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Gender Quotas on Corporate Boards

A Panel Data Analysis of the Short-Term Effects of California Senate Bill No. 826

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Abstract

Gender quotas on corporate boards of directors have become a popular tool to address the persistent disparities in female representation at the top of the corporate hierarchy. In this thesis, I study the effects of the first mandatory gender quota in the United States, California Senate Bill No. 826, implemented in early 2019. I build a panel dataset consisting of data on board composition as well as firm characteristics and financial performance for both firms headquartered in California and firms headquartered outside California. Using a Difference-in-Differences strategy combined with Propensity Score Matching, I examine the effects of the quota on board gender diversity, board size, and financial performance. My findings suggest that, despite having milder sanctions than some European board quotas, the Californian quota led to a significant increase in the share of women on the boards of the affected companies compared to control companies. I observe positive effects on both board size and number of female directors on the board, however, due to Californian companies following a slightly different trend relative to control companies before the implementation of the quota, these results should be interpreted with caution. Furthermore, I do not find reliable evidence of the effects of the quota on financial performance.

Keywords: Gender Quotas, Board Composition, Corporate Governance, Gender Economics

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1. Introduction

Despite considerable improvements during the past decades, women remain underrepresented in top decision-making positions in the private sector. In the United States (U.S.), even though women constitute 47 percent of the total workforce, only 30 percent of board seats and six percent of Chief Executive Officer (CEO) positions in companies listed on the Standard & Poor's 500 Index[1] are held by women (Catalyst, 2021a). The situation is similar in Europe; on average, 30 percent of board members and eight percent of CEOs in the largest listed companies in the European Union (EU) are women, however, there are large differences between the EU member states (European Institute for Gender Equality, 2021a, 2021b). This phenomenon of increasing disparities in female representation the higher we get on the corporate hierarchy is referred to as the glass ceiling (Bertrand, 2018).

At the same time, gender quotas on corporate boards have become a popular tool to address these disparities in female representation. Norway introduced a legally binding gender quota on boards of publicly listed companies already in 2003, after which many other European countries have followed suit: e.g. Belgium and France in 2011, Italy in 2012, and Germany in 2015 (Azmat & Boring, 2020). There are also several other European countries that have set non-binding "soft" quotas to encourage firms to increase gender diversity on their boards of directors (Azmat & Boring, 2020) and the European Commission has had plans to introduce a binding 40-percent quota for listed companies across the EU (Maida & Weber, 2020). In contrast, the United States has been more cautious towards mandatory gender quotas. However, in September 2018, California became the first state in the country to introduce this kind of legislation. The quota legislation became effective on January 1, 2019 and requires all publicly held companies headquartered in California to have at least one female director on their board of directors by December 31, 2019. Furthermore, boards with five members must have at least two women and boards with six or more members at least three women by December 31, 2021, respectively.[2] Despite only being imposed in one state, the quota impacts a considerable share of companies in the United States. Twelve percent of all public U.S. firms are headquartered in California (Greene et al., 2020) and at the time when the quota legislation was passed, 25 percent of the Californian public companies on the Russell 3000 Index[3] had no women on their board of directors (California Legislative Information, 2018).

Despite their popularity, gender quotas have been seen as a somewhat controversial tool to increase female representation in corporate boards. In California, the effectiveness of the quota has been debated for several reasons (von Meyerinck et al., 2021). First, the legality of the quota has been

[1]The Standard and Poor's 500 Index (S&P 500) includes the 500 leading publicly traded companies in the United States and covers approximately 80 percent of total market capitalization (S&P Global, 2021).

[2]In October 2020, this legislation was followed by a diversity quota requiring Californian companies to have at least one director from an "underrepresented community" (Steele, 2020). This is, to my knowledge, the first board diversity quota that goes beyond gender. Furthermore, in December 2020, Nasdaq proposed that its listed companies should have at least one woman and one minority director on the board of directors or explain why this is not the case (Nasdaq, 2021). The proposal was accepted in August 2021 (DiNapoli, 2020).

[3]The Russell 3000 Index includes the 3,000 largest publicly traded firms in the United States.

challenged by e.g. the California Chamber of Commerce (Fox, 2018). So far, several lawsuits have been filed against the quota, the latest as recently as July 2021, on the grounds that it violates the U.S. Constitution by discriminating based on gender.[4] Furthermore, the fact that the quota legislation also applies to companies incorporated in other states has been legally challenged (Posner, 2021). If the quota only applied to publicly held companies both headquartered and incorporated in California, it would have a very limited reach (von Meyerinck et al., 2021). Of the 625 impacted companies, as listed by California Secretary of State (2021), only 43 are both incorporated and headquartered in California. Second, the penalties for non-compliance are relatively weak compared to e.g. the widely-studied gender quota in Norway,[5] which makes the Californian quota an interesting case for investigating whether there are differences in the effects of quotas depending on the strictness of sanctions for non-compliance. Finally, as companies are not required to replace existing male directors, it is not clear from the outset whether the quota would result in companies merely increasing the number of board members (von Meyerinck et al., 2021), which could lead to more modest effects on the share of female directors or the dynamics within the board of directors.

Hence, in my thesis, I explore the following main research questions:

- *Did the quota succeed in increasing the share of female directors at the affected firms?*
- *Did the quota result in increased board size or did firms replace existing male directors?*

My main outcome variable of interest is board gender diversity, defined as the percentage share of women on the board of directors. I decide to study this measure, instead of compliance with the quota requirements[6] as I argue that this is a more interesting one to look at. Whether the quota may have any effects beyond just mechanically increasing the number of women on the boards of the affected companies could depend on the level of female representation reached after the quota. Merely having one woman on the board does not necessarily have any impact on firm outcomes as there may be a certain critical mass required for women to have any significant impact on the board. In addition to board gender diversity and board size, I investigate whether the quota led to any effects on the financial performance of the affected companies as this is a popular potential outcome associated with increased diversity, see e.g. McKinsey & Company (2020).

The existing research on the Californian quota focuses on stock price reactions, documenting negative announcement returns for the companies affected, which decreases shareholder value.[7] However, these studies present several different explanations for the negative stock market reactions. Greene et al. (2020) and Hwang et al. (2018) argue that the negative stock price returns are due to a limited supply

[4]The Equal Protection Clause of the Fourteenth Amendment of the U.S. Constitution states that individuals in similar situations should be treated equally by the law (Legal Information Institute - Cornell Law School, 2021). The lawsuits claim that the quota violates this clause by discriminating based on gender.

[5]The sanctions in Norway implied that if a company failed to comply with the 40-percent quota, it would face forced dissolution, see more details in Bertrand et al. (2019). In California, the sanctions consist of potential financial penalties, however, no fines have been imposed as of October 2021. See more details in Section 3.

[6]Compliance with the current quota requirements is achieved if the company has at least one woman on the board of directors for at least a portion of the calendar year (California Legislative Information, 2018).

[7]See Gertsberg et al. (2021), Greene et al. (2020), Hwang et al. (2018), and von Meyerinck et al. (2021).

of female directors, which can lead to higher search costs related to finding qualified candidates or suboptimal directors being appointed. In contrast, von Meyerinck et al. (2021) suggest that the negative returns are not explained by a lack of female director candidates but rather by shareholders' disapproval of government interventions. Furthermore, studying shareholder support for director nominees, Gertsberg et al. (2021) find that firms can access a large enough number of qualified female candidates and show that the firms that experience a negative stock price reaction are those who do not replace the director that receives lowest support in shareholder meetings with a female director. These studies do not, however, investigate whether actual financial performance, measured in e.g. operating revenues or firm profitability, is affected.

The effectiveness of the quota in increasing the share of female directors on the boards of the affected companies is explored empirically to some extent by the aforementioned studies, documenting positive effects.[8] However, since the quota is recent, the data used by von Meyerinck et al. (2021), Greene et al. (2020), and Hwang et al. (2018) only spans three, six, and eighteen months after the quota went into effect, respectively, and the analyses are made based on comparisons between two periods. Since the deadline for compliance with the quota requirements is still ahead, it is interesting to explore the effects of the quota on the share of women on the boards of the affected companies further. Moreover, Hwang et al. (2018) and von Meyerinck et al. (2021) only analyze companies included in the Russell 3000 Index and the BoardEx database,[9] resulting in samples of 405 and 458 firms, respectively. However, as pointed out by Greene et al. (2020), considering all firms affected by the quota is important as smaller firms not included in major stock indices often have fewer female directors. Therefore, my thesis aims to build on the existing research on the Californian board quota, exploring more recent data. I also consider the full sample of firms affected by the quota as defined by California Secretary of State (2021).

To answer my research questions, I build a dataset consisting of company-level panel data for the period 2017-2020 for both publicly held companies headquartered in California and publicly held companies headquartered in other states that act as the control group. I employ both a traditional Difference-in-Differences strategy as well as a Difference-in-Differences strategy combined with Propensity Score Matching to enhance the comparability between the two groups of companies. Comparing the difference in outcomes before and after the quota in Californian companies to the difference in control companies, I find that the quota had a significant and positive effect on board gender diversity using both strategies. Furthermore, I observe positive effects for both board size and number of female directors on the board, however, the trends in these variables before the quota was applied were slightly different between the two groups. This can be interpreted as evidence against the plausibility of the key identifying assumption of my empirical strategy, the parallel trends assumption, and hence, these results should be interpreted with caution. I do not either find reliable evidence

[8]However, the analysis of Gertsberg et al. (2021) is merely descriptive and the results of Greene et al. (2020) cannot necessarily be interpreted as causal evidence.

[9]The Russell 3000 Index includes the 3,000 largest publicly traded firms in the United States. I was not able to find detailed information on how BoardEx chooses its firm coverage.

of the effects of the quota on financial performance. This is likely due the fact that the quota is so recent. Furthermore, as the Covid-19 pandemic coincides with the last year of my period of analysis, potentially affecting Californian companies differently from companies headquartered in other states, it is difficult to disentangle any potential effects of the quota.

The rest of this thesis is structured as follows. In Section 2, I summarize the existing literature on gender quotas on corporate boards and in Section 3, I provide background information on the institutional setting of the Californian quota. In Sections 4 and 5, I describe the data as well as the empirical strategy and methods. In Section 6, I present the results, in Section 7, I provide robustness tests, and in Section 8, I discuss the implications of my results as well as their validity and generalizability. Finally, in Section 9, I provide concluding remarks and suggestions for future research.

2. Literature Review

It is a highly popularized claim that organizations led by a more gender-diverse group make different decisions and achieve superior outcomes.[10] However, much of the research on the effects of female leadership in the corporate sector is observational and does not establish a causal relationship. Many studies examining the effects of female leaders and directors are based on correlations and are difficult to interpret as gender diversity in leadership positions is endogenous (Matsa & Miller, 2013). Similarly, numerous empirical studies suggest a positive relationship between female leadership and female labor market outcomes such as female employment or gender wage gaps (Maida & Weber, 2020). However, these studies are often either based on cross-sectional analysis or on models with firm fixed effects and cannot necessarily be interpreted as causal (Maida & Weber, 2020). Hence, as gender quotas on corporate boards have increased in popularity, a growing body of research has started to exploit them as a source of exogenous variation in gender diversity in leadership (Baltrunaite et al., 2021).

In the following sections, I summarize previous literature on gender quotas on corporate boards. First, I discuss the existing evidence of the effectiveness of quotas in achieving their mandate of increasing the share of women on the board of directors as well as the effects of quotas on female labor market outcomes. Second, I present literature on the effects of quotas on board member qualifications and third, on firm policies and performance.

2.1 Female Representation and Female Labor Market Outcomes

Existing research on gender quotas on corporate boards suggests that they tend to increase female representation on corporate boards if they are designed properly, i.e. there are sufficient sanctions for

[10] For example, during her time as the Managing Director of the International Monetary Fund, Christine Lagarde famously said: "[...] if it had been Lehman Sisters rather than Lehman Brothers, the world might well look a lot different today" (Lagarde, 2018).

non-compliance.[11] There is however a fair amount of variation in the design of existing board quotas, e.g. in terms of hardness of sanctions and deadlines for compliance as well as the mandated share of women on the board. Moreover, cultural differences could lead to differential reactions to board quotas. Hence, the results from one country cannot necessarily be generalized to another.

However, whether or not a quota has effects beyond only mechanically increasing the number of women on the board is an important consideration for the success of the quota policy (Bertrand, 2018). Studies examining whether board quotas have effects on the labor market outcomes of women appointed to the board post-quota, as well as women outside the board, often use registry data, which allows researchers to study the career development of all women employed in the companies subject to a quota. In the Norwegian context, Bertrand et al. (2019) find that as the share of women on the boards increased after the quota, the gender gap in earnings within boards fell considerably. They note that this finding can be seen "nearly as a necessary condition for the hope of any positive spillovers of the quota policy beyond its mechanical effect" (Bertrand et al., 2019, p. 228). However, the authors do not find reliable evidence of benefits for other women employed in the companies affected by the quota. In a similar fashion, Maida and Weber (2020) find that while the Italian board quota substantially increased the share of women on the boards of directors, there is no evidence of spillover effects on the representation of women in top executive or top earning positions.

2.2 Board Member Qualifications

A popular argument against board quotas is that directors should be appointed based on merits rather than gender. Moreover, a common concern raised in the context of board quotas is that if the lack of female representation is due to a lack of qualified female candidates, mandating a certain number of board seats to be taken by women will lead to the appointment of female directors with lower qualifications. This could further lead to a decrease in firm value and performance as the quota would result in suboptimal boards being appointed (Baltrunaite et al., 2021). This argument is brought up by e.g. Hwang et al. (2018) in the context of California. In contrast, if the current underrepresentation of women is due to discrimination, enlarging the pool of director candidates through a quota should lead to increased quality of director candidates; "all should agree that an economy that is tapping into a limited pool (men) to find its leaders must be operating inside the efficiency frontier" (Bertrand, 2018, p. 208).

