Board Gender Diversity and Buyer-Supplier Relationships

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Abstract

How does corporate board diversity affect buyer-supplier relationships? Experimental studies show that women are more risk averse than men. Therefore, we should expect supply chain risk management to increase when women are added to corporate boards. Theory suggests, however, that buyers can reduce supply risk by sourcing from a larger supply base or building stronger relations with fewer suppliers. We test these competing arguments empirically leveraging on a regulation requiring California firms to increase board gender diversity. We find that California buyers consolidated their supply base by reducing suppliers relative to control firms, while retaining long-term and domestic suppliers. These changes occur when female directors are better situated to influence corporate decisions, providing justification to board gender diversity as a channel for operational change. Notably, the suppliers of the treated firms display higher investments, R&D spending, and patenting activities relative to the suppliers of the control firms post reform. These findings combined indicate a heightened propensity to build stronger relationships with a focused number of suppliers. Overall, these operational changes lead to better performance for treated firms. Our results pass a battery of validity and robustness test. The takeaway for corporate leaders, policymakers, and shareholders is that board gender diversity can facilitate operational changes, fostering buyer-supplier relationship specific investments, increasing performance, while also boosting a firm's corporate social responsibility profile.

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1 Introduction

Corporate boards steer firm decisions (e.g., Clark, 1986; Bebchuk, 2005). Motivated by the increasing importance of board diversity regulations globally, studies have shown that board gender diversity impacts corporate policies, for example, on capital structure (Bernile, Bhagwat, and Yonker, 2018), cash holdings (Ahern and Dittmar, 2012), acquisitions (Levi, Li, and Zhang, 2014), and employment (Matsa and Miller, 2013). Notably, one of the main prerogatives of corporate boards is supply chain strategy. A 2017 PricewaterhouseCoopers' survey of more than 886 public firm directors shows that close to 80% of directors analyze information regarding supply chain relationships, with 21% of directors having visited a supplier site in person in the past 12 months (PwC, 2017). Surprisingly, however, the effect of board gender diversity on supply base structure is understudied. The aim of this paper is to fill this gap.

Motivated by the influential work in psychology of Brody (1993), Fujita, Diener, and Sandvik (1991), and Loewenstein et al. (2001), numerous economic experiments have shown that women are more risk averse than men (see, Croson and Gneezy, 2009; Bertrand, 2010, for a review). Relatedly, compared to women, men tend to exhibit greater overconfidence (e.g., Prince, 1993; Barber and Odean, 2001), overestimating the accuracy of their decisions. Therefore, we should expect supply chain risk management to increase when women are added to corporate boards. However, how this affects a buyer's supply base size is an empirical question. On the one hand, supply chain theory has suggested that a larger supply base allows buyers to mitigate supply chain risk due, for example, to disruption risk (e.g., Ang, Iancu, and Swinney, 2017), financial distress (e.g., Babich, Burnetas, and Ritchken, 2007), trade uncertainty (e.g., Wang, Gilland, and Tomlin, 2011), and supplier unreliability (e.g., Tomlin and Wang, 2005). On the other hand, a smaller supply base size allows buyers to improve their suppliers' resilience and ability to deliver, for example, through relational contracts that encourage supplier's investment (e.g., Taylor and Plambeck, 2007a,b), long-term commitments (e.g., Swinney and Netessine, 2009), subsidies (e.g., Babich, 2010) and financing of relationship suppliers (e.g., Tang, Yang, and Wu, 2018), and information sharing (e.g., Ren et al., 2009), while facilitating a higher level of supplier oversight (e.g., Sodhi and Tang, 2019).

To study the effect of board diversity on buyer-supplier relationships, we rely on a regulatory change requiring public listed firms headquartered in California, which have a combined market capitalization of $6.8 trillion, to have at least one female board director by the end of 2019, making California the first and only US state to have ever passed a board gender quota regulation. We call these changes collectively the California Board Gender Diversity Act or the Act. Using a difference-in-difference approach, we find that California buyers (treated firms) consolidated their supply base by reducing the number of suppliers by 7.5% relative to buyers from other states (control firms) in the two years following the Act.

If newly hired female directors are the channel through which the supply chain structure changed for the affected firms, we should expect the changes to occur when female directors are better situated to influence corporate decisions. Therefore, we expect the supply base consolidation to be larger if the newly appointed female directors have a large pre-reform professional reach (which is indicative of their professional experience and corporate influence, e.g., DeMarzo, Vayanos, and Zwiebel, 2003; Larcker, So, and Wang, 2013; Burt, Hrdlicka, and Harford, 2020). Further, if the reform is the reason for the change in supply base for treated firms, we should not find any effect for firms relocating their headquarters outside of California. We find support for both predictions.

We also find that treated firms consolidated their supply base by concentrating on long-term supply chain relationships and domestic suppliers. In line with theory that stronger buyer-supplier relationships encourage suppliers' investment (e.g., Taylor and Plambeck, 2007a,b), our evidence further indicates that the suppliers of the California firms display higher capacity investments, R&D spending, and patenting activities relative to the suppliers of the control firms post reform. Notably, information hand collected from material supply contracts further suggests that treated firms are more accommodating with their suppliers post reform, also in line with an increased focus on building relationships.

Importantly, we also find that the performance of the treated firms increased post Act. These findings are consistent with the evidence that stronger buyer-supplier relationships are more resilient to shocks (e.g., Jain et al., 2022) with positive repercussions on firm performance (Hyndman and Honhon, 2020; Chu et al., 2019; Gutierrez et al., 2020; Webb, 2022).[1] We contribute to this literature by identifying board gender diversity as an important channel to build stronger buyer-supplier relationships that lead to better performance.

Our findings are also related to the broader literature documenting that team diversity leads to better performance (e.g., Schmid and Urban, 2023; Allen and Wahid, 2024).[2] The increase in performance occurs because board diversity encourages corporate investment (Bernile, Bhagwat, and Yonker, 2018; Baik, Chen, and Godsell, 2024) and discourages empire building (Levi, Li, and Zhang, 2014). We contribute to this debate by identifying the role of corporate board diversity

[1]Finance theory has also recognized the importance of stronger firm-lender relationships (e.g., Petersen and Rajan, 1994; Boot and Thakor, 1994). In line with theory, empirical work has shown that access to credit and financing terms improve for firms with stronger firm-lender relationships (e.g., Petersen and Rajan, 1994; Berger and Udell, 1995).

[2]Some of the early studies based on Norway's data have suggested that gender quotas impose unnecessary costs on the firm (e.g., Ahern and Dittmar, 2012; Matsa and Miller, 2013). More recent research finds no evidence that Norway's quotas have a negative effect on performance (Eckbo, Nygaard, and Thorburn, 2022). Relatedly, Bertrand et al. (2019) find that the female board members appointed after the introduction of the Norway gender quota were qualified for the job.

in shaping buyer-supplier relationships — one of the main strategic responsibilities of corporate boards (PwC, 2016; 2017) — as a potentially important operational channel for better corporate performance.

We conduct a number of additional tests to help validate our empirical design. In our first validity test, we find no evidence that the treated firms started to rely on suppliers with more female directors or suppliers from California post Act, mitigating the concern that the documented consolidation in the supply base could be due to the female directors of the affected firms cutting on suppliers with fewer female directors. Also contributing to validating our empirical strategy, we find no indication that our findings are driven by a violation of the parallel trend assumption. Relatedly, we find no evidence that the number of suppliers changed for treated firms in any of the non-overlapping sixteen-quarter "placebo" periods starting in 2003. Overall, these tests contribute to mitigate the concern that our results could be explained by channels other than the increase in board diversity introduced with the California Board Gender Diversity Act.

Is it possible that California firms were affected differently by the coronavirus pandemic? Our tests combined indicate that this is unlikely to be the case. We find that our results hold if we restrict the control group to firms headquartered in states with the 10 largest ports by container imports from China. Similarly, our results hold when we restrict the control group to competitors or rivals of the California firms. Because arguably these firms have similar exposure to imports from China and operate in the same industries as the treated firms, it is less likely that the supplier results could be driven by a differential impact of the pandemic on firms from different industries.

We note additionally that all our regressions include industry-quarter fixed effects. This means that we are effectively comparing the supply chain strategies of treated firms and control firms operating in similar industries and, hence, more likely to be similarly exposed to the pandemic. Also, as discussed above, we find that it is only the treated firms appointing new female directors with significant pre-reform professional reach that change their supply base post Act. That is, the change occurs through the actions of newly appointed female directors who are better situated to influence corporate decisions. If the California firms were changing their supply base because of the pandemic, we should have expected a similar reduction in suppliers even for the California firms appointing new female directors with low professional reach. Overall, our results suggest that it is through the actions of newly appointed female directors that the treated firms consolidate their supply bases post Act.

We conduct a battery of additional tests to further assess the robustness of our findings. In all our regressions, to mitigate the concern that differences between treated and control firms could bias our results, we control for firm characteristics and perform within-firm estimations by including firm fixed effects. As discussed, we also include time-varying industry and state of incorporation fixed effects to ensure that we are comparing outcomes for treated firms and control firms operating in similar industry and affected by similar regulations. In our robustness analysis, we further match treated and control firms (based on relevant pre-Act firm characteristics, board composition, and industry). We find that our supply base results hold in the matched sample. In addition, we saturate our base regression model with several additional control variables that could affect a buyer's supply base. We find that our main findings are qualitatively and quantitatively very similar in the saturated model. Notably, our findings are also robust to alternative samples, model specification, and clustering of the standard errors. We likewise find that our results hold when we use alternative supply base proxies. Altogether, this analysis suggests that our main results are unlikely to be influenced by firm characteristics, sample construction, or model specification.

The main takeaway for corporate executives is that board gender diversity can lead to operational changes that are otherwise difficult to implement, encouraging buyer-supplier relationship specific investments in operations capacity and innovation, boosting performance. Our results also suggest that, even without regulatory requirements, corporate policies that promote equity, diversity, and inclusion can facilitate operations decisions that benefit shareholders by increasing firm performance, while also improving the firm's corporate social responsibility profile.

The rest of the paper is organized as follows. Section 2 discusses additional literature. The California Board Gender Diversity Act is discussed in section 3. Data and empirical design are discussed in section 4. Section 5 presents our main supply base results, tests additional supply base implications of the Act, and discusses robustness and validity tests. Section 6 concludes.

2 Other Related Literature

Diversity, equity, and inclusion are emerging as important focal points in operations and supply chain management, addressing relevant dynamics across a variety of contexts, including team diversity in apparel manufacturing (Narayanan and Terris, 2020), software development and maintenance (Narayanan, Swaminathan, and Talluri, 2014; Huckman and Staats, 2011; Narayanan, Balasubramanian, and Swaminathan, 2009), medical product recalls (Wowak et al., 2021), gender-based pay in beauty services (Pierce, Wang, and Zhang, 2021), gender diversity in product development teams (Sethuraman et al., 2022), workforce diversity and consumer choice (Balakrishnan, Nam, and Buell, 2024), differences in ordering behavior across genders (Ma, Hao, and Aloysius, 2021), racial bias in sourcing (Aral and Van Wassenhove, 2024), and practices of equal employment opportunity in supply chains (Cen, Han, and Wu, 2024). We contribute to this growing literature by studying the link between gender diversity in corporate boards and sourcing strategy. Our work also contributes to the literature on the role of human capital on operations strategy and improvement

(Menor, Kristal, and Rosenzweig, 2007; Carrillo and Gaimon, 2004) highlighting the importance of knowledge and skill diversity (e.g., Lapré and Van Wassenhove, 2001).

Research shows that men are less risk-averse than women across a variety of contexts, including gambling and cognitive tasks such as informed guessing and intellectual risk-taking (see, for example, Croson and Gneezy, 2009; Bertrand, 2010; Byrnes et al., 1999). Using both large-scale survey data and incentivized experiments, Dohmen et al. (2011) show that this is robust to the inclusion of a rich set of controls including wealth (e.g., assets and property values), income level, education, health status, and employment status (e.g., white versus blue collar). Their findings confirm that women demonstrate higher levels of risk aversion in both economic and non-economic decisions. Relatedly, compared to women, men tend to exhibit greater overconfidence (e.g., Prince, 1993), a cognitive bias that lead them to overestimate their knowledge and abilities, leading to poor decision making. In an influential paper, Barber and Odean (2001) show that men trade 45% more frequently than women rather than maintaining stable portfolios that reduce transaction costs. This higher volume of trading results in men's net returns being 2.65 percentage points lower, as their overconfidence leads them to overestimate the accuracy of their market information and insights and the potential benefits of frequent trading.

Our paper is also related to an increasing number of papers in the OM literature relying on quasi-natural experimental methods for identification purposes. This literature has focused on the nexus between airline upgrade policies and ticket prices (Cui, Orhun, and Duenyas, 2019), inventory and sales, (Cachon, Gallino, and Olivares, 2019), taxation and performance in shared-economy lodging (Cui and Davis, 2022), technology and market efficiency (Parker, Ramdas, and Savva, 2016), and managerial flexibility and operations capacity (Aral, Giambona, and Van Wassenhove, 2023). More generally, Roth and Rosenzweig (2020), Terwiesch et al. (2020), and Fisher, Olivares, and Staats (2020) highlight the importance of empirical research in OM, while the necessity of identification studies is discussed in Ho, Lim, and Xia (2017). We contribute to these streams of literature with a unique quasi-natural experimental setting provided by a regulatory shock that significantly increased corporate gender diversity: the California Board Gender Diversity Act. We combine this experimental setting with a compilation of novel datasets that allow us to study the relationship between corporate gender diversity and sourcing strategy. We believe that this setting can be fruitfully used by future research to study other OM policies.

