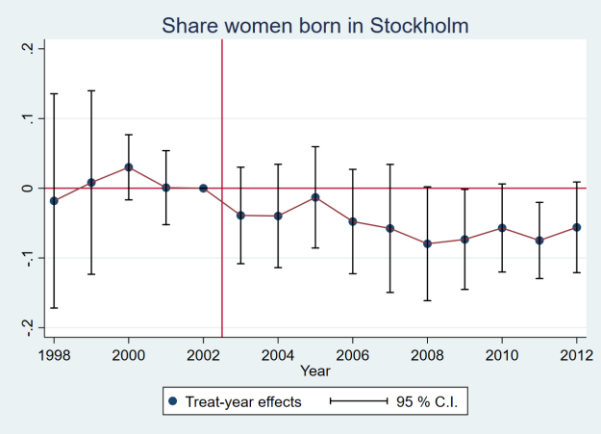


FIGURE 3. SHARE OF FEMALE DIRECTORS ON BOARDS, 1998-2012

Note: We only include firms with at least 5 regular members on the board in 2002. Treatment status, i.e. if the firm is listed on the Stockholm Stock Exchange, is determined based on the status in 2002. The regression in Panel B include industry in 2002*year fixed effects with the standard errors clustered at the industry in 2002 level. The synthetic control group in Panel C consists of a weighted average of industries to match the level of women on listed firms boards in 1998, 2000 and 2002.

Panel A: Share women born in Stockholm



Panel B: Share men born in Stockholm

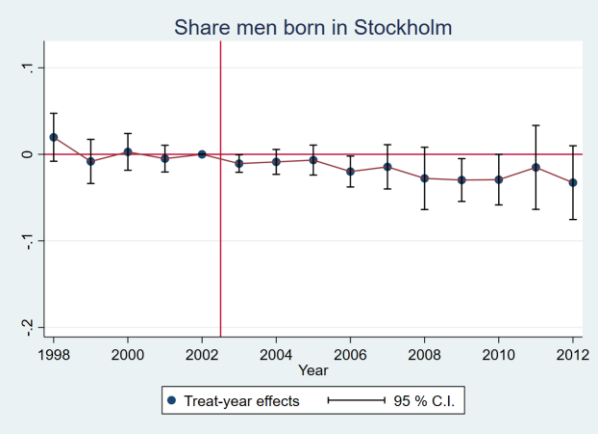


FIGURE 4. MEN AND WOMEN BORN IN STOCKHOLM, 1998-2012

Note: We only include firms with at least 5 regular members on the board in 2002. Treatment status, i.e. if the firm is listed on the Stockholm Stock Exchange, is determined based on the status in 2002. The regressions include industry in 2002*year fixed effects with the standard errors clustered at the industry in 2002 level. The variable is only possible to construct for Swedish citizens born before 1990.

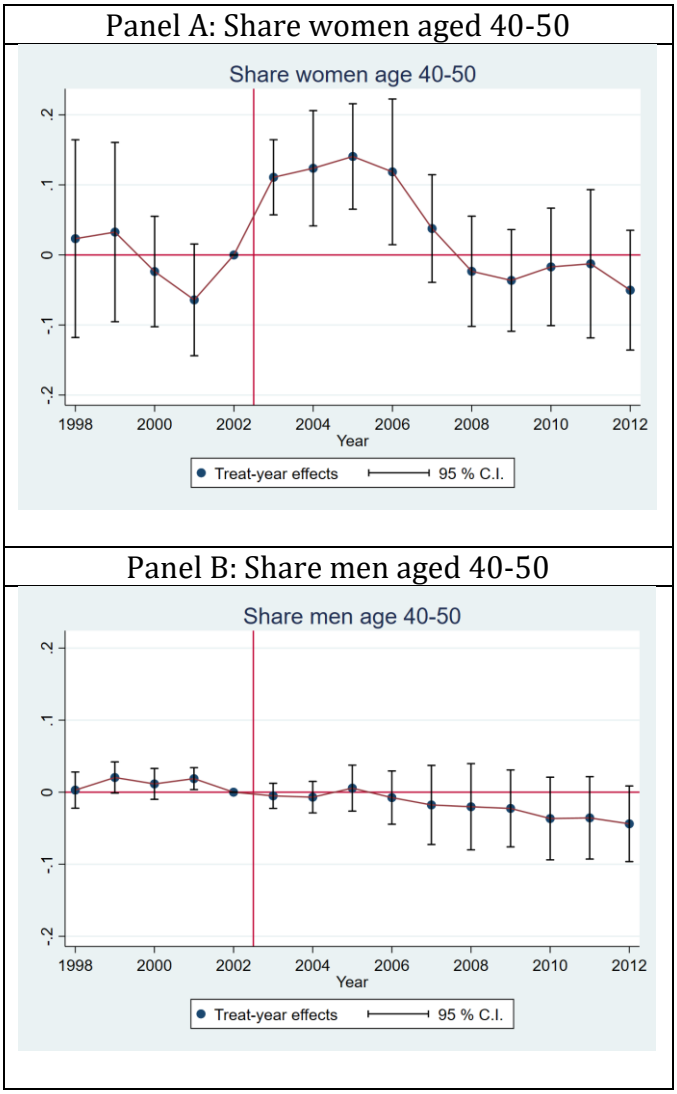
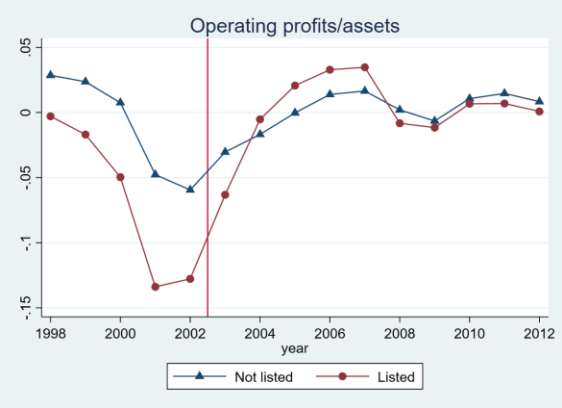