The existing evidence suggests that board quotas do not seem to decrease the "quality" of directors. In fact, the findings of e.g. Bertrand et al. (2019) and Baltrunaite et al. (2021) point to the opposite direction. In the case of Norway, Bertrand et al. (2019) find that the new female directors appointed after the quota were more qualified than their female predecessors, measured in several different ways. The new directors had higher education, higher earnings, and greater board-specific human capital. Similarly, Baltrunaite et al. (2021) find that an Italian board quota affecting state-owned enterprises

[11] The same applies for political gender quotas, see e.g. Campa and Hauser (2020).

resulted in new female directors replacing older and "less talented" men, leading to an increased quality of the board of directors. The authors construct a measure of director quality based on their ability to increase the total factor productivity of the firm. These findings also coincide with those on political gender quotas, see e.g. Besley et al. (2017) in the context of Sweden.

However, it should be kept in mind that it is difficult to define what constitutes a "qualified" director. Previous research mainly uses ex-ante definitions of director quality, such as education and previous experience, as proxies. In contrast, in a recent working paper, Gertsberg et al. (2021) utilize shareholder voting results for director nominees to directly measure shareholders' perceptions of director quality. In the Californian context, the authors find that pre-quota, female director nominees receive greater shareholder support than male nominees, which they interpret as female nominees being held to a higher standard relative to male nominees. After the quota, support for incumbent female nominees remains higher than support for incumbent male nominees, but support for new female nominees decreases to the same level as support for new male nominees. According to the authors, this implies that the bar to become a board nominee for men and women became more similar and that the new female nominees were not of lower "quality" compared to male nominees.

2.3 Firm Policies, Firm Value, and Firm Performance

From the outset, it is not clear whether gender quotas on boards of directors affect company performance positively or negatively (Baltrunaite et al., 2021). On the one hand, greater gender diversity may be beneficial for firm performance and firm value as new female directors may bring specific skills and expertise lacking from male-dominated boards, see e.g. Kim and Starks (2016). On the other hand, as described in the previous section, interference with firms' unconstrained selection of board members could lead to suboptimal boards being appointed, affecting firm performance negatively.

It is not either evident whether board quotas would affect firms' policy decisions (Matsa & Miller, 2013). First, even though there are widely-documented gender differences in the general population, see e.g. Croson and Gneezy (2009), these differences may look different among the individuals who reach the top of the corporate hierarchy.[12] "If women must be like men to break the glass ceiling, we might expect gender differences to disappear among directors" (Adams & Funk, 2011, p. 219). Studying gender differences within boards of directors, Adams and Funk (2011) use a large survey of directors of publicly traded companies in Sweden and show that there are systematic gender differences in values and risk attitudes. However, these gender differences differ to some extent from those found in the general population; in contrast to the findings for the general population, the authors find that female directors are less "tradition and security oriented" and more risk loving than male directors. Second, gender differences in preferences do not necessarily lead to gender differences in policy decisions. For example, in an experimental setting where participants vote for a redistribution policy for their laboratory "society," Ranehill and Weber (2017) observe substantial

[12] Women and men in top corporate positions can however also be different due to other reasons than gender.

gender differences in voting behavior. Women vote for significantly more egalitarian redistribution policies when decision-making is individual. The authors also observe gender differences in voting behavior between female- and male-led groups in a collective decision-making setting, however these differences are much smaller. Hence, it could be the case that individual preferences disappear when aggregated to collective outcomes.

Based on the existing empirical evidence, it is not clear whether gender quotas affect company performance positively or negatively. In the Norwegian context, earlier studies, most notably Ahern and Dittmar (2012) and Matsa and Miller (2013), document negative effects of the quota on firm performance. According to Ahern and Dittmar (2012), this was because firms were already choosing boards to maximize shareholder value before the quota, while Matsa and Miller (2013) suggest that the reason is that women undertook fewer work-force reductions, leading to lower short-term profits. However, in a recent study, Eckbo et al. (2021) find no significant effects on market valuation or firm performance. The authors argue that this is because there was a sufficient number of qualified female director candidates for firms to comply with the quota requirements. In Italy, Baltrunaite et al. (2021) do not find any significant effects on productivity of state-owned enterprises after the quota, however, they find that profitability increases and leverage decreases, decreasing corporate credit risk. Ginglinger and Raskopf (2021) extend the analysis of the effects on financial performance to those on environmental and social (E&S) performance in the context of the French board quota. The authors find positive effects on firms' E&S performance, and that these effects are robust to controlling for director characteristics and values. Furthermore, post-quota, firms are more likely to create an E&S committee and female directors are members and chairs of major committees more often.

3. Institutional Setting

Before I go more in depth into the empirical strategy and the data employed to estimate the effects of the Californian quota, it is useful to know some background information on the quota legislation itself. On September 30, 2018, California became the first state in the United States to legislate a mandatory gender quota on boards of publicly listed companies when Governor Jerry Brown signed Senate Bill No. 826 into law.[13] The quota legislation requires all publicly held companies in California to have at least one female director on the board of directors by December 31, 2019. Furthermore, boards with five members have to have at least two women and boards with six or more members at least three women by December 31, 2021, respectively. See Table 1 for an overview of the quota requirements.

More specifically, the Californian board quota is applied to all publicly held domestic (U.S.) and foreign registered companies with a principal executive office located in the state of California (California Secretary of State, 2021). The principal executive office is identified in the company's 10-K

[13] See California Legislative Information (2018).

filing[14] with the markets regulator United States Securities and Exchange Commission (SEC) and in practice, it is the headquarters of the company (Greene et al., 2020). A company can be incorporated and headquartered in different states, and most publicly held California-headquartered companies are incorporated in Delaware.[15] Furthermore, the quota legislation makes a distinction between publicly held and publicly traded companies. The former is a subset of the latter and only publicly held companies are required to comply with the quota requirements (California Secretary of State, 2021).[16]

Table 1: Overview of the Quota Requirements

Total Number of Directors	Minimum Female Directors Required by End of 2019	Minimum Female Directors Required by End of 2021
4 or fewer	1	1
5	1	2
6 or more	1	3

Source: Author's creation based on California Secretary of State (2021).

Companies can comply by either replacing existing male directors with female directors or by adding new directors, increasing the size of the board. Compliance is determined based on the companies' annual Publicly Traded Corporate Disclosure Statement filed with the Secretary of State.[17] Publicly traded companies that are not publicly held are not required to meet the quota requirements but are required to file an annual Publicly Traded Corporate Disclosure Statement and may voluntarily report information on board diversity (California Secretary of State, 2021).

According to the quota legislation, companies that violate the quota requirements may face financial penalties. First violation of the quota comes with a 100,000-dollar fine and each subsequent violation with a 300,000-dollar fine (California Legislative Information, 2018). In this context, a violation is defined as the lack of female directors.[18] Hence, an all-male board with six directors at the end of 2021 would lack three female directors based on the quota requirements and receive an annual fine of 700,000 dollars, i.e. 100,000 dollars for the first lacking female director and 300,000 dollars each for the second and third lacking female directors (Greene et al., 2020).[19] Furthermore, failure to file information on the board of directors to the California Secretary of State is associated with a fine of 100,000 dollars. However, as of October 2021, the California Secretary of State has not imposed any

[14]The 10-K is a mandatory report that all public companies in the U.S. have to submit annually to the Securities and Exchange Commission. The 10-K includes information on e.g. company structure and financial performance.

[15]This is most likely due to the beneficial corporate tax policies in the state.

[16]*Publicly held companies* are defined as companies that have listed shares on the three major U.S. stock exchanges: the New York Stock Exchange (NYSE), the National Association of Securities Dealers Automated Quotations (NASDAQ), and the NYSE American. In contrast, *publicly traded companies* are a larger group that, in addition to publicly held companies, also includes companies with securities listed on the Over the Counter (OTC) Bulletin Board and on the electronic service operated by OTC Markets Group Inc.

[17]The Publicly Traded Corporate Disclosure Statement must be filed within 150 days from the end of a company's fiscal year. These filings can be accessed through the Business Search function on the Secretary of State's website.

[18]More specifically, the Bill states that "each director seat required by this section to be held by a female, which is not held by a female during at least a portion of a calendar year, shall count as a violation" (California Legislative Information, 2018).

[19]It is also possible that violating the quota comes with reputational penalties, which may be much larger than any financial penalties from non-compliance (Hwang et al., 2018).

fines for violations of the quota requirements (Posner, 2021).

Even though the Californian board quota is the first legally binding initiative to increase the share of women on corporate boards in the United States, California has tried to address the disparities in female representation through a voluntary target before. In 2013, California introduced a non-binding gender diversity initiative (Senate Concurrent Resolution 62) that urged publicly listed companies to increase the share of women on their boards by the end of 2016 (California Legislative Information, 2018). However, only 20 percent of firms met these targets by the deadline (Hwang et al., 2018). The Senate Bill on a mandatory quota was introduced to the California Senate on January 3, 2018. However, the passage of the quota legislation was relatively unexpected in comparison with the European board quotas as California Governor Jerry Brown had not made any statements on his position on the legislation before signing the Bill into law on September 30, 2018 (Gertsberg et al., 2021). The law went into effect three months after the signing of the Bill on January 1, 2019.

4. Data

To study the effects of the Californian quota, I will compare companies affected by the quota ("treatment group") to those that are not ("control group"). As the quota only applies to a subset of publicly held companies in the United States, using publicly held companies headquartered outside California as the control group seems to be the most plausible option. Californian companies that are publicly traded but not publicly held, i.e. Californian companies that are listed but not on the three major U.S. stock exchanges, are another group unaffected by the quota, however, these companies are likely to be quite different from those listed on major stock exchanges. Furthermore, the data coverage on these firms is poor. Using private companies headquartered in California as the control group is not plausible either due to the lack of available data. Even though the resulting control group is not perfect, the companies in the treatment and control groups share an important characteristic, namely being publicly held. I discuss the differences in the characteristics of the two groups of companies as well as the implications of these differences in more detail in Sections 4.2 and 5.

Hence, I gather data on publicly held companies headquartered in California and outside California, respectively. The data comes from four sources. First, I use a list of companies impacted by the quota at the time of its implementation (in 2019) provided by the California Secretary of State to identify the companies belonging to the treatment group. Second, I use company accounting data from the Compustat North America and Snapshot databases from S&P Global Market Intelligence. I access this data through Wharton Research Data Services and refer to it as "Compustat" in the remainder of this thesis. Compustat is widely used within financial research and provides comprehensive data on e.g. income statements and balance sheets as well as company characteristics for public companies in the United States. Third, I collect data on board gender composition and size from the Refinitiv Eikon Environmental, Social and Governance (ESG) database. I refer to this data as "Eikon." The

Eikon ESG data is based on publicly available sources such as company websites, annual reports, and Corporate Social Responsibility reports. Moreover, I use Eikon as a source of additional data on company identifiers and dates of company Initial Public Offerings. Finally, I complement missing board data with hand-collected data from company filings with the SEC, which are publicly available through the Electronic Data Gathering, Analysis, and Retrieval (EDGAR) database.

4.1 Dataset and Exclusion Criteria

4.1.1 Treated Companies

4.1.1.1 Identifying Treated Companies

The office of the California Secretary of State is tasked with reviewing and issuing reports on compliance with the quota requirements (California Secretary of State, 2021). Their annual reports identify the companies affected by the quota based on data from company filings with the SEC. The March 2020 Women on Boards report ("WoB report") lists all publicly held companies that listed California as their principal executive office on their 10-K filed during 2019, which is the year the quota law went into effect. As this report is the most comprehensive list of companies affected by the quota and comes from an official source, I decide to use it as the basis for my research.[20]

According to the WoB report, the total number of publicly held companies with a California principal executive office on their 2019 SEC 10-K filing is 625. In addition to the names of the affected companies, the report contains information on the companies' California Entity Number, state of incorporation, address, phone number, as well as the name of the stock exchange on which the company is listed (NYSE, NASDAQ, or NYSE American). The report also discloses whether the companies reported compliance with the quota through their Publicly Traded Corporate Disclosure Statement. However, as the filing deadline for the Publicly Traded Corporate Disclosure Statement is later than that for the 10-K, there may be gaps in this data.[21] According to the report, 330 of the 625 affected companies filed a 2019 Publicly Traded Corporate Disclosure Statement and 282 reported compliance with the requirements for 2019.

The WoB report is in the form of a PDF file, which I have converted into an Excel file using Adobe Acrobat Pro. As the report does not include information on unique company identifiers used in other databases, I find the CUSIP number of each company to be able to merge the dataset with data from other sources. Both Compustat and Eikon include information on the CUSIP number. CUSIP refers

[20]It is worth to note that previous studies on the Californian quota do not use this list of companies, which leads to the samples being somewhat different.

[21]The Publicly Traded Corporate Disclosure Statement must be filed within 150 days from the end of a company's fiscal year whereas the 10-K must be filed within 60, 75, or 90 days from the end of the fiscal year, depending on the size of the company. The fiscal year is the 12-month period for which a company does its accounting. For most companies, the fiscal year coincides with the calendar year, however some companies choose not to have December 31 as the end of their fiscal year. The end of the fiscal year is usually determined when the company is incorporated and it is unusual to change the end of the fiscal year.

to the Committee on Uniform Securities Identification Procedures and the CUSIP number is a unique identifier used for identifying securities in the United States and Canada in a standardized way. The drawback of using CUSIP numbers is that they are not permanent, and a company can have several CUSIP numbers if it has several securities trading in the stock exchange. However, CUSIP numbers, in contrast to stock tickers, which are another common type of company identifiers, can never be reused for other companies. Therefore, I can use the CUSIP number to identify the treated firms in my dataset.

