A Female Style in Corporate Leadership? Evidence from Quotas*

David A. Matsa and Amalia R. Miller

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Abstract

In 2006, Norway imposed a quota requiring that the boards of directors of public limited companies be composed of at least 40 percent female members. Governments across Europe have since adopted or proposed similar rules. This paper examines the impact of the Norwegian quota on the management style of affected firms by comparing them to other Scandinavian companies, public and private, that were unaffected by the rule. Based on differences-in-differences and triple-difference models, we find that firms affected by the quotas undertook fewer workforce reductions than comparison firms, increasing relative labor costs and employment levels and reducing short-term profits. There is no evidence of preexisting trends, and the effects are strongest among firms that had no female board members before the quota was introduced. The boards appear to be affecting corporate strategy in part by selecting likeminded executives. The results are consistent with changes in board composition affecting corporate governance and strategy, and with prior research suggesting that female managers may be more stakeholder or long-term oriented than their male counterparts.

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*Matsa: Kellogg School of Management, Northwestern University, 2001 Sheridan Road, Evanston, IL 60208. Email: dmatsa@kellogg.northwestern.edu; Miller: Department of Economics, University of Virginia, 237 McCormick Road, Charlottesville, VA 22904. Email: armiller@virginia.edu. We thank Susan Athey, Astrid Kunze, Brian Melzer, Paola Sapienza, Carmit Segal, and seminar participants at Georgetown, Harvard, MIT, Northwestern and the Western Finance Association Meetings for helpful comments. Mads Johnsen provided excellent research assistance.

When you make a decision, whatever that decision is whether it's about an acquisition, whether it's about anything, [being a woman] just makes you more sensitive to everyone that's involved, everyone that's involved; their health care, their retirement, all their benefits.

–Female corporate board member
(Broome, Conley, and Krawiec 2010, p.43; repetition in original)

1. INTRODUCTION

Despite substantial female progress in recent years, business leadership remains largely male dominated. In the past fifteen years, women's share of corporate officer positions in Fortune 500 firms has grown from 8.7 to 15.7 percent; board seats from 9.6 to 15.2 percent; and CEO positions from 0.2 percent to 3.0 percent (Catalyst 2010). This trend is likely to continue as young women increasingly invest in business school training.[1] While the pace of progress has been extensively documented and examined, little is known of how corporate strategy would be different if women were better represented at the top of the corporate ladder.

A growing literature in economics and finance suggests that individual managers bring their own personal styles to managing their firms (e.g., Bertrand and Shoar 2003; Graham and Narsimhan 2004; Malmendier, Tate, and Yan 2010), yet if and how women manage firms differently than men remains unclear. A large qualitative literature has emerged documenting gender differences in self-reported attitudes and leadership styles of business executives (e.g., Rosener 1990). But the few empirical studies in economics that look at gender in management focus on firms' bottom line – profits or value – without much analysis of *what* within the firms women may be doing differently. The studies, many undertaken by Catalyst (e.g., Catalyst 2007), a nonprofit organization committed to promoting women in business, typically find

[1] In 1970, only 3.6 percent of master's degrees and 8.7 percent of bachelor's degrees in U.S. business schools were conferred on women. Today, women earn more than 44 percent of master's degrees in business and management, accounting for 37 percent of MBAs and 48 percent of specialized master's degrees. Among undergraduate business majors, women first outnumbered men in 2002 (AACSB 2010).

positive cross-sectional correlations between accounting performance and the presence of female directors (see also, for example, Carter, Simkins, and Simpson 2003; Erhardt, Werbel, and Shrader 2003; Farrell and Hersch 2005).

But companies that have chosen female leadership may not provide a useful model of what to expect if women rose elsewhere, as these companies may also be unusual in other ways. Selection in the matching of female managers and directors to firms can bias simple correlations of female leadership and firm outcomes if, for example, more profitable firms are more likely to attract the highest quality women, or if the pool of female talent is concentrated in specific industries. In fact, Adams and Ferreira (2009) show that the correlation reverses when they include firm fixed effects. "Of course," explains Ferreira (2010, p.17), "the fixed effects results also do not establish the direction of causality. Past and expected future performance may influence firms' decisions to select female directors." For example, firms may be more likely to promote women following negative performance shocks (Ryan and Haslam 2005).

To address this selection problem, we study an exogenous policy shock that increased female corporate representation in Norway. The law, which took effect in 2006, required all publicly listed companies to increase female representation on their boards of directors to 40 percent within 2 years. This quota was externally imposed on firms, not chosen by managers or investors, and increased female representation by over 20 percentage points at the typical affected firm. We examine this increase as a natural experiment.

Comparing financial data for publicly listed firms in Norway to a matched sample of unlisted firms in Norway and listed and unlisted firms elsewhere in Scandinavia, we find that the quota is associated with a relative decline in corporate profitability. After the quota was implemented, the ratio of operating profits to assets among affected firms decreased by about 4

percentage points, relative to firms that were unaffected by the law. This reduction is consistent with other evidence suggesting that gender diversity on corporate boards is associated with lower firm value for well-governed firms (Adams and Ferreira 2009) and lower offer prices in mergers and acquisitions (Levi, Li, and Zhang 2008).

But why did short run profits decline? We decompose the change in profits to explore *what* the firms were doing differently after the additional women joined the boards. The results suggest that the decrease in profits can be explained entirely by increased labor costs, caused by a relative increase in employment and not the average wage. There is no indication of an increase in other (nonlabor) costs, suggesting the change in labor costs is not simply attributable to the firms negotiating poorly with all of their suppliers after the quota takes effect. On average, employment increased by about a third, relative to firms that were unaffected by the quota. These relative gains largely reflect reductions in worker layoffs among firms affected by the quota.

Women on corporate boards are apparently leading their firms to retain additional rank and file workers when other firms are laying them off. Because low-wage workers are particularly vulnerable to unemployment risk, this pattern may reflect a greater concern on the part of female board members for the well-being of workers at the lower end of the wage distribution. The new board members may also question the long-run profitability of these layoffs. Experiments find that women are generally more altruistic (Andreoni and Vesterlund 2001) and long-term oriented (Silverman 2003) than men, and survey evidence documents corresponding sex differences in corporate directors' preferences and values (Adams and Funk 2009). Our results suggest that these differences translate into meaningful differences in corporate strategy and may be aspects of a distinctively female leadership style.

Based on currently available data, it is impossible to confirm definitively whether or not the female leadership style creates value for the shareholders of these firms. One possible interpretation of the results is that the new leadership style adopted after the quota was simply less effective, perhaps because the new female directors were young and inexperienced. To the contrary, however, we find that overall average director age and executive experience was stable over the period. Where the data are available to test, our results are also robust to controlling for director age and board turnover. Furthermore, the lack of any increase in nonlabor costs suggests this is not simply a matter of poor negotiating skills. Nevertheless, some of these tests are indirect and open to interpretation. In theory, stock market event studies of the policy's announcement could be used to indicate whether the market expected the quota to create value for shareholders, but such studies of the Norway quota find opposite results depending on which announcement date is examined (Ahern and Dittmar 2011; Nygaard 2011).[2] Stock price movements also only reveal the market's *perception* of female directors, who were relatively rare before the quota was announced, and thus may reflect distorted perceptions of women's ability to lead companies (Wolfers 2006). Furthermore, even if the market correctly anticipated that women would reduce layoffs, Edmans (2011) finds that the stock market undervalues the intangible benefits from such strategies. Ultimately, time will tell whether the increase in female leadership creates value for these firms in the long run.

We also examine *how* directors affect corporate strategy. CEO selection appears to provide a mechanism for the effect. We find the quota has a greater effect on corporate strategy

[2] A gender quota on Norwegian boards was first debated in the Norwegian legislature as early as October 1999 (Nygaard 2011). Ahern and Dittmar (2011) examine stock returns in February 2002 when the Minister of Trade and Industry discussed the policy in an interview with Norway's largest newspaper. Nygaard (2011) examines stock returns in December 2005 when it was announced that the quota would actually be mandated and under the threat of forced liquidation. Both events are described as a surprise, but the first is associated with negative average stock returns and the second with positive.

when the board has the opportunity to select a new CEO just after female board representation increases. Still, the quota also has some effect on strategy, albeit smaller and less statistically significant, for boards that did not have the opportunity to select a new CEO, suggesting that board members advising management directly plays a role as well.

These results are robust to a variety of specifications and samples. They are not limited to government-owned firms, and there is no evidence of pre-existing trends. The results are robust to using different matching algorithms and are not driven entirely by outlier observations. A falsification test also finds no evidence of differential patterns among listed Norwegian firms during the previous global recession. As a further test, we divide the sample of firms affected by the quota based on whether they had any women on their board before the quota was adopted. We find the strongest effects among firms that were required to add the most women to comply with the law. Among these firms, return on assets decreased by about 4.5 percentage points as employee costs grew by 25 percent, relative to similar firms that also had no female board members but were not affected by the law.

This paper is part of a growing literature on the Norwegian gender quota, including recent papers by Ahern and Dittmar (2011) and Nygaard (2011). Ours is the first of these papers to exploit a triple-difference identification strategy based on public/private and cross-country differences, in addition to exploiting variation based on firms' distance from compliance before the quota was adopted. Among these papers, ours was also the first to find a significant effect of the quota on accounting variables, such as return on assets, and on employment. Most importantly, our paper is unique in its use of the gender quota to analyze women's leadership styles – to examine *how* women lead firms differently than men.

Our findings contribute to the corporate governance literature on boards of directors. A central question in this literature – how board composition affects firm value and corporate decisions – is inherently complicated by the endogeneity of the director-selection process (for surveys, see Hermalin and Weisbach 2003, and Adams, Hermalin and Weisbach 2010). Depending on the nature of a firm's activities or its CEO, for example, different board structures and compositions may be optimal. While correlations between board member characteristics and corporate decisions are interesting, as long as companies select board members endogenously, these correlations may not reflect the causal impact of changing board composition. To address this issue, we exploit the gender law as a natural experiment, and find that boards of directors indeed play a significant direction-setting role in their organizations. With respect specifically to gender diversity on corporate boards, the closest paper is Adams and Ferreira (2009), who use fixed effects and instrumental variables to link female directors to increased monitoring but potentially lower firm value.

Our results shed light on the mechanisms by which corporate directors can affect their firms' business strategy – an area in which Adams, Hermalin, and Weisbach's (2010, p.80) recent survey of the role of boards of directors concludes "much work remains to be done." Our findings that new female directors appear to affect strategy in part by selecting likeminded executives to run the business and in part by influencing existing managers are consistent with surveys, such as Demb and Neubauer (1992), that describe boards playing an active role in developing corporate strategy and objectives.

The paper also contributes to the economic literature on gender and organizations. Many studies have been concerned with existing barriers to women's professional advancement, including discrimination (e.g., Goldin and Rouse 2000), family obligations that reduce human

capital investments and slow career progress (e.g., Bertrand, Goldin and Katz 2010; Miller 2011; Kunze 2011), and the lack of mentors (e.g., Athey, Avery and Zemsky 2000).[3] In contrast, this paper is primarily concerned with understanding what happens when women overcome these barriers and occupy positions at the top of the corporate hierarchy. Rather than limiting our analysis to the effects of the quota on profits, we explore how management style changes when female leadership is exogenously increased.

Although short-term profits are indeed lower following the introduction of the quota, the mechanism by which profits decrease suggests some caution before attributing these effects to gender differences in skill or ability. Instead, it may be that women bring different values or preferences to corporate leadership, or added patience to invest for the long term. This interpretation resonates with the experimental finding in Niederle, Segal, and Vesterlund (2008) that gender-based affirmative action attracts highly skilled women who otherwise would have chosen not to participate. Within the literature on gender quotas, our approach is closest in spirit to that of Chattopadhyay and Duflo (2004) and Pande (2003), who study how public good provision changes under the leadership of female and minority politicians.[4]

2. THEORETICAL FRAMEWORK

As illustrated by Bertrand and Schoar (2003), corporate executives have their own personal "styles" when making investment, financing, and other strategic decisions. Their

[3] See Blau and Kahn (2000) for more on the sources of the gender pay gap for all workers. For a discussion of gender imbalances among top positions, see Bertrand and Hallock (2001) and Glass Ceiling Commission (1995).

