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

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THIS VERSION: 2018-02-12

Board room quota laws have recently received an increasing amount of attention. However, laws are typically anticipated and firms can react before the effective date. This paper provides new results on female board participation and firm performance in Sweden due to a credible threat of a quota law enacted by the Swedish deputy prime minister. The threat caused a substantial and rapid increase in the share of female board members in firms listed on the Stockholm stock exchange. This increase was accompanied by an increase in different measures of firm performance in the same years, which were related to higher sales and lower labor costs. The results highlight that anticipatory effects of a law could be detrimental to the analysis.

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I. Introduction

Policymakers in Europe have recently begun to focus on the relative underrepresentation of women on corporate boards, and numerous countries are considering the implementation of gender quotas. The first quota law, adopted in Norway in December 2005, required public limited liability companies (ASA) 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 quotas (Eckbo, Nygaard and Thorburn 2016). In Sweden, the policy debate has been intense as well. In 2002, Swedish Deputy Prime Minister Margareta Winberg, supported by Prime Minister Göran Persson, threatened to impose a mandatory law if considerable improvements in board room representation were not achieved in the listed companies within two years. Specifically, the listed companies were asked to increase their share of female directors to 25 %, an increase of approximately 20 percentage points.

Our main contribution is that we estimate a pure *anticipation effect* of a gender quota law and that the effects are large in magnitude. We use a difference-in-difference-design where listed companies, treatment group, saw a direct threat of a quota law where comparable non-listed firms, the control group, did not. Interestingly, the threat increased firm performance, a result which differs from other quasi-experimental studies evaluating gender quotas laws. Our main results shows specifically that the threat caused a substantial and rapid increase in the female board share in firms listed on the Stockholm stock exchange; the short

term effect size was approximately 5-10 percentage points or an approximately 100-200 percent increase. Interestingly, this increase was accompanied by an increase in the measures of firm performance in the exact same years. On average, profits over assets (ROA) increased by approximately 2-4 percent among listed firms after the threat, relative to the change in ROA in unlisted firms in the same time period. However, increased female representation on boards did not lead to the higher recruitment of females as CEOs, either in the short or the long run. In fact, our results indicate the opposite, suggesting that certain female CEOs were recruited to the boards and not always replaced by female CEOs. One way to explain the magnitude of the estimated firm's performance effects is to acknowledge that we are estimating an anticipation effect of a law. The net effect may still be small.

However, the results still seem hard to rationalize from a classic economics perspective, where agents are profit maximizing and have perfect information. Then it is reasonable to conjecture that a quota law or a credible threat of a law should reduce profits, in particular in the corporate sector where the competition pressure is high which should limit suboptimal board composition.

Recently, Besley et al. (2017) study quotas in party politics. They show theoretically, in a setting where competence of new candidates of a party ballot is positively related to success for the party but at the same time is threatening the power of the leader, that the leader trade off party success against survival of power. Thus a gender quota could lead to better candidates as mediocre men are replaced by both better men and women. Besley et al. (2017) find strong empirical support for the model and as pointed out explicitly, this model "could be applied, for example to private organizations such as corporate boards". Our interpretation is that the insider male club that until 2002 had the almost exclusive power may not want to be challenged by more competent and independent

directors. An exogenous credible threat of a gender quota law changes the equilibrium and higher competence should increase firm performance.

Moreover, as has been noted frequently in the literature, we also acknowledge that if the male directors have a distaste for women and/or a taste for homogeneity then diversity and independence could increase firm performance (e.g. Adams & Ferreira, 2009, Smith 2014 and Ferreira 2015).[1] A credible threat could push the board to be more gender neutral and firms could perform better. In the models proposing potentially positive effects of a quota there must be a supply of competent women or of women with different characteristics than men to recruit.[2] The diversity could be manifested in less permanent characteristics such as level of formal training and experience.[3] But gender differences could also be more stable. Related to decision making are differences in preferences and attitudes such as differences in risk attitudes,[4] attitudes towards competition and negations.[5]

Thus, theoretically we cannot determine which of the effects that prevail and we would ideally like to randomize gender quotas on corporate boards in order to evaluate the causal effects. The Norwegian law of quotas in 2005 has been used as such an exogenous shock (Ahern and Dittmar, 2012 and Matsa and Miller,

[1] 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 the bias, analogous to the findings in Beaman (2009).

[2] Women have been more highly educated than men for many years in most OECD countries. Interestingly, Bertrand et al. (2014) find that "the average observable qualifications of the women appointed to the boards of publicly limited companies significantly improved after the reform". Also wage gender gap fell accordingly. 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 (2017) for evidence on Swedish data..

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

[4] For example, it has been suggested that the Lehman brothers' crisis never would have occurred if it would have been Lehman Sisters (Adams and Ragunathan, 2014). However, this argument misses out on the selection into boards as pointed out and documented by Adams and Funk (2012) where they find that the selected female directors are less risk adverse, invalidating the Lehman's sisters "hypothesis" with respect to risk aversion differences.

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

2013). [6] Ahern and Dittmar (2012) use the pre-reform share of women on the board of listed firms and the fact that early adopters are not affected by the law to the same extent. Using this strategy, they find a large negative effect on firms' Tobin's Q ratio. However, as discussed by Ferreira (2015), early adopters are unlikely to be similar in trends to their counterpart. When we replicate their first stage in our setting, the parallel trend assumption is violated due to mean reversion. This finding is illustrated in Figure A1 in the Appendix. Turning to the most similar study, Matsa and Miller (2013) also use a difference-in-difference design, in which a sample non-listed limited liability firms act as the control group to the listed firms. Again, the effect found in Matsa and Miller (2013) on firm performance is negative.[7] Conversely, Nygaard (2011) finds a positive effect of quotas on firm performance when evaluating the Norwegian reform. However, the robustness of the results from these papers has been questioned (Ferreira 2015; Eckbo, Nygaard and Thorburn 2016). When critically assessing the empirical design used in previous papers, Eckbo, Nygaard and Thorburn (2016) find a zero effect of the quota reform on firm performance measures. One major point made in Eckbo, Nygaard and Thorburn (2016) is that firms could anticipate the law after the political debate changed in February 2002. Anticipatory effects are a direct threat to validity in a difference-in-difference setting if they are not properly accounted for (Angrist and Pischke 2009). For example if a law was anticipated, but not acknowledged by the econometrician, the estimated effect may well have wrong sign. One way to understand the bias is to picture a quota law with heterogeneous treatment effects. Some firms will see an increase of firm

[6] 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 in the end resulted in a law being passed in late 2005. The law in turn gave the affected companies two years to comply.

