

DISCUSSION PAPER SERIES

IZA DP No. 18125

**Board Gender Diversity and Workforce
Composition, Compensation, and
Retention for U.S. Publicly Traded Firms**

Tanya Byker
Sara Malik
Elena Patel
Jason Sandvik

SEPTEMBER 2025

DISCUSSION PAPER SERIES

IZA DP No. 18125

Board Gender Diversity and Workforce Composition, Compensation, and Retention for U.S. Publicly Traded Firms

Tanya Byker
Middlebury College

Sara Malik
University of Utah and IZA

Elena Patel
The Brookings Institution

Jason Sandvik
University of Arizona

SEPTEMBER 2025

Any opinions expressed in this paper are those of the author(s) and not those of IZA. Research published in this series may include views on policy, but IZA takes no institutional policy positions. The IZA research network is committed to the IZA Guiding Principles of Research Integrity.

The IZA Institute of Labor Economics is an independent economic research institute that conducts research in labor economics and offers evidence-based policy advice on labor market issues. Supported by the Deutsche Post Foundation, IZA runs the world's largest network of economists, whose research aims to provide answers to the global labor market challenges of our time. Our key objective is to build bridges between academic research, policymakers and society.

IZA Discussion Papers often represent preliminary work and are circulated to encourage discussion. Citation of such a paper should account for its provisional character. A revised version may be available directly from the author.

ISSN: 2365-9793

IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Board Gender Diversity and Workforce Composition, Compensation, and Retention for U.S. Publicly Traded Firms*

We use administrative data from the U.S. Census to estimate the effect of female director representation on workplace gender diversity and women's earnings. Using a difference-in-differences estimator that correctly accounts for variation in treatment timing, we show that first-time female director appointments lead to subsequent increases in workplace gender diversity. We find that the effects are driven by the improved retention of female workers in the middle and upper quartiles of the firm's overall earnings distribution. We find suggestive evidence that the effects are due to the newly appointed female directors' influence on corporate policy, as we observe stronger effects when the director is placed on one of the board's three core committees.

JEL Classification: J13, J30, J31

Keywords: corporate board, directors, wage, women, committee

Corresponding author:

Sara Malik
University of Utah
201 Presidents' Cir
Salt Lake City, UT 84112
USA
E-mail: sara.malik@eccles.utah.edu

* We thank participants of the Corporate Governance and Executive Compensation Research Series, the Drexel Corporate Governance Conference, the Haskayne and Fox Accounting Conference, and the Arizona Junior Conference for helpful feedback. We also thank seminar participants at the University of Colorado Boulder, the University of Southern California, Vanderbilt University, Brigham Young University, and MIT for their comments. We also thank Charles Guess for wonderful work as our research assistant. This research uses data from the Census Bureau's Longitudinal Employer Household Dynamics Program, which was partially supported by the following National Science Foundation Grants SES-9978093, SES-0339191 and ITR- 0427889; National Institute on Aging Grant AG018854; and grants from the Alfred P. Sloan Foundation. Any views expressed are those of the authors and not those of the U.S. Census Bureau. The Census Bureau has reviewed this data product to ensure appropriate access, use, and disclosure avoidance protection of the confidential source data used to produce this product. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2484 (CBDRB-FY23- P2484-R10810).

Comment on Tables

This paper uses confidential microdata from the U.S. Census Bureau. In recent years, the Census Bureau has become increasingly concerned about the accidental disclosure of confidential information and has imposed limits on the number of data points that can be extracted over the course of the project. To avoid prematurely hitting that threshold, we have opted to produce a subset of our tables using the sign and significance of our coefficients. We appreciate your understanding.

1 Introduction

Human capital is an increasingly important driver of firm value in the modern economy (Zingales, 2000; Regier and Rouen, 2023), prompting growing interest in how firms hire, compensate, and retain talent. This focus on employee outcomes has included interest in gender disparities in the workplace, leading to efforts to promote equity through leadership and board diversity initiatives, including quotas and mandates to appoint female directors (Greene et al., 2020; Gertsberg et al., 2021; Gormley et al., 2023). While the effect of board gender diversity on firm performance has been widely studied (Ahern and Dittmar, 2012; Eckbo et al., 2022; Edmans, 2024), less is known about its effect on employee outcomes, particularly outside of the Norwegian context (Bertrand et al., 2018). We address this gap in the literature by estimating the relationship between female director representation and employee-related outcomes for U.S. publicly traded companies using administrative employer-employee matched data from the U.S. Census.

Our study focuses on how first-time female director appointments (i.e., when a woman is added to a previously all-male board) relate to measures of workplace gender diversity. We anticipate a positive relation for several reasons. Tate and Yang (2015) suggest that having women in leadership positions leads to more female-friendly cultures within their firms, and Gormley et al. (2023) emphasize the vital role that directors play in helping diversity, equity, and inclusion (DEI) efforts to materialize within firms. Similarly, research suggests that female leaders may be likely to implement policies that specifically benefit women within the firm (Kowalewska, 2020; Athey et al., 2000; Gorman, 2005; Dobbin et al., 2011; Dancaaster and Baird, 2016). While male directors may also support similar policies, evidence suggests that women are more likely to prioritize issues that disproportionately affect women (Kurtulus and Tomaskovic-Devey, 2012; Sandberg, 2013; Kunze and Miller, 2017; Koning et al., 2021). Although there are examples of female directors leading such efforts (Nasdaq, 2019; Stoll, 2018), it remains unclear whether these actions are widespread or sustained enough to produce measurable changes in workplace gender diversity. Our study

complements and expands this literature by providing direct empirical evidence linking board composition to gender diversity across the broader workforce.

Our analysis uses a novel dataset that we construct by linking high-quality administrative data from the U.S. Census to detailed director information from BoardEx. Specifically, we draw workforce data from the Longitudinal Employer-Household Dynamics (LEHD) database, which matches wage employees to their employers, from 2005–2020. These data allow us to create firm-specific measures of workplace diversity based on employee composition and earnings outcomes at different levels of an organization’s hierarchy. We combine these data with the BoardEx database to determine the gender composition of boards of directors and to identify the timing of changes in board gender composition. This linked dataset enables us to examine how female representation on corporate boards affects employee-related outcomes.

We focus on firms that experience an extensive margin change in board gender diversity between 2006 and 2019. Approximately 32% of the firms in our sample add at least one woman to their previously all-male boards of directors; these are the “treated” firms in our empirical analysis. About 18% of the firms have all-male boards of directors throughout the entire sample period (i.e., “never treated” firms), while the remaining 50% have at least one woman on the board already when they first appear in our data—as described later, we omit these “always treated” firms from the analyses.

To estimate the effect of first-time female director appointments on workplace gender diversity, we use the estimator proposed by [Callaway and Sant’Anna \(2021\)](#). This econometric model accounts for the variation in treatment timing while avoiding biased comparisons between early- and late-treated firms that can arise from simple two-way fixed effects (TWFE) models. Following [Deshpande and Li \(2019\)](#), our control group consists of pre-treatment observations from firms that eventually appoint a female director later in the sample period. Our primary outcomes of interest are female labor share, equal to the fraction of the firm’s employees that are female, and female earnings share, equal to the fraction of total annual

earnings within the firm that are earned by women.¹

We show that the first-time appointment of a female to the board of directors leads to an increase in female labor share. We also find that this increase in female labor share is accompanied by a commensurate increase in female earnings share. These effects represent a 1%–2% increase in female labor and earnings share, relative to the control group mean. Importantly, we find evidence of common trends in the pre-appointment outcomes of treatment and control group firms, and our inferences are robust to using alternative control groups. Furthermore, we do not find that a firm’s performance, financial position, or pre-existing workforce gender composition reliably predict the timing of first-time female director appointments, which assuages some of the endogeneity concerns that are inherent in studies of board composition (Coles et al., 2008).

We find that the increases in female labor and earnings share are concentrated in higher-paying jobs, rather than being limited to low-wage positions. Specifically, we estimate the largest gains in the top three quartiles of the firm’s overall earnings distribution, where female labor and earnings share increase by 1.74%–2.98%. These results contradict the concern that improvements in gender diversity may be limited to lower-paying roles or come at the expense of more balanced representation in high-earning positions.

We find that the estimated increase in female labor share is driven by the retention of incumbent female employees, rather than by the increased hiring of women. To test this, we separately estimate changes in labor share among newly hired women and among existing female employees. We find that the latter group contributes to the observed increase, suggesting that first-time female director appointments enhance the retention of women already employed by the firm.

We also find that the increase in female labor share is especially pronounced among mothers of young children—an effect that is nearly twice the baseline estimate—suggesting that improved retention may be related to policy changes that particularly benefit working

¹We use “female” and “woman” interchangeably throughout the paper.

mothers. In contrast, we do not see similar increases among fathers of young children, consistent with women being more responsive to family-friendly policies. To further explore mechanisms, we investigate whether there is additional heterogeneity in our estimates across a wide range of subgroups based on employee characteristics. We do not, however, find evidence of changes in the labor or earnings share of other subgroups based on education, age, race, or ethnicity, which is consistent with the notion that female directors are likely to address issues that are especially relevant for women, as opposed to issues that are more broadly relevant to other demographic groups.

Finally, we leverage employee review data from Glassdoor to investigate whether dimensions of employee sentiment change following the first-time appointment of female directors. We find that overall employee ratings significantly increase by 3.31% after a woman is appointed to the board for the first time. This increase in overall firm ratings appears to be driven by improvements in employee satisfaction with compensation and benefits, their firm’s culture and values, and their senior leadership. We also study the words Glassdoor contributors use when they review the “pros” of working at the firm. We find that the usage of the word “child” (including “children” and “childcare”) more than doubles in the years after a first-time female director appointment. Taken together, these findings provide additional suggestive evidence that female directors may be helping to facilitate more family-friendly work environments.

To address concerns about omitted variables and alternative explanations, we conduct several additional tests. First, we examine whether the impact of first-time female director appointments depends on the influence granted to the new appointee. We show that the effects on female labor and earnings share are strongest when the new female director is also appointed to one of three core board committees—compensation, nomination/governance, or audit. Our estimates are robust regardless of initial board size or whether the new director replaces an existing member or fills an expanded seat. In addition, searches of 10-Ks and proxy statements reveal no evidence of workplace policy changes being planned or announced

prior to the first female director appointment. Finally, we conducted phone interviews with several female directors who joined previously all-male boards in our sample period, and their commentary underscored the proactive efforts that they and other female directors make to foster diversity within the workplace. Taken together, these findings reduce the concern that the observed increases in female labor and earnings share are driven by a concurrent managerial initiative to attract and/or retain women in the firm, with the first-time appointment of a female director being a symbolic gesture (e.g., tokenism ([Gormley et al., 2023](#))).

We make several important contributions to the literature. We are among the first to consider the effects of board gender composition on the outcomes of rank-and-file employees. [Bertrand et al. \(2018\)](#) consider these effects in the context of Norway’s board gender quota law, and they conclude that the mandate “had very little discernible impact on women in business beyond its direct effect on the women who made it into boardrooms.” In contrast, we find evidence that increased female representation on corporate boards does impact the outcomes of the non-director female workers in the firm. One potential reason for the difference in results is that at the time of the Norwegian mandate, Norway already had relatively progressive policies related to maternity leave and other family support. This and the presence of other pre-existing female-friendly policies among Norwegian firms potentially reduced the scope for the newly appointed female directors of Norwegian firms to implement policy and organizational changes that improved outcomes for female workers. The United States, on the other hand, does not guarantee parental leave, nor are there legal mandates that impose female-friendly workplace policies, which means that female directors in the U.S. likely have greater scope to enact policies that improve the working environments for the women in their firms.

Other research has used alternative settings to study the effects of female leadership on employee outcomes in the U.S. context. [Matsa and Miller \(2011\)](#) estimate a positive relationship between female director representation and the hiring of female executives, but

they do not consider the impact on workplace gender diversity at other levels of the organization. [Tate and Yang \(2015\)](#) consider the effect of female managers in reducing gender pay gaps among newly-hired rank-and-file workers, and they conclude that female leadership contributes to female-friendly cultures in the workplace. Our work adds to the findings of these previous studies, as we consider additional employment outcomes beyond gender pay gaps, including workforce composition, and as we examine effects at different levels of the organization’s hierarchy. Furthermore, our focus on board gender diversity, as opposed to C-suite diversity, allows us to relate our findings to some of the recent external pressures for increased female director representation within corporate boards ([Gormley et al., 2023](#)).

Our paper is also related to two concurrent working papers that explore the effects of board diversity on employee-related outcomes. [Cai et al. \(2022\)](#) use data from LinkedIn and find evidence of increased workplace diversity, based on gender and race, following the appointment of a diverse director (i.e., a women or a racial minority). [Bian et al. \(2023\)](#) present evidence that California-based companies responded to the passage of the state’s board gender quota law by implementing gender bias in job ads, wherein they appear to use more masculine language to attract male candidates. They interpret these findings as evidence of backlash against female labor market participants in response to board gender quotas. Our use of Census data allows us to overcome several of the limitations of these studies, and it allows us to address some of the remaining gaps in the literature. First, our measures of female labor and earnings share are not biased by the self-selection concerns and wage estimation issues that are inherent in data that relies on social media profiles to estimate workforce compositions. Second, the data on employee gender and race in the U.S. Census records are much more reliable than are the gender and race indicators that data vendors impute based on a person’s name. In addition, our assessment of staggered voluntary female director appointments—as opposed to mandated appointments—is more relevant to the current regulatory environment, as board diversity requirements are being repealed ([Ramonas, 2024](#)).

2 Background and Related Literature

To set up the motivational framework for this study—and to understand why female director representation might impact workplace gender diversity—we discuss some of the existing research on the unique obstacles that women face in the workplace. We then discuss the studies that have examined the role of corporate leaders, specifically corporate directors, in mitigating these obstacles.

2.1 Obstacles Faced by Women in the Labor Market

Although women have made great strides in the workforce in the last 50 years (Goldin, 2006, 2014), substantial gender disparities in labor market outcomes persist. For example, the gender pay gap remains widespread and cannot be fully explained by occupational segregation or other observable differences between men and women (Goldin, 1990; Olivetti and Petrongolo, 2016; Blau and Kahn, 2017; Fortin et al., 2017; Blau and Kahn, 2020). Furthermore, women continue to be under-represented in mid-level management positions (OECD, 2017), and they hold less than 12% of the positions as top executives (Matsa and Miller, 2011; Huang and Kisgen, 2013; McShane and Pye, 2024). This disparity in leadership representation persists even though women have surpassed men in college enrollment rates (Charles and Luoh, 2003; Goldin et al., 2006; NSC, 2020).

