As California goes, so goes the nation? Board gender quotas and the legislation of non-economic values*

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Abstract

In 2018, California became the first U.S. state to introduce a mandatory board gender quota for all firms headquartered in the state. Even though the constitutionality of the law is still debated, we document large negative announcement returns to the adoption of the gender quota for California firms and large spillover effects for non-California firms. We show that these effects are not explained by frictions in the director labor market and propose a novel explanation: Shareholders' disapproval of the government's attempt to legislate non-economic values. Consistently, we find that non-California firms in states that followed California's legislative lead in the past by, e.g., introducing gender quota proposals, adopting stricter environmental laws, raising minimum wages, or legalizing cannabis react more strongly to the California gender quota. We also find that California and non-California firms with higher sensitivity to policy uncertainty react more negatively to the quota's adoption.

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1 Introduction

Women are still heavily underrepresented in corporate leadership positions. Numbers based on the Russell 3000 index, which represents approximately 98% of U.S. equity market capitalization, show that as of June 2018, women held 16.9% of director positions (see Figure 1). Furthermore, the fraction of female directors in the U.S. is growing slowly, at an average annual rate of 0.8%. If no proactive measures are taken and the current rate of growth remains unchanged, it would take more than 40 years to achieve gender parity at U.S. boards. Assuming that innate talent is equally distributed among men and women, this means that the economy will be operating inside the efficient frontier for many years to come (Bertrand (2017)).

Several countries have responded to gender inequality in boardrooms by adopting mandatory quotas. The first country to act was Norway, which introduced a gender quota of 40% female representation in 2003 (Ahern and Dittmar (2012); Matsa and Miller (2013); Eckbo, Nygaard, and Thorburn (2021)). Following Norway's lead, Belgium, France, Germany, Iceland, India, Israel, Italy, and Portugal have all established similar quotas.[1]

In the U.S., California was the first state to adopt a mandatory board gender quota on September 30, 2018. Senate Bill 826 requires that all national and foreign companies headquartered in California with a listing at a major U.S. stock exchange have at least one female director on their board by the end of 2019. Two female directors must be appointed to boards with five members, and three female directors must be appointed to boards with six members or more by the end of 2021. The Bill was presented to the Governor's office on September 10$^{\text{th}}$, 2018. Governor Brown is known for his willingness to veto bills (he vetoed 12% of bills in 2017 and 15% in 2016).[2] He did not initially announce what he would do with Senate Bill 826, and contemporary news and commentary indicated that it was uncertain whether he would sign the Bill.[3] On Sunday September 30$^{\text{th}}$, 2018, he signed SB 826 into law and announced it on the same day.

Even though signed into law, the effectiveness of the California gender quota is strongly de-

[1] More information on gender quotas introduced around the world, can be found here: https://www.idea.int/data-tools/data/gender-quotas.

[2] See Angela Hart, *Jerry Brown consistently signs more bills than GOP governors*, Sacramento Bee, Oct. 17, 2017. See also Tim Arango and Jose A. Del Real, *5 Takeaways from California Gov. Jerry Brown's Last Bill Signing Session*, The New York Times, Oct. 1, 2018 (Governor Brown has a "willingness to wield the veto pen").

[3] See also Jorge L. Ortiz, *Gender quotas: California ponders breakthrough bill to boost female executives*, USA Today, Sept. 18, 2018 ("As California Gov. Jerry Brown ponders whether to sign a landmark bill...").

bated. First, the statute is non-criminal with comparably weak penalties, including a payment of $100,000 for the first violation and $300,000 for each subsequent violation. The maximum fine imposable under the requirements of the Bill is $900,000 per year for an all-male board with six or more directors. Second, it is still an open question whether the quota is constitutional, as it may conflict with the corporate internal affairs doctrine as well as California and U.S. federal civil rights laws (Grundfest (2019)). Fisch and Solomon (2019) and Grundfest (2019) argue that the law is unconstitutional because it tries to apply a California corporate law to firms incorporated in other states or countries. As of the date of this paper, two lawsuits have been filed challenging the law on the grounds that it violates the equal protection clause of the U.S. constitution.[4] Hence, if adjusted to comply with the corporate internal affairs doctrine, the law would have very limited reach, given that only 72 companies are chartered and headquartered in California (Grundfest (2019)). Third, it is questionable whether the quota results in major board overhauls as firms are not obliged to replace directors and may simply add a limited number of female directors to incumbent male-dominated boards. Hence, it is still unclear whether the law will help to promote women into key board positions. Taken together, one may not expect large reactions to the adoption of the gender quota given its low penalties, potential unconstitutionality, and potentially limited effect on corporate board workings. Nevertheless, the quota may be perceived as binding by companies because of public pressure and reputational concerns even if, in fact, it may not be binding based on legal grounds.