[7] The authors pick the treatment period as post-2006. As Figure 1 in Bertrand et al. (2014) demonstrates, the increase in the share of females on boards began back in 2002 and continued until 2008. Thus, their first stage does not seem to exhibit parallel trends prior to their treatment period.

performance due to more women and some will be hurt. Under the reasonable assumption that the firms with positive treatment effects are more likely to start the process of recruiting female directors, then we would estimate a positive firm anticipation effect and a negative effect of the effective law. The net of the law, the anticipation and the effective law effects, may be zero, positive or negative. Thus, a credible difference-in-difference strategy uses the first date when the law was anticipated as treatment date.

Other related literature on gender quotas and firm performance are Ferrari et al (2016) that finds no overall impact on firm performance but a positive effect on stock market value in Italy, Comi et al (2016) find mixed results across Belgium, France, Italy, and Spain. For studies on gender quotas and labor market and internal organization see Bertrand et al. (2014) using the Norwegian quotas and Ferreira et al 2017 using the quotas in France.

Given the large degree of disagreement regarding the effects of the Norwegian reform, we propose another testing ground to provide evidence of the effects of gender quotas. We use a credible threat by the Swedish deputy prime minister as the exogenous variation. We try carefully and addressing the methodological concerns discussed in Ferreira (2015) and Eckbo, Nygaard and Thorburn (2016), by for example making use of standard test for parallel trends and linear treatment specific trends.

The remainder of the paper is outlined as follows: In section 2, we document the background of the threat. In section 3, we describe the methodology, data, and sampling. In section 4, we provide the results, and in section 5 we conclude.

II. Background

Sweden has a long history of male-dominated board rooms in listed companies. In the 1990s, the female share was steady at just below 5 %. In 2003, the female

share began to increase, tripling within 3 years. Anecdotally, the 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, took office in 1998 as a minister of gender equality. In our study, identification is linked to the timing of the threat, and therefore it is crucial to describe the threats carried out over time. Figure 1 shows the number of printed articles in newspapers 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, quota, women and board.[8] In 1999, as depicted in Figure 1, Winberg began to discuss, although rarely, the role of board room 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 the board rooms were absent from the debate, as depicted in Figure 1.

[Insert Figure 1 Here]

In 2002, the temperature rose. During the year, the number of printed articles mentioning Winberg´s name in combination with quotas, women and boards exploded. In July, in the leading business daily *Dagens Industri*, Winberg indicated 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

[8] 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".

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 history, the representation of women on the boards of listed companies began to rise consistently.

The dotted line in Figure 1 denotes 2002. In this study, we set 2002 as the baseline year since we observe data annually. 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 will be the first year of treatment.[9]

The time series of the articles ends in 2003, the year when 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 their boards by 2008; otherwise, a fine would be paid every time a new board was elected. The

[9] In the Appendix, Table A4 depicts the results if 2001 is set as the baseline year. The results do not differ substantially.

investigator argued that other limited liability companies also should not be subject to the law.[10] 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 steady 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 speaking, 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, Data and Sampling

A. Methodology

A naïve regression population function could be written as follows:

$$Y_{ct} = a + \beta Share\_female_{ct} + e_{ct} \quad (1)$$

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

where Y_{ct} is firm *c´s* performance outcome such as operating profits/assets (ROA) at time *t*. It is clear that unobserved firm characteristics can determine both the variable of interest, the share of female directors on a firm´s board, and the outcome. Thus, to estimate β with no bias, we would need an instrument for the variable of interest. In addition to being strong, an instrument must be: (i) "as good as" randomly assigned and (ii) excludable, i.e., the only channel through which it operates is the endogenous variable (exclusion restriction). The "as good as" randomly assigned condition ensures a causal interpretation of the reduced form. In our setting, we could under (i) estimate the causal effect of the threat of a quota law. In a DID-setting (i) translates to parallel trends of the outcome across treatment and control groups. Thus, the reduced form in our setting becomes

$$(2) \qquad Y_{clt} = \alpha + \gamma Listed_l + \lambda\, Post_t + \delta(Listed_l * Post_t) + \varepsilon_{clt}$$

where *Post* is a dummy taking the value one for the period after 2002 and otherwise taking the value zero. *Listed* is equivalently a dummy for listed firms in 2002. Under the assumption of parallel trends, δ, the parameter of the interaction, will measure the causal effect of the threat of a quota law on, for example, the share of female directors or the ROA. The subscript l=1,2 denotes treatment or control group.

If we also assume the exclusion restriction to hold, we could also write the first stage equation as the following:

$$(3)\ Share\_female_{clt} = b + \tau Listed_l + \phi\, Post_t + \xi(Listed_l * Post_t) + \omega_{clt},$$

and we could estimate the causal effect (a LATE) of increasing the share of women from 0 to 1 on firm performance by OLS with $\frac{\widehat{\delta}}{\widehat{\xi}}$.

In this paper, we suggest that it is unlikely to assume that the exclusion restriction would hold both in the setting of a law and in the setting of the threat of a law. First, imposing quotas could affect firms' recruitment procedure in numerous ways. Having to recruit women will most likely include using new expertise, networks and recruitment firms, which could have a direct effect on the outcome. Moreover, the threat of a law might signal future government interventions in general, which could influence firm actions. Further, the presence of more women on corporate boards might increase the size of the board; research suggests that board size may be important to performance through monitoring and advising (Jensen 1993; Yermack 1996). Lastly, having additional women on the board is correlated with other factors that have been found to be of importance for firm performance, such as director independence (see the survey in Adams, Hermalin and Weisbach 2010) and the size of the board. Thus, director independence could affect firm performance, and any outsider group, not just females, would affect independence and potentially firm performance. Consequently, we view equation (3) as an interesting reduced form and one potential channel. Thus, this paper focuses on estimating the causal effect of the threat of imposing gender quotas for listed firms, and hence, parallel trends will be the major identifying assumption.

Given the large amount of disagreement in the evaluations of the Norwegian reform, we provide a battery of specification tests in this paper. First, we address compositional bias by adding industry fixed effects and thus non-parametrically control for the industry- level specific factors.[11] An even more flexible specification could include firm specific effects instead of the dummy *Listed* and year fix effects instead of the dummy *Post*. However, in absence of compositional

[11] 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 potentially 2003, the first year of treatment is three years after the burst the dot-com bubble and one could worry that certain industries, for example IT or telecom, would drive our results. Fortunately, our results are robust when leaving out one industry at a time.

effects, this should not affect the coefficient of interest and untabulated results also confirm that.