Multiple explanations have been proposed for the persistent gender disparities in labor market outcomes. A common supply-side explanation is the decision of many women to take extended time out of the labor force to raise children (Lundberg and Rose, 2000; Goldin and Katz, 2002; Anderson et al., 2002; Bertrand et al., 2010; Miller, 2011). Others point to gender differences in competitiveness (Niederle and Vesterlund, 2007), though evidence in support of this notion is mixed and may reflect cultural norms rather than innate traits (Gneezy et al., 2009; Buser et al., 2014). While negotiating behavior is also often cited, Leibbrandt and List (2015) show that women are just as likely as men to negotiate for higher wages

in settings where wage negotiation is standard. More recently, a growing body of work attributes the majority of the modern gender wage gap to the so-called child penalty, or the sharp, persistent drop in women’s earnings relative to men’s following childbirth (Kleven et al., 2019a,b).

In addition to supply-side barriers, women may face demand-side barriers in the form of statistical or taste-based discrimination, which can reduce their likelihood of being hired, promoted, or retained in certain settings (Neumark et al., 1996; Goldin and Rouse, 2000; Booth and Leigh, 2010; Bagues and Esteve-Volart, 2010; Sin et al., 2017). Conditional on being hired, women may face an uphill battle in their career development if they have inequitable access to mentors (Athey et al., 2000). The refusal of some male leaders to mentor women provides an additional explanation for why women are under-represented in management positions, especially at the highest levels of the organization (Waldman, 2017). Along these lines, recent research finds that some men exhibit a negative preference for working with and for women, in part because they believe that they will have a more difficult time receiving promotions if they have more female coworkers and managers (LaViers and Sandvik, 2022). Taken together, there is ample evidence that women have faced (and continue to face) unique obstacles in the labor market; obstacles that can potentially be mitigated by the proactive efforts of a company’s leaders, such as its directors.

2.2 Corporate Directors’ Ability to Address Obstacles

While legislation and regulations have been enacted to address some of these obstacles,² many of the regulatory policies that concern workplace diversity are being repealed (Ramonas, 2024; Carpenter and Graber, 2025). This raises an important question: what can organizational leaders do to mitigate the obstacles faced by their current and prospective female employees?

Corporate boards of directors hold substantial authority over firm decision-making, and

²Consider, for example, anti-discrimination laws in the U.S., the Norwegian board gender quota, California Senate Bill No. 826, and the Cope-Zimmermann law in France.

directors are well-positioned to advise executives about human capital management policies and practices. In their role as monitors, directors aim to maximize shareholder value by setting optimal executive compensation contracts and by replacing poor-performing leadership (Fama, 1980; Jensen and Murphy, 1990; Coles et al., 2006; Jenter and Kanaan, 2015). At the same time, their advisory contributions often reflect their individual expertise, skills, and experiences (Ellis et al., 2018; Chen et al., 2020). Kim and Starks (2016) suggests that female directors contribute unique perspectives to the board room. Especially in light of women’s growing participation in the workforce (Goldin, 2006; BLS, 2020), female directors can play an important advisory role regarding how a company can attract and retain female employees.

Kowalewska (2020) highlights that increased female representation on corporate boards can lead to two types of trickle-down benefits for other women within the firm. First, female directors can create new opportunities for women through targeted hiring practices and mentorship programs (Athey et al., 2000; Gorman, 2005; Kurtulus and Tomaskovic-Devey, 2012). Second, female directors may help enact policies that facilitate a more female-friendly workplace. For example, Dobbin et al. (2011) show that firms with a greater proportion of female leaders are more likely to adopt firm-wide diversity programs. In addition, Lancaster and Baird (2016) show that female leadership is a strong predictor of the uptake of work-care arrangements, such as work-from-home flexibility and leeway in workday start and end times.

While executives also have the authority to address barriers facing women in the workplace, corporate directors may be better positioned to do so. Directors are typically older than executives (Fracassi and Tate, 2012; Adams et al., 2018), are often retired, and are more likely to have previously served as a CEO (Kang et al., 2018), making them less concerned about any career risks associated with challenging the status quo among male and female employees. Directors also commonly sit on multiple boards simultaneously (Ferris et al., 2003), giving them broader exposure to policy innovations across firms and the ability to

bring these ideas back to the companies they oversee. Together, these factors make corporate directors uniquely well-suited among business leaders to drive policy changes that support gender equity in the workplace.

Some female directors are especially well-known for their proactive efforts to enact programs and policies that benefit women in the workforce. For instance, Betsy Atkins, who has served as a director of Volvo Cars and Wynn Resorts, often interacts with and mentors the female employees at Volvo and has organized networking events for the female employees of Wynn (Stoll, 2018). One of Atkins’s fellow female board members, Dee Dee Myers, has said, “Betsy ... and I not only bring our unique perspectives and experiences to our roles, but we also bring new skills, and that has been helpful as the board develops and rolls out strategies to address a range of challenges, from improving workplace diversity and inclusion, to compliance, compensation and communications” (Nasdaq, 2019). These anecdotes highlight a few examples of female directors actively improving the work environment for female employees, but it is not clear if such efforts are ubiquitous or intense enough to lead to significant changes in a firm’s workplace gender diversity.

2.3 Existing Research on Board Gender Diversity

The introduction of board gender diversity mandates, such as those in Norway, the European Union, and California, has spurred a large empirical literature examining the follow-on effects. However, the findings in this area are mixed, particularly with regards to firm performance. Several influential studies of quota laws implemented in Norway and enacted—but subsequently reversed—in California find negative effects on firm value and profitability among firms required to add women to boards (Ahern and Dittmar, 2012; Matsa and Miller, 2013; Greene et al., 2020). Moreover, Bertrand et al. (2018) find little evidence that Norway’s quota improved outcomes for women beyond those appointed to board seats, raising questions about whether increased board diversity leads to broader gains for female employees.

At the same time, however, other studies have challenged these conclusions. [Eckbo et al. \(2021\)](#) find that Norway’s quota did not have a statistically significant effect on firm value, and they emphasize several of the difficulties in establishing causality based on quotas. Similarly, [Allen and Wahid \(2024\)](#) suggest that California’s Senate Bill No. 826 had neutral or even slightly positive effects on firm value, and [Greene et al. \(2025\)](#) have attempted to reconcile the mixed empirical evidence on the topic. Beyond these policy contexts, a broader literature has explored the relationship between board diversity and firm-level outcomes like risk-taking, acquisitions, IPO pricing, and R&D ([Adams and Ferreira, 2009](#); [Huang and Kisgen, 2013](#); [Levi et al., 2014](#); [Sila et al., 2016](#); [Schwartz-Ziv, 2017](#); [Bernile et al., 2018](#); [Rau et al., 2024](#)).

Despite this growing body of work, there remains relatively little empirical evidence on how board gender diversity affects the composition and compensation outcomes of rank-and-file workers. This is largely due to limitations in accessing detailed workforce data. Most studies rely on firm-level or board-level performance metrics that are readily reported or easily computed based on public information and financial statements, but these datasets lack the granularity needed to study internal labor dynamics. We fill this gap by using high-quality administrative data from the U.S. Census to directly measure how first-time female director appointments affect the composition and compensation of a firm’s workforce. Specifically, we estimate the effect on female labor share and earnings share—two important indicators of gender equity in employment outcomes.

3 Data Compilation

Our analysis is based on a novel panel dataset that links commercially available data describing the composition of boards of directors with confidential, administrative employer-employee matched data from the U.S. Census. These data allow us to study the dynamic relationship between board gender diversity and firm-level trends in workforce diversity and

earnings outcomes.

3.1 Board of Directors Data: BoardEx

BoardEx provides detailed information about the composition of boards of directors, including longitudinal information about individual board members and the firms that they advise. These data include director-specific information, such as gender, in addition to board-specific information (e.g., committee assignments and the start and end date of each appointment).

We gather data from BoardEx from 2001 to 2020, and we study those firms that can be matched to the CRSP-Compustat data (Center for Research in Security Prices linked to Compustat). The CRSP-Compustat data is used to measure firm characteristics like size, profitability, and financial structure. Across our sample period, there are over 8,000 publicly listed firms in the CRSP-Compustat data which also have data availability for their boards of directors in BoardEx.

Figure 1 summarizes recent trends in board gender diversity. The fraction of organizations with at least one woman on their board has steadily increased over the last two decades, as depicted by the blue (top) line in the chart. In the mid-2000s, less than 50% of firms had gender-diverse boards.³ That number increased to nearly 80% by 2019. At the same time, the overall fraction of board seats occupied by women continues to be relatively small, as illustrated by the green (bottom) line. In 2006, female directors occupied less than 10% of the overall board seats for the average company. By 2019, their share of the total board seats doubled, but remained below 20%. Consistent with this, the red (middle) line in the chart shows that most firms do not have more than one female director, with the fraction of firms with two or more female directors starting below 20% in 2006 and growing to around 40% by 2019. In summary, female representation on corporate boards remains disproportionately low despite recent changes in the likelihood of adding at least one female director to the

³As we describe in more detail below, we focus on instances in which boards appoint a female director for the first time from 2006 to 2019, as this treatment window is fully covered by the data from the U.S. Census.

boards of most organizations.

We leverage the staggered timing of first-time appointments of women to corporate boards across firms to estimate the relation between female director representation and the employment outcomes of interest. We are primarily interested in the effect of an *extensive* margin change in board gender composition. We identify firms that transitioned from no gender diversity on the board (i.e., only male directors) to appointing at least one female director. These firms are categorized as “treated” firms. Firms with all-male boards throughout the entire observation period are categorized as “never treated” firms. Finally, firms that we observe with gender-diverse boards—those with at least one woman on the board—in their first year of observation are categorized as “always treated” firms.

3.2 Employee Composition and Earnings Data: U.S. Census

We access employer-employee matched data using the U.S. Census Longitudinal Employer-Household Dynamics (LEHD) database. The LEHD database includes wages by worker for all wage employees, as well as information about the worker’s employer. LEHD coverage is comprehensive: approximately 96% of all private-sector jobs are included in the LEHD files (Abowd et al., 2005). Annual earnings include wages that are covered by the state’s unemployment insurance program: salaries, bonuses, equity, tips, and the dollar value of other perquisites (e.g., meals, housing, and retirement contributions) (BLS, 2016).⁴ The longitudinal nature of these data permit us to characterize employee outcomes, both within and across employers.⁵ We merge the LEHD data with other datasets from the U.S. Census that include expanded demographic data. We use these auxiliary datasets to append information describing the age, gender, nationality, number of children and dates of child birth,

⁴The LEHD is constructed from unemployment insurance records that employers submit to state agencies. Accordingly, it captures only wage and salary workers who receive IRS Form W-2 and are subject to FICA payroll taxes. Independent contractors and other nonemployee workers paid via Form 1099-MISC are not included in the LEHD.

⁵Employees are matched to their employer according to the employer’s state employer identification number. We rely on Census crosswalks to combine multi-state employers that are associated with the same parent employer. In this way, we study employment that is aggregated across multiple subsidiaries of a single parent.

and race of employees within a firm.⁶

We limit our sample to employer-employee-year tuples for workers aged 20 to 59. We exclude workers who report less than \$8,000 in average annual wages over their lifetime earnings as captured by the LEHD. We also exclude workers who work fewer than two quarters each year on average as reported in the LEHD. We use Census resources to ensure that the reported employer is the parent-level organization. Finally, we limit our analysis to those firms that employ at least one male worker and at least one female worker in aggregate. The resultant dataset of firm-year level employment-outcomes extends from 2005 to 2020, from one year before the first treatment year (2006) to one year after the last treatment year (2019).

3.3 Summary Statistics

Table 1 reports summary statistics describing matched firms in our dataset, i.e., those firms in the BoardEx-CRSP-Compustat data that match to employer-employee data within the U.S. Census (approximately 4,650 firms).⁷ The top row reports the number of firms that fall into each of the three treatment groups. First, roughly 50% of firms have gender-diverse boards in the first year they are observed in the data—as we will later explain, we will exclude these always treated firms from all empirical analyses. Second, about 18% of firms retain non-gender-diverse boards of directors throughout our entire observation period (never treated). Third, and most importantly, extensive margin changes in board gender diversity occur frequently in our sample period, as roughly 32% (1,500) of firms are observed to transition from non-gender-diverse to gender-diverse boards of directors between 2006 and 2019. In Section 4, we describe how we define and compare treated and control firms in a

⁶The binary nature of our data requires that we categorize workers as male or female. Although this does not fully capture the experience of non-binary individuals and those of other genders, our results still inform the on-going debate on how board gender composition impacts the outcomes of rank-and-file employees. We use “male” and “female” interchangeably with “man” and “woman,” respectively, throughout the manuscript, with the intent being to capture an individual’s self-reported gender, not necessarily their sex assigned at birth.

⁷Note, all summary statistics have been rounded in concordance with the confidentiality protections required by the U.S. Census Bureau.

staggered difference-in-differences framework to study changes in employee-related outcomes that occur in the years after the first female director is added to the board.

We use the employer-employee matched data to create several firm-level summary measures of employment and earnings outcomes. The two primary outcomes of interest are (i) *Female Labor Share*, equal to the fraction of the firm’s employees that are female, and (ii) *Female Earnings Share*, equal to the fraction of total annual earnings within the firm that are earned by women. In [Table 1](#), we report summary statistics describing the treated and never-treated firms in our sample, which includes approximately 20,500 firm-year observations from 2,350 unique firms. For treated firms ($N = 1,500$), we separately report summary statistics before and after treatment in Columns (1) and (2), respectively. We report statistics for never treated firms in Column (3) ($N = 850$). Finally, we report statistics for the approximately 2,300 always-treated firms—which are excluded from our analysis—in Column (4) to provide additional context about the population of firms in our matched dataset. The average level of female labor share is 37.1% before treated firms appoint a female director for the first time, and it is 40.1% in the years after the appointment. The average level of female earnings share is 28.2% before treated firms appoint a female director for the first time, and it is 31.9% in the years after the appointment. Never treated firms have average female labor and earnings share values that are similar to treated firms in the pre-treatment period, whereas always treated firms have larger values than both eventually treated and never treated firms.

Next, we examine firm and board size characteristics, which generally indicate that treated firms are larger than never-treated firms. In the post-treatment period, treated firms tend to have one more director, on average, than treated firms in the pre-treatment period (with 8.3 and 7.1 directors, respectively). Never-treated firms tend to have smaller boards (with 6.5 directors), while always-treated firms tend to have larger boards (with 9.2 directors). For additional context, we show that post-treatment firms and always treated firms have relatively similar values of female CEO presence, total fraction of female directors

on the board, and total fraction of female directors on the three key committees. The table also reports summary statistics for return on assets, leverage, and firm size, and the female share of employees in the top versus the bottom of the firm’s earning distribution, across the four groups of firms.⁸

4 Empirical Methods

To estimate the effect of female director representation on the outcomes of interest, we leverage variation in the timing of first-time appointments of female directors across firms. We posit that such extensive margin changes are likely to be particularly relevant for employee-related outcomes, as female directors may implement business practices and policies that appeal to and benefit female workers. We estimate these effects using the estimator for staggered difference-in-differences proposed by [Callaway and Sant’Anna \(2021\)](#). This approach avoids the common confounding effect of a standard staggered treatment design, wherein researchers could incorrectly incorporate the comparison between already treated firms with just treated firms ([Barrios, 2021](#)). This errant comparison can bias treatment effect estimates, especially when treatment effects are dynamic ([Baker et al., 2022](#)). Following this literature, we exclude always treated firms from the empirical tests.

