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Real and Capital-Market Effects of Labor Market Frictions: Gender Quotas, Tokenism, and Unionization

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Abstract

Reaching our sustainable development goals (SDG) by 2030 requires an understanding of the mechanisms that might impede or drive the attainment of these goals. This thesis examines real and capital-market effects of labor market frictions that address three of the 17 SDGs, namely gender equality, decent work and economic growth, and reduced inequalities. The first essay identifies gender-specific discriminatory mechanisms that lead to the underrepresentation of women in non-executive and executive board positions in Europe between 2002 and 2019. The second essay finds a significant decrease of board and firm performance after the implementation of mandatory board gender quotas in Europe between 2008 and 2019. The third essay assesses shareholders' perspectives on unionization efforts in the United States between 2011 and 2019. Shareholders only react negatively and instantly at the last certified step of successful unionization efforts when all uncertainty surrounding the election outcome is resolved. Overall, this thesis provides novel empirical evidence on the tradeoffs between different sustainability dimensions. This evidence could help policymakers to specifically address the mechanisms inhibiting the attainment of our SDGs and to further consider the heterogeneity between different countries, industries, and stakeholders' interests.

Contents

List of Figures

List of Tables

Nomenclature

Abbreviations

AAR	Average Abnormal Returns
ACAR	Average Cumulative Abnormal Returns
BvD	Bureau van Dijk
C	Country
C.V.	Coefficient of Variation
CAR	Cumulative Abnormal Returns
CRSP	Center for Research on Security Prices
EB	Executive Board
ESG	Environmental, Social, and Governance
F	Firm
Max.	Maximum
Min.	Minimum
N	Number of observations
NLRB	National Labor Relations Board
NLRA	National Labor Relations Act
OLS	Ordinary Least Squares
PRI	Principles for Responsible Investment
PRO	Protecting the Right to Organize
S	Two-digit SIC code
S.D.	Standard Deviation
SB	Supervisory Board
SDG	Sustainable Development Goals
U.S.	United States
ULP	Unfair Labor Practices
Y	Year

Symbols

#	Number of
%	Percent

\$	United States dollar

1 Introduction

1.1 Goal and Status-Quo Discrepancies

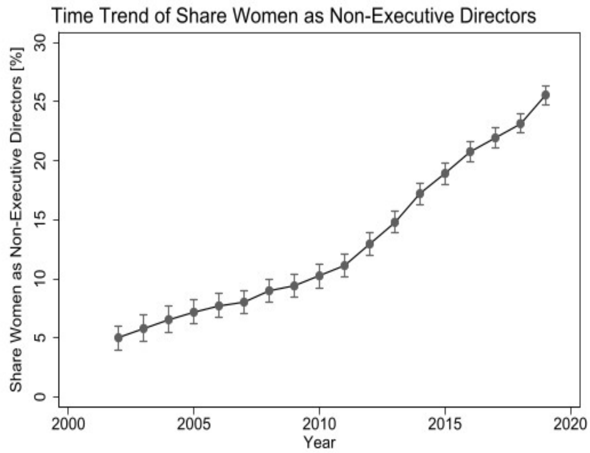
The United Nations Member States are committed to tackle today's challenges and reach the proposed 17 Sustainable Development Goals (SDGs) by 2030. These goals address various sustainability dimensions, e.g. people's health, education, the economy, and the environment (United Nations, 2015). In this thesis, we[1] evaluate mechanisms and regulatory pressures that inhibit or advance sustainable equality and focus on three sustainable development goals: gender equality (SDG 5), decent work and economic growth (SDG 8), and reduced inequality (SDG 10).

One particular facet of inequality, which has been receiving increasing attention in the last decades, is gender inequality. Today, women are disadvantaged in political and economic participation and opportunity in all countries of the world. At the current rate of progress, we will only reach global gender parity in 132 years (World Economic Forum, 2022). Next to women's participation in the labor force, especially women's underrepresentation in technical, senior, legislative, and managerial roles lead to a low economic participation and opportunity subindex of global gender parity (World Economic Forum, 2022). We illustrate this issue in Figure 1.1 and depict women's representation in corporate board positions in Europe. The empirical data indicate that the average gender gap is substantial and widens for more executive positions.

In order to attain the SDG 5 targets of ending all forms of discrimination against women and ensuring women's effective participation and equal opportunities at all levels of decision-making, we first need to understand the causes behind today's shortcomings. Why aren't there more women in leadership positions when women have been earning more college degrees than men for over four decades (OECD, 2020, 2022)? In Chapter 2 of this thesis, we build on organizational

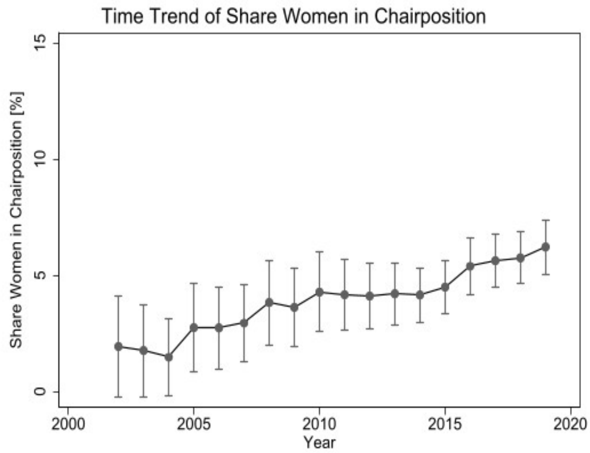
[1] For the sake of consistency and inclusion, the author uses the first person plural in all but one chapter. Chapter 3 is a single-authored essay and is written in the first person singular.

and group-level behavior theories and empirically examine how firm-internal board structures and dynamics impact the appointment probabilities of women to corporate boards. We find that demand-side and gender-based discrimination inhibit women's appointments.



(A) Share Women as Non-Executive Directors

(B) Share Women as Executive Directors



(C) Share Women in Chairposition

(D) Share Women in CEO Position

FIGURE 1.1: Time Trend of Women Directors in Europe

This figure depicts firm averages of the share of women in board positions in 17 European countries. Own illustration based on data from Orbis, further described in Chapter 2.

With more and more governments introducing legislation to promote gender equality and economic empowerment of women, it is further important to assess whether the implemented laws affect real economic outcomes. In Chapter 3, we examine how an exogenous shock to the board of directors, induced by mandatory gender quotas, affects board dynamics and firm outcomes. Our market- and accounting-based measures of board and firm performance decrease after quota implementations. These effects are strongest in the male-dominated industries mining, construction, and manufacturing.

Another important facet of inequality, relevant to society, is income inequality. Recent labor movements in Britain demonstrate the relevance and timeliness of income inequality. On February, 1st 2023, in the biggest walkout in over a decade, half a million workers striked across Britain and demanded wages matching the rising prices of the inflation (CNN, 2023). Over the past 100 years, economists have observed and explained determinants of the inverse relationship between U.S. income inequality and union density, as shown in Figure 1.2. Farber et al. (2021) find that between the 1940s and 1960s, disadvantaged groups such as the less educated and nonwhite households had higher union representation and experienced large family income premiums. Finally, residual income inequality is compressed for union households and may explain the mid-century's low levels of inequality during the era's high levels of union density (Farber et al., 2021).

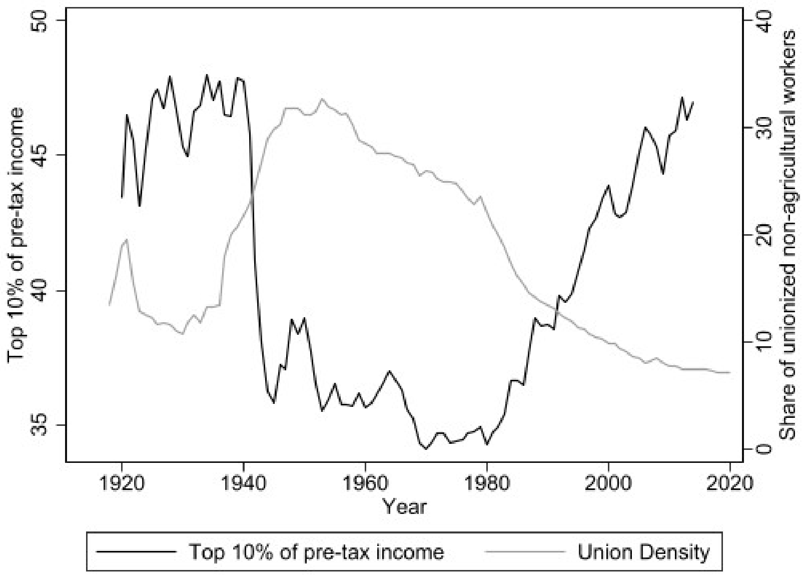


FIGURE 1.2: Time Trend of Union Density and Income Inequality in the United States

This figure is an own illustration that follows Farber et al. (2021) and depicts the time trend of union representation and income inequality. We gather top-share individual income inequality from Piketty et al. (2018). The top 10% of pre-tax income depicts the share of pre-tax income going to the top 10% of adult earners. We collect union density data from Troy (1965), Freeman (1997), and Macpherson and Hirsch (2022). Union density is the number of unionized workers as a share of the non-agricultural workforce.

Next to improving income inequality (SDG 10), unions can help reach particular targets of decent work for all (SDG 8). These include protecting labor rights, promoting safe and secure working environments, strengthening the use of social dialogue, and equal pay for work of equal value.

One particular target includes the implementation of the Global Jobs Pact of the International Labour Organization by 2020. This explicitly mentions the right to organize and the effective recognition of the right to collective bargaining (ILO, 2009).

If collective bargaining can improve income equality and decent work for all, why have we been observing a steady drop in union membership rates in the last half-century (Macpherson and Hirsch, 2021)? The final chapter (Chapter 4) of this thesis examines capital-market effects of unionization. We closely observe when and how the market reacts to different steps of the unionization process. The process of establishing a union in the United States is complicated and often influenced by the employer. Therefore, it has often been criticized and mentioned as an important reason for today's low union membership rates (New York Times, 2022b,a). We shed light on the shareholders' perspective and show that shareholders expect unionization efforts and anticipate objections, disturbances, and outcome uncertainties during the unionization process. They react instantly and negatively at the final step, the public announcement of successful union certifications.

The mechanisms we observe in this thesis, discrimination, quotas for the minimum representation of a minority group, and unionization can cause labor market frictions. These include search, matching, and reallocation frictions that lead to employee misallocations, unemployment, and reduced net employment adjustments for the affected firms (Bilal et al., 2022). Especially firms operating in more rigid labor markets face higher costs and delays when hiring workers (Fajgelbaum, 2020). We want to shed light on the roles of different actors and regulatory pressures toward more sustainable equality and examine the economic magnitudes of particular mechanisms and the frictions they produce.

Throughout this thesis, we empirically investigate the effects of specific labor market frictions with facility- and firm-level archival data. We seek to overcome endogeneity problems and speak to true cause-and-effect relationships with different empirical identification strategies. As we can never observe potential outcomes for the same unit of analysis with and without these labor-market frictions, we need to compare different firms' average outcomes. However, firms subject to particular labor market frictions might be characterized by other unobservable factors that confound with our outcome variables. In order to overcome this potential endogeneity of our independent variables, we employ instrumental variable, difference-in-differences, and event study approaches.

In the next subchapters, we shed light on sustainability from a firm's perspective and provide a brief background on specific targets of the following SDGs: gender equality, decent work and economic growth, and reduced inequality. Next, we discuss each chapter's research questions and define the focus and scope. We provide information on the methodologies used to identify causal effects and give a brief overview of our empirical results. The subsequent chapters are based on papers published in or submitted to peer-reviewed academic journals. Thus, each chapter also represents a stand-alone essay with an individual contribution.

1.2 Background: Sustainable Equality in the Labor Market

1.2.1 Sustainability as a Business Strategy

Besides intrinsic motivation to contribute to the sustainable development goals, firms increasingly adapt their strategies in favor of SDG targets to comply with investor demands (United Nations, 2021). The concept of Environmental, Social, and Governance (ESG) scores is first officially proposed in the 2004 United Nations Global Compact Report and endorsed by twenty major financial institutions pledging to integrate ESG considerations into investment analysis and decision-making (United Nations, 2004). Two years later in 2006, the launch of the United Nation's Principles for Responsible Investment (PRI) promotes a wider adoption of the ESG concept in the investment process. In 2021, more than 120 trillion U.S. dollars in assets under management are managed by 3,826 PRI signatories (United Nations, 2021).

Due to the increasing consideration of ESG metrics, sustainability has become an essential part of firms' business strategy as a long-term source of competitive advantage (Khan et al., 2016, Welch and Yoon, 2022). There exists substantial, but mixed, empirical evidence on the relationship between ESG and financial performance. However, scholars agree that the evidence points towards a positive relationship (Huang et al., 2020, Velte et al., 2020, Vishwanathan et al., 2020). For example, superior ESG performance is positively associated with enhanced access to financing resources and lower cost of capital (Cheng et al., 2014, Bolton and Kacperczyk, 2021).

A recent stream of literature also investigates the relationship between ESG performance and various non-financial metrics. Amorelli and García-Sánchez (2020) find a positive association between board gender diversity and corporate social responsibility disclosure. Others find that specific dimensions of ESG, namely human rights, governance (Beji et al., 2021), and environmental dimensions (Liu, 2018), are positively associated with increased board gender diversity. Ertugrul and Marciukaityte (2021) and Heitz et al. (2021) find that unions extract welfare for their members at the expense of external stakeholders and are associated with lower ESG scores. This evidence exemplifies how various dimensions of sustainability can advance or inhibit each other and stresses the importance of understanding cause-and-effect relationships.

1.2.2 Representation of Women in Leadership Positions

Even though Europe has the smallest, but still substantial, gender gap in political empowerment, it ranks only third on the economic participation and opportunity subindex (World Economic Forum, 2022). Consequently, the low representation of women in leadership and board positions is receiving considerable attention in the academic literature and policy debates.

Barriers to women's upward mobility negatively affect gender parity and can cause labor market frictions, because an important part of the talent pool is neglected. In fact, women directors have been found to enhance the board's human and social capital through more diverse knowledge and experience (Adams and Ferreira, 2009, Terjesen et al., 2009, Kim and Starks, 2016). Furthermore, empirical evidence shows that women directors have better board meeting attendance records, lead to higher research and development investments, and lead to more efficient innovation processes (Schwartz-Ziv, 2017, Bernile et al., 2018). On the other hand, increased diversity has been found to create conflict, subgroup formation, and create inter-group bias (Williams and O'Reilly, 1998, Hewstone et al., 2002, Talke et al., 2010), which could negatively affect firm performance. Yet, we still know little about the drivers and impediments to attain diversity in the boardroom.

Various supply- and demand-side factors have been discussed as impediments to gender diversity on corporate boards. Supply-side factors include education, work experience, qualification in certain areas of expertise, career interruptions, and preferences for competition (Niederle and Vesterlund, 2007, Bertrand et al., 2010, Flabbi and Tejada, 2012, Maggian et al., 2020). At the same time, demand-side factors including institutional barriers, unconscious and conscious discriminatory, and stereotypical biases are found to contribute to a "glass ceiling"[2] blocking

[2]Glass ceiling is defined as *"an intangible barrier within a hierarchy that prevents women or minorities from obtaining upper-level positions"* (Webster, 2023).

women's upward mobility (Bjerk, 2008, Bertrand et al., 2019, Finseraas et al., 2016, Field et al., 2020).

Case studies and testimonials corroborate the theories from the academic literature on the low representation of women in leadership positions. In October 2022, we interviewed women in different positions at global institutional investors.[3] We only considered women without career interruptions as they are often used as supply-side arguments for advancement differences between men and women (Bertrand et al., 2010) and focus on a typically male-dominated industry. The financial services industry has made significant progress in terms of gender composition, with 32.4% women at the board level (Deloitte, 2021). However, women are still shut out of executive positions and manage smaller portfolios. In 2021, for every dollar in a portfolio managed by a man, a woman only managed 72 cents (Citywire, 2021).

Interestingly, the observations from our interviews have direct parallels to empirically supported theories from the academic literature.

"I think where it does make a difference is the seniority of positions. Especially as a young woman, you're considered more junior than a man. Let's take a thirty-year-old woman and a thirty-year-old man. The man is seen as experienced and the woman is still seen as a sweet little doll, in some cases. And I think that has an impact on what responsibility you get and what grading and seniority level you get. So I think our male employees will be promoted to a higher seniority level even faster and of course they will have a higher salary."

— Interviewee, Executive at institutional investor firm

This observation relates to the stream of literature on selection procedures for men and women. Spence (1973), Finseraas et al. (2016), and Guo et al. (2020) find that women need stronger competence signals and often additional skills in education, reputation, and career experience compared to men to be appointed or promoted. These mechanisms help explain the underrepresentation of women in leadership positions.

"Most of the leaders have been men in the past, it's a very masculine industry as we all know. And other men function similarly to those who are already there. It's easier to promote someone who is similar to you, maybe that's a theory."

— Interviewee, Senior Manager at institutional investor firm

[3] Yilun Xu conducted interviews as part of his Master thesis at the Chair of Management Accounting under the supervision of Eline Schoonjans.

Again, this observation directly relates to homophily theory (Pfeffer and Salancik, 1978) and inter-group bias (Hewstone et al., 2002), where groups tend to pick new members in line with their own profile. This behavior has been found empirically in corporate and leadership settings (Westphal and Stern, 2007, McDonald and Westphal, 2013, Zhu and Westphal, 2014, Gabaldon et al., 2016). A parallel stream of literature highlights that board directors are traditionally recruited from a limited pool of socially-connected candidates. These traditionally male-dominated networks may hinder women to enter top-management positions (Zimmerman, 2019, Michelman et al., 2022). Today, we are missing cross-country empirical evidence on demand-side factors of board appointment dynamics that takes into account the international market for board positions and varying board structures.

1.2.3 Mitigation of Women's Underrepresentation

Policymakers have been addressing the underrepresentation of women in decision-making positions for over three decades. First, political reservations appeared, where political seats at the sub-national level had to be reserved for women. For example, Argentina required all political parties to nominate 30% women in the early 1990s. Empirical evidence shows that these quotas are followed by greater legislative attention to the interests and priorities of women as a group (Chattopadhyay and Duflo, 2004).

Since 2008, countries have been implementing similar legislation for positions in corporate decision-making positions. Several European countries have implemented voluntary and mandatory quotas for women directors. Mandatory quotas induce significant sanctions for non-compliance, including fines, the cancellation of director benefits and compensation, and the nullification of director appointments.[4] Voluntary quotas refer to comply-or-explain and discretionary target rules without strict enforcement. Figure 1.3a provides an overview of mandatory gender quotas across Europe until 2019.

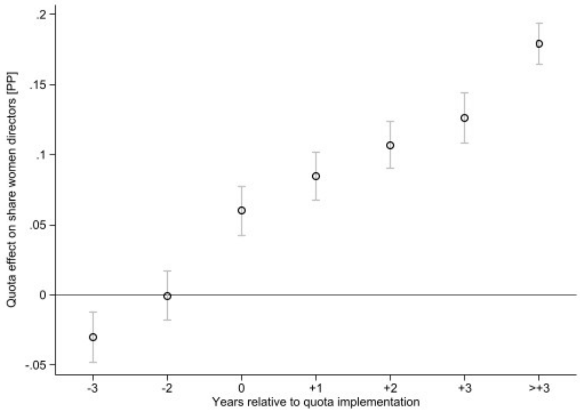
Depending on the board structure, these quotas can either apply to all board directors or only to non-executive directors. Whereas Italy, France, Belgium, and Portugal implemented quotas between 2008 and 2019 for the representation of women for both executive and non-executive directors, Norway, Germany, and Austria explicitly exclude executive directors.[4]

[4]We gather individual requirements and sanctions of the quotas from national law summaries (Regjeringen, 2003, Gazzetta Ufficiale Della Repubblica Italiana, 2011, Secrétariat général du Gouvernement, 2011, Bundesanzeiger Verlag, 2015, Moniteur Belge, 2011, Bundesgesetzblatt für die Republik Österreich, 2017, Diário da República eletrónico, 2017).

In Europe, an essential distinction can be made between monistic one-tiered (Anglo-Saxon) structures and dualistic two-tiered boards traditionally predominant in continental Europe. Some European countries allow both 'one-tier' and 'two-tier' board structures. While two-tiered boards prescribe a strict separation of executive and non-executive directors, one-tiered systems combine executive and non-executive directors on a unitary board, sometimes including a dual CEO-Chairman (Gelter and Siems, 2021). The main tasks of members of dualistic executive boards and executive directors on one-tiered boards include strategy development and day-to-day operations of a company. Members of dualistic supervisory boards and non-executive directors on one-tiered boards are responsible for advising, monitoring, and deciding about the remuneration and appointment of executive directors (Davies and Hopt, 2013). While executive directors perform their tasks as a full-time job, non-executive directors often have multiple mandates in different firms.

Prior studies and our empirical data indicate that the percentage of women on the board increases significantly after the introduction of gender quotas. Moreover, mandatory quotas that enforce sanctions for non-compliance are most effective in reaching the targets (Arndt and Wrohlich, 2019). In Figure 1.3b, we depict the increase in the share of women directors following the mandatory quota implementations in Europe. The year before the quota implementation represents our baseline and we observe small anticipatory effects. By the second year after quota implementation, firms have increased their share of women directors by more than ten percentage points.

(A) Countries with Mandatory Quotas



(B) Effectiveness of Mandatory Quotas

FIGURE 1.3: Prevalence and Effectiveness of Mandatory Board Gender Quotas

In Figure 1.3a, we accentuate the seven European countries that have implemented a mandatory quota between 2008 and 2019. Mandatory quotas refer to regulations that are enforced with sanctions for non-compliance. Figure 1.3b depicts the average percentage point increase in the share of women directors following the implementation of these European mandatory gender quotas. The baseline reference category is the year before quota implementation and other European countries that did not implement a board gender quota. The intervals are pointwise 95% confidence intervals for the corresponding elements of the event-time path of the share of women directors. Both figures are own illustrations based on data from Orbis and Refinitiv, further described in Chapter 3.

Even though the quotas' effectiveness on board diversity has been shown empirically in many countries, further effects on firm dynamics and performance remain unclear. Still, countries continue implementing quotas, such as Greece in 2021 and the Netherlands in 2022, without extensive economic assessments of existing quotas. Some empirical studies have investigated first-order and second-order effects in Norway, the first country with a board gender quota (Ahern and Dittmar, 2012, Matsa and Miller, 2013, Bertrand et al., 2018). However, we have no empirical evidence on mandatory gender quotas from cross-country studies. Furthermore, sparse attention has been drawn to the direct effects of the quota on board dynamics and outputs. Especially, board monitoring outputs have not been examined thoroughly in the quota setting. In non-quota settings, board diversity is associated with increased board monitoring quality. Women are found to be more independent, to be tougher monitors, to increase earnings quality, and to act more ethically in ambiguous situations (Adams and Ferreira, 2009, Gul et al., 2011, Srinidhi et al., 2011, Wahid, 2019). This stream of literature raises the question as to whether these benefits remain if firms are mandated to appoint women.

In the United States, only one state has implemented a quota so far. Californian-listed firms had to comply to the first step of the new law (at least one woman on the board) by 2020. However, the secretary of state's office says that only about 42% of the 716 publicly-held corporations with a principal executive office in California showed that they were complying with the law's requirements in their 2021 company disclosure statements (CNN, 2022). Moreover, in 2022, judges ruled that the quota law violates the Equal Protection Clause of the California Constitution. For now, this ruling further prevents the law's enforcement (CNN, 2022). The U.S. setting demonstrates that even the effectiveness of such quotas can vary immensely depending on the countries' institutional environment and stresses the importance of evaluating regulations across countries.

1.2.4 Collective Bargaining through the Unionization Process

Next to legal disputes over board diversity mandates, we observe increased public and legal attention to labor disputes in the United States. In 2022 for example, Amazon is found guilty of violating labor law during unionization processes at two warehouses on Staten Island. The judge ruled Amazon's threats to withhold wage and benefit increases and the removal of digital invitations by co-workers to sign a petition as illegal. Other accusations were dismissed, including promises to subsidize workers' educational expenses if they chose not to unionize and indications that workers would be fired if they unionized and failed to pay union dues. Finally, Amazon did not get charged for mandatory anti-union meetings at which supervisors questioned the credibility of the Amazon Labor Union. Under labor board precedent, such meetings are legal (New York Times, 2023).

Even though employers' efforts of hindering their employees to join a union are considered as unfair labor practices (ULP) and violate the United States National Labor Relations Act (NLRA) of 1935, we increasingly observe managerial opposition to unionization efforts in the United States. Union-busting campaigns include captive audience meetings, threats to close plants, cut benefits, and discharge workers (Bronfenbrenner, 2009, New York Times, 2022b,a). The number of unlawful employee discharges and employer intimidation charges filed per election has continued to rise through the twenty-first century (Magner, 2021). The Economic Policy Institute finds that U.S. employers are charged with violating federal law in over 40% of all union election campaigns and spend roughly $340 million annually on "union avoidance" consultants (EPI, 2019). For example, union avoidance consultants reported receiving $837,000 from FedEx between 2014 and 2018. In one out of five campaigns, employers are even charged for firing workers that participate in union activity. In Figure 1.4, we depict the frequency of different unfair labor practice charges filed against employers between 2015 and 2018 in the United States (EPI, 2019).

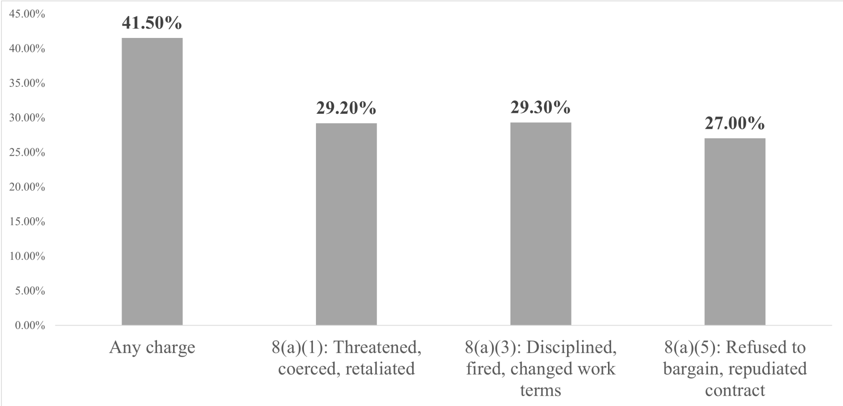


FIGURE 1.4: Share of Elections with Charges Filed against the Employer

Illustration from EPI (2019) and their analysis of National Labor Relations Board election data for calendar years 2016–2017 and Unfair Labor Practice (ULP) filings from fiscal years 2015–2018. ULP charges are charges that are filed against an employer who violated the terms of the National Labor Relations Act (Section 8(a) of the labor code) by interfering with workers' rights to form a union and bargain collectively (EPI, 2019).

An extensive stream of literature examines real effects of unions. Freeman (1976) introduces the *collective voice theory* as the main benefit of organized labor through unions. He argues that unions are able to improve overall workplace conditions, which affect productivity and subsequently have a positive economic impact on firm performance. The underlying idea is that employees no longer have to communicate with management as individuals but rather have a union that speaks for a collective of employees who share the same views. Moreover, unions can reduce residual income inequality, wage dispersions among more and less skilled workers, more and less disadvantaged groups, and between higher and lower-paying establishments (Freeman, 1980, Card et al., 2020, Farber et al., 2021). Freeman and Medoff (1979) coins the concept of unions' *monopolistic labor supply*. Through collective bargaining activity, unions can raise labor prices above the market level, causing inefficiencies and frictions in the labor market. This wage premium might be justified if unions are indeed able to increase productivity and profits (Blanchflower and Bryson, 2017). However, previous research mainly argues that these premiums, enforced by rent-seeking unions, are at the expense of firm profits and redistribute profits from the owners of capital to the workers, therefore imposing an indirect tax on normal profits (Clark, 1984).

The different steps of the unionization process in the United States have not yet been examined from a shareholder's perspective. The setting in the United States is very particular and different from many European countries. First, workers directly vote for their bargaining representative at the workplace. Second, employers' counter-campaigns often result in 15% to 20% drops in union support during elections and are rarely sanctioned as unfair labor practices (New York Times, 2022a). This increased illegal or legal coercion and anti-labor laws lead to a discrepancy between actual and demand for union representation in the United States. In the last century, between 50% and 70% of Americans approved of unions (Gallup, 2020), whereas only 11% of employees and 6% of private-sector employees are union members or covered by collective bargaining agreements in the United States today (Macpherson and Hirsch, 2021). Therefore, an open question remains how shareholders react to this particular process of unionization.

The legal process of unionization in the United States varies in complexity, but it involves similar key events: *Showing of Interest*, *Filing of Petition*, *Investigation and Election Agreement*, *Election*, and *Certification and Case Closing*.[5] Employees *show* their *interest* with a list of signatures of at least 30% of the workplace in support of unionization.[6] The National Labor Relations Board (NLRB) officially *files* the *petition* and distributes it to all related parties. Next, the NLRB *investigates* the case and publicly announces an *election agreement*. The *election* by secret ballot allows the employees to either vote for or against (one of) the proposed union(s) as their collective bargaining representative. The union wins the *election* if it receives a simple majority, that is, more than 50% of the valid votes. The votes are counted immediately after the voting polls close, but the parties involved can still object to the election result within seven days. Once potential disputes are resolved, the NLRB publicly *certifies* the result and *closes* the unionization process. In case of majority, the union becomes the elected bargaining representative. If the NLRB certifies a union loss, it does not allow another election at the same workplace for the following twelve months.

[5] The steps are explained in detail in the NLRB Outline of Law and Procedure in Representation Cases (National Labor Relations Board, 2017) and the NLRB Casehandling Manual, Part Two, Representation Proceedings (National Labor Relations Board, 2020).

[6] Generally, the employees of a workplace are a group of employees that belong to the same employer subdivision such as a facility, a warehouse, or other appropriate subdivisions of the employer company. The NLRB's definition of an appropriate employer subdivision is intentionally open to further interpretation.

1.3 Research Questions, Designs, and Results

1.3.1 Appointment Dynamics of Women as Directors

Chapter 2 of this thesis aims to provide an understanding of demand-side factors in the appointment dynamics of board directors. We examine how discriminatory and institutional barriers lead to gender-specific appointment dynamics. Moreover, we question whether a voluntary or mandatory increase in the representation of women on corporate boards hinders (*saturation* effect) or promotes (*exposure* effect) the appointment of additional women.

We extend single-country studies by Farrell and Hersch (2005), Gregory-Smith et al. (2014), and Smith and Parrotta (2018), who find that fewer women already on and women leaving the board are associated with higher probabilities of appointing additional women. They observe these effects in the 1990s in the United States, the late 1990s and 2000s in the UK, and the 2000s in Denmark. With our European cross-country study, we consider the increasingly international market for top managers, the heterogeneous institutional contexts, and the representation of women via quotas.

Moreover, we add to Matsa and Miller (2011) and Bozhinov et al. (2021) and their analyses of diversity spillover effects in the United States and Germany. We explicitly differentiate between non-executive and executive roles of board members and examine spillover effects in appointment dynamics. As non-executive directors are responsible for appointing executive directors, the dynamics we expect inside the board could also spill over from non-executive to executive directors.

We determine the probability of appointing a woman as non-executive or executive director in linear fixed effect probability models. Our sample includes 3,353 publicly-listed firms in 17 European countries between 2002 and 2019. We combine data from several sources. We obtain detailed information on board members and firm ownership from the ORBIS database provided by Bureau van Dijk. Financial information stems from Worldscope provided by Refinitiv. From OECD, we obtain the GDP per capita, total employment rate, and women's participation in the labor force.

Combined, our results confirm that board director appointments are gender specific and suggest that demand-side factors such as explicit and implicit norms drive women's appointments up to a certain threshold. First, women are more likely to be appointed to non-executive than executive roles. Second, we find that the appointment likelihood for women declines if more women are already on the board. Third, we show that the appointment likelihood for women is significantly larger when a woman leaves, compared to when a man leaves the board. Fourth, we do not find spillover effects from non-executive to executive boards.

These findings could reflect *t(w)okenism*, where women are primarily appointed as "tokens" to signal compliance with implicit or explicit norms. Tokens act as representatives of their category but have limited influence on corporate decisions (Kanter, 1977b). Recently, the *twokenism* norm with two women on the board has replaced *tokenism* in many firms and industries (Chang et al., 2019). In cross-sectional analyses, we find stronger evidence for gender-specific appointment dynamics before reaching gender balance in environments with increased external demand for and decreased supply of women director candidates.

Next to accounting for country, firm, and board characteristics, these results are robust to dynamic and econometric model specifications that address potential endogeneity concerns using matching and instrumental variables. We construct a data-driven instrument that is uncorrelated with the product of heteroscedastic errors (Lewbel, 2012).[7] Instead of regressing the dependent variable on a potentially endogeneous independent variable, our instrumental variable approach regresses the dependent variable on a predicted independent variable. The predicted values are obtained from a first-stage regression with an exogenous instrumental variable.

Our results have two important implications for practitioners and policymakers. First, we show that demand-side labor market frictions impede women's appointments. Therefore, solely relying on market mechanisms will not close the gender gap. Second, we find *saturation*, rather than *exposure* effects on appointment probabilities. These board dynamics reveal how women's representation will evolve beyond quota thresholds. Before reaching gender balance, increasing the participation of women through quotas does not lead to additional voluntary women appointments. Our results suggest that quotas are not "self-running".

1.3.2 Effects of Board Gender Quotas on Board and Firm Performance

In Chapter 3, we exploit the countries' varying quota implementation years to understand how they affect firm performance and board monitoring quality. The *captured boards theory* and the

[7] See Baum and Lewbel (2019) for a more detailed discussion of the method.

theory of efficient boards predict opposite effects of quotas on performance. On one hand, an exogenously enforced increase in board diversity could break up the existing barriers from gender-based biases (Finseraas et al., 2016, Schoonjans et al., 2023) and old boys networks (McPherson et al., 2001, McDonald and Westphal, 2013, Michelman et al., 2022). On the other hand, if shareholders choose boards that maximize value, laws that constrain shareholders' choices can decrease firm value (Demsetz and Lehn, 1985). Women directors appointed through quota processes might differ from voluntarily appointed women directors, especially if there is a lack of eligible candidates (Ahern and Dittmar, 2012). Exogenously dictated gender quotas could affect board attributes other than gender and, ultimately, negatively impact group dynamics.

So far, the existing empirical evidence of quota effects focuses on single countries and is mixed. Previous studies find that the Norwegian quota has a negative effect on firms' market and accounting performance (Ahern and Dittmar, 2012, Matsa and Miller, 2013, Bertrand et al., 2018). Studies from France show negative and mixed effects (Bennouri et al., 2018, Kuzmina and Melentyeva, 2021), whereas Italy shows some evidence of positive and no impacts on firm performance and market reactions (Comi et al., 2019, Kuzmina and Melentyeva, 2021, Ferrari et al., 2022).

To the best of our knowledge, our study is the first to investigate a mandatory quota effect on firm performance in an exhaustive European multi-country quasi-experimental setting. Kuzmina and Melentyeva (2021) examine performance effects in a multi-country setting, but include voluntary quota-implementing countries such as Spain and the Netherlands in the treatment group. Moreover, we are first to causally examine a quota effect on earnings management, a measure of board monitoring performance. Damak (2018) and Saona et al. (2018) estimate the relationship between board gender diversity and earnings management and include quota-implementing countries, but do not employ a difference-in-differences design to identify causal effects.

Our empirical approach exploits a quasi-natural experimental setting in Europe. We estimate the quota effect on firm and board performance using a staggered difference-in-differences design with a variety of fixed effects and control variables. The quotas are exogenously imposed on the firms and their implementation is not subject to selection bias on the firm level. In our difference-in-differences design, we compare firms before and after the quota implementations that are subject to a quota with firms that are not. Additionally, due to the staggered implementation of the quotas, we can estimate effects for a restricted sample of quota-adopting countries only. This approach mitigates concerns about fundamental differences between quota-adopting and non quota-adopting countries. It implicitly takes as the control group all firms that do not implement a quota in a specific year, even if they have implemented or will implement a quota at some point in the past or future (Bertrand and Mullainathan, 2003).