Second, we acknowledge that the estimations of the standard errors are problematic in our study since 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 treatment only varies once at the control – treatment group level, this might not be conservative enough. Here, we follow the Pettersson-Lidbom and Thoursie (2013) application of the results in Donald and Lang (2007). The problem is that treatment only varies at one time as at the group level *l,* listed and non-listed and not on the firm, *c*, or industry level. The error term could contain both a firm error r_{clt} and a group - time error j_{lt} ; therefore, $\varepsilon_{clt} = r_{clt} + j_{lt}$. In the presence of a group time error, standard errors are biased; clustering on the firm or industry level will not help, and clustering on *l* cannot be done due to the low size of 2.

We address the clustering problem as discussed in Moulton (1986) by aggregation. Thus, we calculate the mean for every time period for the groups listed and non-listed and estimate equation (2) on the group level (listed and non-listed). Although this addresses the Moulton (1986) problem, the error could still be serial correlated. Taking the difference between the two groups, however, we represent one time series as:

$$\Delta Y_t = \gamma + \delta Post_t + \Delta\mu_t, \tag{4}$$

where $\Delta Y_t = Y_{listed,t} - Y_{non-listed,t}$, $\gamma = \gamma_{listed} - \gamma_{non-listed} and\ \Delta\mu_t = \mu_{listed,t} - \mu_{non-listed,t}$. With this transformation, the estimate of δ will be identical to an estimate from a fixed-effect model (where N=2 and T=15 when annual data). When estimating equation (4), we make the standard errors robust to heteroscedasticity and serial correlation by applying the Newey-West estimator.

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 independent variable $Post$. If the parallel trends assumption hold, the coefficient should come out both close to zero and statistically insignificant. We show these results graphically. Furthermore, we could add a linear trend to the specification, and if the parallel trend assumption holds true and there are no dynamic effects, then the effect should remain stable. 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. We could also match on the pre-trends according to the method of synthetic control, developed in Abadie et al (2010)

Importantly, there could be other major factors affecting listed companies differently than non-listed companies around 2002-2003. In any DID-setting with one policy change and two groups, and in particular with annual data, this is the major concern. In the end it is not testable. However, there are some sanity checks that could be made. Firstly, we have 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, Column (2) and (3). Our results are similar for this shorter period which makes it less likely that these two changes are drivers. [12]

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

B. Data

Our data consist of two data sets that have been merged. The first one is composed of all, except financial, limited liability firms' final accounts and key figures over the time period 1998-2012.[13] 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 time period 1998-2012.[14] 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 on the board based on these members. All data comes from the Swedish Companies Registration Office (but in two mergable data sets). The office keeps track of all companies and their CEOs and directors. The firm data are available for the universe of limited liability companies, excluding financial firms. The office keeps track of for example the financial statement items and number of employees. Each firm must by Swedish law file this information within 6 months after the end of a fiscal year.

[12] In the future we plan to use an alternative control group: I.e. listed firms that are registered abroad. Firms registered abroad follow the law of their country of register.. Moreover, this allows also for market based outcomes.

[13] 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 only (weakly) 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.

[14] 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.

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 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 are active in 2002. A non-active firm is a firm in which there is no intent to operate a normal business. Furthermore, we define treatment status based on whether a firm is listed or not in 2002. This means that we can use the number of firms as an indicator of compositional bias due to delisting.

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 for the firms to be comparable.

Furthermore, we only consider ordinary board members as part of the board, and thus, we exclude labor union representatives, deputy directors and the likes. Although, our results are not very sensitive when also including these.

While a number of other reasonable restrictions could be made, our main analysis will hinge on these restrictions. However, in the Appendix Table A3, we show results for other plausible restrictions, including restrictions on the share capital that differs across groups or public or private limited liability firms and number of employees.[15] These different restrictions are not driving the results.

Finally, we determine the gender of the board members through their personal identification number for all Swedish residents. Using personal numbers, we

[15] 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.

obtain exact gender information for 95.72 % of the data.[16] For non-Swedish residents, however, we rely on board members' first name only. We obtain our results by using the list of all names given to more than 10 born boys or girls 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) for the time period 1998-2012, along with information on the boards' gender composition.

Moreover, since a firm can belong to a group of firms, we focus our analysis on the parent firm if it belongs to a group. If the firm is not part of a group then we study this sole firm. The definition of a parent firm is one that controls other firms in the group (the subsidiaries). Policies affecting a parent company thus have spillover effects on other companies in the group. Since listed companies are commonly the parent of non-listed subsidiaries, including the subsidiaries would mean a violation of SUTVA (Rubin, 1980). Thus, we focus on the parent companies as the unit of observation if there exists a group and subsidiaries are not part of the main analysis. Since the parent company board is in charge of the subsidiaries, this poses no problem with respect to measuring the female director share, which is simply the share in the board of the parent. However, regarding firm performance measures such as operating profits/assets, we could either use the parent company financial statements or the group financial statements. Using the parent financial statements would generally underestimate the firm performance. However, DID estimation hinges on a parallel trends assumption,

[16] 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.

and thus we need not only this underestimation to be different across the groups but also to evolve differently over time across groups to cause a methodological problem. Therefore, using the financial statement of the parent company should not automatically pose a threat to internal validity. To verify this, we also use the financial variables from the group financial statement; our coefficient of interest is indeed unchanged. Lastly, we also redo the analysis using only parent firms that are part of a group, i.e. excluding also single firms (with no subsidiaries). Lastly, In the Appendix, Table A2, Column (6) also shows the results when all individual firms are treated as independent, whether they are parent firms or subsidiaries.

As is standard in the previous literature, we winsorize all financial variables at the 1 % and 99 % level. Thus, we cap all values above the 99th percentile and below the 1st percentile to the value at the 99th and 1st percentile, 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 change

The summary statistics for the listed and non-listed firms after the process of winsorizing are presented in Table 1. Panel A shows the statistics for all independent firms. That is parent firms or firms that belong to no group, i.e., firms that are independent with no subsidiaries. First, the share of female directors is approximately 14 % for the period. Second, one can note that the mean of operating profits/assets is negative for the period on average, although the median remains positive. Turning to Panel B, where we have instead used the group financial statement for the parent firms belonging to a group, we see no major differences, although both the balance sheets and the results are larger in absolute terms to some extent. Mostly, we observe approximately 170,000 observations, where one observation represents a parent firm or an independent firm for a given year.