[4] Much of the affirmative action literature, in contrast, focuses on documenting the direct effects of programs or legislation on the employment or college admissions of the protected group (see, for example, Holzer and Neumark 2000 and references therein). Studies that consider private sector business activity tend to focus on performance, assessing the potential efficiency costs from binding regulations (e.g., Marion's 2009 study of race and sex preferences in government contracting) rather than asking if women bring a new leadership style to organizations.

results suggest that managers' personalities and values – separate from firm, industry, or market factors – can shape the companies they manage, imprinting the leaders' personal marks on the companies they manage. Although Bertrand and Schoar are careful to point out that their framework cannot estimate definitively causal effects of specific managers or personality characteristics on their firms, their results invite the question of how increasing female representation at the top of the corporate world would affect business strategy.

A large literature on gender points to fundamental differences between men and women (see Marini 1990; Croson and Gneezy 2009). For example, studies suggest that women tend to be more altruistic (Andreoni and Vesterlund 2001), to be more risk averse (Eckel and Grossman 2008; Sapienza, Zingales and Maestripieri 2009), and to shy away from competition (Niederle and Vesterlund 2007). In the political sphere, women tend to be more left-wing (Edlund and Pande 2002) and to favor spending on public health interventions, especially when directed at helping children (Miller 2008). In experiments, women are less inclined than men to lie in order to secure monetary payoffs (Dreber and Johannesson 2008) and more likely to take actions that confer large but delayed rewards (Silverman 2003). In surveys, such gender differences in core values are robust across cultures (Schwartz and Rubel 2005).

Until recently, however, much less has been known about gender differences among business executives. Traditional stereotypes associate corporate leadership with masculinity (Koenig et al. 2011) and with masculine traits, such as power, confidence, aggression and objectivity (Offermann and Beil 1992); women in leadership positions may experience internal psychic costs or punishment from others for violating expectations about appropriate feminine behavior (Blau and Ferber 1986; Jamieson 1995; Akerlof and Kranton 2000). Rather than adopting characteristically male behavior, there is evidence in the management literature of

successful female leaders adopting a distinctive, "transformational" style (Rosener 1990; Bass and Avolio 2006; Dezső and Ross 2008).[5] For example, while men are more likely to assert themselves in a controlling manner, women tend to take into account the rights of others (Eagly and Carli 2007).

Most economic studies of gender differences in preferences focus on students, workers, or the general population, so it is unclear whether the conclusions extend to the selected group of men and women at the top of the corporate world. There are many reasons to be skeptical. For example, women who choose to enroll in business school have unusually high levels of testosterone (Sapienza, Zingales and Maestripieri 2009), which may affect how managers lead their organizations (Levi, Li and Zhang 2010). Women who act too "feminine" may be denied promotion (Branson 2006), and women who do make it to the top in a predominantly male environment may adapt their behavior such that gender differences disappear (Adams and Funk 2009).

To better understand gender differences among corporate leaders, Adams and Funk (2009) surveyed the universe of resident directors and CEOs of publicly traded companies in Sweden in 2005. Gender equality and board structure in Sweden are similar to Norway, which we study in this paper. Their survey instrument consisted of Schwartz's (1992) 40-question Portrait Value Questionnaire plus a question designed to measure risk aversion. The Schwartz value survey is among the most advanced that psychologists use, has been replicated in many countries, and produces consistent and reliable results. Researchers have also shown that these values predict economic behavior in experimental settings, such as voting decisions and altruistic behavior (e.g., Andreoni & Vesterlund 2001).

[5] In their meta-analysis of 44 studies of leadership style, Eagly et al. (2003) find women score 0.10 standard deviations higher in the "transformational" dimension than men.

Adams and Funk's results are summarized in Figure 1. Even relative to male directors at the same firm, female directors report significantly different values than their male colleagues. Female directors care less about self-enhancement values (achievement and power), and more about self-transcendent values (universalism and benevolence). Women board members are also more independent-minded, valuing self-direction and stimulation more than men and valuing tradition and conformity less. All of these differences (except achievement) are sizeable in magnitude, measuring about one-third of a standard deviation, and statistically significant. Although among the general population women are typically more risk averse than men (Byrnes et al. 1999; Eckel and Grossman 2008), women in the boardroom are not and may even be more risk loving. In fact, women assign less value than men to security.

The significant differences between male and female directors suggest that increases in women's representation on corporate boards could have causal effects on firms' outcomes. For example, Adams and Funk conclude that female directors, motivated by their more self-transcendence values, may lead their firms to decisions that are more stakeholder-oriented, such as maintaining their payrolls in periods of low demand, even at the expense of short-run profits. Indeed, Adams, Licht, and Sagiv (2010) show that directors who value benevolence and universalism are more likely to side with stakeholders when presented with vignettes based on actual legal cases in which there was a tension between the interests of shareholders and those of other stakeholders. The large gender gaps in valuing conformity and tradition, possibly related to women's exclusion from male social networks (e.g., Ibarra 1992), may shed some light on Adams and Ferreira's (2009) finding that female directors are tougher monitors of CEOs; women may be more willing to go against the grain to challenge established practices. Contrary to conventional wisdom, Adams and Funk find no evidence that increasing gender diversity would

lead to more risk-averse decision-making, suggesting that having female directors is less likely to affect a firm's financial leverage or the risk profile of investment.

3. EMPIRICAL SETTING

The regulation we study requires that Norwegian public limited companies have at least 40 percent representation from each sex on their board, among directors appointed by shareholders. In Norway, public limited companies are required to have a capitalization of at least 1 million Norwegian krone (about 150,000 U.S. dollars) and an auditor, and their shares must be available to the general public, typically through a stock exchange. Similar gender quotas have been proposed or adopted elsewhere in Europe, and disclosure requirements have been introduced in the U.S. and Australia.[6] In Norway, a quota was initially proposed on a voluntary basis in 2003. After firms failed to increase their female representation, however, a mandatory quota was instituted, and firms were given two years to comply. According to official records from Statistics Norway, nearly all firms complied by February 2008 and all firms complied by April 2008.

Although efficiency arguments have been advanced in support of greater board diversity, they do not appear to be the reason why Norway adopted the quota scheme. The stated motivation was diversity in itself.[7] Scholars summarize the events leading up to the quota as "the equity argument has become law" (Randøy, Thomsen and Oxelheim 2006, p.2).

[6] Following the Norwegian gender quota, others have been passed in Spain (in 2007), the Netherlands (2009), France (2010), and Iceland (2010), and have been proposed in Belgium, Finland, and Sweden. The Spanish law combines incentives for increased female board membership along with requirements that women represent at least 40 percent of candidates filed on political party ballots. The Swedish proposal was withdrawn following a change of government in 2006.

[7] Without consulting the prime minister or other government or ministry officials, Ansgar Gabrielsen, the Minister of Trade and Industry, first proposed the quota in the press in 2002, reportedly to overcome the perceived problem of "boys' clubs" that excluded women from corporate boards (Reiersen and Sjåfjell, 2010).

Policymakers appear to have been aiming to promote equality, rather than targeting any particular economic outcome.

To ensure that our estimates capture the effects of the quota for board members and not other policy changes, we investigated other legal changes implemented in Norway around the time of the quota. In corporate law, the Competition Act of 1993 was replaced by the Competition Act of 2004, which harmonized Norwegian law with EU competition rules and introduced a pre-merger notification scheme. In commercial law, an act relating to cosmetic products was passed in 2005 and effective in 2006. We do not expect either of these changes to affect the corporate outcomes we study in this paper. More relevant for our analysis is the 2005 Working Environment Act. The main changes introduced were: an obligation to consult with employee representatives regarding the company's expected financial status and activities that might affect the working environment; new protections against discrimination; and preferential rights for dismissed and part-time workers in filling new vacancies. The requirement for information and consultation with employee representatives prior to "collective redundancies" (layoffs affecting 10 or more workers) remained unchanged. Similar improvements in employment protection were not implemented elsewhere in Scandinavia. The European Union also adjusted accounting standards in 2005 from domestic GAAP (Generally Accepted Accounting Principles) to IFRS (International Financial Reporting Standards). While these changes may have increased business costs slightly for Norwegian firms or affected some financial reporting, they should not lead to differential effects between listed and unlisted firms *within* Norway, which features prominently in our identification strategy.

3.1 DATA AND EMPIRICAL APPROACH

We use a panel of Scandinavian companies in the years 1999 to 2009 from Bureau Van Dyjk's Orbis database. We start with the full set of public limited companies in Norway, excluding banks and financial institutions, which are subject to different ownership rules.[8] We limit the sample to firms with shares listed on an exchange and with nonmissing information on directors, industry, assets, employees, labor costs, and operating profits in 2006, leaving us with 104 listed, Norwegian companies. Restricting the treatment group to listed companies ensures comparability across countries for the listed versus unlisted classification used for identification. This choice also focuses our analysis on the subset of companies subject to the quota that were least likely to be able to avoid it. To avoid the regulation, a listed firm would have to delist and concentrate its ownership before it could convert to private status. Indeed, a substantial number of *unlisted* Norwegian public limited companies converted to private status in the wake of the quota, whereas few public *listed* companies changed to private status (Nygaard 2011).[9]

Our estimation approach relies on difference-in-difference comparisons with matched samples of private firms in Norway and public and private firms elsewhere in Scandinavia.[10] For each public company in Norway, we identify the five closest firms in each of the 3 comparison groups, based on industry, assets, employees and operating profits in 2006. We

[8] Norway also changed the registration requirements for financial firms during this period, leading many to change their organization form away from being public limited companies.

[9] Appendix Table A1 shows that the main results are robust to alternative sample selection rules. Panel A includes unlisted public limited Norwegian companies in the treated group and draws additional matched controls for them. As expected, the effects are diluted by including these firms; the estimates are smaller in magnitude than our main results, yet still statistically significant. Panel B forgoes the matching procedure described in the next paragraph and finds similar results using a control group comprised of *all* unlisted companies in Norway and companies elsewhere in Scandinavia for which data are available. Panel C shows that the main results are also robust to limiting the control sample to companies relatively more similar to the affected companies (within a distance of 5 units) and excluding the 2 affected companies with no close matches.

[10] If firms differ along both observable and unobservable dimensions, estimation that combines matching on observables and fixed effects (to account for time-invariant unobservables) can yield more reliable estimates than matching alone (Smith and Todd 2005).

create a single index for firms' proximity using Abadie et al.'s (2004) matching algorithm, weighting differences in each firm characteristic using the inverse of its sample standard error. Although matching helps ensure similarity between the various samples, we obtain similar results when we forgo matching and use the full samples (see Appendix Table A1). Financial variables are winsorized at the 1 percent tails, as is common when working with accounting data.

Summary statistics for the various samples are reported in Table 1 for 2006. Affected firms are similar to the matched firms, although they have somewhat higher assets and lower profits and labor costs as a percent of assets (these differences are not statistically significant at conventional levels). Industry mix is similar across the groups as well, with many firms coming from manufacturing; transport, storage and communication; or real estate, renting, and business (i.e., service) activities. Because each affected firm is matched with its nearest neighbors, the matching procedure can draw the same comparison firm multiple times. In all, the 1,560 observations for comparison firms correspond to 1,103 distinct firms. We account for the repeated observations in the estimation by adjusting the standard errors for clustering at the firm level.

Data on board members are from Orbis, for the years 2006 and 2009. For each named director, we impute gender by matching first and middle names to official lists of names used in Scandinavia. In cases where the record includes a gendered title, such as Mr. or Mrs., we use the title. We are unable to determine gender for 0.57 percent of the directors and instead assign a weight of one half for both male and female in these cases.[11] Using these data, we compute the female share of each board in 2006 and 2009, among directors whose names are listed in the database.

[11] The unassigned gender cases are due to directors with gender-neutral names and no gendered titles. The results are identical if we exclude these individuals from the director sample or assume that they are all men.