3 The Board Gender Diversity Act of California

To study the effect of board gender diversity on supply chain strategies, we rely on several important changes that were introduced with the California Board Gender Diversity Act of 2018 (Senate Bill 826 – SB 826). Prior to SB 826, California had a resolution, the Senate Concurrent Resolution

62 of September 2013, which only encouraged, with little success, California firms to increase female representation on their boards (Jackson, Atkins, and Levya, 2018). It took nearly 5 years for California to pass a regulation requiring firms headquartered in the state to increase female representation on their boards. SB 826 was introduced on January 3, 2018, it was signed into law by Governor Jerry Brown on September 30, 2018, and chaptered by the Secretary of State as Chapter 954, (Statutes of 2018) on September 30, 2018.[3] SB 826 made California the first and only state in the US to ever pass a board gender diversity law.[4]

SB 826 required that by the end of 2019 all publicly listed firms headquartered in California have at least one female director on their corporate boards. In addition, SB 826 required that by the end of 2021 California corporations have two or more female directors or three or more female directors for boards of five members and boards of six or more members, respectively. Our analysis shows that 86.9% of the California firms in our sample had to add one or more female directors following the Act. However, by the end of 2019, 2020, 2021, 93.4%, 97.0%, and 99.3% of the California firms had at least one female director, respectively, while about 35% of the California firms with larger boards still needed to add one or more female directors at the end of the 2021 deadline. Overall, these figures suggest that California firms complied with the law, which is expected given that each first violation is fined $100,000 and subsequent violations are fine $300,000 each. Notably, the Act introduced a binding obligation to add and retain female directors. This means that the Act is important for both firms that needed to add female directors and firms that had already a sufficient number of female directors, giving these directors more protection from the risk of being removed from their roles. The reform affected about 16.2% of all publicly listed firms in the US, with a combined market capitalization of $6.8 trillion.

In our empirical tests, we study the supply chain strategies of California firms (those affected by the Act: the treated group) relative to firms headquartered in other states (the control group) following the increased female representation on corporate boards mandated by the Act. To assess the validity of our empirical design, we start by analyzing how board gender diversity and propensity to relocate changed for California firms after the Act. See Table A1 in the E-Companion for

[3]SB 826 was declared unconstitutional on May 13, 2022, by a California Superior Court judge. California secretary of state has announced the intention to appeal the ruling. This type of reversals highlights the importance of studies on the role of corporate diversity on corporate decisions to help inform the policy debate. Our paper focuses on the period ending on December 31, 2021, when the Act was in full effect.

[4]Six other states have enacted legislation to encourage firms to increase board diversity (Colorado, Ohio, and Pennsylvania) or to disclose information on board diversity (Maryland, Illinois, and New York). Five other states, including Hawaii, Massachusetts, Michigan, New Jersey, and Washington, are each considering mandatory board diversity legislation. Although none of these states have mandatory board diversity legislation in place during our sample period, in our robustness tests, we show that our results hold if we excluded firms headquartered in those states. There are also two bills under discussion in the U.S. Congress to increase disclosure and further analysis of board diversity issues.

detailed definitions of the main variables used in the article. Table A2 (E-Companion) shows that the presence of female board members increased by 6% for the California firms relative to control firms in the four quarters leading to 2019Q4, the date by when SB 826 required California firms to have at least one female board member. We further find that the number of female board members increased by 8.8% in the quarters ending in 2021Q4, when the Act required additional female board members for California firms with larger boards. We also find that about 3% of the firms headquartered in California prior to the Act relocated to a different state by the end of 2019 (Table A3, E-Companion). This relatively low percentage of corporate headquarters changes is perhaps unsurprising given that relocation can be costly, requiring firms, for example, to conform to state-level regulations, potentially face challenges to retain some of their key employees, and incur direct costs associated with the relocation. As discussed in the result section, our main findings hold for firms that did not relocate outside of California, but do not hold for firms that relocated and hence are unaffected by the reform.

4 Data and Empirical Design

Our data come from a variety of sources. Supply chain relationship data is from the FactSet Revere Supply Chain Relationships database. We compile quarterly data on the number of suppliers with a direct supply chain relationship with the firm (tier 1 suppliers), the location of the supplier, and the duration of the relation with each supplier. As discussed on the FactSet website, the dataset contains up-to-date information of material intercompany relationships obtained from supply contracts, purchase obligations, SEC 10-K filings, investor presentations, press releases, and other public sources. The FactSet database coverage represents a significant improvement compared to the COMPUSTAT Segment Files of earlier studies, which are limited to large suppliers representing 10% or greater of public buyers' sales and hence are skewed towards small buyers.

We combine the supply chain relationship data with buyer-level financial information from COMPUSTAT using CUSIPs.[5] Our main sample includes publicly listed buyers, except financial firms (SICs 6000-6999), with data available both in FactSet and COMPUSTAT. Because FactSet is skewed towards relatively larger firms, we exclude firms with sales lower than five million. We also exclude firms with sales in the current period double the sales in the previous period. This mitigates the concern that FactSet differences in supplier coverage between treated and control firms could bias our findings.[6] In our robustness tests, we further match treated and control

[5]See, for example, Cronqvist and Yu (2017), Oehmke and Zawadowski (2017), and Musto, Nini, and Schwarz (2018) for recent papers using CUSIPs.

[6]Before applying these filters, median quarterly sales are $177 million for firms in FactSet relative to $7 million for firms not in FactSet. After applying the filter, median sales are $312 million for FactSet firms versus $83 million for non-FactSet firms.

firms based on size and other characteristics. We obtain additional information for buyers from the following sources: historical headquarters information is parsed from corporate filings on the SEC EDGAR Database,[7] while corporate board information is from BoardEx and transaction-level import data is from Panjiva. We hand-collect information on material supply contracts from corporate filings on the SEC EDGAR Database. Financial data for domestic and foreign suppliers is from WORLDSCOPE. Data on the number of US patents of domestic and foreign suppliers is parsed from the PatentsView database of the United States Patent and Trademark Office (USPTO) and manually matched with the suppliers in our sample. Board data for the US and European suppliers is from BoardEx.

To test whether supply base changed for public firms headquartered in California after 2019Q4, we estimate the following difference-in-difference model:

$$\begin{split} &Log\ of\ Suppliers_{i,q} = \\ &\qquad \beta(Treated \times Post\text{-}Act) + \mathbf{Controls}_{i,q-1} + y_i + z_q + i_i \times z_q + s_i \times z_q + \epsilon_{i,q} \end{split} \tag{1}$$

where $Log\ of\ Suppliers_{i,q}$ is the natural logarithm of the number of suppliers with a direct relationship with buyer i in quarter q. $Treated$ is an indicator for firms headquartered in California (treated firms) in 2017 (the year before the Act was signed into law). *Post-Act* is an indicator equal to 1 for the eight quarters from 2020Q1 onward. Table A1 in the E-Companion contains detailed definitions of our main variables. We consider 2020Q1 as the first post reform period because we are interested in testing the effect of the presence of female directors on supply base and the Act gave public firms headquartered in California until the end of 2019 to have at least one female corporate board director. y_i are firm fixed effects, z_q are quarter fixed effects, and $i_i \times z_q$ and $s_i \times z_q$ are industry (2-digit SIC code) times quarter fixed effects and Delaware incorporation times quarter fixed effects, respectively. Our main analysis focuses on the sample period 2018Q1–2021Q4: a sixteen-quarter time window centered on 2019Q4. The focus of our analysis is on $Treated \times Post\text{-}Act$: difference-in-difference estimator.

Our main set of control variables includes the following company characteristics: (1) Size, the logarithm of assets; (2) Gross Margin, the ratio of earnings before interest, taxes, depreciation, and amortization to sales; (3) Capital Intensity, the ratio of property, plant, and equipment to assets; (4) Leverage, the ratio of debt to market assets. Standard errors are clustered at the headquarters' state level (e.g., Bertrand and Mullainathan, 2003; Cameron and Miller, 2005).

Table 1 reports basic descriptive statistics for firms headquartered in California (treated firms)

[7]COMPUSTAT only includes the most current headquarters information. Because for our study we need historical headquarters data, we parse it directly from corporate filings.

and firms headquartered in other states (control firms). On average, the treated firms have 15.975 material suppliers, compared with 16.810 for the control groups. Table 1 displays some other differences between the two groups, with the treated firms, for example, being smaller than the control firms, Size: 6.883 vs. 7.574. Table A4 in the E-Companion provides detailed descriptive statistics for all the variables used in the paper.

[Table 1]

To mitigate the concern that some of these differences could bias our results, we: (1) control for firm characteristics in all our regressions; (2) perform within-firm regression estimations by including firm fixed effects; (3) control for industry-quarter and Delaware incorporation-quarter fixed effects; (4) match treated and control firms on the basis of relevant characteristics using the Abadie and Imbens (2006) matching estimator; (5) saturate the regression model with additional control variables. We note that the inclusion of industry–quarter and Delaware incorporation–quarter fixed effects in all our regressions implies that we are comparing the supply chain policies of treated firms and control firms operating in similar industries and in the same incorporation region and hence potentially affected by the same industry-wide economic and regulatory shocks with the difference being that treated firms are headquartered in California (and hence affected by the Act), while the control firms are headquartered in other states. Our main findings and robustness tests suggest that results are unlikely to be influenced by differences in firm characteristics across treated and control firms.

In addition to supply base, we test several additional supply chain implications of the California Board Gender Diversity Act related to buyer-supplier relation specific investments, supply base risk, and performance. We further test whether the propensity to hire female directors and move headquarters from California to some other states changed for treated firms relative to control firms following the Act. We similarly analyze whether the propensity to focus on suppliers with more female directors or suppliers located in California changed following the reform. We test these additional effects by estimating a difference-in-difference model similar to Eq. (1).

5 Supply Base of Treated Firms after the Board Gender Diversity Act

In this section, we examine the effect of the California Board Gender Diversity Act on the number of suppliers of California firms (treated group) relative to non-California firms (control group) by estimating Eq. (1). In these estimations, the dependent variable is the natural logarithm of the number of suppliers. Table 2 presents these results. Across all first four estimations in

Table 2, the coefficient on the interaction term of interest—Treated × Post-Act—is negative and always statistically significant at the 1% level. This finding indicates that treated firms reduced the number of suppliers relative to control firms following the Act-induced increase in the number of female directors for treated firms. The effect is also economically large. Focusing on column 4 (estimation with all control variables), the coefficient on the interaction term suggests that treated firms consolidated their supply base by reducing the number of suppliers by 7.5% relative to control firms after the Act. Overall, these results are in line with theories that buyers can increase their supply chain's resilience by building stronger relationships with a smaller number of suppliers (e.g., Taylor and Plambeck, 2007a,b; Swinney and Netessine, 2009).

To test how the supply base of the affected firms changed over time, we re-estimate Eq. (1) by adding interactions of the treated dummy with indicators for each of the quarters from 2020Q1 to 2021Q4 (post Act quarters). Table 2, column 5, reports these results. The reduction was gradual starting at 4.9% in 2021Q1 (coefficient on Treated × 2020Q1), reaching a sizable 7.8% in 2020Q2, and became even larger in absolute value in the following quarters, with a decrease of 8.0%, statistically significant at the 1% level, in quarter 4 of 2021. Figure A1 (E-Companion) presents a visual representation of this supply base consolidation process. Overall, the evidence in Table 2 points to sizable and long-lasting effects of board gender diversity on supply base size. Turning briefly to the control variables, as expected, Table 2, columns 4 and 5 (specifications with all controls) shows that larger firms have a higher number of suppliers. Other control variables are not statistically significant.

[Table 2]

To explore the mechanism through which female directors can influence supply base size, in our next test, we partition the newly appointed female directors into those with high and low pre-reform professional reach, where reach is the number of overlaps of the director with other professionals through employment, education, and other activities (source: BoardEx). In line with the literature, the logic of this measure is that the extent of a director's reach is indicative of their professional experience and corporate influence (e.g., DeMarzo, Vayanos, and Zwiebel, 2003; Larcker, So, and Wang, 2013; Burt, Hrdlicka, and Harford, 2020). In Table 3, we estimate Eq. (1) by interacting Treated × Post-Act with High Reach Female Director and Low Reach Female Director, which are indicators for new female directors with a number of professional overlaps above and below the sample 75$^{\text{th}}$ percentile in 2019Q4 (i.e., above, or below 2,071 professional overlaps), respectively. In column 2, specification with all control variables, we find our main variable of interest to be significantly negative when interacted with High Reach Female Director (-14.7%, statistically significant at the 1% level), but insignificant when interacted with Low Reach Female Director (-4.1%, statistically insignificant). A Wald test reported at the bottom of Table 3 shows that the two interaction terms are statistically different at the 1% level.