[Insert Table 1 Here]

IV. Main Results

A. Graphical Evidence

We begin by inspecting the number of firms in the treatment group over time. Since we condition based on the firms being listed in 2002, it must follow that there are (weakly) fewer firms before and after 2002. Cleary, 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 in the listed group 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]

Turning to the share of female directors as an outcome, we begin by graphically inspecting the time series in Figure 3. Column 1 shows the share for the independent firms, and Column 2 shows the share for independent firms but for the matched sample where group financial statement have been used for the firms with subsidiaries. Since the match rate is high, the time series should be similar, which Figure 3 shows. Interestingly, in the years before the quota threat, we can 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. After the threat, there is an extraordinary increase for listed firms, whereas the non-listed firms

remain in the same approximate trend. After 2006, when the law rejected, parallel trends emerge once again. The first year´s reactions are the mildest, showing some dynamic effects before stabilizing around 2006. Panel B shows the estimates as annual treatment effects, as discussed by Angrist and Pischke (2009). The estimates suggest small and mostly 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 show small effects before the threat, there may be weak evidence of an increase in the share of female board members before the threat, i.e., testing whether the effect survives when including linear treatment and control groups trend will be of interest. However, the overall pattern is consistent with a causal interpretation of the effects. The effects size seems to be approximately 8 percentage points.

[Insert Figure 3 Here]

We now turn to our main firm performance measure, operating profits divided by total assets (ROA), as used in Matsa and Miller (2013).[17] Figure 4 of Panel A shows a rather similar downward trend 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 pedagogically shows the point of having a control group. Interestingly, listed parent companies have a negative ROA for the entire period, not only in the crisis following 2000. Clearly, negative ROA for such a long period can hardly resemble real firm performance. Thus, it is of interest to instead use the operating profits/assets from the group financial statement if the parent belongs to a group. Column 2 of Panel A shows that using the group financial

[17] 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.

statement instead of only the financial parent statement yields a more reliable measure of firm performance. 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, 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 reassuring that we do not see a pattern that the listed firms after the Lehman Brothers crisis are seeing some years of faster growth rates of profits/assets. Thus, the estimated effects for the threats in the period from 2003 and onwards are unlikely to merely be a convergence effect driven by the dot-com bubble in 2000. Profits increased approximately 2-4 percent of assets among listed firms after the threat, relative to the change in profits in unlisted firms in the same time period.

[Insert Figure 4 Here]

Moreover, there is an interesting correspondence between Figures 3 and 4. Both outcomes appear to be parallel before the threat. There is then a large reaction for the listed group until 2005-2006, both for the share of females and profits over assets, before stabilization occurs.

Lastly, to address concerns about linear trends in the reduced form regarding the share of female directors and concerns that the effect might be driven by an Ashenfelter's dip, we perform a robustness check using a synthetic control group approach. Following the advice in Abadie et al. (2010), we match the dependent variable in 1998, 2000 and 2002. Both graphs show a good correspondence before 2002 and a sharp divergence afterward. The effect sizes are 8 percentage points for the share of female directors and approximately 3 percentage points for

profits.[18] Thus, concerns about pre-trends or dips are not critical to our results. Notably, Figure 5 also suggests that our results are not driven by functional form assumptions.

[Insert Figure 5 Here]

B. Main Regression Result

In Table 2 we present our main results, beginning with estimating the model outlined in equation (2), in Column 1. In Panel A we show the results when the share of female directors is the outcome. The threat of quotas caused the share of females to increase by approximately 8 percentage points, an increase of approximately 150 %. Adding industry flexible time trends in column 2 does not alter the results, thereby strengthening the indication that attrition does not cause compositional bias. In column 3, linear trends are added. Thus, our identification strategy no longer hinges on parallel trend assumption; instead, if the trend differs, it differs linearly. Since Figure 3 indicates a slightly upward trend, it is not surprising that the estimate is changed. 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 late spring, then part of the "true" effect is controlled away when adding linear trends. Lastly, in Column 4 we present the results from estimating equation (4), i.e., using collapsed data and a time series of 15 observations to address the Moulton and serial correlation problem when estimating the standard errors. Although the standard errors double in size, the effect remains significant.

[18] 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. This leaves us with 57 time series, where one is the treatment group and the other 56 are the remaining companies in their respective industries. To this 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.

[Insert Table 2 Here]

Turning to firm performance and profits, we see in general that using the financial statements from the parent firm (Panel B) yields somewhat smaller estimates compared to using the group financial statements if the firms is the parent of a group (Panel C). However, in relation to the size of the standard errors, the effects are roughly the same. In summary, profits increased by approximately 2 – 5 percent of assets among listed firms after the threat relative to the change in profits in unlisted firms in the same time period.

Lastly, in Table 3, we restrict the sample by only using parent firms belonging to groups; this means using approximately 30,000 observations (groups) compared to approximately 170,000 in Table 2. In general, the results depicted in Table 2 remain.

[Insert Table 3 Here]

C. Additional Results

In Tables 4 and 5, we use the group's financials to construct other outcomes. We use our basic DID model, as presented in equation (2). In Column 1, Table 4, the basic estimate in which the outcome is operating profits over assets is re-tabulated. Since operating profits include depreciation and amortization, we also show the effect for the outcome EBITDA/assets in Column (2). Again, our estimate is a statistically significant EBITDA/assets increase of approximately 4 percent of assets among listed firms after the threat, relative to the change in profits in unlisted firms in the same time period. When considering only total revenue/assets, we again obtain a positive estimate, although less precisely estimated. Interestingly, labor costs/assets decrease by approximately 2 percent of assets among listed firms after the threat, relative to the change in profits in

unlisted firms in the same time period. Again, this finding 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 and labor costs to decrease. Two alternative outcomes, operating profits per employee and value added per employee, are presented in Columns (5) and (6). The results show the same sign as our other firm performance measure but are imprecisely estimated.

[Insert Table 4 Here]

[Insert Table 5 Here]

Turning to Table 5, Column (1), we confirm that the numerator of our major outcome, operating profits /assets, is positively and significantly affected by the threat. Thus, our effect is not driven by decreasing the denominator. Columns (2) and (3) show an increase in the number employed, although the figures are somewhat functionally specific because the effect becomes insignificant when using the logs instead of the levels. Columns (4)-(6) speak directly to our concern about using a gender quota law or a threat as an instrument with respect to the validity of the exclusion restriction. Column (4) shows that the female proportion of CEOs decreases by 2.5 percentage points. This result is consistent with female CEOs being recruited to corporate boards and not replaced solely by women. Columns (5) and (6) suggest that the board is also increasing in size. A back of the envelope calculation suggests that boards are expanded by one woman due to the quota threat. Thus, this finding illustrates clearly how the gender quota threat is affecting numerous potential channels that affect firm performance.