3.2 EFFECTS OF THE QUOTA ON BOARD COMPOSITION

The law significantly affected observable characteristics of corporate directors in Norway. We begin by examining the direct impact of the quota on female representation using data on firms' board members from 2006 and 2009. Over the three year period, the average female share on boards of directors more than doubled among affected firms, while that share increased by only 35 percent for private firms in Norway. Elsewhere in Scandinavia, the increase was 58 percent for public firms and 45 percent for private firms. On average, affected firms increased female representation by about one board member (from 1.1 to 2.3 women) and reduced their male membership by a similar amount (from 5.0 to 4.1 men). Board size increased slightly over the period from 6.1 to 6.3 members. Average board member turnover was not permanently affected by the quota (the share of newly appointed board members was 21.8 percent from 2003 to 2006, and 21.2 from 2007 to 2009), although there was a surge in 2007 (when 27.0 percent of members were newly appointed). As board size and turnover may directly affect board performance (Adams, Hermalin and Weisbach 2010), we control for these changes in our analysis of the impact of the quota below, where the data are available. In all, 122 new women assumed positions on boards of public companies after the law was implemented.[12]

To further document the direct impact of the quota, we estimate the following regression model for gender representation on the board of firm *j*:

$$Y_j = \beta * Norway_j * Listed_j + \delta * Norway_j + \lambda * Listed_j + \varepsilon_j$$

[12] Based on administrative data from the Norwegian government, the number of distinct women serving on the corporate boards of public companies in our sample increased from 159 at the end of 2005 to 281 in 2009. Although the additional women substantially changed board composition, they represent a trivial share of the more than 80,000 Norwegian women with more than an undergraduate education. In addition to this domestic supply of talent, Norwegian companies also draw both male and female directors from outside of the country (as long as one half of the directors are residents of Norway or citizens and residents of a different country in the European Union or European Economic Area), which greatly increases the pool of potential directors; in 2009, foreigners comprised 16.4% of male and 17.0% of female directors at public Norwegian companies (Ahern and Dittmar 2011, Appendix Table 2).

where $Norway_j$ and $Listed_j$ are indicator variables. We report the difference-in-differences estimate for β in the first row of Table 2. Relative to other listed firms outside of Norway and to unlisted firms in Norway, affected firms have female board shares in 2009 that are 20 percentage points higher (column 1), following an average increase in female board share from 2006 that is 17 percentage points larger (column 2). These dramatic differences indicate that the law had a substantial impact on board composition and provide support for the statistical power of the main identification strategy used throughout this paper. The estimates for δ show no differences in female representation between private companies in Norway and elsewhere in either 2006 or 2009. Outside of Norway, listed companies have about 6 percent lower female representation in both years (there is a difference in first column for 2009, but not for the change between 2006 and 2009 in the second column).

Adding women may have also changed other characteristics of companies' board members, such as their age and management experience. We use official data from the Norway business register to track changes in average director age for public listed companies from 2002 to 2009. Figure 2 plots the average age of all directors, male directors, and female directors. The averages of these values across firms are presented with 95-percent confidence intervals (across companies for each year). Although women on boards are younger than men on average, this is true throughout the period. Perhaps surprisingly, the average age of female directors did not change over the period, and neither did the average age of *all* directors; the increasing share of female directors was offset by the rising average age of male directors. Director age is also stable throughout its distribution, not just its mean: between 2003 and 2009, the 10^{th} percentile of the age distribution is level at 37, the median is level at 49, and the 90^{th} percentile decreases only slightly from 62 to 61.

For additional information on director characteristics, we collect data from Thomson ONE Banker, which is available for 94 of the 104 public Norwegian firms in our main analytic sample. Table 3 summarizes the average values for key background and demographic characteristics of male and female board members of these firms in 2009. Female board members are about 5 years younger than males, on average, and are significantly less likely to have worked as a CEO.[13] Although women, on average, have less previous board experience, the difference is not statistically significant, and they tend to be currently serving on more boards, possibly as a result of the quota. Although they have similar educational backgrounds, women are more likely to have experience in education, law or the public sector, and less likely to have worked in engineering. These differences in demographics, though they may be important, appear modest compared to the gender differences in directors' values and preferences shown in Figure 1 above.

To shed some light on whether women appointed to board seats after the quota were similar to women appointed previously, Table 3 also compares female board members, serving in 2009, who were first appointed to their boards before and after the quota. The women are broadly similar. Differences in CEO experience, education, and other board positions are small and not statistically significant. The only statistically significant differences (other than the mechanical difference in age that would be expected if all women were initially appointed to boards at the same age) are increases in directors with work experience in education (from 0 to 5

[13] As shown in Figure 2, despite the age difference between male and female board members, average director age was remarkably stable throughout the period. Unfortunately, our director panel does not contain information on which directors were also CEOs at the time. Appendix Figure 1 uses data reported in Ahern and Dittmar (2011; Table 2) to plot the shares of directors at affected firms whose outside occupation was CEO and who had any prior CEO experience, for the years 2001 to 2009. Although there is a decline in the share of directors with any CEO experience throughout the decade, the share remains relatively stable in the period following the quota (59 percent in 2006 and 58 percent in 2009). There is even more stability in the share of directors who are currently CEOs. That share is slightly higher in 2008 than in 2001 (26 percent versus 25 percent), and slightly lower in 2009 (24 percent).

percent) and information technology (from 4 to 12 percent). The increasing representation of people with backgrounds in information technology is also present for male of board members (see Appendix Table 2). In all, women appointed to board positions after the quota have similar education, professional experience, and other characteristics to woman appointed before the quota took effect.

4. Effects of the Quota on Corporate Activity

4.1 Corporate Profits

This section describes our main empirical strategy and presents results for the overall effects of the quota on profitability as a measure of overall short-term performance. To isolate the impact of the quota, we compare changes in outcomes among the affected, listed firms in Norway in the years immediately preceding and following the imposition of the quota with changes during the same time period among a sample of similar comparison firms that were not directly affected by the quota. Because no single comparison group is ideal, we implement the strategy in three stages and report results in the three columns of Table 4.

In the first column, we use the full sample of listed Norwegian firms, combined with the matched control sample of the five closest unlisted Norwegian companies that are not bound by the rule. The within-Norway estimates compare listed and unlisted firms:

$$Y_{ijt} = \beta_1 * Listed_j * Post2006_t + \gamma_i * Year_t + \alpha_j + \tau_t + \varepsilon_{ijt} \quad (1)$$

where Y_{ijt} measures profits at firm j in industry i in period t, and the terms α_j and τ_t are year and firm fixed effects, respectively. The term γ_i allows for differential linear time trends by industry

group.[14] The sample period is from 2003 to 2009 and standard errors are adjusted to allow for arbitrary within-firm correlations in the error term.

The impact of the quota reported in column 1 is negative, indicating that annual profits decreased: within-Norway, profits declined after 2006 by 2.7 percent of assets among listed firms relative to the change in profits in unlisted firms during the same time period. However, this empirical approach will not isolate the impact of increasing the share of women in the boardroom if the assumption of common time trends for listed and unlisted companies in Norway is violated. Although our matching procedure provides a comparison sample that is more similar than the full set of all unlisted firms, listed companies were nonetheless larger in terms of workers and assets and had slightly lower average profits in 2006 (see Table 1). An important concern is that private companies may not provide a useful comparison group if being publicly traded affords affected firms greater access to capital for growth or exposes their managers to different pressures under similar market conditions. In that case, the common year trends in specification (1) would not produce the appropriate counterfactual for what would have happened to listed companies absent the reform.

To address this concern, we employ a second identification strategy that exploits variation within the set of listed companies in Scandinavia. In this approach, listed companies in Denmark, Finland, and Sweden provide the comparison group used to estimate the counterfactual year effects:

$$Y_{ijt} = \beta_2 * Norway_j * Post2006_t + \gamma_i * Year_t + \alpha_j + \tau_t + \varepsilon_{ijt} \quad (2)$$

In equation (2), the impact of the law is represented by β_2, the coefficient for the interaction term between Norway and the post reform period. The estimate for β_2 is reported in the third column

[14] We classify firms into the industry groups listed in Table 1 using NACE revision 1.1 classification codes.

of Table 4. The estimate is remarkably similar to that in the first column: the quota is associated with a decline in profits of 3.4 percent of assets (again statistically significant at the 5 percent level), this time, relative to the change in profits of listed companies outside of Norway.

Our third identification strategy combines these two approaches to estimate a triple-difference for the impact of the law. This approach simultaneously allows public and private firms to follow different trends while also accounting for any differential changes in business conditions or regulatory environments that may have affected Norwegian firms relative to firms elsewhere in Scandinavia. In this specification, we compare the change in outcomes between listed and unlisted companies in Norway and elsewhere in the region:

$$Y_{ijt} = \beta_3 * Norway_j * Listed_j * Post2006_t + \lambda * Norway_j * Post2006_t + \delta * Listed_j * Post2006_t \quad (3)$$
$$+ \gamma_i * Year_t + \alpha_j + \tau_t + \varepsilon_{ijt}$$

The impact of the law is captured by β_3, the triple-interaction between indicators for Norway, listed, and post-2006. This term can be interpreted as the difference in how the profit differential between listed and unlisted companies in Norway changed after 2006, compared to how the corresponding differential changed in other countries at the same time. Equivalently, it is the difference in how the profit differential between Norway and other countries changed differentially for listed and unlisted companies after the quota was implemented.

As in the previous columns, the effect of the quota on profitability is negative, with a magnitude here of 4.1 percent of assets (column 5). The difference in trends between Norway and other countries that affect *all* companies are measured by the term λ, which can be interpreted as the difference-in-difference between Norway and other countries among unlisted companies. Similarly, the term δ captures the differential change in listed versus unlisted companies outside of Norway. In specification (3), these simple difference-in-difference

estimates are unrelated to the effect of the quota. These estimates can be thought of as specification checks for equations (1) and (2) in that there is no reason to expect the quota to induce any variation along these dimensions after controlling for the triple difference. Indeed, the estimates are small, positive, and statistically indistinguishable from zero.

To determine if the effects of the quota are mediated through changes in board structure unrelated to gender composition, we estimate augmented versions of each specification with additional controls for the total board size and the average number of other board seats occupied by its members. We were able to obtain these variables for all firms in our sample from the Bureau Van Dyjk's Orbis database for the years 2006 and 2009.[15] We apply the 2006 values for all years before 2007 and the 2009 values for the post-quota years. Including these control variables has minimal affect on the estimates (see columns 2, 4, and 6 of Table 4).

We also obtained annual data on the names and ages of board members specifically for Norwegian firms from official registers maintained by the Norwegian government. This enables us to estimate an expanded version of the first specification, using within-Norway variation by listed status, that includes controls for board size, the average number of other boards, the average board member age, and the share of new directors (i.e., those who were not directors at the same firm in the previous year). The results are again unaffected. Although profits are higher among firms with older directors (a 10 year increase in average age is associated with a 2 percentage point increase in profitability) and stable boards (a 10 percentage point increase in share of new directors is associated with a 0.2 percentage point decrease in profits), adding the controls leave the coefficient on *Listed*Post2006* virtually unchanged at -0.025 (standard error of 0.011). The estimates' robustness to the inclusion of these controls for board characteristics

[15] The Orbis database is maintained to include only current information on boards of directors. We were able to obtain the data specifically for 2006 from an historical backup located for us by Bureau Van Dyjk.

suggests that the gender of the new members, rather than their age, their newness to the board, their other commitments, or the total size of the board, drives our results.

The relative change in profits is also not due to affected firms increasing their assets while leaving sales revenue unchanged. The estimated effect of the quota on total assets, measured in either levels or logs, is not statistically significant. Indeed, Appendix Table A3 shows that the results are robust to measuring profits relative to sales, rather than assets.

4.2 Labor Outcomes

While the profitability results in the previous section provide an important summary measure of the immediate impact of the quota, the main goal of this paper is to uncover *how* corporate decision-making changes with the growth in female leadership. Our next analysis, reported in Table 5, decomposes the change in profits into the effects on revenues and costs, and identifies increased employment as a root cause. Each column reports the triple-difference estimate of the effect of the quota using specification (3) for a different dependent variable.

The first column of Table 5 repeats the estimate for overall profitability: operating profits decrease by 4.1 percent of assets. Because of the accounting identity, this decrease in profits must reflect some combination of a decrease in revenues and/or an increase in costs. To explore the corporate changes that lead to the decrease in profits, we examine the impact of the quota on these items separately. Because the triple-difference specification is common across these regressions, the sum of the effects on each of these components must sum to the aggregate effect on overall profitability. The second column reports the results for revenues. The estimate, which is noisy and not statistically significant, suggests that revenues may decrease by a modest 0.9 percent of assets, on average, after the quota is imposed.

The most substantial effect of the law, both statistically and economically, appears to be its effect on labor costs. As reported in the third column of Table 5, labor costs increase by 4.1 percent of assets, on average, relative to unaffected firms after the quota. Recall that in this triple-difference specification, this estimate is the difference in how the employment cost differential between Norway and other countries changed differentially for listed versus unlisted companies after the quota was implemented.