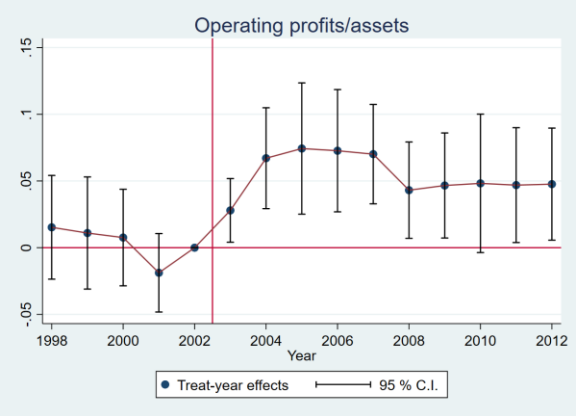
FIGURE 5. SHARE WOMEN AND MEN AGED 40-50, 1998-2012

Note: We only include firms with at least 5 regular members on the board in 2002. Treatment status, i.e. if the firm is listed on the Stockholm Stock Exchange, is determined based on the status in 2002. The regressions include industry in 2002*year fixed effects with the standard errors clustered at the industry in 2002 level.

Panel A: Time series



Panel B: Annual treatment effects



Panel C: Synthetic control

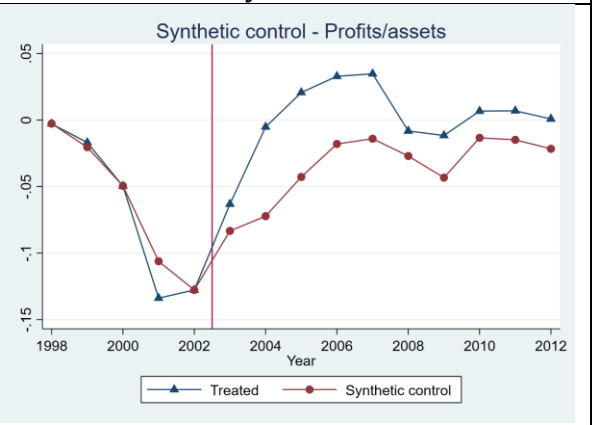


FIGURE 6. PROFITS/ASSETS, 1998-2012

Note: We only include firms with at least 5 regular members on the board in 2002. Treatment status, i.e. if the firm is listed on the Stockholm Stock Exchange, is determined based on the status in 2002. The regression in Panel B include industry in 2002*year fixed effects with the standard errors clustered at the industry in 2002 level. The synthetic control group in Panel C consists of a weighted average of industries to match the level of return on assets (ROA) for listed firms in 1998, 2000 and 2002.

TABLES

Table 1—Summary Statistics, 1998-2012

	Mean	Median	SD	Min	Max	Count
Panel A: All firms financial statements						
Female board share	.1404427	0	.2018218	0	1	163953
Operating profits	21.33559	.153	349.7982	-1331.731	16455	163576
Total assets	402.2665	9.991	4423.191	.065	204426	163603
Profits/assets	-.0061049	.0298155	.2938932	-1.812992	.6003702	163301
Total revenue	283.4309	9.972	3219.345	0	132879	163646
No. on board	5.627755	5	2.736571	1	63	164432
Labor cost/assets	.5963454	.3934298	.6430314	.0011105	3.367133	118242
Labor cost	82.5143	6.16	909.2228	.011	36253.39	118305
No. employed	188.9427	7	2874.25	0	279641	158337
EBITDA	33.80559	.406	466.0118	-526.594	21192	162240
Average board age	51.51972	51.875	7.16873	19	97	164432
Born Stockholm	.1776222	0	.257769	0	1	164432
Non-EU immigrant	.0215081	0	.0885894	0	1	164432
EU immigrant	.1055174	0	.2397314	0	1	164432
Observations	165306					
Panel B: Only groups' financial statements						
Female board share	.1269623	0	.177745	0	1	34381
Operating profits	100.1256	2.4895	769.9309	-1356	16556	34456
Total assets	1709.469	82.009	9829.374	.303	206656	34461
Profits/assets	.0172641	.0425472	.2148286	-1.336231	.5084573	34401
Total revenue	1174.042	85.571	7052.383	0	133150	34479
No. on board	6.139292	6	2.798906	1	48	34460
Labor cost/assets	.4533434	.3167387	.4750934	.0014631	2.56626	30432
Labor cost	277.5177	25.111	1803.191	.24	37064.9	30460
No. employed	729.8293	42	6134.185	0	279641	33874
EBITDA	155.7236	5.216	1031.768	-536.408	21425	34219
Average board age	52.41547	52.71429	6.162107	24	84	34460
Born Stockholm	.1901888	.0909091	.251273	0	1	34460
Non-EU immigrant	.0316427	0	.1034999	0	1	34460
EU immigrant	.1061684	0	.222512	0	1	34460
Observations	34681					