V. Conclusion

Gender quotas on corporate boards have recently received increased attention. The first quota law was adopted in Norway in December 2005. Other European countries have subsequently implemented quotas. Empirically, we know little about the effects of quotas in the board rooms on firm performance. This paper uses a credible threat of gender quotas aimed at listed firms. Since the law was never implemented we are likely to capture a pure anticipation effect of a law. 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 effect of the quota law was large, consistent with a credible threat. Interestingly, this increase was accompanied 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 increased by approximately 2-4 percent of assets among listed firms after the threat, relative to the change in profits in unlisted firms.

However, increased female representation on boards did not lead to more frequent recruitment of females as CEOs, either in the short or the long run. In fact, our results indicate the opposite, which suggests that some of the female CEOs were recruited to the boards and were not always replaced by female CEOs. Moreover, labor costs decreased and sales increased, although these figures were imprecisely estimated. Our results indicate that parallel trends are a reasonable assumption, and our result is highly robust.

Since our study differ with respect others as we estimate the anticipation effect of a law instead of the effect of a law, comparisons of estimates are problematic. It is not unlikely that the anticipation effect is different from an effect estimated based on firms waiting to hire female directors and therefore having to adapt

quickly to the law. The effect for early adaptors might well be different than for slow movers. Thus, our results are also consistent with a model where a quota law has a neutral or negative *net* effect on firm performance. But, if studying a setting were the law was put in place our results strongly suggest that it is of importance to account for anticipation effects in order to estimate an unbiased *net* effect of the law.

Another difference might be that the effects of gender quotas on firm performance are a nonlinear function of female representation. The threat increased female representation from approximately 5 to approximately 15 percent which was far from the level of 40 percent that was the intended goal in Norway.

In the future, we plan to collect additional information regarding how organizational structures are affected by more female directors, in line with the questions posed by Bertrand et al. (2014). For example, will there be more females positioned in middle and top management? Will male workers and managers utilize the generous parental leave system in Sweden to a larger extent?

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FIGURES

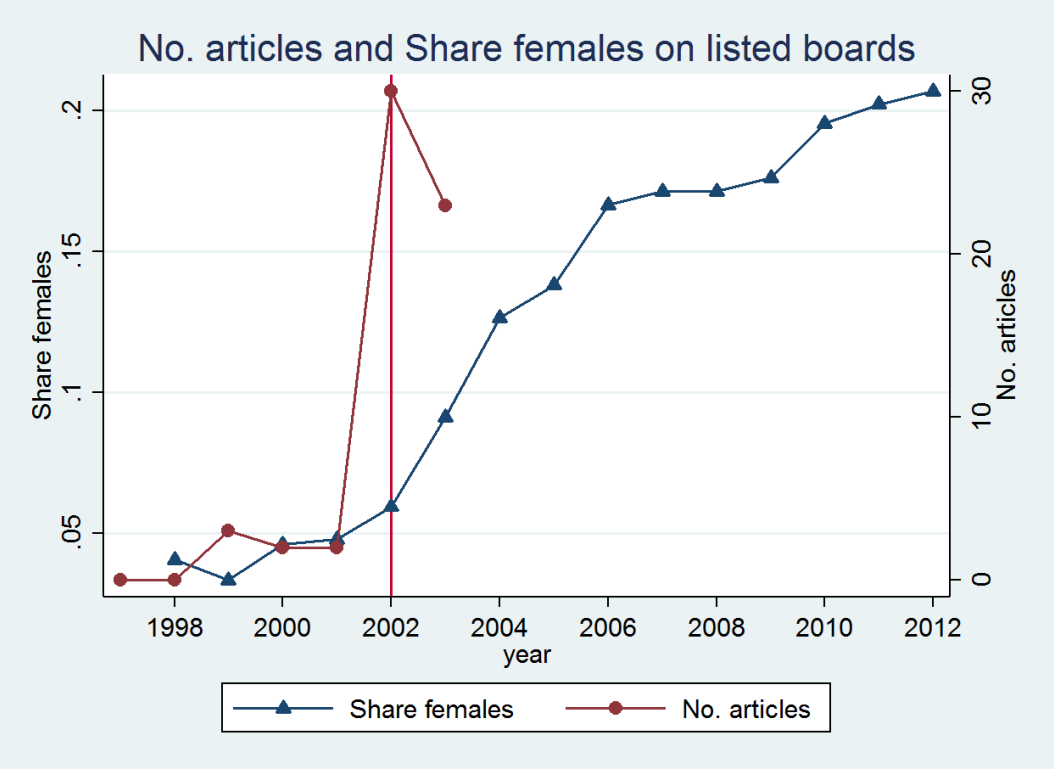


FIGURE 1. SHARE OF FEMALE REPRESENTATION ON THE BOARDS OF LISTED FIRMS AND ANNUAL NUMBER OF PRINTED ARTICLES IN SWEDISH PRESS FROM 1998-2003

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

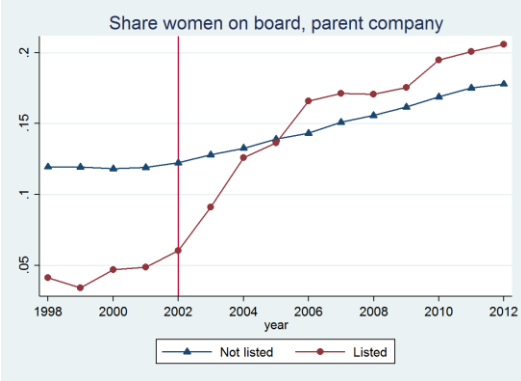
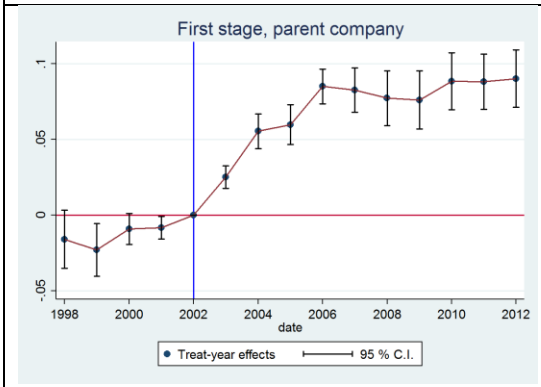
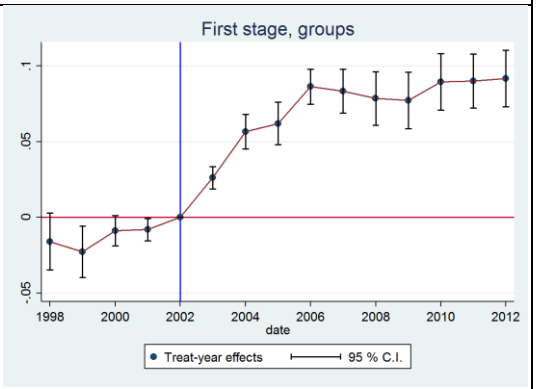
Independent firms (Parent or firms with no subsidiaries)	Independent firms but the sample where parent firms are matched with the group financial report
Panel A: Time series	
	