In contrast to the results for labor costs, we find that other costs decrease by about 1.0 percent of assets, on average, after the quota, and the decrease is not statistically significant (column 4). The fact that other costs *decrease* suggests that the increase in labor costs are not simply attributable to the firms negotiating poorly with all of their suppliers after the quota takes effect. This evidence is most consistent with sex differences in board members' style and preferences, rather than skill or experience, causing the drop in profitability. The pattern is repeated in the second panel of Table 5 where estimates are reported that include controls for board size and number of other boards.

To investigate the impact on labor outcomes in further detail, we estimate separately the quota's effects on employment (number of workers) and labor costs (the sum of compensation costs for all workers). We unfortunately do not observe the full distribution of wages paid at these firms. The results are reported in Table 6. Each of the three panels reports estimates from a different identification strategy described in the previous section: specification (1), the within-Norway comparison, is in Panel A; specification (2), within-listed companies, is in Panel B; and specification (3), the triple-difference is in Panel C. The 2005 update to employment regulation in Norway (described in Section 3) makes the within-Norway and triple-difference approaches especially valuable for exploring labor effects of the quota.

These estimated effects of the quota are again quite similar across the three empirical approaches. For total employment, the triple-difference estimate finds an increase of 0.31 log points, or 36 percent ($p < 0.01$). For the median listed, Norwegian firm, this corresponds to about 110 additional jobs that were either created or not destroyed, relative to the comparison firms. The estimates for total labor costs are also positive and statistically significant across the three models. The magnitudes are slightly smaller than the employment estimates: about 0.15 log points, or 16 percent, in the triple-difference specification. The difference between these estimates implies that the quota led to greater relative employment, but did not also increase average wages per worker. For all specifications, the results are robust to including controls for board size and the average number of other boards (columns 2 and 4). We also have data on board members' ages and turnover for Norwegian firms. The within-Norway estimates in Panel A are also robust to controlling for the board members' average age and the share of new board members in addition to the other variables: the point estimates are 0.47 (standard error of 0.08) for log-employment and 0.30 (standard error of 0.07) for log-labor costs.

The employment effects are not driven by a few outlier observations with large employment changes. To investigate the possibility, we first re-estimated the main DDD employment model on a sample limited to observations with standardized regression residuals of less than three (positive or negative). The estimated effect of the quota dropped to a more moderate 0.24 log-points (with a standard error of 0.03). We also estimated robust regression[16] and least absolute deviation versions of our main DDD model, which yielded point estimates of 0.11 log-points (standard error of 0.02) and 0.20 (standard error of 0.02), respectively. We also experimented with dropping each of the treated companies (and their associated controls) in turn:

[16] The robust regression procedure first estimates the model using all observation with no weights. Then the main estimates are computed excluding all influential observations (with Cook's D values greater than 1) and lower weight on observations with large absolute residuals from the original regression.

the point estimates ranged from 0.27 to 0.33. These results support the robustness of the positive employment estimate, but also reveal some sensitivity in the exact magnitude of the estimate to individual observations. Adjusting for outliers reduces the size of estimate, by as much as two-thirds.

This pattern of changes in employment and labor costs is consistent with women on corporate boards leading management to hire or retain additional workers whose salaries are below average for the firm. These lower-compensated workers are more vulnerable to risks of negative employment shocks than their highly-compensated counterparts. To examine the role of layoffs in these effects, we measure the effect of the quota on year-to-year net reductions in employment. Table 7 reports estimates for the triple-difference model (equation 3). The first column presents estimates of a linear probability model of an indicator for a greater than 1 percent decline in total firm employment. The probability of such a reduction in total firm employment in a given year decreases by 25 percentage points among affected firms, relative to other firms, after the quota is adopted. This constitutes a sizeable drop, especially when compared to the sample mean of 32 percent and standard deviation of 47 percent. In the next two columns, we separately examine the incidence of larger layoff events, affecting more than 3 percent or 5 percent of the workforce. Layoffs fitting these definitions occur in 25 percent and 20 percent, respectively, of firm-year observations in the full sample. But after female board representation increases with the quota, the incidence of these larger layoffs among affected firms also decrease, by 20 percentage points (for the 3 percent cutoff, column 2) and 13 percentage points (for the 5 percent cutoff, column 3) relative to unaffected firms. Across the alternative specifications, the estimates reflect 65 to 80 percent reductions in the annual

incidence of layoffs, relative to the sample means, and 32 to 54 percent of the sample standard deviations.

The quota's impact on employment changes is not symmetric. We also examine the incidence of year-to-year *increases* in employment of more than 1, 3 and 5 percent. Workforce expansions are more common than layoffs in our sample and these occur in 55, 48 and 42 percent of firm-year observations. We find little evidence of a relation between the quota and employment expansions; the estimates are much smaller relative to the sample means and standard deviations and are not statistically significant (columns 3-5).

Together, these estimates suggest that the growth in relative employment after the quota results primarily from fewer workforce reductions rather than more workforce expansions. These apparent transfers from shareholders to workers, at least in the short-run, are consistent with survey evidence from Adams and Funk (2009) indicating that women in the boardroom are more likely than men to consider stakeholders' preferences (such as those of workers) in addition to those of shareholders. Similarly, in a survey of business newspaper readers asked to consider a hypothetical scenario of a manager facing diminished demand, Rubinstein (2006) finds gender differences in attitudes toward layoffs. Rubinstein describes women's greater willingness to retain rank-and-file workers at the expense of profits as "more compassionate."

Another possibility is that the gender-diverse boards are maximizing long-run shareholder value by avoiding layoffs. Layoffs directly reduce payroll costs and improve short-run operating performance, but they may also increase costs later by decreasing employee morale and requiring the recruiting and training of new hires when demand rebounds (Parsons 1972; Katz 1986).[17] Under this interpretation, the new women on these boards are encouraging their

[17] Indeed, some management scholars argue that employment downsizing often fails to generate the benefits sought by management (e.g., Cascio 2002). A number of studies find that layoffs are associated

firms to adopt strategies that view employees as assets with specific human capital to be developed rather than as costs to be cut. This is consistent with women being more patient than men (Silverman 2003; Frederick 2005) or with women being more likely to use implicit contracts to provide workers with job security.[18] Either way, female board members appear to be pivotal in preventing mass layoffs in periods of low demand. In that regard, our results suggest that sex differences in preferences or attitudes among directors can translate into meaningful differences in corporate strategy.

4.3 Alternative Hypothesis: Declining Corporate Activity

The results in the previous sections indicate that the increase in female directors after the quota affected corporate strategy. An alternative explanation for the observed changes in profits and employment is that boards affected by the quota were somewhat dysfunctional during the transition period. One might imagine that the quota could lead boards to be relatively inactive either because of contentiousness between senior male board members and their new female colleagues or because the new directors were more tentative than those they replaced. For example, it is possible that new directors, particularly new directors selected because of an external mandate, would feel hesitant about major corporate activities, including downsizing in the face of a global recession.

In order to test if the employment results are due to a general reduction in corporate activity following the quota rather than an intentional reduction in layoffs, we compare rates of

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with low stock prices or accounting performance (Worrell et al. 1991; Lin and Rozeff 1993; Cascio et al. 1997; Palmon et al. 1997; Hallock 1998), but it is difficult to separate the effects of a layoff from the effects of the adverse economic conditions that caused it.

[18] This interpretation would be analogous to family firms that have been shown to pay lower wages but to protect workers' jobs in periods of reduced demand (Sraer and Thesmar 2007). Bassanini et al. (2010) show that family firms in France provide workers with greater job stability: even when they reduce total employment, family firms tend to rely less on dismissals and more on hiring reductions.

mergers and acquisitions, joint ventures and minority stake purchases in the three-year windows before and after the quota was implemented. We calculate these rates separately for listed Scandinavian companies (drawn from our group of affected firms and the matched control sample) inside and outside of Norway.

The results, reported in Table 8, provide no support for the inactivity hypothesis. Within Norway, activity levels actually increased modestly after the quota for each of the three measures. Furthermore, the changes in activity levels are all statistically indistinguishable between the listed companies affected and unaffected by the quota. We similarly find no indication of inactivity when we include private companies in the sample, and estimate a DDD model in which we compare changes in activity between listed and unlisted companies in Norway and elsewhere in the three-year periods before and after the quota. The quota does not appear to have reduced corporate activity. This indicates that the relative reduction in layoffs for affected firms was not the result of their boards being incapable of taking such actions.

4.4 Robustness Checks

4.4.1 Government Ownership

We examine various robustness checks to verify the connections between the gender quota for board composition, decreased profits, and increased relative employment. We start by verifying that the results are not limited to government owned firms. This exercise is useful for ruling out political explanations for the apparent shift in corporate priorities to favor employment, especially of lower-wage workers (e.g., La Porta and López-de-Silanes 1999; Megginson and Netter 2001). We obtain information on stock ownership from Orbis. Our sample inclusion criteria (requiring the availability of non-missing information) already excluded

all companies for which the Norwegian government's direct ownership stake exceeds one percent. Nevertheless, it is possible that the government can influence the appointment of directors or corporate strategy even when its ownership role is indirect, such as through pension or sovereign wealth funds.

To examine the role of political explanations, we exclude 32 listed firms in Norway for which the sum of the government's direct and indirect ownership share is at least one percent, as well as their matched comparison firms.[19] Table 9 reports the estimated impact of the quota for the main outcomes, using the triple-difference identification strategy described above. The estimates are very similar to those for the full sample. Profitability declines by 4.1 percent, while employment increases by 29 log point and labor costs increase by 21 log points. Layoffs affecting more than 3 percent of the workforce decline by 17 percent. These results closely resemble the findings for the full sample reported in Tables 4, 6, and 7.

4.4.2 PRE-EXISTING TRENDS

Next, we test for pre-existing trends. We compare relative changes among affected firms during the period preceding the mandatory quota for two reasons. First, the quota itself was not a surprise when the mandate was adopted in 2006, and there may have been anticipatory changes. Starting in 2003, public firms in Norway were encouraged to increase female representation on their boards of directors and warned of mandatory quotas starting in 2006. In practice, few firms added women in earnest until the mandatory period. Thus the timing of the effects may suggest whether the results are in fact due to the addition of women to the boards, or instead attributable to an omitted variable that led Norway to adopt the rule. Second, we test for differences between

[19] In a related exercise, we also confirmed that the results are unchanged if we exclude companies in the petroleum industry from the analysis.

affected and unaffected firms before 2007 as a falsification check for pre-existing trends that could obscure proper inference of the quota's effects.

To investigate the presence of pre-existing trends between 2003 and 2006, we expand our dataset back to 1999 to include a baseline period before 2003. We estimate an expanded version of specification (3) with two sets of triple-difference effects. The first set, reported in the first row of Table 10 are the main treatment effects in the post-2006 period. As in the previous tables, these effects are computed relative to the 2003 to 2006 period and reveal significant declines in profits and increases in employment and employee costs. The second row of Table 10 reports triple-difference effects for the 2003 to 2006 period, relative to the earlier 1999 to 2002 period. These estimates show no indication of differential effects in the preceding period; the estimates are substantially smaller, statistically insignificant, and inconsistent in sign relative to the post-quota estimates.

4.4.3 EFFECTS OF GLOBAL RECESSION

Our results find that listed companies in Norway responded differently than other listed and unlisted companies inside and outside Norway to the recent global recession. We link these patterns to the companies' new female leadership, but another possibility is that there is something else about listed Norwegian companies that make them less likely to layoff workers in recessions. To examine this alternative hypothesis, we collected additional data from the pre-quota period and estimated our DDD model using data from the previous global recession. This exercise provides a falsification check to test if the differential changes in profits and employment, related to fewer layoffs, are linked to the quota itself or if they are instead a generic feature of public companies in Norway.

We study the global recession that started around 2001 using financial data on Scandinavian public and private firms from Orbis for the period from 1997 to 2003. Our sample of firms includes all listed Norwegian companies with data available in 2000, the last year in the pre-period, and a set of matched control firms from each of the 3 other groups (private Norway, public and private elsewhere) using the same matching algorithm as before.

In contrast with our findings for the period surrounding the implementation of the quota, there is no relative change in profits, employment, employee costs or layoffs in listed firms in Norway in the period following the 2001 recession. The DDD estimates in Table 11 are small and statistically insignificant. The direction of the effect is reversed for profits, which has a positive point estimate. The lack of an effect in this earlier period supports our interpretation that the relative changes in profits and employment amid the recent global downturn indeed reflect the impact of the quota.