NOTE: ACCOUNTING VARIABLES EXPRESSED IN MILLIONS OF SWEDISH KRONA. WE ONLY INCLUDE FIRMS WITH AT LEAST 5 REGULAR MEMBERS ON THE BOARD IN 2002.

Table 2—Intentional Effect of the Threat of a Quota Law on Board Composition

	(1)	(2)	(3)	(4)	(5)
Outcome	Basic	Company and time fixed effects	Compositional bias test	Linear trends	Collapsed
Share Female	0.0865***	0.0851***	0.0836***	0.0420***	0.0872***
	(0.00501)	(0.00528)	(0.00474)	(0.00608)	(0.0102)
Industry trends	No	No	Yes	No	No
Standard errors	Clustered at industry	Clustered at industry	Clustered at industry	Clustered at industry	Newey-West

The table presents difference-in-difference estimates where firms listed on the Stockholm Stock Exchange are treated and the non-listed companies are used as controls. The outcome variable is the share of female board members on the corporate board, The standard errors are clustered at the industry level (57 clusters), errors in Columns 1-4. Column 5 presents Newey-West standard errors. Column 4 include a separate linear time trend for listed and non-listed firms. *p<0.10, **p<0.05, ***p<0.01. The number of observations is 163,953 in Columns 1-4. In Column 5, the number of observations is 15.

Table 3—Unintentional Effects of the Threat of a Quota Law on Firm Governance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Born Stockholm	Non-EU immigrant	EU immigrant	Average board age	CEO background (1998-2000)	Log(No. boards sitting on)	Director turnover	CEO turnover
					Panel A: Effect on all board members			
Estimate	-0.0232**	0.0196**	0.0177	-0.915**	-0.0185	0.0582**	-0.0402***	-0.0215**
	(0.00991)	(0.00746)	(0.0139)	(0.373)	(0.0141)	(0.0283)	(0.00566)	(0.00934)
					Panel B: Effect on female board members			
Estimate	-0.0596*	-0.00697	0.0252	-1.433***	-0.0185	-0.00890	-0.0458***	-0.0287
	(0.0355)	(0.00890)	(0.0156)	(0.511)	(0.0386)	(0.0442)	(0.0170)	(0.114)
					Panel C: Effect on male board members			
Estimate	-0.0204	0.0172**	0.0146	-0.147	-0.00512	0.0953***	-0.0347***	-0.0161*
	(0.0144)	(0.00752)	(0.0108)	(0.402)	(0.0140)	(0.0306)	(0.00589)	(0.00931)
Period	1998-2012	1998-2012	1998-2012	1998-2012	2001-2012	1998-2012	1998-2012	1998-2012
Sample	Firms	Firms	Firms	Firms	Firms	Firms	Directors	Directors

The table presents difference-in-difference estimates where firms listed on the Stockholm Stock Exchange are treated and the non-listed companies are used as controls. Standard errors clustered at the industry level (57 clusters). All specifications include a full set of industry*year fixed effects. *p<0.10, **p<0.05, ***p<0.01. The number of observations in Panel A is 164,432 in columns 1-4, 128,281 in column 5, 159,215 in column 6, 1,086,707 in column 7 and 105,797 in column 8. The number of observations in Panel B is 72,539 in columns 1-4, 42,685 in column 5, 69,554 in column 6, 154,012 in column 7 and 8,027 in column 8. The number of observations in Panel C is 162,136 in columns 1-4, 125,030 in column 5, 157,091 in column 6, 885,114 in column 7 and 94,836 in column 8.

Table 4—Board Turnover Heterogeneity

	(1)	(2)	(3)	(4)	(5)	(6)
	Turnover	Turnover	Turnover	Turnover	Turnover	Turnover
			Panel A: Effect on board members			
Estimate	-0.0350***	-0.0458***	-0.0827***	-0.0264	-0.0262*	-0.0431***
	(0.0123)	(0.00555)	(0.0249)	(0.0196)	(0.0133)	(0.00553)
			Panel B: Effect on CEOs			
Estimate	-0.00447	-0.0315***	0.0210	0.0276	0.00634	-0.0304***
	(0.0162)	(0.0102)	(0.107)	(0.240)	(0.0188)	(0.0106)
Period	1998-2012	1998-2012	1998-2012	1998-2012	1998-2012	1998-2012
Av. ROA 1998-2002	Negative	Positive	Negative	Positive	Negative	Positive
Board members	All	All	Only female	Only female	Only male	Only male
Av. ROA 1998-2002	Negative	Positive	Negative	Positive	Negative	Positive

The table presents difference-in-difference estimates where firms listed on the Stockholm Stock Exchange are treated and the non-listed companies are used as controls. Standard errors clustered at the industry level (57 clusters). All specifications include a full set of industry*year fixed effects.
*p<0.10, **p<0.05, ***p<0.01. The number of observations in Panel A is 364,727 in column 1, 721,980 in column 2, 48,688 in column 3, 105,324 in column 4, 297,231 in column 5 and 587,883 in column 6. The number of observations in Panel B is 34,331 in column 1, 71,466 in column 2, 2,676 in column 3, 5,351 in column 4, 30,339 in column 5 and 64,497 in column 6.