To match a company with a CUSIP number, I use data from the Compustat North America Fundamentals Annual database that contains financial and identifying information for both active and inactive public companies in the United States and Canada. I create a list of publicly traded companies headquartered in California with non-zero total assets from Compustat and match this list with the list of companies affected by the quota using the first seven letters of the company name.[22] I manually check that the matching leads to the correct company name being identified and correct any incorrect matches.[23] For missing values or companies that were incorrectly matched, I manually search for the CUSIP number from Compustat or Eikon. For companies that have changed the company name since 2019, I find the new name using Google. Refinitiv Eikon also contains information on the date of the name change. I can identify the CUSIP number for 624 of the 625 companies on the list. The only company for which I do not identify a CUSIP number is a company that files 10-K reports with the SEC but has, however, never been listed in any major U.S. stock exchange. Therefore, this company is incorrectly identified as an affected company in the report and the number of treated companies should be 624 instead of 625.

4.1.1.2 Identifying Board Composition for Treated Companies

The lack of readily available data on board gender diversity for the full set of companies affected by the quota poses a challenge for my analysis.[24] Refinitiv Eikon has firm-level historical data on board size and board gender diversity for a relatively large subset of the affected companies for at least some years during the period 2017-2021.[25] However, there are 132 companies for which I was not able to download even one datapoint during that period. Refinitiv does not provide clear information on what company characteristics its Eikon ESG data coverage is based on, however, I suspect that the data is not missing at random. I tackle this selection issue by complementing the data from Eikon with hand-collected data on board size and number of female directors from company filings

[22] Matching with the full name yields very few matches as the names of the companies would have to be spelled the exact same way in both datasets.

[23] The matching method yields incorrect results mainly for companies whose names start with common words such as "American," "California," or "Pacific."

[24] For example, the Directors Database from Institutional Shareholder Services, which is a common database used in the literature on corporate boards, includes data on characteristics of individual directors from annual shareholder meetings for the universe of companies on the S&P 1500 Index. However, this database covers only 219 of the 624 companies affected by the quota for at least one year during 2014-2020. Furthermore, the S&P Capital IQ database only contains information on the current board of directors and I do not have access to the BoardEx database.

[25] The data coverage is considerably worse after 2017.

with the SEC, which I can access through the EDGAR database. Both Greene et al. (2020) and Gertsberg et al. (2021) use data from SEC filings in their analysis.[26] I search for the filings using the company names listed on the WoB report. As the list of affected companies is based on data from SEC filings, the risk of identifying the wrong company based on its name is low. Some companies have changed the company name since 2019, which implies that the company filings are under the new name. Filings for the years before the name change include the old company name.

Information on the board of directors can be found either on the annual report (10-K) or the company's proxy statement (DEF 14A) filed ahead of the annual shareholder meeting. The filings include bibliographical information on the directors and use gendered prefixes (Mr./Ms.) or gendered pronouns from which I am able to identify whether the director is male or female. I collect the data on the firm-level and do not include any individual-level information on the directors in my dataset. Eikon reports data on board size and board gender diversity at the end of the company's fiscal year.[27] In contrast, the data from the 10-K filings reflects the situation at the time when the report was filed, which happens between two to three months after the end of the fiscal year, and the proxy statements reflect the situation at the time of the shareholder meeting. To ensure that the hand-collected data matches the data from Eikon, I check for information on board member appointments and resignations from the companies' 8-K filings where this type of information is reported under item 5.02.

4.1.1.3 Matching With Data on Other Firm Characteristics

Finally, I match the aforementioned data with accounting data and data on company characteristics from Compustat. I observe 623 of the 624 treated companies in the Compustat database for at least one year. After matching, I am able to observe 513 firms every year for the period 2017-2020 in both Compustat and Eikon. However, there are 10 companies that have missing datapoints for some important company characteristics for the first pre-treatment year (2017). As I will later use these variables to match treated companies to control companies to achieve a higher balance in company characteristics, I drop these companies from the dataset. The remaining sample consists of 503 companies that each have four yearly observations during the period 2017-2020, resulting in a balanced panel. I discuss the characteristics of companies included in and excluded from the sample due to missing data further in Section 4.3.

4.1.2 Control Companies

To identify a pool of potential control firms, I first download the full Compustat North America Fundamentals Annual database for the period January 2016 to October 2021. I include both active

[26] Greene et al. (2020) use data from company proxy statements filed ahead of the annual shareholder meeting to complement board data from Institutional Shareholder Services. Gertsberg et al. (2021) collect data from company proxy statements and 8-K filings to obtain information on characteristics of director nominees and election results from the annual shareholder meeting.

[27] The fiscal year usually ends on December 31. This type of data reporting implies that directors staying on the board for less than a year will not be captured in the data.

and inactive companies, which results in a sample of 13,884 companies and 57,276 observations in total. As not all companies are observed each year, the resulting panel dataset is unbalanced. I restrict the sample to publicly held companies headquartered in another state than California both before and after the quota. To identify these companies, I need historical data on company headquarters as well as information on the date of the Initial Public Offering (IPO)[28] and the stock exchange where the company is listed. However, a major limitation of the Compustat North America Fundamentals Annual database is that company identifying information is in the form of header data that only reflects current information. This implies, for example, that if a company has changed their headquarters, all historical values are updated, and it is not possible to observe the correct historical values. I tackle this problem by complementing the data with data from the Compustat Snapshot database that documents data as it was first reported. However, as this database lacks the latest available record for each company, I must combine the two datasets. If the data reported in Compustat Snapshot differs from the header data reported in Compustat Fundamentals Annual, I correct the header data with the correct historical information.

After correcting for eventual changes in company identifying information, I restrict the dataset to companies whose headquarters were in another U.S. state than California during 2016-2021. This implies that I drop companies that are listed in North American stock exchanges but are not headquartered in the U.S. or Canada, companies that are headquartered in Canada, and companies that are headquartered in the U.S. territories Guam, U.S. Virgin Islands, and Puerto Rico. This results in 6,018 companies being dropped. I further drop companies that did not have shares listed on the three major U.S. stock exchanges, resulting in a sample of 4,856 companies. For each company, I subset the most recent year of observation and drop companies that I do not observe in 2019 as they have e.g. become bankrupt or delisted. The resulting sample consists of 4,084 companies.

I want to further restrict this sample to companies that were publicly held in 2017, however I lack data on the date of the company's IPO for 1,901 companies. Therefore, I complement Compustat data with data from Eikon. I do not find records from Eikon for 93 of the 4,084 companies. Looking at the characteristics of these companies based on Compustat data, over a half of the missing records are inactive companies or companies that do not have any significant operations.[29] From the remaining set of 3,991 companies, I drop companies that do not have data on the date of their IPO,[30] companies that go through an IPO in 2017 or later, as well as companies that lack accounting data on Compustat. In the end, I have a set of 2,542 potential control companies for which I download data on board composition from Eikon.

Finally, I match these two datasets and end up observing 1,655 of the 2,542 companies every year

[28] The Initial Public Offering refers to the process of a private company turning into a public company through issuing shares that can be traded in a stock exchange.

[29] They have Standard Industry Classification (SIC) 9995.

[30] I lack data on the date of the IPO for 371 observations after complementing the Compustat data with data from Eikon. However, 90 percent of these observations are entities such as Exchange Traded Funds, Trusts, or Funds that are not counted as public companies and do not have any accounting data available on Compustat.

during 2017-2020 in both Compustat and Eikon, resulting in 6,620 observations. However, 22 companies lack datapoints for important company characteristics used in matching, so the final sample of control firms consists of 1,633 firms. I discuss the characteristics of companies included in and excluded from the sample due to missing board data in section 4.3.

4.1.3 Final Dataset and Variable Definitions

I combine the datasets of treated and control companies and the final dataset consists of 8,544 observations, four yearly observations for 503 companies in the treatment group and 1,633 companies in the control group. The variables included in the dataset are:

- *Board Gender Diversity*; defined as the percent of female directors on the board of the company.
- *Board Size*; defined as the number of directors on the board.
- *Female Directors*; defined as the number of female directors on the board.
- *Number of Employees*; measured in thousands.
- *Net Sales*; measured in million U.S. dollars. Net sales are the company's gross sales, which is the amount of billings to customers for regular sales, reduced by discounts, sales allowances, and returned sales. This variable reflects the revenues the company has from its regular operations.
- *Total Assets*; measured in million U.S. dollars.
- *Earnings Before Interest and Taxes (EBIT)*; measured in million U.S. dollars. This variable measures profitability of the company's operations.
- *Industry*; a six-digit code based on the 2017 definitions of the North American Industry Classification System (NAICS).[31] I create dummy variables of each sector that take the value one if the company belongs to the sector in question.
- *High Technology Firm*; defined as a dummy variable taking the value one if the company belongs to an industry with a considerably high share of employment within Science, Technology, Engineering, and Mathematics (STEM). The definition of a high technology industry is based on Goldschlag and Miranda (2016) who define 15 industries with the highest share of STEM employment. I decide to construct this variable as the share of high technology firms is most likely higher in California compared to other states and the NAICS industry classification does not capture this detail.[32]
- *State of Headquarters*; defined as the state in which the company's primary address is located. This can differ from the state of incorporation.

[31] The first two digits give the sector and there are 20 different sectors (United States Census Bureau, 2021). These 20 sectors can be aggregated further to 10 supersectors (U.S. Bureau of Labor Statistics, 2021).

[32] For example, Apple Inc. is considered as a manufacturing firm according to the NAICS sector classification.

- *Fiscal Year*; defined as the year for which the company does its accounting.
- *Fiscal Year End Month*; defined as the month in which the company's fiscal year ends.
- *Data Date*; defined as the date for which the data is reported and used as a unique identifier together with the CUSIP Number.
- *CUSIP Number*; unique identifier for a company.

4.2 Descriptive and Summary Statistics

I present summary statistics on pre-treatment (2017) company characteristics separately for the treatment and control groups in Tables 2 and 3. As we can see in Table 2, there is large variation in the variables for both groups. For the average company in the treatment group, the share of women on the board of directors is 14.27 percent. The average board size for the treatment group is approximately eight directors. The average company in the control group has slightly higher board gender diversity, 15.95 percent, and a board size of approximately nine directors. Furthermore, there seem to be large differences in company characteristics such as number of employees and total assets, with treated companies being smaller in size and having lower revenues on average.

In Table 3, I present the differences in the means of all variables between the treatment and control groups.[33] As we can see, a larger share of companies in the treatment group is active in the manufacturing and information industries and a smaller share in the finance industry compared to the control group. Moreover, there is a higher share of high technology companies in California compared to the other states. The two columns furthest to the right represent the t-statistic and p-value from a t-test for a significant difference in means, respectively. Many of the variables are statistically significantly different between the treatment and control groups. I will discuss the implications of these differences in company characteristics further in the empirical strategy.

Furthermore, I plot the development of the share of women on the board of directors (*Board Gender Diversity*) for both groups of companies in Figure 1. Despite the differences in the levels of gender diversity, the figure suggests that both groups of companies are experiencing a positive trend in board gender diversity. However, at the end of 2018, see the vertical line, the growth rate in board gender diversity for the treatment group increases. The control group seems to continue the positive development at the same rate as before.

[33] I present the industry data for the 10 NAICS supersectors.

Table 2: Summary Statistics on Pre-Treatment (2017) Characteristics

		Treated	Control
Board Gender Diversity	Mean	14.27	15.93
	Median	13.33	14.29
	Sd.	11.93	11.48
	Min	0	0
	Max	50.0	80.0
	N	503	1,633
Board Size	Mean	7.93	9.26
	Median	8	9
	Sd.	2.12	2.64
	Min	1	1
	Max	16	32
	N	503	1,633
Female Directors	Mean	1.21	1.54
	Median	1	1
	Sd.	1.07	1.19
	Min	0	0
	Max	5	6
	N	503	1,633
Number of Employees	Mean	6.92	14.49
	Median	0.62	2.38
	Sd.	23.88	43.62
	Min	0	0
	Max	263	566
	N	503	1,633
Net Sales	Mean	3,322	5,316
	Median	286	920
	Sd.	14,709	17,149
	Min	0	0
	Max	229,234	242,137
	N	503	1,633
Total Assets	Mean	10,844	17,827
	Median	536	2,130
	Sd.	91,223	109,407
	Min	0.148	1.64
	Max	1,951,757	2,533,600
	N	503	1,633
EBIT	Mean	663	762
	Median	11.7	120
	Sd.	3,967	2,818
	Min	-1,565	-5,392
	Max	61,344	52,778
	N	503	1,633

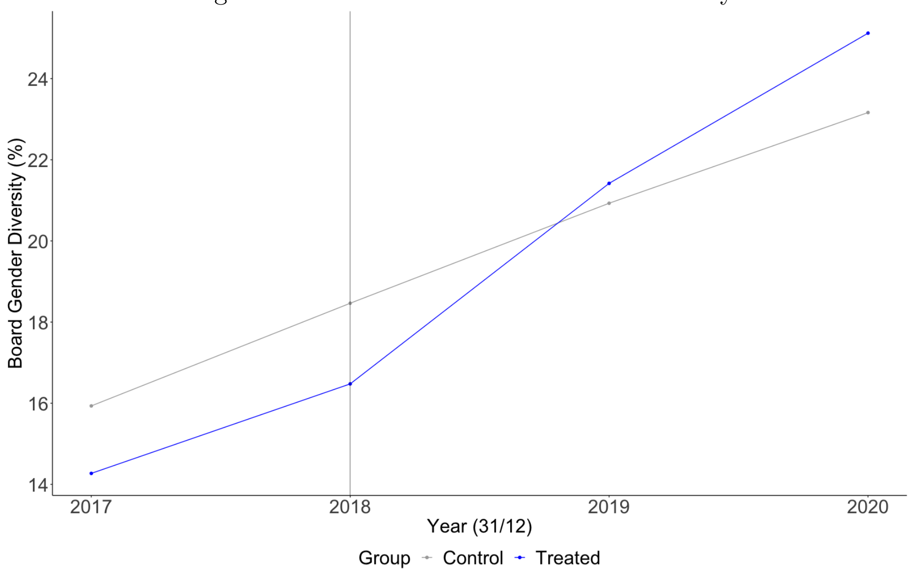
Note: *Board Gender Diversity* is measured in percent and *Board Size* and *Female Directors* in number of directors. *Number of Employees* is measured in thousands and *Net Sales*, *Total Assets*, and *EBIT* in million U.S. dollars.