4.4.4 DISTANCE FROM COMPLIANCE

As a final robustness check, we test the prediction that firms furthest from compliance in 2006 should display the greatest effects of the law. About half the firms in the affected group had no women on their boards in 2006. In analysis reported in Table 12, we estimate whether these firms, which were effectively required to add a greater number of women to their boards before the 2008 deadline, exhibited greater effects.

In Panel A of Table 12, we limit the sample to the affected firms and estimate differential effects of the quota based on firms' distance to compliance. Because all of the firms in this specification are affected by the law, we are not able to control for baseline time trends. Instead, we simply compare changes in the main outcomes after 2006 for firms depending on their

boards' status in 2006. We find that profits were relatively stable for firms with some women, but declined significantly after the quota was adopted for those that had no women on their boards in 2006. Both employment numbers and costs increased for both sets of firms, but the increases are significantly larger among firms that had no women on their boards in 2006 ($p < 0.06$). The incidence of layoffs declined for all treated firms, but the effect is larger and more precisely estimated for the firms with some women on their boards in 2006.

While this variation within the treatment group provides additional support for the main results for profits and employment, there are important limitations to the analysis. In particular, without a control group, we are not able to identify the actual impact of the quota on either set of firms. To address this issue, we estimate an expanded version of our main triple-difference model (equation 3), which allows for heterogeneous effects based on whether the board included any women before the law was adopted. The results are reported in Table 12, Panel B. These estimates are based on the full sample of affected and matched comparison firms. To identify the impact of the quota, we also include additional controls for the interactions between the "No Women in 2006" indicator variable and the variables for *Post2006*, *Norway*Post2006* and *Listed*Post2006*.

The results find larger estimates of the quota's effect on all outcomes for firms with no women on the board in 2006 before the mandate was adopted. Although the estimates for firms with some female board members in 2006 are similar in magnitude to the overall estimates reported in the previous sections, the estimates are less precisely estimated (on the reduced set of firms) and not always statistically significant. The effects for firms with no women in 2006 are larger and statistically significant for each of the main outcomes: profits decline by 4.5 percent of

assets, employment increases by 0.46 log points, or 58 percent, employee costs increase by 0.25 log points, or 28 percent, and the rate of layoffs declines by 24 percentage points.

This analysis suggests that our main findings are indeed attributable to the gender quota rather than another, unobserved shock affecting listed firms in Norway after 2006. Nevertheless, it is also important to recognize the limitations of this last approach. Even if the quota is exogenous, the *timing* of compliance is not. It is likely that the firms that complied during the voluntary period found it less costly to do so, and may differ from non-complier firms in other dimensions as well. Even during the mandatory period, the exact timing of compliance may be related to unobservable firm characteristics that may also affect profitability and employment. For example, firms may have differed in their abilities to identify and attract capable women to serve on their boards, especially in a period of suddenly increasing demand for female board members. For these reasons, our main analysis relies on identification based on the imposition of the quota, rather than the observed timing of compliance.

4.5 FINANCIAL LEVERAGE

We also examine womens' impact on their firms' financial leverage – an outcome that might be expected to decrease if board members become more risk averse. Population studies find significant sex differences in risk aversion, with women tending to prefer more certain outcomes. But as discussed above in Section 2, these differences may not hold for the selected sample of women who rise to positions of prominence in industry. We examine womens' effect on leverage using the various difference-in-difference and triple difference approaches described above, and the results are reported in Table 13.

In the triple-difference model, we find a negative association between the gender quota and firms' debt over asset ratios, consistent with female board members preferring safer financial strategies. This result, however, is not statistically significant and not consistent across estimation approaches. Furthermore, the magnitude of the response – equivalent to about a tenth of a standard deviation – is modest when considering the large shifts in board composition. If sex differences in board member preferences explain the reduced profitability after the quota, the strong association with employment outcomes and weaker association with leverage suggest that self-transcendent values are more important than risk aversion in forming women's distinctive style.

5. How Do Directors Affect Corporate Strategy?

Our analysis above indicates that increased female board representation shifts corporate strategy. There appears to be a female style in corporate leadership, in that when more women are placed exogenously on boards of directors, short-run corporate outcomes shift to increase employment. But what are female board members actually doing to affect these outcomes? In this section, we explore potential mechanisms that may underlie this effect.

The primary function of a company's board of directors is to hire executives to run the firm's day-to-day operations, to advise management, and to approve changes in corporate control. In existing studies, the role of the board in actively setting corporate strategy is unclear. In a classic descriptive analysis of boards, Mace (1971) concludes that boards of large and medium-sized U.S. corporations serve largely as a sounding board for the CEO and top management but do not establish corporate objectives, strategies, or policies. Yet later studies, such as Demb and Neubauer (1992) and MacAvoy and Millstein (1999), describe boards playing

a more active role. Even when boards do not set strategy directly, they can guide it indirectly when hiring the top executives who will make these decisions. Examining how the new female board members in Norway are able to reduce layoffs may shed greater light on boards' role in setting strategy more broadly.

One way for female board members to influence the company's direction may be to appoint likeminded individuals to top executive positions – that is, men or women with similar, more stakeholder-oriented preferences or more patience.[20] Indeed, using data for S&P 1,500 companies in the United States, Matsa and Miller (2011) show that greater female representation on corporate boards increases the likelihood the CEO will be a woman and, to a lesser extent, increases women's representation among the company's other top-five executives. To investigate the role of CEO turnover in Norway, we supplement our data on CEOs with administrative data from the Norwegian government. Between 2003 and 2009, we find 144 cases of CEO turnover from one year to the next, representing 70 out of the 104 listed firms. Turnover rates in listed firms actually declined slightly (by about 1 percentage point, not statistically significant) in the years immediately following the quota, while those in unlisted firms were stable. This is consistent with the lack of an overall association between board diversity and CEO turnover reported by Adams and Ferreira (2009).

Even if the rate of turnover does not increase, the gender-diverse boards may be selecting new chief executives with different characteristics. For example, in the period before the quota, all replacement CEOs were male. After the quota, 2 (5 percent) were female. This increased hiring of female CEOs is suggestive; it is quantitatively small, but the supply of Norwegian female executives was also likely reduced by the spike in demand for female directors after the

[20] More female board members may also indirectly increase demand for female managers. Even when it results from a quota, exposure to female leaders has been shown to improve perceptions of female leaders among decision makers (Beaman et al. 2009).

quota (Lublin 2007). Even among potential male CEO candidates, the gender-diverse boards may be selecting executives who share their values, which unfortunately are not observable in any of our datasets.

To test if CEO changes are a means through which female directors influence policy, we separate listed Norwegian firms into those with a CEO change (34 firms) between December 2005 and December 2007 and those without (69). We select this time period to capture CEO turnover events that occur after the quota but early enough that they are not likely to be caused by changes in corporate outcomes in the post-2006 treatment period. We estimate heterogeneous treatment effects of the quota for our main outcomes, allowing for differences based on whether the board selected a new CEO after female representation increased. We do this by interacting the main effects of interest in our main regression models (equations 1 – 3) with indicators for having a CEO change and for not having one. In order to ensure that counterfactual time trends for each set of Norwegian listed firms is based on the appropriate set of control firms, we also add a separate regressor to each model that interacts the post-quota period indictor with an indicator for being a listed Norwegian firm with a CEO change or one of its matched control firms. In all cases, the estimates for these control variables are quantitatively small and statistically insignificant.

The estimated effects of the quota, separated by CEO turnover, are reported in Table 14. Across all outcomes, the estimated effects are larger for firms with CEO changes. In the triple-difference model reported in Panel A, the decline in profits is 6.0 percentage points for firms with CEO changes, compared to 3.0 for those without. The employment increases are 0.39 log-points for firms with changes, and 0.27 log-points for those without. Similarly for employee costs, firms with CEO turnover see an increase of 0.24 log-points, while those without increase

by 0.11 log-points. Layoffs declined by 25 percentage points at firms with CEO turnover and by 18 percentage points at those without. The estimates are essentially unchanged if we add an additional control for CEO turnover in any companies (including the comparison firms). These results, reported in Panel B of Table 14 imply that the estimates in Panel A are not being caused by characteristics of CEO turnover that are unrelated to the quota.

The pattern of differential effects suggests that selecting a firm's executives is one way in which female board members influence their firms' corporate decisions. Consistent with this interpretation, only 2 of the 39 new CEOs hired at these firms had ever served as a CEO of a publicly traded firm before the quota period. Rather than the boards using CEO turnover to influence strategy, a possible alternative interpretation is that poor accounting performance led these firms to replace their CEOs. To evaluate this possible interpretation, we researched the circumstances surrounding the exiting CEOs' separations from these firms. While it is inherently difficult to classify whether a separation was forced or voluntary (Eisfeldt and Kuhnen 2010), using firms' disclosures and press accounts, we were able to verify 15 cases in which the CEO left the firm for reasons unrelated to firm performance, such as a bona fide illness, retirement, or better opportunities elsewhere. In these cases, we also find large effects, though with some loss of precision. The results are reported in Panel B for the triple-difference model. Profits decline by 6.2 percentage points, employment increases by 0.53 log-points, employee costs increase by 0.46 log points, and layoffs decline by 22 percentage points.

We conclude that selecting a firm's executives is one way in which female board members influence their firms' corporate decisions. Nevertheless, we find that increased female board representation enabled by the quota also affects policies even when firms' CEOs did not change (see Panel A). This suggests a viable, albeit potentially more modest role, for boards to

influence corporate strategy by advising and directing current managers, at least in Norway. Although the managers were retained at these firms, it may also be that the board's authority to select new management is what ultimately induced these executives to change corporate policy.

6. CONCLUSIONS

The quota introduced in Norway in 2006 mandating gender balance on corporate boards provides a unique opportunity for researchers to learn about how female leadership affects corporate outcomes. Using financial data for publicly listed firms in Norway, and a matched control sample of unlisted firms in Norway and all firms elsewhere in Scandinavia, we find evidence of a relative decline in operating profits over assets associated with the quota. Decomposing the change in profits, we identify increased labor costs, from fewer layoffs and higher relative employment, as the primary cause. This suggests that compliance with the quota was costly for firms in the short term, but raises important questions about the long-term impacts. The fewer layoffs may reflect a more stakeholder-oriented attitude on the part of female directors, or a more long-term perspective if women are more willing to incur higher short-term labor costs to increase workers' productivity in future periods.

By studying the first national quota for female board representation, this paper provides insight into the potential impact of future gender quotas in corporate leadership. Governments across Europe have adopted or considered rules to improve gender diversity on corporate boards. Our results show that gender quotas can be effective at increasing board diversity and may shift the balance of corporate governance away from the short-term interests of investors and toward those of rank-and-file workers. Although the impact of any particular quota may depend on the economic, social and cultural context, the fact that we document important effects in Norway is

itself meaningful. Scandinavian countries are ranked highly in international comparisons of gender equality (e.g., Guiso, Monte, Sapienza, and Zingales 2008); we would expect to find larger effects of a quota in countries with more traditional gender roles or less public commitment to gender equality.

It is natural to ask if the results in this paper can shed light on female corporate leadership styles more generally. As women take leadership positions in more and more corporations around the world, an important question is how, if at all, this demographic shift will affect industry. This paper shows that profitability fell and employment rose after women were brought onto corporate boards *because* of a quota. These results suggest some reasons, other than prejudice against female leaders, why shareholders in Norway were reluctant to bring more women onto their boards before the quota was imposed, even after the threat of a quota was introduced in 2003. However, that may not predict what will happen when women rise to boards without a quota.

Still, there are indications that female corporate leaders exhibit more self-transcendent values and preferences outside of Norway as well—in environments without quota restrictions. In the United States, for example, International Survey Research (2004) recently found that female senior executives attach the greatest importance to what they describe as the "communal" aspects of the workplace, such as working relationships, customer quality focus, and communication. By contrast, male senior executives are driven more by personal-reward factors, such as career development and compensation.

Although we find large differences in managerial style between male and female corporate leaders today, these differences may diminish over time. As more women ascend the ranks of corporate hierarchies, and as the supply of available female talent increases, gender

differences in leadership style may diminish if shareholders are better able to select women who match their preferences from the larger pool of candidates. Over time, the presence of additional women on boards may also influence the equilibrium behavior of men serving with them. For example, Adams and Ferrerira (2009) find that male board members have better attendance records when serving on diverse boards, and Boyd, Epstein and Martin (2010) find that male judges vote differently when they serve on judicial panels with women. The long-term effects of greater gender diversity in corporate leadership present an important area for future research.