Our sample counts 7,055 publicly-listed firms in 16 European countries between 2001 and 2019, where 1,293 firms from seven European countries are eventually subject to the quota. Again, we combine data from several sources. We obtain detailed information on board members and firm ownership from the ORBIS database provided by Bureau van Dijk. Financial information stems from Worldscope and analysts' forecast information from I/B/E/S, both provided by Refinitiv. From OECD, we obtain the GDP per capita, total employment rate, and women's participation in the labor force.

Our results show that an exogenous shock to the board of directors, induced by mandatory gender quotas, has negative effects on board- and firm-level performance outcomes. We measure market-based firm performance with Tobin's Q and find a significant and economically meaningful average decline of 6%, evaluated at the treatment firms' mean. Further, we find a significant increase of around 8% in earnings management, measured by absolute discretionary accruals and evaluated at the treatment firms' mean.

In our cross-sectional analyses, we find that the negative quota effect on firm performance is strongest in Norway and France, in typically "male-dominated" industries, and for firms with higher internal and external measures of monitoring quality. We find a stronger negative quota effect on performance for firms with higher ex-ante shares of independent directors and lower ex-ante levels of earnings management. A higher number of analysts following, as an external indicator for monitoring quality, is also associated with a stronger negative quota impact on firm performance.

All results hold within a sample of quota-adopting countries only and are robust to alternative and industry-adjusted measures of firm performance and earnings management. We further control for important board capital attributes such as gender, age, tenure, and multi-directorship and show that our main results capture direct quota effects on board- and firm-level outputs.

Our results suggest that imposing restrictions on specific board attributes can produce inefficient board dynamics and negative real effects. We deduce two important policy implications. First, policymakers need to evaluate the tradeoffs such mandates entail. Specifically, mandatory quotas are effective in reducing gender inequality at the board level (SDG 5). However, we show that, on average across all European countries that implemented a mandatory quota, firm performance decreases significantly (SDG 8). Second, imposing a general mandate disregards the heterogeneity across firms and industries that have varying restrictions. The negative quota effect we observe is stronger for boards with higher monitoring quality before the quota and in industries where the supply of suitable women candidates is limited.

1.3.3 Timing of Capital-Market Effects of Unionization

With the gaining momentum of unionization efforts, we seek to shed light on their resulting capital-market effects in today's institutional setting in Chapter 4. We explore when and how shareholders update their expectations about the potential costs and benefits of unionization at publicly-listed firms and their subsidiaries in the United States between 2011 and 2019.

Previous studies find negative capital-market effects of unionization, yet the evidence on the timing of these effects diverges. Ruback and Zimmerman (1984) find significant negative capital-market effects in the months before and of the *petition filing*. In contrast, a more recent study finds the effects to materialize in the months after unionization case *closings* (Lee and Mas, 2012). Election outcomes might not have been as uncertain in the 60s and 70s, the sample period Ruback and Zimmerman (1984) examine, as they have been in the last decades. We have been observing a steady increase in union-busting activities, objections, allegations, and complaints per election since the Joy Silk doctrine was abandoned in 1969 (Magner, 2021). Moreover, for the first time in almost a century, employer-favoring labor laws dominate the legal environment in the US. Specifically, between 2011 and 2019, five additional U.S. States adopted right-to-work laws, leading to more U.S. States with than without these laws.

Based on the prevailing semi-efficient market hypothesis, we expect unionization effects to materialize rapidly when the new publicly-available information diverges from shareholders' conditional expectations. We exploit detailed and daily data on four key events of the unionization process, *petition filing*, *election agreement*, *election*, and *case closing*. Thereby, we are first to conduct a short-horizon event study on unionization effects. Moreover, we are first to identify an important driver of heterogeneity in shareholders' conditional expectations and resulting capital-market effects. We examine whether the timing of capital-market effects differs for firms with and without previous unionization experience.

To determine the average short-term capital-market effect of unionization, ideally, we would compare the actual daily return data of the affected firm's corresponding security at the time of unionization with the daily returns that the firm's corresponding security would have achieved under normal market conditions i.e., without unionization. To make this assessment, we conduct a short-horizon event study. We estimate the effect of unionization on the market value of the affected firms by calculating daily abnormal returns in a short period of five days around the event date, the event window. Following standard short-horizon event study methodology (MacKinlay, 1997), we calculate the abnormal returns by subtracting the corresponding security's actual daily returns in the event window from the expected daily returns for the event window under normal market conditions in absence of the event. We predict the expected daily returns from the market model in an estimation window of 200 trading days 200 days prior to the event window.

Next to the short horizon of our analysis, our study has one more advantage to ease concerns of confounding events.[8] We estimate an average effect of unionization events that occur at four steps across different firms at different times. Confounding events would have to systematically occur at the same time as the event dates of each step of the different firms to impose significant distortions to the measured abnormal returns (Larcker et al., 2011).

We collect data from four sources. We use data on unionization in the United States from the NLRB, information on corporate ownership from Bureau van Dijk's (BvD) Orbis, and stock-market and financial data from the Center for Research on Security Prices (CRSP) and Refinitiv. We derive our main sample of 763 union representation cases from 3,111 cases closed by the NLRB in the United States between 2011 and 2019 that we are able to match to publicly-listed firms with complete stock-market and financial data.

We find four results that, combined, support the semi-efficient market hypothesis in the setting of union elections. First, stock prices decrease for successful unionization efforts with leaks and lags of one to two trading days. For successful unionizations, the average cumulative abnormal return of the last certification step is -0.27% and corresponds to average market value losses of $130,000 per firm and $753 per newly unionized worker. These effects are more pronounced with increasing vote share in support of the union and persist when controlling for election-, firm-, and industry-specific characteristics.

[8] A confounding event takes place around or during the same time as the event of interest and might therefore influence the measured abnormal returns in the event window and distort their attributability. Examples include macroeconomic events, such as government regulations, or firm-specific events, such as earnings announcements or news about a new product line.

Second, shareholders update their expectations about future firm performance with public rather than private information. We do not find significant abnormal returns at certification steps that are not publicly announced by the NLRB.

Third, the market mainly reacts to public information that resolves the uncertainty about the election outcome. Shareholders' reactions suggest that they do not fully anticipate the election outcome before the NLRB officially and publicly certifies it.

Fourth, we show that shareholders of firms that experience a first-time election in a three-year period do not expect unionization efforts strong enough to conduct an election. They already react negatively to the *petition filing* and *election agreement*. Contrarily, firms with unionization experience expect unionization efforts and are especially aware of the uncertainty surrounding the election process and only react at the last certification step when successful unionization becomes certain.

Our results suggest that, on average, the market perceives successful unionization as detrimental to future firm performance, expects unionization efforts, and mainly reacts to public and certified outcome announcements. Policymakers need to consider that union-busting campaigns before and vote challenges after the election also increase shareholders' uncertainty during the unionization process.

1.4 Structure of the Thesis

The remainder of the thesis is organized as follows. Chapter 2 empirically investigates how ex-ante board diversity and the gender of a departing board member impact the appointment probability of women to non-executive and executive positions in European publicly-listed firms between 2002 and 2019. In Chapter 3, we examine how the exogenous shock of European mandatory board gender quotas in publicly-listed firms between 2008 and 2019 impacts board- and firm-level outcomes. Chapter 4 presents short-term shareholder reactions at different steps of the unionization process at publicly-listed firms and their subsidiaries in the United States between 2011 and 2019. Chapter 5 concludes the thesis with a discussion of results, their relevance to different stakeholders, implications for research and policy, and avenues for future research.

2 | Welcome on Board? Appointment Dynamics of Women as Directors

by Eline Schoonjans, Hanna Hottenrott, Achim Buchwald[9]

Increasing the participation of women in top-level corporate boards is high on the agenda of policymakers. Yet, we know little about director appointment dynamics and the drivers and impediments of women appointments. This study builds on organizational and group-level behavior theories and empirically investigates how ex-ante board structures and gender-specific board dynamics impact the representation of women on corporate boards. We study boards of listed firms in Europe between 2002 and 2019 and find a declining appointment probability for every additional woman, i.e. the share of women already on the board negatively predicts the likelihood of additional women appointments. Further, we find evidence of a *replacement* effect, i.e. the likelihood of a woman being appointed as director is significantly larger when a woman, compared to when a man, leaves the board. We do not find spillover effects from non-executive to executive boards. These results are robust to econometric model specifications that address potential endogeneity concerns using matching and instrumental variables. Our results confirm that board director appointments are gender specific and suggest that demand-side factors such as explicit and implicit norms drive women appointments up to a certain threshold.

[9] This essay is published in the Journal of Business Ethics (Schoonjans et al., 2023). My contributions are as follows: advanced development of the research idea and design, literature review, formulation of the statistical model, data curation, primary empirical analysis, creation of the visualizations, first draft writing, and joint editing.

2.1 Introduction

Boards of directors play a central role in the corporate governance of listed firms. Board structures and their determinants therefore receive considerable attention in both public debate and academic research.[10] One of the most debated trends in the development of corporate boards is the representation of women (Baker et al., 2020). In light of women earning more college degrees than men in many OECD countries for nearly 40 years (OECD, 2020), it is striking that their presence in boardrooms and c-level positions does not reflect this evolution. In 2020, women held only 6.4% of Fortune 500 chairperson roles, and only around one-fourth of all board members in US firms are women (Deloitte, 2021). The picture is similar in Europe. Recent publications report low, although increasing, levels of women in executive and non-executive board roles in the largest listed firms in the European Union. In 2020, 31% of the non-executive, and 18% of the executive directors were women. However, only 8% held the role of board chair or CEO (European Institute of Gender Equality, 2021). These observations raise the question of how board director appointment dynamics contribute to these outcomes.

Besides education, work experience, and qualification in certain areas of expertise,[11] other *supply-side* factors such as differences in career interruptions (Bertrand et al., 2010) and preferences for competition (Niederle and Vesterlund, 2007, Maggian et al., 2020) have been discussed as drivers of the underrepresentation of women on corporate boards. At the same time, institutional barriers and *demand-side* factors - including unconscious and conscious discriminatory and stereotypical biases - contribute to a “glass ceiling” blocking women’s upward mobility (Bjerk, 2008, Bertrand et al., 2019, Field et al., 2020). Women often need stronger leadership competence signals (Finseraas et al., 2016) and have less elite networks (Zimmerman, 2019, Michelman et al., 2022).

In this paper, we focus on *demand-side* drivers and impediments of gender diversity in the boardroom. We derive hypotheses from organizational and group-level theories and empirically investigate how ex-ante voluntary and mandatory gender composition of the board and the

[10] See Hermalin and Weisbach (1988) for a seminal study of board composition and Deutsch (2005) for a meta study.

[11] A prominent gender gap still exists throughout the entire career path in the STEM fields. Data from a subset of OECD countries has indicated that not only are young women less likely to graduate in engineering and computer science, moreover among graduates with science degrees, 71% of men but only 43% of women work as professionals in physics, mathematics, and engineering (Flabbi and Tejada, 2012). In other fields, women are well-represented at early career stages and in business schools, however very few climb the ladder to the top (Maggian et al., 2020).

gender of any departing board member influence appointment decisions of executive and non-executive women directors. Explicit and implicit norms can increase the attention on gender and lead to *t(w)okenism* (Kanter, 1977b, 1987, Chang et al., 2019) and an early *saturation* of board gender diversity. On the other hand, according to homophily theory (Pfeffer and Salancik, 1978), groups tend to pick new members in line with their own profile. With an increasing representation of women (*exposure*), especially after reaching a certain threshold (*critical mass*), the degree of the minority's influence on group decisions and outcomes will grow (Konrad et al., 2008, Broome et al., 2011) and favor women appointments. Finally, the status quo bias (Kahneman et al., 1991) suggests that if appointments are not to disrupt internal dynamics, they could follow a gender-matching heuristic (Tinsley et al., 2017), where women are only appointed to replace departing women. These theoretical considerations suggest that ex-ante board structures and dynamics affect appointments. As directors have different roles, these structures and dynamics could vary between executive and non-executives. Executives are the highest c-level managers while non-executives are responsible for advising, monitoring, appointing, and remunerating executive directors.

Our analyses contribute to research on corporate governance, particularly to work that draws attention to the determinants of board diversity. Previous research draws from institutional, resource-dependency, and group-level theories to explain drivers of board size, independence, multi-directorships, and diversity. While external environmental (Brammer et al., 2009, Grosvold and Brammer, 2011, Arena et al., 2015, Tyrowicz et al., 2020) and internal firm-specific factors, such as firm size, network linkages, strategic orientation, and performance, have been examined (Hillman et al., 2007, Withers et al., 2012, Gregorič et al., 2017, Markoczy et al., 2020, Barrios et al., 2022), the evidence on ex-ante board composition and dynamics driving women appointments is limited.

This study extends single-country studies by Farrell and Hersch (2005) on firms in the 1990s in the United States, Gregory-Smith et al. (2014) during the late 1990s and the 2000s in the UK, and Smith and Parrotta (2018) during the 2000s in Denmark. They find that the likelihood of adding a woman to the board in a given year negatively depends on the number of women already on the board. Further, they show that the probability of appointing a woman is higher when a woman director departs the board. With our European cross-country focus, we observe heterogeneous institutional contexts, different types of board structures, and more explicit attention to the representation of women via quotas.

Finally, we add to Matsa and Miller (2011) and Bozhinov et al. (2021) and their analyses of diversity spillover effects in the US and Germany by explicitly differentiating between non-executive and executive roles of board members and their appointment dynamics. As non-executive directors are responsible for appointing executive directors, the dynamics we expect inside the board could also spill over from non-executive to executive directors.

Our analyses build on data comprising executive and non-executive director appointments in 3,353 listed European firms between 2002 and 2019. We first provide descriptive evidence on board composition for mandatory quota and non-quota implementing countries. Next, we illustrate director appointment dynamics over time, where we observe important differences between non-executive and executive roles. Whereas women have been increasingly appointed to non-executive roles as of 2010, the share of women in executive roles has been rather constant at low levels over time.

We account for country, firm, and board characteristics and find that women are more likely to be appointed to non-executive than executive roles. Second, we find that the appointment likelihood for women declines the more women are already on the board. Thus, we find evidence of early board diversity *saturation* effects. Third, we show that the likelihood of a woman being appointed is significantly larger when a woman leaves, compared to when a man leaves the board. Combined, these findings could reflect *t(w)okenism*, where efforts to increase the representation of women on the board are made to reach or maintain a specific threshold below gender balance. Yet these efforts do not allow an equal opportunity of appointment to all director positions, especially to important executive positions (Chang et al., 2019, Gregory-Smith et al., 2014). Finally, we do not find evidence for spillover effects regarding the impact of gender diversity among non-executive directors on executive women appointments.

These results are robust to addressing potential endogeneity issues of the initial board composition using econometric matching techniques (Imbens, 2004) and a heteroscedasticity-based instrumental variable approach (Lewbel, 2012). The findings are also robust to dynamic model specifications, alternative measures of women director participation and appointments, and different control variables. In additional analyses, we examine potential differences between firms in countries with and without mandatory quotas, countries with different levels of female labor force participation, and firms operating in men- versus women-dominated industries. We find stronger evidence for gender-specific appointment dynamics before reaching gender balance in environments with increased external demand for and decreased supply of women director candidates.

Our findings have important implications for the debate on increasing board diversity and the roles women take on corporate boards. While the data provide evidence that the share of women in European boards has been increasing over time, they also show that new appointments are mostly to non-executive roles and that demand for diversity quickly saturates with higher existing diversity. Moreover, the appointment dynamics show that public pressure and mandated quotas trigger gender-specific appointments without reaching gender balance. We do not find robust evidence in favor of *exposure* or *critical-mass* effects. While quotas may be an appropriate instrument to increase diversity, two important aspects need to be considered. First, supply might be a constraining factor if the institutional environment disadvantages women, for example by limiting the extent to which women can combine family and job responsibilities or discrimination at earlier career stages. Second, board quotas do not lead to executive position spillovers if these positions are not specifically targeted. Social policy reforms and training that address career interruptions and unconscious biases may be more effective than mandatory quotas in increasing the representation of women in corporate top roles.

2.2 The Role of Gender in Board Appointment Dynamics

Growing empirical literature provides evidence that the composition and structure of boards of directors are relevant for the governance and performance of firms. Studies have focused on explaining the influence of women directors on corporate behavior and outcomes (e.g. Torchia et al., 2011, Adams and Funk, 2012, Ahern and Dittmar, 2012, Green and Homroy, 2018, Wahid, 2019, Carbonero et al., 2021). It is often argued that the appointment of women directors enhances human and social capital in the boardroom because a wider and more diverse talent pool regarding knowledge and experience can be exploited (Adams and Ferreira, 2009, Terjesen et al., 2009, Kim and Starks, 2016). However, studies also hint at challenges related to diversity in the boardroom. Relations-oriented diversity in terms of age, gender, and ethnicity can result in conflict, subgroup formation, or an inter-group bias (Williams and O'Reilly, 1998, Hewstone et al., 2002, Talke et al., 2010) and hence negatively affect firm performance.

Yet, we still know little about the drivers and impediments of attaining diversity in the boardroom. Following the *supply* logic, directors can and will be appointed from a pool of qualified candidates, regardless of their gender. Even if gender disparity could be explained by factors leading to a smaller pool of qualified women compared to men, the process of director appointment would not be gender-specific. In this case, the gender of an appointed director should be independent from the initial board composition or the gender of a departing board member. However, corporate governance research shows that the supply of suitable candidates can not fully explain the dynamics of the observed appointment bias (Adams and Kirchmaier, 2013). During the last decades, more women entered the lower and middle management levels and thereby increased the pool of qualified candidates for the board. This is in line with findings by Singh et al. (2008) who show that newly appointed women directors in the UK, although slightly younger than their male counterparts, have at least equal qualifications.

Recent studies, therefore, focus on *demand-based* factors of appointments. Demand for women directors can either be advanced or inhibited by external environmental and internal firm-specific factors. Institutional and cultural norms can foster unconscious or conscious biases forming a "glass ceiling" as a barrier to women's career advancement. Different types of discrimination, statistical, taste-based, and implicit, can hinder women's appointment to leadership positions (Bjerk, 2008, Gabaldon et al., 2016). There exists empirical evidence speaking to this argument. Selection procedures for men and women seem to differ in the sense that women need stronger signals and more often additional skills in terms of education, reputation, competence, and board and career experience than men to be appointed or promoted (Spence, 1973, Finseraas et al., 2016, Guo et al., 2020). Further, research highlights that board directors are traditionally recruited from a limited pool of socially-connected candidates. As a result, dense networks of multiple directorships can be observed (Adams and Ferreira, 2009, Fracassi and Tate, 2012). These traditionally male-dominated networks may hinder women to enter top-management positions (McDonald and Westphal, 2013, Zimmerman, 2019, Michelman et al., 2022).

Public opinion, regulatory and reputational pressure, as well as shareholder activism can create positive external demand for diversity in board composition (Brammer et al., 2009, Green and Homroy, 2018, Tyrowicz et al., 2020, Gormley et al., 2021). Especially larger firms that are more in the public eye are often more reactive to diversity demand (Agrawal and Knoeber, 2001, Carter et al., 2003, Hillman et al., 2007). Moreover, social norms for diversity can originate inside organizations and professional groups (Brammer et al., 2007, Mateos de Cabo et al., 2012, Mawdsley et al., 2022), but are typically influenced by external factors such as implicit industry standards or explicit quotas (Arena et al., 2015, Chang et al., 2019). The pressure for gender diversity from different stakeholders through explicit and implicit norms can make gender more salient in appointment processes (Knippen et al., 2019). Since gender is only one dimension of diversity, demand for additional women may evaporate once women have some representation. Farrell and Hersch (2005), Gregory-Smith et al. (2014), and Smith and Parrotta (2018) empirically show that in the 1990 and 2000s, when demand for women leaders was still relatively low, women were more likely to be appointed to a board with lower ex-ante representation. More recently, Bonet et al. (2020) find that in some leadership settings, women have an advantage of being appointed as long as there is no or only one other executive woman. This evidence suggests that external pressure creates demand for diversity that is saturated before reaching gender balance.

Board appointments consistent with these demand-side arguments may result in the addition of a few women only when the ex-ante board representation of women is low. This gender-specific appointment pattern might be stronger when public attention to gender issues and external pressure to appoint women according to a social norm is higher. Based on these considerations, we hypothesize the following.

Hypothesis 1a (Saturation): *The probability of appointing a woman as director decreases with higher ex-ante female representation.*

While outside pressure, combined with discriminatory biases, suggests an early *saturation* effect of the presence of women board members on new appointments, the *exposure* argument suggests that the appointment of an additional woman is more likely the larger the representation of women currently on the board. Exposure to women directors may lead to men updating their beliefs about the suitability of women leaders and act as signaling to potential women candidates (Carrell et al., 2015, Finseraas et al., 2016, Porter and Serra, 2020). Gangadharan et al. (2016) argue that women who attained leadership positions through quotas face male rejection which is only mitigated by higher exposure to women leaders. More generally, Guiso and Rustichini (2018) find that the participation of women in management is higher in countries with more pronounced emancipation of women.

Beyond pure *exposure*, *critical-mass* theory predicts that when a certain threshold is reached, the degree of the minority's influence grows (Konrad et al., 2008). The concept of critical mass hence implies that relative representation matters for the dynamics of heterogeneous groups (Kanter, 1977b, 1987). Once a certain minority reaches a critical mass, members can form coalitions and affect group decisions and outcomes. Previous research found some support for the *critical-mass* theory on different types of board- and firm-level outcomes (Konrad et al., 2008, Torchia et al., 2011, Joecks et al., 2013). Yet we know little about its effect on the dynamics of board director appointments. Research suggests that groups show a tendency to select new group members who resemble the existing group, labeling this tendency "homophily" (Pfeffer and Salancik, 1978) or inter-group bias (Hewstone et al., 2002). These patterns create barriers for out-group members and appear to also occur on corporate boards (Westphal and Stern, 2007, McDonald and Westphal, 2013, Zhu and Westphal, 2014, Gabaldon et al., 2016). If women reach a critical mass of board representation, they could influence appointment decisions towards candidates that resemble them, e.g. with respect to gender.

If the above arguments hold, we expect that ex-ante gender diversity should have a positive impact on future diversity and that the growing influence of women when attaining a critical mass additionally favors the appointment of women directors.

Hypothesis 1b (Exposure): *The probability of appointing a woman as director increases with higher ex-ante female representation.*

In principle, this suggests that once women achieve higher shares on corporate boards, inter-group biases may also result in an over-representation, i.e. holding more than 50% of board positions. However, it is unclear whether such dynamics would materialize given that once such gender parity is achieved, other norms and mechanisms may unfold. hence these arguments apply to settings with zero to full gender diversity, where the latter relates to a gender-balanced board with 40-60% women.

The variation of diversity inside the board room affects internal group dynamics and may have consequences beyond *saturation* and *exposure*. Often, individuals are more afraid of losses from change, than they appreciate respective gains and will prefer the status quo (Kahneman et al., 1991). This 'status quo' bias can also apply to the boardroom setting (Gregory-Smith et al., 2014). Tinsley et al. (2017) find that exits of women directors increase the probability of women re-appointments. They label this phenomenon 'gender-matching heuristic'. Such a heuristic implies that boards may aim to maintain a certain share of women, consistent with the respective norm without disrupting existing internal dynamics.

In line with this idea, we argue that the gender of the departing director plays a role in the new appointment of women.

Hypothesis 2 (Replacement): *The probability of appointing a woman as director is higher in case of the departure of a woman compared to no departure or the departure of a man.*

We expect the *replacement* effect to be higher with increased demand through external pressure and with a lower supply of women director candidates through their participation in the workforce.

Even though most diversity reforms address non-executive and executive board roles combined, women tend to be appointed to non-executive positions, which are typically less influential (European Women On Boards, 2021) and receive lower financial compensations (Rebérioux and Roudaut, 2019). This suggests that explicit norms such as legally-mandated gender quotas may have unintended consequences, where women are less likely to be appointed into major board roles (Knippen et al., 2019, Hwang et al., 2018). For example, Foss et al. (2022) show that while generally, a higher share of women in management positions is related to greater innovativeness of firms, this link is weaker in the presence of legally-mandated gender quotas. Such patterns suggest that women are primarily appointed as "tokens" to signal compliance with implicit or explicit norms, e.g. when mandatory quotas are in effect or a firm is particularly distant from diversity norms. Tokens act as representatives of their category, but have limited influence on corporate decisions (Kanter, 1977b). Recently, the *twokenism* norm has replaced *tokenism* in many firms and industries, where having exactly two women on the board is very common in US firms (Chang et al., 2019). Gregory-Smith et al. (2014) find that in the UK, non-executive appointments are gender specific while executive appointments are not. These observations stress the importance of distinguishing between appointment dynamics for executive and non-executive roles.

We hypothesize that if appointments occur to conform to norms without influencing major decision-making processes, women could be predominantly appointed to non-executive positions. Again, this pattern is likely stronger in settings where the norm is more explicit.

Hypothesis 3 (Role-Specificity): *Gender-specific appointment dynamics are more prevalent for non-executive than executive directors.*

Finally, we take into account that non-executive directors are typically responsible for appointing executive directors (Matsa and Miller, 2011, Bozhinov et al., 2021). The empirical evidence on whether "women help women" is mixed. While Derks et al. (2016) argue that because of the *queen-bee* effect women tend not to support or even undermine women subordinates, others suggest that female leaders help other women advance in the firm, leading to gender-diverse spillovers on lower hierarchical levels (Cohen et al., 1998, Kleinbaum et al., 2013, Kunze and Miller, 2017). These women and their direct environment are less likely to view other women through the lens of traditional gender stereotypes (Stainback et al., 2011, Clark et al., 2021) and they enforce female-friendly policies and organizational cultures (Gagliarducci and Paserman, 2015, Tate and Yang, 2015). Matsa and Miller (2011) and Bozhinov et al. (2021) find compelling evidence for spillover effects from the non-executive to the executive board in line with the latter argument.

Following this reasoning, we hypothesize that a growing influence of non-executive women in appointment decisions through higher representation, especially after reaching a critical mass, will have a positive impact on executive women's appointments.

Hypothesis 4 (Spillover): *The probability of appointing a woman as executive director increases with higher ex-ante representation of non-executive women directors.*

2.3 Institutional Framework, Data and Method

2.3.1 Institutional Framework

Existing studies on gender diversity frequently rely on national data. Due to an increasingly international market for top managers, we base our empirical investigation on a sample of Western European firms. This approach allows us to exploit cross-firm and cross-country variation and consider institutional and legal differences between countries when examining appointment dynamics of executive and non-executive roles.

In Europe, an essential distinction can be made between monistic one-tiered (Anglo-Saxon) structures and dualistic two-tiered boards traditionally predominant in continental Europe. Some European countries allow both 'one-tier' and 'two-tier' board structures. The majority of

French and Spanish firms, for instance, have voluntarily implemented one-tier board structures. Countries such as Austria or Germany have mandatory two-tier board structures (OECD, 2012, Gelter and Siems, 2021). While two-tiered boards prescribe a strict separation of executive and non-executive directors, one-tiered systems combine executive and non-executive directors on a unitary board, sometimes including a dual CEO-Chairman. Recently, several European countries have implemented voluntary and mandatory quotas for women directors. Depending on the board structure, these apply either to all board directors or only non-executive directors. We account for the countries' varying institutional and legal settings and distinguish between director roles.

Empirical literature argues that board roles and their responsibilities are similar in both two-tiered and one-tiered boards and that structures and processes in Europe converge due to governance codes (Fauver and Fuerst, 2006, Davies and Hopt, 2013). The main tasks of members of dualistic executive boards and executive directors on one-tiered boards include day-to-day operations of a company. Members of dualistic supervisory boards and non-executive directors on one-tiered boards are responsible for advising, monitoring, and decisions about the remuneration and appointment of executive directors. While executive directors perform their tasks as a full-time job, non-executive directors often have multiple mandates, multi-directorships. The type and intensity of cooperation between executive and non-executive directors in the boardroom depend on the respective structure of the board. Due to the strict separation of management and control, non-executive directors on two-tiered boards are typically more independent but information asymmetries between executive and non-executive directors are more pronounced compared to one-tiered boards (Adams and Ferreira, 2007).

Generally, in dualistic systems, the shareholder representatives elect the members of the supervisory board at the annual general meeting, while the latter appoints the members of the executive board. Nomination committees are supposed to ensure the participation of supervisory boards in the appointment and removal process of executive directors by identifying and recommending potential candidates (European Commission, 2005). In monistic systems, the shareholders appoint all directors at the annual general meeting. The CEO of a company takes an outstanding position in the boardroom (particularly in the case of CEO-chairman duality) and may influence executive appointments (Shivdasani and Yermack, 1999).

As a consequence, in a multi-country setting, it is important to classify individual board members according to the role they take. We therefore carefully categorize directors by differentiating between non-executive and executives according to their role and position descriptions as listed in the ORBIS database. We draw this distinction by applying a role-based categorization which takes into account that board structures differ between European countries. Members of the two-tier supervisory board and one-tier directors with non-executive roles are considered non-executive directors. In our analyses, we call them supervisory directors. We categorize members of the two-tier executive board and one-tier directors with executive roles as executive directors.

2.3.2 Data and Sample

Our empirical analysis is based on combined data from several sources. We obtain detailed information on board members and firm ownership from the ORBIS database provided by Bureau van Dijk. Financial information stems from Worldscope provided by Refinitiv. Our main sample includes 27,486 firm-year observations from 3,353 listed firms observed during the period 2002 to 2019 in 17 European countries. In line with previous studies, we exclude utilities and financial firms with two-digit SIC codes 49 and 60-69 (Adams et al., 2018). We follow Kim and Starks (2016) and restrict our attention to firm-year observations where the director appointment and departure dates are available for a particular firm.[12] In order to correctly capture board composition, we include only firm-year observations where data for at least two directors are available.[13]

Figure 2.1 shows the development of women director representation in the different countries included in our main sample. The figure illustrates that, on average, the share of women on the board of directors has been increasing in the past two decades both in countries with (Norway, Italy, France, Belgium, Germany, Austria, and Portugal) and without mandatory quotas (Switzerland, Denmark, Spain, Finland, UK, Greece, Ireland, Luxembourg, Netherlands, Sweden).

[12] Note that we check the sensitivity of our findings to relaxing this rule and find that our main results are robust to a left censored data sample, i.e. where directors with missing appointment dates are included in the sample.

[13] Our results and the inferences we draw from them are robust to different sample specifications, such as including only observations with three directors or more, as required by law.

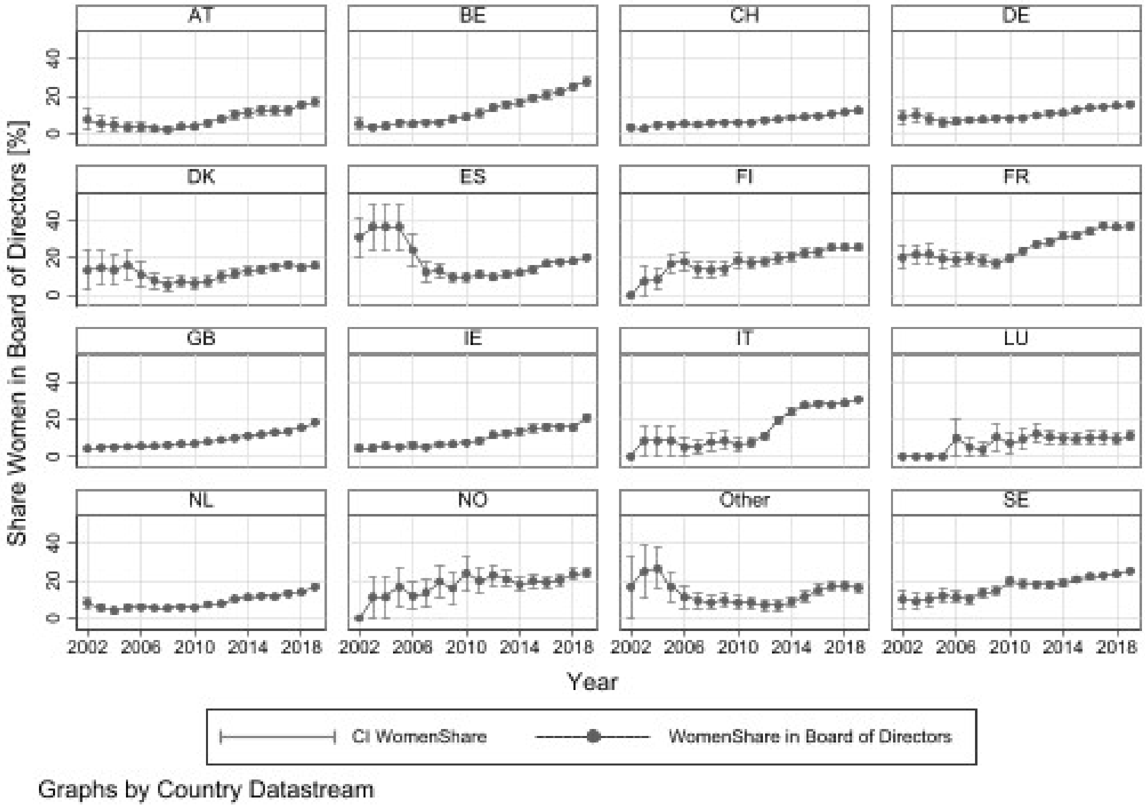


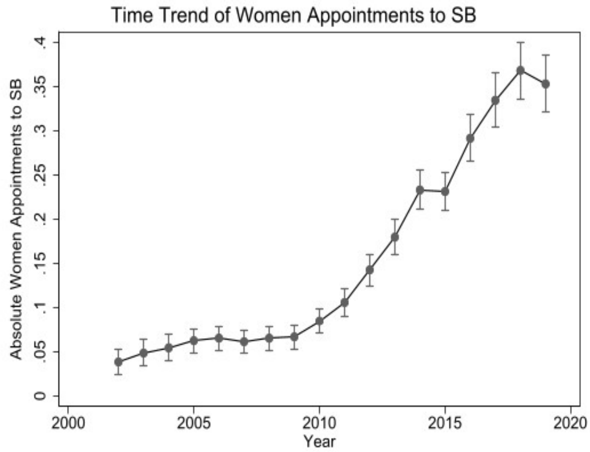
FIGURE 2.1: Share of Women on Boards (Country Averages)

This figure reports the average time trend of the share women in each country's board of directors. "Other" countries have low number of observations and include Portugal and Greece. Between 2002 and 2019, seven countries implemented mandatory quotas for a minimal share of the underrepresented gender on corporate boards. These include Norway, Italy, France, Belgium, Germany, Austria, and Portugal.

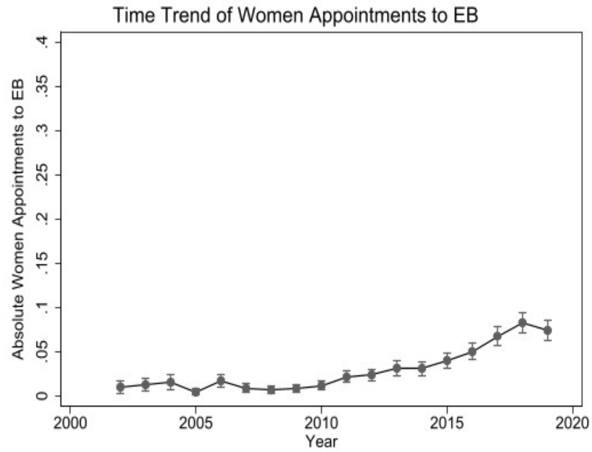
2.3.3 Definition of Variables

Our main variable of interest is an indicator for the appointment of at least one non-executive or executive woman in a given year. In additional analyses, we replace this main dependent flow variable with the number of appointed women and the difference (delta) between the share of women in a given year and the year before.