Panel B: Annual treatment effects	
	

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

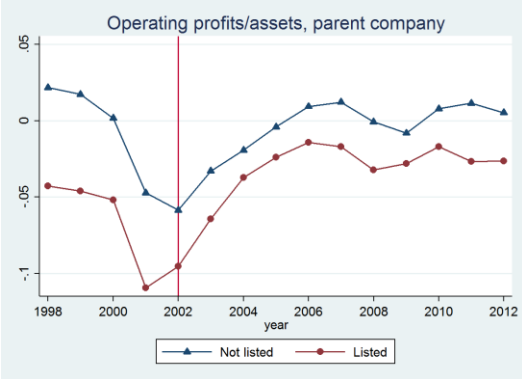
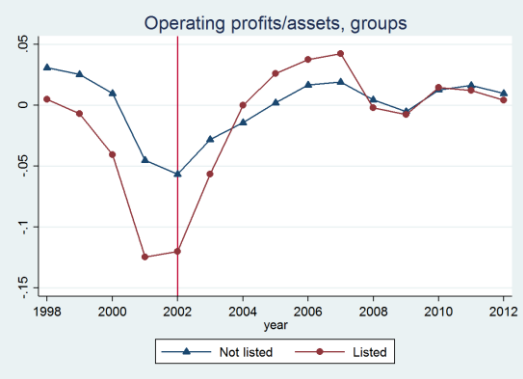
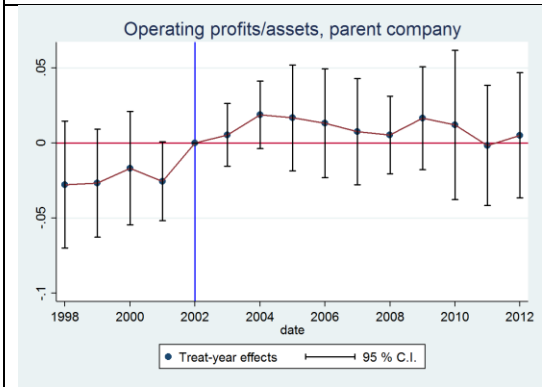
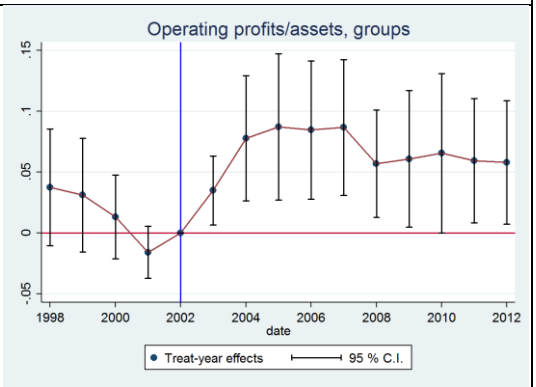
Independent firms (Parent or firms with no subsidiaries)	Independent firms but the sample where parent firms are matched with the group financial report
Panel A: Time series	
	
Panel B: Annual treatment effects	
	

FIGURE 4. PROFITS/ASSETS, 1998-2012

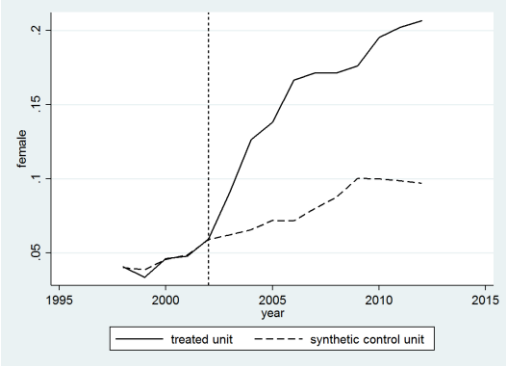
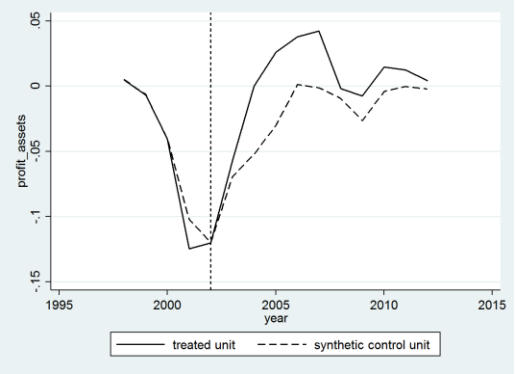
Female share of directors Independent firms but the sample where parent firms are matched with the group financial report	**Operating profits over assets** Independent firms but the sample where parent firms are matched with the group financial report
	