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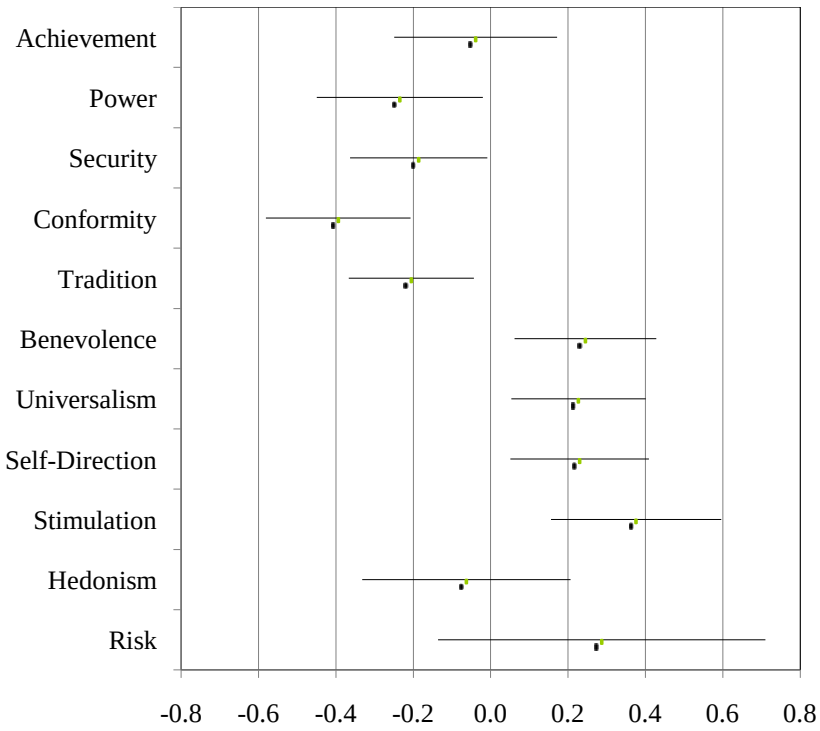


Figure 1. Differential values and risk attitudes of female directors, relative to male directors at the same firm, Sweden 2005. Average differences and 95 percent confidence intervals are reported. Estimates are from Adams and Funk (2009, Table 4). Raw value scores range between 1 and 6, with higher numbers reflecting a higher importance of the value dimension. The dependent variables are first demeaned with respect to the individual's average response in order to reflect the respondent's relative value priorities. Specifications control for age and fim fixed effects.

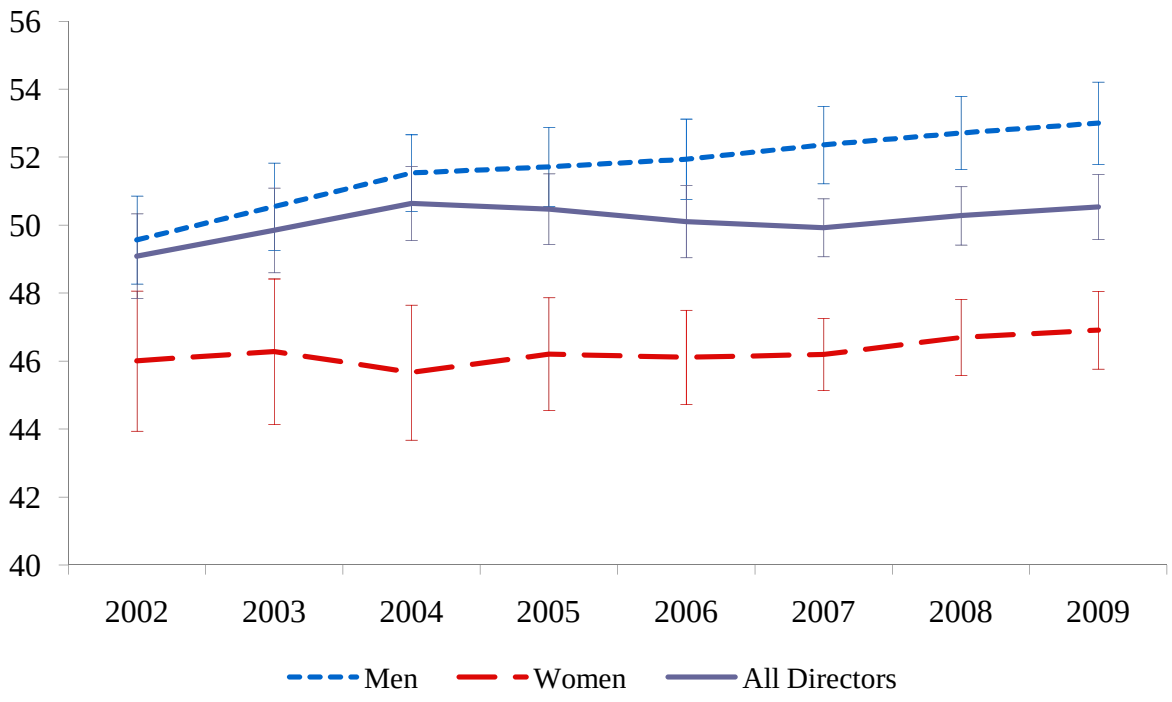


Figure 2. Average director age among public listed companies in Norway, 2002-2009. This figure plots summary statistics for the average age of male directors, female directors, and all directors among public listed companies in Norway. Averages of these values across firms are presented with 95-percent confidence intervals (across companies for each year).

Table 1. Summary statistics for public listed companies in Norway and matched comparison companies, 2006

	Norway		Other Scandinavian Countries	
	Listed	Unlisted	Listed	Unlisted
Assets (€ millions)				
Mean	836	470	780	485
Median	104	95	74	92
St dev	2213	1097	2215	1137
Employment				
Mean	2024	796	2634	1620
Median	302	96	310	319
St dev	5433	2469	6017	4198
Operating profit / Assets				
Mean	0.034	0.071	0.043	0.053
Median	0.061	0.063	0.068	0.058
St dev	0.175	0.097	0.160	0.104
Labor cost / Assets				
Mean	0.220	0.252	0.283	0.250
Median	0.187	0.194	0.236	0.202
St dev	0.189	0.245	0.225	0.229
Debt / Assets				
Mean	0.190	0.146	0.139	0.153
Median	0.157	0.011	0.080	0.042
St dev	0.176	0.201	0.150	0.207
Industry Share				
Agriculture, hunting and fishing	4.8	4.0	0.0	4.8
Mining	1.0	1.0	0.8	1.0
Manufacturing	37.5	36.7	42.3	36.5
Electricity, gas and water supply	2.9	3.7	2.9	3.7
Construction	2.9	2.3	2.9	2.3
Wholesale and retail trade	9.6	11.0	9.8	10.4
Transport, storage and communication	18.3	18.5	18.3	17.3
Real estate, renting and business activities	22.1	21.9	22.1	22.7
Other community, social, personal services	1.0	1.0	1.0	1.4
Observations	104	520	520	520
Number of distinct firms	104	396	293	414

Notes: This table reports summary statistics in 2006 for public listed companies in Norway and three sets of matched comparison companies. Each set of comparison firms consists of the 5 most similar firms, based on assets, operating profits, labor costs and industry in 2006 (matched with replacement using Abadie et al.'s (2004) matching algorithm and inverse variance weighting). Variables are winsorized at the 1 percent tails.

Table 2. Change in gender representation on corporate boards

	Share female, 2009	Change in share female, 2009 - 2006
Norway*Listed	0.199***	0.166***
	[0.019]	[0.021]
Norway	-0.005	-0.004
	[0.010]	[0.010]
Listed	-0.057***	-0.006
	[0.010]	[0.010]
Observations	1,660	1,660
R-squared	0.081	0.052

Notes: This table reports coefficient estimates and standard errors (in brackets) from regressions of different dependent variables on indicators for whether the firm is in Norway, whether the firm is listed, and whether the firm is both in Norway and listed. Norway listed firms were affected by the gender representation law. Each observation is a firm, and the sample includes all listed companies in Norway and the matched set of control firms, described in Table 1, with non-missing data.
*** p<0.01, ** p<0.05, * p<0.1

Table 3. Board member characteristics by gender and tenure, 2009

				Female board members		
	Male	Female	p-value of difference	Appointed before 2006	Appointed since 2006	p-value of difference
Professional experience						
Age	54.135	48.902	<.0001	51.211	48.139	0.035
	[0.594]	[0.636]		[1.227]	[0.731]	
CEO	0.314	0.170	<.0001	0.220	0.154	0.319
	[0.026]	[0.026]		[0.059]	[0.029]	
CFO	0.064	0.044	0.302	0.060	0.039	0.565
	[0.014]	[0.014]		[0.034]	[0.015]	
Other current board positions	0.638	0.858	0.064	1.060	0.795	0.329
	[0.064]	[0.099]		[0.248]	[0.105]	
Other previous board positions	0.900	0.714	0.170	1.020	0.621	0.123
	[0.093]	[0.098]		[0.232]	[0.106]	
Education						
MBA	0.158	0.130	0.360	0.100	0.139	0.444
	[0.020]	[0.023]		[0.043]	[0.028]	
Other masters' degree	0.176	0.188	0.744	0.200	0.184	0.801
	[0.021]	[0.027]		[0.057]	[0.031]	
Ph.D.	0.030	0.048	0.317	0.020	0.057	0.177
	[0.010]	[0.015]		[0.020]	[0.019]	
Role on board						
Audit committee	0.319	0.633	0.004	0.556	0.667	0.596
	[0.057]	[0.090]		[0.176]	[0.105]	
Compensation committee	0.377	0.467	0.416	0.444	0.476	0.881
	[0.059]	[0.093]		[0.176]	[0.112]	
Nominating committee	0.145	0.033	0.042	0.000	0.048	0.329
	[0.043]	[0.033]		[0.000]	[0.048]	
Committee chair	0.290	0.167	0.168	0.333	0.095	0.212
	[0.055]	[0.069]		[0.167]	[0.066]	
Employee representative	0.209	0.133	0.019	0.100	0.143	0.403
	[0.022]	[0.023]		[0.028]	[0.043]	

Table 3. Board member characteristics by gender and tenure, 2009 (cont.)

				Female board members		
	Male	Female	p-value of difference	Appointed before 2006	Appointed since 2006	p-value of difference
Industry experience						
Consulting	0.104	0.141	0.210	0.140	0.141	0.986
	[0.017]	[0.024]		[0.050]	[0.028]	
Education	0.012	0.039	0.073	0.000	0.051	0.004
	[0.006]	[0.014]		[0.000]	[0.018]	
Energy	0.134	0.160	0.413	0.180	0.154	0.675
	[0.019]	[0.026]		[0.055]	[0.029]	
Engineering	0.195	0.107	0.004	0.140	0.096	0.427
	[0.022]	[0.022]		[0.050]	[0.024]	
Finance	0.159	0.160	0.960	0.140	0.167	0.646
	[0.020]	[0.026]		[0.050]	[0.030]	
High tech	0.024	0.015	0.411	0.020	0.013	0.745
	[0.009]	[0.008]		[0.020]	[0.009]	
IT	0.064	0.097	0.182	0.040	0.115	0.049
	[0.014]	[0.021]		[0.028]	[0.026]	
Law	0.058	0.112	0.036	0.100	0.115	0.759
	[0.013]	[0.022]		[0.043]	[0.026]	
Medicine	0.012	0.024	0.329	0.040	0.019	0.493
	[0.006]	[0.011]		[0.028]	[0.011]	
Pharmacueticals	0.021	0.039	0.265	0.040	0.039	0.962
	[0.008]	[0.014]		[0.028]	[0.015]	
Public sector	0.021	0.058	0.044	0.040	0.064	0.483
	[0.008]	[0.016]		[0.028]	[0.020]	
Tourism	0.003	0.005	0.753	0.000	0.006	0.319
	[0.003]	[0.005]		[0.000]	[0.006]	
Observations	330	211		50	161	

Notes: This table reports means and standard errors (in brackets) for various characteristics of board members of listed firms in Norway in 2009. These firms are affected by the gender representation law. Average characteristics are reported separately for men and women, along with the p-value associated with the difference in means. Fewer observations are available for age and committee memberships.