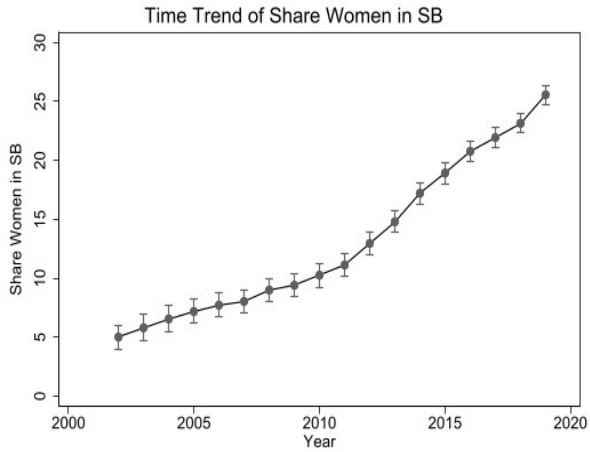
Figure 2.2, Chart (a) shows that on average, 0.05 female directors were appointed to the supervisory board (SB) in 2002. This number increased to 0.35 in the year 2019. We also observe a slightly increasing number of female directors appointed to the executive board (EB) in Chart (b), yet on a significantly lower level. Similar findings appear for the total share of female directors: Chart (c) shows that the fraction of female supervisory directors increased from five percent in the year 2002 to more than 25 percent in 2019. The fraction of female executive directors increased from five to eleven percent in the same period (Chart (d)). According to our sample data, the difference between the share of women on the supervisory board in two consecutive years also reflects an upward trend from 0.5 percentage points in 2003 to 1.5 percentage points in 2019 (e). In contrast, our sample shows a largely constant difference of share women between two consecutive years of 0.4 percentage points for the share of female executive directors in Chart (f).



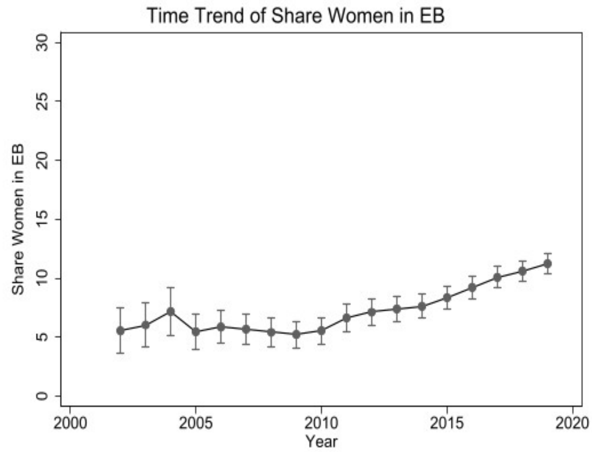
(A) Women Appointments to SB



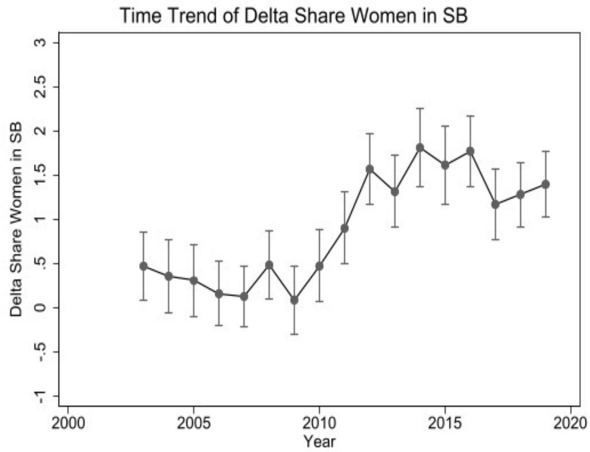
(B) Women Appointments to EB



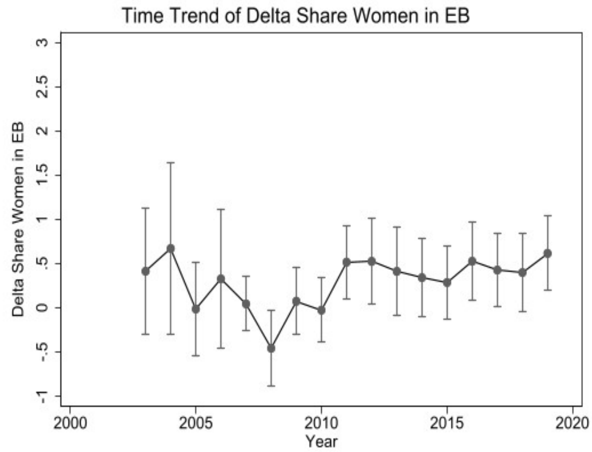
(C) Share Women in SB



(D) Share Women in EB



(E) Delta Share Women in SB



(F) Delta Share Women in EB

Figure 2.2: Appointments and Share of Women Directors (Over Time)

This figure reports the average time trend of board appointments and share women. SB refers to the supervisory board and EB refers to the executive board.

We follow Farrell and Hersch (2005) and use the lagged share of women directors as the main predictor variable. Furthermore, we generate two indicator variables for men and women director exits in the given year. Exits include all reasons for director departure. We calculate lagged board size and test the influence of and robustness to the inclusion of other one-year lagged board-level variables in additional specifications, such as the share of independent, foreign, and multi-directors. Multi-directors represent those directors that hold at least one additional board position in an external company. Further, we account for average director age and tenure, as well as a binary variable indicating whether the CEO or chairperson is a woman.

Table 2.1 presents descriptive statistics of our main variables reflecting the dynamics of executive and non-executive director appointments. All non-executive directors are included in the *supervisory board* observations of our sample, all executive directors are included in the *executive board* observations. As we have less data on executive directors for the key variables, the executive board sample only counts 20,672 instead of 27,486 firm-year observations. Appendix Table A.1 provides the variable definitions and their respective data origins.

Table 2.1: Descriptive Statistics

This table reports descriptive statistics of the dependent and independent variables of interest for the supervisory and executive board analyses. The statistics of the control variables are categorized according to their aggregation levels, board, firm, and country. For each variable, we report the number of non-missing observations, the mean, the standard deviation, the coefficient of variation, the minimum, median, and maximum value.

Variable name	Observations	Mean	S.D.	C.V.	Min.	Median	Max.
Supervisory Board Variables							
Women Appointment to SB	27486	0.14	0.34	2.50	0	0	1
# Women Appointment to SB	27486	0.19	0.58	3.05	0	0	11
# Women Exit from SB	27486	0.05	0.27	5.63	0	0	8
# Men Exit from SB	27486	0.39	0.88	2.27	0	0	16
Share Women in SB [%]	27486	14.42	20.48	1.42	0.00	0.00	100
Executive Board Variables							
Women Appointment to EB	20672	0.04	0.19	4.93	0	0	1
# Women Appointment to EB	20672	0.04	0.23	5.17	0	0	3
# Women Exit from EB	20672	0.01	0.10	10.64	0	0	4
# Men Exit from EB	20672	0.11	0.39	3.47	0	0	12
Share Women in EB [%]	20672	7.73	21.19	2.74	0.00	0.00	100
Board characteristics:							
Director Tenure	27486	4.59	3.25	0.71	0	4	38
Director Age	27018	54.52	5.59	0.10	20	54.75	88
Share Independent Directors [%]	27486	80.81	26.98	0.33	0.00	100	100
Share Foreign Directors [%]	27486	11.72	20.72	1.77	0.00	0.00	100
Share Multidirectors [%]	27486	36.24	24.16	0.67	0.00	33.33	100
Chairwoman	10693	0.05	0.22	4.28	0	0	1
CEO is a Woman	12245	0.04	0.21	4.66	0	0	1
Board Size	27486	6.36	3.66	0.58	2	6	56
Firm characteristics:							
Dependence Indicator	22244	0.48	0.50	1.04	0	0	1
Employees	25773	11867.68	41989.19	3.54	0	1050	664496
Tobin's Q	27486	2.63	47.06	17.88	-0.03	1.36	5416.50
ROA	27448	2.43	76.01	31.32	-11150.00	5.75	591.67
Firm Age	27486	16.83	12.93	0.77	0	14	54
log(Total Assets)	27486	5.46	2.36	0.43	-6.21	5.30	13.01
Country characteristics:							
GDP per Capita	27486	42737.75	9495.99	0.22	22615.96	41269.35	116622.24
Employment Rate [%]	27486	70.50	5.68	0.08	48.80	71.60	80.10
Women Labor Force Rate [%]	27486	46.40	1.28	0.03	39.15	46.52	49.78

In all specifications, we include firm age and the logarithm of total assets to control for maturity and firm size. The average age of the firms in our sample is 16.8 years with a maximum of 54 years. This low number is partly due to changes in legal structure resulting in updated firm identifiers. Our sample's median values for firm size measures amount to 201 million euros in total assets and 1,050 employees. Approximately one-third of our sample's firms are considered small and medium enterprises (SMEs), which have fewer than 250 employees and fewer than 50 million euros in annual turnover or 43 million euros in total assets (European Union, 2003). Further, Tobin's Q captures the expected influence of market-based firm performance on the likelihood of new women appointments. Tobin's Q amounts to an average of 2.6% per year over the entire period 2002 to 2019. Additionally, a dummy variable based on ownership data provided by Bureau van Dijk controls for potential ownership concentration. In line with the literature, this block indicator takes the value of 1 if one or more shareholders with a fraction of at least 25% of the capital stock are identified (Czarnitzki and Kraft, 2009). GDP per capita, total employment rate, and women's participation in the labor force are included to control for country-specific time-variant labor market factors.

2.3.4 Empirical Methodology

We examine the specific factors that predict women director appointments according to our hypotheses in a multivariate regression framework. The probabilities (P) of appointing a woman as supervisory and executive director are estimated from linear probability models for firm $i =$ 1, ..., N at time period $t =$ 1, ..., T:

$$\begin{aligned} P(y_{(supervisory)_{it}}) = \alpha_{it} + \beta_1 Predictors_{(supervisory)_{it}} \\ + \mathbf{X}_{it}\delta + \lambda_t + c_i + \varepsilon_{it}, \end{aligned} \tag{2.1}$$

$$\begin{aligned} P(y_{(executive)_{it}}) = \alpha_{it} + \beta_1 Predictors_{(executive)_{it}} \\ + \beta_2 Predictors_{(supervisory)_{it}} + \mathbf{X}_{it}\delta + \lambda_t + c_i + \varepsilon_{it}. \end{aligned} \tag{2.2}$$

The set of *Predictors* includes the lagged share of female (non-)executive directors. To account for a possible nonlinear relationship between the previous year's proportion of female non-executive directors and the likelihood of a current female director appointment, we add the squared term of this share. Following empirical evidence for a *critical-mass* effect and social norms (*twokenism*) for a specific number of a minority group's representation, we replace the lagged share of female (non-)executive directors with the lagged number of (non-)executive directors in additional specifications. For executive appointments, we follow Matsa and Miller (2011) by taking into account both the lagged share of female non-executive and executive directors. We include dummy variables indicating female and male (non-)executive exits from the board. The exit and appointment variables are from the same year, as they are often decided at the shareholders' meeting in the first half of the fiscal year, based on the previous year's annual report. $\mathbf{X}_{it}$ is the vector of lagged board-, firm-, and country-specific controls. Furthermore, we include year fixed effects (λ_t) to capture aggregate time trends and fluctuations and firm-fixed effects to absorb the time-invariant unobserved heterogeneity c_i between firms. This heterogeneity could be differences in firm culture, strategic orientation, or location. We draw statistical inferences based on firm-clustered standard errors robust to heteroscedasticity and autocorrelation.

Due to the binary nature of our dependent variable, women appointment, the linear probability model can only approximate probabilities. However, the coefficients of interest can still give reasonable estimates of average partial effects (Wooldridge, 2010). We estimate the linear probability models with Ordinary Least Squares (OLS). For robustness, we report the results of logit and Poisson model estimations in the Appendix.

2.4 Empirical Analysis

The first set of results describes the dynamics and predictors of supervisory director appointments and we subsequently discuss the results for appointments as executive directors. We start with presenting correlations before we account for the potential endogeneity of key variables in the model.

2.4.1 Analysis of Supervisory Director Appointments

Table 2.2 reports the main results for the probability of appointing women to the supervisory board. The coefficients estimate average partial effects on the linear approximation of these probabilities. All specifications include firm- and country-specific time-variant control variables and year- and firm-fixed effects. Specification (1) shows that, on average, the probability of appointing a female supervisory director decreases by 0.7 percentage points if the previous year's share of female supervisory directors increases by 1 percentage point, ceteris paribus. This average marginal effect takes into account the first- and second-order term of the share of women variable. The squared term is positive and significant, but smaller. The demand for women directors is increasingly saturated up to a certain point.

TABLE 2.2: Estimation Results for Women Appointments as Supervisory Directors

This table reports the results of the impact of supervisory board composition in terms of director gender and supervisory board dynamics in terms of director exits on the probability of female supervisory director appointments in linear probability models. Specification (2) adds the Dependence Indicator, equal to one if ownership is concentrated. Specification (3) adds additional board-level variables as controls. Specification (4) replaces the share women in the board with an integer indicating the number of women directors. Note that we combine cases with four and more than four women in the same category. The linear models are estimated with OLS and fixed effects on the Year (Y) and Firm (F) level. Standard errors clustered at the firm level are in parentheses; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

	(1) Main Predictors	(2) Dependence Indicator	(3) Board-Level Predictors	(4) Number Women
Share Women in SB	-0.909***	-1.045***	-1.212***	
	(0.043)	(0.051)	(0.109)	
Number Women in SB				-0.059***
				(0.005)
Share Women in SB × Share Women in SB	0.601***	0.674***	0.675***	
	(0.057)	(0.067)	(0.174)	
Women Exit from SB	0.274***	0.277***	0.355***	0.253***
	(0.016)	(0.017)	(0.028)	(0.017)
Men Exit from SB	0.086***	0.093***	0.126***	0.095***
	(0.006)	(0.007)	(0.014)	(0.006)
Dependence Indicator		-0.010	-0.018	
		(0.008)	(0.014)	
Chairwoman			0.196**	
			(0.070)	
Director Tenure			-0.003	
			(0.004)	
Share Independent Directors			0.003	
			(0.052)	
Share Foreign Directors			-0.068	
			(0.063)	
Share Multidirectors			0.132***	
			(0.040)	
Director Age			-0.001	
			(0.002)	
Board Size	-0.006***	-0.007***	-0.018***	-0.004**
	(0.001)	(0.002)	(0.003)	(0.001)
Firm Age	0.017***	0.024***	0.025***	0.013***
	(0.002)	(0.002)	(0.005)	(0.002)
log(Total Assets)	0.009	0.009	0.011	0.006
	(0.005)	(0.006)	(0.012)	(0.004)
Tobin's Q	0.000***	0.000	0.003	0.000*
	(0.000)	(0.000)	(0.003)	(0.000)
GDP per Capita	-0.000	-0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Employment Rate	0.030	-0.146	-0.206	-0.054
	(0.204)	(0.246)	(0.383)	(0.185)
Women Labor Force Rate	1.746*	1.074	1.038	1.990**
	(0.709)	(1.045)	(1.922)	(0.650)
Constant	-0.876*	-0.534	-0.417	-0.948**
	(0.352)	(0.522)	(0.878)	(0.325)
Fixed Effects	Y F	Y F	Y F	Y F
N	27486	22244	9247	27486

We visualize the non-linear relationship between share women on the board and the appointment probability of appointing at least one woman in Figure 2.3. The figure shows margins of the appointment probability approximation of women at different thresholds of lagged share women, holding all other predictors constant at their mean. Margins decline steeply with increasing female representation up to 50% in line with the *saturation* hypothesis. Once, gender balance is reached, the relationship becomes flat at very low appointment probabilities. At this end of the share distribution, the number of observations is low which makes interpreting the range beyond gender balance difficult. Attaining a critical mass of 30% does not lead to a higher appointment likelihood for women. This finding is in line with our *saturation* hypothesis 1a and contradicts the *exposure* and *critical-mass* hypothesis 1b.

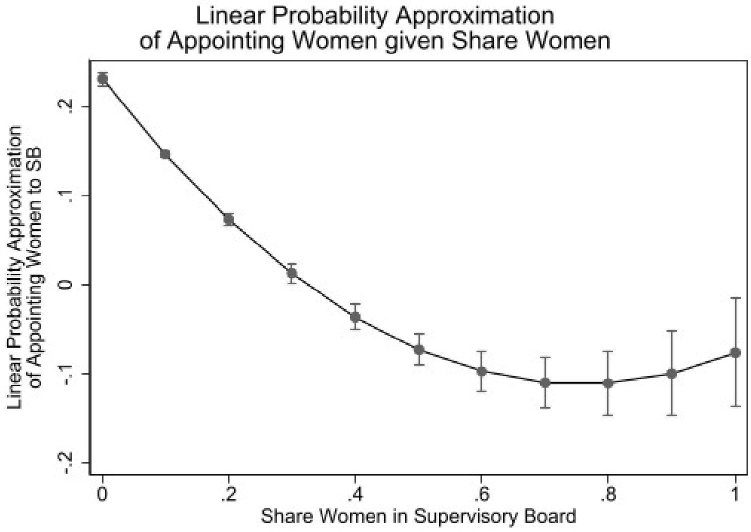


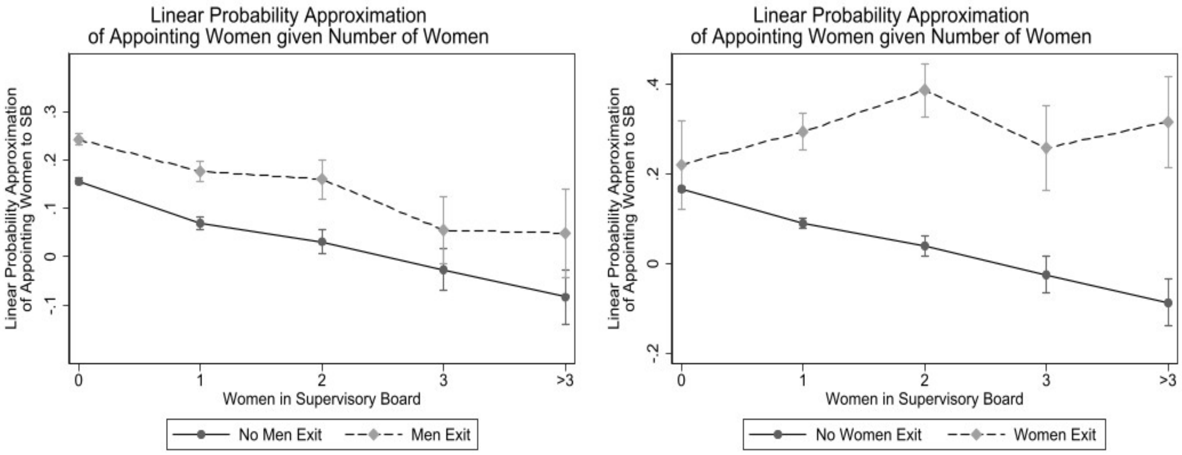
FIGURE 2.3: Margins of Supervisory Women Appointment Probabilities at Share Women Thresholds

Further, in Specification (1) we see that the appointment probability of women increases by 27 percentage points when a woman leaves the supervisory board in the same year, compared to only 9 percentage points when a man leaves the board. The probability increase of appointing at least one woman is three times higher when a woman compared to when a man leaves the board. A t-test confirms their statistically significant difference at a 1% level in line with our *replacement* hypothesis 2. This result suggests that firms follow a gender-matching heuristic, in line with the status-quo bias (Gregory-Smith et al., 2014, Tinsley et al., 2017). The declining probability of women appointments with higher initial shares, i.e. the *saturation* effect, combined with the higher *replacement* likelihood when a woman leaves, show that supervisory director appointments are gender specific. Firms take into account gender as a characteristic to appoint new directors and may pursue the (unstated) goal to not move backwards in their level of gender diversity as a response to outside pressure.

The size of the board also plays a role. Inside a firm, periods with larger boards are characterized by a lower probability of appointing a female supervisory director. Firm size measured by total assets and firm performance, measured by Tobin's Q, is not significantly related to the likelihood of new female supervisory directors when controlling for firm-fixed effects. Finally, a country's higher women labor force participation is associated with a higher appointment probability.

These insights are robust to including the dependence indicator in Specification (2), which does not significantly affect the appointment probability. The dependence indicator equals one if ownership is concentrated, i.e. if at least one shareholder holds 25% or a higher fraction of the shares. With our firm-fixed effects, we already control for time-invariant firm heterogeneity. Ownership concentration does change over time, but often not substantially. Specification (3) includes additional board characteristics. Due to missing values, the sample size is restricted to 9,247 observations. Even for this subsample, we find the same patterns. The previously found *saturation* effect is more pronounced in this subsample and when controlling for additional board-level characteristics. Having a woman as chair of the supervisory board increases the appointment probability by 20 percentage points, on average. This effect is in line with the prevalent empirical evidence of women leaders helping other women, leading to spillovers to lower hierarchical levels. Moreover, the share of directors that serve on other boards, multidirectors, is positively associated with women's appointment probability.

In Specification (4), we replace the share women with an integer indicating the number of women directors. Our results remain unchanged. With each additional woman already on the board, the probability of appointing a new female supervisory director decreases by 6 percentage points, on average. We visualize the effects of the number of women on the appointment probability in Figure 2.4. Compared to Specification (4), we include an interaction between the number of women in SB and our director exit indicators. In Subfigure 2.4b, we show that the *replacement* effect outweighs the average *saturation* effect if the board has two or fewer female supervisory directors. The likelihood of appointing a new female supervisory director increases with each additional woman on the board if one woman leaves and no more than two women are already on the board. The appointment probability does not increase if the number of women already on the board reaches a critical mass. These findings are in line with *twokenism*, where adding two women to the board conceptually signals compliance with current norms.



(A) Table 2.2 Specification (4) with Men Exit Interaction

(B) Table 2.2 Specification (4) with Women Exit Interaction

FIGURE 2.4: Margins of Supervisory Women Appointment Probabilities at Number of Women Thresholds

2.4.2 Heterogeneity in Supervisory Director Appointments

To better understand possible country and industry heterogeneity in our main findings, we perform several additional analyses. We investigate the three dimensions: industries' average board gender diversity, countries' level of female labor force participation, and countries' mandatory quota legislation. Firms operating in industries with relatively high shares of women directors are likely characterized by different appointment procedures than firms in industries with comparatively low board gender diversity. We define an industry to be diverse if it has an above-median share (i.e. more than 12% in our sample) of women in board positions. In a more diverse industry, there is likely both higher *supply* and higher *demand* for female directors. To disentangle *supply* and *demand* heterogeneity, we include two additional dimensions. We argue that high female labor force participation increases the *supply* of suitable women director candidates. Mandatory quotas reflect increased public attention to gender diversity, external pressure to achieve it, and salience of social diversity norms and increase the *demand* for women directors up to a certain threshold. Therefore, we expect *saturation* and *replacement* effects to be more pronounced in environments with increased external *demand* for and decreased *supply* of women candidates.

Table 2.3 presents the results. We find no substantial differences between "female- and male-dominated" industries in Specifications (1) and (2). The *saturation* effect is slightly higher in industries with higher share of women on the boards of directors. The *replacement* effect, i.e. the difference in appointment probability between when a woman, compared to when a man leaves the board, is similar. In countries with low female labor force participation, we see stronger *saturation* and *replacement* effects. Thus, our previous results are stronger in settings with lower *supply* of women candidates.

Table 2.3: Subsample Analyses for Women Appointments as Supervisory Directors

This table reports cross-sectional results of the main specification (Specification (1) in Table 2.2). Specifications (1) and (2) compare industries with high and low share of women directors. Specifications (3) to (5) compare countries with high, medium, and low women labor force participations (LFP). Specifications (6) and (7) compare observations in years and countries after mandatory board gender quota implementation to those without mandatory quotas. Fixed effects are on the Year (Y) and Firm (F) level. Standard errors clustered at the firm level are in parentheses; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	High SW Industry	Low SW Industry	High LFP	Middle LFP	Low LFP	Quota Treated	No Quotas
Share Women in SB	-0.982***	-0.835***	-1.281***	-1.207***	-1.506***	-1.630***	-0.925***
	(0.060)	(0.062)	(0.098)	(0.067)	(0.137)	(0.181)	(0.046)
Share Women in SB × Share Women in SB	0.663***	0.543***	0.820***	0.801***	1.337***	1.126***	0.604***
	(0.082)	(0.076)	(0.111)	(0.100)	(0.185)	(0.202)	(0.063)
Women Exit from SB	0.270***	0.281***	0.196***	0.206***	0.481***	0.440***	0.197***
	(0.021)	(0.026)	(0.056)	(0.022)	(0.028)	(0.043)	(0.018)
Men Exit from SB	0.090***	0.082***	0.106***	0.070***	0.116***	0.260***	0.063***
	(0.009)	(0.008)	(0.029)	(0.007)	(0.013)	(0.041)	(0.006)
Board Size	-0.006***	-0.005*	-0.013***	-0.015***	-0.023***	-0.033***	-0.005**
	(0.002)	(0.002)	(0.004)	(0.003)	(0.003)	(0.004)	(0.002)
Firm Age	0.018***	0.016***	0.030***	0.025**	0.062***	0.039*	0.016***
	(0.003)	(0.003)	(0.008)	(0.008)	(0.008)	(0.018)	(0.002)
log(Total Assets)	0.007	0.013*	0.021	0.003	0.014	-0.065*	0.013**
	(0.008)	(0.006)	(0.016)	(0.008)	(0.013)	(0.029)	(0.005)
Tobin's Q	0.000	0.000***	-0.001	0.000	0.000	-0.011	0.000***
	(0.000)	(0.000)	(0.002)	(0.000)	(0.000)	(0.010)	(0.000)
GDP per Capita	0.000	-0.000	-0.000	-0.000	-0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Employment Rate	0.063	0.097	2.129**	1.271	0.033	2.943	0.152
	(0.294)	(0.261)	(0.677)	(0.663)	(0.388)	(3.613)	(0.199)
Women Labor Force Rate	2.251*	0.303	4.958	-10.097*	-5.022**	30.783***	1.517*
	(0.941)	(1.037)	(3.882)	(4.344)	(1.783)	(8.671)	(0.728)
Constant	-1.139*	-0.263	-3.883*	3.851*	1.839*	-15.764***	-0.934**
	(0.486)	(0.496)	(1.970)	(1.906)	(0.865)	(3.762)	(0.360)
Fixed Effects	Y F	Y F	Y F	Y F	Y F	Y F	Y F
N	15335	12151	7433	13587	6466	3664	23822

Consistent with our expectations, we find that in quota country observations with increased *demand* for women directors, the *saturation* effect is stronger (Specification (6)). It should be noted, however, that the appointment probability for women is overall higher in the presence of binding quotas. The re-appointment effect is also stronger in quota observations for both women and men leaving the supervisory board. The difference between the two coefficients remains similar in both subsamples. These findings show that gender-specific appointments are more pronounced in environments with increased external *demand* for and decreased *supply* of female candidates.

2.4.3 Robustness Tests and Sensitivity Analyses of Supervisory Director Appointments

The inferences we draw from our main analysis rely on the assumption of exogenous predictors. Yet, our variables of interest, in particular, the dummy for departing directors and the share of women on the board could be considered endogenous. In order to address this concern, we tackle potential endogeneity issues arising from confounding observable and unobservables factors influencing the predictors of interest as well as the appointment probability.

First, we rerun our main analysis on subsamples including at least one director appointment in each firm-year observation in Specification (1) of Table 2.4 and at least one director departure (Specification (2)). A director appointment is not necessarily a reaction to a departure and therefore, the two subsamples and their dynamics might differ. In these specifications, we aim to reduce unobserved time-variant heterogeneity between firms resulting in particular appointment patterns. The share women coefficients are larger than in our main specification because the average appointment probabilities are higher in both subsamples. The average women appointment probability in our main sample is 14%, while the appointment and exit subsample probabilities are 34% and 21%, respectively. In Specifications (1) and (2), we find that a 1 percentage point increase in the share women on the supervisory board results in respective 1.6 and 1.2 percentage points decreases of the appointment probability for women. The *saturation* and *replacement* effects are prominent in both subsamples. There is no evidence for an *exposure* or *critical-mass* effect.

In Specification (3) of Table 2.4, we estimate a dynamic model and include lagged values of the dependent variable as auto-regressive terms to control for persistence in the dependent variable (Matsa and Miller, 2011). These auto-regressive terms show that the appointment of a woman in previous years is associated to a lower appointment probability in the subsequent year. The

Table 2.4: Robustness Checks for Women Appointments as Supervisory Directors

This table reports robustness checks on our main Specification (1) from Table 2.2. Specification (1) includes firm-year observations with at least one supervisory director appointment. Specification (2) includes firm-year observations with at least one supervisory director exit. Specification (3) is a dynamic auto-regressive model of order two (AR(2)) which controls for the persistence of the dependent variable. Specification (4) compares nearest neighbor firms that had a female supervisory director before increased external demand in 2010 to firms that did not. Specification (5) uses heteroscedasticity-based exogenous instruments for our main endogenous variables of interest (Share Women in and Exits from SB). We include the previous additional firm (F) - and country (C) - level controls. Fixed effects are on the Year (Y) and Firm (F) or Year (Y), Country (C), and two-digit SIC-industry (S) level. Standard errors clustered at the firm level are in parentheses; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

	(1)	(2)	(3)	(4)	(5)
	Appointment	Exit	Dynamic	NNM:	IV:
	Subsample	Subsample	Specification	Early Women	Het.-Based
Share Women in SB	-1.597***	-1.214***	-0.692***	-1.341***	-0.256***
	(0.082)	(0.125)	(0.053)	(0.086)	(0.068)
Share Women in SB × Share Women in SB	0.810***	0.269	0.462***	1.176***	0.205**
	(0.117)	(0.264)	(0.063)	(0.178)	(0.070)
Women Exit from SB	0.207***	0.201***	0.255***	0.231***	0.302***
	(0.020)	(0.020)	(0.017)	(0.036)	(0.017)
Men Exit from SB	-0.017		0.089***	0.107***	0.090***
	(0.012)		(0.006)	(0.015)	(0.006)
L.Women Appointment to SB			-0.080***		
			(0.010)		
L.2.Women Appointment to SB			-0.066***		
			(0.009)		
Weak Instrument Test					56.90
Fixed Effects	Y F	Y F	Y F	Y F	Y C I
Additional Controls	C F	C F	C F	C F	C F
N	11164	6875	25300	8488	27486

lagged appointments pick up some of the previously captured *saturation* dynamics, but our results remain robust to those from the main specification in Table 2.2. The appointment probability is almost three times bigger when a woman leaves compared to when a male board member leaves.

Next, we follow Nekhili et al. (2020) and employ a matching technique to account for observable differences between firms with varying initial representations of women on their boards. The goal of this approach is to achieve better comparability between firms with and without women on the board. Since a relatively large share of firms has no or only one woman, we distinguish between firms with (group 1) and without any women (group 0) on the board in the first half of our sample period when external pressure was still considerably lower. That is, we only compare firms that have had at least one female director before the year 2010 to those firms without a female director, but that are otherwise very similar. The idea of the 2010 cut-off is that there was an increased external demand for female directors throughout Europe afterward.

We use Mahalanobis Distance-based Nearest-Neighbor matching to find the most similar firms in both groups (Imbens, 2004). Distance matching allows finding the closest neighbor(s) of a particular observation within a radius in terms of the applied characteristics (industry, country, firm age and size, Tobin's Q, and board size) to all other observations in the sample. Each observation from group 0 obtains a weight after the matching. The weights balance the distribution of the characteristics of group 0 according to the distribution of those in group 1, i.e. a t-test of differences in means is insignificant for all included measures. The weight of a group 1 observation is always equal to one, while the sum of the weights of its counterfactuals also adds up to 1 (Doherr, 2021). The weights are then used for the subsequent estimation of Specification (4) in Table 2.4. Previous conclusions regarding the negative link between the ex-ante share of women and the likelihood of a woman being appointed to the supervisory board hold. However, the *saturation* effects disappears after 30% of women representation, instead of 50% as in our main specification in Table 2.2. This result by itself points to *critical-mass* effects, but is not robust to alternative specifications. The *replacement* effect is still present and statistically significant.

Finally, we address remaining endogeneity concerns by generating instrumental variables for our main predictors. We follow the approach proposed by Lewbel (2012) who develop a method of a two-stage least squares regression without the need for an external instrumental variable. Finding appropriate instrumental variables which satisfy all formal requirements is often difficult in settings like ours. In Lewbel's method, identification is achieved by including regressors from within the data that are uncorrelated with the product of heteroscedastic errors.[14] One precondition is that the first-stage errors are indeed heteroscedastic. In our case, this is fulfilled for all our endogenous variables, i.e the shares and exit dummies. We do not over-identify our model and have as many generated exogenous instruments as endogenous predictors. We perform a test for the presence of weak instruments proposed by Stock and Yogo (2005) and find the Kleibergen-Paap Wald F-statistic of 56.9 above the rule-of-thumb critical values. Therefore, we can reject concerns for weak instruments. The results from this heteroscedasticity-based instrumental variable approach (Specification (5) of Table 2.4) are in line with our main and alternative specifications. We observe a negative significant effect of the share of women on the appointment

[14]See Baum and Lewbel (2019) for a more detailed discussion of the method.

probability of at least one female supervisory director and a statistically significantly higher appointment probability if a woman leaves, compared to when a man leaves the board.

2.4.4 Analysis of Executive Director Appointments

The descriptive evidence in Figure 2.2 suggests differences in appointment dynamics between supervisory and executive boards. Therefore, we analyse the appointments of female directors to executive positions. We re-run previous models with non-executive and executive predictors on women appointment probabilities to the executive board. The estimation results in Table 2.5 show similar but weaker, negative relationships between the lagged share of executive women directors and the probability to appoint at least one new female executive director. In Specification (1) of Table 2.5, we find that a 1 percentage point increase in the share women on the executive board results in an, on average, 0.6 percentage points decrease of the appointment probability for women. The squared term is positive and significant, but smaller. Again, the demand for women directors is increasingly saturated up to a certain point.

We illustrate the *saturation* effect dynamics from Specification (1) in Figure 2.5. The point at which the *saturation* effect stagnates is lower for executive director appointments and before gender balance is reached. The negative relationship between female executive representation and appointment is statistically significant at low shares of female executives. The dynamics point to an *exposure* effect, however the appointment probability does not yet increase after attaining the critical mass of 30%.

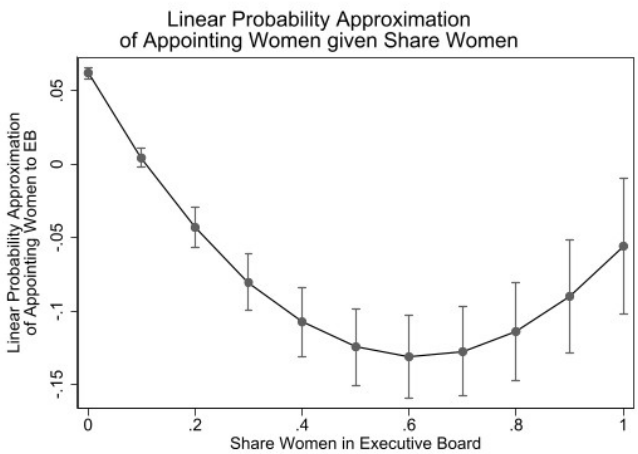


FIGURE 2.5: Margins of Executive Women Appointment Probabilities at Share Women Thresholds

TABLE 2.5: Estimation Results for Women Appointments as Executive Directors

This table reports the results of the impact of executive and supervisory board composition in terms of director gender and executive board dynamics in terms of director exits on the appointment probability of executive women in linear probability models. Specification (2) adds the Dependence Indicator, equal to one if ownership is concentrated. Specification (3) adds additional board-level variables as controls. Specification (4) replaces the share women in the executive board with an integer indicating the number of women directors. Note that we combine cases with four and more than four women in the same category. The linear models are estimated with OLS and fixed effects on the Year (Y) and Firm (F) level. Standard errors clustered at the firm level are in parentheses; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

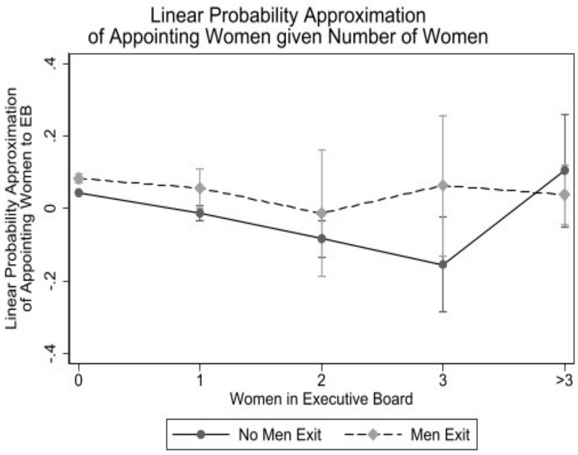
	(1) Main Predictors	(2) Dependence Indicator	(3) Board-Level Predictors	(4) Number Women
Share Women in EB	-0.629***	-0.723***	-1.229***	
	(0.053)	(0.059)	(0.164)	
Share Women in SB	0.032	0.026	-0.160**	0.022
	(0.029)	(0.033)	(0.062)	(0.028)
Number Women in EB				-0.047***
				(0.010)
Share Women in EB × Share Women in EB	0.511***	0.583***	1.018***	
	(0.056)	(0.063)	(0.272)	
Share Women in SB × Share Women in SB	-0.009	0.010	0.182*	-0.008
	(0.040)	(0.045)	(0.079)	(0.038)
Women Exit from EB	0.119***	0.129***	0.207**	0.080**
	(0.028)	(0.032)	(0.075)	(0.028)
Men Exit from EB	0.040***	0.042***	0.057**	0.043***
	(0.007)	(0.008)	(0.018)	(0.007)
Dependence Indicator		-0.002	-0.009	
		(0.006)	(0.010)	
CEO is a Woman			0.057	
			(0.086)	
Chairwoman			0.027	
			(0.056)	
Director Tenure			-0.005	
			(0.003)	
Share Independent Directors			-0.023	
			(0.037)	
Share Foreign Directors			0.090	
			(0.084)	
Share Multidirectors			0.019	
			(0.030)	
Director Age			-0.004*	
			(0.002)	
Board Size	0.006***	0.007***	0.002	0.005***
	(0.001)	(0.001)	(0.003)	(0.001)
Firm Age	0.004**	0.005**	0.014**	0.003**
	(0.001)	(0.002)	(0.005)	(0.001)
log(Total Assets)	-0.004	-0.004	-0.006	-0.005
	(0.003)	(0.004)	(0.007)	(0.003)
Tobin's Q	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.001)	(0.000)
GDP per Capita	-0.000	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Employment Rate	-0.115	-0.211	-0.284	-0.107
	(0.140)	(0.173)	(0.281)	(0.130)
Women Labor Force Rate	0.492	1.078	0.779	0.555
	(0.522)	(0.751)	(2.061)	(0.470)
Constant	-0.155	-0.396	0.025	-0.182
	(0.270)	(0.376)	(0.928)	(0.246)
Fixed Effects	Y F	Y F	Y F	Y F
N	20672	17328	5379	20672

Testing the conjecture of possible spillover effects, we further investigate the influence of ex-ante gender diversity among the non-executive directors who are generally involved in hiring the executives of a firm. For this purpose, we include the share of women in the supervisory board as an additional predictor in all specifications. We find no consistent empirical indications of a positive or negative relationship between the presence of female supervisory directors and the promotion of women as executive directors. The share of women on the supervisory board does not seem to influence the executive women director appointment probability, contradicting our spillover hypothesis 4.