We also estimate Eq. (1) by interacting Treated $\times$ Post-Act with Not Relocated and Relocated, which are indicators for firms that did not relocate or did relocate their headquarters outside of California when the reform became binding starting in 2020Q1. In column 4, specification with all control variables, the coefficient on Treated $\times$ Post-Act $\times$ Not Relocated suggests that treated firms that did not relocate outside of California reduced their supply base by 8.1% (statistically significant at the 1% level) following the Act, while our variable of interest was insignificant when interacted with Relocated. A Wald test shows that the two interaction terms are statistically different at the 1% level.

Notably, the evidence in Table 3 reveals that the changes in supply base following the Act occur if the newly appointed female directors have a significant professional reach and are therefore better situated to influence corporate decisions and for firms that did not relocate outside of California, for which the reform was binding. Overall, the evidence in Tables 2 and 3 indicates that the changes in group dynamics associated with adding women to corporate boards affected decisions concerning buyer-supplier relationships for the treated firms.

[Table 3]

If team gender diversity improves a firm's decision process, we should observe treated firms to consolidate their supply base by focusing on the stronger buyer-supplier relationships. We start by analyzing whether affected buyers are more likely to retain longer-term suppliers after the Act, which would suggest an increased emphasis on focused relationships (e.g., Jain, Girotra, and Netessine, 2022). We also analyze if post reform treated firms are more likely to rely on relationships with domestic suppliers, which are easier to establish while avoiding the geopolitical and economic uncertainty of sourcing from (more distant) foreign suppliers (e.g., Chu, Tian, and Wang, 2019). We aggregate data of all the existing suppliers of each buyer in any given quarter in these tests. Although FactSet does not report information on how much a supplier sells to a given customer, FactSet's focus on material suppliers suggests that the buyers in our sample are very likely to derive sizable benefits from supplier investments and innovative activities.

Table 4, columns 1 and 2, presents results on length and proximity of supply chain relationships after the reform. In column 1, we estimate our difference-in-difference model using as dependent variable the average length of all existing supply chain relationships for the buyer. In column 2, we estimate a similar model using the ratio of domestic suppliers to total suppliers as dependent variable. The significantly positive coefficients on Treated × Post-Act in columns 1 and 2 suggest that supply chain relationship length and proportion of domestic suppliers increased by 0.385 quarters and 2.0 percentage points (pp), respectively, for treated firms relative to control firms after the Act. Notably, as treated firms focus more on domestic suppliers, imports in terms of weights (column 3), volume (column 4), and number of containers (column 5) went down for them post Act. Importantly, however, the combined changes in the supply chain structure for treated firms are associated with an increase in inventory holdings after the Act (column 6). Overall, these findings indicate that treated firms established stronger relationships with a consolidated number of suppliers by focusing on longer-term relationships with domestic suppliers.

[Table 4]

Next, we switch the focus on the profile of the suppliers and test whether the suppliers of the treated firms display higher investment, R&D expenses, and patenting activities following the Act. Table 5 presents this evidence. We only consider suppliers that do not overlap across treated and control firms. Starting from column 1, supplier investment (the ratio of capital expenditures to assets for all suppliers multiplied by 100) regression, the coefficient of 0.064, statistically significant at the 1% level, on the interaction term, suggests that the suppliers of the treated firms display about 8% higher investment than the suppliers of the control firms after the Act. This 8% increase is relative to the average supplier investment of 0.763 (0.064/0.763=0.084 or 8.4%) in Table A4, Panel A.

We likewise find higher innovation activities for the suppliers of the treated firms. In the supplier R&D regression (the ratio of R&D expenses to assets for all suppliers multiplied by 100) (column 2), the coefficient of 0.054, statistically significant at the 1% level, on the interaction term, suggests that R&D expenses for the suppliers of the treated firms are a sizable 8.8% higher following the reforms relative to the average supplier R&D expenses of 0.614 in Table A4, Panel A. Similarly, column 3 suggests that the suppliers of the treated firms display nearly 18.4% higher number of patents relative to the suppliers of the control group after the Act, respectively. Altogether, the findings in Table 5 are in line with OM theories showing that stronger buyer-supplier relationships encourage suppliers' investment and innovation (e.g., Taylor and Plambeck, 2007a,b).

[Table 5]

Overall, our findings thus far support the argument that the increase in team diversity with the Act improved the decision process of the affected firms, leading to more focused buyer-supplier relationships. To our knowledge, our paper is the first to identify the role of team diversity in shaping buyer-supplier relationships as a potentially important operational channel for the increased performance documented in several studies (e.g., Bernile, Bhagwat, and Yonker, 2018; Allen and Wahid, 2024; Schmid and Urban, 2023).

5.1 Robustness Analysis and Validation Tests

To mitigate the concern that time-invariant unobservable differences between treated and control firms could bias our results, we estimate all our regressions with firm-fixed effects. Further, in all our estimations we include industry-quarter and Delaware incorporation-quarter fixed effects to control for differences in industry and regulatory considerations between treated and control firms. These fixed effects ensure that we are comparing treated firms to control firms operating similar industries and subject to the same regulatory requirements. In the regressions, we also control for observable time-varying firm characteristics, including size, gross margin, capital intensity, and leverage.

To further account for potential differences between treated and control firms, in our first robustness test, we match each treated firm (Treated: Yes) to its nearest control firm (Treated: No), based on size, gross margin, capital intensity, and exact match them on industry (2-digit SIC code) in 2019Q4 (the last pre-act quarter) (Matched Sample 1). In a second matching, we match firms on size and exact match them on the number of board members and number of female directors (Matched Sample 2). We implement our matching using the Abadie and Imbens' (2006) matching estimator. Control firms will be selected to be identical to the treated firms on the exact matching variables. In Table A5 (E-Companion), Panels A and B, we present the mean difference t-test and distributional test for treated and control groups in the two matched samples. In both matched samples, the p-values (for the mean difference t-tests and the distributional tests) are all above the 10% threshold. This suggests that, in the matched samples, treated and control firms share similar characteristics and distributional assumptions. In Figure A2 (E-Companion), we further consider the kernel density function of characteristics for treated and control firms. Comparing visually the full sample before matching (column 1) with the matched samples (column 2) shows that the density functions of the firm characteristics are very similar in the matched samples (in line with Table A5).

Using the matched samples, we re-estimate our main supply base size model. Table A6 (E-Companion) shows that results in the matched samples are economically stronger than the results in the full sample (Table 2). The coefficients on the interaction term in the first and second matched samples, columns 2 and 4, respectively, suggest that treated firms reduced the number of suppliers by 13.7% and 14.2%, respectively, after the Act, compared with 7.5% in Table 2, column 4.

In a related test, we directly control for potential time varying differences between treated and control firms by saturating our supply base model with additional explanatory variables. Namely, we further control for R&D expenses, growth prospects, debt maturity, and payables. Our evidence in Table A7 (E-Companion) shows that adding these variables has limited consequences for the statistical significance and economic effects of the interaction term of interest.

We further assess the robustness of our results to 1) sample construction, 2) model specification, and 3) standard error clustering (e.g., Rothstein, Sutton, and Borenstein, 2005; Christensen and Miguel, 2018; Andrews and Kasy, 2019). Table 6 reports results from these robustness tests. In our main sample, we drop firms with sales lower than or equal to $5 million and firms with sales in the current period equal to or more than double the sales in the previous period. As columns 1 - 6, Panel A, Table 6 shows, removing either of these two filters or both has no effect on our main findings. Further, columns 7 - 8 show that our results are very similar if we use annual data instead of quarterly data. In Panel B, Table 6, we analyze whether our results could be biased by the type of fixed effects used in our main model estimation. We find that removing any of the fixed effects or all does not alter our main conclusion that treated firms reduced the number of supplier after the reform. Finally, Table 6, Panel C, shows that our results are robust if we use alternative clustering of the standard errors.

[Table 6]

Next, we assess whether the California firms were affected differently by the coronavirus pandemic. Our tests combined help mitigate this concern. We start by reiterating that all our regressions include industry-quarter fixed effects. This means that we are effectively comparing the supply chain strategies of treated firms and control firms operating in similar industries and, hence, more likely to be similarly exposed to the pandemic. Also, as discussed above, we find that it is only the treated firms appointing new female directors with significant pre-reform professional reach that change their supply base post Act. That is, the change occurs through the actions of newly appointed female directors who are better situated to influence corporate decisions. If the California firms were changing their supply base because of the pandemic, we should have expected a similar reduction in suppliers even for the California firms appointing new outside female directors or female directors with low professional reach.

Our results could be biased if California firms were affected differently by the coronavirus pandemic. Our tests combined indicate that this is unlikely to be the case. Table 7, Panels A and B, report these results. We find that our results hold if we restrict the control group to firms headquartered in states with the 10 largest ports by container imports from China (Panel A, column 1). Similarly, our results hold when we restrict the control group to competitors (FactSet) or rivals (Hoberg and Phillips, 2010, 2016) of the California firms, Panel A, columns 2 and 3, respectively.

Because arguably these firms have similar exposure to imports from China and operate in the same industries as the treated firms, it is less likely that the supplier results could be driven by a differential impact of the pandemic on firms from different industries. Overall, our results suggest that it is through the actions of newly appointed female directors that the treated firms consolidate their supply bases post Act.

[Table 7]

In Table A8 (E-Companion), we assess the robustness of our findings to alternative proxies for sourcing. Specifically, we use the ratios of number of suppliers to sales and number of suppliers to assets in columns 1 and 2 and columns 3 and 4, respectively. In these tests, we find a significant decrease in the supply base size of the treated firms with these alternative proxies.

To assess the validity of our identification strategy, it is important to verify that, prior to the Act, sourcing for treated and control firms followed a parallel trend. A violation of the parallel trend assumption could be problematic because it would suggest that a trend specific to treated firms, rather than the Act, is the reason why the supply base changed for the treated firms. To test for parallel trends, we estimate our supply base model with additional control variables as in Table A7 by adding interaction terms of the treated dummy variable with quarter indicators for 2018q2–2021q4 (with 2018q1 as the omitted case) (e.g., Autor, 2003; Gormley and Matsa, 2011; Freyaldenhoven, Hansen, and Shapiro, 2019; Goodman-Bacon, 2021).

Figure 1 plots the coefficients on the interaction terms from these estimations, together with 90% confidence intervals. Notably, the interactions of the treated dummy with the pre-Act indicators (2018q2–2019q4) are statistically insignificant. In line with the parallel trend assumption, we find no indication that number of suppliers changed for treated firms relative to the control firms prior to the Act. However, there is a significant decrease in the supplier base following the Act. These patterns are in line with the logic of our empirical design. California firms had until the end of 2019 to comply with the requirement to have at least one female director. Given that it takes time to modify existing supply chain relationships, it is expected that the number of suppliers only goes down starting in 2020.

[Figure 1]

In Table A9 (E-Companion), we present results from several placebo estimations. Our main identifying assumption is that the change in supply base is a direct consequence of the Board Gender Diversity Act requiring California firms to have at least one female director by the end of 2019. If that is the case, we should not find any significant effect on supply base outside of the event window (e.g., Roberts and Whited, 2013). To assess this conjecture, we consider the following six additional 16-quarter estimation windows: 2003Q3-2007Q2, 2007Q3-2011Q2, 2011Q3-2015Q2, 2015Q3-2019Q2, 2016Q3-2020Q2, and 2017Q1-2020Q4. Reassuringly, Table A9 shows that the coefficients on the interaction terms of interest are always insignificant in these "placebo-window" estimations. We only find the interaction term to be significantly negative for the period 2017Q1-2020Q4. This is expected because this time window includes four post reform quarters, 2020Q1 to 2020Q4, when the requirement for California to have at least one female director was already binding. The decline in suppliers is smaller compared with our base estimation, -6.0% vs. -7.5%, which includes eight post reform quarters, 2020Q1 to 2021Q4. The interaction term is insignificant for the period 2016Q3-2020Q2, which only includes two post Act quarters, and for the other four non-overlapping placebo estimations. Overall, this analysis brings additional credibility to our identification strategy by suggesting that the effect on supply base that we document is directly related to the California Board Gender Diversity Act rather than a pre-trend in supply base specific to treated firms.[8]

Is it possible that the number of suppliers decreased for treated firms because the female directors are discontinuing relationships with suppliers with fewer female directors? This would suggest that our findings could be driven by a personal preference of the newly appointed female directors of treated firms to retain suppliers with more female directors rather than a drive to establish stronger relationships with few suppliers. To assess this possibility, we test whether the number of female directors increased for the suppliers of treated firms after the reform. Reassuringly, the insignificant coefficient on the interaction term of interest in Table 8, columns 1 and 2, suggests that the treated firms did not discontinue some of their supply chain relationships to focus on suppliers with more female directors. Relatedly, Table 8, columns 3 and 4, shows no evidence that the treated firms increased the proportion of California suppliers after the Act. Arguably, if the objective of the female directors was to focus on suppliers with more female directors, then one way to do so would have been to increase the proportion of California suppliers, who would also be subject to the reform. Columns 3 and 4 provide no evidence that this was the case.