The [Callaway and Sant’Anna \(2021\)](#) approach separately considers the treatment effects across treatment cohorts; in our setting, treatment cohorts are defined by the year in which a firm first appoints a female director, g . We estimate cohort- and time-specific average treatment effects on the treated firms, $\widehat{ATT}(g,t)$, using two-period/two-group difference-in-differences estimators. We report event-study treatment effects by aggregating together cohort- and time-specific estimates in each year following treatment, weighted by the size of each treatment cohort. In our setting, $G_j = g \in \{2006, \dots, 2019\}$ represents treatment cohorts (i.e., all firms j that are treated in year g), and $C_j = 1$ for control group firms. An unconditional estimator for $ATT(g,t)$, the average effect of a first-time female director

⁸Variable definitions are provided in [Table A.1](#) in the Appendix.

appointment for firms in cohort g at time $t \geq g$ is

$$\widehat{ATT}_{un}^{eve}(g, t) = \frac{\sum_j \Delta Y_{j,g-1,t} \mathbb{1}\{G_j = g\}}{\sum_j \mathbb{1}\{G_j = g\}} - \frac{\sum_j \Delta Y_{j,g-1,t} \mathbb{1}\{C_j = 1\}}{\sum_j \mathbb{1}\{C_j = 1\}}, \quad (1)$$

where $\Delta Y_{j,g-1,t} = Y_{j,t} - Y_{j,g-1}$. The identifying assumption of this approach is one of parallel trends in outcomes among firms in a given treatment cohort and those in the control group. We provide evidence in Section 5 that suggests this assumption is likely satisfied in our setting.

For a given treatment cohort, g , we identify two control groups of firms that did not experience any change in female director representation in year g : (1) those firms that add women to their boards of directors for the first time in some later period $n > g$ (i.e., not-yet treated firms); and (2) those firms that we never observe as having gender-diverse boards in the sample period (i.e., never treated firms). As discussed by [Deshpande and Li \(2019\)](#), one benefit of using not-yet treated firms as a control group is that this design allows us to exploit variation in the timing of first-time female director appointments, rather than variation in the occurrence of such appointments.⁹

These control groups present two distinct types of counterfactual outcomes. On the one hand, the never treated control group captures concurrent trends in employee-related outcomes that are, by definition, unaffected by gender diversity on boards of directors in any time period. On the other hand, the never treated control group may differ from the set of firms that choose to add a woman to their board of directors in unobservable ways. This highlights the contribution of the not-yet treated control group and reflects the reason that we choose the not-yet treated control group for our baseline analyses. Not-yet treated firms are likely to be selected on the same unobservable characteristics that lead our treated firms to add a woman in year g . Later, we empirically test and find evidence in support of this assumption. In addition, we conduct sensitivity analyses to highlight the robustness of

⁹See [Guryan \(2004\)](#) for another example of using not-yet treated units as a control group, as opposed to never treated units.

our results based on whether our control group comprises only not-yet treated firms, only never treated-firms, or both. In our empirical estimations, we also include firm-year level controls for return on assets, leverage, firm size, and number of employees two years before the appointment.¹⁰

Figure 2 depicts the variation in the timing of treatment among the treated firms that we study. The graph shows that the number of firms appointing female directors for the first time ranges between 60 and 150 in any given year, with relatively more firms adding women towards the end of our treatment period. We leverage this variation in treatment timing across the sample period to identify effects using the [Callaway and Sant’Anna \(2021\)](#) estimator.

We acknowledge that the decision to appoint a female director can be endogenous to other decisions a firm may be in the process of implementing with regards to employee hiring, compensation, and turnover. To address this concern, we will conduct a wide array of heterogeneity and robustness tests, the results of which help to reduce the likelihood that our findings are driven by omitted variables bias. Of course, no method short of random assignment will completely eliminate the influence of endogeneity, but these additional tests provide increased confidence in the inferences we draw from the results.

5 Results

We begin with a discussion of our main results in [Section 5.1](#). We first document a significant, positive effect on a firm’s female labor and earnings share. Next, we explore where within the company’s earning distribution such changes occur, showing that our results are driven by employees in the top three earnings quartiles, and we present evidence that the effects are driven by improved retention, as opposed to differential hiring. Then in [Section 5.2](#), we evaluate a key identification strategy: among treated firms, we cannot predict when treatment will occur. For this analysis, we estimate a hazard model of the timing of first-

¹⁰Our results are not sensitive to the inclusion or exclusion of these control variables.

time female director appointments, finding no evidence of a predictable pattern based a firm’s financial circumstances or workforce compositions. This reduces much of the concern that our findings are driven by omitted variables bias. Finally, in Section 5.3, we explore the possible mechanisms that connect first-time female director appointments to increased female labor and earnings share. We specifically consider how the compositional changes are experienced among particular subgroups of employees, including parents of young children.

5.1 Effects on Female Labor Share and Earnings Share

Table 2 reports that baseline effect of a first-time female director appointment on the outcomes of interest. In Column (1), the dependent variable is the fraction of the firm’s employees that are female (i.e., “female labor share”). The estimate on *Pre-Treat* captures the aggregate difference in the dependent variable in years two and three before the appointment, relative to the year before the appointment (i.e., the omitted reference period). The small, statistically insignificant estimate indicates that there was no average differential pre-trend between treatment and control firms, consistent with the usual parallel trends assumption required of any difference-in-differences analysis. In contrast, the larger, statistically significant estimate on *Post-Treat* indicates that first-time female director appointments are associated with a significant subsequent increase in female labor share. The effect size of 0.563 percentage points reflects a 1.4% increase in female workplace representation, relative to the control group mean of 0.395.

Below the average treatment effect estimates in Column (1), we report the corresponding event-year effects. This allows us to more comprehensively test the parallel trends assumption, and it allows us to observe the dynamic nature of the treatment effects of first-time female director appointments on the outcomes of interest. The estimates on $t - 3$ and $t - 2$ are both small and statistically insignificant, again suggesting that there was no differential pre-trend between treatment and control firms before the appointments. The estimate on $t = 0$ is also indistinguishable from zero, suggesting that female labor share does not

significantly change in the same year of the first-time female director appointment. The positive coefficients on $t + 1$ to $t + 4$ capture a significant increase in female labor share from year one to year four after the appointment, with the magnitudes representing a 1%–2.6% increase relative to the control group mean.¹¹ **Figure 3** plots the event-year differences in female labor share between treated firms and control firms, providing a clear visual of the uptick in female labor share following the first-time appointment of a female director.

In Column (2) of **Table 2**, we repeat the same analysis with an alternative dependent variable: the fraction of total annual earnings within the firm that are earned by women (i.e., “female earnings share”). This provides a test for whether the increase in female labor share is met with a commensurate increase in female earnings share. For instance, if after a first-time female director appointment, women experience increased representation in low-paying positions, but decreased representation in high-paying position, then the overall effect of the appointment on female earnings share could be null or even negative. On the other hand, if the director appointment leads to increased representation of women in high-paying positions, then the effect on female earnings share should be positive. Consistent with the latter, we find evidence of a positive and statistically significant treatment effect of first-time female director appointments on female earnings share. The estimate on *Post-Treat* captures an increase in female earnings share of about 2% of the control group mean. As in Column (1), we find no evidence of differential pre-trends between treatment and control group firms, and the post-treatment effect is positive and statistically significant in each year from one to four after the appointment.

As previously described, we use not-yet treated firms as the basis for comparison in our main tests in **Table 2**. In **Table A.2**, we show that our inferences are very similar when we use alternative control groups. In Columns (1) and (3) we use only never treated firms in the control group, and in Columns (2) and (4) we use both not-yet treated and never treated firms in the control group. The estimates on *Post-Treat* are always statistically significant at

¹¹The control group mean values reflect the average outcome measured two years prior to treatment.

either the 1% or 5% level, and the magnitudes of the effects are similar to the corresponding estimates in [Table 2](#). Furthermore, in all four specifications we continue to find evidence that there was no differential pre-trend between treatment and control firms before the first-time female director appointments.

As previously described, we follow [Deshpande and Li \(2019\)](#) and prioritize the not-yet treated control group throughout the subsequent analyses, unless specified otherwise. We make this choice because not-yet treated firms are likely more representative of the currently-treated firms that make up the treatment group. Said another way, one might be worried that firms that appoint female directors for the first time are different from firms that never do in unobserved ways that also contribute to changes in female labor and earnings share. By using not-yet treated firms as the basis for comparison, we hold fixed the unobserved “firm type” distinction and identify effects based on quasi-random variation in the timing of when first-time female director appointments occur. In [Section 5.2](#), we provide statistical evidence that is consistent with this assumption about the timing of treatment.

5.1.1 Effects Across the Earnings Distribution

In the previous section we presented evidence that both female labor share and female earnings share increase following the first-time appointment of a female director. Next we identify precisely where within the firm’s hierarchy of positions these effects take place. The LEHD data does not include information about job titles or position ranks, so we proxy for a worker’s position within the firm’s hierarchy using their position within the firm’s overall earnings distribution. Specifically, we re-compute our main dependent variables, female labor share and female earnings share, within each quartile of the firm’s earnings distribution.¹² We then re-estimate our baseline tests separately for each of the four earnings-quartile-specific outcome variables.

¹²We characterize each firm’s earnings quartiles based on the prior year’s within-firm earnings distribution. We then bucket current-year employees based on their position within prior-year earnings quartiles. We use prior-year earnings quartiles to avoid any confounding effect of within-year earnings or composition changes.

We tabulate the results in [Table 3](#). We do not find any evidence of changes in female labor share or female earnings share among the lowest paying jobs, as shown by the small and statistically insignificant estimates on *Post-Treat* in Columns (1) and (5), respectively.¹³ The bottom row of the table reports the control group mean of the dependent variable in each specification, and it shows that at baseline women realize about 50% of the labor share and earnings share among jobs in the lowest earnings quartiles (48.9% and 49.5%, respectively). Thus, the absence of a treatment effect within this quartile of the earnings distribution may be due to the pre-existing gender-parity among those jobs.

We find the greatest effect of first-time female director appointments on female labor and earnings share within the second earnings distribution quartile (i.e., 25–50%). Column (2) shows that female labor share increases by 2.77% relative to the control group mean, and Column (6) shows that female earnings share increases by about the same amount. We also find significant, positive effects in the top two quartiles of the earnings distribution, with increases in female labor share (earnings share) equal to 1.74%–2.48% (1.74%–2.98%) of the control group mean, as shown in Columns (3) and (4) (Columns (7) and (8)). Taken together, these findings are inconsistent with the concern that the increases in female labor share are only present in low-paying jobs. Instead, the increase in female worker representation is realized among middle- and high-paying jobs.

5.1.2 Are the Effects Due to Changes in Hiring or Retention?

The observed increase in female labor share could be driven by an increase in the hiring of women, relative to men, or by differences in the retention of female employees, relative to male employees, or some combination of both. Our next set of results indicate that the effects are most likely due to differences in retention, not hiring. Specifically, we create a new outcome variable, *incumbent* female labor share, equal to the fraction of the firm’s employees

¹³In this and other tables, we present a mix of quantitative and qualitative results. Qualitative results indicate the sign and statistical significance of an estimate without disclosing its magnitude. Throughout our analysis, we balance quantitative and qualitative reporting to mitigate disclosure risks associated with these estimates.

that are female and incumbents (i.e., they were present in the firm the year before), and we also create the outcome variable *newly hired* female labor share, equal to the fraction of the firm’s employees that are female and recently hired (i.e., they were not present in the firm the year before). We then separately estimate the effects of first-time female director appointments on these two outcomes.

In Column (1) of [Table 4](#), we find evidence that incumbent female labor share significantly increases following a first-time female director appointment. The increase is statistically significant in the year of the appointment and in each of the four years afterwards. In contrast, in Column (2), we do not find any evidence of changes in newly hired female labor share. The estimate on *Post-Treat* is small, negative, and statistically insignificant, and none of the event-year post-treatment effects are significant.¹⁴ These results suggest that the observed increase in overall female labor share is not due to differential hiring initiatives, where women are recruited more heavily than men. Instead, the overall effects appear to be driven by improvements in the retention of the firm’s pre-existing female employees.

5.2 Timing of Extensive Margin Changes in Board Diversity

Next, we conduct tests that evaluate whether first-time female director appointments are predictable based on the firm’s performance, financial position, or the pre-existing demographic composition of its workforce. To do this, we estimate a reduced-form hazard model by studying the likelihood of a firm becoming treated in each year between 2006 and 2019, conditional on not having been treated up to that point, following [Deshpande and Li \(2019\)](#). In any given year, we estimate an ordinary least squares regression where the dependent variable equals one if the firm makes its first female director appointment in a particular year, and zero otherwise. The regressors are the firm’s return on assets, leverage, size (i.e., $\log(\text{assets})$), an indicator for whether the firm’s size is above the sample-wide median in the

¹⁴We use both not-yet treated and never treated firms in the control group in these two specifications. The estimates on *Incumbent Female Labor Share* are robust when only using not-yet treated firms in the control group, as shown in [Table A.3](#).

years before the appointment, an indicator for whether the firm’s female labor share is above the sample-wide median in the years before the appointment, and an indicator for whether the firm’s board size is above the sample-wide median in the years before the appointment. We also hold fixed the industry that a firm is in by including industry fixed effects into each specification. We repeat this estimation for each year from 2006–2019, removing firms that made their first female director appointment in a previous year and always setting the dependent variable to be equal to one if the firm makes its first female director appointment in the year of interest, and zero otherwise.

We report the results of these regressions in [Table 5](#). Column (1) displays the predictors of first-time female director appointments in 2006. The other years’ predictors are displayed in Columns (2)–(14). We find that return on assets and leverage never consistently predict first-time female director appointments, with the estimates on *Return on Assets* (*Leverage*) being significant in only four (three) of the fourteen years, and with the signs of the estimates occasionally switching from negative to positive. Similarly, the estimates on *> Med. Female Labor Share* and *> Med. Board Size* flip signs often and are only significant in one or two of the fourteen specifications. The estimates on *Firm Size* are always positive, and they are statistically significant at the 10% level (or less) in ten of the fourteen years. This would suggest that larger firms, relative to those in the same half of the size distribution, are more likely to appoint female directors for the first time than are smaller firms in their same industry. However, the estimates on *> Med. Firm Size* flip signs often and are only significant in three out of the fourteen years, which introduces some uncertainty regarding the predictive power of firm size for first-time female director appointments. Taken together, neither the firm’s performance, financial position, nor the composition of its workforce appear to be reliable predictors of the decision to appoint a female director to the board for the first time. This finding helps to mitigate the possible concern that our main results are driven by some alternative trend in the firm’s life-cycle that simultaneously leads to both the decision to appoint a female director and a change in female labor share within the firm.