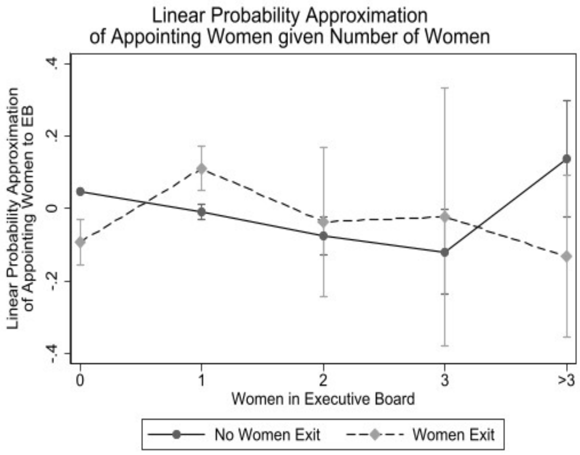
With regard to possible *replacement* effects, we see in Specification (1) that the increase in appointment probabilities for the cases when a man or woman leaves the executive board is smaller than for non-executive appointments. However, the increase for when a woman leaves the executive board is still approximately three times higher than when a man leaves the board. Therefore, we also confirm our *replacement* hypothesis for executive appointments. Interestingly, we find a positive relationship between board size and executive appointment probabilities.

Our results hold when additionally controlling for a firm-year-specific dependence indicator in Specification (2) and become stronger when controlling for further board-specific indicators in Specification (3) of Table 2.5. In Specification (3), we find that the share women in the supervisory board negatively affects the executive women appointment probability. This result is not robust to alternative specifications, but points to *saturation*, spillover, and queen-bee effects. However, we do not find evidence that chairwomen or woman CEOs positively influence the appointment of women executive directors.

In Specification (4), we replace the share women with a categorical variable of the number of women. Our results remain unchanged. With each additional woman already on the executive board, the appointment probability of appointing a new female executive director decreases by 5 percentage points. We visualize the levels of the number of women and their effect on the appointment probability in Figure 2.6. Compared to Specification (4), we include an interaction between the number of women in EB and our director exit indicators. In both subfigures, we see that the *saturation* effect is weaker for executive, compared to supervisory appointments. Especially, when a director, male or female, leaves the board, the *saturation* effect disappears. Finally, we observe a positive *exposure* effect on women's appointment probability when the executive board has more than three women in the previous year.



(A) Table 2.5 Specification (4) with Men Exit Interaction



(B) Table 2.5 Specification (4) with Women Exit Interaction

FIGURE 2.6: Margins of Executive Women Appointment Probabilities at Number of Women Thresholds

Next, we rerun our main analysis on cross-sectional subsamples in Appendix Table A.3 and perform robustness checks in Table 2.6. We include at least one director appointment in each firm-year observation in Specification (1) of Table 2.6 and at least one director exit in Specification (2). The coefficients in Specification (1) are larger than in our main specification. A 1 percentage point increase in the share of executive women leads to an, on average, decrease of 1.6 percentage points in the probability of appointing at least one female executive director. Again, the average appointment probabilities are higher in both subsamples. The average women appointment probability in our main sample is 4%, while the appointment and exit subsample probabilities are 19% and 9%, respectively. We do not observe a significant *replacement* effect in either specification.

In Specification (3), we estimate a dynamic model and include lagged values of the dependent variable as auto-regressive terms to control for persistence in the dependent variable (Matsa and Miller, 2011). The auto-regressive terms show that the appointment of a woman in previous years is associated to a lower appointment probability in the subsequent year. The lagged appointments pick up some of our main specification's *saturation* and *replacement* dynamics.[15]

[15]We do not perform nearest-neighbor matching based on whether the firm had early women presence in the executive board, because the number of these firms in the pre-2010 period is too small rendering the matching infeasible.

Table 2.6: Robustness Checks for Women Appointments as Executive Directors

This table reports robustness checks on the main Specification (1) from Table 2.5. Specification (1) includes firm-year observations with at least one executive director appointment. Specification (2) includes firm-year observations with at least one executive director exit. Specification (3) is a dynamic auto-regressive model of order two (AR(2)) which controls for the persistence of the dependent variable. Specification (4) uses heteroscedasticity-based exogenous instruments for our main endogenous variables of interest (Share Women in EB and SB and Exits from EB). We include the previous additional firm (F) - and country (C) - level controls. Fixed effects are on the Year (Y) and Firm (F) or Year (Y), Country (C), and two-digit SIC-industry (S) level. Standard errors clustered at the firm level are in parentheses; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

	(1) Appointment Subsample	(2) Exit Subsample	(3) Dynamic Specification	(4) IV: Het.-Based
Share Women in EB	-1.654***	-0.606**	-0.487***	0.011
	(0.141)	(0.208)	(0.066)	(0.009)
Share Women in SB	0.118	-0.182	0.035	0.033
	(0.129)	(0.207)	(0.031)	(0.020)
Share Women in EB × Share Women in EB	1.251***	0.512*	0.408***	0.040
	(0.160)	(0.235)	(0.066)	(0.035)
Share Women in SB × Share Women in SB	-0.101	0.356	-0.009	-0.020
	(0.173)	(0.492)	(0.044)	(0.027)
Women Exit from EB	0.061	-0.023	0.100***	0.100***
	(0.049)	(0.050)	(0.028)	(0.027)
Men Exit from EB	-0.010		0.039***	0.042***
	(0.019)		(0.007)	(0.006)
L.Women Appointment to EB			-0.063***	
			(0.017)	
L.2.Women Appointment to EB			-0.044**	
			(0.015)	
Weak Instrument Test				4770.08
Fixed Effects	Y F	Y F	Y F	Y C I
Additional Controls	C F	C F	C F	C F
N	4253	2064	19259	20672

Finally, when accounting for endogeneity in Specification (4) of Table 2.6, we find that the results regarding the ex-ante share of executive women are not robust. In the IV model, neither the share women in the executive nor in the supervisory board is statistically significant. These results indicate that the *saturation* effect from hypothesis 1a is not as evident in the executive board as compared to the supervisory board. The *replacement* effect persists in our IV model and we partly validate hypothesis 2. Finally, we confirm our third hypothesis, where supervisory director appointments are more gender-specific than executive appointments. Taken together, our results suggest that demand-side factors, such as public pressure and biases, play a role in board director appointments. However, the findings also suggest that such factors can lead to *t(w)okenism* and that representation is often bounded to the minimum level which the explicit or implicit norm prescribes.

2.5 Conclusion and Discussion

The presence of women in top corporate boards, and hence their role in corporate decision-making, is receiving considerable attention in the public and policy debate. Yet, factors explaining the decision whether to promote female or male candidates to the board are still understudied from both a theoretical and an empirical perspective. The question is of particular interest to policymakers as well as companies since promoting diversity has become a political objective and attracts much attention and controversy.

The present study aims to provide novel empirical evidence on the dynamics of appointments to corporate boards. Building on a new dataset of director appointments in European listed firms in the period 2002 to 2019, our empirical findings shed light on the influence of internal board characteristics and dynamics on the appointment of new board members. We distinguish in our analyses between executive and non-executive roles arguing that there are different appointment dynamics depending on the board position to be filled. In addition, it allows us to test whether there are spillover effects from non-executive to executive directors as often argued by proponents of quotas.

We build on organizational behavior literature and research on minority and majority influence on group decision-making. Our hypotheses establish a link between ex-ante board structure and dynamics and the appointment of women to board positions. In our first hypothesis, we contrast *saturation* and *exposure* effects. Pressure to comply with explicit or implicit norms in combination with discriminatory biases can lead to a *saturation* of demand for diversity, whereas, according to homophily theory and updated beliefs, increased *exposure* to women above a critical mass increases the demand for diversity.

Our findings indeed support the former theory. The results show that the probability of a woman being appointed to a non-executive director position declines with the share of women already on the board. The appointment probability is highest when no women are present and strongly declines with each additional woman present on the board. Moreover, an appointment is significantly more likely when a woman, compared to when a man, leaves the board. Thus, gender appears to play a significant role in the appointment dynamics of non-executive directors. These patterns are more pronounced in environments - industries and countries - with increased external demand (e.g. in the presence of quotas) and a lower supply of women from the labor force.

For the appointment of executive directors, we find similar but weaker results regarding the relationship between existing diversity and new appointments. The executive appointment probability is highest when no executive women are present and declines with each additional woman until a critical mass of 30% is reached. This *saturation* effect is not robust to our heteroscedastic instrumental variable approach and disappears when another director leaves the board. The *replacement* dynamics persist in most specifications. Diversity norms seem to play a role in appointments to executive positions, however these appointments are less gender-specific and less prone to *tokenism*.

After attaining gender balance on the supervisory board or a critical mass on the executive board, the *saturation* effect diminishes and a further increase in the share of women does no longer result in a lower appointment likelihood for women. The benefits of increased gender diversity might counteract discrimination and external pressure from social norms and result in a flat relationship between gender diversity and women's appointment probability.

Finally, we do not observe spillover effects between roles such that non-executive women directors support more appointments of women as executive directors. The existence of such spillovers has been used to support quotas for non-executive positions based on the assumption that a critical mass of women in any type of board role would support the addition of more women in top corporate jobs (Gagliarducci and Paserman, 2015, Tate and Yang, 2015). The absence of such effects in European listed companies suggests that other considerations may play a stronger role in the appointment to executive roles or that the influence of women in supervisory roles is rather limited.

A reason for weaker gender-specific effects on executive women appointments may be the still very low representation of women in such jobs and hence the lack of sufficient variation in our dependent variable. Women executive appointments, and especially women CEO appointments, are still extremely rare. The share of women among all executive directors accounts for only eight percent during our sample period. Future research may therefore investigate the question of cross-role spillovers once the number of women directors is higher. Then, a separate investigation of internal and external CEO appointments might add valuable insights (Agrawal et al., 2006, Tsoulouhas et al., 2007).

We acknowledge that our empirical investigation is based on listed firms only. It might be interesting to explore whether the findings are transferable to unlisted European firms, since our results may be specific to listed companies that are more in the spotlight of public attention. Second, future research efforts could be undertaken to systematically disentangle possible reasons for director turnover and thus, corresponding succession events. Directors who leave on friendly terms might have a say in their *replacement* and increase the likelihood of being replaced by candidates from their own network. Finally, with information on individual characteristics like education, professional experience, and family situation, future research could investigate supply effects and determine to what extent eligible candidates differ.

Our findings have implications for both business practice and policymakers. While a number of voluntary recommendations for board diversity have been formulated in national or European corporate governance codices, the empirical findings clearly suggest that solely relying on labor market mechanisms does not close the gender gap on corporate boards. Further, a quota does not result in self-reinforcing dynamics with more women appointments once the quota is reached. On the contrary, the appointment probability of women declines strongly with an increasing share of women below gender balance. Quotas increase the attention on gender and seem to increase *token* appointments.

Our analyses show that women appointments to non-executive positions have intensified, while the fraction of women as executive directors remains until now on a very low level. Even though both type of directors have important roles and responsibilities inside a firm, non-executives have less strategic influence and often have full-time responsibilities at other firms. With more detailed data available for European companies, a systematic analysis of committee memberships among male and female non-executive directors would provide further insights into the role newly appointed directors assume and their corresponding influence on corporate decisions. As a consequence, regulations that address diversity could distinguish between different functions and roles on corporate boards. Policymakers should consider further measures to foster gender equality and overcome discrimination, particularly in the fields of education, family, and social policy.

3 | Seven in Heaven? Board Gender Quotas, Monitoring Quality, and Firm Performance

by Eline Schoonjans

Seven European countries have implemented mandatory board gender quotas in the last two decades. I examine how this exogenous shock to the board of directors in publicly-listed firms impacts board- and firm-level outcomes. My analysis is the first to exploit the scattered implementation of these seven European quotas between 2008 and 2019, using a staggered difference-in-differences design. I document an economically meaningful decrease in firm performance and monitoring quality, measured by the level of earnings management. Evidence from moderator analyses show that the negative quota effect on performance is stronger in firms with higher ex-ante monitoring quality. My results suggest that imposing restrictions on specific board attributes can produce inefficient board structures and negative real effects.

3.1 Introduction

Gender parity in 100 years? According to the Global Gender Gap report of the World Economic Forum, projecting current trends, gender parity will not be attained for another 100 years. While educational attainment, health, and survival have developed close to parity, the progress of economic participation and opportunity worldwide has stagnated since 2016. The number of women in leadership positions progresses slowly. Therefore, many countries choose to accelerate this slow-moving process by imposing gender quotas for the representation of women in leadership positions. Half of the countries showing over one-third of female board directors have implemented such quotas for female directors in the last two decades (World Economic Forum, 2020).

In Europe, seven countries - Norway, Italy, France, Germany, Belgium, Austria, and Portugal - have implemented mandatory board gender quotas in the last two decades. I exploit the countries' varying implementation dates through a staggered difference-in-differences design to investigate how these quotas impact firm performance and monitoring quality. Two existing theories about the structure of corporate boards lead to opposing predictions regarding my research question. Hence, I formulate my hypotheses bi-directionally. First, I expect a negative or positive quota effect on firm performance. Next, I expect monitoring quality to increase or decrease upon quota implementation and, finally, to play a mediating or moderating role in the quota-performance relationship.

Gender-based biases can add to the *captured boards theory*, where firms choose CEO-friendly directors from the "old boys" networks (Bebchuk and Fried, 2005, Bertrand et al., 2018). This "old boys" barrier, enforced by stereotypes, might exclude women (McPherson et al., 2001, Michelman et al., 2022) and raise the standards of ability and competence (potential) female directors are evaluated by (Kanter, 1977a, Bilimoria and Piderit, 1994, Biernat and Kobrynowicz, 1997). If this is the case, an exogenously enforced increase in board diversity could improve boards' effectiveness. More diversity could improve group dynamics and shift effort norms. Female directors could reduce conflicts, improve cohesiveness, and contribute unique fundamental preferences, knowledge, and skills (Forbes and Milliken, 1999). A vast body of research has explored the fundamental preferences of female directors and their lower tolerance towards opportunistic behavior (Bernardi and Arnold, 1997, Betz et al., 1989). Substantial empirical evidence suggests that female directors exhibit greater diligence in monitoring, assume positions on committees responsible for transparent reporting and earnings quality, and are associated with higher accounting quality (Adams and Ferreira, 2009, Gul et al., 2011, Srinidhi et al., 2011). Consequently, the benefits of adding female directors should be most visible in measures of monitoring quality.

On the other hand, the *theory of efficient board structures* suggests that shareholders choose their boards in order to maximize value. If this is the case, laws that constrain shareholders' choices can decrease firm values (Demsetz and Lehn, 1985). Female directors appointed through quota processes might differ from voluntarily appointed female directors, especially if there is a lack of eligible candidates (Ahern and Dittmar, 2012). Exogenously dictated gender quotas could affect other board attributes than gender and, ultimately, negatively impact group dynamics. Board dynamics and demographics, such as age, education, and professional experience are susceptible to change after quota adoptions and are likely to affect a board's ability to monitor and advise. In consequence, the direction of the quota effect on monitoring quality and firm performance is yet to be determined empirically.

Existing evidence on the empirical assessment of "quota women" in Europe that goes beyond equality issues is mostly mixed. Norway was the first country to implement board gender quotas and has been subject to most studies to date. Several studies investigate Norway's quota effect on director qualifications, firm performance, profitability, and labor market outcomes (Ahern and Dittmar, 2012, Matsa and Miller, 2013, Bertrand et al., 2018). Their results suggest that there is a negative effect on firms' market and accounting performance. Studies from France show negative and mixed effects (Bennouri et al., 2018, Kuzmina and Melentyeva, 2021), whereas Italy shows some evidence of positive and no impacts on specific measures of performance (Comi et al., 2019, Kuzmina and Melentyeva, 2021) and market reactions (Ferrari et al., 2022) in early quota adoption and implementation years.[16]

Similar conflicting evidence in prior literature relates to mechanisms, such as changes in particular board attributes, through which quotas might affect firm performance. Ahern and Dittmar (2012) find younger and less experienced directors, but cannot rule out additional mechanisms such as varying fundamental preferences of female directors or disrupted board dynamics and processes, that originate from the different behavior of existing and newly appointed directors. Matsa and Miller (2013) rather find quota-induced changes in firm policies and strategies that

[16] Kuzmina and Melentyeva (2021) additionally investigate performance effects in voluntary quota adopting countries UK, Spain, and the Netherlands.

are linked to fundamental differences in gender, not age or experience. They find that these changes in firm policies and strategies are related to the advisory role of the board of directors.

My study addresses some of the internal and external validity concerns prior single-country studies face. I apply a staggered difference-in-differences design to exploit the European countries' scattered implementations of mandatory board gender quotas between 2008 and 2019. My design reduces consistency biases resulting from confounding events, concurrent institutional changes, economic shocks, and market responses to the events that gave rise to the regulation (Leuz and Wysocki, 2016). Especially, the timing of the Norwegian quota has raised some concerns, as its adoption was closely related to the European IFRS implementation period in 2005 and the quota's implementation was closely related to the financial crisis in 2008 and 2009 (Hughes et al., 2017). In additional analyses, I allow for country-specific treatment effects and heterogeneous trends. The advantage of my identification design is the mitigation of concerns about between- and within- firm and -country variations (Bertrand and Mullainathan, 2003).

Besides the second-order effects of gender quotas on firm performance, I investigate first-order effects on the board level. I capture the impact of gender quotas on one measure of monitoring quality, earnings management. The reasons to choose earnings management are threefold. First, the impact of board diversity, often associated with independent thinking and directors (Carter et al., 2003, Adams and Ferreira, 2009), on monitoring quality may be more important in a financial reporting setting, which has often been the focus of attention to regulators, requires extensive financial expertise, continuous monitoring efforts, and a high level of cognitive independence (Wahid, 2019). Second, I attempt to provide results along the causal path by first linking the regulatory mandate to changes in reported earnings and then linking the observed reporting changes to real- and capital-market consequences (Leuz and Wysocki, 2016). Third, the manipulation of earnings is often increased in lower investor protection environments, such as the quota implementing European countries compared to the United States or the United Kingdom (Leuz et al., 2003).

I test my hypotheses in a sample that comprises 67,624 firm-year observations from 7,055 publicly-listed firms in 16 countries. The dataset spans the years from 2001 to 2019. I follow prior literature and capture market-based firm performance by Tobin's Q (Ahern and Dittmar, 2012, Masulis et al., 2012, Adams et al., 2018). I capture firms' earnings management by the absolute value of discretionary accruals estimated from the performance-adjusted Jones and modified-Jones model (Jones, 1991, Dechow et al., 1995, Kothari et al., 2005). For robustness, I additionally examine measures of real earnings management, specifically abnormal levels of cash flow from operations and discretionary expenses (Cohen et al., 2008).

My results suggest that an exogenous shock to the board of directors, induced by mandatory gender quotas, has negative effects on both board- and firm-level outcome measures. First, after the implementation of mandatory quotas, I find a significant and economically meaningful average decline of 6% on Tobin's Q, evaluated at the treatment firms' mean. Next, I investigate country-specific differences in the quotas' effects. The negative effects are strongest in Norway and France. Finally, I find a significant increase of around 8% in absolute discretionary accruals when the quota is implemented.

All results are robust to alternative and industry-adjusted measures of firm performance and earnings management, as well as alternative sample specifications. I perform all additional analyses and robustness checks within a sample of quota-adopting countries only. This approach alleviates some remaining concerns about fundamental and unobserved differences between the quota-adopting and non-adopting countries (Bertrand and Mullainathan, 2003). Moreover, I implement a falsification test and address the potential endogeneity of the quotas' implementation timing (Altonji et al., 2005). I do not find evidence under the alternative explanation that observable local market conditions and other economic shocks induce my results.

Additionally, I perform a mediation analysis to identify potential board-level channels of my effect and document a direct quota effect on market-based firm performance, that is not driven by changes in specific board demographic attributes or levels of earnings management. My results do not detect a significant relationship between these board-level variables and market-based firm performance. Firm performance and shareholders' expectations regarding future firm performance, captured by Tobin's Q, appear to be directly affected by the mandatory quotas and even persist when controlling for important board-level demographic and monitoring attributes.

Finally, my moderation and cross-sectional analyses suggest that internal and external measures of monitoring quality moderate the quotas' effects on firm performance. I find a stronger negative quota effect on performance for firms with higher ex-ante shares of independent directors and lower ex-ante levels of earnings management. A higher number of analysts following proxies as an external indicator for monitoring quality and is also associated with a stronger negative quota impact on performance. Moreover, firms in typically "male-dominated" industries document a stronger negative quota effect on performance.

This study adds to three streams of literature: First, I contribute to the governance literature which explores whether and how board capital and relational attributes impact firm performance through the board's effectiveness. My results provide some evidence in favor of the *efficient board structure theory*. Whether board structures are efficient or inefficient to begin with, imposing additional restrictions through gender quotas makes boards more inefficient and negatively impacts firm performance. Moreover, as predicted by Adams and Ferreira (2009), these restrictions especially hurt firms, who were ex-ante better at choosing value-maximizing boards.

Second, I add to the accounting literature which explores various regulatory mechanisms found to be effective at ensuring financial reporting integrity. Mandatory quotas appear to neutralize and invert the documented positive effect that voluntarily appointed female directors have on monitoring quality and financial reporting integrity. So far, monitoring quality, often examined in the context of voluntary board gender diversity, has gained little attention in the context of mandated gender quotas.[17] To the best of my knowledge, my study is the first to investigate a mandatory quota effect on financial reporting integrity and firm performance in an exhaustive European multi-country setting.

Third, I add to the literature on mandated quotas for the representation of minorities in governments, political parties, and firms. I exploit the implementation of European board gender quotas in a staggered fashion as an exogenous shock to the boards' effectiveness and resulting firm performance. Studying board effectiveness and efficiency in this natural quasi-experimental setting and controlling for country-specific factors such as institutional context and existing gender parity alleviates endogeneity concerns and improves external validity. I validate prior single-country studies and find a robust negative average effect of the mandatory quotas on market-based firm performance in Europe. Moreover, I capture direct quota effects on board- and firm-level outputs, that are not driven by important board capital attributes such as gender, age, tenure, and multi-directorship. Consistent with my results, I expect similar potential direct effects of future regulatory mandates for board diversity along other aspects than gender, including ethnicity, sexual orientation, and affiliation. For example, Nasdaq Inc. recently proposed a quota concerning the representation of directors who identify as either women, an underrepresented minority, or as LGBTQ+.

[17] Nekhili et al. (2020) measure the impact of the French quota on audit fees, others measure the effects on earnings management in France (Damak, 2018) and some voluntary and mandatory quota-adopting European countries (Saona et al., 2018).

The rest of my paper is organized as follows. I discuss the institutional quota backgrounds in Section 3.2 and define my sample and variables of interest in Section 3.3. In Section 3.4, I first outline my research design and then present my empirical results. Section 3.5 presents the results of additional mediation and moderation analyses, and Section 3.6 concludes.

3.2 Institutional Background

> The widespread adoption of gender quotas is one of the most important political developments of the modern era. (O'Brien and Rickne, 2016)

Countries around the world have followed the trend of political gender quotas and have adopted various kinds of corporate governance regulations to address and fasten the slow-moving process of gender parity in economic participation and leadership positions (World Economic Forum, 2020). While many countries have implemented "comply or explain" regulations to increase public scrutiny for deviating from governance norms, others have implemented more strict mandatory quotas of female representation in the board of directors.

Table 3.1 summarizes the implications of mandatory board gender quotas in seven European countries. I focus on quota-adopting countries that enforce sanctions when the quota mandate is violated. This distinguishes my paper from other multi-country studies (Comi et al., 2019, Kuzmina and Melentyeva, 2021). By excluding countries that adopted soft quotas from the treatment group, I reduce selection biases resulting from firms' voluntary implementation and strengthen my measured effect.

TABLE 3.1: Board Gender Quotas in Europe

This table shows the adopted and implemented quotas in seven European countries. The table includes all mandatory quota adopters in Europe. I define quotas as mandatory if their violations result in real sanctions. The implementation date is defined as the date by which firms had to comply in order to avoid sanctions.
SB: Supervisory board, EB: Executive board.
Sources: [1]Regjeringen (2003), [2]Gazzetta Ufficiale Della Repubblica Italiana (2011), [3]Secrétariat général du Gouvernement (2011), [4]Bundesanzeiger Verlag (2015), [5]Moniteur Belge (2011), [6]Bundesgesetzblatt für die Republik Österreich (2017), [7]Diário da República eletrónico (2017)

Country	Adoption	Announcement	Implementation	Quota	Scope SB	Scope EB	Applicability	Sanctions for non-compliance
Norway[1]	Dec 19, 2003	Dec 19, 2003	Jan 1, 2008	40%	◇		Public-limited firms	Refusal to register a new board. Dissolvment by force.
Italy[2]	Jul 12, 2011	Jul 28, 2011	2015	30%	◇	◇	Listed firms	Up to €1mn penalty.
			Aug 12, 2012	20%			large firms	Nullification of appointments.
France[3]	Jan 13, 2011	Jan 28, 2011	Jan 1, 2017	40%	◇	◇	Listed &	Suspension of compensation.
			Jan 1, 2014	20%			large firms	Nullification of appointments.
Germany[4]	Apr 24, 2015	Apr 30, 2015	Jan 1, 2016	30%	◇		Listed co-determined firms	Voidness of election and of board decisions.
Belgium[5]	Jul 28, 2011	Sep 14, 2011	2019	33%	◇	◇	Listed firms	Cancellation of board members' benefits.
			2017				Large listed firms	
Austria[6]	Jul 6, 2017	Jul 26, 2017	Dec 31, 2017	30%	◇		Large listed firms	Nullification of appointments.
Portugal[7]	Jun 23, 2017	Aug 1, 2017	Jan 1, 2020	33%	◇	◇	Listed firms	Appointment is qualified as "merely provisional".
			Jan 1, 2018	20%				

The quotas' implementation dates lie between 2008 and 2019. All adopting countries have a code law legal tradition. The mandated percentage of female representation varies between 20% and 40%, where France and Italy implemented their percentages in a staggered fashion. The range of applicability goes from all listed firms to only big listed firms exceeding a certain threshold in Belgium or Austria, or subject to different governance regulations in Germany.

The scope of the quotas' mandated female representation varies across countries. Whereas the quota applies to supervisory boards in two-tier board structures only, it applies to the complete board of directors in countries and firms with unitary board systems. Norway, Germany, and Austria have two-tier board structures by law. Therefore, the quota does only apply to the employee-, and shareholder-elected supervisory board members. In Italy, France, Belgium, and Portugal firms choose the structure which suits them best.

I define the post-treatment period as the years on and after the quotas' implementation dates for two reasons. First, even though firms started hiring more female directors as of the quota announcement dates, the steepest increases in female board representation occurred in the year of the implementation, as shown in Figure 1.3b. I show the evolution of female director representation in the board of directors for each country in Appendix Figure A.1. Assuming that effects of board composition on board- and especially firm-level outcomes would take at least a couple of months and up to two years to materialize, choosing the implementation year as threshold between the pre- and post-treatment period seems reasonable (Triana et al., 2014).

For Norway, I set the implementation date to January 2008, as firms had a two-year transition period to comply to the new law that became compulsory on January 1, 2006. I deal with the staggered quota adoption in France and Italy by defining the implementation dates of the first threshold relevant for treatment if their share of female directors still falls behind that threshold. Especially, in France many firms had already exceeded the 20% threshold by 2014. Remaining firms are treated on and after the second threshold implementation dates. My results are robust to both defining the treatment period after only the first or the second threshold. I report them in Appendix Table A.7.

All Belgian firms are treated as quota firms as of 2019. Large listed firms, counting over 250 employees and €50m in sales revenue or €43m in total assets are additionally subject to the quota as of 2017. In Germany and Austria, only approximately 100 and 30 firms respectively are subject to the quota mandates and count as treatment firms.

3.3 Sample and Descriptive Statistics

3.3.1 Sample and Data

My initial sample consists of all publicly-listed firms in 16 European countries between 2001 and 2019. I exclude firm-year observations with less than €10m of total assets and utility, financial, and insurance firms,[18] because they are subject to additional regulations and specific financial reporting standards. This yields a sample of 67,624 firm-year observations from 7,055 firms. 1,293 firms from seven European countries are eventually subject to the quota and count 5,334 treated firm-year observations. In Appendix Table A.6, I report the distribution of my sample across all 16 countries.

[18]Two-digit SIC code 49 and one-digit SIC code 6.

I obtain accounting and analysts' forecast information from Thomson Reuter Refinitiv's Worldscope and I/B/E/S. I obtain country-level control variables from the OECD and World Justice Project databases. In additional analyses on a sub-sample, I use director data that I obtain through Orbis from Bureau van Dijk.

I take advantage of the extended time period and perform my earnings management based regressions in a sample period from 2007 to 2019, excluding the IFRS implementation period. I choose to exclude all years prior to 2007, as my earnings management variables include lagged measures that still capture variations from the 2005 fiscal-year IFRS implementation in 2006. I visualize the volatility of my earnings management variable before 2007, possibly due to IFRS, in Appendix Figure A.2. Moreover, I report regression results for excluding all years prior to 2010 in Appendix Table A.8. Hence, I mitigate concerns regarding the robustness of my results to excluding two potentially confounding events, IFRS implementation and the financial crisis, which have been subject to critics in existing studies (Hughes et al., 2017, Eckbo et al., 2022).

3.3.2 Variable Definitions and Descriptives

I include four sets of variables in my regressions. First, the dependent variables measure firm performance and monitoring quality. Second, my variable of interest equals one if the firm is subject to the quota mandate in that year. Third, I include several firm-level and, fourth, country-level control variables. All continuous variables directly obtained from Thomson Reuters or Bureau van Dijk are winsorized at the top and bottom 1% of their distributions. I provide detailed definitions of all variables in Appendix Table A.5.

To test my first undirected hypothesis, I compare changes in firm performance among treatment and control firms. I follow prior literature and use Tobin's Q to capture market-based performance. For completeness, I also study the quota effect on accounting performance captured by the return on assets. I focus on market-based performance for two reasons. One, it directly relates to shareholder value and two, it is less affected by accounting conventions and strategic manipulation of earnings (Dechow et al., 1995). These features are especially beneficial in my setting, because my sample period includes major changes in accounting regulations, induced by IFRS, and because the goal of this study is to separately identify the aforementioned strategic manipulation of earnings.

I follow Ahern and Dittmar (2012) and compute Tobin's Q as the sum of total assets and market equity less common book equity divided by total assets. My results are robust to logarithmic, alternative,[19] and industry-adjusted measures. I obtain return on assets from Worldscope directly, which measures the net income before interest and taxes divided by the average of last and current year's total assets.

I proxy monitoring quality by accrual-based earnings management to test my second hypothesis. Prior literature finds that an increase in monitoring quality contributes to higher accounting and earnings quality, fewer reporting restatements, fewer firm frauds, and fewer tax avoidances (Gul et al., 2011, Srinidhi et al., 2011, Cumming et al., 2015, Wahid, 2019, Fan et al., 2019). I examine the absolute value of discretionary accruals because my hypothesis does not predict any specific direction for earnings management. Moreover, the absolute value also captures accrual reversals following earnings management (Cohen et al., 2008). I provide detailed definitions of all earnings management variables in Appendix Table A.5.

I adhere to Kothari et al. (2005) for minimal rejection rate bias and use a cross-sectional performance-adjusted Jones model to measure discretionary accruals (Jones, 1991). Absolute discretionary accruals equal the absolute value of the cross-sectional estimation error in total accruals, adjusted by lagged return on assets, for each year in the same two-digit SIC code. For all regressions including measures of discretionary accruals, I exclude firm-year observations with fewer than ten observations in any two-digit SIC Code in any given year to prevent imprecise regression-model-based estimates (Kothari et al., 2005).

As is often discussed in prior literature, possible misspecification of earnings management caused by its unobservable nature could introduce measurement errors for its proxy, depending on the model used to estimate earnings management. However, this measurement error should not induce systematic biases in my findings as they should not differ across countries. Additionally, I test the modified and standard Jones model without performance adjustments (Jones, 1991, Dechow et al., 1995).

To validate the results of my second hypothesis, I examine additional real earnings management proxies. I test the quota effect on abnormal levels of cash flow from operations and discretionary expenses. Further, I look at two discretionary expenses separately, research and development and selling, general, and advertising expenses. Given sales levels, unusually low cash flow from

[19] Tobin's Q as the sum of market equity and total debt divided by total assets (Bennouri et al., 2018).

operations and/or unusually low discretionary expenses are likely to indicate firms that manage earnings upward (Cohen et al., 2008).

I test my third hypothesis in the fifth section of this paper and introduce both new firm-level and board-level variables. I include director age, gender, and nationality to capture demographic board attributes and director tenure and board size to capture relational board capital attributes (Bennouri et al., 2018). The share of independent directors and number of analysts following a firm introduce additional internal and external proxies for monitoring quality.

Finally, I follow prior literature and control for the women's representation in the general workforce and gross domestic product per capita in order to capture the time-varying economic environment of each country. On the firm level, I include firm age, firm size measured by total assets, leverage defined as total debt divided by total assets, and profitability computed from the net income before extra items or preferred dividends divided by net sales. To mitigate concerns about bad controls as described by Angrist and Pischke (2008), I repeat my analyses with lagged profitability and leverage and report the results in Appendix Table A.8.

Table 3.2 presents the descriptive statistics for my country-, firm-, and board-level variables. Panel A reports descriptive statistics for the full sample of 7,055 listed firms and Panel B for the 1,293 treated firms.

Table 3.2: Descriptive Statistics for Treatment and Control Firms

Panel A reports the country-, firm- and board-level descriptive statistics for the full sample of 16 European countries between 2001 and 2019.
Panel B reports the country-, firm- and board-level descriptive statistics for the firms that are subject to the mandatory gender quota.
See Appendix Table A.5 for variable definitions.