Table 5—Effect of the Threat of a Quota Law on ROA

	(1)	(2)	(3)	(4)	(5)
Outcome	Basic	Company and time fixed effects	Compositional bias test	Linear trends	Collapsed
			Panel A: Effect on firm performance: All firms		
Profits/assets	0.0552***	0.0595***	0.0523***	0.0694***	0.0569***
	(0.0172)	(0.0169)	(0.0159)	(0.0213)	(0.0123)
			Panel B: Effect on firm performance: Groups only		
Profits/assets	0.0466***	0.0512***	0.0408***	0.0511***	0.0471***
	(0.0144)	(0.0146)	(0.0144)	(0.0180)	(0.0124)
Industry trends	No	No	Yes	No	No
Standard errors	Clustered at industry	Clustered at industry	Clustered at industry	Clustered at industry	Newey-West

The table presents difference-in-difference estimates where firms listed on the Stockholm Stock Exchange are treated and the non-listed companies are used as controls. Standard errors clustered at the industry level (57 clusters) in Columns 1-4. Column 5 presents Newey-West standard errors. All specifications include a full set of industry*year fixed effects. *p<0.10, **p<0.05, ***p<0.01. The number of observations is 163,301 in panel A and 34,401in panel B. In Column 5, the number of observations is 15 in both panels.

Table 6—Additional Outcomes

	(1) EBITDA/assets	(2) Total revenue/assets	(3) Labor cost/assets	(4) Operating profits/employee	(5) Value added/employee	(6) Log(No. employed)	(7) Log(No. on board)
Estimate	0.0379***	0.0184	-0.0369**	29.11	73.91	0.0434	0.219***
	(0.0142)	(0.0326)	(0.0172)	(120.0)	(101.9)	(0.0546)	(0.0183)

The table presents difference-in-difference estimates where firms listed on the Stockholm Stock Exchange are treated and the non-listed companies are used as controls. Standard errors clustered at the industry level (57 clusters). All specifications include a full set of industry*year fixed effects.. $^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$. The number of observations is 161,973 in column 1, 163,370 in column 2, 118,242 in column 3, 121,321 in column 4, 118,067 in column 5, 121,552 in column 6 and 164,432 in column 7.

APPENDIX

Table A1—Synthetic Control Difference Estimates

	(1) Difference: female	(2) Difference: profits/assets
Post-2002	0.0841	0.0414
Constant	-0.00174	-0.00490
Synthetic control difference	Yes	Yes
N	15	15

The table presents the difference between the listed firms and the synthetic control industries after the threat of board gender quotas was presented in 2002. The constant shows the difference in the pre-threat period.

Table A2—Remove Restrictions

	(1) Non-active used	(2) Board>2	(3) All board sizes	(4) 2001 as base	(5) 2 lags in NW
			Panel A: Share of females		
Estimate	0.0816***	0.0951***	0.105***	0.0795***	0.0872***
	(0.00477)	(0.00521)	(0.00536)	(0.00462)	(0.0113)
			Panel B: Operating profits/assets		
Estimate	0.0523***	0.0685***	0.0907***	0.0311**	0.0569***
	(0.0164)	(0.0174)	(0.0207)	(0.0141)	(0.0115)
Standard errors	Clustered at industry	Clustered at industry	Clustered at industry	Clustered at industry	Newey-West

The table presents difference-in-difference estimates where firms listed on the Stockholm Stock Exchange are treated and the non-listed companies are used as controls. Standard errors are clustered at the industry level (57 clusters). All specifications except Column 5 include a full set of industry*year fixed effects.
*p<0.10, **p<0.05, ***p<0.01. The number of observations in Panel A is 170,844 in column 1, 500,155 in column 2, 2,479,415 in column 3, 159,826 in column 4 and 15 in column 5. The number of observations in Panel B is 169,509 in column 1, 498,310 in column 2, 2,482,713 in column 3, 159,576 in column 4 and 15 in column 5.