5.3 Exploring the Possible Mechanisms

The remainder of this section is devoted to exploring the possible mechanisms underlying the positive relation between first-time female director appointments and female labor and earnings share. To do this, we leverage additional administrative data, such the parental status of each employee, and we consider effect heterogeneity across a wide range of additional subgroups. We also use employee review data from Glassdoor to explore whether the director appointments are associated with policy changes that increase the attractiveness of the firm, which could drive the observed retention effects.

5.3.1 Heterogeneous Effects Based on Employee Characteristics

If female directors influence changes (or the expectation of changes) in the firm that make it more female-friendly and mom-friendly, then that could lead to the increases in female labor and earnings share that we document. To investigate this, we consider whether the increases in labor share are also present among female-parents of young children, i.e., those with children younger than six. We then create a new outcome variable, female *parent* labor share, equal to the fraction of the firm’s employees that are female and parents, and we also create *any parent* labor share, equal to the fraction of the firm’s employees that are parents (of any gender). We also construct analogous variables for female parent earnings share and any parent earnings share. We then separately estimate the effects of first-time female director appointments on these outcomes.

The results are reported in [Table 6](#). In Columns (1) and (3), we find evidence of a significant increase in female parent labor share and female parent earnings share, respectively. The effects sizes represent 2.82%–3.59% of the control group mean, which are about double the magnitude of the baseline effects in [Table 2](#). In contrast, the estimates on *Post-Treat* in Columns (2) and (4), which consider the labor and earnings share of *any* parent, regardless of gender, are close to zero and are statistically insignificant. Considering these results in tandem with the retention results from [Table 4](#), female parents appear to be better retained

within the firm after the first-time appointment of a female director, but male parents do not appear to adjust their retention likelihood. These findings are consistent with the notion that women in the U.S. often bear a disproportionately large share of the parenting burden, so changes in corporate policies that make the job more family-friendly—which female directors may enact—are likely to improve the retention outcomes of female parents more so than male parents.

We consider alternative subgroups of employees based on other observable demographics in [Table A.4](#) to look for evidence as to whether the labor and earnings share effects are more widespread than among female employees. Specifically, we consider the labor and earnings share of college educated workers (Column (1)), of workers age forty or older (Column (2)), of Black workers (Column (3)), and of Hispanic workers (Column (4)). We find no evidence that the labor and earnings share of those subgroups significantly change in response to the first-time female director appointments. Across all four subgroups, we do not find any evidence of significant changes in their labor share (Panel A) or earnings share (Panel B). These findings suggest that the first-time appointments of female directors do not coincide with other firm-wide initiatives that are meant to broadly improve worker recruitment and retention outcomes.

5.3.2 Exploring Mechanisms Using Glassdoor Reviews

Next, we use employee review data from Glassdoor.com to explore the potential mechanisms that link first-time female director appointments to increases in female labor and earnings share. We merge the BoardEx data with Glassdoor employee review data sourced from Revelio Labs. The resultant merge leads to a sample of 4,470 firm-year observations from 627 unique firms. The average number of reviews submitted each firm-year is 48. For each firm-year we compute the average value of the employee reviews posted on Glassdoor across the six dimensions for which reviewers leave 1–5 ratings of the firm: the overall firm rating, the firm’s compensation and benefits, the firm’s culture and values, the firm’s senior

leadership, the firm’s career opportunities, and the firm’s work-life balance. We then use these firm-year average ratings values to estimate changes in employee sentiment following the first-time appointment of a female director, again using the [Callaway and Sant’Anna \(2021\)](#) estimator.¹⁵

The results are tabulated in [Table 7](#). We find evidence that the overall employee ratings of their firms significantly increase after a women is appointed to the board for this first time. The estimate on *Post-Treat* in Column (1), of 0.104, represents an increase of 3.31%, relative to the control group mean.¹⁶ This increase in overall firm rating appears to be driven by employees having improved satisfaction with their compensation and benefits (Column (2)), their firm’s culture and values (Column (3)), and their senior leadership (Column (4)). The event-year estimates show that these effects tend to manifest three to four years after the director’s appointment. The event-year estimates also provide evidence that there are no differential pre-trends in employee ratings before the appointments. The estimates on *Post-Treat* in Columns (5) and (6), which consider employees’ ratings of their career opportunities and work-life balance, respectively, are also positive, by they are not statistically significant.

In [Table 8](#), we consider the words Glassdoor contributors use when they review the “pros” of working at the firm. In Column (1), we set the dependent variable to be the average number of instances the word “child” is used in such reviews, at the firm-year level.¹⁷ We find evidence of an average increase in the usage of the word “child” after the first-time appointment of a female director, as captured by the estimate on *Post-Treat* of 0.003. This effect is larger than the control mean of 0.002, indicating that Glassdoor reviewers are more than doubling their use of “child” in their reviews about the pros of working at their firm in the years after a first-time female director appointment. This effect manifests in the year of the director appointment and each of the four subsequent years. In Column (2), we consider

¹⁵We discuss the exact details of the Glassdoor data cleaning procedures in [Appendix B.1](#). These estimations are all conducted outside of the Census RDC, so we cannot say for certain if the firms in our Glassdoor sample are all represented in our Census sample.

¹⁶The magnitude of this effect is comparable to the effects documented in other papers that use Glassdoor data ([Liu et al., 2023](#); [Mkrtchyan et al., 2024](#); [Carter et al., 2025](#)).

¹⁷This also captures words such as “children” and “childcare.”

an extensive word list, which is meant to more broadly capture discussions about benefits, diversity, and family. Specifically, we compute the number of instances in which words like “child,” “time off,” “diversity,” etc. are used in each review of the “pros” of working at the firm.¹⁸ In this case, we estimate a positive coefficient on *Post-Treat*, capturing an increase of 12.6% relative to the control mean, though it is not statistically significant. We also estimate positive coefficients in each of the four post-appointment years, with year four being statistically significant at the 5% level. In Column (3), as a falsification test, we consider words related to productivity and effort.¹⁹ In Column (3), the coefficient on *Post-Treat* is negative, capturing a decrease of 8.1% relative to the control mean, though it is insignificant. Taken together, these findings provide additional suggestive evidence as to what mechanisms may be driving, at least partially, the observed increases in female labor share. Namely, first-time female director appointments appear to be associated with subsequent improvements in the family-friendly (specifically, mom-friendly) nature of the workplace environments.

6 Addressing Alternative Explanations

The primary endogeneity concern of our setting is that an omitted variable is driving both the decision to appoint a female director for the first time and the decision to make changes within the firm that improve the retention and/or recruitment of women. Some may refer to this explanation as a “year of the woman” initiative, wherein the firm decides it wants to increase the gender diversity of its workforce, so it institutes policy changes that lead to increased female labor and earnings share and it simultaneously appoints a female director for the first time. In these situations, the increases in female labor and earnings share that we observe would not be attributable to any influence made by the newly appointed female

¹⁸The words used are as follows: “adoption,” “child,” “dependent,” “diversity,” “family,” “fertility,” “maternity,” “reduced hours,” “work from home,” “kid,” “parent,” “woman,” “women,” “female,” “ivf,” “girl,” “lady,” “ladies,” “gender,” “equity,” “inclusion,” “mother,” “feminine,” “femininity,” “pregnant,” “birth,” “flexible,” “hybrid,” “mom,” “401k,” “dental,” “insurance,” “discount,” “health,” “training,” “holiday,” “retirement,” “gym,” “sick day,” “vacation,” “time off,” and “time-off.”

¹⁹The words used are as follows: “sale,” “productivity,” “productive,” “effort,” “goal,” “revenue,” “resourceful,” and “capable.”

director. Here we use additional heterogeneity tests and data sources to provide evidence that this alternative explanation is not likely at play in our setting.

6.1 Heterogeneous Effects Based on Core Committee Assignments

To address the omitted variable bias concern, we consider instances in which first-time female director appointees are granted relatively more authority and influence within the firm via an appointment to one of the core director committees: the compensation committee, the nomination/governance committee, or the audit committee. We find that the strongest effects on female labor and earnings share are realized when the newly appointed female directors are also placed on a core committee, whereas the effects are weaker when the female directors are granted relatively less power and influence.

These tests are reported in [Table 9](#), where the dependent variable in Panel A is the firm’s female labor share and the dependent variable in Panel B is the firm’s female earnings share. In Column (1), treated firms are those that both appoint a female director for the first time in a given year *and* that place that director on the compensation committee. Similarly, in Column (2) (Column (3)), treated firms are those that both appoint a female director for the first time in a given year *and* that place that director on the nomination (audit) committee. We use all not-yet treated firms in the control group, regardless of the eventual committee assignments of those firms’ first-time female director appointees. In Panel A, the estimates on *Post-Treat* are 0.00742, 0.00699, and 0.00658 in the compensation, nomination, and audit committee specifications, respectively. These capture increases in female labor share of 1.94%, 1.82%, and 1.73%, respectively, relative to the control group mean. In contrast, the baseline effect in [Table 2](#) captures an increase in female labor share of 1.43% of the control group mean. This suggests that the appointment of a female director who is *not* placed on one of the three core committees leads to a smaller increase in female labor share. If the observed increase in female employee representation was driven by an omitted variable that drives both workforce composition and female director appointment, then we

would be less likely to find differential effects based on core committee assignment. That we find such a difference in effects suggests that female directors being put into positions of power and influence (via core committee membership) is a factor in driving the observed increase in retention among female employees.

6.2 Heterogeneous Effects Based Firm Characteristics

To further reduce the primary omitted variable bias concern, we estimate the effects of first-time female director appointments on female employee outcomes separately for firms with small boards (i.e., below the median size) and those with large boards (i.e., above the median size). This approach allows us to assess whether the influence of the new director depends on their relative decision-making power. In larger boards, an individual director is likely to have less sway over firm policy simply due to their relatively smaller share of the board’s decision rights. This would make it harder to influence outcomes like labor composition and compensation, particularly in situations where “tokenism” may be at play (Gormley et al., 2023). By “tokenism” we mean the decision of a firm to appoint a female director for the first time for virtue signaling purposes (i.e., window-dressing), but where the firm does not anticipate (or possibly does not facilitate) the newly appointed female director exerting any real influence. On the other hand, if appointments are associated with changes in female labor share only when women are added to smaller boards, this might suggest that female directors require a relatively larger share of voice to shape outcomes. Instead, we find similar effects across the board size subgroups, supporting the view that first-time female directors can influence policy even in larger, more diffuse governance structures.

In Columns (1) and (2) of Table 10, we estimate effects within the subsamples of firms with small (below median) and large (above median) pre-treatment levels of board size, respectively. We find that both firms with small and large boards realize increases in female labor share following the first-time appointment of a female director. The effect size among small board firms captures 1.26% of the control group mean, and the effect size among large

board firms captures 1.77% of the control group mean. These findings suggest that first-time female directors can have a voice, regardless of the size of the board, reducing the concern that tokenism is the main driver of the decision to appoint female directors for the first time.

We find additional evidence against the tokenism concern when considering whether firms choose to add a board seat to accommodate a newly appointed female director. In Columns (3) and (4) we estimate effects within the subsamples of firms that replace boards seats versus those that add new board seats, respectively, when the newly appointed female director joins the board. If women are being added to boards due to tokenism, firms can accommodate this—without granting the new director too much power—by simply increasing the board size (by adding a board seat) in order to appoint a female director for the first time. If such tokenism motivations are at play, we would expect less of an effect on female labor share among firms that add a seat to accommodate the appointment. We do not find evidence of this, as the estimates on *Post-Treat* in Columns (3) and (4) are both large, positive, and statistically significant. Furthermore, the consistency of the effect on female labor share across the replace seat and added seat subsamples reduces the concern that the effects are driven by a concurrent managerial initiative to attract and/or retain women in the firm, as the need to replace a vacant board seat is more exogenous than is the decision to add a new board seat all together.

6.3 Evidence from Financial Statements

To further reduce the concern that our main findings are all driven by an omitted variable—wherein the firm simultaneously decided to (1) improve the work environment to better retain female workers and (2) appoint a female director for the first time—we look for evidence as to whether or not management communicated ahead of time their plan do these things. For instance, if managers had plans to improve the work environment in the coming years, we would expect them to communicate such plans in their annual reports (e.g., 10-Ks and proxy statements), and we would expect such communication to lead to upticks in female-,

family-, and/or benefit-specific word usage. We find no evidence of such upticks in word usage before the year of the first-time female director appointments.

For this analysis, we sum all the instances of the following words being used in a firm’s 10-K in a given year: women, woman, girl, lady, ladies, gender, diversity, female, mother, feminine, femininity, womanhood, pregnant, mom, birth, parent, kid, kids, child, children, childcare, work from home, flexible, hybrid. Then we divide that sum by the total number of words in the 10-K. Using the same [Callaway and Sant’Anna \(2021\)](#) estimator that we use in our other tests, we estimate year-by-year trends in the usage of these words. When we consider pre-appointment trends in 10-K word usage we fail to reject the null of common pre-trends, with the p -value of the average pre-appointment estimate being 0.936.²⁰ These findings suggest that firms did not communicate their plans to improve the retention of its female workforce in their financial statements in the years before their first-time female director appointment.

6.4 Anecdotal Evidence from Interviews with Directors

Finally, to gain additional insight into the impacts of female directors on rank-and-file employees, we conducted interviews with several women who joined all-male boards during our sample period. A recurring theme from these interviews was the role of female directors in fostering mentorship. One director described her engagement with non-executive employees through female-oriented workplace affinity groups, noting that these venues created valuable opportunities for connection and conversation. Another observed, “Female board members are more likely to go into the field, to get on the factory floor, and visit local offices.” A third highlighted the influence of women on compensation committees, explaining, “We pushed hard to introduce statistical pay equity analyses. While we didn’t uncover systemic problems, we did identify areas that needed correction.” Several directors also emphasized their role in

²⁰We find similar results when we consider word usage in both 10-Ks and proxy statements, with a p -value of the average pre-appointment estimate being 0.717.

encouraging the recruitment of more women into both management and board positions.²¹ Although anecdotal, these accounts reinforce our empirical findings that the appointment of first-time female directors contribute to cultivating a female-friendly environment, thereby boosting the morale and retention among female employees.

Taken together, these additional analyses, tests, and anecdotes strengthen the interpretation that first-time female director appointments play a meaningful role in improving gender diversity within firms. We show that these appointments are associated with increases in the share of female employees and their earnings share, these changes are felt in the top three quartiles of the earnings distribution, and they are driven by increases in the retention of female employees. We have shown that these effects are also the greatest in cases where female directors are granted influence through core committee assignments, and they persist across various board and firm characteristics. In contrast, we do not find evidence that these changes are part of a broader, pre-announced diversity initiative. These findings reduce concerns about the influence of omitted variable bias in explaining our results, although it is never possible to rule this out.