Panel A: Treatment and Control Firms

	N (firm-years)	Standard Deviation	Min.	Median	Mean	Max.
GDP per Capita	67624	9610.03	19533.30	37431.45	39120.41	88496.48
Women Labor Force Rate	67624	1.96	38.92	46.43	46.01	49.98
Tobin's Q	67624	1.58	-0.07	1.25	1.61	107.84
ROA	67106	13.56	-62.54	4.08	1.79	32.22
Log(Total Assets)	67624	1.95	2.37	5.06	5.36	10.58
Log(Net Sales)	67624	2.25	-6.91	4.94	5.05	10.32
Leverage	67624	0.22	0.00	0.20	0.23	10.66
Firm Age	67624	8.58	-4.00	13.00	13.86	39.00
Profitability	67624	152.59	-12765.00	0.03	-2.04	24819.00
Abs(DA)-Jones	45579	0.13	0.00	0.04	0.07	14.03
Abs(DA)-ROA-adj.Jones	45188	0.10	0.00	0.04	0.07	4.91
Abs(DA)-ROA-adj.Mod.Jones	45069	0.10	0.00	0.04	0.07	5.11
Log(Total Debt)	44872	2.64	-8.01	3.89	3.87	9.41
Log(EBITDA)	34761	2.16	-6.91	2.71	2.80	7.50
Log(Net Receiv.)	49316	2.16	-2.59	3.59	3.66	8.66
Log(PPE)	49406	2.67	-4.27	3.65	3.60	9.34
Log(Current Liabilities)	49395	2.09	-0.72	4.17	4.34	9.50
Log(Cash&Equivalents)	49448	2.34	-4.83	3.05	3.09	8.33
Log(Curr.Long-Term Debt)	39755	2.66	-8.11	1.84	1.82	7.73
Log(Net CF from Operations)	38432	2.16	-6.91	3.22	3.33	8.18
Log(R&D Expenses)	18527	2.29	-6.91	2.08	2.12	7.79
Log(SG&A Expenses)	35303	2.01	-1.83	3.76	3.96	8.94
Share Women Directors	31628	0.20	0.00	0.00	0.14	1.00
Share Foreign Directors	31628	0.21	0.00	0.00	0.12	1.00
Director Age	31628	6.05	36.25	54.73	54.29	69.67
Board Size	31628	3.95	1.00	6.00	6.05	20.00
Share Multidirectors	31628	0.29	0.00	0.38	0.38	1.00
Director Tenure	31628	3.43	0.00	4.00	4.49	48.00
Share Independent Directors	31628	0.29	0.00	1.00	0.81	1.00

Panel B: Eventually Treated Firms

	N (firm-years)	Standard Deviation	Min.	Median	Mean	Max.
GDP per Capita	16660	10234.40	19533.30	38432.45	40762.52	67639.11
Women Labor Force Rate	16660	2.36	38.92	47.23	46.18	49.98
Tobin's Q	16660	1.05	0.23	1.21	1.49	21.96
ROA	16580	11.64	-62.54	3.87	2.20	32.22
Log(Total Assets)	16660	2.09	2.37	5.74	5.97	10.58
Log(Net Sales)	16660	2.32	-6.91	5.48	5.61	10.32
Leverage	16660	0.19	0.00	0.23	0.25	2.90
Firm Age	16660	8.91	0.00	14.00	15.23	39.00
Profitability	16660	120.79	-9704.67	0.03	-2.58	2103.03
Abs(DA)-Jones	11818	0.10	0.00	0.04	0.06	2.47
Abs(DA)-ROA-adj.Jones	11755	0.09	0.00	0.04	0.06	2.46
Abs(DA)-ROA-adj.Mod.Jones	11715	0.09	0.00	0.04	0.06	2.50
Log(Total Debt)	12097	2.61	-6.91	4.59	4.55	9.41
Log(EBITDA)	9298	2.18	-4.51	3.21	3.25	7.50
Log(Net Receiv.)	12390	2.07	-2.59	4.35	4.47	8.66
Log(PPE)	12425	2.69	-4.27	4.38	4.29	9.34
Log(Current Liabilities)	12423	2.08	-0.72	4.95	5.10	9.50
Log(Cash&Equivalents)	12431	2.20	-4.83	3.77	3.92	8.33
Log(Curr.Long-Term Debt)	11236	2.54	-8.01	2.67	2.69	7.73
Log(Net CF from Operations)	10157	2.18	-6.91	3.79	3.92	8.18
Log(R&D Expenses)	4683	2.30	-6.91	3.01	3.02	7.79
Log(SG&A Expenses)	7307	2.06	-1.83	4.83	4.94	8.94
Share Women Directors	7642	0.25	0.00	0.17	0.22	1.00
Share Foreign Directors	7642	0.19	0.00	0.00	0.09	1.00
Director Age	7642	6.67	36.25	54.23	54.21	69.67
Board Size	7642	4.98	1.00	4.00	5.93	20.00
Share Multidirectors	7642	0.32	0.00	0.31	0.35	1.00
Director Tenure	7642	3.51	0.00	3.00	3.65	37.00
Share Independent Directors	7642	0.25	0.00	1.00	0.88	1.00

3.4 Empirical Results

3.4.1 Identification Strategy

My primary research objective is to assess the first- and second-order effects of mandated gender diversity on board of directors. Especially, I want to determine how quotas impact firm performance and monitoring quality. To address this question, I employ an empirical approach that exploits a quasi-natural experimental setting in Europe. I estimate the quota effect on firm performance and earnings management using a staggered difference-in-differences design with a variety of fixed effects and control variables.

Specifically, my regression model is as follows:

$$y_{it} = \alpha_{it} + \beta \ Quota_{it} + \mathbf{X}_{it}\delta + \lambda_t + c_i + \varepsilon_{it} \tag{3.1}$$

where i indexes firms, and t indexes time, y_{it} is the dependent variable of interest. $Quota_{it}$ is a dummy that equals one if a mandatory gender quota has been implemented by time t for firm i and β is the coefficient of interest estimating the average quota effect on treated firms. $\mathbf{X}_{it}$ is the vector for firm-, and country-specific controls, and ε_{it} is an error term. Year-fixed effects λ_t control for aggregate time trends and fluctuations. c_i represents the time-invariant unobserved heterogeneity between firms. Depending on the regression specification, I follow prior literature and control for it by including country- and SIC-2-industry fixed effects, leaving some of the firm heterogeneity in the error term, or include firm-fixed effects and completely absorb the heterogeneity term c_i. Without firm-fixed effects, the control vector includes an additional dummy, *Treatment Firm*$_i$, equal to one if firm i is ever subject to the quota.

Due to the staggered implementation of the quotas, my control group is not restricted to countries that never implement a mandatory quota. In fact, Equation (3.1) can be estimated for a restricted sample of adopting countries only. This approach mitigates concerns about fundamental differences between quota adopting and non-quota adopting countries. It implicitly takes as the control group all firms that do not implement a quota in year t, even if they have implemented or will implement a quota at some point in the past or future (Bertrand and Mullainathan, 2003). Further, I attend to potentially correlated omitted variables and unobservable factors by estimating Equation (3.1) with firm-fixed effects. This approach allows for any correlation between time-invariant firm heterogeneity and my explanatory variables. Also, as opposed to first-differenced estimation, with increasing time periods, fixed effects minimize consistency biases resulting from strict exogeneity violations, such as feedback of the error term on any explanatory variable (Wooldridge, 2009).

Finally, I address three remaining potential problems. First, the political economy of the law, where current and future local economic climate and conditions could influence the adoption of the quota (Bertrand and Mullainathan, 2003). Lu (2019) finds, using a duration model, that board gender policies generally arise from specific political party support, plausibly exogenous to the trend in board gender diversity and firm behavior. Second, economic shocks could hit a quota-adopting country at the implementation date. I conduct a falsification test to check the potential threats to identification stemming from omitted factors, correlated with the distribution of the implementation dates (Altonji et al., 2005). Third, I implement alternative difference-in-differences estimators that relax static and dynamic treatment effect assumptions for varying treatment timings in my robustness checks (Callaway and Sant'Anna, 2021) and in Appendix Tables A.9 and A.10 (Sun and Abraham, 2021, Baker et al., 2022).

As my variable of interest, *Quota*, varies mainly on the country level, it is sensible to consider the data as a clustered sample with country-level unobservables possibly affecting the dependent variable. I need to adjust inference in order to account for two potential sources of correlation across observations: serial correlation across time within the same firm and cluster correlation across firms within the same country (Wooldridge, 2010). If I assume that the cluster effect is time-invariant, regressions estimated with firm-fixed effects eliminate all cluster correlation, and inference needs only be made robust to arbitrary serial correlation captured by firm-level clustered standard errors.[20] Otherwise, country-clustered standard errors that allow for within-country correlation across firms and time periods need to be included.[21] Hence, I draw statistical inferences based on firm-clustered standard errors in all firm-fixed effect estimations with constant slopes, including my main specification and robustness checks. I report the results with country-clustered standard errors in additional regressions.

[20] As my firms do not change countries over time, removing firm effects also removes any time-invariant (spatial) correlation across countries. My country clusters need not be an independent draw from the population of all countries in order to extrapolate my results to other countries around the world.

[21] When cluster effects are left in the error term, good asymptotic properties of cluster-robust inference can only be derived for a large number of clusters and small cluster sizes (White, 2014, Arellano, 1987), or when both the number of groups and group sizes go to infinity (Hansen, 2007, Wooldridge, 2010). I acknowledge that my number of clusters counting 16 countries is small and that my country-clustered standard errors might therefore be inconsistent.

3.4.2 Quota Effect on Firm Performance

In this section, I present the results of the empirical analysis addressing my first hypothesis. I estimate the average quota effect on firm performance as described in Equation (3.1) and report the outcomes in Table 3.3. Panel A reports the main results for my full sample of 16 European countries between 2001 and 2019. In Panels B and C, I test whether my main results for the quota effects on Tobin's Q hold in alternative sample specifications and additional robustness checks. Taken together, the results in Table 3.3 suggest that mandatory board gender quotas decrease market-based firm performance. In a mandatory quota setting, the benefit of improved gender parity seems to come at a cost for the firms.

TABLE 3.3: Quota Effects on Firm Performance

Panel A of this Table reports the results of the impact of the mandatory board gender quota on firm performance in the full sample of 16 European countries between 2001 and 2019. Panel B reports the quota effects on Tobin's Q for alternative sample specifications. Panel C reports the results of the quota effect on Tobin's Q for additional robustness and falsification tests on a restricted sample of quota-adopting countries, excluding all years prior to 2007 and Norway. t statistics are in parentheses; Y: year, C: country, S: SIC2-industry, F: firm; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Panel A: Main Results

	(1) Tobin's Q	(2) Tobin's Q	(3) Tobin's Q	(4) Tobin's Q	(5) Tobin's Q	(6) Return on Assets
Quota	-0.221*** (-4.05)		-0.089* (-1.91)		-0.089*** (-2.80)	-0.209 (-0.78)
Quota × Norway		-0.493*** (-7.90)		-0.313*** (-5.89)		
Quota × Italy		-0.098** (-2.52)		-0.028 (-1.37)		
Quota × France		-0.237*** (-6.72)		-0.089** (-2.30)		
Quota × Germany		-0.033 (-0.51)		0.070 (1.43)		
Quota × Belgium		0.003 (0.06)		-0.004 (-0.08)		
Quota × Austria		-0.276*** (-6.02)		-0.085 (-1.49)		
Quota × Portugal		-0.088** (-2.85)		-0.003 (-0.10)		
Treatment Firm	0.088 (1.40)	0.081 (1.35)				
Log(Total Assets)	-0.016 (-1.60)	-0.016 (-1.62)	-0.318*** (-7.01)	-0.317*** (-6.96)	-0.318*** (-11.61)	1.918*** (9.77)
Leverage	-0.295* (-1.84)	-0.290* (-1.80)	0.065 (0.52)	0.069 (0.55)	0.065 (0.95)	-12.922*** (-9.21)
Firm Age	-0.015*** (-8.28)	-0.016*** (-8.59)	-0.011 (-1.12)	-0.011 (-1.16)	-0.011* (-1.73)	0.089* (1.78)
Profitability	-0.000 (-1.30)	-0.000 (-1.32)	-0.000 (-0.55)	-0.000 (-0.56)	-0.000 (-0.61)	0.002** (2.06)
GDP per Capita	0.000** (2.45)	0.000** (2.87)	0.000*** (4.06)	0.000*** (4.30)	0.000*** (7.24)	0.000 (0.24)
Women Labor Force Rate	-0.089*** (-3.39)	-0.101*** (-4.15)	-0.056** (-2.49)	-0.063** (-2.83)	-0.056*** (-3.37)	-0.760*** (-5.72)
Constant	4.810*** (4.17)	5.304*** (4.81)	5.094*** (4.51)	5.370*** (4.84)	5.094*** (6.92)	26.790*** (4.42)
Economic Magnitude	-14.64		-5.92		-5.92	-13.84
Adjusted R^2	0.08	0.08	0.05	0.05	0.05	0.06
Fixed Effects	Y C S	Y C S	Y F	Y F	Y F	Y F
Standard Error Cluster	C	C	C	C	F	F
N	67624	67624	67624	67624	67624	67106

Panel B: Alternative Samples

	(1) Adopting Countries	(2) Balanced Sample	(3) 2007-2019	(4) 2007-2019 w/o Norway
Quota	-0.142*** (-4.57)	-0.145*** (-4.55)	-0.094*** (-3.03)	-0.060* (-1.89)
Economic Magnitude	-9.44	-9.64	-6.21	-4.01
Adjusted R^2	0.07	0.07	0.06	0.06
Additional Controls	C F	C F	C F	C F
Fixed Effects	Y F	Y F	Y F	Y F
N	29106	19727	19634	17762

Panel C: Robustness Checks

	(1)	(2)	(3)	(4)	(5)
	w/o Implementation Year	Heterogenous Trends	AR (2)	Falsification Test	Agg. ATT
Quota	-0.067* (-1.70)		-0.029* (-1.81)	0.030*** (7.78)	-0.117*** (-3.36)
D.Quota		-0.038* (-1.71)			
lagged Tobin's Q			0.762*** (22.35)		
lagged^2 Tobin's Q			0.083*** (2.68)		
Treatment Firm			0.018 (1.34)		
Economic Magnitude	-4.46	-2.52	-1.94	1.96	-7.74
Adjusted R^2	0.07	0.14	0.72	0.76	/
Additional Controls	C F	C F	C F	C F	F
Fixed Effects	Y F	Y F	Y C S	Y F	No
N	16486	16977	16223	17762	17150

In column (1) of Panel A, I include country-, and industry-fixed effects and cluster my standard errors at the country level. I find a first indication for a decrease in market-based performance for treatment firms subsequent to the quota implementation. My coefficient of interest, *Quota*, is significantly negative at the 1% level, economically meaningful, and equals to a 15% decrease scaled by the treatment firms' mean of Tobin's Q. In column (2), I allow for country-specific quota effects in order to provide some further insights into the heterogeneous nature of the estimated average treatment effect. I observe the strongest effect in Norway, followed by Austria and France. However, the interpretation of the individual quota effects in Austria and Portugal should be handled with caution, as their number of treated firms is limited and their post-quota implementation period is restricted to two years, 2018 and 2019.[22]

Next, I address concerns regarding potentially correlated unobservable factors and estimate all following regressions in this section with firm-fixed effects. In columns (3) to (6), I estimate the staggered difference-in-differences regression by fixed effects and suppress the *Treatment Firm* indicator variable, as there is no within-firm variation of treatment. Columns (3) and (5) show a drop in the effect size to a -0.09 point estimate. After quota implementation, treated firms document a 6% decrease in Tobin's Q, scaled at the treatment firms' mean. When allowing for heterogenous treatment in column (4), I observe similar, but smaller country-specific effects as in column (2) with pooled OLS and country and SIC-2-industry dummies. Because of potential issues with country-cluster robust inference in a setting of few countries, I cluster the standard errors on the firm level in my main specification for the full sample in column (5) and in all

[22] Excluding those countries do not alter my results.

following sensitivity analyses and robustness checks. Consequently, my inferences are robust to time-invariant within-country and any within-firm correlations. The reported effect in column (5) is robust to alternative computations of Tobin's Q, including logarithmic and industry-adjusted measures.

Finally, I investigate the mandatory quota effect on return on assets, measuring accrual-based firm performance. The coefficient of interest in column (6) indicates a negative quota effect on return on assets. However, the estimated treatment effect is only statistically different from zero when estimating Equation (3.1) by pooled OLS and including country and SIC-2-industry dummies. The effect size and significance on return on assets vary considerably across alternative sample specifications.

In Panel B, I assess the robustness of my results and re-run my analyses using four alternative and gradually more restrictive samples: Columns (1) and (2) with a sample of the seven adopting countries as proposed by Bertrand et al. (2004), where (2) is a balanced sample, similar to Chen et al. (2018), that requires a firm to appear at least once in the pre-period and one year in the post-period; Column (3) for a restricted adopting-countries sample from 2007 until 2019, excluding IFRS implementation; And column (4) for a restricted adopting-countries sample from 2007 until 2019 without Norway, the quota pioneering country which seems to have the most important impact on the average Tobin's Q decrease.

My coefficients of interest continue to be negative and economically and statistically significant in all sample specifications. The effect size is highest for all adopting countries and the full time period in columns (1) and (2). I report the smallest quota effect on Tobin's Q in column (4), where I exclude the IFRS period and Norway. Scaled at the treatment firms' mean, Tobin's Q decreases by 4% upon quota implementation. As reported in Appendix Table A.8, the effect size and significance increase slightly when additionally excluding the financial crisis up until 2010.

I perform some regressions addressing potential biases from violating the parallel trend assumptions in Panel C of Table 3.3. Even though my robustness check results are robust to all previously mentioned alternative sample specifications, I report the robustness checks for my most restrictive sample (column (4) in Panel B). In column (1), I follow Chen et al. (2018) and delete each country's implementation year of the quota and find a similar effect size. Next, I allow for violations of the parallel trends assumption and explicitly control for heterogeneous trends by estimating a first-differenced transformation by fixed effects. My results still hold, but suffer an effect size decline.

In column (3) of Panel C, I test the robustness of the negative quota effect when controlling for persistence in the dependent variable and estimate an auto-regressive model of order 2 with lagged Tobin's Q variables. As lagged dependent variable models do not allow for fixed effects estimations, I include country- and industry dummies similar to column (1) of Panel A (Wooldridge, 2010). The model shows a significant, but decreasing persistence of Tobin's Q. Still, the quota effect remains significantly negative.

In column (4), I report the coefficient of interest on a predicted Tobin's Q within the scope of a falsification test (Altonji et al., 2005). I predict Tobin's Q based on GDP per capita, aggregate stock market capitalization, and the rule of law and securities regulation indices from La Porta et al. (1997) and La Porta et al. (2006), plus all the control variables from my main specification in column (5) of Panel A. These factors capture local market conditions that could be correlated with the timing of the quotas' implementation. Next, I use the predicted values from this first stage regression as a dependent variable in my main specification. Under the alternative explanation that local market conditions and forces induce my results, the coefficients should not change (Hail et al., 2014). As the reported estimate is positive, I do not find any indications under the alternative explanation. This result reduces concerns about potential threats to identification stemming from omitted factors, correlated with the distribution of the quota implementation dates.

In column (5) of Panel C, I report the aggregated group-time average treatment effect on the treated (ATT) as proposed by Callaway and Sant'Anna (2021). This causal parameter is simply weighted by group size when aggregated and allows for arbitrary treatment effect heterogeneity and dynamic effects. The negative quota effect on Tobin's Q remains significant and economically meaningful.

3.4.3 Quota Effect on Earnings Management

This section examines the effects of mandatory board gender quotas on one important measure of monitoring quality, earnings management. I estimate Equation (3.1) and insert various measures of earnings management as the dependent variable. I report my results in Table 3.4. Panel A reports the main results of the quota effect on accrual-based earnings management for my sample of 15 European countries between 2007 and 2019, excluding Norway. In Panel B, I test whether my main results hold in alternative sample specifications and additional robustness checks. Panel C reports the quota effects on real earnings management. Taken together, the results in Table 3.4 suggest that mandatory board gender quotas increase earnings management. The quotas' costs for the firms manifest at the first- and second-order.

TABLE 3.4: Quota Effects on Earnings Management

Panel A of this Table reports the results of the impact of the mandatory board gender quota on absolute discretionary accruals in the sample of 15 European countries (excluding Norway) between 2007 and 2019. Panel B reports the quota effects on absolute discretionary accruals, estimated by the performance-adjusted Jones model, for additional robustness and falsification tests on a restricted sample of quota-adopting countries, excluding all years prior to 2007 and Norway. Panel C reports the quota effects on various measures of real earnings management on a restricted sample of quota-adopting countries, excluding all years prior to 2007 and Norway. t statistics are in parentheses; Y: year, C: country, S: SIC2-industry, F: firm; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Panel A: Main Results

	(1)	(2)	(3)	(4)	(5)	(6)
	Abs. DA	Abs. DA	Abs. DA	Abs. DA	Abs. DA (mod. Jones)	Abs. DA (no ROA adj.)
Quota	0.003** (2.67)	0.005*** (3.48)		0.005* (1.88)	0.005* (1.71)	0.005 (1.58)
Quota × Italy			0.006*** (3.90)			
Quota × France			0.005*** (3.06)			
Quota × Germany			0.009*** (4.90)			
Quota × Belgium			0.008*** (4.24)			
Quota × Austria			-0.013*** (-8.83)			
Quota × Portugal			0.010*** (7.96)			
Treatment Firm	-0.006** (-2.95)					
Log(Total Assets)	-0.007*** (-14.74)	-0.000 (-0.04)	-0.000 (-0.04)	-0.000 (-0.04)	0.000 (0.08)	-0.002 (-0.55)
Leverage	0.054*** (7.70)	0.037*** (4.11)	0.037*** (4.14)	0.037** (2.55)	0.039*** (2.65)	0.033** (2.24)
Firm Age	-0.000*** (-3.33)	-0.001 (-1.26)	-0.001 (-1.19)	-0.001* (-1.73)	-0.001 (-1.63)	-0.001** (-2.16)
Profitability	0.000 (0.19)	0.000 (0.40)	0.000 (0.40)	0.000 (0.38)	-0.000 (-0.08)	-0.000 (-0.17)
GDP per Capita	-0.000 (-0.22)	-0.000 (-0.68)	-0.000 (-0.69)	-0.000 (-0.71)	-0.000 (-0.65)	-0.000 (-0.11)
Women Labor Force Rate	-0.003 (-0.80)	0.002 (0.76)	0.002 (0.66)	0.002 (1.02)	0.002 (0.84)	0.002 (1.08)
Constant	0.248* (1.77)	-0.016 (-0.12)	-0.005 (-0.04)	-0.016 (-0.16)	-0.003 (-0.03)	-0.017 (-0.17)
Economic Magnitude	4.56	8.67		8.67	7.87	7.57
Adjusted R^2	0.06	0.00	0.00	0.00	0.00	0.00
Fixed Effects	Y C S	Y F	Y F	Y F	Y F	Y F
Standard Error Cluster	C	C	C	F	F	F
N	33338	33338	33338	33338	33244	33530

Panel B: Sample & Robustness Checks

	(1)	(2)	(3)	(4)	(5)	(6)
	Adopting Countries	w/o Implementation Year	Heterog. Trends	Timing Approach	Falsification Test	Agg. ATT
Quota	0.010** (2.48)	0.010** (2.05)			0.001*** (8.85)	.003 (0.97)
D.Quota			0.004 (0.92)			
Quota x Year -2				-0.000 (-0.03)		
Quota x Year -1				0.003 (0.77)		
Quota x Year +1				0.009*** (2.81)		
Quota x Year +2				0.000 (0.16)		
Quota x Year +3				0.004 (0.84)		
Economic Magnitude	15.88	15.50	6.99	14.99	0.88	5.23
Adjusted R^2	0.00	0.00	0.01	0.00	0.92	/
Additional Controls	C F	C F	C F	C F	C F	F
Fixed Effects	Y F	Y F	Y F	Y F	Y F	No
N	12911	11846	11930	12911	12978	12325

Panel C: Real Earnings Management

	(1)	(2)	(3)	(4)
	Abnormal Disc. Expenses	Abnormal R&D	Abnormal SG&A	Abnormal CF from Operations
Quota	-0.019* (-1.77)	-0.008** (-2.27)	-0.011 (-1.26)	-0.006 (-0.60)
Economic Magnitude	-29.13	-32.91	-26.81	-369.60
Adjusted R^2	0.01	0.02	0.01	-0.00
Additional Controls	C F	C F	C F	CF
Fixed Effects	Y F	Y F	Y F	Y F
N	4755	4755	4755	16510

I find a consistent positive effect of the quota on absolute discretionary accruals in all columns of Panel A. Columns (1) to (3) include standard errors at the country level. Column (1) presents the results for including country- and industry-fixed effects, whereas column (2) reports regression coefficients of the firm-fixed effects estimation. Column (3) reports the country-specific treatment effect. All adopting countries, excluding Austria, show a significant positive quota effect on absolute discretionary accruals estimated by the performance-adjusted Jones model. Their point estimates range from 0.005 to 0.01.

All further results report the quota effect on accrual-based earnings management, with inferences based on firm-level clustered standard errors. My main specification in column (4) captures a quota effect on absolute discretionary accruals estimated by the performance-adjusted Jones model. Columns (5) and (6) report the effects on alternative measures of absolute discretionary accruals. They include the modified-Jones model adjusted by lagged return on assets as described in Kothari et al. (2005) in column (5) and the basic Jones model including a constant in order to make the measure more symmetrical in column (6) (Jones, 1991). The effect sizes captured in columns (4) to (6) correspond to a 8% - 9% increase in absolute discretionary accruals, scaled at the treatment firms' means, upon quota implementation.

In column (1) of Panel B, I report the quota effects for my main specification in a restricted sample of potential quota-adopting countries. The effect size increases to a 0.01 point estimate. The remainder of Panel B of Table 3.4 shows the results of several robustness checks on my main variable, *Abs. DA*. Consistent with the previous section, I conduct these checks for my restrictive sample, where I exclude Norway and all non quota-adopting countries. First, I delete the event year of the quota implementation for each country individually in column (2) and find similar results.

Next, I check whether the results hold when allowing for heterogeneous trends in column (3). Even though the direction of the effect is robust to those relaxed assumption, it is not significant. Therefore, I follow Chen et al. (2018) and test the validity of the parallel trend assumption. I split the coefficient of interest, *Quota*, into five timing variables. Two variables, *Quota x Year -2* and *Quota x Year -1*, for the pre-period and three variables, *Quota x Year+1*, *Quota x Year+2*, and *Quota x Year+3*, for the post period. The results reported in column (4) of Panel B show insignificant coefficients before the quota implementation, and a significant positive coefficient in the year following the implementation.

In column (5), I report the coefficient of interest on the predicted absolute discretionary accruals through the same falsification test as reported in the previous section. The coefficient is significant, but its economic magnitude, scaled by the treatment firms' mean of absolute discretionary accruals, is below 1%. Observable local market conditions and shocks do not seem to drive the effect sizes of my results. In column (6), the direction of the simple aggregated ATT on accrual-based earnings management remains robust, but not significant (Callaway and Sant'Anna, 2021).

So far, I have measured earnings management by investigating discretionary accruals. However, in its duty to monitor management, the board of directors needs to control for various kinds of earnings management. Panel C reports the regression results for real earnings management measures. Due to less data availability for some of the real financial statement figures, my sample sizes vary. Previous restrictions of excluding Norway, all years up to 2007, and all non-quota adopting countries still hold. Consistent with the expectation of upward real earnings management, indicated by unusually low cash flow from operations and discretionary expenses, I observe significant negative quota effects on these measures. Column (1) reports a significant negative quota effect on abnormal discretionary expenses and column (2) reports the negative quota effect on research and development, one of the discretionary expenses.

3.5 Additional Analyses

3.5.1 Potential Channel Identification: Mediating Effects

I have established that firms who are subject to mandatory gender quotas exhibit a decline in firm performance, measured by Tobin's Q, and an increase in earnings management, proxying for one important aspect of monitoring quality. In this section, I attempt to gain further insights into the channels driving the quota-performance effect and determine whether monitoring quality plays a crucial role. While mandated board diversity may impact firm outcomes directly, I examine whether there are specific intervening board-level mechanisms that mediate the impact on firm outcomes.

I follow the traditional product-method approach for mediation analyses introduced by Baron and Kenny (1986) and later popularized for economic analyses by Heckman et al. (2013) and Heckman and Pinto (2015). For continuous mediator and outcome variables, this approach provides results identical to the simulation-based approach developed by Imai et al. (2010). In Figure 3.1, I schematically show how to evaluate the mediating effect by relating the coefficients from three different regression models to each other.

$$y_{it} = \alpha_{it} + \beta \ Quota_{it} + \mathbf{X}_{it}\delta + \lambda_t + c_i + \varepsilon_{it} \tag{3.2}$$

$$y_{it} = \alpha_{it} + \beta' \ Quota_{it} + \tau' \ Mediator_{it} + \mathbf{X}_{it}\delta + \lambda_t + c_i + \varepsilon_{it} \tag{3.3}$$

$$Mediator_{it} = \alpha_{it} + \beta'' \ Quota_{it} + \mathbf{X}_{it}\delta + \lambda_t + c_i + \varepsilon_{it} \tag{3.4}$$

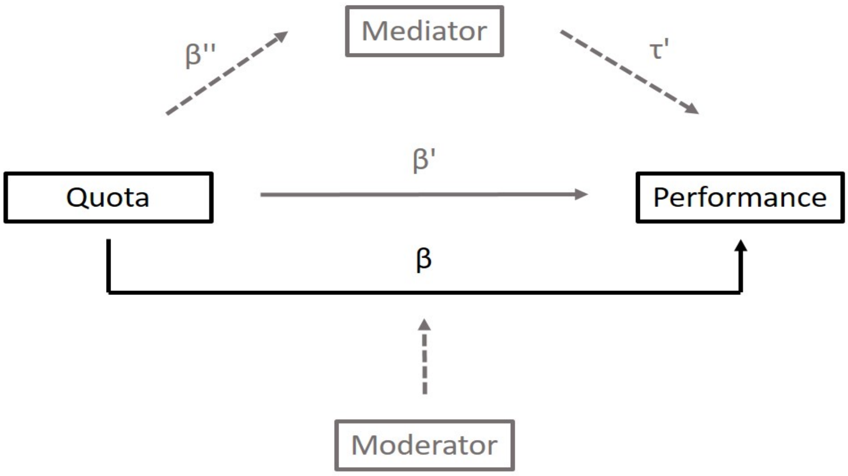


Figure 3.1: Mediating and Moderating Effects

The indirect effect is calculated by taking the product of the quota exposure effect on the mediator β'' (3.4) times the effect of the mediator on the outcome τ' (3.3). Alternatively, it can be determined by subtracting the direct effect β' (3.3) from the total effect β (3.2) if the sample size does not vary in either regression. I apply the Sobel test under normality assumptions to determine the statistical significance of the indirect effect (Sobel, 1982). I report the results of my mediation analyses in Table 3.5.

TABLE 3.5: Mediating Effect of Board Attributes and Earnings Management on Firm Performance

This table reports mediation analyses for the quotas' effect on Tobin's Q. The sample is restricted to the quota-adopting countries, excluding all years prior to 2007 and Norway. For columns (1) to (3), the sample is additionally limited to the firms for which the relevant board-level data is available. t statistics are in parentheses; Y: year, C: country, S: SIC2-industry, F: firm; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

	(1)	(2)	(3)	(4)
	Board Capital Attributes	Board Independence	Board Attributes	abs(Earnings Management)
Quota	-0.129*** (-3.21)	-0.125*** (-3.16)	-0.129*** (-3.24)	-0.070** (-2.11)
Abs(DA)-ROA-adj.Jones				0.059 (0.64)
Share Independent Directors		0.101 (0.79)	0.129 (1.01)	
Director Tenure	0.008 (0.70)		0.008 (0.73)	
Director Age	-0.003 (-0.81)		-0.004 (-0.87)	
Share Foreign Directors	-0.195 (-1.25)		-0.197 (-1.26)	
Board Size	-0.005 (-0.76)		-0.004 (-0.73)	
Share Multidirectors	-0.033 (-0.45)		-0.033 (-0.44)	
Share Women Directors			-0.052 (-0.52)	
% of Total Effect Mediated	-1.55	1.28	-1.73	-0.82
Adjusted R^2	0.08	0.08	0.08	0.06
Additional Controls	C F	C F	C F	CF
Fixed Effects	YF	YF	YF	YF
N	7700	7700	7700	12911

First, following Ahern and Dittmar (2012) and Matsa and Miller (2013), I take a closer look at specific board demographic and relational capital attributes that could have been affected by the quotas and possibly induce my results. The reported capital attributes in column (1) of Table 3.5 do not separately nor jointly significantly mediate the quota effect on firm performance. Taken together, they mediate only -1.55 % of the total effect. Neither does board independence, measured by the share of independent director, reported in column (2) significantly mediate the quota effect on performance. These results are similar to the findings in Matsa and Miller (2013) for Norway, where the direct effect of the quota on performance persists even after controlling for changes in board attributes following the mandate.

Mandated board diversity does not only affect separate attributes, board inputs, but also shifts board dynamics, processes, and resulting board outputs. Striving to generalize my results to other settings of regulatory restrictions on board structures, I control for all available board demographic, relational, and monitoring attributes in column (3), including diversity. I find that none of these attributes separately nor jointly significantly mediate the relationship between the quota and firm performance. These results indicate that fundamental preferences of specific kinds of directors do not seem to drive my results, but rather other channels, such as the potential shift in board dynamics, processes and resulting outcomes. Finally, I include accrual-based earnings management as mediator on the quota-performance relationship in column (4). I do not find a significant mediating effect. The direct effect of the quota on firm performance persists.

3.5.2 Cross-Sectional Analyses: Moderating Effects

In this section, I provide cross-sectional evidence along the two dimensions "ex-ante board input" and "ex-ante board and firm output" to corroborate my main findings of a decrease in firm performance following a shock to the board structure. In line with the *theory of efficient board structures* and following Adams and Ferreira (2009) and their expectation, I expect and find an increased negative quota effect on firm performance for boards exhibiting higher ex-ante monitoring quality. Table 3.6 reports the results of my moderation analyses. In Panel A, I test whether specific board capital and monitoring attributes, the "ex-ante board inputs", moderate the quota effect on firm performance. In Panel B, I report the results of the moderating effects of "ex-ante board and firm outputs", including monitoring quality and firm industry.

Table 3.6: Moderating Effects of Ex-Ante Inputs and Outputs on Firm Performance

This table reports the results of the cross-sectional moderation analyses. Panel A reports the quota effect on Tobin's Q moderated by ex-ante board inputs. The undivided sample corresponds to a sample with available board-level data, quota-adopting countries, and excluding all years prior to 2007 and Norway. Panel B reports the quota effect on Tobin's Q moderated by ex-ante board and firm outputs. The undivided sample corresponds to a sample with available earnings management data, quota-adopting countries, and excluding all years prior to 2007 and Norway. t statistics are in parentheses; Y: year, C: country, S: SIC2-industry, F: firm; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Panel A: Moderating Effect of Board Inputs	(1) Undivided Sample	(2) Low Dir. Age	(3) High Dir. Age	(4) Few Multi-Dir.	(5) Many Multi-Dir.	(6) Many Ind. Dir.	(7) Few Ind. Dir.
Quota	-0.113*** (-2.60)	-0.172*** (-2.64)	-0.021 (-0.42)	-0.160*** (-2.62)	-0.040 (-0.60)	-0.129** (-2.47)	-0.034 (-0.40)
Economic Magnitude	-6.73	-10.24	-1.25	-9.50	-2.36	-7.66	-2.01
Adjusted R^2	0.09	0.11	0.08	0.11	0.09	0.09	0.12
Additional Controls	C F	C F	C F	CF	CF	CF	
Fixed Effects	Y F	Y F	Y F	Y F	Y F	Y F	Y F
N	5973	3005	2968	2913	3060	4516	1457

Panel B: Moderating Effect of Board and Firm Outputs	(1) Undivided Sample	(2) Low Abs. DA	(3) High Abs. DA	(4) Many Analysts	(5) Few Analysts	(6) MCM industries	(7) Other industries
Quota	-0.063* (-1.90)	-0.070** (-2.01)	-0.025 (-0.48)	-0.103* (-1.72)	-0.086* (-1.73)	-0.093*** (-2.64)	-0.005 (-0.07)
Economic Magnitude	-3.75	-4.18	-1.48	-6.15	-5.15	-5.53	-0.28
Adjusted R^2	0.07	0.09	0.07	0.11	0.09	0.09	0.05
Additional Controls	C F	C F	C F	CF	CF	CF	CF
Fixed Effects	Y F	Y F	Y F	Y F	Y F	Y F	Y F
N	10976	5581	5395	4125	5032	6598	4378

First, for additional insight, I compare the quota effects in samples with above- and below-median values of attributes that are expected to change upon quota implementation from previous literature. I choose to divide the samples based on values two years prior to the quota implementation as board inputs already changed to a certain extent in anticipation of the implementation (see Appendix Figure A.1). Columns (2) and (4) of Table 3.6 indicate a stronger quota effect on performance for boards exhibiting lower average director age and fewer multi-directors.