[Table 8]

Our argument is that treated firms reduce the number of suppliers post Act to build stronger buyer-supplier relationships. The evidence in Table 4 that they focus more on longer-term and domestic suppliers post reform combined with the evidence in Table 5 that suppliers display higher investments and innovative activities post Act supports this argument. To further explore this

[8]Table A10 in the E-Companion further shows that our results are robust if we drop firms headquartered in states with disclosure requirements (or considering bills) on board gender diversity (see also footnote 7 in Section 2).

mechanism, we gather material supply contracts from the U.S. Securities and Exchange Commission (SEC) Electronic Data Gathering, Analysis, and Retrieval (EDGAR) database for the sample of treated firms in Table 2 and matched controls firms in Table A6 for the period 2016-2021. These contracts are disclosed as exhibits to financial filings. In total, we identify 51 supply contracts in which the firm is a buyer, 19 for treated firms and 32 for control firms. Although Regulation S-K of the U.S. Security Act of 1933 requires firms to disclose material contracts, what constitutes a material contract is ultimately left to the company's discretion. This can help explain why only a few firms disclose these contracts, in line with previous studies (Pankratz and Schiller, 2024). Because of the limited number of observations, these contracts can only be used to provide anecdotal evidence.

From these contracts, we gather information on whether they include a convenience clause, which gives the buyer the right to terminate the supply agreement "at will" even without cause and trade credit maturity, the number of days that the buyer has to pay an invoice from the supplier. Table 9, Panel A and B, report details about these contracts for treated and control firms, respectively. 9, Panel C, shows that on average 28.6% of the contracts for treated firms have a convenience close post reform compared with 54.6% pre reform. For control firms, the percentage of contracts with a convenience close increases from 5.3% pre reform to 30.8% post event. To assess the statistical significance of the propensity to have a convenience clause post reform for treated firms relative to control firms, we regress an indicator for whether the contract has a convenience clause on Treated × Post-Act, Treated, and Post-Act. The coefficient of -0.306, statistically significant at the 5% level, suggests that convenience clauses are nearly 31% less like for treated firms relative to control firms post Act. Similarly, Iyer and Sautner (2018) find that buyers require less control rights when relationships with suppliers are stronger. We also find that trade credit maturity increases by about 11.6% for treated firms relative to control firms post Act, but this effect is not statistically significant. These findings suggest that buyers rely less on their bargaining power post reform, leading to a reduction in the propensity to ask for a convenience clause to build stronger relationships with their suppliers.

[Table 9]

Did stronger buyer-supplier relations lead to better performance for treated firms? To address this question, we estimate our difference-in-difference model using profitability as dependent variable. Table 10 reports results from these estimations. Focusing on column 2, specification with all control variables, the significantly positive coefficient on the interaction term of interest suggests that profitability increased by 3.0 percentage points for the treated firms relative to control firms after the reform – in line with the equity-market based evidence in Allen and Wahid (2024).

Figure 2 shows that pre-existing trends cannot explain the post reform increase in performance for treated firms. Overall, our findings support the argument that board gender diversity is an important channel to build stronger buyer-supplier relationships that lead to better performance. To our knowledge, our paper is the first to identify the role of corporate board diversity in shaping buyer-supplier relationships as a potentially important operational channel for better corporate performance.

[Table 10], [Figure 2]

6 Conclusion

We study how changes in corporate decision dynamics associated with adding women to corporate boards affect buyer-supplier relationships. Studies in experimental economics have shown that women are more risk averse than men. Therefore, we should expect supply chain risk management to be higher for more gender diverse boards. Supply chain theory has suggested, however, that buyers can reduce supply base risk either by building stronger relations with fewer suppliers or procuring from a larger supply base. We test these competing hypotheses empirically leveraging on a regulation requiring public firms headquartered in California to increase female corporate board representation, combined with several novel data sources.

We find that treated firms consolidated their supply base following the Act, focusing on long-term and domestic suppliers. In line with OM theories showing that stronger buyer-supplier relationships encourage suppliers' investment and innovation (e.g., Taylor and Plambeck, 2007a,b), we also find that the suppliers of the treated firms display higher investments and innovative activities relative to the suppliers of the control firms post reform. The changes in supply base occur among firms appointing female directors that have a significant professional reach and are, therefore, better situated to influence corporate decisions. These operational changes lead to better performance for treated firms.

The takeaway for corporate executives is that board gender diversity helps firms right-size their supply base, encouraging stronger buyer-supplier relationships, and boosting performance. Our findings can also help inform the current policy debate on the role of corporate diversity regulations. At a time when initiatives to facilitate women's corporate participation have suffered several setbacks,[9] our findings suggest that board gender diversity can lead to a shift in strategic decisions and improve performance. Shareholders should recognize that diversity facilitates operational changes that increase value, while also boosting firms' corporate social responsibility profile.

[9]See, for example, Suddath, 2022, "It's Getting Harder to Be a Woman in America," Bloomberg, for a discussion of the recent events that have made women's participation in the corporate world more challenging.

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Table 1: **Descriptive Statistics.** The table reports descriptive statics for our main variables for the period 2018Q1–2021Q4. The supply chain relationships data is from FactSet Revere. Historical headquarters information is parsed from the SEC EDGAR Database. All other firm-level data is from COMPUSTAT. Treated is a dummy variable equal to 1 for firms headquartered in California (treated firms), and 0 for firms headquartered in other states (control firms) in 2017. Number of Suppliers is the number of suppliers with a direct supply chain relationship with the firm (buyer). Refer to Table A1 for detailed variable definitions.

Panel A: Treated Firms: Yes						
Variables	Mean	Std. Dev.	25^{th} Prc.	Median	75^{th} Prc.	Obs.
Number of Suppliers	15.975	38.318	2.000	5.000	12.000	3,639
Size	6.883	1.905	5.591	6.759	7.965	3,639
Gross Margin	0.010	0.402	-0.026	0.088	0.198	3,639
Capital Intensity	0.189	0.207	0.059	0.110	0.224	3,639
Leverage	0.152	0.171	0.027	0.095	0.214	3,639
Panel B: Treated Firms: No						
Variables	Mean	Std. Dev.	25^{th} Prc.	Median	75^{th} Prc.	Obs.
Number of Suppliers	16.810	35.644	2.000	6.000	16.000	19,814
Size	7.574	1.886	6.332	7.608	8.843	19,814
Gross Margin	0.114	0.331	0.059	0.138	0.242	19,814
Capital Intensity	0.314	0.262	0.100	0.216	0.498	19,814
Leverage	0.242	0.188	0.095	0.206	0.354	19,814

Table 2: **Supply Base after the Board Gender Diversity Act.** This table presents estimations from supply chain regressions. The dependent variable is Log of Suppliers, the natural logarithm of the number suppliers. Treated is an indicator for firms headquartered in California (treated firms), and 0 for firms headquartered in other states (control firms) in 2017. Post-Act is an indicator equal to 1 for the quarters 2020Q1 to 2021Q4, and zero for the quarters 2018Q1 to 2019Q4. 2020Q1 is an indicator for quarter 1 of 2020. 2020Q2 – 2021Q4 are defined similarly. Refer to Table A1 for detailed variable definitions. Standard errors reported in parentheses are clustered at the headquarters state level. Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

Dep. variable:	Log of Suppliers				
	Base Model				Adjustment Process Model
	[1]	[2]	[3]	[4]	[5]
Treated × Post-Act	-0.083***	-0.084***	-0.084***	-0.075***	
	(0.021)	(0.020)	(0.020)	(0.020)	
Treated × 2021Q4					-0.080***
					(0.024)
Treated × 2021Q3					-0.056**
					(0.022)
Treated × 2021Q2					-0.101***
					(0.026)
Treated × 2021Q1					-0.090***
					(0.022)
Treated × 2020Q4					-0.085***
					(0.021)
Treated × 2020Q3					-0.064***
					(0.021)
Treated × 2020Q2					-0.078***
					(0.026)
Treated × 2020Q1					-0.049***
					(0.017)
Size	0.178***	0.181***	0.182***	0.182***	0.182***
	(0.025)	(0.026)	(0.026)	(0.034)	(0.034)
Gross Margin		-0.010	-0.011	-0.012	-0.013
		(0.013)	(0.013)	(0.015)	(0.015)
Capital Intensity			0.120	0.102	0.102
			(0.103)	(0.121)	(0.121)
Leverage				-0.086	-0.087
				(0.084)	(0.084)
Firm FE	Yes	Yes	Yes	Yes	Yes
Industry × Quarter FE	Yes	Yes	Yes	Yes	Yes
Incorp. × Quarter FE	Yes	Yes	Yes	Yes	Yes
Clustering	State	State	State	State	State
Obs.	28,414	28,203	28,189	23,453	23,453
R-2 (adjusted)	0.928	0.927	0.928	0.926	0.926

Table 3: **Supply Base after the Board Gender Diversity Act: By Female Director Influence.** This table presents estimations from supply chain regressions. The dependent variable is Log of Suppliers, the natural logarithm of the number suppliers. Treated is an indicator for firms headquartered in California (treated firms), and 0 for firms headquartered in other states (control firms) in 2017. Post-Act is an indicator equal to 1 for the quarters 2020Q1 to 2021Q4, and zero for the quarters 2018Q1 to 2019Q4. High Reach Female Director and Low Reach Female Director are indicators for new female directors (hired in the year ending on December 31, 2019) with a number of professional overlaps above and below the sample 75th percentile in 2019Q4, respectively. Not Relocated and Relocated are indicators for firms that did not relocate or did relocate their headquarters outside of California during 2020Q1-2021Q4, respectively. Refer to Table A1 for detailed variable definitions. Standard errors reported in parentheses are clustered at the headquarters state level. Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

Dep. variable:	Log of Suppliers			
	[1]	[2]	[3]	[4]
Treated × Post-Act × High Reach Female Director	-0.144***	-0.147***		
	(0.022)	(0.025)		
Treated × Post-Act × Low Reach Female Director	-0.069***	-0.041		
	(0.024)	(0.025)		
Treated × Post-Act × Not Relocated			-0.089***	-0.081***
			(0.021)	(0.019)
Treated × Post-Act × Relocated			0.017	0.002
			(0.025)	(0.028)
Size	0.197***	0.201***	0.178***	0.182***
	(0.035)	(0.043)	(0.025)	(0.034)
Gross Margin		-0.028		-0.013
		(0.023)		(0.014)
Capital Intensity		0.116		0.103
		(0.168)		(0.122)
Leverage		-0.078		-0.087
		(0.079)		(0.084)
Firm FE	Yes	Yes	Yes	Yes
Industry × Quarter FE	Yes	Yes	Yes	Yes
Incorp. × Quarter FE	Yes	Yes	Yes	Yes
Clustering	State	State	State	State
Obs.	16,568	13,662	28,414	23,453
Wald Test p-value Interaction Coeff. Differences	<0.001	<0.001	<0.001	<0.001
R-2 (adjusted)	0.934	0.934	0.928	0.926

Table 4: **Buyer-Supplier Relationships after the Board Gender Diversity Act.** This table presents estimations from buyer-supplier relationship regressions. The dependent variables are the Supply Chain Relationship Length (in quarters), Domestic Suppliers, Import Weight/Sales (multiplied by 100), Import Volume/Sales (multiplied by 100), Number of Containers/Sales, and ΔInventory/Assets (multiplied by 100). Treated is an indicator for firms headquartered in California (treated firms), and 0 for firms headquartered in other states (control firms) in 2017. Post-Act is an indicator equal to 1 for the quarters 2020Q1 to 2021Q4, and zero for the quarters 2018Q1 to 2019Q4. Controls include Size, Gross Margin, Capital Intensity, and Leverage. Refer to Table A1 for detailed variable definitions. Standard errors reported in parentheses are clustered at the headquarters state level. Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

Dep. variables:	Supply Chain Relationship Length	Domestic Suppliers	Import Weight/ Sales	Import Volume/ Sales	Number of Containers/ Sales	ΔInventory/ Assets
	[1]	[2]	[3]	[4]	[5]	[6]
Treated × Post-Act	0.385***	0.020***	-0.084***	-0.011***	-0.057***	0.074***
	(0.127)	(0.006)	(0.027)	(0.004)	(0.017)	(0.023)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Incorp. × Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Clustering	State	State	State	State	State	State
Obs.	23,453	23,259	7,553	7,553	7,553	22,716
R-2 (adjusted)	0.787	0.796	0.667	0.736	0.767	0.173

Table 5: **Supplier Characteristics after the Board Gender Diversity Act.** This table presents estimations from buyer-supplier relationship regressions. The dependent variables are Supplier Investment (multiplied by 100), Supplier R&D (multiplied by 100), and Supplier Number of Patents. The sample includes only suppliers that do not overlap across the treated and control groups. Treated is an indicator for firms headquartered in California (treated firms), and 0 for firms headquartered in other states (control firms) in 2017. Post-Act is an indicator equal to 1 for the quarters 2020Q1 to 2021Q4, and zero for the quarters 2018Q1 to 2019Q4. Controls for both buyers and suppliers include Size, Gross Margin, Capital Intensity, and Leverage. Refer to Table A1 for detailed variable definitions. Standard errors reported in parentheses are clustered at the headquarters state level. Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