Table A3—Add Restrictions

	(1) At least 5 employees	(2) At least 10 employees	(3) At least 20 employees	(4) At least 500k SEK in share capital	(5) At least 1000k SEK in share capital
			Panel A: Share of females		
Estimate	0.0791***	0.0764***	0.0723***	0.0749***	0.0709***
	(0.00602)	(0.00649)	(0.00688)	(0.00498)	(0.00503)
			Panel B: Operating profits/assets		
Estimate	0.0539***	0.0522***	0.0443***	0.0507***	0.0459**
	(0.0164)	(0.0169)	(0.0151)	(0.0161)	(0.0186)

The table presents difference-in-difference estimates where firms listed on the Stockholm Stock Exchange are treated and the non-listed companies are used as controls. Standard errors are clustered at the industry level (57 clusters). All specifications include a full set of industry*year fixed effects. $^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$. The number of observations in Panel A is 94,130 in column 1, 72,334 in column 2, 52,295 in column 3, 70,834 in column 4 and 55,380 in column 5. The number of observations in Panel B is 93,957 in column 1, 72,217 in column 2, 52,214 in column 3, 70,722 in column 4 and 55,281 in column 5.

Table A4— Winsorizing at Different Levels; Outcome is Profits/Assets

	(1) 1 percent	(2) 2 percent	(3) 0.5 percent	(4) No winsorizing
Estimate	0.0523***	0.0492***	0.0566***	0.0389
	(0.0159)	(0.0152)	(0.0165)	(0.0377)

The table presents difference-in-difference estimates where firms listed on the Stockholm Stock Exchange are treated and the non-listed companies are used as controls. Standard errors are clustered at the industry level (57 clusters). All specifications include a full set of industry*year fixed effects. $^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$. The number of observations is 163,301.

Table A5, Panel A—Leaving One Industry Out

Profits/assets	0.0523***	0.0523***	0.0523***	0.0524***	0.0508***	0.0524***	0.0508***	0.0528***	0.0522***	0.0521***
	(0.0159)	(0.0159)	(0.0159)	(0.0160)	(0.0156)	(0.0160)	(0.0155)	(0.0162)	(0.0159)	(0.0159)
Industry code	01	02	05	10	100	13	14	15	16	17
N	161597	162612	163211	163204	153270	163247	162993	161551	163280	162936

Table A5, Panel B— Leaving One Industry Out

Profits/assets	0.0523***	0.0522***	0.0523***	0.0523***	0.0524***	0.0523***	0.0510***	0.0520***	0.0521***	0.0526***
	(0.0159)	(0.0159)	(0.0160)	(0.0161)	(0.0163)	(0.0159)	(0.0158)	(0.0159)	(0.0159)	(0.0161)
Industry code	18	19	20	21	22	23	24	25	26	27
N	163151	163209	161780	162602	159307	163229	161964	162551	162618	162872

Table A5, Panel C— Leaving One Industry Out

Profits/assets	0.0528***	0.0513***	0.0513***	0.0538***	0.0485***	0.0500***	0.0522***	0.0521***	0.0526***	0.0523***
	(0.0162)	(0.0159)	(0.0158)	(0.0165)	(0.0146)	(0.0154)	(0.0159)	(0.0160)	(0.0161)	(0.0160)
Industry code	28	29	30	31	32	33	34	35	36	37
N	160810	160877	163097	162618	162852	162284	162704	162869	162346	163085

Table A5, Panel D— Leaving One Industry Out

Profits/assets	0.0527***	0.0523***	0.0528***	0.0522***	0.0522***	0.0531***	0.0523***	0.0521***	0.0525***	0.0523***
	(0.0161)	(0.0159)	(0.0164)	(0.0159)	(0.0169)	(0.0163)	(0.0159)	(0.0159)	(0.0163)	(0.0159)
Industry code	40	41	45	50	51	52	55	60	61	62
N	158113	163157	158826	161409	147843	158660	159729	158406	162683	163146

Table A5, Panel E— Leaving One Industry Out

Profits/assets	0.0526***	0.0499***	0.0580***	0.0522***	0.0575***	0.0482***	0.0509***	0.0447***	0.0521***	0.0723***
	(0.0161)	(0.0153)	(0.0181)	(0.0160)	(0.0170)	(0.0141)	(0.0157)	(0.0117)	(0.0167)	(0.0233)
Industry code	63	64	65	66	67	70	71	72	73	74
N	159021	162288	160585	163245	160374	145584	161827	154999	160652	131440

Table A5, Panel F— Leaving One Industry Out

Profits/assets	0.0523***	0.0513***	0.0522***	0.0522***	0.0523***	0.0521***	0.0523***
	(0.0159)	(0.0157)	(0.0159)	(0.0159)	(0.0159)	(0.0161)	(0.0160)
Industry code	75	80	85	90	91	92	93
N	163088	160480	160068	162379	161559	157603	162966

The table presents difference-in-difference estimates where firms listed on the Stockholm Stock Exchange are treated and the non-listed companies are used as controls. Standard errors are clustered at the industry level (57 clusters). All specifications include a full set of industry*year fixed effects. $^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$.