7 Conclusion

It has been well documented that women are under-represented among business leaders, with some saying that a “glass ceiling” prevents women from advancing to the highest levels of corporate leadership (Matsa and Miller, 2011; Manzi and Heilman, 2021). Female workers may also face substantial barriers in early employment opportunities (Roberson and Perry, 2021), leading to gaps in their employment, compensation, and promotion outcomes compared to male employees (Goldin, 1990, 2006, 2014; Blau and Kahn, 2017). Whether and how these two stubbornly persistent stylized facts are related remains an open empirical question, particularly among publicly traded firms in the United States. Corporate boards

²¹We are unable to empirically study the effects of multiple female directors based on our identification strategy and, given small sample sizes, due to concerns about disclosure risks associated with using these confidential administrative data.

of directors are an especially important setting in which to study the impact of female leadership on the relative position and pay of female employees, as boards of directors influence organizational policies and practices. In this paper, we estimate the impact of increased female director representation on workplace gender diversity and women’s earnings across the within-firm earnings distribution.

We expand upon existing studies of the effect of board gender diversity by turning the focus to employee-related outcomes, as opposed to firm performance and financial outcomes. Analyses of employee-related outcomes require long-horizon administrative data, which is uncommon, especially in the U.S. context. We circumvent this data limitation by using data from the U.S. Census Bureau to link administrative longitudinal employee data to external data on board composition. We find evidence that the first appointment of a female to the board of directors leads to an increase in firm-wide female representation of 1%–2%, with the largest increases being realized in the higher-earning positions within the firm. We find that the effects are most likely driven by improved retention among the firm’s female employees, especially those with young children. Furthermore, the effects are the most pronounced when the newly appointed female director is appointed to one of the three core committees of the board, potentially because this additional authority and influence allows the director to implement female-friendly corporate policies and practices.

Over the last two decades, there has been a proliferation of regulations—both in Europe and in the United States—enacted to mandate minimum levels of gender diversity on boards ([Greene et al., 2020](#)). Towards the end of the 2010s, institutional investors provided additional demand for firms to appoint female directors ([Gormley et al., 2023](#)). While the continuation of these mandates and demands has become quite uncertain since the 2024 Presidential election, there remains an important need to understand the relation between board composition and economic outcomes. Our findings contribute to the literature by showing that female director representation leads to an increased retention of the firm’s female employees. By using Glassdoor employee review data, we are able to shed some light

on the possible mechanisms that underlie the observed relation, though we are limited in our ability to know exactly what role the newly appointed female directors played in driving new corporate policy implementation. Future research should seek to identify such mechanisms more precisely, which may become more achievable as companies begin to disclose additional information about their human capital management practices ([Regier and Rouen, 2023](#); [Jennnings et al., 2024](#); [Carter et al., 2025](#)).

References

- Abowd, John M, John Haltiwanger, Ron Jarmin, Julia Lane, Paul Lengermann, Kristin McCue, Kevin McKinney, Kristin Sandusky. 2005. The relation among human capital, productivity, and market value: Building up from micro evidence. *Measuring capital in the new economy*. University of Chicago Press, 153–204.
- Adams, Renée B, Ali C Akyol, Patrick Verwijmeren. 2018. Director skill sets. *Journal of Financial Economics* **130**(3) 641–662.
- Adams, Renée B, Daniel Ferreira. 2009. Women in the boardroom and their impact on governance and performance. *Journal of financial economics* **94**(2) 291–309.
- Ahern, Kenneth R, Amy K Dittmar. 2012. The changing of the boards: The impact on firm valuation of mandated female board representation. *The Quarterly Journal of Economics* **127**(1) 137–197.
- Allen, Abigail, Aida Sijamic Wahid. 2024. Regulating gender diversity: Evidence from california senate bill 826. *Management Science* **70**(4) 2023–2046.
- Anderson, Deborah J, Melissa Binder, Kate Krause. 2002. The motherhood wage penalty: Which mothers pay it and why? *American economic review* **92**(2) 354–358.
- Athey, Susan, Christopher Avery, Peter Zemsky. 2000. Mentoring and diversity. *American Economic Review* **90**(4) 765–786.
- Bagues, Manuel F, Berta Esteve-Volart. 2010. Can gender parity break the glass ceiling? evidence from a repeated randomized experiment. *The Review of Economic Studies* **77**(4) 1301–1328.
- Baker, Andrew C, David F Larcker, Charles CY Wang. 2022. How much should we trust staggered difference-in-differences estimates? *Journal of Financial Economics* **144**(2) 370–395.
- Barrios, John Manuel. 2021. Staggeringly problematic: A primer on staggered did for accounting researchers. *Available at SSRN 3794859* .
- Bernile, Gennaro, Vineet Bhagwat, Scott Yonker. 2018. Board diversity, firm risk, and corporate policies. *Journal of Financial Economics* **127**(3) 588–612.
- Bertrand, Marianne, Sandra E Black, Sissel Jensen, Adriana Lleras-Muney. 2018. Breaking the glass ceiling? the effect of board quotas on female labour market outcomes in norway. *The Review of Economic Studies* **86**(1) 191–239.
- Bertrand, Marianne, Claudia Goldin, Lawrence F Katz. 2010. Dynamics of the gender gap for young professionals in the financial and corporate sectors. *American economic journal: applied economics* **2**(3) 228–55.
- Bian, Bo, Jingjing Li, Kai Li. 2023. Does mandating women on corporate boards backfire? *Available at SSRN* .
- Blau, Francine D, Lawrence M Kahn. 2017. The gender wage gap: Extent, trends, and explanations. *Journal of economic literature* **55**(3) 789–865.

- Blau, Francine D, Lawrence M Kahn. 2020. The gender pay gap: Have women gone as far as they can? *Inequality in the United States: A Reader*. Routledge, 345–362.
- BLS. 2016. Quarterly census of employment and wages: Handbook of methods <https://www.bls.gov/cew>.
- BLS, The. 2020. Women in the labor force: a databook. *U.S. Bureau of Labor Statistics* URL <https://www.bls.gov/opub/reports/womens-databook/2020/>.
- Booth, Alison, Andrew Leigh. 2010. Do employers discriminate by gender? a field experiment in female-dominated occupations. *Economics Letters* **107**(2) 236–238.
- Buser, Thomas, Muriel Niederle, Hessel Oosterbeek. 2014. Gender, competitiveness, and career choices. *The quarterly journal of economics* **129**(3) 1409–1447.
- Cai, Wei, Aiysha Dey, Jillian Grennan, Joseph Pacelli, Lin Qiu. 2022. Do diverse directors influence dei outcomes? *Available at SSRN* .
- Callaway, Brantly, Pedro HC Sant’Anna. 2021. Difference-in-differences with multiple time periods. *Journal of econometrics* **225**(2) 200–230.
- Carpenter, David, Abigail Graber. 2025. Rescission of executive order 11246, equal employment opportunity: Legal implications <https://www.congress.gov/crs-product/LSB11268>.
- Carter, Mary Ellen, Lisa LaViers, Jason Sandvik, Da Xu. 2025. Ceo pay ratio disclosures and changes in employee pay satisfaction. *Available at SSRN 4430286* .
- Charles, Kerwin Kofi, Ming-Ching Luoh. 2003. Gender differences in completed schooling. *Review of Economics and statistics* **85**(3) 559–577.
- Chen, Sheng-Syan, Yan-Shing Chen, Jun-Koo Kang, Shu-Cing Peng. 2020. Board structure, director expertise, and advisory role of outside directors. *Journal of Financial Economics* **138**(2) 483–503.
- Coles, Jeffrey L, Naveen D Daniel, Lalitha Naveen. 2006. Managerial incentives and risk-taking. *Journal of Financial Economics* **79**(2) 431–468.
- Coles, Jeffrey L, Michael L Lemmon, Yan Albert Wang. 2008. The joint determinants of managerial ownership, board independence, and firm performance. *Second Singapore International Conference on Finance*.
- Dancaster, Lisa, Marian Baird. 2016. Predictors of the adoption of work–care arrangements: A study of south african firms. *The International Journal of Human Resource Management* **27**(4) 456–475.
- Deshpande, Manasi, Yue Li. 2019. Who is screened out? application costs and the targeting of disability programs. *American Economic Journal: Economic Policy* **11**(4) 213–248.
- Dobbin, Frank, Soohan Kim, Alexandra Kalev. 2011. You can’t always get what you need: Organizational determinants of diversity programs. *American Sociological Review* **76**(3) 386–411.
- Eckbo, B Espen, Knut Nygaard, Karin S Thorburn. 2021. Valuation effects of norway’s board gender-quota law revisited. *Management Science* .

- Eckbo, B Espen, Knut Nygaard, Karin S Thorburn. 2022. Valuation effects of norway’s board gender-quota law revisited. *Management Science* **68**(6) 4112–4134.
- Edmans, Alex. 2024. *May Contain Lies: How Stories, Statistics, and Studies Exploit Our Biases—And What We Can Do about It*. Univ of California Press.
- Ellis, Jesse A, C Edward Fee, Shawn Thomas. 2018. Playing favorites? industry expert directors in diversified firms. *Journal of Financial and Quantitative Analysis* **53**(4) 1679–1714.
- Fama, Eugene F. 1980. Agency problems and the theory of the firm. *Journal of Political Economy* **88**(2) 288–307.
- Ferris, Stephen P, Murali Jagannathan, Adam C Pritchard. 2003. Too busy to mind the business? monitoring by directors with multiple board appointments. *The Journal of finance* **58**(3) 1087–1111.
- Fortin, Nicole M, Brian Bell, Michael Böhm. 2017. Top earnings inequality and the gender pay gap: Canada, sweden, and the united kingdom. *Labour Economics* **47** 107–123.
- Fracassi, Cesare, Geoffrey Tate. 2012. External networking and internal firm governance. *The Journal of finance* **67**(1) 153–194.
- Gertsberg, Marina, Johanna Mollerstrom, Michaela Pagel. 2021. Gender quotas and support for women in board elections. Tech. rep., National Bureau of Economic Research Cambridge, MA.
- Gneezy, Uri, Kenneth L Leonard, John A List. 2009. Gender differences in competition: Evidence from a matrilineal and a patriarchal society. *Econometrica* **77**(5) 1637–1664.
- Goldin, C. 2006. The quiet revolution that transformed women’s employment, education, and family .
- Goldin, Claudia. 1990. Understanding the gender gap: An economic history of american women. *National Bureau of Economic Research* .
- Goldin, Claudia. 2014. A grand gender convergence: Its last chapter. *American economic review* **104**(4) 1091–1119.
- Goldin, Claudia, Lawrence F Katz. 2002. The power of the pill: Oral contraceptives and women’s career and marriage decisions. *Journal of political Economy* **110**(4) 730–770.
- Goldin, Claudia, Lawrence F Katz, Ilyana Kuziemko. 2006. The homecoming of american college women: The reversal of the college gender gap. *Journal of Economic perspectives* **20**(4) 133–156.
- Goldin, Claudia, Cecilia Rouse. 2000. Orchestrating impartiality: The impact of “blind” auditions on female musicians. *American economic review* **90**(4) 715–741.
- Gorman, Elizabeth H. 2005. Gender stereotypes, same-gender preferences, and organizational variation in the hiring of women: Evidence from law firms. *American Sociological Review* **70**(4) 702–728.
- Gormley, Todd A, Vishal K Gupta, David A Matsa, Sandra C Mortal, Lukai Yang. 2023. The big three and board gender diversity: The effectiveness of shareholder voice. *Journal of Financial Economics* **149**(2) 323–348.

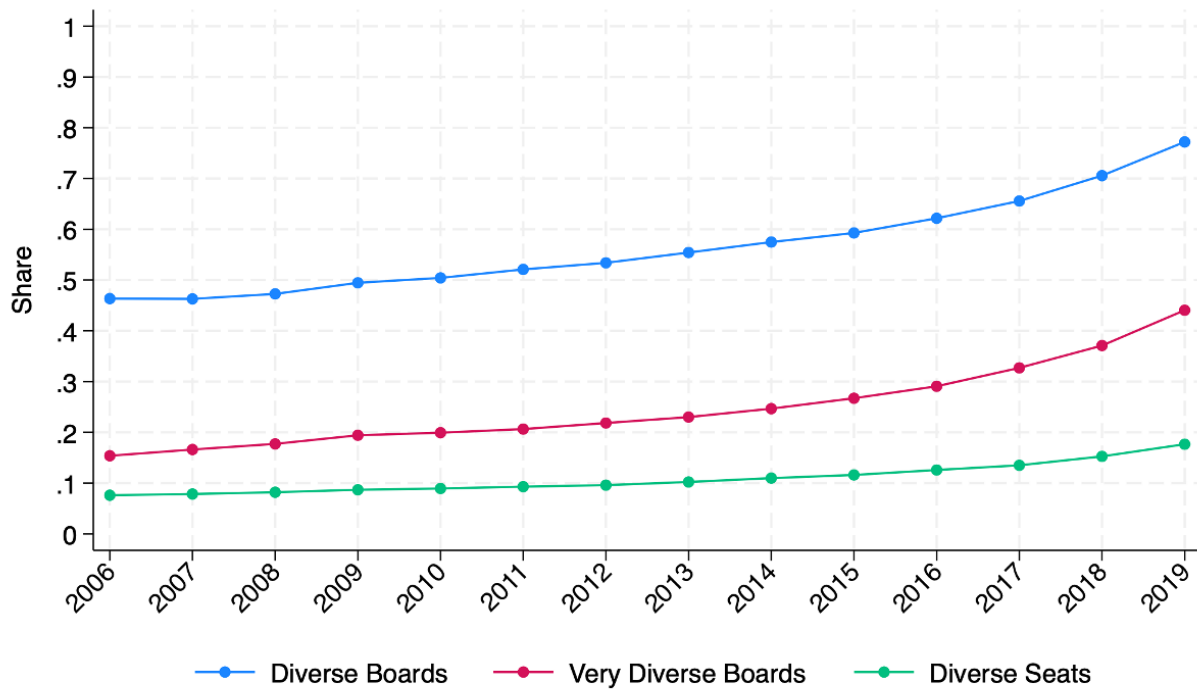
- Greene, Daniel, Vincent J Intintoli, Kathleen M Kahle. 2020. Do board gender quotas affect firm value? evidence from california senate bill no. 826. *Journal of Corporate Finance* **60** 101526.
- Greene, Daniel T, JiHoon Hwang, Vincent J Intintoli, Kathleen M Kahle. 2025. Reconciling the evidence on board diversity mandates. *Journal of Corporate Finance* 102838.
- Guryan, Jonathan. 2004. Desegregation and black dropout rates. *American Economic Review* **94**(4) 919–943.
- Huang, Jiekun, Darren J Kisgen. 2013. Gender and corporate finance: Are male executives overconfident relative to female executives? *Journal of Financial Economics* **108**(3) 822–839.
- Jennnings, Robert, Lisa LaViers, Ethan Rouen, Jason Sandvik. 2024. The effects of human capital disclosures on professional investors’ assessments of firm risk. *Available at SSRN* .
- Jensen, Michael C, Kevin J Murphy. 1990. Performance pay and top-management incentives. *Journal of Political Economy* **98**(2) 225–264.
- Jenter, Dirk, Fadi Kanaan. 2015. Ceo turnover and relative performance evaluation. *The Journal of Finance* **70**(5) 2155–2184.
- Kang, Shinwoo, E Han Kim, Yao Lu. 2018. Does independent directors’ ceo experience matter? *Review of Finance* **22**(3) 905–949.
- Kim, Daehyun, Laura T Starks. 2016. Gender diversity on corporate boards: Do women contribute unique skills? *American Economic Review* **106**(5) 267–71.
- Kleven, Henrik, Camille Landais, Johanna Posch, Andreas Steinhauer, Josef Zweimüller. 2019a. Child Penalties across Countries: Evidence and Explanations. *AEA Papers and Proceedings* **109** 122–126. doi:10.1257/pandp.20191078. URL <https://www.aeaweb.org/articles?id=10.1257/pandp.20191078>.
- Kleven, Henrik, Camille Landais, Jakob Egholt Søgaaard. 2019b. Children and Gender Inequality: Evidence from Denmark. *American Economic Journal: Applied Economics* **11**(4) 181–209. doi: 10.1257/app.20180010. URL <https://www.aeaweb.org/articles?id=10.1257/app.20180010>.
- Koning, Rembrand, Sampsa Samila, John-Paul Ferguson. 2021. Who do we invent for? patents by women focus more on women’s health, but few women get to invent. *Science* **372**(6548) 1345–1348.
- Kowalewska, Helen. 2020. Bringing women on board: the social policy implications of gender diversity in top jobs. *Journal of Social Policy* **49**(4) 744–762.
- Kunze, Astrid, Amalia R Miller. 2017. Women helping women? evidence from private sector data on workplace hierarchies. *Review of Economics and Statistics* **99**(5) 769–775.
- Kurtulus, Fidan Ana, Donald Tomaskovic-Devey. 2012. Do female top managers help women to advance? a panel study using eeo-1 records. *The Annals of the American Academy of Political and Social Science* **639**(1) 173–197.
- LaViers, Lisa, Jason Sandvik. 2022. The effect of workplace gender diversity disclosures on job search decisions. *Available at SSRN 4240155* .