Dep. variables:	Supplier Investment	Supplier R&D	Supplier Number of Patents
	[1]	[2]	[3]
Treated × Post-Act	0.064***	0.054***	0.184***
	(0.016)	(0.017)	(0.065)
Controls: Buyers	Yes	Yes	Yes
Controls: Suppliers	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Industry × Quarter FE	Yes	Yes	Yes
Incorp. × Quarter FE	Yes	Yes	Yes
Clustering	State	State	State
Obs.	18,474	18,474	7,993
R-2 (adjusted)	0.588	0.621	0.775

Table 6: **Robustness to Alternative Samples, Model Specification, and Clustering.** This table presents estimations from supply chain regressions. The dependent variable is Log of Suppliers, the natural logarithm of the number suppliers. Treated is an indicator for firms headquartered in California (treated firms), and 0 for firms headquartered in other states (control firms) in 2017. Post-Act is an indicator equal to 1 for the quarters 2020Q1 to 2021Q4, and zero for the quarters 2018Q1 to 2019Q4. Post-Act in columns [7] and [8] is an indicator equal to 1 for the years 2020 and 2021, and zero for the years 2018 and 2019. Other controls include Gross Margin, Capital Intensity, and Leverage. Refer to Table A1 for detailed variable definitions. Standard errors reported in parentheses are clustered at the headquarters state level. Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

Panel A: Sample Construction

Dep. variable:	Log of Suppliers							
	Sales>5: No		Sales<2×L.Sales: No		No Filters		Annual Data	
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Treated × Post-Act	-0.069***	-0.060***	-0.090***	-0.088***	-0.051***	-0.063***	-0.091***	-0.084***
	(0.019)	(0.019)	(0.020)	(0.020)	(0.018)	(0.020)	(0.021)	(0.018)
Size	0.145***	0.143***	0.175***	0.179***	0.127***	0.134***	0.187***	0.180***
	(0.019)	(0.023)	(0.024)	(0.029)	(0.014)	(0.021)	(0.026)	(0.027)
Other Controls	No	Yes	No	Yes	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Incorp. × Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clustering	State	State	State	State	State	State	State	State
Obs.	31,636	26,372	29,664	24,472	34,495	27,216	7,115	6,577
R-2 (adjusted)	0.925	0.923	0.926	0.924	0.922	0.922	0.923	0.926

Panel B: Model Specification

Dep. variable:	Log of Suppliers							
	Firm FE: No		Industry × Quarter FE: No		Incorp. × Quarter FE: No		No FEs	
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Treated × Post-Act	-0.121***	-0.136***	-0.077***	-0.076***	-0.073***	-0.065***	-0.143***	-0.146***
	(0.017)	(0.022)	(0.015)	(0.015)	(0.019)	(0.018)	(0.014)	(0.012)
Treated	0.264***	0.214***	Absorbed	Absorbed	Absorbed	Absorbed	0.271***	0.196***
	(0.039)	(0.045)					(0.054)	(0.055)
Post-Act	Absorbed	Absorbed	Absorbed	Absorbed	Absorbed	Absorbed	-0.011	-0.006
							(0.012)	(0.013)
Size	0.430***	0.437***	0.181***	0.181***	0.178***	0.183***	0.418***	0.438***
	(0.016)	(0.016)	(0.024)	(0.034)	(0.026)	(0.035)	(0.016)	(0.017)
Other Controls	No	Yes	No	Yes	No	Yes	No	Yes
Firm FE	No	No	Yes	Yes	Yes	Yes	No	No
Industry × Quarter FE	Yes	Yes	No	No	Yes	Yes	No	No
Incorp. × Quarter FE	Yes	Yes	Yes	Yes	No	No	No	No
Clustering	State	State	State	State	State	State	State	State
Obs.	28,414	23,477	28,414	23,453	28,414	23,453	28,414	23,477
R-2 (adjusted)	0.462	0.468	0.927	0.926	0.928	0.926	0.362	0.374

Panel C: Standard Error Clustering

Dep. variable:	Log of Suppliers							
	Clustering of Std. Errors: Industry		Clustering of Std. Errors: Quarter		Clustering of Std. Errors: State & Industry		Clustering of Std. Errors: State, Industry, & Quarter	
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Treated × Post-Act	-0.083***	-0.075**	-0.083***	-0.075***	-0.083***	-0.075***	-0.083***	-0.075***
	(0.022)	(0.029)	(0.016)	(0.017)	(0.017)	(0.021)	(0.019)	(0.022)
Size	0.178***	0.182***	0.178***	0.182***	0.178***	0.182***	0.178***	0.182***
	(0.026)	(0.031)	(0.013)	(0.012)	(0.024)	(0.034)	(0.024)	(0.033)
Other Controls	No	Yes	No	Yes	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Incorp. × Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clustering	Industry	Industry	Quarter	Quarter	State & Industry	State & Industry	State, Industry, & Quarter	State, Industry, & Quarter
Obs.	28,414	23,453	28,414	23,453	28,414	23,453	28,414	23,453
R-2 (adjusted)	0.928	0.926	0.928	0.926	0.928	0.926	0.928	0.926

Table 7: **Supply Base after the Board Gender Diversity Act: Additional Robustness Tests.** This table presents estimations from supply chain regressions. The dependent variable is Log of Suppliers, the natural logarithm of the number suppliers. Treated is an indicator for firms headquartered in California (treated firms), and 0 for firms headquartered in other states (control firms) in 2017. Post-Act is an indicator equal to 1 for the quarters 2020Q1 to 2021Q4, and zero for the quarters 2018Q1 to 2019Q4. In column [1], the control group includes firms headquartered in states where the top 10 U.S. ports in terms of dollar imports from China in 2015 are located (*http://www.worldportsource.com/trade/imports/value/CHN.php*). In column [2], the control group includes FactSet competitors of the treated firms that are not headquartered in California in 2017. In column [3] the control group includes the top 3 rivals of our treated firms that are not headquartered in California in 2017, identified using the Text-based Network Industry Classifications of Hoberg and Phillips (2010, 2016). Controls include Size, Gross Margin, Capital Intensity, and Leverage. Refer to Table A1 for detailed variable definitions. Standard errors reported in parentheses are clustered at the headquarters state level. Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

Dep. variable:	Log of Suppliers		
	Top 10 U.S. Ports by China Imports	FactSet Competitors	Top 3 TNIC Rivals
	[1]	[2]	[3]
Treated × Post-Act	-0.086**	-0.086***	-0.104***
	(0.030)	(0.028)	(0.035)
Controls	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Industry × Quarter FE	Yes	Yes	Yes
Incorp. × Quarter FE	Yes	Yes	Yes
Clustering	State	State	State
Obs.	12,542	10,464	8,073
R-2 (adjusted)	0.924	0.938	0.927

Table 8: **Board Gender Composition of the Suppliers of Treated Firms after the Board Gender Diversity Act.** This table presents estimations from supplier board gender composition regressions. The dependent variables are Log of Supplier Female Directors (columns 1 and 2), the natural logarithm of the number of female directors of the US and European suppliers, and California Suppliers (columns 3 and 4), the ratio of the number of California suppliers to the total number of suppliers. Treated is an indicator for firms headquartered in California (treated firms), and 0 for firms headquartered in other states (control firms) in 2017. Post-Act is an indicator equal to 1 for the quarters 2020Q1 to 2021Q4, and zero for the quarters 2018Q1 to 2019Q4. Data on the board of directors are from BoardEx. Other controls include Gross Margin, Capital Intensity, and Leverage. Refer to Table A1 for detailed variable definitions. Standard errors reported in parentheses are clustered at the headquarters state level. Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

Dep. variables:	Log of Supplier Female Directors		California Suppliers	
	[1]	[2]	[3]	[4]
Treated × Post-Act	-0.030	0.011	0.004	0.003
	(0.029)	(0.027)	(0.006)	(0.005)
Size	0.160***	0.166***	-0.013**	-0.007*
	(0.032)	(0.042)	(0.006)	(0.004)
Other Controls	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry × Quarter FE	Yes	Yes	Yes	Yes
Incorp. × Quarter FE	Yes	Yes	Yes	Yes
Clustering	State	State	State	State
Obs.	24,100	19,821	28,191	23,259
R-2 (adjusted)	0.882	0.882	0.769	0.775

Table 9: **Material Supply Agreements.** This table reports information from 51 material supply agreements for treated firms (Panel A) and control firms (Panel B). Public firms are required to disclose these agreements as exhibits to their financial filings. We gather the contracts manually from the U.S. Securities and Exchange Commission (SEC) Electronic Data Gathering, Analysis, and Retrieval (EDGAR) database for the sample of treated firms in Table 2 (Panel A) and controls firms in Table A6 (Panel B) for the period 2016-2021. We match the contracts with our sample firms using the CIK identifier of the EDGAR filings. Start Date is the date when the contract becomes effective. Convenience Clause is an indicator for whether the buyer has the right to terminate the supply agreement "at will" even without cause. Trade Credit Maturity is the number of days that the buyer has to pay an invoice from the supplier. N/A fields are either redacted or missing from the contract. The table (Panel C) also reports the coefficient from a regression including Treated × Post-Act, Treated, and Post-Act using as dependent variables Convenience Clause and the natural logarithm of Trade Credit Maturity. Standard errors reported in parentheses are clustered at the headquarters state level. Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

	Panel A: Treated Firms				**Panel B: Control Firms**			
	CIK	**Start Date**	**Convenience Clause**	**Trade Credit Maturity**	**CIK**	**Start Date**	**Convenience Clause**	**Trade Credit Maturity**
Pre-event	1029142	7/27/2016	Yes	30	1428522	1/1/2016	No	30
	1444380	11/11/2016	No	30	1446847	4/26/2016	No	N/A
	1269021	12/9/2016	Yes	30	1419600	7/1/2016	No	N/A
	914475	3/9/2017	Yes	N/A	1270073	8/12/2016	No	N/A
	1020214	4/1/2017	Yes	N/A	1300699	10/24/2016	No	N/A
	1515673	12/7/2017	No	N/A	1590976	11/14/2016	No	60
	1515673	1/31/2018	No	N/A	1517022	5/26/2017	No	N/A
	1070494	2/22/2018	No	N/A	1368514	6/6/2017	No	N/A
	1375151	1/31/2019	Yes	30	805928	6/13/2017	No	30
	1375151	6/17/2019	Yes	30	1494650	7/1/2017	No	N/A
	1551693	11/7/2019	No	30	1494650	8/18/2017	No	N/A
					1104506	10/20/2017	No	N/A
					1293971	11/27/2017	No	N/A
					1428522	2/27/2018	No	30
					1653477	3/8/2018	No	30
					1586105	10/1/2018	No	N/A
					105770	1/1/2019	No	30
					1517022	4/9/2019	Yes	N/A
					1586105	7/30/2019	No	N/A
Post-event	921299	1/1/2020	No	N/A	1060736	2/20/2020	No	N/A
	1271214	1/20/2020	No	30	1517022	3/11/2020	No	N/A
	921299	3/2/2020	Yes	N/A	1060736	4/2/2020	No	N/A
	867773	8/26/2020	No	45	1517022	4/2/2020	Yes	N/A
	921299	9/10/2020	No	N/A	1651561	7/1/2020	No	30
	1438533	9/30/2020	Yes	N/A	1517022	9/1/2020	No	N/A
	1479290	4/6/2021	No	N/A	1300699	11/2/2020	No	45
	808326	8/9/2021	No	45	1300699	1/15/2021	No	45
					1145986	2/3/2021	No	N/A
					1517022	2/10/2021	Yes	N/A
					1479290	4/6/2021	Yes	N/A
					1356576	5/12/2021	No	N/A
					1356576	7/13/2021	Yes	N/A

Panel C: Convenience Clause and Trade Credit Maturity after the Board Gender Diversity Act

Pre-event Mean	0.546	30.000	**Pre-event Mean**	0.053	35.000
Post-event Mean	0.286	40.000	**Post-event Mean**	0.308	40.000
(Post-Pre)$_{Treated}$	-0.260	10.000	**(Post-Pre)$_{Control}$**	0.255	5.000
Obs.	19	9	**Obs.**	32	9
(Post-Pre)$_{Treated}$ - (Post-Pre)$_{Control}$	-0.306**	0.116			
	(0.103)	(0.197)			

Table 10: **Profitability after the Board Gender Diversity Act.** This table presents estimations from profitability regressions. The dependent variable is Profitability, the ratio of net income to lagged sales. Treated is an indicator for firms headquartered in California (treated firms), and 0 for firms headquartered in other states (control firms) in 2017. Post-Act is an indicator equal to 1 for the quarters 2020Q1 to 2021Q4, and zero for the quarters 2018Q1 to 2019Q4. 2020Q1 is an indicator for quarter 1 of 2020. 2020Q2 – 2021Q4 are defined similarly. Base controls include Size, Capital Intensity, and Leverage. Additional controls include R&D, Debt, and Payables. Refer to Table A1 for detailed variable definitions. Standard errors reported in parentheses are clustered at the headquarters state level. Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

Dep. variable:	Profitability	
	[1]	[2]
Treated × Post-Act	0.029***	0.030***
	(0.009)	(0.009)
Base Controls	Yes	Yes
Additional Controls	No	Yes
Firm FE	Yes	Yes
Industry × Quarter FE	Yes	Yes
Incorp. × Quarter FE	Yes	Yes
Clustering	State	State
Obs.	23,606	22,443
R-2 (adjusted)	0.635	0.630

Figure 1: **Sourcing before and after the Board Gender Diversity Act: Testing for Pre-Reform Trends**

This figure plots the coefficients on the interactions of the Treated indicator with year-quarter dummies from quarterly difference-in-difference supplier regressions with pre-shock interactions to test for the parallel trend assumption. The dependent variable is Log of Suppliers, the natural logarithm of the number suppliers. Treated is an indicator for firms headquartered in California (treated firms), and 0 for firms headquartered in other states (control firms) in 2017. The coefficients indicate the percentage change in the number of suppliers for treated firms relative to control firms from 7 quarters prior to the treatment to 8 quarters after the treatment over the period 2018Q1-2021Q4. 2018Q1 is the omitted case. Controls include Size, Gross Margin, Capital Intensity, Leverage, R&D, Growth Prospects, Debt, and Payables. Refer to Table A1 for detailed variable definitions. Ninety-percent confidence intervals are also plotted.