Table A6—Window Size and Alternative Female Measure

	(1) Share of females, only known ID	(2) Share of females, limited window	(3) Profits/assets, limited window
Estimate	0.0890***	0.0539***	0.0450***
	(0.00607)	(0.00516)	(0.0130)
Window	1998-2012	1998-2004	1998-2004
N	159215	85491	84520

The table presents difference-in-difference estimates where firms listed on the Stockholm Stock Exchange are treated and the non-listed companies are used as controls. Standard errors are clustered at the industry level (57 clusters). All specifications include a full set of industry*year fixed effects.
*p<0.10, **p<0.05, ***p<0.01

Table A7—Foreign Companies as Controls

	(1) Female board share	(2) Profits/assets	(3) Log(stock price)
Estimate	0.0526**	0.0876***	0.0194
	(0.0241)	(0.0283)	(0.0801)
Period	1998-2012	1998-2012	2002
Company FEs	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes
N	4359	4408	79287

The table presents difference-in-difference estimates where Swedish firms listed on the Stockholm Stock Exchange are treated and foreign companies listed on the Stockholm Stock Exchange are used as controls. Standard errors are clustered at the firm level. All specifications include a full set of firm and time period fixed effects. * p <0.10, ** p <0.05, *** p <0.01

Table A8, Panel A—Window Size and Stock Market Prices around June 17 2002

	(1) Log(stock price)	(2) Log(stock price)	(3) Log(stock price)	(4) Log(stock price)	(5) Log(stock price)	(6) Log(stock price)	(7) Log(stock price)
Estimate	-0.00572	0.00121	0.0170	0.0140	0.0190	0.0274	0.0417
	(0.0123)	(0.0128)	(0.0136)	(0.0173)	(0.0214)	(0.0264)	(0.0313)
Days in window	6	14	28	42	56	84	112
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1566	2480	5602	8675	11658	17109	22923

Table A8, Panel B—Window Size and Stock Market Prices around October 22 2002

	(1) Log(stock price)	(2) Log(stock price)	(3) Log(stock price)	(4) Log(stock price)	(5) Log(stock price)	(6) Log(stock price)	(7) Log(stock price)
Estimate	0.0715	0.0811	0.0726	0.0583	0.0542	0.0460	0.0370
	(0.0629)	(0.0556)	(0.0525)	(0.0479)	(0.0462)	(0.0416)	(0.0411)
Days in window	6	14	28	42	56	84	112
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1535	2748	5811	8894	12015	18272	24613

The table presents difference-in-difference estimates where Swedish firms listed on the Stockholm Stock Exchange are treated and foreign companies listed on the Stockholm Stock Exchange are used as controls. Panel A provide estimates with different window sizes around the initial announcement date of the 17th of June 2002, while panel B provide estimates with different window sizes around the 22th of October 2002, when the threat was made more concrete. Standard errors are clustered at the firm level. All specifications include a full set of firm fixed effects. * p <0.10, ** p <0.05, *** p <0.01

Table A9— Heterogeneity in Effect on Share of Women on the Board

	(1) Female board share	(2) Female board share	(3) Female board share	(4) Female board share	(5) Female board share	(6) Female board share	(7) Female board share	(8) Female board share
Estimate	0.0832***	0.0837***	0.0768***	0.0390	0.0764***	0.0862***	0.0780***	0.0891***
	(0.00810)	(0.00739)	(0.00550)	(0.0383)	(0.0124)	(0.00374)	(0.00909)	(0.00439)
Period	1998-2012	1998-2012	1998-2012	1998-2012	1998-2012	1998-2012	1998-2012	1998-2012
Restriction	Only negative pre ROA	Only positive pre ROA	Higher turnover than median	Lower turnover than median	B2C companies	B2B companies	Women on board in 2002	No women on board in 2002
N	54309	109644	82099	81854	70764	83009	74090	89863

The table presents difference-in-difference estimates where firms listed on the Stockholm Stock Exchange are treated and the non-listed companies are used as controls. Standard errors are clustered at the industry level (57 clusters). All specifications include a full set of industry*year fixed effects. * p <0.10, ** p <0.05, *** p <0.01