In this paper we not only document a robust and significantly negative stock market reaction of California firms to the introduction of the gender quota, but also find large negative spillover effects to non-California firms. We find that announcement returns of California firms are -2.6% on average for a two-day event window, while non-California firms experience a negative announcement return of -1.9%. This is remarkable given that the latter are not directly affected by the introduction of the gender quota in California. In economic terms, the mean reduction in shareholder value amounts to $104.51 million for non-California and $328.31 million for California firms, respectively (based on the mean market capitalization in the two groups).

Why is there such a large value loss for both, California and non-California firms? The predom-

[4]See Andrew Sheeler, *California man sues to overturn 'woman quota' in state gender equity law*, Sacramento Bee, Nov. 13, 2019.

inant explanation put forward in the literature is that there may be frictions in the director labor market that make it costly for firms to hire additional female directors (Ahern and Dittmar (2012); Greene, Intintoli, and Kahle (2019); Hwang, Shivdasasni, and Simintzi (2019)). For example, there may not be enough qualified female candidates and/or the pool of female directors may be costly to access if female directors are part of different job networks to which firms don't have regular access (Calvó-Armengol and Jackson (2004); Beaman, Keleher, and Magruder (2018)). For example, Ferreira, Ginglinger, Laguna, and Skalli (2017) show that French firms changed their director search technology in response to the introduction of the French board gender quota in 2011. This suggests that firms have to establish costly new recruiting procedures to identify suitable female candidates and eventually may have to pay higher wages to these scarce candidates.[5]

While our results suggest that California firms facing larger director labor market frictions react more strongly to the gender quota announcement, a large portion of the negative announcement returns for California firms remains unexplained. This casts doubt on the view that that supply-side constraints are the main driver of the negative announcement returns (Greene, Intintoli, and Kahle, 2019; Hwang, Shivdasasni, and Simintzi, 2019). We argue that, to understand the large and negative announcement returns to the gender quota, a broader view on investors' distaste for government regulation is needed. We propose that, independent of the nature the quota, shareholders may have a general distaste of governmental interference with company affairs and do not appreciate legislation that forces companies to change their organizational structure to achieve non-monetary goals. This general distaste for legislation of non-economic values, together with the expectation that more legislation of this kind may follow, can explain why we observe such large reactions to a quota with presumably limited scope and reach.

Shareholders' expectation of similar legislation to follow the adoption of the gender quota seems justified. In recent years, there have been several attempts to legislate non-economic values. At the federal level, for instance, Senator Elizabeth Warren proposed the Accountable Capitalism Act, a federal bill that would require that employees elect 40% of a board of directors of any corporation with over $1 billion in tax receipts. At the state level, California has frequently been the first state to enact legislation targeting non-economic values, which was often later adopted by other U.S.

[5] An alternative explanation would be that shareholders expect too much monitoring if female directors are added to the board and, as a result, a reduction of firm value (Adams and Ferreira (2007; 2009)).

states. An example is the adoption of a statewide $12 minimum hourly wage (with staged increases to $15 by January 1, 2022), which will more than double the federal minimum wage of $7.25. If shareholders interpret the gender quota as a signal that further (costly) legislation regarding equal opportunities (or other ESG-related aspects) may follow, stock prices will decline even if the gender quota itself does not last.

The gender quota is particularly well-suited to analyze the broader question of whether investors dislike governmental legislation of non-economic values. First, a significant amount of uncertainty related to the law's introduction needs to be resolved at a clearly defined announcement date. Second, the law must only affect a subset of companies within a country, so that there is a natural and homogenous control group to which treated firms can be compared to. Both these requirements are fulfilled by the California board gender quota, but not by the Norwegian quota for instance (e.g., Ferreira, 2015). Moreover, treatment status needs to be observable. While this clearly applies to a board gender quota, (pre-treatment) compliance of corporations, and thus the laws' effect on corporations, is not observable for most other non-economic laws. For instance, the environmental pollution of corporations is not reported in a standardized manner across corporations and thus not easily observable. Fourth, to be able to empirically distinguish between an anticipation of the adoption of the same law in other states versus an anticipation of non-economic laws more generally, the (anticipated) law needs to be relatively similar across states. This applies to the California board quota as board quotas discussed in other states are very similar to the California law and even board gender quotas across countries tend to have very similar requirements on gender representation in the board.

To empirically test whether a distaste of non-economic value legislation is driving the large and negative announcement returns, we check whether our results are stronger for firms that are more sensitive to policy uncertainty. There are several papers showing that the uncertainty associated with possible changes in government policy has negative implications for firm values (e.g., Pástor and Veronesi (2012; 2013); Brogaard and Detzel (2015); Kelly, Pástor, and Veronesi (2016); Koijen, Philipson, and Uhlig (2016); Brogaard, Dai, Ngo, and Zhang (2020)). If the introduction of a gender quota increases shareholders' uncertainty about the legislation of further non-economic values, firms with higher sensitivity to policy uncertainty should react more strongly to the adoption of the gender quota.