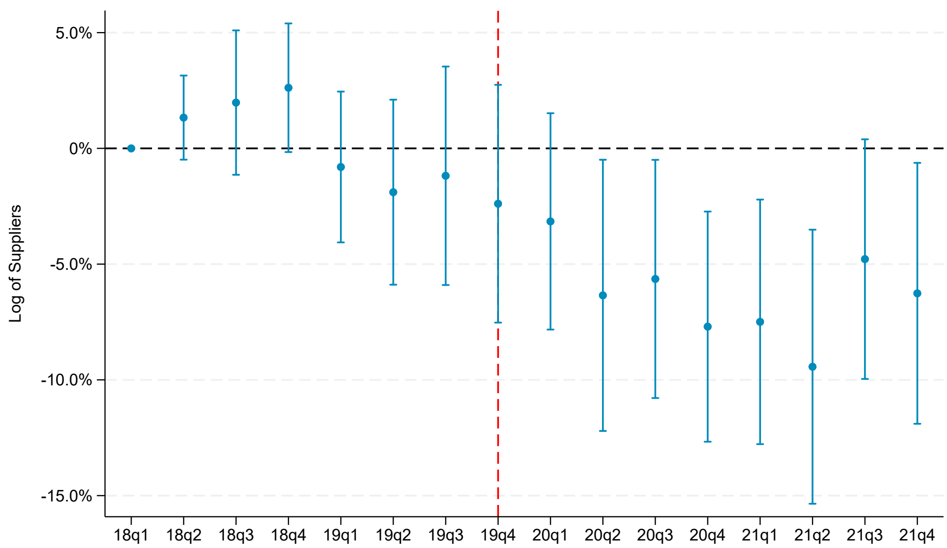
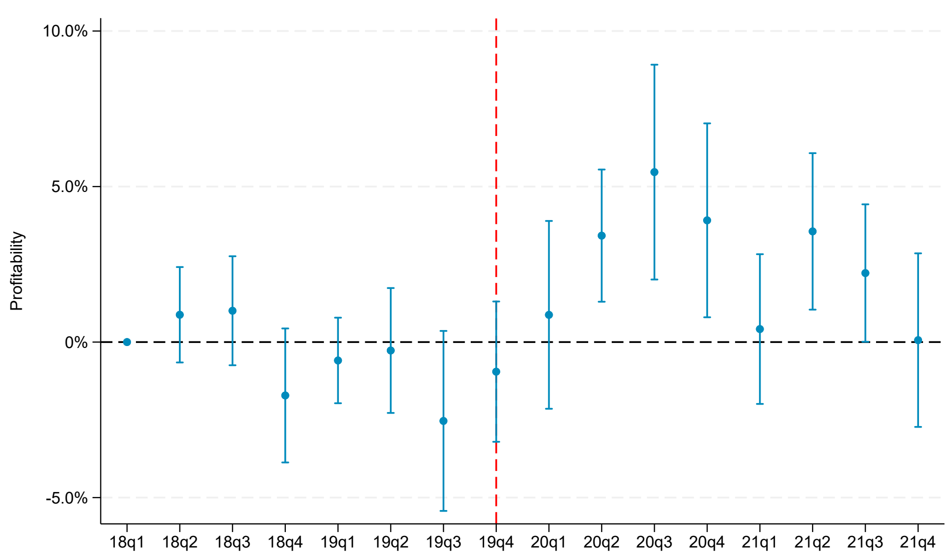


Figure 2: **Profitability before and after the Board Gender Diversity Act: Testing for Pre-Reform Trends**

This figure plots the coefficients on the interactions of the Treated indicator with year-quarter dummies from quarterly difference-in-difference supplier regressions with pre-shock interactions to test for the parallel trend assumption. The dependent variable is Profitability, the ratio of net income to lagged sales. Treated is an indicator for firms headquartered in California (treated firms), and 0 for firms headquartered in other states (control firms) in 2017. The coefficients indicate the percentage change in the number of suppliers for treated firms relative to control firms from 7 quarters prior to the treatment to 8 quarters after the treatment over the period 2018Q1-2021Q4. 2018Q1 is the omitted case. Controls include Size, Capital Intensity, Leverage, R&D, Debt, and Payables. Refer to Table A1 for detailed variable definitions. Ninety-percent confidence intervals are also plotted.



E-Companion to

Board Gender Diversity and Buyer-Supplier Relationships

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Table A1: **Key Variables.** This table provides the definitions of the main variables used in this article.

Main firm's level variables:	**Definition**
Log of Suppliers	The natural logarithm of the number of suppliers with a direct supply chain relationship with the firm. Data on suppliers is from the FactSet Revere Supply Chain Relationships database. Base sample period 2018Q1–2021Q4.
Treated	Indicator for firms headquartered in California in 2017. Historical headquarters information is parsed from corporate filings on the SEC EDGAR Database. Base sample period 2018Q1–2021Q4.
Size	The natural logarithm of book assets (COMPUSTAT's item atq). Base sample period 2018Q1–2021Q4.
Gross Margin	The ratio of earnings before interest, taxes, depreciation, and amortization (COMPUSTAT's item oibdpq) to sales. Base sample period 2018Q1–2021Q4.
Capital Intensity	The ratio of property, plant, and equipment (COMPUSTAT's item ppentq) to book assets. Base sample period 2018Q1–2021Q4.
Leverage	The ratio of total debt (COMPUSTAT's items dlcq + dlttq) to market value of assets (COMPUSTAT's items atq + prccq $\times$ cshoq - ceqq - txditcq). Base sample period 2018Q1–2021Q4.

Additional firm's level variables:	**Definition**
Supply Chain Relationship Length	Equally weighted length (in quarters) of all existing supply chain relationships for the firm from the date when the relationships started. Data on suppliers is from the FactSet Revere Supply Chain Relationships database. Base sample period 2018Q1–2021Q4.
Domestic Suppliers	The ratio of the number of domestic suppliers to the total number of suppliers. Data on suppliers is from the FactSet Revere Supply Chain Relationships database. Base sample period 2018Q1–2021Q4.
Import Weight/Sales	The ratio of the quarterly sum of imports weight (tons) over lagged sales (COMPUSTAT item saleq). We multiply the variable by 100. Data on imports is from Panjiva. Base sample period 2018Q1–2021Q4.
Import Volume/Sales	The ratio of the quarterly sum of imports volume (TEU) over lagged sales (COMPUSTAT item saleq). We multiply the variable by 100. Data on imports is from Panjiva. Base sample period 2018Q1–2021Q4.
Number of Containers/Sales	The ratio of the quarterly sum of the number of imported containers over lagged sales (COMPUSTAT item saleq). Data on imports is from Panjiva. Base sample period 2018Q1–2021Q4.
ΔInventory/Assets	The ratio of changes in inventory over lagged assets (COMPUSTAT items (invtq-l.invtq)/l.atq). We multiply the variable by 100. Base sample period 2018Q1–2021Q4.
Supplier Investment	The ratio of capital expenditures to book assets of all domestic and foreign suppliers of the firm (buyer) with data available in WORLDSCOPE. We multiply the variable by 100. Base sample period 2018Q1–2021Q4.
Supplier R&D	The ratio of R&D expenses to book assets of all domestic and foreign suppliers of the firm (buyer) with data available in WORLDSCOPE. We multiply the variable by 100. Base sample period 2018Q1–2021Q4.

Table A1 continued.

Variable	Definition
Supplier Number of Patents	The natural logarithm of the number of US patents of domestic and foreign suppliers of the firm (buyer). The US patent data is parsed from the PatentsView database of the United States Patent and Trademark Office (USPTO). The patent data is manually matched with the suppliers in our sample. Base sample period 2018Q1–2021Q4.
Supplier Number of Patent Citations	The natural logarithm of the number of US patent citations of domestic and foreign suppliers of the firm (buyer). The US patent data is parsed from the PatentsView database of the United States Patent and Trademark Office (USPTO). The patent data is manually matched with the suppliers in our sample. Base sample period 2018Q1–2021Q4.
Low (High)-Disruption Risk Supply Base	Indicator for firms without (with) suppliers in the top 5 states by flooding risk in 2017Q4. Flooding risk is based on the percentage of the state in special flood hazard areas (Federal Emergency Management Agency). The top 5 states include Louisiana (top 1), Florida, Mississippi, Arkansas, and New Jersey (top 5). Base sample period 2018Q1–2021Q4.
Low (High)-Default Risk Supply Base	Indicator for firms with domestic and foreign suppliers with a combined Altman's z-score higher (lower) than 1.8 in 2017Q4. The Altman's z-score (Altman, 1968) is computed as follows: ($1.2\times X_1 + 1.4\times X_2 + 3.3\times X_3 + 0.6\times X_4 + 1.0\times X_5$), where X_1 is equal to the ratio of working capital to assets, X_2 is equal to the ratio of retained earnings to assets, X_3 is equal to the ratio of earnings before interest and taxes to assets, X_4 is the ratio of market value of equity to debt X_5 is the ratio of sale to assets. Supplier data is from WORLDSCOPE. Base sample period 2018Q1–2021Q4.
Low (High)-Trade Uncertainty Supply Base	Indicator for firms without (with) Chinese suppliers on December 31st 2017 (the year before the US-China trade war started). Base sample period 2018Q1–2021Q4.
Log of Supplier Female Directors	The natural logarithm of the number of female directors of the US and European suppliers of the firm. Data on board of directors are from BoardEx. The sample includes all publicly listed firms, except financial firms (SICs 6000-6999). Sample period 2018Q1–2021Q4.
California Suppliers	The ratio of the number of California suppliers to the total number of suppliers. Data on suppliers is from the FactSet Revere Supply Chain Relationships database. Base sample period 2018Q1–2021Q4.
Profitability	The ratio of net income (COMPUSTAT item niq) to lagged sales (COMPUSTAT's item saleq). Base sample period 2018Q1–2021Q4.
R&D	The ratio of R&D (COMPUSTAT's item xrdq) to book assets. Base sample period 2018Q1–2021Q4.
Growth Prospects	The ratio of the market value of total assets (COMPUSTAT's items atq – ceqq + prccq × cshoq – txditcq) to total book assets (COMPUSTAT item atq). Base sample period 2018Q1–2021Q4.
Debt Maturity	The lagged ratio of short-term debt (COMPUSTAT's item dlcq) to total debt (COMPUSTAT's items dlcq + dlttq). Base sample period 2018Q1–2021Q4.
Payables	The ratio of payables (COMPUSTAT's item apq) to book assets. Base sample period 2018Q1–2021Q4.
Log of Female Directors	The natural logarithm of the total number of female directors. Data on board of directors are from BoardEx. The sample includes all publicly listed firms, except financial firms (SICs 6000-6999). Sample period 2018Q1–2021Q4.

Table A1 continued.

Variable	**Definition**
Suppliers/Sales	The ratio of the number of suppliers to sales. Data on suppliers is from the FactSet Revere Supply Chain Relationships database. Base sample period 2018Q1–2021Q4.
Suppliers/Assets	The ratio of the number of suppliers to book assets. Data on suppliers is from the FactSet Revere Supply Chain Relationships database. Base sample period 2018Q1–2021Q4.