FIGURE 5. SYNTHETIC CONTROL (ABADIE ET AL. 2010), GROUP FINANCIAL STATEMENTS USED

TABLES

Table 1—Summary Statistics, 1998-2012

	Mean	p50	sd	Min	Max	Count
Panel A: Parent firm financial statements						
Female board share	.1390776	0	.200584	0	1	168643
Operating profits	5490755	67000	6.89e+07	-7.60e+08	3.38e+09	168534
Total assets	3.00e+08	9546000	2.57e+09	66000	1.09e+11	168563
Profits/assets	-.0094904	.0177392	.2813509	-1.743985	.5924171	168290
Total revenue	1.03e+08	6516000	4.57e+08	0	1.59e+10	168587
No. on board	5.636735	5	2.728714	1	63	169130
Labor cost/assets	.5686929	.3574007	.6485608	.0006615	3.393548	114408
Labor cost	2.71e+07	4923000	1.06e+08	9000	3.93e+09	114445
R&D costs/assets	-.0147597	0	.0576047	-.4634188	0	22609
Selling costs/assets	-.1233812	-.0051427	.2473822	-1.446863	0	22588
Performance pay board	23.39417	0	277.3693	0	12000	165156
No. employed	51.52356	5	392.4991	0	26379	162678
EBITDA	9970088	224000	9.17e+07	-5.74e+08	4.76e+09	166351
Average board age	51.56429	52	7.128098	19	97	169130
Observations	170019					
Panel B: Group financial statements						
Female board share	.1390241	0	.2005795	0	1	169079
Operating profits	2.42e+07	172000	3.78e+08	-1.09e+09	1.76e+10	168681
Total assets	4.33e+08	1.10e+07	4.38e+09	66000	2.01e+11	168706
Profits/assets	-.0036505	.0308635	.2888744	-1.775194	.5973451	168405
Total revenue	3.08e+08	1.09e+07	3.26e+09	0	1.29e+11	168752
No. on board	5.635953	5	2.728101	1	63	169566
Labor cost/assets	.4628314	.2175555	.6405655	2.03e-06	3.25526	122963
Labor cost	1.36e+07	1765000	4.13e+07	549	3.12e+08	123029
R&D costs/assets	-.0183407	0	.0653342	-.5386208	0	22869
Selling costs/assets	-.161734	-.0557467	.2627354	-1.490032	0	22847
Performance pay board	71333.71	0	1461417	0	6.60e+07	165570
No. employed	201.6156	7	2881.23	0	279641	163390
EBITDA	3.81e+07	450000	5.09e+08	-4.86e+08	2.36e+10	167317
Average board age	51.56599	52	7.131224	19	97	169566
Observations	170460					

Table 2—Effect of the Threat of a Quota Law

Outcome	(1) Basic	(2) Compositional bias test	(3) Linear Trends	(4) Collapsed
		Panel A: Effect on share of female directors		
Share Female	0.0838***	0.0816***	0.0409***	0.0840***
	(0.00505)	(0.00460)	(0.00761)	(0.00959)
		Panel B: Effect on firm performance. Parent company financial statement used		
Profits /assets	0.0260***	0.0227**	0.0273***	0.0292***
	(0.00777)	(0.00919)	(0.00660)	(0.00529)
		Panel C: Effect on firm performance. Group financial statement used		
Profits /assets	0.0516***	0.0488***	0.0658***	0.0540***
	(0.0158)	(0.0151)	(0.0186)	(0.0124)
Industry trends	No	Yes	No	No
Standard errors	Clustered at industry	Clustered at industry	Clustered at industry	Newey-West

The standard errors are clustered at the industry level (57 clusters) errors in Column 1-4. Column 5 presents Newey-West standard errors.*p< 0.10, **p< 0.05, ***p< 0.01. Number of observations are 168,643; 168,290; and 168,405 in panel A, B and C, respectively. In Column 4, the number of observations is always 15 across all panels.

Table 3—Effect of the Threat of a Quota Law, Only Groups

	(1)	(2)	(3)	(4)
Outcome	Basic	Compositional bias test	Linear Trends	Collapsed
	Panel A: Effect on share of female directors, only groups			
Share Female	0.0869***	0.0781***	0.0534***	0.0833***
	(0.00736)	(0.00679)	(0.00924)	(0.00913)
	Panel B: Effect on firm performance. Group financial statement used, only groups			
Profits /assets	0.0344**	0.0303*	0.0386**	0.0354**
	(0.0165)	(0.0171)	(0.0169)	(0.0149)
Industry trends	No	Yes	No	No
Standard errors	Clustered at industry	Clustered at industry	Clustered at industry	Newey-West

The standard errors are clustered at the industry level (57 clusters) errors in Column 1-4. Column 4 presents Newey-West standard errors.*p< 0.10, **p< 0.05, ***p< 0.01. Number of observations are 31,270 in panel A and 31,325 in panel B. In Column 4, the number of observations is always 15 across both panels.

Table 4—Other Outcomes of the Effect

	(1) Profits/assets	(2) EBITDA/assets	(3) Total revenue/assets	(4) Labor cost/assets	(5) Operating profits/employee	(6) Value added/employee
Estimate	0.0516***	0.0375**	0.0329	-0.0225*	167.6	199.8
	(0.0158)	(0.0152)	(0.0379)	(0.0134)	(332.7)	(331.4)

Standard errors in parentheses, Clustered at industry *p< 0.10, **p< 0.05, ***p< 0.01

Table 5—Additional Outcomes

	(1) Operating profits	(2) No. employed	(3) log(No. employed)	(4) Female as CEO	(5) No. on board	(6) log(No. on board)	(7) Average board age
Estimate	444628782.6***	1004.2**	0.0586	-0.0253***	0.722***	0.213***	-0.630
	(85660024.3)	(386.0)	(0.0908)	(0.00757)	(0.110)	(0.0230)	(0.496)

Standard errors in parentheses, Clustered at industry $^{*}p< 0.10$, $^{**}p< 0.05$, $^{***}p< 0.01$

APPENDIX

Table A1—Synthetic Control Difference Estimates

	(1) Difference female	(2) Difference profits/assets
Post 2002	0.0818	0.0313
Constant	-0.000857	-0.00473
Synthetic control difference	Yes	Yes
N	15	15

Table A2—Remove Restrictions

	(1) Non-active used	(2) Board>2	(3) All board sizes	(4) 2001 as base	(5) 2 lags in NW	(6) All individual firms
			Panel A: Share females			
Estimate	0.0832***	0.0976***	0.109***	0.0795***	0.0852***	0.0838***
	(0.00490)	(0.00520)	(0.00555)	(0.00437)	(0.0107)	(0.00505)
			Panel B: Operating profits/assets			
Estimate	0.0511***	0.0661***	0.0856***	0.0337**	0.0540***	0.0260***
	(0.0160)	(0.0156)	(0.0167)	(0.0143)	(0.0115)	(0.00777)
Standard errors	Clustered at industry	Clustered at industry	Clustered at industry	Clustered at industry	Newey-West	Clustered at industry

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

Table A3—Add Restrictions

	(1) At least 5 employees	(2) At least 10 employees	(3) At least 20 employees	(4) Atleast 500k SEK in share capital	(5) Atleast 1000k SEK in share capital
			Panel A: Share female		
Estimate	0.0821***	0.0797***	0.0755***	0.0843***	0.0841***
	(0.00561)	(0.00568)	(0.00645)	(0.00498)	(0.00541)
			Panel B: Operating profits/assets		
Estimate	0.0577***	0.0576***	0.0487***	0.0516***	0.0490***
	(0.0162)	(0.0166)	(0.0174)	(0.0160)	(0.0170)