Table 3: Differences in Pre-Treatment (2017) Characteristics

	Treated		Control				
	Mean	Sd.	Mean	Sd.	Diff.	t-Value	p-Value
Board Gender Diversity	14.27	11.93	15.93	11.48	1.66	2.76	0.006***
Board Size	7.93	2.12	9.26	2.64	-1.33	11.51	0.000***
Female Directors	1.21	1.07	1.54	1.19	-0.33	5.88	0.000***
Number of Employees	6.92	23.88	14.49	43.62	-7.57	4.99	0.000***
Net Sales	3,322	14,709	5,316	17,149	-1,995	2.55	0.011**
Total Assets	10,844	91,223	17,827	10,9407	-6,983	1.43	0.153
EBIT	663	3,967	762	2,818	-98.46	0.52	0.605
High Technology Firm	0.57	0.50	0.22	0.42	0.35	-14.17	0.000***
Natural Resources	0.01	0.09	0.03	0.18	-0.02	4.39	0.000***
Construction	0.01	0.10	0.02	0.13	-0.01	1.32	0.188
Manufacturing	0.51	0.50	0.36	0.48	0.15	-6.14	0.000***
Trade, Transport & Utilities	0.05	0.21	0.12	0.33	-0.07	6.02	0.000***
Information	0.17	0.37	0.08	0.26	0.09	-5.09	0.000***
Financial Activities	0.17	0.38	0.30	0.46	-0.13	6.31	0.000***
Prof. & Business Services	0.05	0.21	0.04	0.20	0.01	-0.45	0.656
Education & Health	0.02	0.13	0.02	0.14	0.00	0.25	0.803
Leisure & Hospitality	0.02	0.13	0.03	0.16	-0.01	1.19	0.236
Other Services	0.00	0.00	0.00	0.05	0.00	2.00	0.045**

Note: *** p<0.01, ** p<0.05, * p<0.1. *High Technology Firm* is a dummy variable taking the value one if the company belongs to an industry with considerably high STEM employment. *Natural Resources*, *Construction*, *Manufacturing*, *Trade, Transportation, and Utilities*, *Information*, *Financial Activities*, *Professional and Business Services*, *Education and Health Services*, *Leisure and Hospitality*, and *Other Services* are based on NAICS 2017 supersectors, see U.S. Bureau of Labor Statistics (2021), and are dummy variables taking the value one if the company is included in the industry.

Figure 1: Evolution of Board Gender Diversity



Source: Author's creation.
Note: The figure plots the means of the variable *Board Gender Diversity* for both treated and untreated companies at the end of each year. The quota is implemented on January 1, 2019, and is represented by the vertical line. There are 503 companies in the treatment group and 1,633 companies in the control group, respectively.

4.3 Data Limitations

I identify three main limitations to my dataset. First, both Eikon and Compustat report data at the end of the fiscal year of each company, which does not coincide with the calendar year for a subset of companies. Second, not all treated and potential control companies can be observed during the whole period between 2017-2020. Third, I lack historical data for a longer period than two years before the quota was implemented in 2019.

Starting with the first limitation, both Compustat and Eikon report data at the end of the fiscal year of each company, which does not coincide with the calendar year for a subset of companies. Of the companies in the final dataset with both treated and control companies, 11.6 percent do not end their fiscal year between the end of October and the end of March. Moreover, 19.1 percent of the companies in the sample do not end their fiscal year at the end of December. There are also eight companies that change their fiscal year end during the four years. I plot the evolution of *Board Gender Diversity* excluding the companies that do not end their fiscal year between October and March and those that change their fiscal year in Figure 5 in Appendix A. The trends look similar, however, both treatment and control companies have slightly lower board gender diversity. I present the same plot excluding companies that do not end their fiscal year in December and those that change their fiscal year in Figure 6 in Appendix A. The same conclusions apply in this case as well.

The fact that all companies in the sample do not report data at the end of December each year could lead me to estimate the magnitude of the effect of the quota incorrectly. On the one hand, companies that ended their fiscal year of 2019 between June and November 2019 might not have increased gender diversity by the end of their fiscal year since they were not required to do so. They could have still appointed a female board member between the end of their fiscal year and December 31, 2019 to be compliant with the requirements, and I would observe this only the following year. On the other hand, companies that ended their fiscal year of 2019 between January and May 2020 were actually already on their second post-treatment year, which could lead me to overestimate the effect of the quota. I will investigate how excluding these firms affects my results in the form of robustness tests, however, based on the aforementioned plots, the evolution of *Board Gender Diversity* looks similar even excluding these firms.

The total number of treated firms in 2019 is 624, however, after data collection, the remaining sample of treated firms consists of 503 companies. The companies that I do not observe for the whole period are companies that were not public before the quota was applied, and hence, lack data before 2019, or companies that were delisted from a stock exchange after the quota was applied as they e.g. got acquired, underwent a merger, or became bankrupt. Some of the companies for which I lack data are also so called "Special Purpose Acquisition Companies," which do not have any significant business operations and are established to acquire or merge with another company. I compare the characteristics of companies that are included in the sample of treated companies (503) to those that are excluded due to missing data (121) in Table 13 in Appendix B. I do not have full coverage of both Eikon and Compustat data for the full sample of companies for any of the four years during my period of analysis, but the data coverage is the highest in 2018. The companies that I do not have data for are less gender diverse, have smaller boards, and are smaller in size. Even though the differences are in some cases quite large, I argue this should not be a serious concern as it is more interesting to identify the effect for the companies that stay in business for a longer period of time. The boards of these companies may actually have an impact on the company's operations, in contrast to the companies I do not have data for. I also compare the characteristics of companies included in the control group (1,633) with the characteristics of companies excluded from it due to missing data (904), see Table 14 in Appendix B. I use 2018 as the comparison year to maintain consistency with the analysis for the treated companies. The means of the outcome variables are not statistically significantly different between the two groups, but the means of some company characteristics are, based on a two-sided t-test. However, as I want to identify the control companies that are the closest to the pool of treated companies, I do not see this as a serious concern.

A third limitation to my analysis is the lack of historical data. There are too many missing values in 2016 for that year to be included in the analysis without complementing the missing data with hand-collected data. Moreover, the data coverage in 2016 is significantly poorer compared to the period 2017-2020. However, I can still investigate whether companies in the control and treatment groups had similar pre-trends based on two pre-treatment years.

5. Empirical Strategy

5.1 Identification Strategy

5.1.1 Difference-in-Differences

A fundamental problem with estimating the effects of the quota legislation, and policies in general, is that we are not able to observe the potential outcome of what would have happened if the Californian companies were not subject to the quota (Rubin, 1974). There are various ways to tackle this problem, however in this thesis, I will use a Difference-in-Differences (Diff-in-Diff) strategy. This follows previous literature on the effects of corporate board quotas.[34] As the quota only applied to a subset of companies in the United States, I am able to use the Difference-in-Differences strategy to create a potential outcome for the companies affected by the quota using the outcome of companies that were not. Hence, this strategy can be used to identify the Average Treatment Effect on the Treated (ATT). In the context of the Californian board quota, treatment refers to being eligible for and subject to the quota and treatment is not randomly assigned as it is determined by the location

[34]See e.g. Baltrunaite et al. (2021) and Matsa and Miller (2013).

of the company's headquarters and by whether the company is publicly held.

In the context of my thesis, a Difference-in-Differences estimator compares the change in outcomes before and after the introduction of the quota in the companies affected by the quota to that in companies not affected by the quota, which deals with the fixed differences in outcomes between the two groups and between different years. In other words, it allows the treatment and control groups to start from different levels of the outcome variable. In contrast, a before-after estimator, comparing the differences in the outcomes of the companies affected by the quota before and after its implementation, would ignore the fact that the outcome variable may be on a long-term trend. The existence of a long-term trend in board gender diversity seems very likely based on Figure 1. Similarly, a treatment-control estimator, comparing the post-quota outcomes of the companies affected by the quota and those not affected by it would ignore the fact that the outcomes of the two groups could have looked different even without the quota.

An important consideration for the Difference-in-Differences strategy is the choice of the control group to which I will compare the companies affected by the quota as the two groups should be sufficiently similar. For example, in the previous literature, Baltrunaite et al. (2021) compare state-owned companies affected by the Italian board quota to companies with public minority share of ownership that were not affected while Matsa and Miller (2013) compare Norwegian listed companies affected by the quota to matched samples of unlisted firms in Norway as well as listed and unlisted firms in other Nordic countries. As described previously, I use publicly held companies headquartered in other states as the control group for Californian companies. In the existing research on the Californian board quota, e.g. Greene et al. (2020) compare Californian firms to matched control firms based in other states, however excluding firms headquartered in states that have, like California, voted for a Democratic Presidential candidate in the past five elections to account for the fact that states sympathetic to California may enact similar laws. Hwang et al. (2018) compare Californian firms included in the Russell 3000 Index that have a higher discrepancy between the pre-quota number of female directors and the quota-mandated number of female directors to non-Californian firms on the Russell 3000 Index, but do not perform matching or exclude any states from their analysis.

Even though the Difference-in-Differences strategy does not require that the treatment and control groups have the same levels of the outcome variable pre-treatment, they cannot have different trends in the outcome variable. In other words, to be able to interpret the Difference-in-Differences estimator as causal, we have to assume that in the absence of treatment, the two groups would have developed in a similar way. This is called the parallel trends assumption and it is the key identifying assumption of the Difference-in-Differences strategy (Angrist & Pischke, 2009). If the assumption holds, then a difference in the trend of the treated group from that of the control group can be interpreted as the causal effect of the treatment. It is not possible to directly test for the parallel trends assumption, as we cannot observe the potential outcome of not being treated for the treatment group after the quota was implemented, however, it can be visually inspected from plotting the evolution of the outcome variable for the two groups using data on multiple periods (Angrist & Pischke, 2009). This plot can be seen in Figure 1. As we can see, the slopes of the curves for the treatment and control groups look fairly similar in the pre-treatment period, which suggests that the parallel trends assumption seems plausible. However, even though the trends for the treated and control groups looked similar during the pre-treatment period, the two groups may still systematically differ from each other due to the non-random assignment of the quota. Even though the Difference-in-Differences strategy does not require that the treatment and control groups were similar in levels of characteristics, the fact that they were different before the quota was implemented makes the parallel trends assumption somewhat less plausible. It could be the case that the companies differ in characteristics due to a trend that is affecting them differently.

As we could see in Table 3, there are many company characteristics that are statistically significantly different between the two groups. Also, *Board Gender Diversity*, *Board Size*, and *Female Directors* differ. Due to these differences and as I do not have historical data for a period longer than two years before the implementation of the quota, I decide to complement my analysis using a combination of Difference-in-Differences and Propensity Score Matching based on pre-treatment company characteristics to ensure that the treatment and control groups are more comparable.

5.1.2 Difference-in-Differences and Propensity Score Matching

For the reasons detailed above, I use Propensity Score Matching to restrict the control group to firms that are more similar to those in the treatment group. The idea behind Propensity Score Matching is to predict the probability of being treated for each observation in the sample based on its pre-treatment characteristics. These probabilities are called propensity scores and the estimated propensity scores are used to match companies in the treatment group to companies in the control group. More formally, the propensity score p_i is calculated as follows:

$$p_i = P(Treated_i = 1|X_i) \tag{1}$$

where X_i represents the variables used in estimating the propensity score. As the outcome variable is binary (a company can be either treated or untreated), the propensity score is often estimated using either probit or logit regression (Angrist & Pischke, 2009). The variables used in estimating the propensity scores should affect both the outcome variable and participation in treatment as well as be unaffected by the treatment or the anticipation of treatment (Caliendo and Kopeinig, 2008; Imbens, 2015). I use pre-treatment (2017) values of the variables used in estimating the propensity scores, which should not be affected by the quota that was implemented in 2019. When Propensity Score Matching is used by itself, without combining it with Difference-in-Differences, it builds on the assumption that the outcome variable must be independent of treatment conditional on the propensity score (Imbens, 2015). This assumption is often referred to as unconfoundedness or selection-onobservables. This is a very strong assumption, however, when Propensity Score Matching is combined with Difference-in-Differences, this assumption can be relaxed (Caliendo & Kopeinig, 2008).

After the propensity scores are estimated, observations in the treatment and control groups are matched based on their propensity scores. However, before matching, it is important to verify that there is common support, i.e. that for each observation in the treatment group, there is at least one observation in the control group with a similar propensity score (Heckman et al., 1997). For the matching strategy to be valid, all observations in the treatment group should be located inside the region of common support, which is the region where the distributions of the propensity scores for the treatment and control groups overlap. Observations in the treatment group with a higher propensity score than the highest score in the control group as well as observations in the treatment group with a lower propensity score than the lowest score in the control group have to be discarded from the analysis as these observations fall outside the region of common support. However, discarding observations from the treatment group can cause problems if the share of discarded observations is too large. The ATT is only estimated for the observations that fall inside the region of common support and hence, discarding a too large share of the treated observations can make the estimated effect unrepresentative of the treatment group as a whole. (Caliendo and Kopeinig, 2008; Imbens, 2015)

There are several different methods for performing matching based on propensity scores of which Nearest Neighbors and Kernel Matching are two common ones. The Nearest Neighbors method matches treated observations to a limited number of control observations and attempts to minimize the distance between the propensity scores (Blundell & Costa Dias, 2009). The matching can be done in the form of one-to-one-matching, where each treated observation is matched to the control observation with the nearest propensity score, or one-to-many-matching where each treated observation is matched to several control observations. If there is large variation in propensity scores, the resulting matches from the Nearest Neighbors method may be of lower quality. In contrast, the Kernel Matching method, which I will use in this thesis, matches a treated observation to a weighted average of all the observations in the control group. The weight assigned to each observation in the control group has an inverse relationship with the distance between propensity scores: the observations that are closer in terms of propensity scores get a higher weight (Blundell & Costa Dias, 2009). The Kernel weights w_i are calculated as follows (Heckman et al., 1997):

$$w_i = \frac{K(\frac{p_i - p_k}{h})}{\sum K(\frac{p_i - p_k}{h})} \tag{2}$$

where K is the Kernel function, p_i is the propensity score for the control observation i, p_k is the propensity score for the treated observation k, and h is the bandwidth parameter. The bandwidth refers to the interval over which observations in the control group get a positive weight. Increasing the bandwidth implies that observations with a more dissimilar propensity score will receive a positive weight. The bandwidth cannot however be too small as this could lead to no matches being identified (Caliendo & Kopeinig, 2008). After the matching is performed, one should confirm that the matching has resulted in the treatment and control groups being similar, i.e. that the characteristics used for matching are balanced between the treatment and control groups (Imbens, 2015). There should not be any significant differences in the means of the characteristics used for matching between the treatment and control groups.