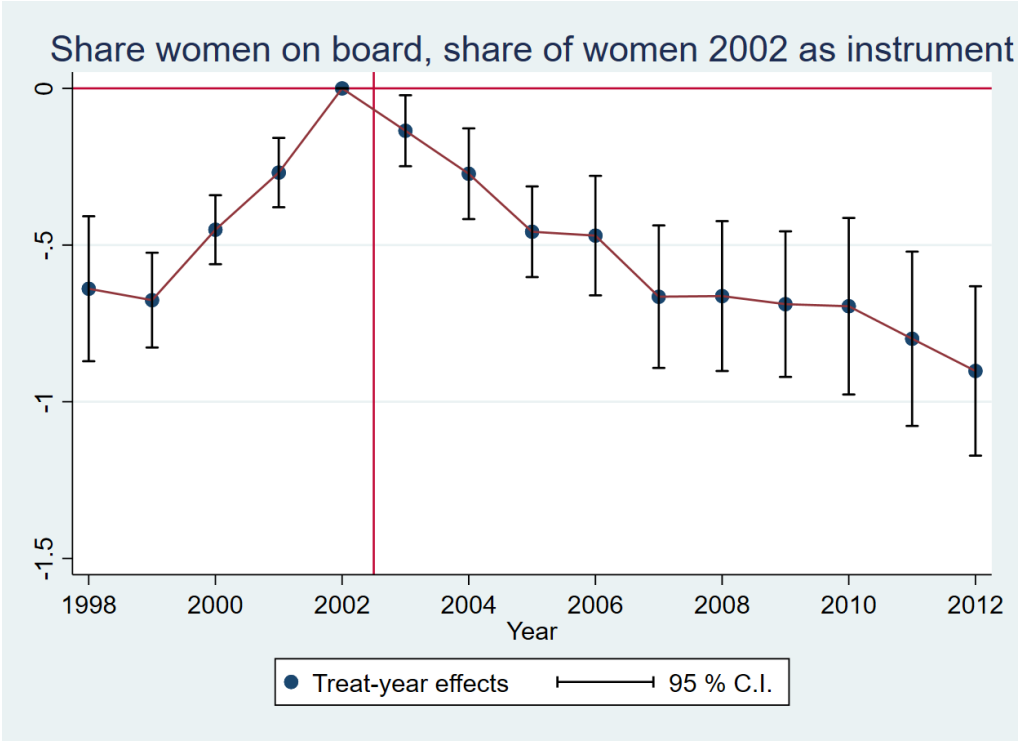


FIGURE A1. AHERN AND DITTMAR (2002) REPLICATION OF FIRST STAGE, ANNUAL EFFECTS

Note: The figure shows a dynamic representation of the first stage when the share of women on the board in 2002 is used as the instrument for future and past share of women. Only listed Swedish firms on the Stockholm Stock Exchange in 2002 with at least 5 regular members on the board in 2002 are included in the model.

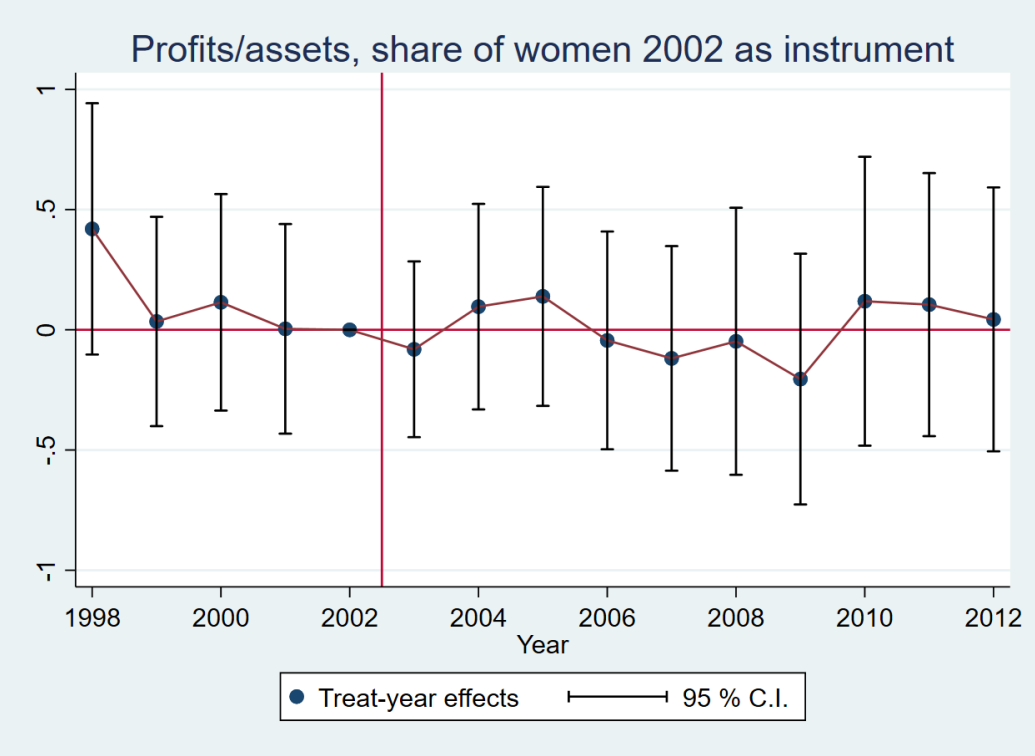


FIGURE A2. AHERN AND DITTMAR (2002) REPLICATION OF REDUCED FORM USING ROA, ANNUAL EFFECTS

Note: The figure shows a dynamic representation of the reduced form estimates when the share of women on the board in 2002 is used as the instrument for future and past share of women. Only listed Swedish firms on the Stockholm Stock Exchange in 2002 with at least 5 regular members on the board in 2002 are included in the model.