Table 4. Changes in operating profits / assets

	DD: Within Norway by Listed		DD: Within Listed by Norway		DDD: By Norway and Listed	
	(1)	(2)	(3)	(4)	(5)	(6)
Norway*Listed*Post 2006					-0.041**	-0.040**
					[0.016]	[0.017]
Norway*Post 2006			-0.034**	-0.034**	0.010	0.010
			[0.015]	[0.014]	[0.007]	[0.007]
Listed*Post 2006	-0.027**	-0.027**			0.015	0.014
	[0.011]	[0.011]			[0.012]	[0.012]
Controls for board size & average number of other board seats		X		X		X
Observations	3,116	3,116	3,460	3,460	8,901	8,901
Number of firms	610	610	596	596	1,620	1,620
R-squared	0.05	0.05	0.10	0.10	0.05	0.05

Notes: This table summarizes the results from firm-panel regressions of firms' profitability (operating profits divided by assets) on variables indicating whether a firm is affected by the gender representation law and a set of controls. Results are reported using three different samples of comparison firms to provide different counterfactuals for what would have happened to listed, Norwegian firms (the affected group) absent the reform: columns 1 and 2 use unlisted, Norweigan firms; columns 3 and 4 use listed, non-Norweigan firms; and columns 5 and 6 use both sets of comparison firms in a triple-difference specification. Controls in all regressions include firm and year fixed effects, industry-specific time trends, and the appropriate uninteracted variables (i.e., indicators for Norway, listed, and post-2006). Where indicated, controls also include board size and the average number of other board seats. Standard errors, adjusted for clustering at the firm level, are reported in brackets. The panel covers firms from 2003 through 2009 but is not balanced due to missing data.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 5. Breaking down change in operating profit / assets, DDD specification

	Operating profit / Assets	Revenue / Assets	Labor costs / Assets	Other costs / Assets
		Panel A. Main results		
Norway*Listed*Post 2006	-0.041**	-0.009	0.041**	-0.010
	[0.016]	[0.057]	[0.017]	[0.049]
Observations	8,901	8,901	8,901	8,901
Number of firms	1,620	1,620	1,620	1,620
R-squared	0.05	0.01	0.02	0.01
		Panel B. With controls for other board characteristics		
Norway*Listed*Post 2006	-0.040**	-0.007	0.042**	-0.009
	[0.017]	[0.057]	[0.017]	[0.050]
Controls for board size & average number of other board seats	X	X	X	X
Observations	8,901	8,901	8,901	8,901
Number of firms	1,620	1,620	1,620	1,620
R-squared	0.01	0.05	0.01	0.03

Notes: This table summarizes the results from firm-panel regressions of firms' profits, revenues, labor costs, and other costs (each divided by assets) on an indicator for whether the firm is affected by the gender representation law using the triple-difference framework reported in Table 4, columns 5 and 6. All regressions include controls for firm and year fixed effects, industry-specific time trends, and the appropriate uninteracted variables (i.e., indicators for Norway, listed, post-2006, and their pair wise interactions). Standard errors, adjusted for clustering at the firm level, are reported in brackets.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 6. Changes in employment and employee costs

	Log employment		Log employee cost	
	(1)	(2)	(3)	(4)
	Panel A. DD: Within Norway by Listed			
Listed*Post 2006	0.468***	0.464***	0.165***	0.169***
	[0.077]	[0.076]	[0.047]	[0.047]
Observations	2,369	2,369	2,369	2,369
Number of firms	520	520	520	520
R-squared	0.20	0.20	0.32	0.33
	Panel B. DD: Within Listed by Norway			
Norway*Post 2006	0.276***	0.292***	0.260***	0.284***
	[0.083]	[0.082]	[0.071]	[0.070]
Observations	3,324	3,324	3,324	3,324
Number of firms	583	583	583	583
R-squared	0.22	0.23	0.22	0.23
	Panel C. DDD: By Norway and Listed			
Norway*Listed*Post 2006	0.310***	0.307***	0.153*	0.144*
	[0.091]	[0.091]	[0.082]	[0.084]
Observations	7,811	7,811	7,811	7,811
Number of firms	1,517	1,517	1,517	1,517
R-squared	0.15	0.15	0.24	0.24
Controls for board size & average number of other board seats		X		X

Notes: This table summarizes the results from firm-panel regressions of firms' log employment and log employee costs on variables indicating whether a firm is affected by the gender representation law and a set of controls. Results are reported using the three different samples of comparison firms reported in Table 4. Controls in all regressions include firm and year fixed effects, industry-specific time trends, and the appropriate uninteracted variables (i.e., indicators for Norway, listed, post-2006, and their pair wise interactions). Standard errors, adjusted for clustering at the firm level, are reported in brackets.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 7. Year-to-year changes in employment, DDD specification

	Downsizing: Decrease in employment			Expansion: Increase in employment		
	> 1%	> 3%	> 5%	> 1%	> 3%	> 5%
Mean of dependent variable	0.32	0.25	0.20	0.55	0.48	0.42
			Panel A. Main results			
Norway*Listed*Post 2006	-0.252***	-0.204***	-0.130**	0.080	0.098	0.110
	[0.065]	[0.063]	[0.060]	[0.071]	[0.072]	[0.072]
Observations	6,872	6,872	6,872	6,872	6,872	6,872
Number of firms	1,347	1,347	1,347	1,347	1,347	1,347
R-squared	0.04	0.04	0.03	0.03	0.03	0.03
			Panel B. With controls for other board characteristics			
Norway*Listed*Post 2006	-0.250***	-0.193***	-0.121**	0.074	0.089	0.117
	[0.066]	[0.062]	[0.060]	[0.072]	[0.073]	[0.073]
Controls for board size & average number of other board seats	X	X	X	X	X	X
Observations	6,872	6,872	6,872	6,872	6,872	6,872
Number of firms	1,347	1,347	1,347	1,347	1,347	1,347
R-squared	0.04	0.04	0.03	0.03	0.03	0.03

Notes: This table summarizes the results from firm-panel regressions of various measures of net changes in firms' employment on an indicator for whether the firm is affected by the gender representation law using the triple-difference framework reported in Table 4, columns 5 and 6. All regressions include controls for firm and year fixed effects, industry-specific time trends, and the appropriate uninteracted variables (i.e., indicators for Norway, listed, post-2006, and their pair wise interactions). Standard errors, adjusted for clustering at the firm level, are reported in brackets.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 8. M&A activity among listed firms before and after the quota

	Merger or Acquisition	Joint venture	Minority Stake
Norway			
Before quota	0.442	0.058	0.154
After quota	0.452	0.096	0.183
Change in Norway	0.010	0.038	0.029
Other countries			
Before quota	0.468	0.031	0.116
After quota	0.478	0.051	0.089
Change in other countries	0.010	0.020	-0.027
Difference-in-difference	-0.001	0.018	0.056
	[0.081]	[0.035]	[0.052]

Notes: This table reports the shares of listed companies that undertook a merger or acquisition, entered a joint venture, or invested a minority stake in another company at least once during the 3 year periods before (2003-2005) and after (2007-2009) the quota was adopted. Statisics are listed separately for companies in Norway and companies in other Scandinavian countries.

Table 9. Robustness check: Excluding firms with any government ownership, DDD specification

	Operating profit / Assets	Log employment	Log employee cost	Decrease in employment > 3%
Norway*Listed*Post 2006	-0.041**	0.287***	0.206*	-0.167**
	[0.020]	[0.111]	[0.110]	[0.075]
Observations	6,079	5,346	5,346	4,657
Number of firms	1,113	1,042	1,042	915
R-squared	0.05	0.17	0.22	0.05

Notes: This table tests whether the results presented above are robust to excluding 32 listed, Norwegian firms that had any direct or indirect government ownership and their matched control firms. Results are reported from firm-panel regressions of firms' profitability (operating profits divided by assets), log employment, log employee costs, and layoffs on variables indicating whether a firm is affected by the gender representation law and a set of controls. Results are reported using the triple-difference framework reported in Table 4, column 5. Controls in all regressions include firm and year fixed effects, industry-specific time trends, and the appropriate uninteracted variables (i.e., indicators for Norway, listed, post-2006, and their pair wise interactions). Standard errors, adjusted for clustering at the firm level, are reported in brackets.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 10. Analysis of pre-existing trends, DDD specification

	Operating profit / Assets	Log employment	Log employee cost	Decrease in employment > 3%
Norway*Listed*Post 2006	-0.040**	0.307***	0.190**	-0.166**
	[0.017]	[0.093]	[0.092]	[0.070]
Norway*Listed*Post 2003	-0.010	-0.165	0.107	0.124
	[0.021]	[0.126]	[0.120]	[0.076]
Observations	11,403	10,063	10,063	8,266
Number of firms	1,463	1,347	1,347	1,116
R-squared	0.06	0.11	0.24	0.03

Notes: This table tests for pre-existing trends. Results are reported from firm-panel regressions of firms' profitability (operating profits divided by assets), log employment, and log employee costs using the triple-difference framework reported in Table 4, column 5. In addition to on an indicator for whether the firm is affected by the gender representation law, the sample is extended back to 1999, and the specification includes a placebo indicator for listed, Noregian firms after 2003. All regressions include controls for firm and year fixed effects, industry-specific time trends, and the appropriate uninteracted variables (i.e., indicators for Norway, listed, post-2003, post-2006, and the appropriate pair wise interactions). Standard errors, adjusted for clustering at the firm level, are reported in brackets.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 11. Falsification check in the pre-quota period

	Operating profit / Assets	Log employment	Log employee cost	Decrease in employment > 3%
Norway*Listed*Post 2000	0.022	0.098	0.058	-0.003
	[0.022]	[0.083]	[0.085]	[0.069]
Observations	7,463	6,821	6,821	5,455
Number of firms	878	825	825	619
R-squared	0.04	0.04	0.12	0.04

Notes: As a falsification test, this table analyzes whether public Norwegian firms displayed a similar differential response to the previous global recession. Results are reported from firm-panel regressions of firms' profitability (operating profits divided by assets), log employment, log employee costs, and layoffs on variables indicating whether a firm is Norwegian, listed, and in the recession period, and a set of controls. Results are reported using a triple-difference framework similar to the regression reported in Table 4, column 5, except for the different time period. Controls in all regressions include firm and year fixed effects, industry-specific time trends, and the appropriate uninteracted variables (i.e., indicators for Norway, listed, post-2000, and their pair wise interactions). Standard errors, adjusted for clustering at the firm level, are reported in brackets.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 12. Heterogenous effects based on distance from compliance

	Operating profit / Assets	Log employment	Log employee cost	Decrease in employment > 3%
	Panel A. Sample of Listed Firms in Norway			
Post 2006*No Women in 2006	-0.042**	0.729***	0.718***	-0.037
	[0.019]	[0.134]	[0.097]	[0.064]
Post 2006*Some Women in 2006	0.003	0.487***	0.515***	-0.102**
	[0.010]	[0.082]	[0.077]	[0.046]
Observations	554	519	519	469
Number of firms	100	95	95	90
R-squared	0.02	0.26	0.29	0.01
	Panel B. DDD: Full Sample			
Post 2006*Norway*Listed	-0.045*	0.456***	0.247**	-0.239**
*No Women in 2006	[0.026]	[0.150]	[0.124]	[0.094]
Post 2006*Norway*Listed	-0.027	0.301**	0.225*	-0.196**
*Some Women in 2006	[0.019]	[0.117]	[0.116]	[0.087]
Observations	8,901	7,811	7,811	6,872
Number of firms	1,620	1,517	1,517	1,347
R-squared	0.004	0.12	0.14	0.02

Notes: This table tests for heterogeneous effects based on distance to compliance when the law was adopted in 2006. Results are reported from firm-panel regressions of firms' profitability (operating profits divided by assets), log employment, and log employee costs on indicator for whether the firm is affected by the gender representation law interacted with whether the firm's board contained any women in 2006, and a set of controls. Results are reported using two different specifications. Panel A limits the sample to affected firms and estimates raw, differential effects of the law based on whether the firm's board contained any women in 2006, with firm fixed effects but without any controls for baseline time trends. Panel B uses the full sample and estimates a quadruple-difference approach: the triple-difference framework reported in Table 4, columns 5 and 6, augmented with a fourth difference based on whether the firm's board contained any women in 2006. These regressions include controls for firm and year fixed effects, industry-specific time trends, and the appropriate uninteracted variables (i.e., indicators for Norway, listed, post-2006, some/no women in 2006, and the appropriate pair wise interactions). Standard errors, adjusted for clustering at the firm level, are reported in brackets.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 13. Changes in debt / assets

	DD: Within Norway by Listed		DD: Within Listed by Norway		DDD: By Norway and Listed	
	(1)	(2)	(3)	(4)	(5)	(6)
Norway*Listed*Post 2006					-0.010	-0.008
					[0.020]	[0.020]
Norway*Post 2006			-0.003	-0.003	0.002	0.000
			[0.017]	[0.017]	[0.008]	[0.009]
Listed*Post 2006	0.002	0.001			0.005	0.003
	[0.015]	[0.015]			[0.009]	[0.010]
Controls for board size & average number of other board seats		X		X		X
Observations	2,814	2,814	3,414	3,414	8,521	8,521
Number of firms	606	606	592	592	1,612	1,612
R-squared	0.07	0.07	0.08	0.08	0.03	0.03

Notes: This table summarizes the results from firm-panel regressions of firms' financial leverage (total debt divided by assets) on variables indicating whether a firm is affected by the gender representation law and a set of controls. Results are reported using the three different samples of comparison firms reported in Table 4. Controls in all regressions include firm and year fixed effects, industry-specific time trends, and the appropriate uninteracted variables (i.e., indicators for Norway, listed, post-2006, and their pair wise interactions). Standard errors, adjusted for clustering at the firm level, are reported in brackets.