- Leibbrandt, Andreas, John A List. 2015. Do women avoid salary negotiations? evidence from a large-scale natural field experiment. *Management Science* **61**(9) 2016–2024.
- Levi, Maurice, Kai Li, Feng Zhang. 2014. Director gender and mergers and acquisitions. *Journal of Corporate Finance* **28** 185–200.
- Liu, Tim, Christos A Makridis, Paige Ouimet, Elena Simintzi. 2023. The distribution of nonwage benefits: maternity benefits and gender diversity. *The Review of Financial Studies* **36**(1) 194–234.
- Lundberg, Shelly, Elaina Rose. 2000. Parenthood and the earnings of married men and women. *Labour Economics* **7**(6) 689–710.
- Manzi, Francesca, Madeline E Heilman. 2021. Breaking the glass ceiling: For one and all? *Journal of personality and social psychology* **120**(2) 257.
- Matsa, David A, Amalia R Miller. 2011. Chipping away at the glass ceiling: Gender spillovers in corporate leadership. *American Economic Review* **101**(3) 635–39.
- Matsa, David A, Amalia R Miller. 2013. A female style in corporate leadership? evidence from quotas. *American Economic Journal: Applied Economics* **5**(3) 136–69.
- McShane, Margot, Hetty Pye. 2024. Gender diversity in the c-suit: Women’s representation in the 2024 sp 100 <https://corpgov.law.harvard.edu/2024/10/18/gender-diversity-in-the-c-suite-womens-representation-in-the-2024-sp-100/>.
- Miller, Amalia R. 2011. The effects of motherhood timing on career path. *Journal of population economics* **24**(3) 1071–1100.
- Mkrtchyan, Anahit, Jason Sandvik, Da Xu. 2024. Employee responses to ceo activism. *Journal of Accounting and Economics* 101701.
- Nasdaq. 2019. Wynn resorts: 7 tactics to cultivate a world-class employment brand. *Nasdaq* URL <https://www.nasdaq.com/articles/wynn-resorts-7-tactics-cultivate-world-class-employment-brand-2019-03-13>.
- Neumark, David, Roy J Bank, Kyle D Van Nort. 1996. Sex discrimination in restaurant hiring: An audit study. *The Quarterly journal of economics* **111**(3) 915–941.
- Niederle, Muriel, Lise Vesterlund. 2007. Do women shy away from competition? do men compete too much? *The quarterly journal of economics* **122**(3) 1067–1101.
- NSC. 2020. Term enrollment, fall 2020 https://nscresearchcenter.org/wp-content/uploads/CTEE_Report_Fall_2020.pdf. Accessed: April 21st, 2022.
- OECD. 2017. *The Pursuit of Gender Equality: An Uphill Battle*. OECD.
- Olivetti, Claudia, Barbara Petrongolo. 2016. The evolution of gender gaps in industrialized countries. *Annual review of Economics* **8** 405–434.
- Ramonas, Andrew. 2024. Nasdaq board diversity rules struck down by fifth circuit. *Bloomberg Law* URL <https://news.bloomberglaw.com/esg/nasdaq-board-diversity-regulations-struck-down-by-fifth-circuit>.

- Rau, P Raghavendra, Jason Sandvik, Theo Vermaelen. 2024. Ipo price formation and board gender diversity. *Journal of Corporate Finance* **88** 102629.
- Regier, Matthias, Ethan Rouen. 2023. The stock market valuation of human capital creation. *Journal of Corporate Finance* **79** 102384.
- Roberson, Quinetta, Jamie L Perry. 2021. Inclusive leadership in thought and action: A thematic analysis. *Group & Organization Management* 10596011211013161.
- Sandberg, Sheryl. 2013. *Lean in: Women, work, and the will to lead*. Random House.
- Schwartz-Ziv, Miriam. 2017. Gender and board activeness: the role of a critical mass. *Journal of Financial and Quantitative Analysis* **52**(2) 751–780.
- Sila, Vathunyoo, Angelica Gonzalez, Jens Hagendorff. 2016. Women on board: Does boardroom gender diversity affect firm risk? *Journal of Corporate Finance* **36** 26–53.
- Sin, Isabelle, Steven Stillman, Richard Fabling. 2017. What drives the gender wage gap? examining the roles of sorting, productivity differences, and discrimination. .
- Stoll, John. 2018. To shatter the glass ceiling, don't force it. *The Wall Street Journal* URL <https://www.wsj.com/articles/to-shatter-the-glass-ceiling-dont-force-it-1536379201>.
- Tate, Geoffrey, Liu Yang. 2015. Female leadership and gender equity: Evidence from plant closure. *Journal of Financial Economics* **117**(1) 77–97.
- Waldman, Paul. 2017. ??? *The Washington Post* URL <https://www.washingtonpost.com/blogs/plum-line/wp/2017/03/30/pences-unwillingness-to-be-alone-with-a-woman-is-a-symptom-of-a-bigger-problem/>.
- Zingales, L. 2000. In search of new foundations. *Journal of Finance* **55** 1623–1653.

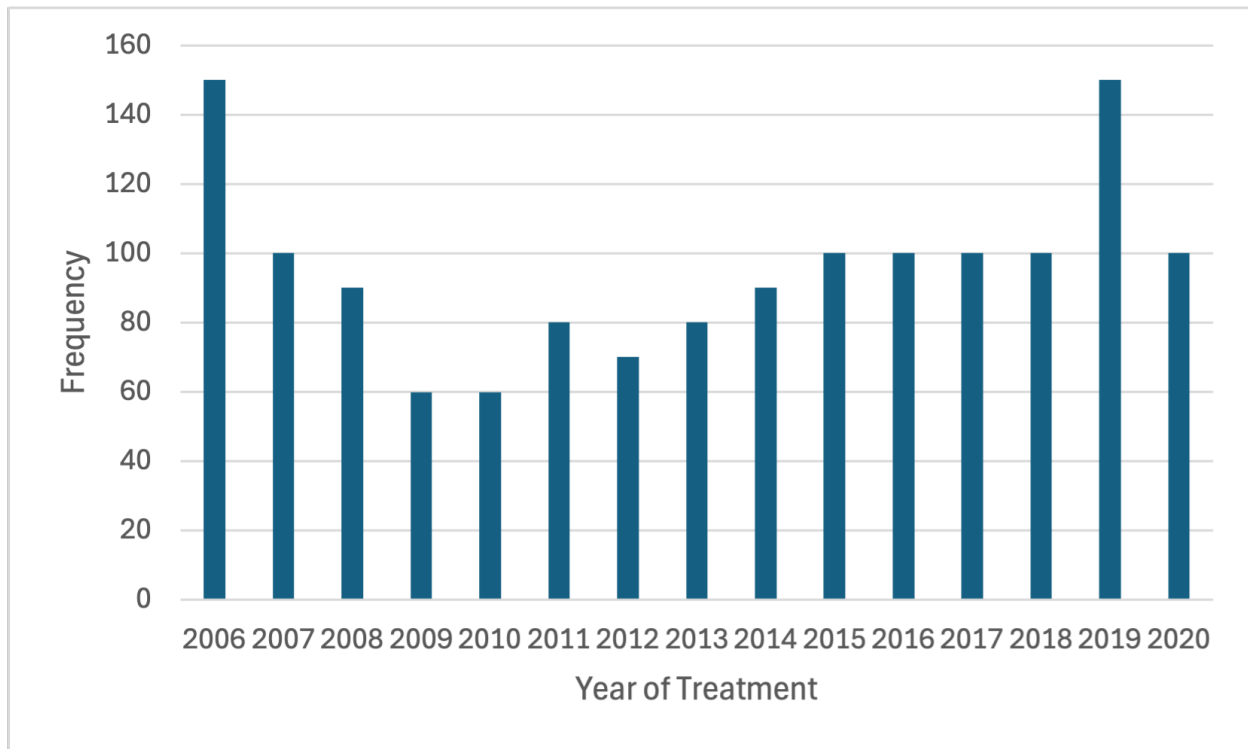
Figures and Tables

Figure 1: Trends in Female Director Representation Over Time



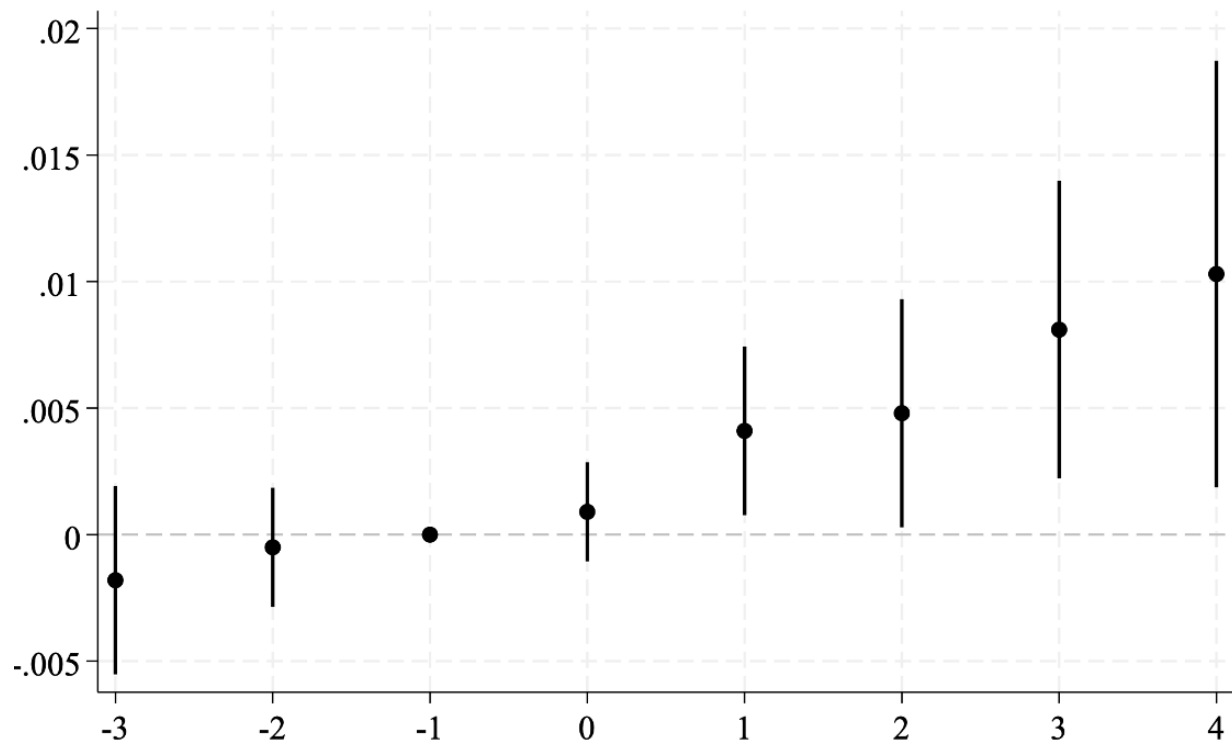
Notes: This figure illustrates the trends in female director representation over time, using data outside of the RDC from BoardEx. The blue (top) line captures the fraction of firms with at least one female director on the board in a given year. The red (middle) line captures the fraction of firms with at least two female directors on the board in a given year. The green (bottom) line captures the average fraction of the board seats held by women across all boards in a given year.

Figure 2: Frequency of Treatment Timing Over Time



Notes: This figure illustrates the timing of treatment for firms that add women to their board of directors for the first time between 2006 and 2020. In our empirical tests, our set of “treated” firms include those with treatment timing from 2006–2019. We report the number of treated firms in 2020 in this figure because these firms are used as controls for earlier treated firms.

Figure 3: Baseline Effects of Appointing a First Female Director on Female Labor Share



Notes: This figure plots the event-year differences in female labor share between treated and control firms in the years leading up to and after treated firms add a female director to the board for the first time.