Table A2: **Board Gender Composition for Treated Firms after 2018Q3.** This table presents estimations from board gender composition regressions. The dependent variable is Log of Female Directors, the natural logarithm of the number of female directors . Treated is an indicator for firms headquartered in California (treated firms), and 0 for firms headquartered in other states (control firms) in 2017. Post-2018Q3 is an indicator equal to 1 for the quarters 2018Q4 and following quarters. Data on the board of directors is from BoardEx. Sample periods range from 2017Q4-2019Q3 (column 1), 2017Q3-2019Q4 (column 2), 2016Q4-2020Q3 (column 3), 2015Q4-2021Q3 (column 4), and 2015Q3-2021Q4 (column 5), respectively. Other controls include Gross Margin, Capital Intensity, and Leverage. Refer to Table A1 for detailed variable definitions. Standard errors reported in parentheses are clustered at the headquarters state level. Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

Dep. variable:	Log of Female Directors				
	2017Q4-2019Q3	2017Q3-2019Q4	2016Q4-2020Q3	2015Q4-2021Q3	2015Q3-2021Q4
	[1]	[2]	[3]	[4]	[5]
Treated × Post-2018Q3	0.060***	0.059***	0.073***	0.086***	0.088***
	(0.008)	(0.009)	(0.010)	(0.010)	(0.010)
Size	0.039*	0.041**	0.077***	0.086***	0.087***
	(0.022)	(0.020)	(0.014)	(0.012)	(0.012)
Other Controls	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Industry × Quarter FE	Yes	Yes	Yes	Yes	Yes
Incorp. × Quarter FE	Yes	Yes	Yes	Yes	Yes
Clustering	State	State	State	State	State
Obs.	13,775	17,385	27,273	38,862	41,584
R-2 (adjusted)	0.907	0.901	0.870	0.851	0.848

Table A3: **Propensity of Being Headquartered in California after 2018Q3.** This table presents estimations from board gender composition regressions. The dependent variable is California HQ's (Yes=1), a time-varying indicator for firms headquartered in California. Treated is an indicator for firms headquartered in California (treated firms), and 0 for firms headquartered in other states (control firms) in 2017. Post-2018Q3 is an indicator equal to 1 for the quarters 2018Q4 and following quarters. Historical quarterly headquarters information is parsed from corporate filings on the SEC EDGAR Database. Sample period ranges from 2017Q4-2019Q3. Other controls include Gross Margin, Capital Intensity, and Leverage. Refer to Table A1 for detailed variable definitions. Standard errors reported in parentheses are clustered at the headquarters state level. Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

Dep. variable:	California HQ's (Yes=1)	
	[1]	[2]
Treated × Post-2018Q3	-0.032***	-0.029***
	(0.002)	(0.002)
Size	0.003	-0.005
	(0.003)	(0.008)
Other Controls	Yes	Yes
Firm FE	Yes	Yes
Industry × Quarter FE	Yes	Yes
Incorp. × Quarter FE	Yes	Yes
Clustering	State	State
Obs.	18,803	15,031
R-2 (adjusted)	0.984	0.985

Table A4: **Descriptive Statistics.** This table reports descriptive statistics for the additional variables used in this article for the Treated: Yes firms (Panel A) and the Treated: No firms (Panel B). Base sample period 2018Q1–2021Q4. Refer to Table A1 for detailed variable definitions.

Panel A: Treated Firms: Yes						
Variables	Mean	Std. Dev.	25^{th} Prc.	Median	75^{th} Prc.	Obs.
Supply Chain Relationship Length	9.506	5.899	5.667	8.200	12.000	3,639
Domestic Suppliers	0.640	0.301	0.444	0.667	1.000	3,594
Import Weight/Sales	0.335	0.960	0.002	0.018	0.238	978
Import Volume/Sales	0.054	0.127	0.001	0.005	0.051	978
Num. of Containers/Sales	0.364	0.761	0.010	0.063	0.369	978
ΔInventory/Assets	0.304	1.780	-0.048	0.000	0.456	3,548
Supplier Investment	0.763	0.978	0.172	0.314	0.928	3,543
Supplier R&D	0.614	0.968	0.031	0.142	0.879	3,543
Supplier Number of Patents	4.083	2.331	2.197	3.932	6.180	2,539
Log of Supplier Female Directors	2.225	1.093	1.386	2.079	2.862	3,036
California Suppliers	0.245	0.256	0.000	0.182	0.375	3,594
Profitability	-0.015	0.093	-0.013	0.003	0.011	3,639
R&D	0.022	0.025	0.000	0.015	0.035	3,639
Growth Prospects	3.128	2.390	1.410	2.213	4.019	3,639
Inventory	0.091	0.117	0.000	0.049	0.140	3,565
Payables	0.074	0.084	0.020	0.044	0.097	3,639
Log of Female Directors	0.975	0.472	0.693	1.099	1.386	3,000
Suppliers/Sales	0.092	0.148	0.012	0.030	0.097	3,639
Suppliers/Assets	0.018	0.034	0.002	0.005	0.015	3,639

Panel B: Treated Firms: No						
Variables	Mean	Std. Dev.	25^{th} Prc.	Median	75^{th} Prc.	Obs.
Supply Chain Relationship Length	9.362	6.181	5.500	8.100	11.333	19,814
Domestic Suppliers	0.638	0.300	0.450	0.667	0.903	19,666
Import Weight/Sales	0.369	1.066	0.004	0.032	0.190	6,575
Import Volume/Sales	0.044	0.132	0.001	0.004	0.023	6,575
Num. of Containers/Sales	0.323	0.789	0.008	0.050	0.219	6,575
ΔInventory/Assets	0.203	1.811	-0.121	0.000	0.404	19,172
Supplier Investment	0.571	0.736	0.162	0.260	0.688	18,998
Supplier R&D	0.355	0.783	0.025	0.033	0.255	18,998
Supplier Number of Patents	3.439	2.360	1.609	3.219	5.263	12,029
Log of Supplier Female Directors	2.366	1.132	1.609	2.303	3.135	16,785
California Suppliers	0.099	0.178	0.000	0.000	0.143	19,666
Profitability	-0.017	0.121	-0.006	0.008	0.017	19,813
R&D	0.007	0.016	0.000	0.000	0.005	19,814
Growth Prospects	2.045	1.531	1.130	1.506	2.354	19,814
Inventory	0.101	0.124	0.006	0.060	0.150	19,269
Payables	0.072	0.070	0.024	0.050	0.094	19,702
Log of Female Directors	0.979	0.483	0.693	1.099	1.386	16,918
Suppliers/Sales	0.060	0.133	0.007	0.015	0.043	19,814
Suppliers/Assets	0.012	0.031	0.001	0.003	0.008	19,812

Table A5: **Pre-Act Mean Difference and Distributional Tests for Treated and Control Firms: Matched Samples.** This table reports the mean difference t-test p-value and distributional test p-value of firm characteristics for treated firms (Treated: Yes) and control firms (Treated: No) in 2019Q4 for two samples based on matching on Size, Gross Margin, Capital Intensity and exact matching on 2-digit SIC industry codes (Panel A: Matched Sample 1) and matching on Size and exact matching on board size and number of female directors (Panel B: Matched Sample 2). We use the Abadie-Imbens matching estimator to identify the control firms from the universe of control firms (Abadie and Imbens, 2006). Refer to Table A1 for detailed variable definitions.

Panel A: Matched Sample 1		Mean	Treated–Control	Mean Difference t-test p-value	Distribution Test p-value
Size	Treated	6.869	0.020	0.885	0.958
	Control	6.849			
Gross Margin	Treated	0.009	-0.017	0.553	0.887
	Control	0.026			
Capital Intensity	Treated	0.203	-0.005	0.776	0.670
	Control	0.208			
Leverage	Treated	0.169	-0.016	0.284	0.261
	Control	0.185			
Panel B: Matched Sample 2		Mean	Treated–Control	Mean Difference t-test p-value	Distribution Test p-value
Size	Treated	6.901	0.015	0.918	0.991
	Control	6.886			

Table A6: **Supply Base after the Board Gender Diversity Act: Matched Samples.** This table presents estimations from supply chain regressions. The dependent variable is Log of Suppliers, the natural logarithm of the number suppliers. Treated is an indicator for firms headquartered in California (treated firms), and 0 for firms headquartered in other states (control firms) in 2017. Post-Act is an indicator equal to 1 for the quarters 2020Q1 to 2021Q4, and zero for the quarters 2018Q1 to 2019Q4. We use the Abadie-Imbens matching estimator to identify matched-control firms from the universe of control firms (Abadie and Imbens, 2006). In 2019Q4, we match treated and control firms on Size, Gross Margin, Capital Intensity, and exact matching on 2-digit SIC industry codes (Panel A: Matched Sample 1) and Size and exact match on board size and number of female directors (Panel B: Matched Sample 2). Refer to Table A1 for detailed variable definitions. Standard errors reported in parentheses are clustered at the headquarters state level. Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

Dep. variable:	Log of Suppliers			
	Panel A: Matched Sample 1		**Panel B: Matched Sample 2**	
	[1]	[2]	[3]	[4]
Treated × Post-Act	-0.099**	-0.137***	-0.117***	-0.142***
	(0.042)	(0.046)	(0.040)	(0.044)
Size	0.156***	0.171***	0.144***	0.156***
	(0.025)	(0.031)	(0.025)	(0.043)
Gross Margin		0.055		-0.013
		(0.034)		(0.055)
Capital Intensity		0.533**		0.493**
		(0.213)		(0.236)
Leverage		-0.112		-0.399***
		(0.167)		(0.121)
Firm FE	Yes	Yes	Yes	Yes
Industry × Quarter FE	Yes	Yes	Yes	Yes
Incorp. × Quarter FE	Yes	Yes	Yes	Yes
Clustering	State	State	State	State
Obs.	8,064	6,206	7,765	6,062
R-2 (adjusted)	0.914	0.915	0.922	0.917

Table A7: **Supply Base for Treated Firms after the Board Gender Diversity Act: Additional Control Variables.** This table presents estimations from supply chain regressions. The dependent variable is Log of Suppliers, the natural logarithm of the number suppliers. Treated is an indicator for firms headquartered in California (treated firms), and 0 for firms headquartered in other states (control firms) in 2017. Post-Act is an indicator equal to 1 for the quarters 2020Q1 to 2021Q4, and zero for the quarters 2018Q1 to 2019Q4. Refer to Table A1 for detailed variable definitions. Standard errors reported in parentheses are clustered at the headquarters state level. Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

Dep. variable:	Log of Suppliers				
	[1]	[2]	[3]	[4]	[5]
Treated × Post-Act	-0.075***	-0.075***	-0.076***	-0.059***	-0.059***
	(0.020)	(0.020)	(0.020)	(0.019)	(0.019)
Size	0.182***	0.184***	0.188***	0.190***	0.196***
	(0.034)	(0.035)	(0.036)	(0.037)	(0.039)
Gross Margin	-0.012	-0.011	-0.013	-0.011	-0.011
	(0.015)	(0.015)	(0.014)	(0.019)	(0.019)
Capital Intensity	0.102	0.099	0.095	0.102	0.108
	(0.121)	(0.122)	(0.122)	(0.115)	(0.118)
Leverage	-0.086	-0.086	-0.058	-0.030	-0.023
	(0.084)	(0.084)	(0.083)	(0.074)	(0.072)
R&D		0.325	0.257	0.800	0.759
		(0.499)	(0.499)	(0.510)	(0.513)
Growth Prospects			0.011**	0.013**	0.013**
			(0.005)	(0.005)	(0.005)
Debt Maturity				-0.018	-0.018
				(0.035)	(0.035)
Payables					0.289
					(0.221)
Firm FE	Yes	Yes	Yes	Yes	Yes
Industry × Quarter FE	Yes	Yes	Yes	Yes	Yes
Incorp. × Quarter FE	Yes	Yes	Yes	Yes	Yes
Clustering	State	State	State	State	State
Obs.	23,453	23,453	23,453	22,408	22,296
R-2 (adjusted)	0.926	0.926	0.926	0.931	0.931

Table A8: **Supply Base for Treated Firms after the Board Gender Diversity Act: Alternative Sourcing Variables.** This table presents estimations from supply chain regressions. The dependent variables are Suppliers to Sales (columns 1 and 2), the ratio of the number of suppliers to sales, and Suppliers to Assets (columns 3 and 4), the ratio of the number of suppliers to book assets. Treated is an indicator for firms headquartered in California (treated firms), and 0 for firms headquartered in other states (control firms) in 2017. Post-Act is an indicator equal to 1 for the quarters 2020Q1 to 2021Q4, and zero for the quarters 2018Q1 to 2019Q4. Other controls include Gross Margin, Capital Intensity, and Leverage. Refer to Table A1 for detailed variable definitions. Standard errors reported in parentheses are clustered at the headquarters state level. Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

Dep. variables:	Suppliers/Sales		Suppliers/Assets	
	[1]	[2]	[3]	[4]
Treated × Post-Act	-0.017***	-0.021***	-0.003***	-0.004***
	(0.002)	(0.003)	(0.000)	(0.001)
Size	-0.023***	-0.025***	-0.009***	-0.010***
	(0.003)	(0.004)	(0.001)	(0.001)
Other Controls	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry × Quarter FE	Yes	Yes	Yes	Yes
Incorp. × Quarter FE	Yes	Yes	Yes	Yes
Clustering	State	State	State	State
Obs.	28,414	23,453	28,407	23,451
R-2 (adjusted)	0.856	0.860	0.884	0.884