5.2 Assumptions

In this section, I discuss two key assumptions, in addition to parallel trends, that are related to the validity of my results. I continue the discussion on potential threats to internal validity in Section 8.

In addition to the parallel trends assumption, there should not be any anticipatory effects of the quota. The Bill was signed on September 30, 2018, and previous literature argues that this was relatively unanticipated, see e.g. Gertsberg et al. (2021). I use 2019, which is the year when the quota was implemented, as the first treatment period. As the quota was legislated three months before January 1, 2019, there is a possibility that the 2018 values are affected by the quota. However, this would imply that the companies would have had to find a female board member and appoint her within three months or less. This seems quite unlikely as the treated companies are publicly held companies that have to e.g. follow stock exchange rules for the share of independent directors, i.e. directors that come from outside the company in question. Hence, the companies have limited opportunities to quickly appoint an executive officer or an employee as a director of the company. I will further investigate the plausibility of this assumption by regressing the outcome variable on an interaction between the pre-treatment year of 2018 and the treatment (Angrist & Pischke, 2009). The coefficient on this interaction should be statistically insignificant, otherwise the quota had effects before it was implemented in early 2019.

Furthermore, a threat for identifying the effect of the quota would be if other policies with a potential impact on board gender diversity were implemented during the period of analysis. The main concern in this case is if there were policies that would impact the treated firms in a different way compared to the control firms. I am not aware of any other gender-related policies targeted at Californian publicly held companies that were implemented during the period of analysis. However, certain other states have implemented such initiatives both before 2019, in 2019, and after. By October 2018, when the Californian quota was legislated, four states (Illinois, Massachusetts, Pennsylvania, and Colorado) had passed non-binding board gender diversity resolutions. These voluntary targets were introduced between 2015 and 2017 (Hwang et al., 2018), so they are unlikely to be problematic. However, after the introduction of the quota, four states (Maryland, Illinois, New York, and Washington) have enacted measures to increase gender diversity on corporate boards. The measures are however much "softer" than the Californian quota. In Maryland, Illinois, and New York, the measures imply that public companies have to disclose the number of women they have on the board. In contrast, Washington has enacted a 25-percent gender quota in June 2020, however, it does not involve financial penalties. Several other states (Hawaii, Massachusetts, Michigan, Ohio, New Jersey, and Pennsylvania) have reportedly considered similar legislation to California without taking any formal action.[35] As including these states in the analysis could lead to biased estimates, I test the robustness of my results by excluding control companies headquartered in these states. However, plotting the evolution of *Board Gender Diversity* excluding the four states that formally implemented gender diversity initiatives after the quota in Figure 7 and excluding all aforementioned states in Figure 8 in Appendix A, the trends between the two groups do not seem to differ, even though gender diversity is slightly lower in the control group when excluding the states in question.

5.3 Regression Models

5.3.1 Difference-in-Differences

To estimate the average effect of the quota on the treated companies, I first estimate the following simple Difference-in-Differences model:

$$Y_{i,t} = \alpha + \beta_1 Treated_i \times Post_t + \beta_2 Treated_i + \beta_3 Post_t + \epsilon_{i,t} \tag{3}$$

where $Y_{i,t}$ represents the outcome variable and β_1 is the Difference-in-Differences estimator that captures the effect of the gender quota. $Treated_i$ is a dummy variable that takes the value one for firms that are affected by the quota and zero otherwise and $Post_t$ is a dummy variable that takes the value one during the period after the quota was implemented (in 2019 and 2020) and zero during the period before implementation (in 2017 and 2018). $\epsilon_{i,t}$ is the error term. As the policy was defined at the state-level and I have data on individual firms, I cluster standard errors at the state level in all regression models (Abadie et al., 2017).

In a second model, I include company-level fixed effects. As including both company fixed effects and the $Treated_i$ dummy in the same equation would make the variables collinear, I exclude the $Treated_i$ dummy. I estimate the following model:

$$Y_{i,t} = \alpha + \beta_1 Treated_i \times Post_t + \beta_2 Post_t + \eta_i + \epsilon_{i,t} \tag{4}$$

where η_i represents company fixed effects. I do not include firm characteristics such as sales or firm performance as control variables as they could be influenced by the quota.

Furthermore, in a third model, I investigate whether the effect of the quota differs between different

[35]See Catalyst (2021b), Hatcher and Latham (2020), and The National Law Review (2020).

years, i.e. if there were any dynamic effects, by interacting the $Treated_i$ dummy with dummy variables for each year (2018_t, 2019_t, and 2020_t). I use 2017 as the baseline and estimate the following model:

$$\begin{aligned}Y_{i,t} = \alpha + \beta_1 Treated_i \times 2018_t + \beta_2 Treated_i \times 2019_t + \beta_3 Treated_i \times 2020_t + \beta_4 2018_t \\ +\beta_5 2019_t + \beta_6 2020_t + \eta_i + \epsilon_{i,t}\end{aligned} \tag{5}$$

I would expect the treatment effect to differ between the year the quota was implemented (2019) and the year after (2020) as many companies were already in compliance with the quota requirements for the end of 2019 (at least one woman on the board of directors). However, additional women are required by the end of 2021 for boards with five or more members. The average board size in the treated companies in 2017 was approximately eight. Furthermore, when it comes to company performance, we might expect there to be different effects immediately after implementation and two years after implementation (end of 2020). It could also be the case that it takes longer time than two years for the quota to have any potential impact on company performance.

Model 5 also allows me to investigate whether the quota was anticipated as I include a lead of the treatment by including the interaction term $Treated_i \times 2018_t$ (Angrist & Pischke, 2009). As mentioned before, a statistically significant coefficient on the interaction term $Treated_i \times 2018_t$ would suggest that the quota had effects before it was implemented. Furthermore, if the coefficient of the interaction term is statistically insignificant, this can be interpreted as evidence in support of the parallel trends assumption. I again include company fixed effects and cluster standard errors at the state level.

5.3.2 Difference-in-Differences and Propensity Score Matching

When combining Difference-in-Differences with Propensity Score Matching, I estimate the following model:

$$\triangle Y_{i,t} = \beta_1 Treated_i + \epsilon_{i,t} \tag{6}$$

where $\triangle Y_{i,t}$ is the difference between the 2020 and 2017 values of the outcome variable. I use the difference in outcomes over the whole period as the outcome variable as the Kernel Matching strategy does not allow for interaction terms (Segú, 2020). As the differencing already accounts for time-invariant characteristics, I do not include year fixed effects or company fixed effects in this model.

6. Results

In this section, I present and analyze the results from estimating the regression models specified in the previous section. In Section 6.1, I present the results from the Difference-in-Differences estimation without matching. Estimating models 3 and 4, I compare the change in outcomes from the pre-quota period to the post-quota period in companies affected by the quota (treatment group) to that in the companies not affected by the quota (control group). Hence, I capture the average effect of the quota for the two post-treatment years (2019 and 2020). Estimating model 5, I introduce interaction terms for each year in the sample period, using the first year as the baseline, to capture differences in the effect between years and to investigate potential anticipation of the quota. In Section 6.2, I present the results from the Propensity Score Matching analysis used to address the systematic differences between treated and control firms that could make the parallel trends assumption implausible, as well as the Difference-in-Differences estimation combined with Propensity Score Matching.

6.1 Difference-in-Differences

6.1.1 Board Gender Diversity

I present the results from estimating equations 3-5 using *Board Gender Diversity* as the outcome variable in Table 4. As we can see in column 1, the coefficient on $Treated_i \times Post_t$ is statistically significant and positive, suggesting that the quota led to a 3 percentage points higher increase in the board gender diversity of the treated Californian firms compared to control firms. The magnitude of the coefficient does not change when including company fixed effects in column 2.

The average board gender diversity in the treatment group over the two pre-treatment years (2017 and 2018) was approximately 15.4 percent, while for the control group, the corresponding figure was higher, 17.2 percent. This difference in the pre-treatment means is demonstrated by the negative coefficient on $Treated_i$. The average board gender diversity in the treatment group over the two post-treatment years (2019 and 2020) was 23.3 percent, while in the control group, it was 22.0 percent. This indicates that Californian firms had 1.3 percentage points higher share of female directors after the quota compared to the control firms, despite starting from a lower level. The board gender diversity of treated companies increased by 8 percentage points from pre-quota to post-quota and the positive coefficient on $Post_t$ suggests that board gender diversity increased by 4.8 percentage points from pre-quota to post-quota for control companies. The Difference-in-Differences estimator corresponds then to the difference between the difference in the post-treatment average board gender diversity and the difference in the pre-treatment average board gender diversity for the two groups.

In column 3, we can see that the coefficient on $Treated_i \times 2018_t$ is not statistically significant, suggesting that the quota did not have effects on board gender diversity before it was implemented.

Hence, it does not seem to be the case that board gender diversity is affected by anticipation by firms impacted by the quota. This result suggests that the parallel trends assumption seems plausible for the outcome variable in question. Further, the estimated coefficients on $Treated_i \times 2019_t$ and $Treated_i \times 2020_t$ suggest that the effect of the quota on board gender diversity is increasing over time. I find an average increase of 2.2 percentage points in 2019 compared to 2017 and in 2020, this effect was 3.6 percentage points. This seems to be consistent with the step-wise increase in the quota requirements, where higher gender diversity is required by the end of 2021 than by the end of 2019. The time dummies are positive and statistically significant, indicating an overall positive trend in board gender diversity during the period of analysis.

Table 4: Results - Board Gender Diversity

	(1)	(2)	(3)
Treated x Post	3.049***	3.049***	
	(0.279)	(0.279)	
Treated	−1.826***		
	(0.546)		
Post	4.848***	4.848***	
	(0.185)	(0.185)	
Treated x 2018			−0.325
			(0.220)
Treated x 2019			2.153***
			(0.375)
Treated x 2020			3.619***
			(0.387)
2018			2.531***
			(0.111)
2019			4.995***
			(0.210)
2020			7.230***
			(0.244)
Firm Fixed Effects	No	Yes	Yes
Number of Treated Firms	503	503	503
Number of Control Firms	1633	1633	1633
Observations	8,544	8,544	8,544
R^2	0.058	0.833	0.844
Adjusted R^2	0.058	0.777	0.792

Note: *p<0.1; **p<0.05; ***p<0.01

6.1.2 Board Size and Female Directors

I turn then to analyzing the effects on *Board Size* as well as on another measure of female representation, *Female Directors*. The results are presented in Tables 5 and 6. I estimate the same models as for *Board Gender Diversity*. If firms comply with the quota requirements, an increase in the number of female directors is expected. In contrast, the effect on the share of female directors is less obvious as it could be the case that firms only increase board size to comply with the quota requirements.

As we can see in Table 5, the coefficients on $Treated_i \times Post_t$ in both columns 1 and 2 are significant and positive, however small in size, indicating that Californian companies increased their board size slightly from the post-quota period relative to control companies. The average board size for the two pre-treatment years (2017 and 2018) was 8 members for the treatment group and 9.3 members for the control group, respectively. Again, the positive coefficients on $Post_t$ indicate that board size increased slightly in the control companies as well, but the change was larger for Californian companies. However, as we can see in column 3, even though the coefficient on the interaction term $Treated_i \times 2018_t$ is small, it is statistically significant at 5 percent. This indicates that treated companies increased their board size already in 2018, which can be interpreted as evidence against the plausibility of the parallel trends assumption for the outcome variable in question, which is why any conclusions should be made with caution. Based on the positive coefficients on the time dummies, board size seems to follow an increasing trend in both groups of companies.

In Table 6, I can observe a positive treatment effect for the outcome variable *Female Directors* and that the number of female directors is increasing in both treated and control firms. However, the coefficient on $Treated_i \times 2018_t$ is again statistically significant at 5 percent, which is indicative of the parallel trends assumption being less plausible for the outcome variable in question. Albeit very small in size, the coefficient is negative, indicating that the number of female directors at the treated companies did not increase as much as it did at the control companies between 2017 and 2018. It is not clear why this would be the case, and the result could be a false statistically significant finding. Furthermore, while it is true that the coefficient on $Treated_i \times 2018_t$ is significant, the coefficients on $Treated_i \times 2019_t$ and $Treated_i \times 2020_t$ are positive and much larger in magnitude, suggesting a sizeable jump in the number of female directors in the post-treatment period. However, as before, conclusions on these results should be made with caution.

It is also worth to note that the fact that the treated and control companies had slightly different trends during the pre-treatment period for the outcome variable *Female Directors* does not imply that the parallel trends assumption would not be plausible for the outcome variable *Board Gender Diversity*. As pointed out by Angrist and Pischke (2009), the parallel trends assumption is only valid for one transformation of a variable, e.g. if the parallel trends assumption holds in logs, this does not imply that it would do so in levels and vice versa. In this case, *Board Gender Diversity* measures the percentage share of female directors, whereas *Female Directors* measures the number of female directors. However, the fact that the parallel trends assumption seems less plausible for the outcome variable *Female Directors* is still a potential limitation of my analysis.