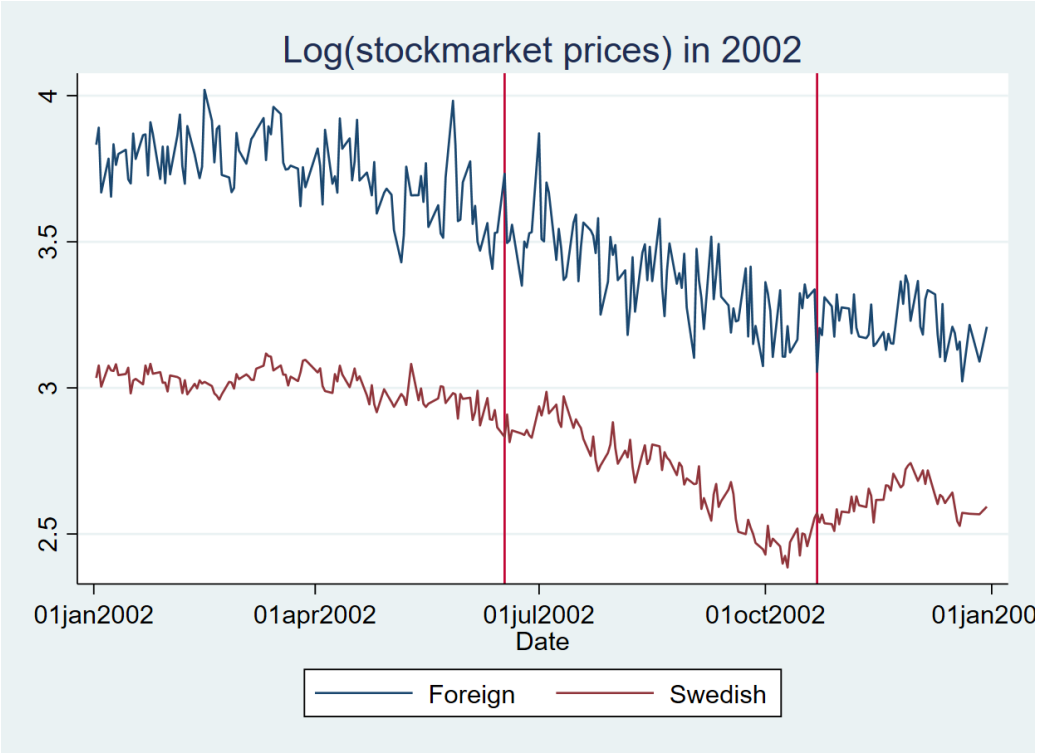


FIGURE A3. STOCK MARKET PRICES IN 2002

Note: The figure shows the average of log(stockmarket prices) over 2002 for Swedish and foreign firms listed on the Stockholm Stock Exchange. The first red line lies on the 17th of June, which is the day of the first announcement by the deputy prime minister. The second red line lies on the 22nd of October, when the threat was made more concrete.

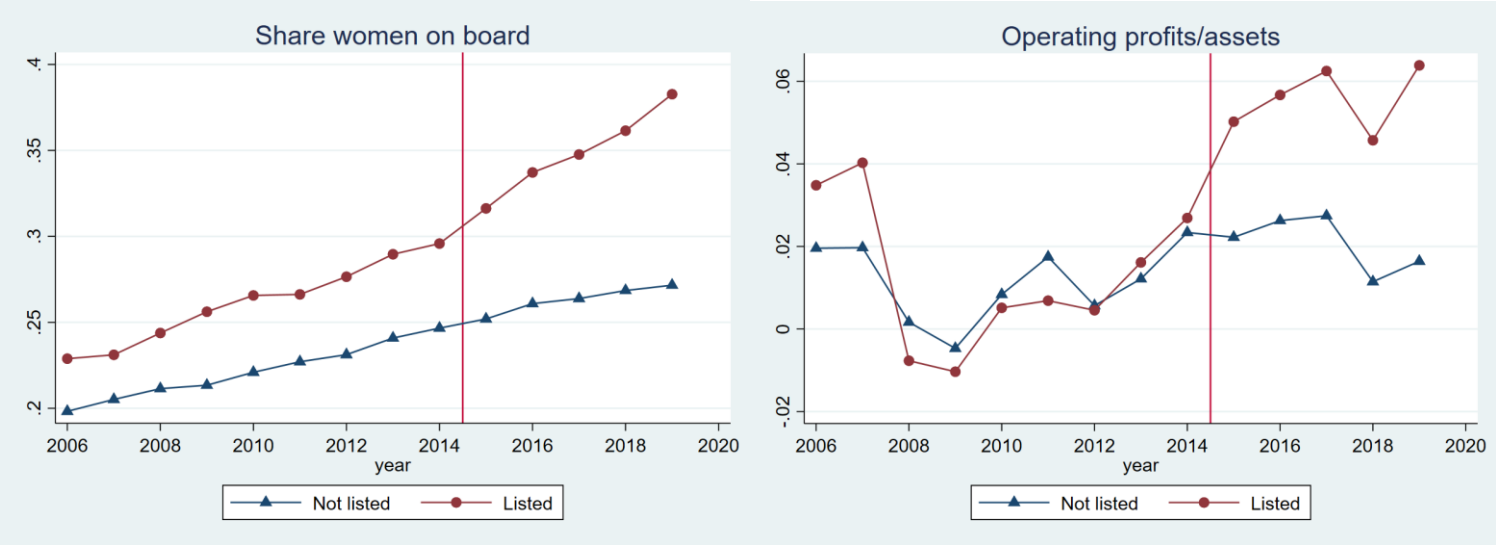


FIGURE A4. THE EFFECT OF THE RETURN OF THE SOCIAL DEMOCRATS

Note: The figure shows the average share of women and ROA for listed and non-listed firms. The Social Democrats return to power in September 2014, while a right-wing coalition have been in power since 2006, with no intention of introducing a quota on boards in Sweden.