Next, I examine the share of independent directors, a board input often used as proxy for monitoring quality. As expected and reported in columns (6) and (7) of Panel A, boards with more independent directors than the median observation prior to the quota implementation experience a stronger and significant decrease of firm performance compared to the insignificant decrease for boards with fewer independent directors.

In Panel B, I continue to explore the moderating role of monitoring quality proxies. I proxy high monitoring quality by below median absolute accrual-based earnings management in column (2). Next, in columns (4) and (5), I examine the number of analysts following, an external proxy often used for unobservables in monitoring quality. In untabulated results, I check whether this assumption holds true in my setting and find a significant decrease in accrual-based earnings management with a higher number of analysts following. I find an increased quota effect in all samples displaying proxies for higher monitoring quality (columns (2) and (4)).

As a concluding cross-sectional analysis, I compare the quota effect from a sample of "typically male-dominated" industries, including mining, construction, and manufacturing (two-digit SIC codes below 40), to the effects in the undivided sample and in a sample with the remaining industries. Upon quota implementation, I find an economically stronger decrease of 6% in Tobin's Q, scaled at the treatment firm's mean, compared to the 4% decrease in Tobin's Q in the undivided sample in column (1). As expected, the quota effect is larger for industries that have been typically dominated by men in the past and for which the mandated female board representation would probably have been more difficult to achieve.

3.6 Conclusion

This paper examines how mandatory gender quotas in seven European countries affect firm performance and monitoring quality. I argue that this question faces two opposing theories, the *gender-biased captured board theory* and the *theory of efficient board structures*. Although previous studies find a positive effect of voluntarily adopted women directors on monitoring and earnings quality, mandating gender diversity can result in changes of board attributes, dynamics, and processes that go beyond fundamental preferences of women directors.

My results show an average decline in treatment firms' market-based performance upon quota implementation. I conduct several sensitivity analyses to ensure that this main result prevails in light of several competing explanations. Next, I find an economically meaningful increase in absolute accrual-based earnings management and some evidence of upward real earnings management. Both increases in earnings management suggest a decline in monitoring quality. Rather than an impact of changes in monitoring quality, I find an impact of ex-ante monitoring quality levels on the quota-performance relationship. Firms that exhibit indicators for high monitoring quality suffer a more pronounced negative effect of the quota on market-based performance.

In sum, my findings lend support to the *efficient board structures theory*, where imposing binding constraints on the shareholders' board structure choices lead to declines in firm value. This effect is strongest for ex-ante more efficient boards. They also suggest that regulatory restrictions for the board structure have real consequences in terms of unobservable board processes and dynamics and observable board attributes and outputs, such as earnings management. I cannot preclude that alternative unobservable board- and firm-level channels may contribute to my findings. I can, however, minimize the concerns of market- and country-level shocks explaining my findings through a falsification test.

Several caveats apply to my study. First, I focus on market-based firm performance. Whereas Tobin's Q has been a measure often used to capture firm value and firm performance in prior literature, it does not cover all metrics relevant to the success of a firm. Amongst others, it neglects many facets important to stakeholder value.

Second, my results capture observable characteristics that intensify the quota-performance relationship. I observe and measure particular moderating aspects of monitoring quality, including earnings management, board independence, and the number of analysts following. Other proxies for monitoring or even advisory quality are often difficult to measure and go beyond the scope of this study.

Finally, my design does not allow to separately identify the causal effect of gender on board and firm-level outcomes. My empirical analysis cannot determine whether any observed effect is due to the presence of the quota or to differences between men and women as the quotas apply to all women candidates or board members and are therefore confounded with gender (Hughes et al., 2017). Mandated representation of women may change the nature of the competition between candidates and thus have direct effects. For example, it may lower the average competence in the pool of eligible candidates (Chattopadhyay and Duflo, 2004).

That said, the goal of my study was to measure and understand the effects of restricting a specific board attribute through a regulatory mandate on board- and firm-level outcomes. Many countries in Europe and around the world have followed the mandated board diversity trend.

Even more, the understanding of board diversity has gone beyond gender and increasingly includes other diversity attributes, such as ethnicity, sexual orientation and affiliation. My results show first indications to the mechanisms and effects future gender and further inclusive diversity quotas could entail.

Following my mediation results, avenues for future research could address the question as to which extent the quota impact on board- and firm-level outcomes reflects fundamental preferences of women directors and to which extent they reflect changing group dynamics between existing and newly appointed directors. This question is of particular importance as, over time, the presence of additional women on boards could influence the equilibrium behavior of women and the men serving with them (Matsa and Miller, 2013).

4 | Timing is Key: When Does the Market React to Unionization Efforts?

by Bastian Hofmann and Eline Schoonjans[23]

We estimate short-term capital-market effects of unionization efforts at publicly-listed firms and their subsidiaries in the United States between 2011 and 2019. Our short-horizon event study reports significant negative average cumulative abnormal returns at the public announcement of successful union election certifications. These effects are more pronounced with an increasing vote share in support of the union and persist when controlling for election-, firm-, and industry-specific characteristics. Our results suggest that, on average, the market perceives successful unionization as detrimental to future firm performance, expects unionization efforts, and mainly reacts to public and certified outcome announcements. We attribute the timing of the measured effect to the semi-efficient market hypothesis and the increasing trend in anti-union campaigns, where only the certification resolves the uncertainty surrounding the unionization process. Finally, we show that shareholders' expectations regarding unionization efforts differ for firms with *recurrent* and *rare* election cases.

[23] A shorter version of this essay is published in Finance Research Letters (Hofmann and Schoonjans, 2023). My contributions are as follows: development of the research idea and design, literature review, joint empirical analysis, creation of the visualizations, and writing.

4.1 Introduction

Despite a drop in union membership rates, the labor movement's popularity remains high and has slightly increased in the United States in the past decade. The high approval rate may be in part due to increased awareness and media coverage of union and anti-union activities at high-profile companies such as Amazon, Starbucks, and Alphabet. In 2020, 65% of Americans approve of labor unions (Gallup, 2020), whereas only approximately 11% of employees are union members or covered by collective bargaining agreements. Private sector union membership has fallen from 24% to 6% in the last 50 years and is the major driver of the observed overall drop (Macpherson and Hirsch, 2021).

Private employer opposition and employer-favoring labor laws help explain the discrepancy between union approval and membership rates in the United States today (New York Times, 2022b). Employer opposition includes anti-union activities and hiring "union avoidance" law firms that "bust" unionization drives (Lee and Mas, 2012, Kim et al., 2021). Employers' counter-campaigns often result in 15% to 20% drops in union support during elections and are rarely sanctioned as unfair labor practices (New York Times, 2022a). With the end of the Joy Silk doctrine and the introduction of right-to-work laws, union positions are weakened before, during, and after representation efforts. Without Joy Silk, employers can insist on elections without a “good faith doubt” in the union's claim of a majority at the time of *interest showing* (Magner, 2021). In 27 right-to-work law states, union bargaining power is often reduced because employees no longer need to become union members or pay fees for representation. Opposing this trend, U.S. Presidents Obama and Biden have called for laws strengthening rather than weakening labor unions. U.S. Senator Sanders argues that the Protecting the Right to Organize (PRO) Act would "finally give workers a fair chance to win organizing elections" and enforce sanctions against union busting (Wall Street Journal, 2021).

We seek to shed light on shareholders' reactions during these elections in today's institutional setting and determine the timing of short-term capital-market unionization effects at publicly-listed firms and their subsidiaries in the United States between 2011 and 2019. To the best of our knowledge, we are first to empirically investigate immediate capital-market effects around key event dates of the unionization process, *petition filing*, *election agreement*, *election*, and *case closing*. We explore when and how shareholders update their expectations about the potential costs and benefits of unionization. We formulate our first hypothesis bidirectionally and expect a (pronounced) negative or positive capital-market effect of (successful) unionization efforts. Second, based on the prevailing semi-efficient market hypothesis, we expect the effects to materialize rapidly when the new publicly-available information diverges from shareholders' conditional expectations. Third, we expect cross-sectional variations for these conditional expectations, based on previous experience with unionization efforts.

The existing evidence on unionizations' capital-market effects is restricted to mid- to long-horizon event studies with data from the twentieth century. Ruback and Zimmerman (1984) and Lee and Mas (2012) both find significant negative capital-market effects. Their results suggest that, from a value perspective, unions' *monopolistic rent-seeking* behavior outweighs the benefits of their *collective voice* (Krueger, 1974, Freeman, 1976). The empirical evidence does not converge on the timing of these capital-market effects. While Ruback and Zimmerman (1984) only find capital-market effects before and during the *petition filing*, the first step of the unionization process, Lee and Mas (2012) find them on the last step and subsequent months. One short-horizon event study finds positive capital-market effects of successful union decertifications which also materialize at the last step of the legal process, the *closing* of the case (Huth and MacDonald, 1990).

We approach our research question by estimating the daily abnormal returns in five-day event windows around four key steps of the unionization process. First, employees *file* a *petition* for union representation. Second, after the National Labor Relations Board (NLRB) publicly announces the *election agreement*, they vote for or against union representation during the *election*. Third, the NLRB publicly *certifies and closes* the election. Our sample includes 763 union representation cases closed in the United States between 2011 and 2019. Subsequent to our short-horizon event study, we assess variations in the magnitude of the measured effect and account for election-, firm-, and industry-specific characteristics in a multiple regression analysis. Finally, we test the robustness of our main results to alternative sample and event-study parameter specifications.

We find that, on average, there is a significant negative short-term capital-market effect of successful unionization. The average cumulative abnormal return of the five-day *closing* event window is -0.27% and corresponds to average market value losses of $130,000 per firm and $753 per newly unionized worker. This result supports the theory that unions, as monopolies, seek to maximize their rent and that the accompanying real effects on managers' behavior are, from a shareholder's perspective, detrimental to future firm performance. This effect holds in our multiple regression analysis. A clear union win, where we expect increased union bargaining power, leads to a stronger effect. We address concerns about confounding events by arguing that our sample's key event dates are evenly distributed over time because the unionization process occurs discretely at the workplace level and by reducing our event window to two days with robust results.

Further, we validate our second hypothesis on the timing of the observed capital-market effect around the case *closing* date, when the unionization outcome is certain, certified, and made publicly available. Shareholders instantly react to public information and seem to be aware of the uncertainty surrounding the outcome of the unionization process, which only resolves with the certification of the election results at the case *closing*. We also observe a weak reaction to union win and loss cases combined, once the election proceeding is certain and the NLRB publicly announces its investigation result and *election agreement*. We do not find significant shareholder reactions at the *filing* and *election* dates. Typical union-busting campaigns before and vote challenges after the election decrease the possibility to anticipate the final unionization outcome at the time of the *petition filing* and *election*. The number of unlawful employee discharge and employer intimidation charges filed per election has continued to rise through the twenty-first century (Magner, 2021). If the PRO Act were in fact enacted, unfair labor practices might decline, election outcomes might become more predictable, and average short-term capital-market effects might shift towards the first steps of the unionization process.

Consistent with our third hypothesis, we identify previous experience and familiarity with the process of unionization as a major driver of timing differences. We find that shareholders' expectations about potential union elections, regardless of their outcome, differ for firms with multiple (*recurrent*) and first-time (*rare*) elections in a three-year period. Shareholders expect first-time elections less and already react to the *filing of petition*. Shareholders from multiple-election firms seem to be more aware of the uncertainty and the usual drop in employee support during the unionization process. Consequently, only the public announcement of the certified election win result at the *case closing* generates significant short-term capital-market effects for firms with multiple elections in a three-year period.

Our contribution to the literature is threefold. First, we contribute to the existing finance and economics literature by assessing the effects of one particular labor market friction, unionization, in today's institutional setting. For the first time in almost a century, employer-favoring labor laws dominate the legal environment in the US. Specifically, between 2011 and 2019, five additional U.S. states adopted right-to-work laws, leading to more U.S. states with than without these laws. Second, to the best of our knowledge, we are first to determine short-horizon capital market effects of union certification cases with daily stock-market data. We need daily data to test the assumption of (semi-)efficient markets, where the stock price immediately and instantaneously reacts to new (public) information (Lee and Mas, 2012). With our approach of examining these effects around multiple key events, we have fewer concerns for confounding events and we capture gradually incorporated information in the stock price (AitSahlia and Yoon, 2016). Third, we empirically identify experience with unionization processes as an important driver of heterogeneity in shareholders' conditional expectations and resulting capital-market effects. We show that the timing of short-term capital market effects moves from the beginning of the unionization process for first-time election firms to the end of the process for multiple-election firms.

The rest of our paper is organized as follows. Section 4.2 summarizes the relevant institutional and theoretical background. Section 4.3 describes our sample and outlines our research design. In Section 4.4, we present our empirical results and Section 4.5 concludes the paper.

4.2 Hypotheses Development

4.2.1 Unionization Process

Under the United States National Labor Relations Act (NLRA) of 1935, employees are granted the right to elect a union of their choice as their bargaining representative or to form a union of their own. Employees covered by the NLRA must be able to exercise this right of self-organization and collective bargaining representation without interference from their employer. The National Labor Relations Board (NLRB) is the federal agency responsible for the enforcement of the NLRA and the point of contact for employees who seek to unionize (National Labor Relations Board, 2021). Currently, 27 states have enacted right-to-work laws weakening the NLRA and allowing employees that are covered by a negotiated collective bargaining agreement to not pay union dues or become union members. The proposed PRO Act 2021 would overrule right-to-work laws and require all employees represented by a collective bargaining agreement to contribute fees to the labor organization. Additionally, the bill targets increasing managerial opposition to unionization efforts and would allow the NLRB to fine employers up to $50,000 for every violation of labor law (Protecting the Right to Organize Act, 2021). Such managerial opposition includes captive audience meetings, threats to close pants, cut benefits, and discharge workers (Bronfenbrenner, 2009, New York Times, 2021, 2022a).

The legal process of unionization can vary in complexity, but it involves similar key events: *Showing of Interest*, *Filing of Petition*, *Investigation and Election Agreement*, *Election*, and *Certification and Case Closing*.[24] Employees *show* their *interest* with a list of signatures of at least 30% of the workplace in support of unionization.[25] Then, the NLRB officially *files* the *petition* and distributes it to all related parties, i.e. the petitioner, the employer, and the respective union. The *filing of the petition* becomes publicly available through the NLRB website, which especially news outlets can draw upon for further dissemination of the information. The NLRB does not publicly communicate information about the *showing of interest*, such as the number of signatures collected.

Following the *petition filing*, the NLRB *investigates* the case and the involved parties either voluntarily agree to conduct an election or the NLRB directs an *election agreement*. The *agreement* is publicly announced and includes information on the time, location, and eligible voters of the election. The NLRB allows a certain number of uninvolved interested spectators at the *election* as long as the employer permits them to enter the premises. The *election* by secret ballot allows the employees to either vote for or against (one of) the proposed union(s) as their collective bargaining representative. The union wins the *election* if it receives a simple majority, that is, more than 50% of the valid votes. The votes are counted immediately after the voting polls close, but the parties involved can still object to the election result within seven days. Once potential disputes are resolved, the NLRB *certifies* the result, and in case of a majority, the union becomes the elected bargaining representative. If the NLRB certifies a union loss, it does not allow another election at the same workplace for the following twelve months. The *certification* of the election result is disclosed to the public via the NLRB website and *closes* the case.

[24]The steps are explained in detail in the NLRB Outline of Law and Procedure in Representation Cases (National Labor Relations Board, 2017) and the NLRB Casehandling Manual, Part Two, Representation Proceedings (National Labor Relations Board, 2020)

[25]Generally, the employees of a workplace are a group of employees that belong to the same employer subdivision such as a facility, a warehouse, or other appropriate subdivisions of the employer company. The NLRB's definition of an appropriate employer subdivision is intentionally open to further interpretation.

4.2.2 Capital-Market Union Effects

Previous studies find convincing negative capital-market effects of unionization, yet the evidence on the timing of these effects diverges. Ruback and Zimmerman (1984) find a significant average negative effect on stock prices of US-listed firms in the months preceding and including the *petition filing* of union representation cases. They argue that at the time of the *petition*, stock prices incorporate shareholders' expectations about successful and unsuccessful union elections and their resulting real effects. On the other hand, both Lee and Mas (2012) and Huth and MacDonald (1990) find capital-market effects that materialize on and after unionization case *closings*. While similar to Ruback and Zimmerman (1984), Lee and Mas (2012) need to rely on a long-horizon event study because of missing exact *filing*, *election*, and *closing* dates, they only find a significant negative effect on stock prices of U.S.-listed firms after the NLRB *certifies* and *closes* the successful unionization case. They argue that the market does not immediately and instantly adjust to information about future union presence, resulting in long-run capital-market effects. Only with union decertification cases, significant short-term capital-market effects were detected. Successful decertifications resulted in positive capital-market effects on the *closing* dates (Huth and MacDonald, 1990).

In a different setting, Afik et al. (2019) examine stock market reactions to unions' labor strike announcements and find that the highest significant negative effects occur after 15 days of labor strike announcements, where the 15-day period of deliberation following such an announcement is the period with the most uncertainty. Ghaly et al. (2021) find neither a short- nor long-term effect of unionization on stock price fluctuations, size, and variance. Kim et al. (2021) show that unionization lowers stock price crash risk, a benefit which is arguably consistent with previous empirical evidence on management's reactions to unionization, constraining risk-taking, curbing over-investment, and improving transparency. Finally, debt capital markets have been shown to be positively affected by industry unionization, because these industries tend to invest in less risky projects and are less likely to be acquired (Chen et al., 2012).

According to the efficient market hypothesis, shareholders' expectations about the firm's economic prospects and future performance determine the firm's current capital-market value (Modigliani and Miller, 1958). Following the prevailing semi-efficient market hypothesis, a short-term capital-market effect is therefore an immediate and one-time adjustment in the market value of a firm due to new publicly available information about expected future free cash flows

(Fama, 1970). The adjustment is detectable using daily returns on the firm's common shares net from market-wide effects and represents, apart from confirmation effects, the difference between the amount of new public information and the conditional expectations (Ball and Brown, 1968). Consequently, the direction of the adjustment, immediately after some information on unionization becomes publicly available, should reflect whether shareholders view unionization as beneficial or detrimental to firm performance (Fama et al., 1969).

Previous empirical evidence urges us to examine whether the varying timing of significant capital-market effects can be explained by the heterogeneous nature of the investigated samples and whether systematic characteristics potentially drive these timing differences. With recent and daily data on exact *filing*, *election agreement*, *election*, and *closing* dates, we can address two remaining open questions. First, do capital-market effects of union certification cases become apparent in a short horizon? Second, which is the appropriate event on which most of the information about the probability of future unionization is incorporated? We formulate our first hypothesis bidirectionally, where we expect a (pronounced) negative or positive capital-market effect of (successful) unionization efforts. We propose two hypotheses addressing our second research question. Based on the prevailing literature, we expect the effects to be most prominent when the content of new public information and shareholders' expectations diverge most. Finally, we expect varying shareholder expectations, based on systematic characteristics such as experience.

4.3 Data and Research Design

4.3.1 Sample and Data

Our main sample consists of 763 union representation cases closed by the NLRB in the United States between 2011 and 2019, involving U.S. or internationally listed firms and their subsidiaries. We collect complete data for our main sample from four sources. We use data on unionization in the United States from the NLRB, information on corporate ownership from Bureau van Dijk (BvD) Orbis, and stock-market and financial data from the Center for Research on Security Prices (CRSP) and Refinitiv's Thomsen Reuters Datastream. We derive our main sample from 3,111 union representation cases between October 2010 and September 2019 that we are able to match to publicly-listed firms with complete stock-market and financial data. We only include cases with exactly one union election candidate in our sample. Furthermore, similar to previous studies, we set a threshold for the minimum size of the election to at least 50 eligible voters (He et al., 2016, Ghaly et al., 2021).

During our observation period, one listed firm may be affected by several union representation cases, because it may have multiple subsidiaries and operate from different workplaces across the US, where employees can unionize. We follow previous literature and treat these different union representation cases affecting the same firm as separate observations of unionization in our sample (Ruback and Zimmerman, 1984, Lee and Mas, 2012). Exceptionally, we combine elections and treat them as a single observation when two or more cases affecting the same firm share the same respective dates of *filing*, *election*, and *closing* and have the same election result (Ruback and Zimmerman, 1984, Huth and MacDonald, 1990). The 763 union representation cases in our main sample affect 332 distinct listed firms.

There may be additional events along the unionization process that are relevant for the shareholders. Possibly, the collection of signatures as *showing of interest* prior to *filing the petition* may contain relevant information if it reached the shareholders. However, from a semi-strong efficient market perspective such private information should not be fully reflected in the stock price. Contrarily, the market might respond to the investigation and resulting *election agreement* that follows the filing of the petition and is made public by the NLRB. While the NLRB election reports do not specify the date the investigation result became public, we scrape this additional data for a subsample of 606 representation cases via the NLRB website.

We describe every union representation case with election-, firm-, and industry-specific variables. These variables include the number of eligible voters, which is defined as the number of employees at the workplace who are entitled to cast a vote in the election, as well as the number of valid votes cast for and against the union in the election. We match these election-specifics to the number of total employees at the corresponding publicly-listed firm in the election year and obtain the fraction of total employees eligible to vote and the fraction of total employees that actually voted in the election. In addition, based on the vote count reported by the NLRB, we calculate the vote margin as the difference between the vote share for the union and the majority threshold of 50%. To gain insights into the broader circumstances of the elections, we use standard accounting and financial variables, as well as industry union coverages to describe firm- and industry-specific characteristics in the election year. We report definitions of our variables in Appendix Table A.11.

4.3.2 Descriptive Statistics

In Table 4.1, we report descriptive statistics of election-specific variables. Of the 763 union representation cases, 368 result in a union win and 395 result in a union loss. A statistical comparison between the average number of eligible and actual voters in the first two rows shows a higher average voter turnout for union losses on the 1%-significance level, i.e. the fraction of eligible voters that actually cast their vote in the unsuccessful election case is higher. Also, the average fraction of eligible voters (5% significance) and actual voters (1% significance) of the total number of employees of the firm is higher for union losses. As the threshold to construct our main sample of at least 50 eligible voters is relatively low, it is not surprising that across all observations, the average fraction of eligible voters does not exceed 1.35% of the affected firm's total number of employees. The average vote share reveals the clarity of the union wins and losses. With an average vote share of 71.16% for the union, the union wins the election more clearly than union loss cases, with an average of 65.11% votes against the union. Finally, we report the number of days between the unionizations' key events. On average, 54 days separate the *filing* from the *election* date and 32 days lie between the *election* and *closing* of the union representation case. We illustrate the distribution of cases over our sample period in Appendix Figure A.3 and over industries in Appendix Table A.12.

TABLE 4.1: Election Descriptives

This table reports mean values and standard deviations in parentheses of election-specific variables for our main sample of 763 union representation cases across all observations and across all union wins and union losses. We define our variables in Appendix Table A.11.

	Union win	Union loss	All observations
Number of Eligible Voters	172 (221.4)	208.1 (325.8)	190.7 (280.7)
Valid Votes Cast	139.2 (190.9)	188.7 (303.4)	164.8 (256.4)
Fraction of Total Employees Eligible to Vote (%)	1.01 (2.58)	1.67 (4.98)	1.35 (4.02)
Fraction of Total Employees Voting (%)	.83 (2.09)	1.503 (4.47)	1.18 (3.55)
Vote Margin (%)	21.16 (15.37)	-15.11 (9.545)	2.39 (22.13)
Process length (days)	83 (140)	88 (213)	86 (181)
Filing to Election (days)	46 (89)	61 (193)	54 (152)
Election to Closing (days)	36 (101)	28 (63)	32 (84)
N	368	395	763

In addition to election-specific variables, we report descriptive statistics of firm- and industry-specific characteristics for the affected firm in the year of the election in Table 4.2. The average number of total employees in the year of the election is higher at the 1%-significance level for cases that result in a union win. Also, the debt ratio, often discussed in the setting of unionization, is on average higher for union wins than union losses (1% significance). In terms of firm performance, we draw on Return on Assets and Tobin's Q, which on average are slightly higher for union loss cases. Other size criteria, such as market capitalization and total assets do not significantly differ between union loss and union win cases. On average, our sample's firms employ 100,000 employees and are valued at $26 billion.

Table 4.2: Firm- and Industry-Specific Descriptives

This table reports mean values and standard deviations in parentheses of firm-specific financial and accounting variables and an industry-specific variable in the year of the election for our main sample of 763 union representation cases across all observations as well as union wins and union loss cases. We define our variables in Appendix Table A.11.

	Union win	Union loss	All observations
Return on Assets (%)	4.56 (5.26)	5.51 (6.29)	5.05 (5.83)
Tobin's Q	1.16 (.549)	1.33 (.752)	1.25 (.667)
Debt ratio	.713 (.154)	.67 (.197)	.69 (.179)
Market Capitalization (Million USD)	24,614 (61,931)	27,135 (55,976)	25,919 (58,898)
Size: ln(Total Assets)	16.3 (1.61)	16.4 (1.82)	16.3 (1.72)
Total Employees	114,685 (142,318)	83,486 (123,931)	98,534 (133,940)
Industry Union Coverage (%)	14.7 (13.2)	12.8 (9.5)	13.7 (11.5)
N	368	395	763

4.3.3 Short-Horizon Event Study

To estimate the average short-term capital-market effect of unionization at listed firms, we conduct a short-horizon event study. We estimate the effect of unionization on the market value of the affected firms by calculating daily abnormal returns in a short period of five days around the event date, the event window. The event date is the date on which new information, potentially indicative of future firm performance, reaches the market (Binder, 1998). Our data allows us to consider the date of *petition filing*, the date of *election agreement*, the date of *election*, and the date of *case closing* as possible and separate event dates in the short-horizon event study, which represents the 'cleanest evidence we have on efficiency' (Fama, 1991, Kothari and Warner, 2007). Events affecting the same firm share the same corresponding security, but if they have different event dates, they are treated as separate observations.

Following standard short-horizon event study methodology as described by MacKinlay (1997), we calculate the abnormal returns by subtracting the corresponding security's actual daily returns in the event window from the expected daily returns for the event window under normal market conditions in absence of the event i.e., normal performance. First, we estimate the normal performance of the corresponding security for each event $i = 1, ..., N$. Following the approach of Brown and Warner (1980), we rely on the market model as a well-specified model to assess daily return data.

$$R_{it} = \alpha_i + \beta_i R_{mt} + \varepsilon_{it} \tag{4.1}$$

where R_{it} is the return of the security of event i in period t and R_{mt} is the return of the market portfolio in period t. The parameter α_i measures the part of the return that is unrelated to market movements. The term $\beta_i R_{mt}$ measures the part of the security's return that depends on market-wide factors. The error term ε_{it} with $E(\varepsilon_{it}) = 0$ and $Var(\varepsilon_{it}) = \varsigma_{it}^2$ measures the part of the return that cannot be explained by either overall market movements or the part that is unrelated to the market return. We estimate $\hat{\alpha}_i$ and $\hat{\beta}_i$ for the $i = 1, ..., N$ events in our sample using daily security return data and the CRSP value-weighted market index as a reference market in an estimation window prior to the event window. With a sufficient gap between estimation and event window, we reduce the chance of unwanted influence of event-related information on the estimation parameters (MacKinlay, 1997).

The event time τ is expressed as trading days relative to the event date. We define the event date as $\tau = 0$. We define the event window as τ_1 to τ_2 .The length of the event window $[T_1; T_2]$ is $T = \tau_1 + \tau_2 + 1$. In our short-horizon event study, we follow the prevailing literature and apply an event window of five days ($T = 5$) surrounding the event date, which we denote as [-2; +2]. We define the estimation window as $\tau = S_1$ to $\tau = S_2$. For each of the $i = 1, ..., N$ events in our sample, we thus use a separate estimation window $[S_1; S_2]$ to estimate the parameters $\hat{\alpha}_i$ and $\hat{\beta}_i$. We apply an estimation window of 200 trading days prior to the event window with a gap of 200 trading days in between the two windows.[26]

From our estimated parameters $\hat{\alpha}_i$ and $\hat{\beta}_i$, we calculate the abnormal return (AR) for event i on event day τ.

$$AR_{i\tau} = R_{it} - \hat{\alpha}_i - \hat{\beta}_i R_{mt} \tag{4.2}$$

[26] We conduct our event study in SAS and base our code on the event study macro by Glushkov and WRDS (2012), which is available at: https://wrds-www.wharton.upenn.edu/pages/support/research-wrds/macros/wrds-macros-evtstudy/. Our code includes modifications to the WRDS code made by (Chen, 2015), which are publicly available at: http://kaichen.work/?p=418.

Next, we aggregate AR over the time of the event window from τ_1 to τ_2, where $\tau_1 = T_1$ and $T_1 \leq \tau_2 \leq T_2$, into a cumulative abnormal return (CAR). In accordance with our goal to measure the average short-term capital-market effect of unionization, the AR are aggregated over the total number of events N and averaged to obtain a one-day average abnormal return (AAR) for event day τ. Respectively, CAR can be aggregated over the total number of events N and averaged to compute an average cumulative abnormal return ($ACAR$) of the event window.

Given the previously established relationship between new publicly-available information and short-term capital-market effects, a positive or negative sign on the $ACAR$ indicates whether shareholders perceive the information on the event day as positive or negative, respectively. We test the null hypothesis of no significant short-term capital-market effect of unionization using the standardized cross-sectional t-statistic proposed by Boehmer et al. (1991).

4.3.4 Regression Analysis

To analyze the determinants and to better understand the magnitude of the measured abnormal returns, we consider firm-, industry-, and election-specific characteristics for the year of the election in a multiple regression analysis, as suggested in MacKinlay (1997). We specify our regression model as follows

$$CAR_i(\tau_1, \tau_2) = \beta_0 + \sum_{k=1}^{M} \beta_k \ VAR_{ik} + \varepsilon_i \tag{4.3}$$

where the dependent variable $CAR_i(\tau_1, \tau_2)$ is the cumulative abnormal return for event $i = 1, ..., N$ at the end of the [-2; +2] event window. β_0 is the intercept of the regression, β_k is the coefficient for the independent variable VAR_{ik} with $k = 1, ..., M$, and ε_i is the error term. We compute heteroskedasticity-consistent standard errors (White, 1980).

In addition to the previously described election-, firm-, and industry-specific variables from Tables 4.1 and 4.2, we control for the affected firm's exposure to systematic risk, proxied by the $\hat{\beta}_i$ estimated in the estimation window prior to the event. We further control for potential differences in the magnitude of the CAR for internationally listed firms. We also control for the degree to which shareholders are informed, i.e. through analysts that cover the firm and gather relevant information about future firm performance (Healy and Palepu, 2001). Ağca and

Mozumdar (2008) argue that higher analyst followings reduce capital-market imperfections. We use the inclusion in the Standard and Poor's 500 index (S&P 500) as a proxy for high analyst coverage, since firms that are S&P 500 constituents are more likely to be covered by a high number of analysts (Yu, 2008). Following the approach of Ruback and Zimmerman (1984), we control for the three most frequent industries in our sample. Finally, similar to other studies, we control for union bargaining power by using information about whether the election took place in a U.S. state with a right-to-work law (e.g. Cheng, 2017). We report the definitions of the entire set of independent variables in Appendix Table A.11.

4.4 Empirical Results

4.4.1 Timing of Short-Term Capital-Market Effects

In Table 4.3, we present the average (cumulative) abnormal returns of our main sample around the key events of the unionization process: *filing*, *election agreement*, *election*, and *closing*. We validate our first hypothesis and find a negative average cumulative capital-market effect of successful unionization at the time the NLRB publicly announces the *election agreement* and when the NLRB certifies and *closes* a successful union representation case. Regarding the timing of the capital-market effect, we validate our second hypothesis and find that most divergence between new public information and shareholders' expectation is when the uncertainty surrounding the legal process of unionization and its outcome is resolved.

Table 4.3: Main Short-Horizon Event-Study Results

This table reports the abnormal returns of our main analysis on the key event dates of the 763 union representation cases. Panel A and B present the abnormal returns for the *filing* and *agreement* date, which we compute for all observations, as well as for union win and union loss cases. Panel C and D present the abnormal returns for the *election* and *closing* dates, respectively, which we compute for union win and union loss cases. Abnormal returns appear in percentages (%) with the Boehmer et al. (1991) t-statistic in parentheses. The asterisks *, **, and *** denote two-tailed statistical significance at the 0.10, 0.05, and 0.01 level, respectively.

	Union win		Union loss		All observations	
Panel A: Filing Date						
Event time	AAR	ACAR	AAR	ACAR	AAR	ACAR
-2	.243*	.243*	-.045	-.045	.094	.094
	(1.91)	(1.91)	(-.64)	(-.64)	(1.09)	(1.09)
-1	.011	.254	.072	.026	.043	.136
	(-.88)	(.60)	(.61)	(.04)	(-.29)	(.49)
0	-.095	.159	-.046	-.019	-.070	.066
	(-.93)	(-.04)	(-.57)	(-.31)	(-1.09)	(-.22)
1	-.036	.123	-.195**	-.214	-.118	-.052
	(-.37)	(-.26)	(-2.09)	(-1.43)	(-1.52)	(-1.07)
2	-.194	-.071	.067	-.147	-.059	-.111
	(-1.57)	(-0.90)	(0.96)	(-.79)	(-.56)	(-1.19)
N	368		395		763	
Panel B: Agreement Date						
Event time	AAR	ACAR	AAR	ACAR	AAR	ACAR
-2	-.118	-.118	.078	.078	-.014	-.014
	(-1.64)	(-1.64)	(.16)	(.16)	(-.92)	(-.92)
-1	.011	-.107	-.078	.0005	-.036	-.050
	(.37)	(-.95)	(-1.29)	(-.77)	(-.78)	(-1.19)
0	.110	.003	-.119	-.119	-.011	-.062
	(1.07)	(-.20)	(-1.44)	(-1.43)	(-.35)	(-1.22)
1	-.180	-.177	.065	-.054	-.050	-.112
	(-1.08)	(-.78)	(.78)	(-.99)	(-.35)	(-1.25)
2	-.200**	-.377	-.003	-.057	-.096**	-.208*
	(-2.06)	(-1.59)	(-.62)	(-1.12)	(-1.98)	(-1.91)
N	285		321		606	

	Union win		Union loss	
Panel C: Election Date				
Event time	AAR	ACAR	AAR	ACAR
-2	.070	.070	.099	.099
	(1.04)	(1.04)	(1.02)	(1.02)
-1	.063	.133	.012	.112
	(.25)	(.88)	(-.24)	(.57)
0	.126	.259	-.161	-.049
	(1.41)	(1.50)	(-1.08)	(-.56)
1	-.033	.226	.027	-.022
	(-.07)	(1.32)	(.73)	(-.25)
2	.014	.240	.083	.061
	(.19)	(1.25)	(1.22)	(.35)
N	368		395	
Panel D: Closing Date				
Event time	AAR	ACAR	AAR	ACAR
-2	-.159*	-.159*	.100	.100
	(-1.94)	(-1.94)	(.71)	(.71)
-1	.024	-.136	-.068	.032
	(.35)	(-1.27)	(-.56)	(.11)
0	-.004	-.140	.065	.097
	(-.77)	(-1.47)	(.14)	(.18)
1	-.108*	-.248**	-.044	.053
	(-1.81)	(-2.37)	(-1.16)	(-.39)
2	-.022	-.270**	.012	.065
	(-.63)	(-2.44)	(.51)	(-.13)
N	368		395	

Panel A of Table 4.3 shows the abnormal returns for the *filing* date. We distinguish between union wins, union losses, and the full set of observations because the outcome of the election is not yet known at the time of *petition filing*. The *ACAR* at the end of the event window for all observations is -0.111%. The *ACAR* at the end of the event window is -0.071% for the union win cases and with -0.147% approximately twice as large for the union loss cases. The stronger negative coefficient for union losses can be explained by the fact that a *petition filing* is even more surprising in workplaces with lower interest in unionization. With t-statistics of -1.19, -0.90, and

-0.79, respectively, neither of these abnormal returns are statistically significantly different from zero. The results in Panel A of Table 4.3 suggest that shareholders are not surprised by the *petition filing*, probably anticipating unionization efforts, and that the *petition filing* does not contain relevant or sufficient information about the election outcome and potential effects on future firm performance. We cannot validate our first hypothesis based on the *filing* date as relevant event date. Regarding our second hypothesis, we can conclude that the new amount of information by the *filing of the petition* does not diverge from the conditional expectations of the average shareholder.