We use the Economic Policy Uncertainty (EPU) index of Baker, Bloom, and Davis (2016) to compute a firm-level measure of sensitivity to the firm's regulatory environment, which we then interact with the California headquarter dummy. While our sample firms in the high and low sensitivity groups do not differ with respect to female board representation, we find that California firms with higher sensitivity to economic policy uncertainty react more strongly to the introduction of the gender quota than firms with lower sensitivity to policy uncertainty. This result holds after we control for frictions in the director labor market.

In the next step, we test whether non-California firms in states that are likely to follow California's legislative lead react more strongly to the introduction of the gender quota in California than firms in states that are unlikely to follow California's legislation. California has frequently been the first state to enact non-economic value legislation, which was often later adopted by other U.S. states. Examples of California's leadership in legislating non-economic values are the introduction of a board gender quota, which is currently discussed in five other states, as well as the adoption of a statewide $12 minimum hourly wage. California is also well-known for adopting strict environmental legislation which strongly affected policy decisions in other states. A prominent example is California's co-foundation of the United States Climate Alliance, a bipartisan coalition of states and other U.S. territories that are committed to upholding the objectives of the 2015 Paris Agreement on climate change, formed after President Trump's announcement to withdraw from the agreement.

These examples illustrate that shareholders of firms in states that followed California's legislation in the past may also react to California's gender quota, because they expect similar legislation to follow in their state, too. In line with this view, we find that non-California firms in states that also have gender quota proposals in place show more negative announcement returns. In addition, firms located in states that followed California's legislation in the past, for instance, by announcing the introduction of stricter environmental laws, increasing minimum wages, or by legalizing cannabis, also show more negative announcement returns. Among these firms headquartered in states that have a high propensity to follow California's legislative lead, those that are more sensitive to economic policy uncertainty react strongest.

These results provide a novel explanation for the large and negative announcement effects that have been documented for the introduction of gender quotas (e.g., Ahern and Dittmar (2012); Matsa and Miller (2013); Greene, Intintoli, and Kahle (2019); Hwang, Shivdasasni, and Simintzi (2019)). Earlier studies, most of them analyzing the Norwegian quota (e.g., Ahern and Dittmar (2012); Matsa and Miller (2013)), assign the negative quota effect to frictions associated with the appointment of female directors to the board.[6] Two contemporaneous studies (Greene, Intintoli, and Kahle (2019); Hwang, Shivdasani, and Simintzi (2019)) also document negative announcement effects to the California board gender quota and attribute the main effect to the presence of frictions in the director labor market. Our paper questions the view that frictions in the director labor market are the main driver of the negative announcement returns. Rather, shareholders' distaste for the government's attempt to legislate non-economic values and their estimation of how likely a state will follow California's legislative lead explain why there is such a large negative reaction to the California gender quota, even for firms located in other states.

Our findings imply that non-economic laws with respect to the corporation have economic effects. Shareholders seem to assess legislation on gender equality as portending to future legislation which will affect (and reduce) the economic value of the firm, at least with respect to shareholder wealth. This is particularly relevant in times where the purpose of the corporation is under reconsideration in the U.S., with CEOs of major U.S. companies announcing in August 2019 that they are committed to lead their companies for the benefit of all stakeholders, and not just maximization of shareholder value.[7]

2 Data and sample selection

2.1 Main data sources

To construct our sample, we select all firms in Compustat with a data entry within one calendar year before the date of the introduction of the gender quota in California (i.e., at the end of

[6]Note that there is also contradicting evidence on the Norwegian gender quota. Most notably, Eckbo, Nygaard, and Thorburn (2021) criticize the empirical settings of Ahern and Dittmar (2012) and Matsa and Miller (2013). Using alternative specifications that address these concerns, they find no significant announcement effect associated with the introduction of the quota, which leads them to conclude that the pool of female director candidates was large enough to absorb the increase in demand associated with the adoption of the gender quota. When we use alternative specifications that address the concerns raised by Eckbo, Nygaard, and Thorburn (2021), we find the results on the California quota to remain qualitatively unchanged (see Section 2 and Tables OA.1 to OA.7 in the Online Appendix).

[7]The full statement is available at https://www.businessroundtable.org/business-roundtable-redefines-the-purpose-of-a-corporation-to-promote-an-economy-that-serves-all-americans. See also, for example, David Gelles and David Yaffe-Bellany, *Shareholder Value Is No Longer Everything, Top C.E.O.s Say*, The New York Times, Aug. 19, 2019.