Table 5: Results - Board Size

	(1)	(2)	(3)
Treated x Post	0.216^{***}	0.216^{***}	
	(0.038)	(0.038)	
Treated	-1.283^{***}		
	(0.166)		
Post	0.080^{***}	0.080^{***}	
	(0.028)	(0.028)	
Treated x 2018			0.082^{**}
			(0.038)
Treated x 2019			0.231^{***}
			(0.051)
Treated x 2020			0.284^{***}
			(0.041)
2018			0.077^{**}
			(0.029)
2019			0.113^{***}
			(0.026)
2020			0.123^{***}
			(0.039)
Firm Fixed Effects	No	Yes	Yes
Number of Treated Firms	503	503	503
Number of Control Firms	1633	1633	1633
Observations	8,544	8,544	8,544
R^2	0.039	0.909	0.909
Adjusted R^2	0.039	0.879	0.879
Note:	*p<0.1; **p<0.05; ***p<0.01		

Table 6: Results - Female Directors

	(1)	(2)	(3)
Treated x Post	0.216^{***}	0.216^{***}	
	(0.021)	(0.021)	
Treated	-0.351^{***}		
	(0.064)		
Post	0.452^{***}	0.452^{***}	
	(0.016)	(0.016)	
Treated x 2018			-0.044^{**}
			(0.017)
Treated x 2019			0.135^{***}
			(0.028)
Treated x 2020			0.254^{***}
			(0.026)
2018			0.241^{***}
			(0.012)
2019			0.472^{***}
			(0.019)
2020			0.673^{***}
			(0.022)
Firm Fixed Effects	No	Yes	Yes
Number of Treated Firms	503	503	503
Number of Control Firms	1633	1633	1633
Observations	8,544	8,544	8,544
R^2	0.050	0.867	0.876
Adjusted R^2	0.050	0.822	0.834

Note: *p<0.1; **p<0.05; ***p<0.01

6.1.3 Financial Performance

As additional exploratory analysis, I investigate whether we can observe effects on the financial performance of the affected companies after the quota. I measure financial performance in terms of *Net Sales*, which measures a company's operating revenues (in million U.S. dollars) and *Earnings Before Interest and Taxes (EBIT)*, which measures a company's profitability from its regular operations (in million U.S. dollars). Previous literature suggests that the quota was followed by negative stock market reactions for the companies affected by the quota, decreasing firm value. Hwang et al. (2018) also suggest that research analyst expectations of earnings forecasts experienced a decline around the passage of the quota law. This does not however necessarily mean that firms' accounting performance would be affected. Moreover, it could take a lot longer than one to two years to observe any potential effects on firm profitability and revenues.

Looking at the time trends in these variables in Figures 2 and 3, the two groups of companies seem to follow similar positive trends before 2019, suggesting that the parallel trends assumption may hold. However, as pointed out already in Section 4, there are large differences in the levels of these variables between the treated and control firms, especially for *Net Sales.* These differences would further justify using matching to make the control firms more comparable to the treated firms. Given that the control firms are much larger in scale (sales) than the treated firms, they may also systematically differ along many other dimensions and react differently to time-varying shocks.

Figure 2: Evolution of Net Sales

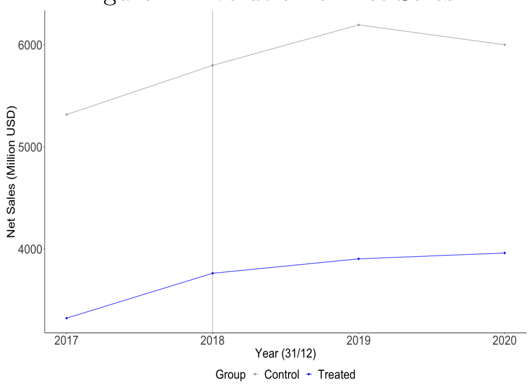
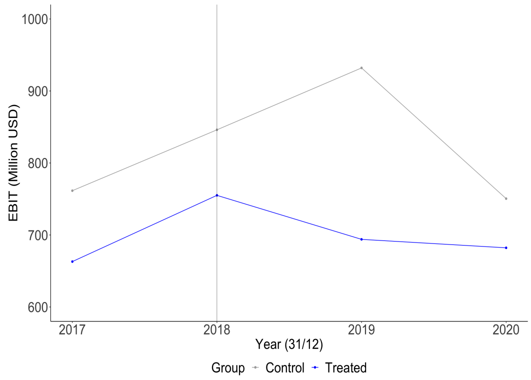


Figure 3: Evolution of EBIT



Source: Author's creation.
Note: The figures plot the means of variables *Net Sales* (in million U.S. dollars) and *EBIT* (in million U.S. dollars) for both treated and untreated companies at the end of each year, respectively. The quota is implemented on January 1, 2019, and is represented by the vertical line. There are 503 companies in the treatment group and 1,633 companies in the control group, respectively.

I present the results from estimating models 4 and 5 on outcome variables *Net Sales* and *EBIT* in Table 7. The coefficients on $Treated_i \times Post_t$ in columns 1 and 3 are negative for both outcome variables, however they are not statistically significant. In columns 3 and 4, I observe statistically significant negative coefficients on $Treated_i \times 2019_t$, however the coefficients on $Treated_i \times 2020_t$ are not statistically significant. As there does not seem to be a clear pattern in the results, the coefficients should not be over-interpreted. Furthermore, as the data is very noisy due to e.g. the outbreak of the

Covid-19 pandemic in 2020, which could have impacted firms heterogeneously, and there are likely to be other reasons for the decrease in financial performance for the treated firms, these results do not necessarily provide reliable evidence of the effects of the Californian gender quota.

Table 7: Results - Financial Performance

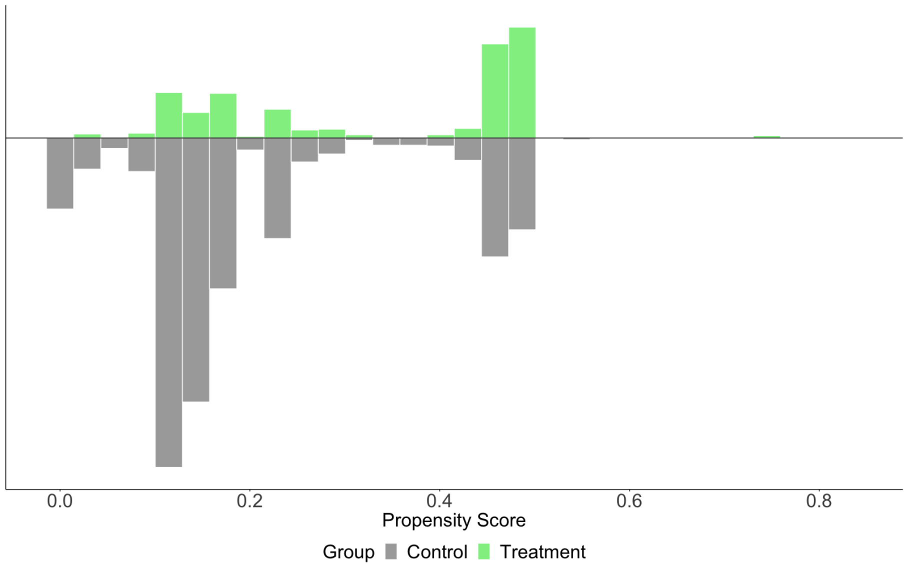
	Net Sales		EBIT	
	(1)	(2)	(3)	(4)
Treated x Post	-150.840		-58.472	
	(181.584)		(53.635)	
Post	540.540***		37.474	
	(178.592)		(51.915)	
Treated x 2018		-42.130		7.469
		(70.401)		(26.102)
Treated x 2019		-297.658*		-139.647**
		(158.533)		(57.181)
Treated x 2020		-46.153		30.171
		(259.667)		(53.880)
2018		482.093***		84.550***
		(74.231)		(24.925)
2019		878.705***		170.536***
		(157.035)		(56.719)
2020		684.469***		-11.038
		(253.193)		(51.623)
Firm Fixed Effects	Yes	Yes	Yes	Yes
Number of Treated Firms	503	503	503	503
Number of Control Firms	1633	1633	1633	1633
Observations	8,544	8,544	8,544	8,544
R^2	0.974	0.974	0.915	0.915
Adjusted R^2	0.966	0.966	0.887	0.887
Note:				*p<0.1; **p<0.05; ***p<0.01

6.2 Difference-in-Differences and Propensity Score Matching

As a first step in the Propensity Score Matching analysis, I estimate the probability of being treated (i.e. having been a publicly held company headquartered in California in 2019) given a set of company characteristics on their 2017 levels using a logistic regression. I use 2017 values instead of 2018 values to ensure that the variables are not affected by anticipation of treatment. The variables I use in the estimation of the propensity score are *Net Sales*, *Number of Employees*, *Total Assets*, *Earnings Before Interest and Taxes (EBIT)*, a dummy variable indicating whether the company is a high technology company (*High Technology Firm*), and a set of sector dummies.[36] From this estimation, I obtain a propensity score between 0 and 1 for each observation in the treatment and control groups, which indicates the likelihood with which the observation would have been assigned treatment given its characteristics.

As described in more detail in Section 5.1.2, for the matching strategy to be valid, there needs to exist common support, which implies that for each observation in the treatment group, there must be at least one observation in the control group with a similar propensity score. This can be inspected by plotting the distribution of the estimated propensity scores for the treatment and control groups, see Figure 4. The x-axis represents the estimated propensity scores (the probability of being treated) and the y-axis the frequency of companies with a specific estimated propensity score. As we can see, the distribution of the propensity scores of the treatment group overlaps with that of the control group to a large extent, however there are five observations in the treatment group that fall outside the region of common support. I identify these companies,[37] and as they cannot be seen as representative of the average publicly held company in California based on their characteristics, discarding these observations from the analysis is unlikely to cause issues for estimating the ATT.

Figure 4: Distribution of Propensity Scores by Treatment Status



Source: Author's creation.
Note: The y-axis plots the frequency of the companies according to the estimated propensity score. The sample consists of 503 companies in the treatment group and 1,633 companies in the control group.

[36] I use a set of 20 dummy variables based on NAICS sectors, see United States Census Bureau (2021). However, I omit four sectors (*Management of Companies and Enterprises*, *Other Services (except Public Administration)*, *Transportation and Warehousing*, and *Public Administration*) as the first three do not include any firms in the treatment group and there are no firms included in the NAICS sector *Public Administration*.

[37] The companies that fall outside the region of common support are Apple Inc., three companies active within the NAICS sector *Agriculture, Forestry, Fishing and Hunting*, and one company active withing the NAICS sector *Administrative and Support and Waste Management and Remediation Services*. Apple has the highest *Net Sales* and *EBIT* in the whole treatment group. Furthermore, there are only four companies in the whole sample of treated and control companies that are active in the sector *Agriculture, Forestry, Fishing and Hunting*, of which three are located in California. The company within the NAICS sector *Administrative and Support and Waste Management and Remediation* has almost ten times the average number of employees compared to other treated companies in the same sector.

After estimating the propensity scores, I match treated companies to control companies using Kernel Matching on the propensity scores and estimate the Difference-in-Differences treatment effect. I use the Stata command *diff* that allows me to incorporate Kernel propensity score weights to Difference-in-Differences estimation simultaneously (Villa, 2016). I restrict the estimation on the region of common support (which drops five observations in the treatment group) and cluster standard errors at the state level, as before. Before presenting the results from the estimation, it is important to check that the matching resulted in a balance in the covariates used for matching between the treatment and control groups. I present the results from a balance test for differences in the pre-treatment (2017) characteristics after matching in Table 8. As we can see, the Kernel Matching has considerably increased the balance in the pre-treatment characteristics since there are no statistically significant differences in the means. The first row presents the resulting pre-treatment means of my main outcome variable of interest, *Board Gender Diversity*, for the treatment and control groups.

Table 8: Balance Test for Pre-Treatment (2017) Characteristics After Matching

Weighted Variables	Mean Control	Mean Treated	Diff.	t-Value	p-Value
Board Gender Diversity	14.686	14.270	-0.416	0.65	0.5212
Number of Employees	6.583	6.287	-0.296	0.30	0.7632
Net Sales	2,580.404	2,884.181	303.776	0.79	0.4345
Total Assets	9,841.233	10,210.326	369.093	0.12	0.9020
EBIT	458.160	546.115	87.955	0.92	0.3612
High Technology Firm	0.571	0.571	0.001	0.01	0.9916
Agriculture	0.000	0.000	0.000	.	.
Mining	0.004	0.002	-0.002	0.87	0.3895
Utilities	0.015	0.014	-0.001	0.38	0.7066
Construction	0.009	0.010	0.001	0.16	0.8712
Manufacturing	0.512	0.513	0.001	0.03	0.9779
Wholesale Trade	0.015	0.016	0.001	0.25	0.8009
Retail Trade	0.018	0.018	0.000	0.00	0.9994
Information	0.156	0.169	0.013	0.71	0.4790
Finance & Insurance	0.114	0.103	-0.012	0.61	0.5455
Real Estate	0.076	0.074	-0.002	0.13	0.8984
Professional Services	0.028	0.028	0.000	0.07	0.9436
Administrative & Support	0.018	0.016	-0.002	0.19	0.8521
Education	0.002	0.002	-0.000	0.08	0.9374
Healthcare	0.014	0.016	0.002	0.59	0.5582
Arts, Entertainment & Recreation	0.002	0.002	-0.000	0.16	0.8713
Accommodation & Food	0.015	0.016	0.001	0.11	0.9167

Note: *p<0.1; **p<0.05; ***p<0.01. *High Technology Firm* is a dummy variable taking the value one if the company belongs to an industry with considerably high STEM employment. *Agriculture, Forestry, Fishing and Hunting*, *Mining, Quarrying, and Oil and Gas Extraction*, *Utilities*, *Construction*, *Manufacturing*, *Wholesale Trade*, *Retail Trade*, *Information*, *Finance and Insurance*, *Real Estate and Rental and Leasing*, *Professional, Scientific, and Technical Services*, *Administrative and Support and Waste Management and Remediation Services*, *Educational Services*, *Healthcare and Social Assistance*, *Arts, Entertainment, and Recreation*, and *Accommodation and Food Services* are based on NAICS 2017 sectors, see United States Census Bureau (2021), and are dummy variables taking the value one if the company is included in the industry.