In Panel B, we apply the same research design of our short-horizon event study to a reduced sample of 285 union wins and 321 union losses. As event date, we define the day on which the investigation result becomes public and shareholders are informed about the upcoming election through the *election agreement*. Panel B reports negative, but insignificant, $ACARs$ for both union wins and union losses at the end of the event window. The $ACAR$ at the end of the event window for all observations is -0.208% and with a t-statistic of -1.91 statistically significant at the 10% level. These results indicate that the NLRB's confirmation of a forthcoming election represents new information that diverges from the shareholders' expectations and resolves uncertainty. Typical union-busting campaigns decrease the possibility to anticipate whether a union election will even take place. At the time of the publicly-announced election agreement, after the NLRB's investigation, shareholders can better anticipate the election and its outcome and react accordingly.

Panel C of Table 4.3 shows the abnormal returns around the *election*. The *election* contains further information about the likelihood of successful or unsuccessful unionization, since it is the day when the votes are cast and counted. Accordingly, we discuss the results for union wins and union losses. The $ACAR$ at the end of the event window for the union win cases is 0.240%. The $ACAR$ at the end of the event window for the union loss cases is 0.061% which is approximately one-fourth in comparison to the union win cases. With t-statistics of 1.25 and 0.35, respectively, these $ACAR$ are not significantly different from zero. The positive coefficients point to a reversal of the effects after the first announcement observed in similar event studies (Barakat et al., 2014). The amount of new information from the *election*, which is not made publicly available by the NLRB, does not induce significant capital-market effects. This finding could support the semi-efficient market hypothesis about private versus public information or indicate that the uncertainty surrounding the legal process of unionization and its outcome does not seem to be resolved at the time of the *election*. In our sample, less than one-third of the representation cases are closed within the 7-day objection window and 15% did not close within one month after the election. The longer the period between the *election* and the *closing*, the higher the probability of valid objections and vote challenges from either side (Roomkin and Block, 1981).

Panel D of Table 4.3 shows the abnormal returns for the *closing* date. The certification of the election result on the *closing* date is the last step in the unionization process. First, we find an insignificant $ACAR$ of 0.065% at the end of the event window for union loss cases. Second, we find the $ACAR$ at the end of the event window for the union win cases at -0.270% with a t-statistic of -2.44 and a statistical significance of 5%. Our results regarding the union win cases at the *closing* date support our first hypothesis for a short-term capital-market effect of unionization affecting publicly-listed firms or their subsidiaries. The negative sign further indicates that shareholders perceive the information about a certified election result in favor of the union as negative information regarding future firm performance. To gauge economic magnitudes, we follow Lee and Mas (2012) and calculate that a 0.270% negative abnormal return corresponds to a market value loss of $130,000 per firm.[27] We approximate the size of the collective bargaining unit by the number of eligible voters and find a lost market value of $753 per newly unionized worker.

Our results imply that assessing shareholder reactions to unionization does not necessarily require examining capital-market effects over months, as the effect is also detectable in the short-term using daily return data and accurate dates of the key unionization events. Our results are in line with Huth and MacDonald (1990) and Lee and Mas (2012) who find significant capital-market effects around the *closing* date, but stand in contrast to the long-horizon study results of Ruback and Zimmerman (1984). They find significant negative abnormal returns in the *filing* month which are highest for union wins. They infer that the market anticipates the election outcome. Election outcomes might not have been as uncertain in the 60s and 70s, the sample period Ruback and Zimmerman (1984) examines, as they are today. First, union-busting activities have become more common and professionalized, where consulting firms provide seminars on union avoidance. Second, objections, allegations, and complaints per election have increased every decade since the Joy Silk doctrine has been abandoned in 1969 (Magner, 2021). A parallel

[27] The market value loss per firm corresponds to each union win firm's market value multiplied by its cumulative abnormal return.

stream of literature reasons that the *closing* date is appropriate to capture market reactions of union elections in the twenty-first century (Ertugrul and Marciukaityte, 2021).

4.4.2 Experience Heterogeneity in Short-Term Capital-Market Effects

Various factors might influence shareholders' conditional expectations at the time of our key event dates. Expectations about upcoming union elections and their outcomes depend on previous cases in the firm, the industry, and the time period. For a firm that experiences the unionization process for the first time, information about possible upcoming elections at the *filing* date might already diverge from shareholders' conditional expectations, meaning that they did not expect unionization efforts regardless of their outcome. Furthermore, the expectations regarding the outcome of the union election depend on union victory rates of the firm, the industry, and the time period. The percentage of union victories has varied substantially in the 85 years since the NLRB was founded and started conducting union elections. While unions won 80% of their elections in the 1940s, between 1970 and 2000 workers were losing around half of these elections (Mishel et al., 2020). In the last decade, union victories have increased again to 65%. Representation cases of listed firms and their subsidiaries systematically have lower victory rates than the complete sample of representation cases. While our main sample reports nearly 50% of union victories, Lee and Mas (2012) report only approximately 30% of union victories between 1961 and 1999.

We test our hypothesis about shareholders' diverging expectations and whether they depend on how common unionization is for the firm and divide our sample into two subsamples. We follow Bradley et al. (2017) and compute a subsample of *rare cases*, including unique-election firms and every first unionization case that occurs in a three-year period for firms that are affected by multiple elections. The second subsample of *recurrent cases* includes all remaining unionization cases of firms that are affected by multiple elections. We apply the parameters of our short-horizon event study to these subsamples.

In Table 4.4, we report a shift in the timing of the negative short-term capital-market effects. Panel A and B present a significant negative $ACAR$ at the end of the event windows of the *filing* and *agreement* date for all *rare case* observations. Together these event dates convey the information that an election is certain. Our results indicate that shareholders did not anticipate unionization efforts, i.e. they did not expect sufficient interest or the need for a union election.

Shareholders do not further react at the two subsequent events, *union election* and *case closing*, as reported in Panels C and D of Table 4.4.

When examining union win and union loss cases separately, we find similar effects as in our main analysis. For the *filing* date, the negative capital-market effects are more pronounced for union losses and for the *agreement* date, they are more pronounced for union wins. We interpret these results in two ways. First, per definition, union losses occur in settings with lower interest in unionization. In firms where the interest for unions is low, the fact that a *petition* is *filed* is even more surprising. Second, shareholders do not seem to be able to anticipate the election outcome at the *filing* date but get closer at the *agreement* date. The NLRB's investigation provides new and public information for the shareholders.

Table 4.4: Short-Horizon Event-Study Results of Rare Cases

This table shows the abnormal returns on the key event dates of first-time union representation cases. This subsample includes all unique-election firms and every first case that occurs in a three-year period for firms that are affected by multiple elections. Abnormal returns appear in percentages (%) with the Boehmer et al. (1991) t-statistic in parentheses. The asterisks *, **, and *** denote two-tailed statistical significance at the 0.10, 0.05, and 0.01 level, respectively.

	Union win		Union loss		All observations	
Panel A: Filing Date						
Event time	AAR	ACAR	AAR	ACAR	AAR	ACAR
-2	.276	.276	.074	.074	.152*	.152*
	(1.58)	(1.58)	(.94)	(.94)	(1.79)	(1.79)
-1	.114	.390	-.010	.065	.038	.190
	(-.05)	(.99)	(-.56)	(.18)	(-.45)	(.83)
0	-.203	.187	-.109	-.044	-.145*	.045
	(-1.60)	(.01)	(-1.13)	(-.55)	(-1.91)	(-.38)
1	-.047	.140	-.257***	-.302*	-.176**	-.131
	(-.40)	(-.16)	(-2.63)	(-1.83)	(-2.28)	(-1.35)
2	-.261*	-.121	-.006	-.307	-.104	-.235*
	(-1.67)	(-.82)	(-.17)	(-1.63)	(-1.26)	(-1.73)
N	146		232		378	
Panel B: Agreement Date						
Event time	AAR	ACAR	AAR	ACAR	AAR	ACAR
-2	-.118*	-.118*	.060	.060	-.008	-.008
	(-1.83)	(-1.83)	(-.45)	(-.45)	(-1.33)	(-1.33)
-1	-.068	-.187	-.248**	-.188*	-.180*	-.188**
	(-.08)	(-1.24)	(-2.09)	(-1.76)	(-1.76)	(-2.15)
0	.121	-.065	-.148	-.336	-.045	-.233**
	(.92)	(-.61)	(-.66)	(-2.06)	(-.00)	(-2.03)
1	-.472	-.537	.144*	-.192	-.090	-.323*
	(-.70)	(-1.00)	(1.68)	(-1.32)	(.39)	(-1.66)
2	-.258*	-.796	.096	-.096	-.039	-.362*
	(-1.76)	(-1.61)	(.179)	(-1.09)	(-1.06)	(-1.86)
N	111		181		292	

	Union win		Union loss	
Panel C: Election Date				
Event time	AAR	ACAR	AAR	ACAR
-2	-.039	-.039	.138	.138
	(.19)	(.19)	(1.30)	(1.30)
-1	.230	.191	-.102	.035
	(1.02)	(.92)	(-.88)	(.33)
0	.277	.468	-.127	-.091
	(1.05)	(1.35)	(-.82)	(-.55)
1	-.089	.379	-.072	-.163
	(-.12)	(1.16)	(-.36)	(-.74)
2	-.262	.117	.068	-.096
	(-1.05)	(.60)	(.75)	(-.42)
N	146		232	
Panel D: Closing Date				
Event time	AAR	ACAR	AAR	ACAR
-2	-.291*	-.291*	.132	.132
	(-1.95)	(-1.95)	(1.20)	(1.20)
-1	.091	-.200	-.060	.072
	(.57)	(-1.27)	(-.24)	(.69)
0	.162	-.038	-.052	.020
	(.78)	(-.71)	(-.39)	(.37)
1	-.139	-.178	-.048	-.028
	(-1.46)	(-1.37)	(-1.01)	(-.21)
2	.148	-.030	.012	-.016
	(1.10)	(-.79)	(.16)	(-.13)
N	146		232	

For the subsample of *recurrent cases* in Table 4.5, we find that shareholders, on average, react negatively to the *closing* of successful certification cases. We interpret this result, which is consistent with our main analysis, along the lines of shareholders' experience and updated expectations. First, the characteristics of our *recurrent* subsample point to an average increased union interest in the firm, where shareholders to some degree expect union elections. The subsample is characterized by higher union victory rates and by election experience. The new information of a petition *filing* and the public announcement of the *election agreement* do not seem to diverge from the shareholders' conditional expectations. In Panels A and B, we find no significant capital-market effects.

The *election* itself in Panel C does not induce short-term capital-market reactions. "Experienced" shareholders perceive unionization as undecided matter until the results of the election are eventually certified, i.e. when the uncertainty surrounding the legal process of unionization and its outcome is resolved. We find an insignificant $ACAR$ of 0.177% at the end of the *closing* event window for union loss cases in Panel D. We find the $ACAR$ at the end of the event window for the union win cases at -0.372% with a t-statistic of -2.43 and a statistical significance of 5%. Even though the election outcome might be anticipated before and known after the election, shareholders possibly weigh in the high amount of unlawful employee discharge and employer intimidation charges. In the three decades before our sample period, 3.2 of such charges were filed per election, resulting in possible complaints and reelections (Magner, 2021). Our results remain qualitatively similar if we divide our sample into *unique* cases with unique-election firms and *recurrent* cases with firms that are affected by multiple elections.

TABLE 4.5: Short-Horizon Event-Study Results of Recurrent Cases

This table shows the abnormal returns on the key event dates of recurrent union representation cases. This subsample includes all remaining cases of firms that are affected by multiple elections. Abnormal returns appear in percentages (%) with the Boehmer et al. (1991) *t*-statistic in parentheses. The asterisks *, **, and *** denote two-tailed statistical significance at the 0.10, 0.05, and 0.01 level, respectively.

	Union win		Union loss		All observations	
Panel A: Filing Date						
Event time	AAR	ACAR	AAR	ACAR	AAR	ACAR
-2	.221	.221	-.216**	-.216**	.036	.036
	(1.16)	(1.16)	(-2.20)	(-2.20)	(-.24)	(-.24)
-1	-.057	.164	.188*	-.028	.047	.083
	(-1.06)	(-.06)	(1.69)	(-.20)	(.01)	(-.15)
0	-.024	.140	.044	.016	.005	.088
	(-.02)	(-.06)	(.66)	(.20)	(.30)	(.06)
1	-.029	.111	-.106	-.090	-.061	.026
	(-.22)	(-.20)	(-.29)	(-.02)	(-.34)	(-.18)
2	-.150	-.039	.170*	.081	-.014	.012
	(-.66)	(-.47)	(1.77)	(.74)	(.46)	(.03)
N	222		163		385	
Panel B: Agreement Date						
Event time	AAR	ACAR	AAR	ACAR	AAR	ACAR
-2	-.118	-.118	.102	.102	-.020	-.020
	(-.73)	(-.73)	(.80)	(.80)	(.01)	(.01)
-1	.061	-.057	.143	.245	.098	.078
	(.63)	(-.18)	(.84)	(1.15)	(1.05)	(.67)
0	.103	.046	-.083	.162	.020	.098
	(.67)	(.24)	(-1.37)	(.15)	(-.46)	(.28)
1	.006	.053	-.037	.125	-.013	.085
	(-.83)	(-.21)	(-.36)	(-.04)	(-.86)	(-.18)
2	-.162	-.110	-.132	-.007	-.149*	-.064
	(-1.25)	(-.75)	(-1.24)	(-.45)	(-1.72)	(-.86)
N	174		140		314	

	Union win		Union loss	
Panel C: Election Date				
Event time	AAR	ACAR	AAR	ACAR
-2	.142	.142	.044	.044
	(1.15)	(1.15)	(-.13)	(-.13)
-1	-.047	.095	.176	.220
	(-.57)	(.37)	(.85)	(.50)
0	.027	.122	-.210	.010
	(.94)	(.80)	(-.98)	(-.14)
1	.004	.126	.168*	.178
	(.02)	(.73)	(1.83)	(.80)
2	.194	.320	.105	.283
	(.93)	(1.12)	(.95)	(1.24)
N	222		163	
Panel D: Closing Date				
Event time	AAR	ACAR	AAR	ACAR
-2	-.164	-.164	.065	.065
	(-1.49)	(-1.49)	(.17)	(.17)
-1	.048	-.116	-.043	.022
	(.24)	(-1.00)	(-.30)	(-.09)
0	-.108	-.224*	.202	.224
	(-1.45)	(-1.66)	(.53)	(.22)
1	-.034	-.258**	-.080	.144
	(-.79)	(-2.09)	(-1.02)	(-.23)
2	-.115	-.372**	.033	.177
	(-1.45)	(-2.43)	(.83)	(.17)
N	222		163	

4.4.3 Robustness and Sensitivity Analyses

We test our main results' sensitivity to a multiple regression analysis and present our findings in Table 4.6. Here, we analyze our main results along election-specific characteristics and control for firm- and industry-specific variables in different specifications of our regression model (Equation 4.3). Consistent with our main results, we find a significant negative relation between the indicator variable *Union Win* and the *CAR* at the end of the *closing* event date window in all four specifications. In Column (1), ceteris paribus, *Union Win* results in an average *CAR* decrease of 1.1 percentage points. Consistent with Panel B of Table 4.3, we find limited evidence for a significant negative relation between the indicator variable *Union Win* and the average *CAR* at the end of the *agreement* event window in Appendix Table A.13. As expected from Panels A and C, we find no comparable significant relationship for the *filing* and *election* dates and do not report those results. Our regression results remain unchanged if we control for aggregate time trends by including year-fixed effects.

Table 4.6: Short-Horizon Regression Results: Closing Date

This table shows the regression results for the *CAR* at the end of the *closing* event window, controlling for election-, firm-, and industry-specific variables in four specifications of our regression model. We report the definitions of the variables in Appendix Table A.11. The t-statistics are in parentheses. The asterisks *, **, and *** denote two-tailed statistical significance at the 0.10, 0.05, and 0.01 level, respectively.

	(1)	(2)	(3)	(4)
Panel: Closing Date				
Union Win	-.011**	-.005**	-.011**	-.011**
	(-2.46)	(-2.06)	(-2.41)	(-2.32)
Vote Margin	.049***		.049***	.049***
	(2.95)		(2.93)	(2.98)
Union Win × Vote Margin	-.057***		-.056***	-.053***
	(-2.71)		(-2.69)	(-2.61)
Fraction Voting	.004		.005	-.010
	(.14)		(.18)	(-.42)
Right-to-Work	-.002		-.002	-.001
	(-.71)		(-.73)	(-.60)
Constant	-.001	-.006	-.005	.010***
	(-.04)	(-.35)	(-.28)	(2.75)
Firm-Level Controls	Yes	Yes	Yes	No
Industry-Level Controls	Yes	Yes	No	Yes
N	763	763	763	763

The vote margin for the union plays a significant role for the capital-market effects of both successful and unsuccessful unionization cases. With increasing vote margin, the cumulative abnormal returns increase for union losses and decrease for union wins. A clear union win, measured by a more positive vote margin, is associated with an on average more pronounced negative short-term capital market effect of successful unionization. One could argue that shareholders expect the union to have a stronger bargaining position with intensified support, resulting in stronger negative real effects, such as wage premiums. The most straightforward explanation for an on average more positive short-term capital-market effect of unsuccessful unionization with more employees voting for the union is the increased uncertainty about the outcome before and during the election, only resolving at the *closing* date. In untabulated results, we show that our effects hold for close-call union elections with 10% divergence from the majority.

Finally, we tackle some remaining issues in robustness and sensitivity tests. One potential obstacle to the validity of event studies is the issue of confounding events. A confounding event might take place around or during the same time as the event of interest. It therefore influences the measured abnormal returns in the event window and distorts their attributability. The importance of controlling for confounding events increases with the length of the applied event window and thus the risk of including confounding events (McWilliams and Siegel, 1997). There are three reasons why we do not actively control for confounding events in our short-horizon event study.

First, our study does not focus on measuring market effects of a one-time event that occurs exactly once, such as the passage of a law, but on an average effect of unionization events that we observe over multiple union representation cases affecting multiple firms at different times between 2011 and 2019. This means that confounding events would have to systematically occur at the same time as the dates of the key events in our sample to impose significant distortion to our measured average abnormal returns (Larcker et al., 2011). Second, we use a short-horizon event study approach with a five-day event window that we apply for each event date i.e., *filing*, *agreement*, *election*, and *closing* dates, separately. The short event window and the usage of staggered event dates, instead of one long event window including all four dates, reduce the likelihood of including confounding events (McWilliams and Siegel, 1997). Third, for the interpretation of the $ACAR$ at the end of each event window, we additionally consider the one-day AAR and their significance in the event window to assess how they contribute to the $ACAR$ and to which degree they are attributable to the event. The AARs of our main results suggests one-to-two-day information dissemination leaks and lags, substantiating our five-day event window choice.

Despite these reasons, we narrow down the event window specification to two trading days to further reduce the risk of including confounding events. We thus test whether our findings for the *closing* date are robust for a [0; 1] event window i.e., the event date and the trading day after the event date, which is the minimum event window (MacKinlay, 1997). Table 4.7 shows a negative $ACAR$ at the end of the *closing* event window for the union win cases that is significant at the 10% level. This result is in line with Section 4.4.1, where we find a negative AAR on the trading day after the *closing* date.

Table 4.7: Narrowed Event Window: Closing Date

This table shows the abnormal returns for the *closing* date with a narrowed event window [0; 1]. We use this analysis as a robustness test to potential confounding events. Abnormal returns appear in percentages (%) with the Boehmer et al. (1991) t-statistic in parentheses. The asterisks *, **, and *** signal two-tailed significance at the .10, .05 and .01 level, respectively.

	Union win		Union loss	
Panel: Closing Date				
Event time	AAR	ACAR	AAR	ACAR
0	-.004	-.004	.065	.065
	(-.78)	(-.78)	(.12)	(.12)
1	-.108*	-.112*	-.046	.019
	(-1.80)	(-1.95)	(-1.16)	(-.72)
N	368		395	

Another potential obstacle to the validity of our results is the clustering of event windows. We test our hypotheses based on the standardized cross-sectional t-statistic proposed by Boehmer et al. (1991), which assumes that the abnormal returns are cross-sectionally uncorrelated. This assumption may be violated if event windows are clustered, i.e., the event windows of the events in the sample overlap or fall on the same trading days (MacKinlay, 1997).[28] We therefore create a declustered version of our main sample by eliminating observations until there are no more overlapping event windows and repeat our short-horizon event study with the same specifications as before. Although this drastically reduces the number of observations, Table

[28] Events and estimation windows overlap for unionization cases that last between 200 and 400 trading days. Our sample includes 31 such unionization cases and our results are robust to excluding them.

4.8 reports significant negative *ACAR* for the union win cases at the end of the *closing* event window. Our main finding thus remains robust to declustered event windows.

TABLE 4.8: Declustered Sample: Closing Date

This table shows the abnormal returns of our analysis on the *closing* date, using a declustered version of our main sample, i.e. only event windows that do not overlap. Abnormal returns appear in percentages (%) with the Boehmer et al. (1991) *t*-statistic in parentheses. The asterisks *, **, and *** denote two-tailed statistical significance at the 0.10, 0.05, and 0.01 level, respectively.

	Union Win		Union Loss	
Panel: Closing Date				
Event time	AAR	ACAR	AAR	ACAR
-2	-.140	-.140	.261**	.261**
	(-1.58)	(-1.58)	(2.09)	(2.09)
-1	.042	-.098	.006	.267*
	(.70)	(-.85)	(.34)	(1.78)
0	-.014	-.112	-.032	.235
	(-.76)	(-1.18)	(-.99)	(1.05)
1	-.059	-.172	.014	.249
	(-.54)	(-1.37)	(.26)	(1.01)
2	-.174*	-.346**	.039	.287
	(-1.80)	(-2.16)	(.94)	(1.28)
N	170		184	

4.5 Discussion and Conclusion

From a shareholder's perspective, the costs of successful unionization seem to outweigh the benefits. First, we empirically confirm that successful unionization efforts lead to a negative short-term capital-market effect. We find that, on average, the market does expect unionization efforts, because it does not react to *petition filings*, where it cannot predict the election outcome. A subsample of *rare* unionization cases differs with regard to this timing, where unionization efforts do not seem to be anticipated and their announcement through *petition filing* results in a market reaction. For the whole sample, the investigation result and announcement of an *election agreement* provide shareholders with new information and seems to help predict the election outcome. The *election* itself does not induce significant market reactions. The market reacts significantly and negatively to successful unionization at the final and public step of the representation case, the *certification and closing*. These results are in line with the prevailing semi-efficient market hypothesis and the instant (leaks and lags of one to two days) adjustment to new public information.

In a subsequent multiple regression analysis, we find that the magnitude of the negative effect is higher for cases that were clearly won by the union, which is in line with previous research and the argument of stronger bargaining power with stronger vote support. Additionally, we find that an increasing vote share in union loss cases positively affects the cumulative abnormal returns. This result suggests that shareholders are exceedingly "relieved" about union losses when support for unions was strong and the election outcome most uncertain. Our results are robust to control variables, to changes of the event-study parameters, and to sample adjustments.

We address limitations from previous research by analyzing a recent sample of union representation cases that affect national and international publicly-listed firms in a short-horizon even study. The five- and two-day event windows from our main and robustness analyses ease concerns about confounding events. One remaining limitation represents the fraction of total employees that are affected by the unionization process. Due to the nature of capital-market effects, we are limited to a sample of publicly-listed firms that employ, on average, 74 times as many employees as those employees that are affected by the workplace unionization efforts. Nevertheless, our findings show that the capital-market effects are detectable, significant, and economically meaningful.

We are first to show that and when capital-market effects of union certifications are detectable in the short term. Our evidence suggests that the timing of capital-market effects can be attributable to specific sample characteristics. First, shareholders probably incorporate information about future firm performance quicker in the period between 2011 and 2019 than between 1961 and 1980 or 1999 (Ruback and Zimmerman, 1984, Lee and Mas, 2012). Second, we derive an important driver of timing heterogeneity: experience with unionization efforts. For *rare* cases, the market reacts at the *filing* of seemingly unexpected unionization efforts. Consistent with our main sample, *recurring* unionization efforts are valued by shareholders at the final and definitive step. They incorporate the uncertainty about the certification outcome, caused by potential objections during the unionization process, in their expectations. Unlawful discharge allegations per election have been increasing and successful bargaining agreements have been decreasing since the end of the Joy Silk doctrine in 1969. This development of union support drop throughout the election campaign might also help explain the difference in timing of long-term capital-market effects between Ruback and Zimmerman (1984) and Lee and Mas (2012).

The United States NLRA of 1935 does not provide enough legal ground to prevent and punish unfair labor practices. Therefore, similar to the unsuccessful Employee Free Choice Act, a new legislation, the PRO Act, has been proposed in 2019 and again in 2021. If this legislation were enacted, unions' bargaining power and victory rates might increase. If union-busting activities and meetings were prohibited and punished by economically-relevant fines, union election outcomes might become more predictable. Consequently, we would expect short-term capital-market effects to be more prominent at the beginning of the unionization process because shareholders would recognize that management is less likely to interfere with the process and prevent a union certification.

5 | Discussion and Conclusion

5.1 Research Overview and Policy Implications

In order to reach our sustainable development goals, we need to understand the interests and frictions that create a discrepancy between these goals and today's status quo. Once, these interests and frictions are identified, we need to assess and evaluate mechanisms that help achieve specific targets of our goal. This thesis examines real and capital-market effects of three labor market frictions, namely, gender discrimination, board gender quotas, and unionization processes. While the first explains one channel for the lack of gender equality in leadership positions, frictions two and three exemplify mechanisms that aim at reaching specific targets of gender equality (SDG 5), decent work for all (SDG 8), and reduced inequalities (SDG 10).

Chapter 2 provides novel empirical evidence on demand-side factors inhibiting the appointment of women to European corporate boards. First, we find that the pressure to comply with explicit or implicit norms in combination with discriminatory biases can lead to a *saturation* of demand for diversity. Second, based on the 'status quo' bias, we validate that boards aim at complying with norms without disrupting existing internal dynamics and find significant *replacement* effects. The probability of a woman's appointment is higher if the share of women already on the board is low and when a woman compared to when a man leaves the board. These effects are strongest for non-executive appointments and in environments, industries, and countries with increased external demand for women and with a lower supply of women from the labor force.

We validate that European board director appointments are gender-specific. Our results suggest that demand-side rather than supply-side factors influence appointment dynamics. Following the supply logic, directors can and will be appointed from a pool of qualified candidates, regardless of their gender. Even if gender disparity could be explained by factors leading to a smaller pool of qualified women compared to men, the process of director appointment should not be gender-specific.

Our results are of particular interest to policymakers and firms since promoting diversity has become a political objective and attracts much attention and controversy. While several voluntary recommendations for board diversity have been formulated in national or European corporate governance codices, our empirical findings suggest that solely relying on labor market mechanisms will not close the gender gap on corporate boards. Further, a quota does not result in self-reinforcing dynamics with more women appointments once the quota is reached. Neither for quota-implementing nor for non-quota-implementing countries do we find evidence of *exposure* or spillover effects. On the contrary, below gender balance, the appointment probability of women declines strongly with an increasing share of women. Quotas increase the attention on gender and seem to increase *token* appointments.

Despite these possible biases from increased and mandatory demand for gender diversity, a number of countries have implemented quotas for the representation of women on corporate boards of directors. While the existing empirical evidence suggests that quotas are effective in reaching the set threshold, their economic effects remain understudied.

In Chapter 3, we oppose two theories to evaluate firm and board performance effects of mandatory gender quotas in seven European countries. Our results are in line with the *theory of efficient board structures*. We find an average decline in treatment firms' performance and boards' monitoring quality upon quota implementation. This decline is more pronounced for boards with higher ex-ante monitoring quality, who are probably more efficient, and for male-dominated industries, where binding constraints increasingly disrupt board dynamics due to a lower supply of suitable candidates.

Our study aims at measuring and understanding the effects of restricting a specific board attribute through a regulatory mandate on board- and firm-level outcomes. Many countries and stock markets around the world have followed the trend for mandated board diversity. Even more, the understanding of board diversity has gone beyond gender and increasingly includes other diversity attributes, such as ethnicity, sexual orientation, and sexual affiliation. Our results show first indications to the mechanisms and effects future diversity quotas could entail.

We further look at other dimensions of inequality in the labor market and evaluate the unionization process. Unions' major goals are reducing income inequality and giving workers an active and collective voice. In Chapter 4, we evaluate the perspective of shareholders on the unionization process in the United States, which has been largely criticized and subject to many legal pursuits. We are first to empirically determine short-term capital-market effects at the four key steps of the unionization process. On average, the market expects unionization efforts and reacts negatively and significantly to the public *certification* of union wins. Further, we show that the magnitude of the abnormal returns during *certification* depends on the number of votes in favor of the union.

Our results are in line with the prevailing semi-efficient market hypothesis and the instant (leaks and lags of one to two days) adjustment to new public information. Moreover, we derive an important driver of capital-market timing heterogeneity: experience with unionization efforts. Shareholders of firms with *rare* unionization cases do not anticipate unionization efforts and react to *petition filings*, the first step of the unionization process. On the other hand, shareholders with *recurrent* cases seem to incorporate the uncertainty about the certification outcome, caused by potential objections during the unionization process, in their expectations and only react at the last step, the *certification* of union wins. We show that today's unionization process in the United States contributes substantial uncertainty not only to workers and managers, but also to shareholders.

U.S. Presidents Obama and Biden have called for laws strengthening labor unions. U.S. Senator Sanders argues that the newly proposed Protecting the Right to Organize (PRO) Act would "finally give workers a fair chance to win organizing elections" and enforce sanctions against union busting (Wall Street Journal, 2021). If union-busting activities and meetings were prohibited and punished by economically-relevant fines, union election outcomes might become more predictable. Consequently, we would expect short-term capital-market effects to be more prominent at the beginning of the unionization process.

Finally, our results exemplify that policymakers face tradeoffs between different dimensions of sustainability. Specifically, gender quotas positively impact one gender equality target (SDG 5): participation and equal opportunities for leadership at all levels of decision-making in economic life. On the other hand, they negatively impact economic performance measures (SDG 8, United Nations, 2015). Similarly, unions can help achieve targets of decent work for all (SDG 8) and reduced inequalities (SDG 10), but our results suggest that shareholders view unionization as detrimental to future firm performance and economic growth (SDG 8, United Nations, 2015).

Policymakers could implement regulations that specifically target the mechanisms causing today's goal and status quo discrepancies. Education and family policies might be more effective than quota mandates in tackling discrimination, an important demand-side inhibitor to women's economic participation. Regulations to increase the transparency and enforcement of fair unionization processes could reduce detrimental uncertainty for several stakeholders, including shareholders.

5.2 Avenues for Future Research

Throughout this thesis, we employ particular research designs to overcome endogeneity problems and causally identify and evaluate labor-market frictions. With these designs, we are able to answer open questions from various streams of literature. First, we contribute to the governance literature, in particular to firm- and board-level determinants and effects of voluntary and mandatory board diversity. Second, we contribute to the finance and economics literature that aims at assessing why and how the capital market reacts to particular labor market frictions.

Further valuable contributions to the governance, finance, and economics literature could be generated by examining additional data and extended sample periods within our employed research designs. Our empirical results raise new and relevant questions that could be addressed with specific data on director characteristics, board and shareholder meetings, and sustainability-related metrics. More specifically, we derive and propose several avenues for future research from each chapter.

While we examine a gender-specific demand-side channel of board director appointments in Chapter 2, we do not distinguish between different reasons for director turnover. Future research efforts could be undertaken to systematically disentangle voluntary and mandatory board exits. Directors who leave on friendly terms might have a say in their replacement and increase the likelihood of being replaced by candidates from their own network. Next, similar to our differentiation between non-executive and executive roles, differentiating between firm-internal and external director appointments could add valuable insights into board appointment dynamics. Finally, future research could focus on supply-side effects of board director appointments that might reinforce our empirically validated demand-side effects. One could examine to what extent eligible director candidates differ with respect to education, professional experience, and family situation.

Future research could corroborate the findings of our mediation analysis in Chapter 3 and look into the fundamental preferences of directors with different observable characteristics, such as gender, age, tenure, nationality, experience, education, and independence. Then, we could explicitly split the negative performance effects of quotas into two distinct mechanisms. First, how much of the effect can be explained by differences in individual director preferences? Second, how much can be explained by changing group dynamics between existing and newly appointed directors? This evidene could give more precise assessments on performance effects of potential quotas targeting diversity dimensions beyond gender. Finally, our cross-country staggered difference-in-differences design can evaluate long-term quota effects in the future.

The short-term capital-market effect analysis of Chapter 4 can be extended to other events of the collective bargaining process. After the steps of union certification, different steps of the bargaining agreement and labor disputes occur. Evaluating them can add insights into shareholders' perspectives on unions' effectiveness at implementing a collective voice. Beyond capital-market effects, future research can assess real social or environmental effects of unionization. For instance, we examine a safety-ecology tradeoff workers face in toxic waste facilities and examine whether unionization leads to more or less toxic waste releases (Schauf and Schoonjans, 2022).

With the right understanding of existing and regulation-induced frictions, we can evaluate the costs and benefits of mechanisms targeting our sustainable development goals. With these tools, we can find efficient regulatory- and market-based ways to reach sustainability.

Appendix

Appointment Dynamics of Women as Directors — Appendix

Table A.1: Variable Definitions

This table describes the data sources and definitions of the main variables used in the analyses. GDP per Capita and Women Labor Force Rate are obtained from the OECD statistics datasets "Level of GDP per capita and productivity" and "LFS by sex and age". The Employment Rate is from OECD data.