Table 1: Summary Statistics

	Treated		Never	Always
	Pre-Treat	Post-Treat	Treated	Treated
	(1)	(2)	(3)	(4)
N. Unique Firms	1,500	1,500	850	2,300
Female Labor Share (FLS)	0.3711 (0.1831)	0.4006 0.1864	0.3717 (0.2005)	0.4455 (0.1990)
FLS (Below Med. Earnings)	0.4544 (0.2034)	0.4733 (0.2020)	0.4545 (0.2201)	0.5213 (0.2129)
FLS (Above Med. Earnings)	0.2900 (0.1851)	0.3310 (0.1877)	0.2888 (0.2024)	0.3716 (0.2010)
Female Earnings Share (FES)	0.2816 (0.1589)	0.3185 (0.1656)	0.2860 (0.1743)	0.3580 (0.1750)
FES (Below Med. Earnings)	0.4500 (0.2095)	0.4699 (0.2074)	0.4504 (0.2257)	0.5192 (0.2194)
FES (Above Med. Earnings)	0.2350 (0.1575)	0.2783 (0.1646)	0.2363 (0.1730)	0.3132 (0.1740)
Number of Employees	1,700 (5,500)	3,300 (13,000)	806 (2,100)	7,800 (26,000)
Board Size	7.070 (2.137)	8.291 (2.242)	6.495 (1.756)	9.189 (2.597)
Female CEO	- -	0.0339 (0.1811)	- -	0.0530 (0.2240)
Frac. Female Directors	- -	0.1556 (0.0895)	- -	0.1768 (0.1055)
Frac. Female Comp.	- -	0.1569 (0.1736)	- -	0.1824 (0.1871)
Frac. Female Nom./Gov.	- -	0.1725 (0.1784)	- -	0.1999 (0.1921)
Frac. Female Audit	- -	0.1775 (0.1731)	- -	0.1908 (0.1860)
Return on Assets	0.0254 (0.1865)	0.0263 (0.1707)	-0.0109 (0.2170)	0.0462 (0.1499)
Leverage	0.2173 (0.2294)	0.2511 (0.2276)	0.2001 (0.2278)	0.2401 (0.2080)
Firm Size	6.392 (1.575)	7.170 (1.725)	5.387 (1.540)	7.691 (1.977)

Notes: This table reports summary statistics.

Table 2: Baseline Effects of Appointing a First Female Director

	Female Labor Share	Female Earnings Share
	(1)	(2)
Pre-Treat	-0.00120 (0.00138)	-0.00175 (0.00139)
Post-Treat	0.00563*** (0.00215)	0.00601*** (0.00212)
$t - 3$	-0.00182 (0.00196)	-0.00269 (0.00189)
$t - 2$	-0.00058 (0.00127)	-0.00082 (0.00135)
$t = 0$	0.00093 (0.00101)	-0.00055 (0.00137)
$t + 1$	0.00405** (0.00172)	0.00514*** (0.00191)
$t + 2$	0.00478** (0.00230)	0.00567** (0.00235)
$t + 3$	0.00808*** (0.00306)	0.00934*** (0.00304)
$t + 4$	0.01030** (0.00435)	0.0105*** (0.00388)
Controls	✓	✓
Control Group	Not-Yet Treated	Not-Yet Treated
N. Obs.	15,000	15,000
N. Firms	1,500	1,500
N. Treated	1,400	1,400
Cont. Mean	0.395	0.307

Notes: This table reports estimates of the effects of appointing the first female board member to a previously all-male board. The dependent variable in Column (1) captures female labor share, equal to the fraction of the firm's employees that are female. The dependent variable in Column (2) captures female earnings share, equal to the share of earnings earned by female employees, as a fraction of the total earnings earned by all employees in the firm. We employ the [Callaway and Sant'Anna \(2021\)](#) estimator in both specifications. The control group in both specifications is made up of not-yet (but eventually) treated firms. Both specifications include firm-year level controls for return on assets, leverage, firm size, and number of employees two years before the appointment. Standard errors are doubly robust as per the procedure used by [Callaway and Sant'Anna \(2021\)](#). Statistical significance of 10%, 5%, and 1% is denoted by *, **, and ***, respectively.

Table 3: Baseline Effects within Each Quartile of the Firm's Earnings Distribution

	Female Labor Share in Each Earnings Quartile				Female Earnings Share in Each Earnings Quartile			
	0–25%	25–50%	50–75%	75–100%	0–25%	25–50%	50–75%	75–100%
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pre-Treat	0.00391 (0.00368)	0.00088 (0.00381)	-0.00129 (0.00286)	0.00059 (0.00197)	0.00513 (0.00388)	0.00044 (0.00380)	-0.00041 (0.00288)	-0.00072 (0.00194)
Post-Treat	0.00149 (0.00380)	0.01250*** (0.00425)	0.00651* (0.00372)	0.00651*** (0.00245)	0.00261 (0.00408)	0.01250*** (0.00428)	0.00646* (0.00372)	0.00662*** (0.00242)
$t - 3$	+	+	–	–	+	+	–	–
$t - 2$	+	+	+	+	+	–	+	+
$t = 0$	+	+***	–	+	+	+***	–	–
$t + 1$	+	+*	+	+*	+	+*	+	+***
$t + 2$	–	+***	+	+*	–	+**	+	+**
$t + 3$	–	+***	+*	+***	–	+***	+*	+***
$t + 4$	+	+**	+*	+***	+	+**	+*	+**
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Control Group	Not-Yet Treated	Not-Yet Treated	Not-Yet Treated	Not-Yet Treated	Not-Yet Treated	Not-Yet Treated	Not-Yet Treated	Not-Yet Treated
N. Obs.	15,000	15,000	15,000	15,000	15,000	15,000	15,000	15,000
N. Firms	1,500	1,500	1,500	1,500	1,500	1,500	1,500	1,500
N. Treated	1,400	1,400	1,400	1,400	1,400	1,400	1,400	1,400
Cont. Mean	0.489	0.451	0.375	0.262	0.495	0.449	0.372	0.222

Notes: This table reports estimates of the effects of appointing the first female board member to a previously all-male board. The dependent variables in Columns (1)–(4) capture female labor share, equal to the fraction of the firm's employees that are female within each of the four quartiles of the firm's overall earnings distribution. The dependent variables in Columns (5)–(8) capture female earnings share, equal to the share of earnings earned by female employees, as a fraction of the total earnings earned by all employees within each of the four quartiles of the firm's overall earnings distribution. Employees are placed into quartiles based on their earnings in the prior year. We employ the [Callaway and Sant'Anna \(2021\)](#) estimator in all specifications. The control group in all specifications is made up of not-yet (but eventually) treated firms. All specifications include firm-year level controls for return on assets, leverage, firm size, and number of employees two years before the appointment. Standard errors are doubly robust as per the procedure used by [Callaway and Sant'Anna \(2021\)](#). Statistical significance of 10%, 5%, and 1% is denoted by *, **, and ***, respectively.

Table 4: Effects on Retention vs. Hiring

	Incumbent Female Labor Share	Newly Hired Female Labor Share
	(1)	(2)
Pre-Treat	-0.00149 (0.00217)	-0.00304 (0.00365)
Post-Treat	0.01040*** (0.00266)	-0.00224 (0.00385)
$t - 3$	—	—
$t - 2$	—	—
$t = 0$	+***	—
$t + 1$	+***	+
$t + 2$	+***	—
$t + 3$	+***	—
$t + 4$	+***	—
Controls	✓	✓
Control Group	Both	Both
N. Obs.	20,500	20,500
N. Firms	2,300	2,300
N. Treated	1,400	1,400

Notes: This table reports estimates of the effects of appointing the first female board member to a previously all-male board. The dependent variable in Column (1) captures incumbent female labor share, equal to the fraction of the firm’s employees that are female and incumbents (i.e., they were present in the firm the year before). The dependent variable in Column (2) captures newly hired female labor share, equal to the fraction of the firm’s employees that are female and newly hired (i.e., they were not present in the firm the year before). We employ the [Callaway and Sant’Anna \(2021\)](#) estimator in all specifications. The control group is made up of not-yet (but eventually) treated firms and never treated firms. All specifications include firm-year level controls for return on assets, leverage, firm size, and number of employees two years before the appointment. Standard errors are doubly robust as per the procedure used by [Callaway and Sant’Anna \(2021\)](#). Statistical significance of 10%, 5%, and 1% is denoted by *, **, and ***, respectively.

Table 5: Yearly Predictors of Appointing the First Female Director

Appointing the First Female Director in Year:														
	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Return on Assets	−**	−	−	−	−	−	−	−	−***	−***	+	−**	−	−
Leverage	−	+	+	−**	+	−	+	−	−	+	−	−***	−**	+
Firm Size	+***	+***	+	+***	+	+	+	+**	+**	+***	+***	+***	+**	+*
> Med. Firm Size	−	−	+	−*	−	+**	−	−	+*	−	−	−	−	−
> Med. Female Labor Share	+*	+	+	+	−	+**	+	+	−	−	+	−	−	+
> Med. Board Size	−***	−	−	+	+	−	+	+	−	−	−	+	−	−
Industry Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
N. Obs.	950	850	750	700	700	650	600	600	600	550	500	400	350	250

Notes: In this table, we restrict the data to the full sample of firms that make a first-time female director appointment in the year 2006 or later. In Column (1), we use one observation per firm, and we run an ordinary least squares regression where the dependent variable equals one if the firm makes its first-time female director appointment in 2006, and zero otherwise, and the regressors are the firm's return on assets, leverage, size (i.e., $\log(\text{assets})$), an indicator for whether the firm's size is above the median in the years before the appointment, an indicator for whether the firm's female labor share is above the median in the years before the appointment, and an indicator for whether the firm's board size is above the median in the years before the appointment. We then repeat this estimation for each year from 2007–2019, removing firms that made their first-time female director appointment in a previous year and always setting the dependent variable to be equal to one if the firm makes its first-time female director appointment in the year of interest, and zero otherwise. Statistical significance of 10%, 5%, and 1% is denoted by *, **, and ***, respectively.

Table 6: Heterogeneous Effects Based on Employee Characteristics (Parent Subgroups)

	Female Parent Labor Share	Any Parent Labor Share	Female Parent Earnings Share	Any Parent Earnings Share
	(1)	(2)	(3)	(4)
Pre-Treat	0.000348 (0.00058)	0.000212 (0.00149)	0.00032 (0.00054)	0.00142 (0.00172)
Post-Treat	0.00161** (0.00070)	-0.00027 (0.00137)	0.00151** (0.00069)	0.000542 (0.00176)
$t - 3$	0.000354 (0.00075)	+	0.00018 (0.00068)	+
$t - 2$	0.000342 (0.00061)	+	0.000451 (0.00065)	+
$t = 0$	0.000532 (0.00050)	—	0.000205 (0.00049)	+
$t + 1$	0.00153** (0.00069)	+	0.00108 (0.00068)	+
$t + 2$	0.00162* (0.00086)	—	0.00158* (0.00083)	—
$t + 3$	0.00223** (0.00096)	—	0.00223** (0.00093)	+
$t + 4$	0.00212* (0.00122)	—	0.00247** (0.00115)	+
Controls	✓	✓	✓	✓
Control Group	Not-Yet Treated	Not-Yet Treated	Not-Yet Treated	Not-Yet Treated
N. Obs.	15,000	15,000	15,000	15,000
N. Firms	1,500	1,500	1,500	1,500
N. Treated	1,400	1,400	1,400	1,400
Cont. Mean	0.057	0.166	0.042	0.163

Notes: This table reports estimates of the effects of appointing the first female board member to a previously all-male board. The dependent variable in Column (1) (Column (2)) captures female parent labor share (any parent labor share), equal to the fraction of the firm's employees that are female and parents (parents, regardless of gender). The dependent variable in Column (3) (Column (4)) captures female parent earnings share (any parent earning share), equal to the share of earnings earned by female parents (parents, regardless of gender), as a fraction of the total earnings earned by all employees in the firm. We employ the [Callaway and Sant'Anna \(2021\)](#) estimator in all specifications. The control group in all specifications is made up of not-yet (but eventually) treated firms. All specifications include firm-year level controls for return on assets, leverage, firm size, and number of employees two years before the appointment. Standard errors are doubly robust as per the procedure used by [Callaway and Sant'Anna \(2021\)](#). Statistical significance of 10%, 5%, and 1% is denoted by *, **, and ***, respectively.

Table 7: Effects of Appointing a First Female Director on Employee Sentiment

Dependent Variable: Firm-Year Average Glassdoor Rating						
	Overall Rating	Compensation and Benefits	Culture and Values	Senior Leadership	Career Opportunities	Work-Life Balance
	(1)	(2)	(3)	(4)	(5)	(6)
Pre-Treat	-0.019 (0.052)	-0.028 (0.045)	0.012 (0.071)	0.010 (0.060)	-0.020 (0.051)	0.029 (0.053)
Post-Treat	0.104** (0.046)	0.080** (0.039)	0.139** (0.065)	0.103* (0.056)	0.069 (0.046)	0.054 (0.043)
$t - 3$	-0.026 (0.073)	-0.034 (0.056)	-0.031 (0.094)	0.021 (0.079)	-0.024 (0.069)	0.001 (0.071)
$t - 2$	-0.011 (0.054)	-0.022 (0.053)	0.054 (0.072)	-0.002 (0.065)	-0.016 (0.052)	0.058 (0.059)
$t = 0$	0.007 (0.054)	0.028 (0.049)	0.013 (0.066)	0.032 (0.061)	0.000 (0.057)	-0.004 (0.051)
$t + 1$	0.076 (0.053)	0.100** (0.044)	0.070 (0.069)	0.082 (0.061)	0.089 (0.053)	0.030 (0.054)
$t + 2$	0.079 (0.057)	0.075 (0.047)	0.113 (0.070)	0.026 (0.068)	0.056 (0.056)	-0.002 (0.053)
$t + 3$	0.182*** (0.064)	0.097* (0.054)	0.196** (0.095)	0.176** (0.078)	0.119* (0.065)	0.119** (0.060)
$t + 4$	0.176** (0.077)	0.101 (0.063)	0.305** (0.125)	0.200** (0.093)	0.082 (0.073)	0.127* (0.074)
Control Group	Not-Yet Treated	Not-Yet Treated	Not-Yet Treated	Not-Yet Treated	Not-Yet Treated	Not-Yet Treated
N. Obs	4,309	4,287	2,884	4,286	4,285	4,293
N. Firms	627	625	624	625	625	625
Cont. Mean	3.141	3.206	3.120	2.817	2.897	3.239

Notes: This table reports estimates of the effects of appointing the first female board member to a previously all-male board. The dependent variables capture firm-year average levels of employee ratings across different categories from Glassdoor.com. For each firm-year we compute the average value of the employee reviews posted on Glassdoor across the six dimensions for which reviewers leave 1–5 ratings about the firm: the overall firm rating, the firm’s compensation and benefits, the firm’s culture and values, the firm’s senior leadership, the firm’s career opportunities, and the firm’s work-life balance. We then use these firm-year average ratings values to estimate changes in employee sentiment following the first-time appointment of a female director, using the [Callaway and Sant’Anna \(2021\)](#) estimator. The control group in all specifications is made up of not-yet (but eventually) treated firms. Standard errors are doubly robust as per the procedure used by [Callaway and Sant’Anna \(2021\)](#). Statistical significance of 10%, 5%, and 1% is denoted by *, **, and ***, respectively.