I present the results from estimating regression model 6 on the matched sample of treated and control firms in Table 9. The estimated treatment effect corresponds to the difference in the outcome variable from 2017 to 2020 in the treated group relative to the difference in the control group and is equivalent to the coefficient on $Treated \times 2020$ in model 5. Estimating the effect on *Board Gender Diversity*, see column 1, we can see that the treatment effect is slightly smaller (3.017 vs. 3.619) compared to the estimate without matching. As Propensity Score Matching deals with systematic differences between treated and control firms that could make the parallel trends assumption implausible, the results from combining Propensity Score Matching with Difference-in-Differences are more trustworthy. I can see that the coefficients on *Board Size* and *Female Directors* are similar to the ones estimated in Tables 5 and 6, however, as the plausibility of the parallel trends assumption cannot be evaluated with only two periods of data, these results should be interpreted with caution.

As for the financial performance indicators, the results after matching suggest a positive effect on *Net Sales* and *EBIT*, however, the coefficient on *EBIT* is not statistically significant. These results are very different to those presented in Table 7 and the trends observed in Figures 2 and 3 for the unmatched sample, which could mean that the results before matching are driven by some control firms that have been discarded after matching and/or by the five treated firms that fall outside the region of common support. However, even though the companies are more comparable after matching, these results should be interpreted with caution due to e.g. the potential heterogeneous effects caused by the Covid-19 pandemic.

Table 9: Results - Difference-in-Differences and Propensity Score Matching

	(1) Board Gender Diversity	(2) Board Size	(3) Female Directors	(4) Net Sales	(5) EBIT
Diff-in-Diff	3.017***	0.195***	0.228***	262.8***	12.38
	(0.506)	(0.0622)	(0.0355)	(91.02)	(25.18)
Observations	4,082	4,082	4,082	4,082	4,082
R^2	0.152	0.020	0.124	0.001	0.001
Mean Control t(0)	14.60	8.560	1.314	2415	417.5
Mean Treated t(0)	14.27	7.926	1.211	2884	546.1
Diff. t(0)	-0.329	-0.634	-0.102	469.6	128.6
Mean Control t(1)	22.47	8.773	2.015	2702	413.1
Mean Treated t(1)	25.16	8.334	2.141	3434	554.1
Diff. t(1)	2.688	-0.439	0.125	732.3	141

Note: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. t(0) indicates year 2017 and t(1) year 2020.

7. Robustness Tests

In this section, I conduct three types of checks to investigate whether my results on the effects of the quota on *Board Gender Diversity* are robust to two types of exclusions as well as changes in the bandwidth parameter used to calculate the Kernel weights.

7.1 Fiscal Year End Month

First, as described previously, both Compustat and Eikon report data at the end of the fiscal year of each company, which does not coincide with the calendar year for a subset of companies. As all companies in the sample do not report data at the end of December each year, this could lead me to either over- or underestimate the effect of the quota on *Board Gender Diversity.* On the one hand, companies that ended their fiscal year of 2019 between June and November 2019 might not have increased gender diversity by the end of their fiscal year since they were not required to do so. They could still have appointed a female board member between the end of their fiscal year and December 31, 2019 to be compliant with the quota requirements, and I would observe this only the following year. On the other hand, companies that ended their fiscal year of 2019 between January and May 2020 were actually already on their second post-treatment year, which could lead me to overestimate the effect for 2019.

I investigate if excluding these firms affects my results and present the results from estimating my preferred model, i.e. the regression model used in my analysis combining Difference-in-Differences and Propensity Score Matching (model 6), in Table 10. I exclude companies whose fiscal year end month does not fall between October and March in column 1 and companies whose fiscal year does not coincide with the calendar year (i.e. does not end in December) in column 2, respectively. I also exclude companies that change their fiscal year end during the period of analysis in these checks. As we can see in Table 10, the estimated coefficients are slightly smaller in magnitude compared to those in Table 9, however, I cannot be certain whether the coefficients are statistically significantly different from each other.

Table 10: Robustness Tests - Board Gender Diversity and Fiscal Year End Month

	(1) FY October-March	(2) FY December
Diff-in-Diff	2.960***	2.860***
	(0.576)	(0.591)
Observations	3,648	3,362
R^2	0.151	0.149
Mean Control t(0)	14.44	14.37
Mean Treated t(0)	14.26	13.85
Diff. t(0)	-0.177	-0.516
Mean Control t(1)	22.40	22.34
Mean Treated t(1)	25.18	24.68
Diff. t(1)	2.783	2.344

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors in parentheses. t(0) indicates year 2017 and t(1) year 2020.

7.2 Other Board Gender Diversity Initiatives

In Table 11, I present results from excluding control companies headquartered in states that have introduced board gender diversity initiatives during the period of analysis. In column 1, I exclude companies headquartered in Maryland, Illinois, New York, and Washington. These states have formally implemented board gender diversity initiatives after the Californian quota was implemented and are likely to be more problematic compared to the states that have introduced voluntary targets between 2015 and 2017 (Illinois, Massachusetts, Pennsylvania, and Colorado) or considered similar legislation without taking any formal action (Hawaii, Massachusetts, Michigan, Ohio, New Jersey, and Pennsylvania).[38] In column 2, I exclude all aforementioned states from the control group, which makes the sample size around half of the full sample. As we can see, the magnitudes of the estimates in Table 11 are similar to those in Table 9.

[38]Note that Illinois and Massachusetts have implemented and/or considered initiatives both before and after the Californian quota.

Table 11: Robustness Tests - Board Gender Diversity and Other Board Gender Diversity Initiatives

	(1) Initiative Post Quota	(2) Initiative
Diff-in-Diff	2.759***	2.938***
	(0.455)	(0.448)
Observations	3,346	2,430
R^2	0.161	0.158
Mean Control t(0)	14.32	14.38
Mean Treated t(0)	14.21	14.13
Diff. t(0)	-0.107	-0.252
Mean Control t(1)	22.46	22.36
Mean Treated t(1)	25.11	25.05
Diff. t(1)	2.652	2.687

Note: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. t(0) indicates year 2017 and t(1) year 2020.

7.3 Bandwidth Parameter

The choice of the bandwidth parameter is more important than the choice of the Kernel function when using the Kernel Matching method (Caliendo & Kopeinig, 2008). Therefore, I also investigate whether changing the bandwidth parameter used in the calculation of the Kernel weights significantly impacts my results. I use a bandwidth of 0.06 in the estimation of my results.[39] Decreasing the bandwidth restricts the interval over which observations in the control group get positive weights, and vice versa. Based on the estimates in Table 12, the results do seem to be significantly affected by the choice of bandwidth.

Table 12: Robustness Tests - Board Gender Diversity and the Bandwidth Parameter

	(1) bw = 0.04	(2) bw = 0.06	(3) bw = 0.08
Diff-in-Diff	2.999***	3.017***	3.055***
	(0.512)	(0.506)	(0.503)
Observations	4,080	4,082	4,082
R^2	0.151	0.152	0.151
Mean Control t(0)	14.58	14.60	14.66
Mean Treated t(0)	14.27	14.27	14.27
Diff. t(0)	-0.312	-0.329	-0.394
Mean Control t(1)	22.47	22.47	22.50
Mean Treated t(1)	25.16	25.16	25.16
Diff. t(1)	2.688	2.688	2.661

Note: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. t(0) indicates year 2017 and t(1) year 2020.

[39] This is the default option provided by Stata.

8. Discussion

8.1 Comparison With Previous Literature

Based on the results presented in the previous section, I draw the conclusion that the Californian quota seems to have been effective in increasing the share of female directors on the boards of the affected companies. Furthermore, I observe positive effects for both board size and number of female directors on the board, however, due to Californian companies following a slightly different trend relative to control companies before the implementation of the quota, these results should be interpreted with caution.

The significant increase in board gender diversity in the affected firms is in contrast to the aforementioned concerns that companies might merely increase board size to become compliant with the quota requirements, leading to more modest effects of the quota on board gender diversity. Moreover, these results are interesting considering the relatively mild sanctions on non-compliance compared to e.g. those in Norway and the ongoing lawsuits challenging the constitutionality of the quota. A potential explanation as to why companies seem to increase female representation on the board, even despite of these facts, is that awareness of gender equality issues is constantly increasing, which could lead to large reputational penalties from not complying with the quota requirements. The previous effort to increase board gender diversity in California from 2013, which consisted of voluntary targets, had low success, however, a lot has changed since then due to e.g. the Me Too movement.

I find a bit larger effects than Hwang et al. (2018) who study companies included in the Russell 3000 Index and use June 2020 as the post-quota point in time. However, my sample of firms is somewhat different as I also include smaller companies and have half a year more data. This could indicate that smaller companies may react more to quotas, or were lagging more behind. Furthermore, it probably takes some time for firms to find the right female director, so we would expect the full effect of the quota to be visible only one to two years later. Furthermore, I do not find reliable evidence of the quota having an impact on the financial performance of the affected companies. The previous literature on the Californian quota finds negative announcement returns for the affected companies in response to the quota and Hwang et al. (2018) further suggest that research analyst expectations of earnings forecasts experienced a decline around the passage of the quota law. While there is a lot of noise in the data due to the Covid-19 pandemic, it is also likely that any potential effects on firm performance are visible only a lot later than after two years from implementation. It is also worth to note that it is difficult to establish a causal relationship between increased female representation in connection with board quotas and firm performance as many other things change at the board at the same time as the share of female directors. For example, just the influx of new members might impact the dynamics within the board of directors and the decision-making within the board.

Finally, my results from the context of the Californian quota are difficult to compare with results from studies in other countries. For example, the widely-studied Norwegian board quota was implemented in 2003 and the effects of a board quota implemented more than 15 years before the quota in California are likely to be quite different.

8.2 Validity

In this thesis, I compare the change in outcomes of publicly held Californian companies before and after the implementation of the quota to that in companies headquartered outside California. The visual inspection of the time trends in board gender diversity during the pre-treatment period suggests that the parallel trends assumption, which is the key identifying assumption for the Difference-in-Differences strategy, seems plausible. However, there are systematic differences between treated and control firms that are likely to make this assumption less plausible. I tackle this concern by combining the Difference-in-Differences strategy with Propensity Score Matching. In addition to data limitations, potential anticipatory effects of the quota, as well as confounding gender equality policies during the period of analysis, which I have addressed previously in this thesis, I identify three potential threats to validity.

First, the Stable Unit Treatment Value Assumption (SUTVA) assumes that treatment assignment of an observation does not affect the potential outcome of other observations. The SUTVA implies that the post-quota outcomes of the control group should not be affected by the treatment of the treatment group, i.e. there should not be any spillover effects. It is possible that this assumption is violated in the context of my study, introducing bias to the estimates. The bias depends on the direction of the possible spillovers. In the case of board gender diversity, if the fact that Californian companies increase the share of female directors on their boards put pressure on control firms to do the same as they compete for customers on the same market, there would be positive spillover effects. If the quota also increased the share of female directors in the control companies, this would lead me to underestimate the true effect of the quota, which is to some extent less problematic than overestimating the effect as it is more conservative. In contrast, if the "pool" of potential female directors to serve on the boards of publicly held companies was limited in the whole United States and the fact that Californian companies are required to increase the share of female directors led to lower availability of female directors for control companies, there would be negative spillovers and the estimates would be biased upwards. However, I see this scenario as unlikely. For financial performance, the violation of SUTVA is more likely and a more serious concern. An increase in the financial performance of treated companies is more likely to have negative spillovers on the performance of control companies on the same market, or vice versa, even though competition for customers is not a zero-sum game.

Second, the potential heterogeneous effects of the Covid-19 pandemic, which occurs during the last year of my period of analysis, on Californian firms compared to control firms is a potential concern for the 2020 estimates. While I do not see this as a serious concern when studying variables related to the board of directors, it is possible that the financial performance of Californian firms was affected differently due to differential state-level restrictions, leading to differential effects on economic activity.

Third, potential avoidance of the quota requirements in the form of relocating to another state or delisting the company should be addressed. While it is not technically possible for treated companies to also appear in the control group in my regression analysis, as they are defined as treated during the whole period of analysis, if companies change headquarters to another state during the post-quota period, they do not have to comply with the quota requirements. Hence, including them in the sample could lead me to underestimate the effect of the quota. I do not see it as likely that companies move headquarters or delist from the stock exchange just to avoid the quota regulations, since the potential financial penalties are low and no fines have been imposed so far. Looking at the current headquarters of the treated companies, which I can observe from the Compustat data, I find that there are some changes. Six companies have moved to another state in 2020 and eight in 2019. As these companies only represent less than three percent of the treatment group, I do not believe that this will have a significant impact on my results.

When it comes to the external validity of my study, I consider it rather difficult to generalize the results to other contexts. As mentioned before, there is a fair amount of variation in the design of existing board quotas, e.g. in terms of hardness of sanctions and deadlines for compliance as well as the mandated share of women on the board. Moreover, cultural differences could lead to differential reactions to board quotas.

9. Conclusion

Gender quotas on corporate boards of directors have become a popular tool to address the persistent disparities in female representation at the top of the corporate hierarchy. In this thesis, I study the effects of the first mandatory gender quota in the United States, California Senate Bill No. 826, implemented in early 2019. To conduct my analysis, I build a dataset consisting of company-level panel data for the period 2017-2020 for both publicly held companies headquartered in California as well as publicly held companies headquartered in other states that act as the control group. I employ both a traditional Difference-in-Differences strategy as well as a Difference-in-Differences strategy combined with Propensity Score Matching to ensure comparability between the two groups of companies. Comparing the differences in outcomes before and after the quota in Californian companies to the difference in control companies, I find that the quota had a significant and positive effect on board gender diversity using both strategies. Furthermore, I observe positive effects for both board size and number of female directors on the board, however, the trends in these variables before the quota was applied were slightly different between the two groups. This can be interpreted as evidence against the plausibility of the parallel trends assumption and hence, these results should be interpreted with caution. I do not either find reliable evidence of the effects of the quota on financial performance. This is likely due the fact that the quota is so recent. Furthermore, as the Covid-19 pandemic coincides with the last year of my period of analysis, potentially affecting Californian firms differently from companies headquartered in other states, it is difficult to disentangle any potential effects of the quota.