Variable	Definition	Source
Country-Level		
GDP per Capita	Gross domestic product per capita	OECD
Women Labor Force Rate	Women's share of labor force	OECD
Employment Rate	Total share of labor force	OECD
Firm-Level		
Tobin's Q	Sum of total assets and market equity less common book equity divided by total assets	Worldscope
Total Assets	Total assets	Worldscope
Firm Age	Years since first accounts	Worldscope
Dependence Indicator	Dummy=1 if concentrated ownership of at least 25% (Blockholder)	Orbis
Board-Level		
Share Women in SB	Share women directors in supervisory board	Orbis
Share Women in EB	Share women directors in executive board	Orbis
Number Women in SB	Number of women directors in supervisory board (Categorical from 0 to 4)	Orbis
Number Women in EB	Number of women directors in executive board (Categorical from 0 to 4)	Orbis
Women Appointment to SB	Women appointments to supervisory board (Absolute and Dummy=1 if at least one new woman)	Orbis
Women Appointment to EB	Women appointments to executive board (Absolute and Dummy=1 if at least one new woman)	Orbis
Women Exit from SB	Women leave from supervisory board (Absolute and Dummy=1 if at least one woman leaves)	Orbis
Women Exit from EB	Women leave from executive board (Absolute and Dummy=1 if at least one woman leaves)	Orbis
Men Exit from SB	Men leave from supervisory board (Absolute and Dummy=1 if at least one man leaves)	Orbis
Men Exit from EB	Men leave from executive board (Absolute and Dummy=1 if at least one man leaves)	Orbis
Board Size	Absolute number of directors in supervisory and executive board	Orbis
Share Foreign Directors	Share foreign directors in supervisory and executive board	Orbis
Director Age	Average director age in supervisory and executive board	Orbis
Share Multi-directors	Share multi-directors in supervisory and executive board	Orbis
Director Tenure	Average director tenure in supervisory and executive board	Orbis
Share Independent Directors	Share independent directors in supervisory and executive board	Orbis
Chairwoman	Dummy=1 if chair position is held by a woman	Orbis
CEO is a Woman	Dummy=1 if CEO position is held by a woman	Orbis

Table A.2: Additional Regressions Supervisory Board

This table reports additional estimations of linear probability models with alternative dependent variables and estimations of poisson and logit models. Specification (1) estimates OLS with an alternative dependent variable, Δ Share Women, which is the difference of share women between two years and captures the dynamics, including appointments and exits. Specification (2) reports the OLS estimation results for the number of women director appointments. Specification (3) reports estimation results from a poisson model for the number of women director appointments. Specifications (4) and (5) estimate logit model with our main dependent variable, Women Appointment, equal to one if at least one woman was appointed to the Supervisory Board. Specifications (3) and (4) use Year (Y), Country (C), and two-digit SIC-industry (I) fixed effects. Specifications (1), (2), and (5) use Year (Y) and Firm (F) fixed effects. Standard errors clustered at the firm level are in parentheses; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

	(1)	(2)	(3)	(4)	(5)
	OLS	OLS	Poisson	Logit	Logit
	Δ Share Women	# Women Appointment	# Women Appointment	Women Appointment	Women Appointment
Share Women in SB	-0.300***	-1.222***	-0.620**	-1.072***	-0.102***
	(0.017)	(0.068)	(0.210)	(0.245)	(0.004)
Share Women in SB × Share Women in SB	-0.048	0.734***	-0.012	0.390	0.001***
	(0.035)	(0.086)	(0.290)	(0.316)	(0.000)
Women Exit from SB		0.695***	0.971***	1.407***	1.359***
		(0.048)	(0.052)	(0.083)	(0.089)
Men Exit from SB		0.179***	0.792***	0.870***	0.833***
		(0.012)	(0.047)	(0.057)	(0.060)
Board Size	-0.001***	-0.014***	-0.026***	-0.016**	-0.078***
	(0.000)	(0.003)	(0.005)	(0.006)	(0.009)
Firm Age	0.004***	0.026***	0.002	0.001	
	(0.000)	(0.003)	(0.001)	(0.002)	
log(Total Assets)	0.003*	0.003	0.254***	0.302***	0.174**
	(0.001)	(0.007)	(0.010)	(0.012)	(0.058)
Tobin's Q	0.000***	0.000*	0.001*	0.001*	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.002)
GDP per Capita	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Employment Rate	0.061	-0.093	2.217	2.750	1.816
	(0.058)	(0.288)	(1.758)	(2.022)	(2.335)
Women Labor Force Rate	0.244	2.974**	9.771	10.013	11.234
	(0.266)	(1.061)	(6.209)	(7.098)	(8.121)
Constant	-0.156	-1.242*	-10.598***	-12.032**	
	(0.125)	(0.524)	(3.171)	(3.701)	
Fixed Effects	Y F	Y F	Y C I	Y C I	Y F
N	27445	27486	27486	27486	18659

Table A.3: Subsample Analyses for Women Appointments as Executive Directors

This table reports cross-sectional results of the main specification ((Specification (1) in Table 2.5). Specifications (1) and (2) compare industries with high and low share of women directors. Specifications (3) to (5) compare countries with high, medium, and low women labor force participations (LFP). Specifications (6) and (7) compare observations in years and countries after mandatory board gender quota implementation to those without mandatory quotas. Fixed effects are on the Year (Y) and Firm (F) level. Standard errors clustered at the firm level are in parentheses; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

	(1) High SW Industry	(2) Low SW Industry	(3) High LFP	(4) Middle LFP	(5) Low LFP	(6) Quota Treated	(7) No Quotas
Share Women in EB	-0.649***	-0.603***	-1.192***	-0.893***	-1.233***	-1.626***	-0.567***
	(0.063)	(0.097)	(0.135)	(0.071)	(0.171)	(0.177)	(0.057)
Share Women in SB	0.038	0.020	-0.077	-0.014	0.058	0.245	0.019
	(0.039)	(0.045)	(0.064)	(0.042)	(0.076)	(0.139)	(0.031)
Share Women in EB × Share Women in EB	0.535***	0.477***	0.887***	0.755***	0.918***	1.315***	0.462***
	(0.069)	(0.093)	(0.124)	(0.076)	(0.188)	(0.182)	(0.060)
Share Women in SB × Share Women in SB	-0.020	0.012	0.058	0.108	-0.048	-0.318*	0.009
	(0.055)	(0.056)	(0.082)	(0.070)	(0.069)	(0.149)	(0.045)
Women Exit from EB	0.102**	0.154**	0.108	0.122***	0.261***	0.474***	0.077**
	(0.034)	(0.047)	(0.071)	(0.035)	(0.077)	(0.110)	(0.026)
Men Exit from EB	0.046***	0.033***	0.052*	0.022**	0.066***	0.116***	0.033***
	(0.010)	(0.009)	(0.026)	(0.008)	(0.016)	(0.033)	(0.007)
Board Size	0.006***	0.006*	0.012**	0.005**	-0.002	-0.005	0.006***
	(0.002)	(0.002)	(0.004)	(0.002)	(0.002)	(0.004)	(0.001)
Firm Age	0.004*	0.004*	0.000	0.006	0.007	0.037**	0.004**
	(0.002)	(0.002)	(0.005)	(0.005)	(0.005)	(0.013)	(0.001)
log(Total Assets)	-0.004	-0.004	0.007	-0.006	0.012	0.001	-0.006
	(0.005)	(0.004)	(0.011)	(0.005)	(0.009)	(0.025)	(0.003)
Tobin's Q	-0.000	-0.000	-0.002	-0.000	0.000	0.000	-0.000
	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.006)	(0.000)
GDP per Capita	0.000	-0.000	0.000	-0.000	-0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Employment Rate	-0.160	-0.057	-0.789	-0.531	0.115	3.785	-0.057
	(0.203)	(0.195)	(0.646)	(0.387)	(0.172)	(2.652)	(0.130)
Women Labor Force Rate	0.467	0.515	0.621	1.755	0.252	9.493	0.618
	(0.715)	(0.768)	(2.974)	(2.868)	(1.038)	(7.299)	(0.526)
Constant	-0.122	-0.192	0.143	-0.307	-0.268	-7.508*	-0.247
	(0.373)	(0.393)	(1.541)	(1.251)	(0.497)	(3.190)	(0.270)
Fixed Effects	Y F	Y F	Y F	Y F	Y F	Y F	Y F
N	11383	9289	5399	10974	4299	2822	17850

Table A.4: Additional Regressions Executive Board

This table reports additional estimations of linear probability models with alternative dependent variables and estimations of poisson and logit models. Specification (1) estimates OLS with an alternative dependent variable, Δ Share Women, which is the difference of share women between two years and captures the dynamics, including appointments and exits. Specification (2) reports the OLS estimation results for the number of women director appointments. Specification (3) reports estimation results from a poisson model for the number of women director appointments. Specifications (4) and (5) estimate logit model with our main dependent variable, Women Appointment, equal to one if at least one woman was appointed to the Executive Board. Specifications (3) and (4) use Year (Y), Country (C), and two-digit SIC-industry (I) fixed effects. Specifications (1), (2), and (5) use Year (Y) and Firm (F) fixed effects. Standard errors clustered at the firm level are in parentheses; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

	(1)	(2)	(3)	(4)	(5)
	OLS	OLS	Poisson	Logit	Logit
	Δ Share Women	# Women Appointment	# Women Appointment	Women Appointment	Women Appointment
Share Women in EB	-0.305***	-0.626***	1.594***	1.478**	-0.121***
	(0.031)	(0.063)	(0.442)	(0.469)	(0.008)
Share Women in EB × Share Women in EB	-0.049	0.499***	-1.409**	-1.278*	0.001***
	(0.043)	(0.064)	(0.537)	(0.587)	(0.000)
Share Women in SB	0.029	0.018	0.740	0.877	0.011
	(0.016)	(0.034)	(0.430)	(0.461)	(0.008)
Share Women in SB × Share Women in SB	-0.023	0.018	-0.688	-0.694	-0.000
	(0.020)	(0.044)	(0.553)	(0.604)	(0.000)
Women Exit from EB		0.156***	0.824***	0.995***	1.126***
		(0.040)	(0.192)	(0.243)	(0.273)
Men Exit from EB		0.049***	0.908***	0.947***	0.809***
		(0.008)	(0.101)	(0.112)	(0.134)
Board Size	0.001	0.008***	0.021*	0.032**	-0.007
	(0.001)	(0.002)	(0.009)	(0.010)	(0.016)
Firm Age	0.000	0.005**	0.002	0.002	
	(0.001)	(0.001)	(0.003)	(0.003)	
log(Total Assets)	-0.001	-0.005	0.269***	0.265***	-0.159
	(0.002)	(0.004)	(0.025)	(0.026)	(0.136)
Tobin's Q	-0.000	-0.000	0.001*	0.001*	0.000
	(0.000)	(0.000)	(0.000)	(0.001)	(0.006)
GDP per Capita	0.000	-0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Employment Rate	-1.789	-9.203	198.273	133.721	-691.211
	(5.955)	(17.459)	(468.681)	(489.306)	(621.943)
Women Labor Force Rate	34.514	42.447	2700.698	3154.840*	3450.254
	(24.899)	(60.813)	(1448.111)	(1511.418)	(2480.596)
Constant	-0.122	-0.141	-21.454**	-23.085**	
	(0.123)	(0.323)	(7.719)	(8.165)	
Fixed Effects	Y F	Y F	Y C I	Y C I	Y F
N	20321	20672	20672	20378	5434

Effects of Board Gender Quotas on Board and Firm Performance — Appendix

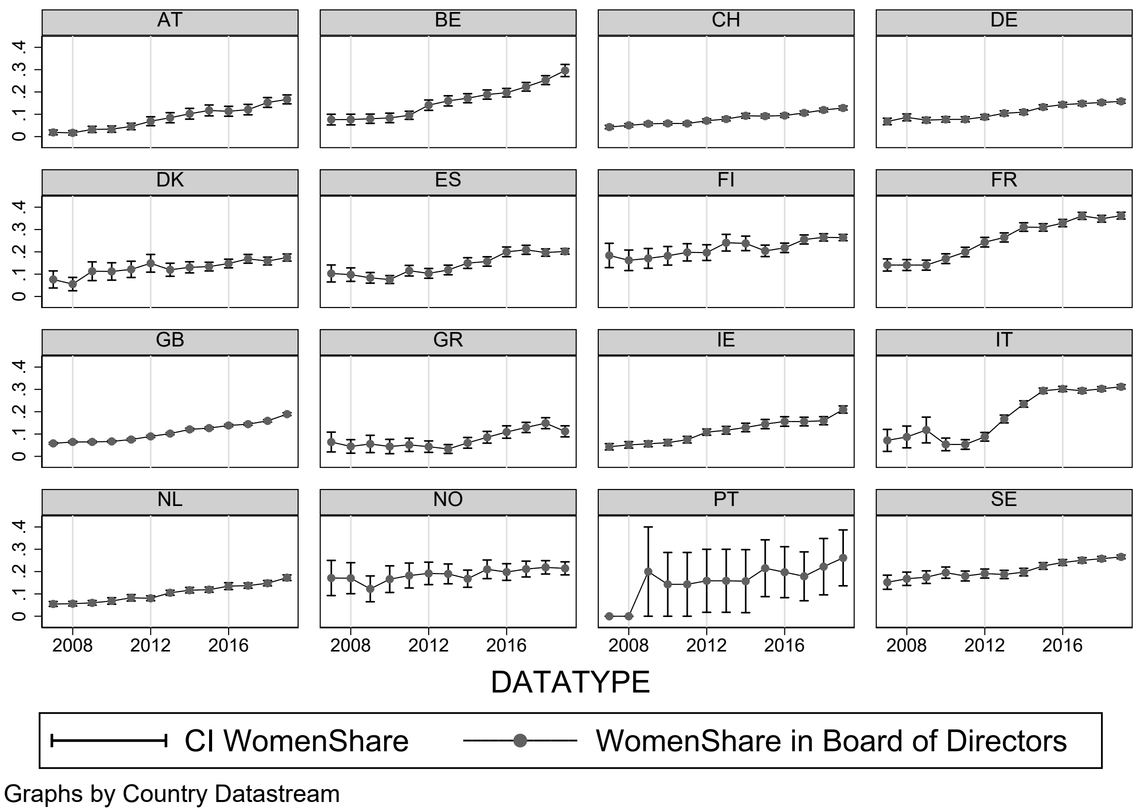


FIGURE A.1: Time Trend of Female Board Representation (Country Averages)

This figure depicts the country-level average time trend of the share of women on corporate boards.

Table A.5: Variable Definitions

This table describes how my variables are computed.

Variable	Definition	Source
GDP per Capita	Gross domestic product per capita	OECD
Women Labor Force Rate	Women's share of labor force	OECD
Tobin's Q	Sum of total assets and market equity less common book equity divided by total assets	Worldscope
ROA	Net income before interest and taxes divided by the average of last and current year's total assets	Worldscope
Total Assets	Total assets	Worldscope
Leverage	Total debt divided by total assets	Worldscope
Firm Age	Years since first accounts	Worldscope
Profitability	Net income before extra items or preferred dividends divided by net sales	Worldscope
Abs(DA)-Jones	Absolute value of the cross-sectional estimation error in total accruals	Worldscope
	for each year in the same two-digit SIC code (Jones, 1991)	
	$TA_{it} = \beta_0 + \beta_1(1/TotalAssets_{it-1}) + \beta_2 \Delta Sales_{it} + \beta_3 PPE_{it} + \epsilon_{it}$	
	where $\Delta Sales_{it}$ is change in sales and PPE_{it} is net property, plant, and equipment,	
	both scaled by lagged total assets	
Abs(DA)-ROA-adj.Jones	Absolute value of the cross-sectional estimation error in total accruals	Worldscope
(Main variable: *Abs. DA*)	for each year in the same two-digit SIC code (Kothari et al., 2005)	
	$TA_{it} = \beta_0 + \beta_1(1/TotalAssets_{it-1}) + \beta_2 \Delta Sales_{it} + \beta_3 PPE_{it} + \beta_4 ROA_{it-1} + \epsilon_{it}$	
Abs(DA)-ROA-adj.mod.Jones	Absolute value of the cross-sectional estimation error in total accruals	Worldscope
	for each year in the same two-digit SIC code (Dechow et al., 1995, Kothari et al., 2005)	
	$TA_{it} = \beta_0 + \beta_1(1/TotalAssets_{it-1}) + \beta_2(\Delta Sales_{it} - \Delta REC_{it}) + \beta_3 PPE_{it} + \beta_4 ROA_{it-1} + \epsilon_{it}$	
	where ΔREC_{it} is change in net receivables scaled by lagged total assets	
Abnormal R&D	Value of the cross-sectional estimation error in the following equation (Cohen et al., 2008):	Worldscope
	$RD_{it} = \beta_1(1/TotalAssets_{it-1}) + \beta_2 Sales_{it} + \epsilon_{it}$	
	where RD_{it} are research and development expenses, scaled by lagged total assets	
Abnormal SG&A	Value of the cross-sectional estimation error in the following equation (Cohen et al., 2008):	Worldscope
	$SGA_{it} = \beta_1(1/TotalAssets_{it-1}) + \beta_2 Sales_{it} + \epsilon_{it}$	
	where SGA_{it} are research and development expenses, scaled by lagged total assets	
Abnormal Disc. Expenses	Value of the cross-sectional estimation error in the following equation (Cohen et al., 2008):	Worldscope
	$DiscExp_{it} = \beta_1(1/TotalAssets_{it-1}) + \beta_2 Sales_{it} + \epsilon_{it}$	
	where $DiscExp_{it}$ is the sum of research and development	
	and selling, general, and administration expenses, scaled by lagged total assets	
Abnormal CF from Operations	Value of the cross-sectional estimation error in the following equation (Cohen et al., 2008):	Worldscope
	$CFO_{it} = \beta_1(1/TotalAssets_{it-1}) + \beta_2 Sales_{it} + \beta_3 \Delta Sales_{it} + \epsilon_{it}$	
Share Women Directors	Share women directors in supervisory and executive board	Orbis
Share Foreign Directors	Share foreign directors in supervisory and executive board	Orbis
Director Age	Average director age in supervisory and executive board	Orbis
Board Size	Absolute number of directors in supervisory and executive board	Orbis
Share Multi-directors	Share multi-directors in supervisory and executive board	Orbis
Director Tenure	Average director tenure in supervisory and executive board	Orbis
Share Independent Directors	Share independent directors in supervisory and executive board	Orbis

TABLE A.6: Sample Distribution per Country

This table describes how my sample is distributed across countries.

	Firm-year Observations	Firm-year Observation Share	Unique Firms	Unique Firm Share	Tobin's Q	Women Director Share
AT	1153	1.66	96	1.33	1.31	0.08
BE	1760	2.53	145	2.01	1.54	0.13
CH	3114	4.48	248	3.43	1.76	0.07
DE	9865	14.20	875	12.11	1.52	0.11
DK	1918	2.76	159	2.20	1.79	0.13
ES	2257	3.25	193	2.67	1.55	0.15
FI	2247	3.23	180	2.49	1.59	0.22
FR	10653	15.33	981	13.58	1.52	0.27
GB	20014	28.80	2318	32.09	1.94	0.10
GR	4239	6.10	325	4.50	1.17	0.10
IE	966	1.39	91	1.26	2.34	0.09
IT	4137	5.95	380	5.26	1.39	0.24
NL	2469	3.55	220	3.05	1.61	0.09
NO	3094	4.45	326	4.51	1.60	0.20
PT	892	1.28	69	0.96	1.18	0.16
SE	5558	8.00	618	8.55	1.99	0.21
Total	67624	100	7055	100	1.67	0.14

Table A.7: Alternative Quota Thresholds

This table reports the results of my main specifications on firm performance and earnings management, for alternative *Quota* thresholds, in the sample of 15 European countries between 2007 and 2019. In columns (1) and (2), I define the post-treatment period in Italy on and after 2012 and on and after 2014 in France. In columns (3) and (4), I report the results when defining the post-treatment periods 2015 and 2017 for Italy and France respectively, in a quota-adopting sample. t statistics are in parentheses; Y: year, C: country, S: SIC2-industry, F: firm; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

	(1)	(2)	(3)	(4)
	Tobin's Q	Abs. DA	Tobin's Q	Abs. DA
	First Quota Threshold	First Quota Threshold	Second Quota Threshold	Second Quota Threshold
Quota	-0.061* (-1.78)	0.005* (1.71)	-0.103*** (-2.79)	0.010** (2.40)
Economic Magnitude	-4.06	8.00	-6.84	16.38
Adjusted R^2	0.04	0.00	0.07	0.00
Additional Controls	C F	C F	C F	CF
Fixed Effects	Y F	Y F	Y F	Y F
Standard Error Cluster	F	F	F	F
N	43365	33338	17762	12911

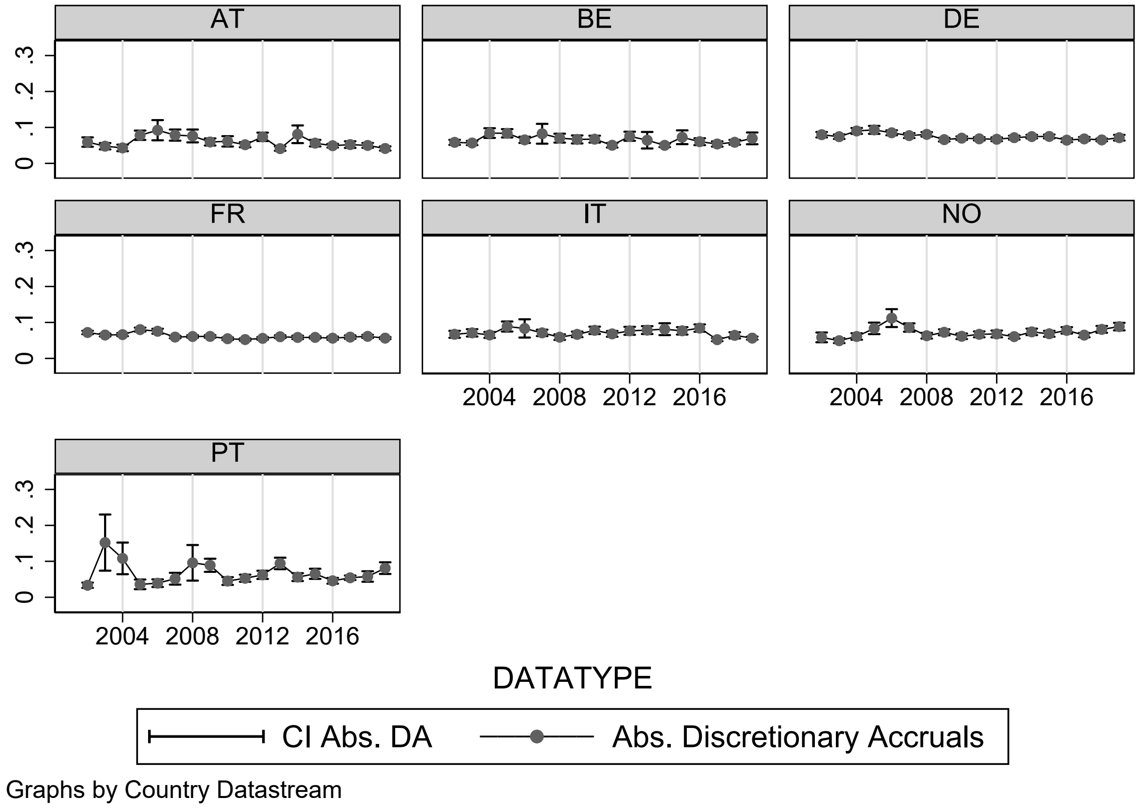


FIGURE A.2: Time Trend of Absolute Discretionary Accruals (Country Averages)

This figure depicts the country-level average time trend of absolute discretionary accruals.

Table A.8: Exclusion Financial Crisis and Lagged Firm Controls

This table reports the results for alternative versions of my main specifications on firm performance and earnings management in the restricted sample of quota-adopting countries, excluding all years prior to 2007 and Norway. In columns (1) and (2), I additionally exclude all years prior to 2010. In columns (3) and (4), I lag my firm-level control variables, that could also have been affected by the quota, profitability and leverage. t statistics are in parentheses; Y: year, C: country, S: SIC2-industry, F: firm; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

	(1)	(2)	(3)	(4)
	Tobin's Q	Abs. DA	Tobin's Q	Abs. DA
	w/o Financial Crisis	w/o Financial Crisis	Lagged Firm Controls	Lagged Firm Controls
Quota	-0.073** (-2.46)	0.010** (2.46)	-0.062** (-1.98)	0.008** (2.15)
Economic Magnitude	-4.84	15.90	-4.10	13.71
Adjusted R^2	0.04	0.00	0.07	0.01
Additional Controls	C F	C F	C F	CF
Fixed Effects	Y F	Y F	Y F	Y F
Standard Error Cluster	F	F	F	F
N	13223	10383	17392	12909

Recently, the staggered difference-in-differences methodology has been criticised due to the necessary assumptions for estimating unbiased average treatment effects on the treated. Research designs combining a cohort-staggered timing of treatment effects and treatment effect heterogeneity across firms or over time can result in biased difference-in-differences estimates. In fact, the estimates can produce the wrong sign compared to the true average treatment effects (Baker et al., 2022).

First, I follow Baker et al. (2022) and test the robustness of my results with one of their proposed remedy. In Table A.9, I estimate the quota effect including the full set of relative-time indicator variables, excluding only the necessary number of relative time indicators to avoid multi-collinearity. Because each relative-time indicator is only turned on once for each unit, this design helps to resolve some of the variance-weighted averaging concerns of the current literature stream. I follow standard practice by excluding the relative time indicator for the period before treatment, so that the coefficients for the relative time indicators can be viewed as the mean differences from the average value of the outcome in the period before treatment.[29] The coefficients' signs are robust to my main results.

Next, to ensure that the estimation process is not contaminated by comparing late versus earlier treated firms, I follow Sun and Abraham (2021) and compare the treated firms to a "clean" set of control firms that are never treated. I estimate the dynamic interaction-weighted (IW) average treatment effect and report the results in Table A.10. This estimator estimates the interaction between the cohort-specific treatment intensity and the relative event time indicators in a first step. Following this procedure, the estimator is consistent for the average dynamic effect at a given relative time even under heterogeneous treatment effects (Sun and Abraham, 2021, Sun, 2021). The coefficients' signs are quite robust to my main results regarding the quota impact on firm performance, but they are not robust to relaxed assumptions regarding the quota impact on earnings management.

[29]As noted in Borusyak and Jaravel (2017) and Baker et al. (2022), I exclude an additional relative time indicator in my sample of adopting countries only.

TABLE A.9: 1st Event Study DID Design Alternative

This table reports the estimation results with relaxed assumptions about the within-cohort timing and intensity heterogeneity. Specification (1) reports the results of the impact of the mandatory board gender quota on firm performance in the restricted sample of quota-adopting countries between 2001 and 2019. Specification (2) reports the results of the impact of the mandatory board gender quota on absolute discretionary accruals in the restricted sample of quota-adopting countries (excluding Norway) between 2007 and 2019. t statistics are in parentheses; Y: year, C: country, S: SIC2-industry, F: firm; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

	(1)	(2)
	Tobin's Q (Baker, Larcker & Wang, 2021)	Abs. DA (Baker, Larcker & Wang, 2021)
-16 years from the treatment start	0.135^{**} (3.61)	
-15 years from the treatment start	0.077 (1.69)	
-14 years from the treatment start	0.053 (0.90)	
-13 years from the treatment start	0.058 (0.72)	
-12 years from the treatment start	0.027 (0.34)	
-11 years from the treatment start	0.034 (0.45)	0.000 (.)
-10 years from the treatment start	0.087 (1.35)	-0.014 (-1.31)
-9 years from the treatment start	0.099 (1.92)	-0.015 (-1.50)
-8 years from the treatment start	0.141^{**} (3.11)	-0.014 (-1.94)
-7 years from the treatment start	0.115^{*} (2.34)	-0.018^{**} (-2.71)
-6 years from the treatment start	0.119^{**} (2.55)	-0.013^{*} (-2.28)
-5 years from the treatment start	0.100^{*} (2.26)	-0.008 (-1.67)
-4 years from the treatment start	0.083 (1.41)	-0.012^{***} (-4.93)
-3 years from the treatment start	0.078 (1.44)	-0.009^{*} (-2.40)
-2 years from the treatment start	0.042^{*} (2.12)	-0.006^{**} (-3.10)
Treatment start	-0.057 (-1.58)	0.003 (0.66)
1 years from the treatment start	-0.002 (-0.05)	0.009 (1.12)
2 years from the treatment start	-0.050 (-1.30)	0.000 (0.05)
3 years from the treatment start	0.007 (0.10)	0.005 (0.69)
4 years from the treatment start	-0.102 (-1.72)	0.000 (0.07)
5 years from the treatment start	-0.177^{**} (-2.89)	-0.005 (-0.42)
6 years from the treatment start	-0.193^{**} (-2.46)	0.009 (0.73)
7 years from the treatment start	-0.097 (-0.76)	-0.005 (-0.39)
8 years from the treatment start	0.025 (0.14)	
9 years from the treatment start	-0.252 (-1.49)	
10 years from the treatment start	-0.093 (-0.58)	
11 years from the treatment start	0.161 (0.73)	
Adjusted R^2	0.07	0.00
Additional Controls	C F	C F
N	29106	12911

Table A.10: 2nd Event Study DID Design Alternative

This table reports the estimation results with relaxed assumptions about the within-cohort timing and intensity heterogeneity. Specification (1) reports the results of the impact of the mandatory board gender quota on firm performance in the full sample of 16 European countries between 2001 and 2019. Specification (2) reports the results of the impact of the mandatory board gender quota on absolute discretionary accruals in the sample of 15 European countries (excluding Norway) between 2007 and 2019. t statistics are in parentheses; Y: year, C: country, S: SIC2-industry, F: firm; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

	(1)	(2)
	Tobin's Q (Sun & Abraham, 2020)	Abs. DA (Sun & Abraham, 2020)
More than 1 year before treatment start	.0463 (1.27)	-.0028 (-0.78)
Treatment start	-.0053 (-0.27)	-.0008 (-0.21)
1 year after treatment start	.0744** (2.52)	.0044 (1.00)
2 years after treatment start	.0168 (0.53)	-.0052 (-1.09)
3 years after treatment start	.0606(1.34)	-.0028 (-0.36)
4 years after treatment start	-.0741 (-1.44)	-.0090 (-0.88)
5 years after treatment start	-.1629** (-2.19)	-.0156* (-1.69)
6 years after treatment start	-.2330*** (-2.85)	-.0048 (-0.28)
7 years after treatment start	-.1883** (-2.06)	-.0189 (-1.12)
More than 7 years after treatment start	-.1679 (-1.30)	
Adjusted R^2	0.57	0.16
Additional Controls	C F	C F
N	67316	32907

Timing of Capital-Market Effects of Unionization — Appendix

TABLE A.11: Variable Definitions

This table describes how our variables are computed.

	Definition	Source
Election-specific variables		
Number of Eligible Voters	Number of employees entitled to cast a vote in the election	NLRB
Valid Votes Cast	Number of actual voters in the election	NLRB
Fraction of Total Employees Eligible to Vote	Number of Eligible Voters / Total Employees	NLRB
Fraction of Total Employees Voting	Valid Votes Cast / Total Employees	NLRB
Vote share	Vote share for union -0.5	NLRB
Union Win	Indicates a union win	NLRB
Right-to-Work	Indicates if affected workplace is in state with right-to-work law	NLRB
Industry-specific variables		
Manufacturing	Indicates if case affects firm in manufacturing industry	CRSP/Compustat, Refinitiv
Transport	Indicates if case affects firm in transportation industry	CRSP/Compustat, Refinitiv
Accommodation	Indicates if case affects firm in accommodation industry	CRSP/Compustat, Refinitiv
Industry Union Coverage	Employees covered by collective bargaining agreements	Macpherson and Hirsch (2021)
Firm-specific variables		
Return on Assets	Net Income / Total Assets	CRSP/Compustat, Refinitiv
Tobin's Q	(Market Capitalization + Total Debt)/Total Assets	CRSP/Compustat, Refinitiv
Debt ratio	Total Liabilities / Total Assets	CRSP/Compustat, Refinitiv
Market Capitalization	Market Value of Equity in million U.S. Dollars	CRSP/Compustat, Refinitiv
Size	Natural Logarithm of Total Assets	CRSP/Compustat, Refinitiv
Employees	Total Employees	CRSP/Compustat, Refinitiv
Beta	Market model β-coefficient	CRSP
International	Indicates if case affects internationally listed firm	CRSP/Compustat
S&P 500	Indicates if case affects S&P 500 constituent	CRSP/Compustat

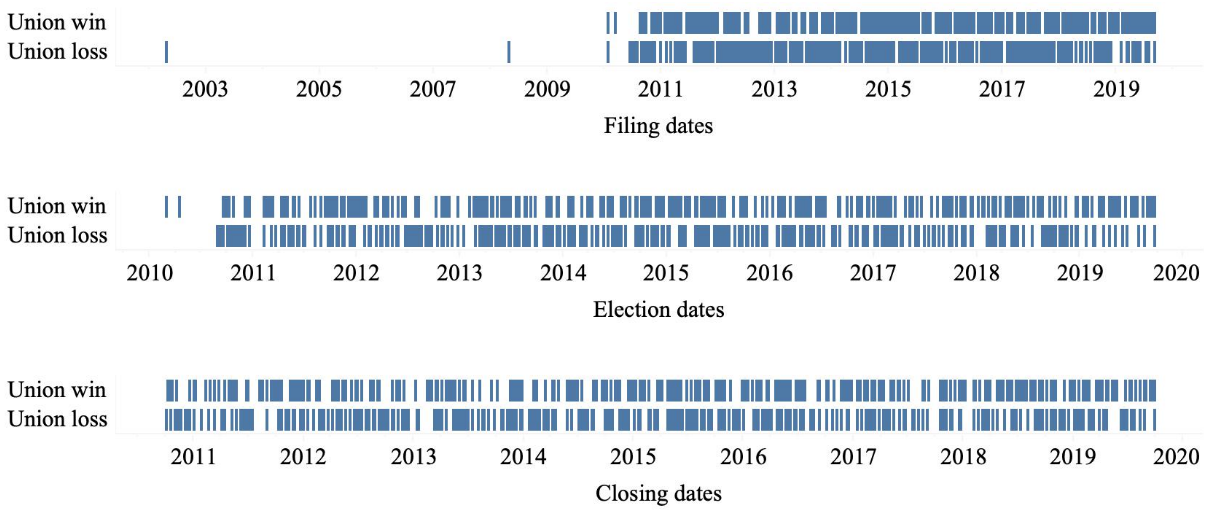


FIGURE A.3: Timeline Illustration of Event Date Distributions

This figure illustrates the distribution of the distinct *filing*, *election*, and *closing* dates of the sample's union representation cases by election outcome. The three dates are evenly distributed over the observation period, with a median year of 2015 in all three instances, showing that our observations are not concentrated in a single year or a short period of time. The figure shows isolated *filing* and *election* dates that precede the first *closing* date on the timeline. This is partly because in order to assign union representation cases to fiscal years, the NLRB uses the respective closing date of the case. Second, it illustrates that the legal process of unionization can occasionally take a considerable amount of time, even years in certain cases.

Table A.12: Sample Distribution per Industry

This table describes how our sample is distributed across industries.

	Union win		Union loss		All cases	
	#	%	#	%	#	%
Accommodation and Food Services	54	14.67	28	7.09	82	10.75
Adm. and Support and Waste Man. and Rem. Services	40	10.87	23	5.82	63	8.26
Agriculture, Forestry, Fishing and Hunting	1	0.27	0	0.00	1	0.13
Arts, Entertainment, and Recreation	6	1.63	8	2.03	14	1.83
Construction	9	2.45	3	0.76	12	1.57
Finance and Insurance	8	2.17	10	2.53	18	2.36
Health Care and Social Assistance	8	2.17	3	0.76	11	1.44
Information	23	6.25	14	3.54	37	4.85
Management of Companies and Enterprises	1	0.27	0	0.00	1	0.13
Manufacturing	71	19.29	190	48.10	261	34.21
Mining	2	0.54	6	1.52	8	1.05
Other Services (except Public Administration)	3	0.82	1	0.25	4	0.52
Professional, Scientific, and Technical Services	19	5.16	10	2.53	29	3.80
Real Estate Rental and Leasing	17	4.62	7	1.77	24	3.15
Retail Trade	5	1.36	18	4.56	23	3.01
Transportation and Warehousing	72	19.57	28	7.09	100	13.11
Utilities	16	4.35	19	4.81	35	4.59
Wholesale Trade	13	3.53	27	6.84	40	5.24
Total	368	100.00	395	100.00	763	100.00

Table A.13: Short-Horizon Regression Results: Agreement Date

This table shows the regression results for the *CAR* at the end of the *investigation* event window, controlling for election-, firm-, and industry-specific variables in four specifications of our regression model. We report the definitions of the variables in Appendix A.10. The *t*-statistics are in parentheses. The asterisks *, ** and *** denote two-tailed statistical significance at the 0.10, 0.05, and 0.01 level, respectively.

	(1)	(2)	(3)	(4)
Panel: Agreement Date				
Union Win	-0.009	-0.004	-0.009	-0.013**
	(-1.50)	(-1.02)	(-1.45)	(-2.05)
Vote Margin	0.006		0.007	0.024
	(0.31)		(0.35)	(1.27)
Union Win × Vote Margin	0.013		0.013	-0.001
	(0.53)		(0.54)	(-0.04)
Fraction Voting	0.086*		0.083*	0.070
	(1.77)		(1.71)	(1.44)
Right-to-Work	-0.003		-0.004	-0.004
	(-0.88)		(-0.92)	(-0.92)
Constant	-0.006	0.007	-0.011	0.006
	(-0.27)	(0.28)	(-0.51)	(1.06)
Firm-Level Controls	Yes	Yes	Yes	No
Industry-Level Controls	Yes	Yes	No	Yes
N	606	606	606	606

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