Table 8: Effects of Appointing a First Female Director on Non-Pecuniary Job Features

	Use of Word: “child”	Benefits, Diversity, & Family	Productivity & Effort
	(1)	(2)	(3)
Pre-Treat	-0.001 (0.001)	-0.014 (0.013)	-0.011 (0.009)
Post-Treat	0.003*** (0.001)	0.014 (0.011)	-0.003 (0.008)
$t - 3$	-0.001 (0.001)	-0.020 (0.016)	-0.008 (0.010)
$t - 2$	0.001 (0.001)	-0.007 (0.015)	-0.014* (0.008)
$t = 0$	0.003** (0.001)	-0.010 (0.013)	-0.006 (0.008)
$t + 1$	0.003** (0.001)	0.005 (0.012)	-0.004 (0.009)
$t + 2$	0.002** (0.001)	0.010 (0.014)	-0.001 (0.010)
$t + 3$	0.004** (0.001)	0.017 (0.014)	0.004 (0.011)
$t + 4$	0.004* (0.002)	0.048** (0.021)	-0.008 (0.013)
Control Group	Not-Yet Treated	Not-Yet Treated	Not-Yet Treated
N. Obs	4,309	4,309	4,309
N. Firms	627	627	627
Cont. Mean	0.002	0.111	0.037

Notes: This table reports estimates of the effects of appointing the first female board member to a previously all-male board. The dependent variables capture firm-year average levels of content-specific word-counts in employees’ free-response reviews of the “Pros” of working at the company, from Glassdoor.com. In Column (1), the dependent variable is the firm-year average of the number of instances that the word “child” appears in the review. In Column (2), the dependent variable is the firm-year average of the number of instances that words appear in the review that relate to benefits, diversity, or family: “adoption,” “child,” “dependent,” “diversity,” “family,” “fertility,” “maternity,” “reduced hours,” “work from home,” “kid,” “parent,” “woman,” “women,” “female,” “ivf,” “girl,” “lady,” “ladies,” “gender,” “equity,” “inclusion,” “mother,” “feminine,” “femininity,” “pregnant,” “birth,” “flexible,” “hybrid,” “mom,” “401k,” “dental,” “insurance,” “discount,” “health,” “training,” “holiday,” “retirement,” “gym,” “sick day,” “vacation,” “time off,” and “time-off.” In Column (3), the dependent variable is the firm-year average of the number of instances that words appear in the review that relate to productivity and effort: “sale,” “productivity,” “productive,” “effort,” “goal,” “revenue,” “resourceful,” and “capable.” We then use these firm-year average word-count values to estimate changes in non-pecuniary job features following the first-time appointment of a female director, using the Callaway and Sant’Anna (2021) estimator. The control group in all specifications is made up of not-yet (but eventually) treated firms. Standard errors are doubly robust as per the procedure used by Callaway and Sant’Anna (2021). Statistical significance of 10%, 5%, and 1% is denoted by *, **, and ***, respectively.

Table 9: Heterogeneous Effects Based on Core Committee Assignments

Panel A: Female Labor Share			
	Compensation	Nomination	Audit
	(1)	(2)	(3)
Pre-Treat	-0.00171 (0.00173)	-0.00159 (0.00191)	-0.00078 (0.00184)
Post-Treat	0.00742*** (0.00256)	0.00699*** (0.00264)	0.00658** (0.00274)
$t - 3$	—	—	—
$t - 2$	—	+	—
$t = 0$	+	+	+
$t + 1$	+	+	+
$t + 2$	+	+	+
$t + 3$	+	+	+
$t + 4$	+	+	+
Cont. Mean	0.383	0.384	0.380

Panel B: Female Earnings Share			
	Compensation	Nomination	Audit
	(1)	(2)	(3)
Pre-Treat	-0.00175 (0.00191)	0.000634 (0.00201)	-0.00060 (0.00195)
Post-Treat	0.00566** (0.00258)	0.00717** (0.00280)	0.00868*** (0.00267)
$t - 3$	—	—	—
$t - 2$	—	+	—
$t = 0$	—	+	—
$t + 1$	+	+	+
$t + 2$	+	+	+
$t + 3$	+	+	+
$t + 4$	+	+	+
Cont. Mean	0.298	0.294	0.292

Notes: This table reports estimates of the effects of appointing the first female board member to a previously all-male board. We specifically focus on instances in which the newly appointed female director is also appointed to one of the three key board committees: compensation, nomination, or audit. The dependent variable in Panel A captures female labor share, equal to the fraction of the firm's employees that are female. The dependent variable in Panel B captures female earnings share, equal to the share of earnings earned by female employees, as a fraction of the total earnings earned by all employees in the firm. We employ the [Callaway and Sant'Anna \(2021\)](#) estimator in all specifications. The control group in all specifications is made up of not-yet (but eventually) treated firms. All specifications include firm-year level controls for return on assets, leverage, firm size, and number of employees two years before the appointment. Standard errors are doubly robust as per the procedure used by [Callaway and Sant'Anna \(2021\)](#). Statistical significance of 10%, 5%, and 1% is denoted by *, **, and ***, respectively.

Table 10: Heterogeneous Effects Based Firm Characteristics

Dependent Variable: Female Labor Share				
	Ex Ante Board Size		Board Size Changes	
	Small	Large	Replace Seat	Added Seat
	(1)	(2)	(3)	(4)
Pre-Treat	-0.00306 (0.00195)	0.00009 (0.00162)	-0.00059 (0.00234)	-0.00217 (0.00186)
Post-Treat	0.00495* (0.00291)	0.00696*** (0.00247)	0.00682** (0.00317)	0.00606*** (0.00225)
$t - 3$	—	—	—	—
$t - 2$	—	+	—	—
$t = 0$	+	+	+	+
$t + 1$	+	+**	+*	+**
$t + 2$	+	+*	+*	+*
$t + 3$	+*	+***	+	+***
$t + 4$	+	+***	+**	+**
Controls	✓	✓	✓	✓
Control Group	Not-Yet Treated	Not-Yet Treated	Not-Yet Treated	Not-Yet Treated
N. Obs.	11,000	12,000	9,200	12,000
N. Firms	1,400	1,300	1,200	1,300
N. Treated	650	750	250	750
Cont. Mean	0.394	0.393	0.378	0.393

Notes: This table reports estimates of the effects of appointing the first female board member to a previously all-male board. The dependent variable in all columns captures female labor share, equal to the fraction of the firm's employees that are female. We employ the [Callaway and Sant'Anna \(2021\)](#) estimator in all specifications. The control group in all specifications is made up of not-yet (but eventually) treated firms. All specifications include firm-year level controls for return on assets, leverage, firm size, and number of employees two years before the appointment. In Columns (1) and (2), we estimate effects within the subsamples of firms with small (below median) and large (above median), respectively, pre-treatment levels of board size. In Columns (3) and (4), we estimate effects within the subsamples of firms that replace boards seats or add new board seats, respectively, when the newly appointment female director joins the board. Standard errors are doubly robust as per the procedure used by [Callaway and Sant'Anna \(2021\)](#). Statistical significance of 10%, 5%, and 1% is denoted by *, **, and ***, respectively.

A Appendix - Variable Definitions and Robustness

Table A.1: Variable Definitions

Variable	Definition	Source
Board Characteristics		
Board Size	Equal to the total number of directors on the board.	BoardEx
Frac. Female	Equal to the number of female directors on the board divided by the total number of directors on the board.	BoardEx
Employment Outcomes		
Female Labor Share	Equal to the number of female employees in the firm divided by the total number of employees.	LEHD
Female Earnings Share	Equal to the fraction of total annual earnings within the firm that are earned by women.	LEHD
Other Firm Characteristics		
Firm Size	Log of total assets.	Compustat
ROA	Operating income after depreciation over total assets.	Compustat
Leverage	Equal to the sum of debt in current liabilities and total long-term debt over total assets.	Compustat
Glassdoor Employee Review Outcomes		
Overall	Equal to the firm-year average overall firm rating given by Glassdoor contributors (based on a 1–5 scale).	Revelio
Compensation and Benefits	Equal to the firm-year average compensation and benefits rating given by Glassdoor contributors (based on a 1–5 scale).	Revelio
Culture and Values	Equal to the firm-year average culture and values rating given by Glassdoor contributors (based on a 1–5 scale).	Revelio
Senior Leadership	Equal to the firm-year average senior leadership rating given by Glassdoor contributors (based on a 1–5 scale).	Revelio
Career Opportunities	Equal to the firm-year average career opportunities rating given by Glassdoor contributors (based on a 1–5 scale).	Revelio
Work-Life Balance	Equal to the firm-year average work-life balance rating given by Glassdoor contributors (based on a 1–5 scale).	Revelio

Table A.2: Robustness of Baseline Effects to Alternative Control Groups

	Female Labor Share		Female Earnings Share	
	(1)	(2)	(3)	(4)
Pre-Treat	-0.00083 (0.00165)	-0.00123 (0.00137)	-0.00181 (0.00179)	-0.00182 (0.00147)
Post-Treat	0.00487** (0.00248)	0.00614*** (0.00199)	0.00617** (0.00262)	0.00643*** (0.00200)
$t - 3$	—	—	—	—
$t - 2$	—	—	—	—
$t = 0$	+	+	+	+
$t + 1$	+*	+**	+**	+***
$t + 2$	+	+**	—**	+***
$t + 3$	+**	+***	—***	+***
$t + 4$	+	+***	+*	+***
Controls	✓	✓	✓	✓
Control Group	Never Treated	Both	Never Treated	Both
N. Obs.	20,500	20,500	20,500	20,500
N. Firms	2,300	2,300	2,300	2,300
N. Treated	1,400	1,400	1,400	1,400

Notes: This table reports estimates of the effects of appointing the first female board member to a previously all-male board. The dependent variable in Columns (1)–(2) captures female labor share, equal to the fraction of the firm’s employees that are female. The dependent variable in Columns (3)–(4) captures female earnings share, equal to the share of earnings earned by female employees, as a fraction of the total earnings earned by all employees in the firm. We employ the [Callaway and Sant’Anna \(2021\)](#) estimator in all specifications. The control group in Columns (1) and (3) is made up of only never treated firms. The control group in Columns (2) and (4) is made up of both never-treated firms and not-yet (but eventually) treated firms. All specifications include firm-year level controls for return on assets, leverage, firm size, and number of employees two years before the appointment. Standard errors are doubly robust as per the procedure used by [Callaway and Sant’Anna \(2021\)](#). Statistical significance of 10%, 5%, and 1% is denoted by *, **, and ***, respectively.

Table A.3: Robustness of the Effects on Retention

	Incumbent Female Labor Share
	(1)
Pre-Treat	-0.00280 (0.00204)
Post-Treat	0.01080*** (0.00283)
$t - 3$	-0.00334 (0.00255)
$t - 2$	-0.00226 (0.00221)
$t = 0$	0.00571*** (0.00210)
$t + 1$	0.00848*** (0.00269)
$t + 2$	0.0103*** (0.00306)
$t + 3$	0.0152*** (0.00405)
$t + 4$	0.0141*** (0.00489)
Controls	✓
Control Group	Not-Yet Treated
N. Obs.	15,000
N. Firms	1,500
N. Treated	1,400
Cont. Mean	0.388

Notes: This table reports estimates of the effects of appointing the first female board member to a previously all-male board. The dependent variable captures incumbent female labor share, equal to the fraction of the firm's employees that are female and incumbents (i.e., they were present in the firm the year before). We employ the [Callaway and Sant'Anna \(2021\)](#) estimator. The control group is made up of not-yet (but eventually) treated firms. The specification includes firm-year level controls for return on assets, leverage, firm size, and number of employees two years before the appointment. Standard errors are doubly robust as per the procedure used by [Callaway and Sant'Anna \(2021\)](#). Statistical significance of 10%, 5%, and 1% is denoted by *, **, and ***, respectively.

Table A.4: Heterogeneous Effects Based on Employee Characteristics (Other Subgroups)

Panel A: Employee Subgroup Labor Share				
	College Degree	Age ≥ 40	Black	Hispanic
	(1)	(2)	(3)	(4)
Pre-Treat	0.00323 (0.00197)	0.00044 (0.00202)	0.00013 (0.00067)	-0.00047 (0.00125)
Post-Treat	-0.00208 (0.00230)	0.00125 (0.00290)	0.00113 (0.00117)	0.00055 (0.00205)
$t - 3$	+	+	+	+
$t - 2$	+**	+	-	-
$t = 0$	+	+	+	+
$t + 1$	+	-	-	-
$t + 2$	-	-	+	+
$t + 3$	-	+	+	-
$t + 4$	-	+	+	+
Cont. Mean	0.404	0.526	0.063	0.123

Panel B: Employee Subgroup Earnings Share				
	College Degree	Age ≥ 40	Black	Hispanic
	(1)	(2)	(3)	(4)
Pre-Treat	0.00052 (0.00233)	-0.00180 (0.00217)	0.00061 (0.00050)	-0.00059 (0.00106)
Post-Treat	-0.00009 (0.00287)	0.00105 (0.00295)	0.00126 (0.00085)	0.00060 (0.00183)
$t - 3$	-	-	+	+
$t - 2$	+	-	+	-*
$t = 0$	+	-	+	+
$t + 1$	+	-	+	+
$t + 2$	-	-	+	+
$t + 3$	-	-	+	-
$t + 4$	+	+	+	+
Cont. Mean	0.510	0.649	0.044	0.090

Notes: This table reports estimates of the effects of appointing the first female board member to a previously all-male board. The dependent variables in Panel A capture employee subgroup labor share, equal to the fraction of the firm's employees that are from the specified demographic group. The dependent variables in Panel B capture employee subgroup earnings share, equal to the share of earnings earned by employees from the specified demographic group, as a fraction of the total earnings earned by all employees in the firm. We employ the [Callaway and Sant'Anna \(2021\)](#) estimator in all specifications. The control group in all specifications is made up of not-yet (but eventually) treated firms. All specifications include firm-year level controls for return on assets, leverage, firm size, and number of employees two years before the appointment. Standard errors are doubly robust as per the procedure used by [Callaway and Sant'Anna \(2021\)](#). Statistical significance of 10%, 5%, and 1% is denoted by *, **, and ***, respectively.

B Appendix - Data Compiling and Matching Details

B.1 Compiling Glassdoor Employee Ratings

We begin with the universe of Glassdoor ratings that we have access to via Revelio Labs. We merge in firm-level identifiers (e.g., *gvkey*) based on manual matching performed by one of our research assistants. We drop all ratings from workers located outside of the United States (to align with the fact that the U.S. Census data only covers employees in the U.S.). We drop all ratings from former employees who departed the firm more than two years before the posting. In doing so, the majority of the ratings come from current employees, with some coming from recently departed workers. We then collapse the data to the firm-year level, taking the average rating along a particular dimension for a given firm in a given year. We merge the BoardEx data into the Glassdoor data via *gvkey* and *year*. We then use these firm-year average ratings values to estimate changes in employee sentiment following the first-time appointment of a female director, using the [Callaway and Sant’Anna \(2021\)](#) estimator.