Standard errors in parentheses, Clustered at industry $^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$

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

	(1) 1 percent	(2) 2 percent	(3) 0.5 percent	(4) No winsorizing
Estimate	0.0516***	0.0474***	0.0565***	0.0412
	(0.0158)	(0.0155)	(0.0156)	(0.0394)

Standard errors in parentheses, Clustered at industry $^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$

Table A5, Panel A—Leaving One Industry Out

Profits/assets	0.0514***	0.0509***	0.0517***	0.0516***	0.0498***	0.0517***	0.0504***	0.0521***	0.0515***	0.0514***
	(0.0158)	(0.0156)	(0.0158)	(0.0158)	(0.0154)	(0.0159)	(0.0155)	(0.0160)	(0.0158)	(0.0158)
Industry code	01	02	05	10	100	13	14	15	16	17
N	166774	167666	168321	168308	152283	168351	168110	166631	168384	168033

Table A5, Panel B—Leave One Industry Out

Profits/assets	0.0516***	0.0515***	0.0515***	0.0521***	0.0513***	0.0516***	0.0504***	0.0512***	0.0514***	0.0519***
	(0.0158)	(0.0158)	(0.0159)	(0.0161)	(0.0160)	(0.0158)	(0.0156)	(0.0158)	(0.0158)	(0.0160)
Industry code	18	19	20	21	22	23	24	25	26	27
N	168250	168317	166852	167648	164311	168347	167026	167585	167729	167934

Table A5, Panel C—Leave One Industry Out

Profits/assets	0.0516***	0.0520***	0.0499***	0.0532***	0.0488***	0.0515***	0.0515***	0.0515***	0.0522***	0.0516***
	(0.0160)	(0.0161)	(0.0156)	(0.0164)	(0.0149)	(0.0159)	(0.0158)	(0.0159)	(0.0160)	(0.0158)
Industry code	28	29	30	31	32	33	34	35	36	37
N	166041	165737	168169	167644	167884	167383	167751	167957	167504	168198

Table A5, Panel D—Leave One Industry Out

Profits/assets	0.0515***	0.0516***	0.0516***	0.0514***	0.0479***	0.0521***	0.0511***	0.0513***	0.0518***	0.0516***
	(0.0159)	(0.0158)	(0.0161)	(0.0158)	(0.0155)	(0.0162)	(0.0157)	(0.0157)	(0.0162)	(0.0158)
Industry code	40	41	45	50	51	52	55	60	61	62
N	163137	168294	163857	166589	152471	163709	165027	163543	167732	168232

Table A5, Panel E—Leave One Industry Out

Profits/assets	0.0522***	0.0498***	0.0558***	0.0515***	0.0554***	0.0508***	0.0513***	0.0443***	0.0528***	0.0712***
	(0.0161)	(0.0152)	(0.0176)	(0.0158)	(0.0167)	(0.0164)	(0.0159)	(0.0125)	(0.0166)	(0.0180)
Industry code	63	64	65	66	67	70	71	72	73	74
N	164001	167469	165716	168343	165579	151045	167032	160375	166061	137007

Table A5, Panel F—Leave One Industry Out

Treated	0.0516***	0.0491***	0.0509***	0.0514***	0.0515***	0.0514***	0.0517***
	(0.0158)	(0.0151)	(0.0157)	(0.0158)	(0.0158)	(0.0159)	(0.0158)
Industry code	75	80	85	90	91	92	93
N	168231	165678	165258	167531	166568	163004	168063

Standard errors in parentheses, Clustered at industry. $^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$

Table A6—Window Size and Alternative Female Measure

	(1) Share females, only known ID	(2) Share females	(3) Profits/assets
Treated	0.0904***	0.0520***	0.0456***
	(0.00548)	(0.00652)	(0.0116)
Window	1998-2012	1998-2004	1998-2004
N	164311	88231	87239

Standard errors in parentheses. Note: The standard errors are clustered at the industry level (57 clusters).

*p< 0.10, **p< 0.05, ***p< 0.01

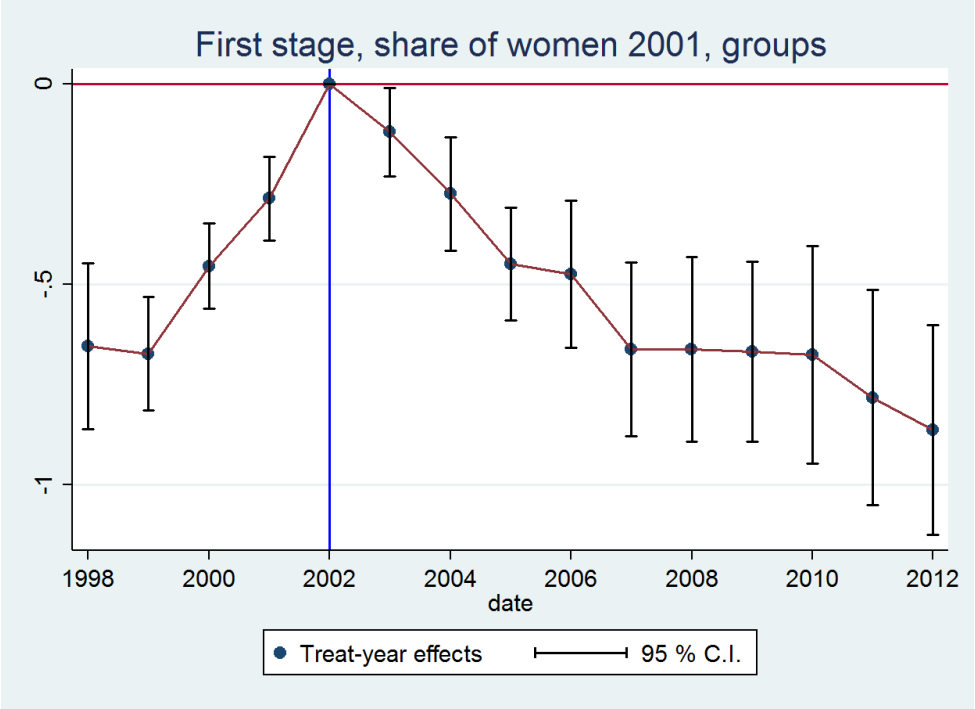


FIGURE A1. ALTERNATIVE FIRST STAGE ANNUAL EFFECTS