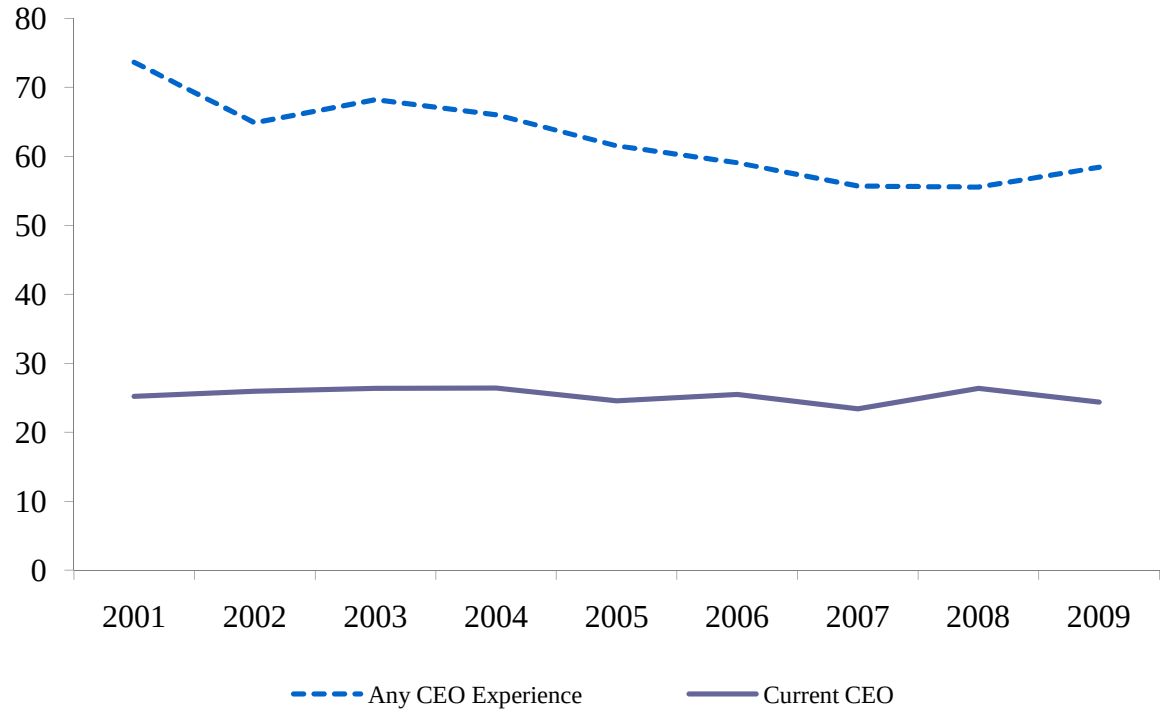
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 14. Heterogeneous effects based on CEO change

	Operating profit / Assets	Log employment	Log employee cost	Decrease in employment > 3%
	Panel A. DDD: Full Sample			
Norway*Listed*Post 2006*CEO Change	-0.060**	0.385***	0.241*	-0.251***
	[0.027]	[0.138]	[0.131]	[0.083]
Norway*Listed*Post 2006*No Change	-0.030*	0.269**	0.105	-0.179***
	[0.016]	[0.106]	[0.088]	[0.068]
Observations	8,901	7,811	7,811	6,872
Number of firms	1,620	1,517	1,517	1,347
R-squared	0.05	0.16	0.24	0.04
	Panel B. DDD: Full Sample Controlling for all CEO Changes			
Norway*Listed*Post 2006	-0.058**	0.384***	0.250*	-0.271***
	[0.028]	[0.140]	[0.133]	[0.085]
Norway*Listed*Post 2006*No Change	-0.031*	0.270**	0.101	-0.170**
	[0.016]	[0.106]	[0.089]	[0.068]
Observations	8,901	7,811	7,811	6,872
Number of firms	1,620	1,517	1,517	1,347
R-squared	0.05	0.16	0.24	0.04
	Panel B. DDD: Sample of CEO Changes Unrelated to Performance and Matched Controls			
Norway*Listed*Post 2006	-0.062	0.533**	0.457**	-0.218*
	[0.040]	[0.230]	[0.224]	[0.121]
Observations	1,307	1,152	1,152	1,019
Number of firms	238	224	224	202
R-squared	0.04	0.16	0.24	0.05

Notes: Panel A reports separate estimates of the impact of quotas for Norwegian listed firms whose CEO changed between December 2005 and December 2007 and those whose CEO did not change. Results are reported from firm-panel regressions using the triple-difference framework reported in Table 4, column 5. Controls in all regressions include firm and year fixed effects, industry-specific time trends, and the appropriate uninteracted variables (i.e., indicators for Norway, listed, post-2006, CEO change/No change and their pair wise interactions). To ensure the proper counterfactual is estimated, the CEO change and No change variables are defined for the matched controls based on the status of their matched listed firm, not their own realized value. In Panel B, the sample is restricted to affected firms with a CEO change that was classified as unrelated to firm performance based on firms' disclosures and press accounts, and these firms' matched controls. Standard errors, adjusted for clustering at the firm level, are reported in brackets.

*** p<0.01, ** p<0.05, * p<0.1



Appendix Figure 1. Average director CEO experience among public listed companies in Norway, 2001-2009. This figure plots summary statistics for the average percent of directors who are currently a CEO or have any current or previous CEO experience among public listed companies in Norway. Source: Ahern and Dittmar (2011), Table 2.

Table A1. Robustness to alternative samples, DDD specification

	Operating profit / Assets	Log employment	Log employee cost	Decrease in employment > 3%
Panel A. Include unlisted companies in treated group				
Norway*Listed*Post 2006	-0.026*	0.231***	0.137*	-0.123**
	[0.015]	[0.077]	[0.074]	[0.056]
Observations	15,928	13,837	13,837	11,768
Number of firms	2,967	2,751	2,751	2,327
R-squared	0.03	0.12	0.21	0.03
Panel B. Full sample of control firms				
Norway*Listed*Post 2006	-0.021*	0.380***	0.264***	-0.172***
	[0.011]	[0.074]	[0.067]	[0.043]
Observations	89,279	76,967	76,967	55,598
Number of firms	20,791	19,327	19,327	11,604
R-squared	0.02	0.10	0.26	0.02
Panel C. Limit sample to close matches				
Norway*Listed*Post 2006	-0.041***	0.314***	0.188**	-0.232***
	[0.016]	[0.091]	[0.090]	[0.063]
Observations	7,822	6,928	6,928	6,080
Number of firms	1,418	1,339	1,339	1,185
R-squared	0.05	0.15	0.20	0.04

Notes: This table tests whether the results presented above are robust to alternative sample selection and controls. Panel A uses all potential control firms (private Norwegian and other public and private Scandinavian firms). Panel B includes unlisted public limited Norwegian firms in the treated group. Panel C using the main sample and adds a full set of industry*year interaction terms (instead of linear trends). Results are reported from firm-panel regressions of firms' profitability (operating profits divided by assets), log employment, log employee costs, and layoffs on variables indicating whether a firm is affected by the gender representation law and a set of controls. Results are reported using the triple-difference framework reported in Table 4, column 5. Controls in all regressions include firm and year fixed effects, industry-specific time trends, and the appropriate uninteracted variables (i.e., indicators for Norway, listed, post-2006, and their pair wise interactions). Standard errors, adjusted for clustering at the firm level, are reported in brackets.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A2. Characteristics of male board members by tenure, 2009

	Male board members		
	Appointed before 2006	Appointed since 2006	p-value of difference
Professional experience			
Age	56.717	52.166	<.0001
	[0.796]	[0.816]	
CEO	0.268	0.347	0.123
	[0.038]	[0.035]	
CFO	0.058	0.068	0.700
	[0.020]	[0.018]	
Other current board positions	0.558	0.696	0.272
	[0.086]	[0.092]	
Other previous board positions	0.949	0.864	0.645
	[0.135]	[0.126]	
Education			
MBA	0.151	0.163	0.767
	[0.031]	[0.027]	
Other masters' degree	0.166	0.184	0.659
	[0.032]	[0.028]	
Ph.D.	0.022	0.037	0.409
	[0.012]	[0.014]	
Role on board			
Audit committee	0.324	0.314	0.936
	[0.081]	[0.080]	
Compensation committee	0.324	0.429	0.375
	[0.081]	[0.085]	
Nominating committee	0.206	0.086	0.164
	[0.070]	[0.048]	
Committee chair	0.353	0.229	0.263
	[0.083]	[0.072]	
Employee representative	0.158	0.246	0.047
	[0.031]	[0.031]	

Table A2. Characteristics of male board members by tenure, 2009 (cont.)

	Male board members		
	Appointed before 2006	Appointed since 2006	p-value of difference
Industry experience			
Consulting	0.101	0.105	0.911
	[0.026]	[0.022]	
Education	0.007	0.016	0.462
	[0.007]	[0.009]	
Energy	0.094	0.163	0.061
	[0.025]	[0.027]	
Engineering	0.159	0.221	0.157
	[0.031]	[0.030]	
Finance	0.196	0.132	0.127
	[0.034]	[0.025]	
High tech	0.022	0.026	0.789
	[0.013]	[0.012]	
IT	0.029	0.090	0.017
	[0.014]	[0.021]	
Law	0.073	0.047	0.354
	[0.022]	[0.016]	
Medicine	0.022	0.005	0.225
	[0.013]	[0.005]	
Pharmacueticals	0.029	0.016	0.437
	[0.014]	[0.009]	
Public sector	0.015	0.026	0.446
	[0.010]	[0.012]	
Tourism	0.000	0.005	0.319
	[0.000]	[0.005]	
Observations	139	191	

Notes: This table reports means and standard errors (in brackets) for various characteristics of male board members of listed firms in Norway in 2009. These firms are affected by the gender representation law. Average characteristics are reported separately based on when directors were appointed, along with the p-value associated with the difference in means. Fewer observations are available for age and committee memberships.

Table A3. Alternative measure of performance

	Operating Profits over Sales		
	(1)	(2)	(3)
	DD: Within Norway by Listed	DD: Within Listed by Norway	DDD: By Norway and Listed
Norway*Listed*Post 2006			-0.151**
			[0.074]
Norway*Post 2006		-0.149**	0.029
		[0.073]	[0.018]
Listed*Post 2006	-0.116*		0.043
	[0.064]		[0.035]
Controls for board size & average number of other board seats	X	X	X
Observations	2,680	3,411	8,430
Number of firms	579	587	1,582
R-squared	0.02	0.03	0.02

Notes: This table summarizes the results from firm-panel regressions of firms' profitability (operating profits divided by sales) on variables indicating whether a firm is affected by the gender representation law and a set of controls. Results are reported using three different samples of comparison firms to provide different counterfactuals for what would have happened to listed, Norwegian firms (the affected group) absent the reform: column 1 uses unlisted, Norweigan firms; column 2 uses listed, non-Norweigan firms; and column 3 uses both sets of comparison firms in a triple-difference specification. Controls in all regressions include firm and year fixed effects, industry-specific time trends, board size and the average number of other board seat, and the appropriate uninteracted variables (i.e., indicators for Norway, listed, and post-2006). Standard errors, adjusted for clustering at the firm level, are reported in brackets. The panel covers firms from 2003 through 2009 but is not balanced due to missing data.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$