Table A9: **Supply Base for Treated Firms after Placebo Acts.** This table presents estimations from supply chain regressions. The dependent variable is Log of Suppliers, the natural logarithm of the number suppliers. In the base case, Treated is an indicator for firms headquartered in California (treated firms), and 0 for firms headquartered in other states (control firms) in 2017. Post-Act is an indicator equal to 1 for the quarters 2020Q1 to 2021Q4, and zero for the quarters 2018Q1 to 2019Q4. In the partial treatment estimation with 4 post-Act quarters, Treated is an indicator for firms headquartered in California (treated firms), and 0 for firms headquartered in other states (control firms) in 2016. Post-2018Q4 is an indicator equal to 1 for the quarters 2019Q1 to 2020Q4, and zero for the quarters 2017Q1 to 2018Q4. This estimation includes four placebo quarters (2019Q1 to 2019Q4) and four post reform quarters (2020Q1 to 2020Q4) in the post-estimation period. In the partial treatment estimation with 2 post-Act quarters, Treated is an indicator for firms headquartered in California (treated firms), and 0 for firms headquartered in other states (control firms) in 2015. Post-2018Q2 is an indicator equal to 1 for the quarters 2018Q3 to 2020Q2, and zero for the quarters 2016Q3 to 2018Q2. This estimation includes six placebo quarters (2018Q3 to 2019Q4) and two post reform quarters (2020Q1 and 2020Q2) in the post-estimation period. In the first placebo estimation, Treated is an indicator for firms headquartered in California (treated firms), and 0 for firms headquartered in other states (control firms) in 2014. Post-2017Q2 is an indicator equal to 1 for the quarters 2017Q3 to 2019Q2, and zero for the quarters 2015Q3 to 2017Q2. The treated and post indicators are defined similarly for the other non-overlapping placebo periods: 2011Q3 – 2015Q2; 2007Q3 – 2011Q2; 2003Q3 – 2007Q2. All estimations include the following control variables: Size, Gross Margin, Capital Intensity, and Leverage. Refer to Table A1 for detailed variable definitions. Standard errors reported in parentheses are clustered at the headquarters state level. Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

Dep. variable:	Log of Suppliers [1]	Obs. [2]	Sample Period [3]
Base case: 8 post-Act quarters			
Treated × Post-Act	-0.075***		
	(0.020)	23,453	2018Q1 – 2021Q4
Partial treatment: 4 post-Act quarters			
Treated × Post-2018Q4	-0.060**		
	(0.021)	23,834	2017Q1 – 2020Q4
Partial treatment: 2 post-Act quarters			
Treated × Post-2018Q2	-0.015		
	(0.019)	23,714	2016Q3 – 2020Q2
Placebo estimations:			
Treated × Post-2017Q2	-0.024		
	(0.014)	23,408	2015Q3 – 2019Q2
Treated × Post-2013Q2	-0.020		
	(0.019)	21,798	2011Q3 – 2015Q2
Treated × Post-2009Q2	0.026		
	(0.018)	21,969	2007Q3 – 2011Q2
Treated × Post-2005Q2	-0.019		
	(0.020)	24,676	2003Q3 – 2007Q2

Table A10: **Supply Base for Treated Firms after the Board Gender Diversity Act: Excluding Potentially Affected Control Firms.** This table presents estimations from supply chain regressions. The dependent variable is Log of Suppliers, the natural logarithm of the number suppliers. Treated is an indicator for firms headquartered in California (treated firms), and 0 for firms headquartered in other states (control firms) in 2017. Post-Act is an indicator equal to 1 for the quarters 2020Q1 to 2021Q4, and zero for the quarters 2018Q1 to 2019Q4. The sample excludes control firms headquartered in potentially affected states, Colorado, Hawaii, Illinois, Maryland, Massachusetts, Michigan, New Jersey, New York, Ohio, Pennsylvania, and Washington. Other controls include Gross Margin, Capital Intensity, and Leverage. Refer to Table A1 for detailed variable definitions. Standard errors reported in parentheses are clustered at the headquarters state level. Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

Dep. variable:	Log of Suppliers	
	[1]	[2]
Treated × Post-Act	-0.065***	-0.055**
	(0.023)	(0.024)
Size	0.161***	0.156***
	(0.031)	(0.045)
Other Controls	No	Yes
Firm FE	Yes	Yes
Industry × Quarter FE	Yes	Yes
Incorp. × Quarter FE	Yes	Yes
Clustering	State	State
Obs.	17,978	14,626
R-2 (adjusted)	0.920	0.917

Figure A1: **Post-Act Change in Number of Suppliers for Treated Firms (%)**

This figure plots the coefficients on the interaction terms from Table 2, column 5. The coefficients indicate the percentage change in the number of suppliers for treated firms relative to control firms in the period 2020Q1 to 2021Q4 compared to 2018Q1-2019Q4.

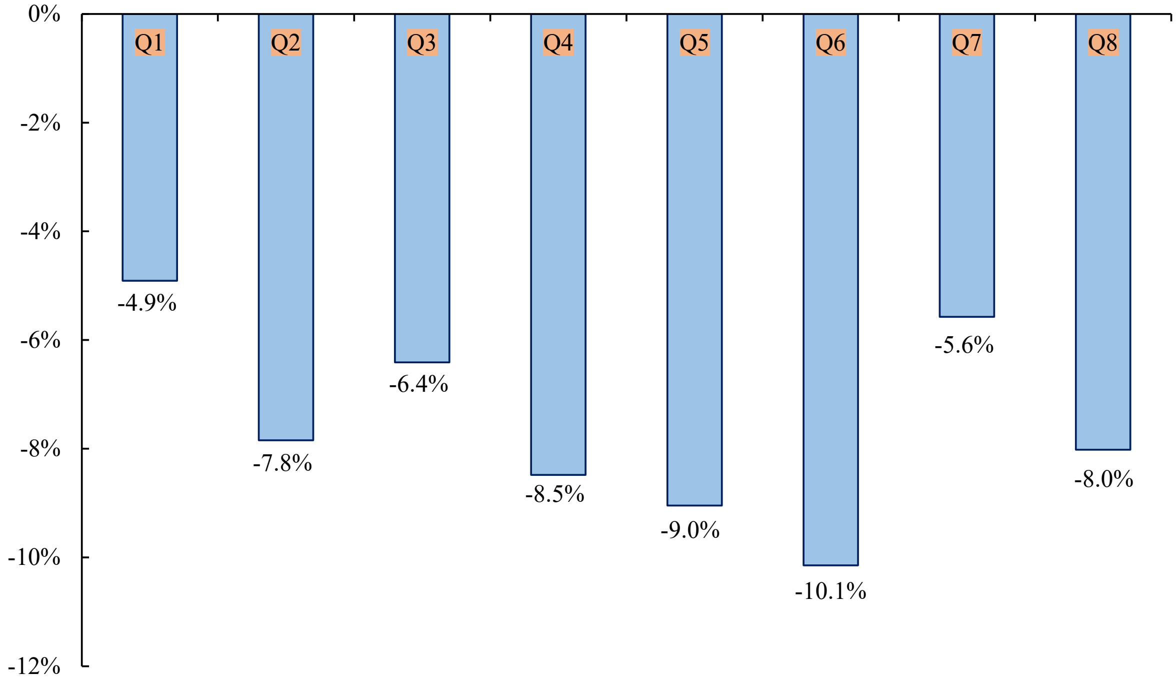

Figure A2: **Pre-Act Distributional Differences Between Treated and Control Firms: Before and After Matching**

This figure presents the kernel density function of firm characteristics for treated firms (Treated: Yes) and control firms (Treated: No) in 2019Q4 for the main sample (column 1) and the matched sample (column 2). In the matched samples, we use the Abadie-Imbens matching estimator to identify matched-control firms from the universe of control firms (Abadie and Imbens, 2006). We match treated and control firms on Size, Gross Margin, Capital Intensity, and exact match on 2-digit SIC industry codes (Panel A: Matched Sample 1) and Size and exact match on board size and number of female directors (Panel B: Matched Sample 2). Refer to Table A1 for detailed variable definitions.

Panel A: Matched Sample 1

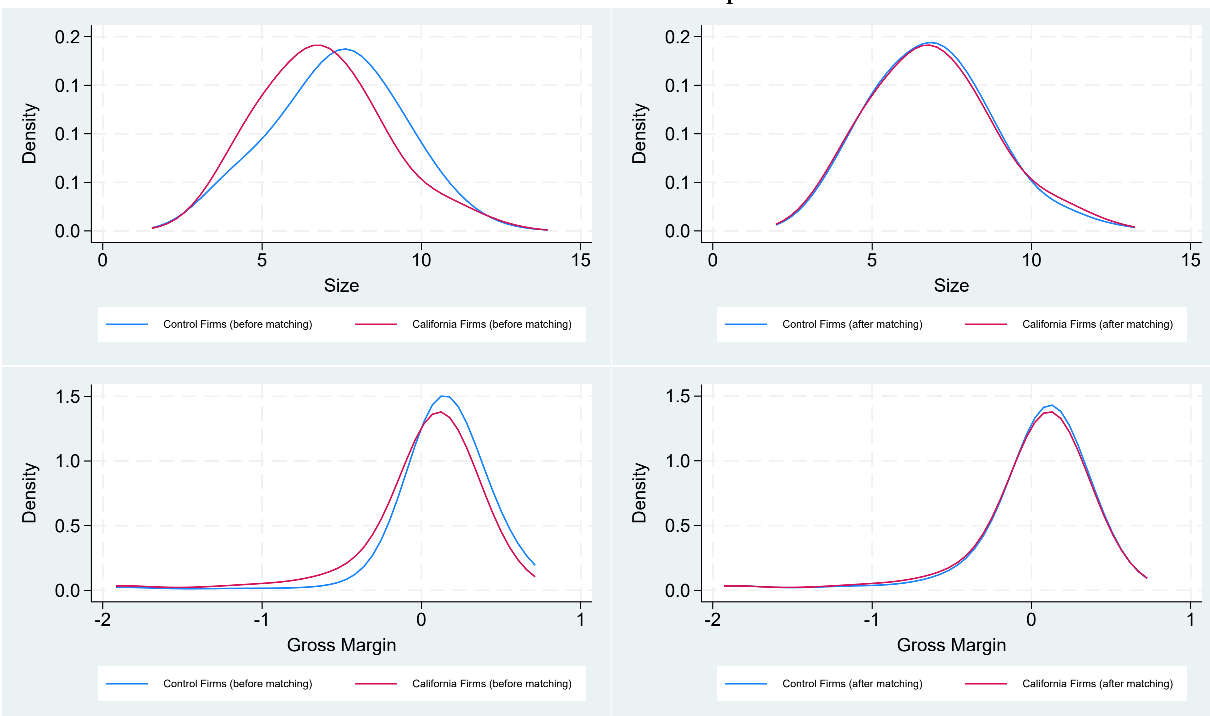

Figure A2: **Pre-Act Distributional Differences Between Treated and Control Firms: Before and After Matching (cont.)**

Panel A: Matched Sample 1 (cont.)

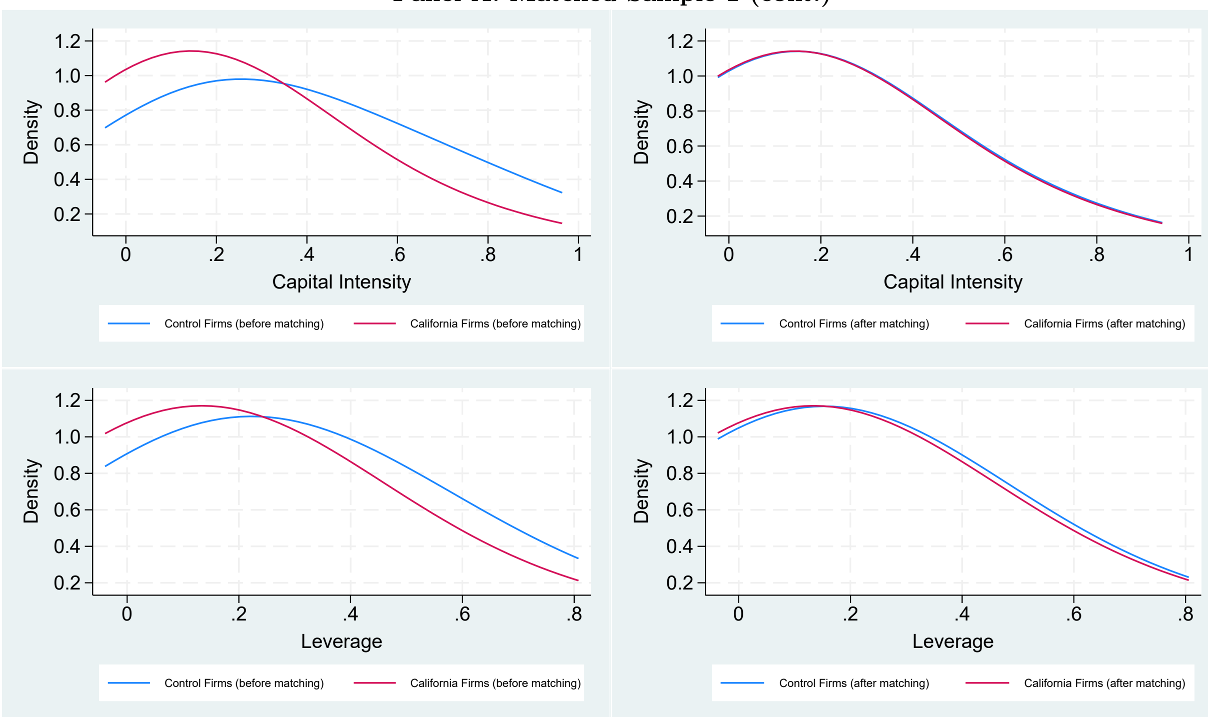

Figure A2: **Pre-Act Distributional Differences Between Treated and Control Firms: Before and After Matching (cont.)**

Panel B: Matched Sample 2