My analysis sheds light on the effectiveness of the quota on a more aggregate level and suggests that the quota led to a sizeable increase in the share of female directors at the affected companies. However, my analysis has its limitations, stemming from e.g. the lack of readily available data. Future research could focus on studying the long-term effects of the quota, when more data for the post-quota period becomes available. Furthermore, using more granular-level data to investigate the potential effects of the quota on the labor market outcomes of the women on the board as well as women on other levels of the organization is important since whether the quota will even have the possibility to contribute to breaking the glass ceiling will depend on the effect of the quota on the overall labor market outcomes of women. If the impact of the quota is merely a mechanical increase in the share of women on the board, its success can be seen as more limited. Finally, investigating the effects of other types of gender equality policies, such as mentoring programs, diversity training, or family friendly policies (Azmat & Boring, 2020), is important to evaluate the relative success of board gender quotas as a policy instrument.

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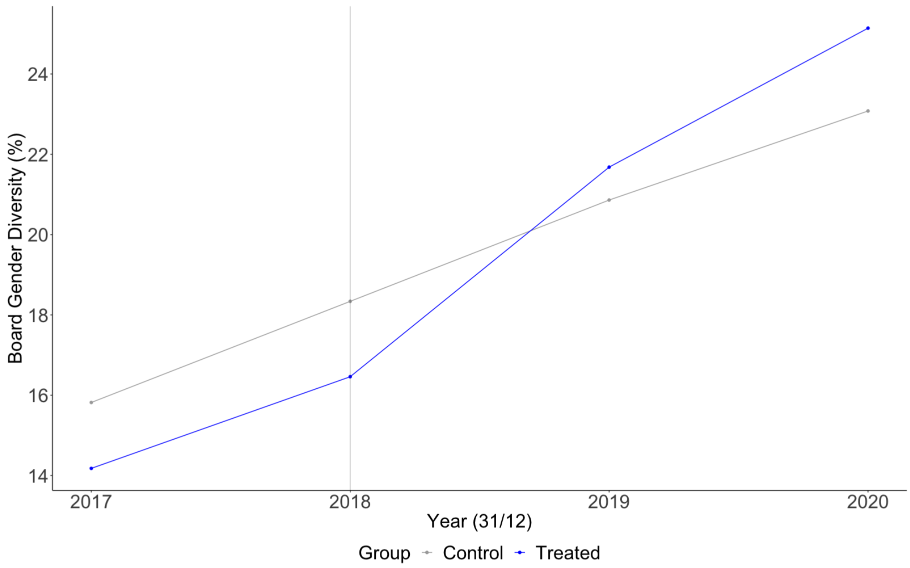
Appendices

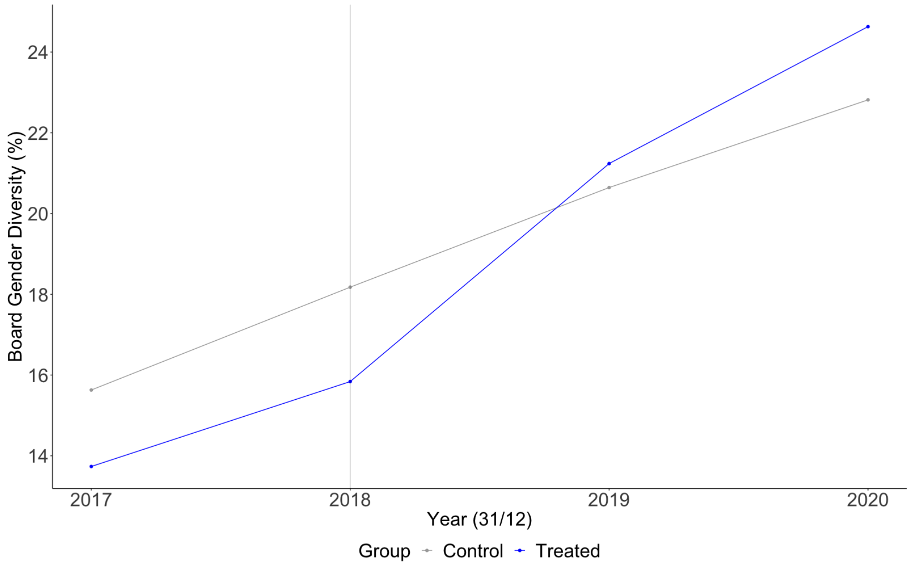
A. Appendix

Figure 5: Evolution of Board Gender Diversity, Firms With Fiscal Year End October-March



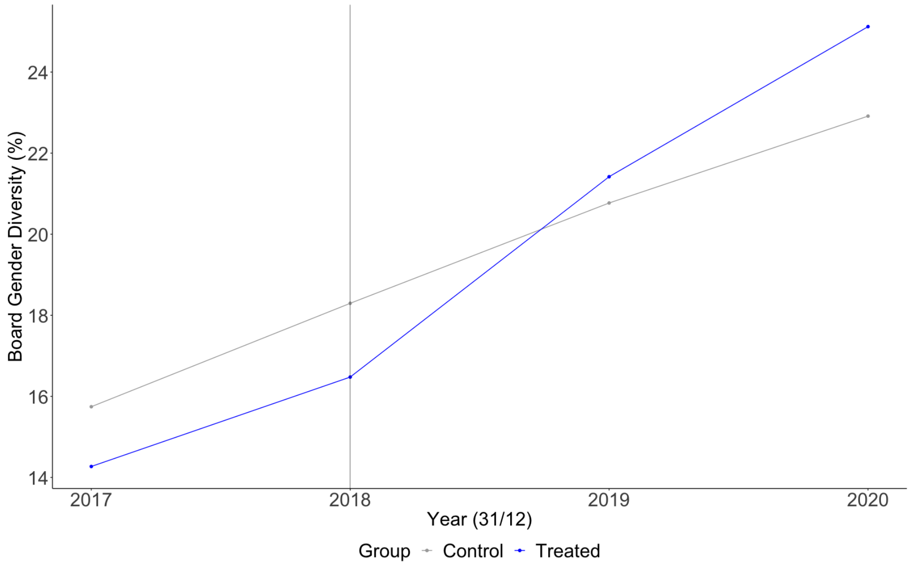
Source: Author's creation.
Note: The figure plots the means of *Board Gender Diversity* for both treated and untreated companies at the end of each year. The quota is implemented on January 1, 2019, and is represented by the vertical line. Companies whose fiscal year does not end between October and March and companies that change their fiscal year end during the period are excluded. There are 428 companies in the treatment group and 1,458 companies in the control group, respectively.

Figure 6: Evolution of Board Gender Diversity, Firms With Fiscal Year End in December



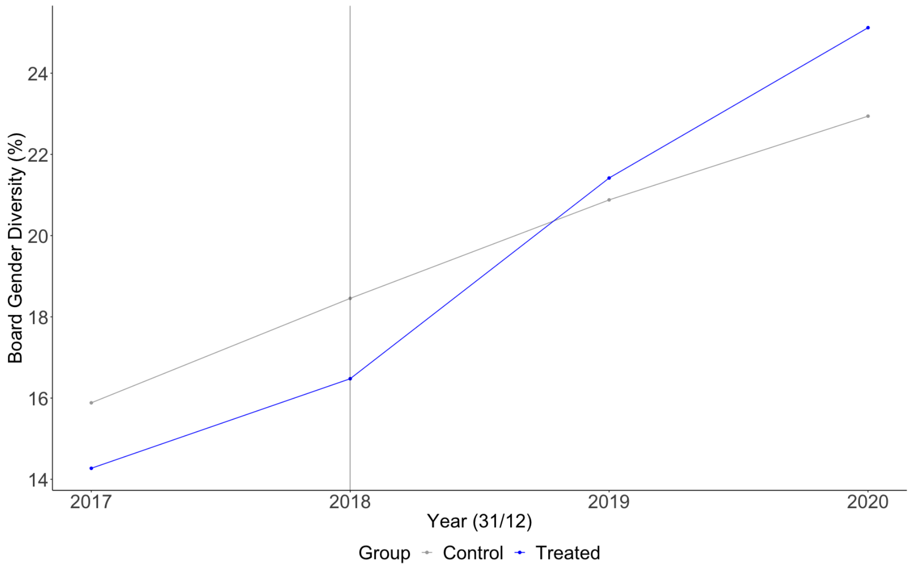
Source: Author's creation.
Note: The figure plots the means of *Board Gender Diversity* for both treated and untreated companies at the end of each year. The quota is implemented on January 1, 2019, and is represented by the vertical line. Companies whose fiscal year does not end in December and companies that change their fiscal year end during the period are excluded. There are 365 companies in the treatment group and 1,363 companies in the control group, respectively.

Figure 7: Evolution of Board Gender Diversity, Excl. Initiatives Post-Quota



Source: Author's creation.
Note: The figure plots the means of *Board Gender Diversity* for both treated and untreated companies at the end of each year. The quota is implemented on January 1, 2019, and is represented by the vertical line. Companies headquartered in Maryland, Illinois, New York, and Washington during the period of analysis are excluded from the control group. There are 503 companies in the treatment group and 1,252 companies in the control group, respectively.

Figure 8: Evolution of Board Gender Diversity, Excl. Initiatives Before/During Period of Analysis



Source: Author's creation.

Note: The figure plots the means of *Board Gender Diversity* for both treated and untreated companies at the end of each year. The quota is implemented on January 1, 2019, and is represented by the vertical line. Companies headquartered in Maryland, Illinois, New York, Washington, Massachusetts, Hawaii, Michigan, Ohio, New Jersey, Pennsylvania, and Colorado during the period of analysis are excluded from the control group. There are 503 companies in the treatment group and 787 companies in the control group, respectively.

B. Appendix

Table 13: Comparison of 2018 Characteristics for Treated Firms

		Full Sample (N = 624)	Non-Missing Data (N = 503)	Missing Data (N = 121)
Board Gender Diversity	Mean	15.9	16.5	13.4*
	Median	14.3	14.3	12.5
	Sd.	12.4	12.1	13.6
	Min	0	0	0
	Max	66.7	66.7	66.7
	N	620	503	171
Board Size	Mean	7.92	8.09	7.20***
	Median	8	8	7.00
	Sd.	2.04	2.08	1.70
	Min	2	2	4
	Max	15	15	12
	N	620	503	171
Female Directors	Mean	1.34	1.41	1.03***
	Median	1.00	1.00	1.00
	Sd.	1.10	1.09	1.09
	Min	0	0	0
	Max	5	5	5
	N	620	503	171
Number of Employees	Mean	6.31	7.50	0.963***
	Median	0.484	0.7	0.115
	Sd.	23.6	25.9	2.30
	Min	0	0	0.002
	Max	259	259	17
	N	615	503	112
Net Sales	Mean	3,157	3,762	490***
	Median	251	316	33.6
	Sd.	15,508	17,094	2,017
	Min	0	0	0
	Max	265,359	265,359	20,609
	N	617	503	114
Total Assets	Mean	9,188	11,021	1,238**
	Median	529	662	213
	Sd.	80,483	89,144	6,503
	Min	0.125	0.1	0.49
	Max	1,895,883	1,895,883	69,225
	N	619	503	114
EBIT	Mean	630.68	775	82.0**
	Median	-0.13	9.55	-16.5
	Sd.	4,024	4,406	1,300
	Min	-1,260	-438	-1,260
	Max	70,662	70,662	13,756
	N	617	503	114

Note: *** p<0.001, ** p<0.01, * p<0.05. *Board Gender Diversity* is measured in percent and *Board Size* and *Female Directors* in number of directors. *Number of Employees* is measured in thousands and *Net Sales*, *Total Assets*, and *EBIT* in million U.S. dollars.

Table 14: Comparison of 2018 Characteristics for Control Firms

		Full Sample (N = 2,537)	Non-Missing Data (N = 1,633)	Missing Data (N = 904)
Board Gender Diversity	Mean	18.38	18.47	17.99
	Median	18.18	18.18	18.18
	Sd.	11.7	11.7	11.7
	Min	0	0	0
	Max	80	80	55.6
	N	1,998	1,633	365
Board Size	Mean	9.30	9.33	9.15
	Median	9	9	9
	Sd.	2.57	2.62	2.33
	Min	1	4	1
	Max	32	32	18
	N	1,998	1,633	365
Female Directors	Mean	1.77	1.78	1.74
	Median	2.00	2.50	2.00
	Sd.	1.24	1.23	1.27
	Min	0	0	0
	Max	8	8	6
	N	1,998	1,633	365
Number of Employees	Mean	13.3	15.0	10.0
	Median	1.53	2.50	0.37
	Sd.	59.7	45.2	80.1
	Min	0	0	0
	Max	2,200	648	2,200
	N	2,498	1,633	865
Net Sales	Mean	5,038	5,798	3,649*
	Median	660	1,010	184
	Sd.	19,892	18,685	21,871
	Min	-8.80	-8.80	-0.391
	Max	511,729	232,887	511,729
	N	2,526	1,633	893
Total Assets	Mean	14,296	18,476	6,670***
	Median	1,546	2,286	487
	Sd.	93,162	112,584	36,114
	Min	0.987	4.06	0.987
	Max	2,622,532	2,622,532	853,531
	N	2,528	1,633	893
EBIT	Mean	680	846	376***
	Median	80.4	131	15.3
	Sd.	2,686	3,142	1,490
	Min	-815	-815	-161
	Max	59,298	59,298	22,124
	N	2,526	1,633	893

Note: *** $p<0.001$, ** $p<0.01$, * $p<0.05$. *Board Gender Diversity* is measured in percent and *Board Size* and *Female Directors* in number of directors. *Number of Employees* is measured in thousands and *Net Sales*, *Total Assets*, and *EBIT* in million U.S. dollars.