September, 2018). We drop utility and financial firms, firms with missing information on the state in which they are headquartered, firms headquartered outside the U.S., firms with negative book value of equity, and firms with missing financial control variables (total assets, market-to-book ratio, and ROA). As the quota law only applies to firms headquartered in California with outstanding shares listed on a major United States stock exchange, we additionally drop firms that only list American Depository Receipts and firms without a listing on NYSE, AMEX, or NASDAQ. We supplement this sample with stock price data from Compustat. If more than one stock price series for a firm is reported, we choose the time-series with the highest market capitalization as of the event date among those time-series with sufficient data to estimate the market model. We also require data on the board of directors of our sample firms, which we obtain from BoardEx. Our final sample consists of 2,454 firms, out of which 458 (18.7%) are headquartered in California and 1,996 are headquartered in other states of the U.S. or the D.C. We describe all variables used in our empirical analyses in detail in Appendix A of the paper.

Governor Brown signed the law on Sunday, September 30$^{\text{th}}$, 2018, and the adoption of the law was publicly announced on the same day. We define the event date as the first trading day after the announcement: Monday, October 1$^{\text{st}}$. The choice of this event date is justified by the patterns reported in Figure 2, which displays the distribution of newspaper coverage on the gender quota in California. The weekly distribution of articles on the gender quota show that newspaper coverage is concentrated in the week following the signature of the bill by Governor Brown on Sunday, September 30. Reading a random subset of articles published before September 30 confirms that there was considerable uncertainty whether the Governor would sign the controversial bill.

It is important to note that the governor of California decided on 183 bills over the same weekend that he signed SB 826 into law. The revelation of both, the passage and veto of other bills unrelated to SB 826, could thus threaten the validity of our analysis. To address this concern, we compile a list of all 183 bills and analyze their weekly newspaper coverage. One single law received larger newspaper coverage than the gender quota law: California's net neutrality law (SB 822). The bill governs full and equal access to the internet and ensures that consumers are not charged extra for access to websites. Most telecommunication companies opposed the law, which was supported by consumer groups and Democratic politicians.[8] In robustness tests described in the Online Appendix, we test whether the net neutrality law affects our results. Our results show that SB 822 is not a concern for our analysis of the market reaction to SB 826 (see Section 2.1 and Table OA.1 in the Online Appendix).

There are three more laws that could potentially affect our results as they also target corporations. These are AB 3080, SB 1300, and SB 1343. AB 3080 was vetoed by the governor and would have prohibited non-disclosure agreements regarding sexual harassment, as well as ban arbitration agreements for any alleged violation of the Labor Code or the Fair Employment and Housing Act in the Government Code. SB 1300 passed and prohibits employers from forcing new employees or those seeking raises to sign non-disparagement agreements or waive their rights to file legal claims. SB 1343 also was signed into law and modifies the original AB 1825 mandate by requiring all California employees (both supervisory and non-supervisory) to receive sexual harassment training by January 2020. These three laws, however, received much less attention than SB 826, with only half the media coverage, or even less.[9]

To quantify the losses in shareholder value around the adoption of the quota, we compute daily abnormal returns (ARs) as the observed return less the predicted return from a market model regression estimated over a 250-day estimation window that ends on Friday, September 21$^{\text{st}}$, i.e., six trading days before the event date. As a proxy for the market return, we use the return of a value-weighted market index consisting of all sample firms.[10] For a firm to be included in our final sample, we require at least 125 daily return observations during the estimation window and complete return data during a symmetric five-day window around the event date. We compute a set of alternative cumulative abnormal returns (CARs) over different sub-periods within the five-day event window. Our base case measure of the market reaction to the introduction of the gender quota is based on a two-day event window, which includes the event day (October 1$^{\text{st}}$) and the following day (October 2$^{\text{nd}}$). All abnormal returns are winsorized at the 1$^{\text{st}}$ and 99$^{\text{th}}$ percentiles to

[8] See Cecilia Kang, *California lawmakers pass nation's toughest net neutrality law*, The New York Times, Aug. 31, 2018 and Melody Gutierrez, *California OKs net-neutrality rules: Trump administration promptly sues*, San Francisco Chronicle, Sep. 30, 2018.

[9] Greene, Intintoli, and Kahle (2019) conduct a series of robustness tests that suggest that it is highly unlikely that any of these laws drive the announcement returns to the California board gender quota.

[10] In a robustness test reported in Table OA.2 and discussed in Section 2.2 in the Online Appendix, we reestimate our main event study tests with an equally-weighted market index as a proxy for the market return and find very similar results.

mitigate the effect of outliers.

2.2 Descriptive statistics

Panel A of Table 1 reports descriptive statistics for the 458 firms subject to California's board gender quota. As of September 30$^{\text{th}}$, these firms have a mean (median) board size of 7.7 (8.0), out of which 14.6% (14.3%) or 1.2 (1.0) directors are female directors. The quota imposed on firms headquartered in California mandates that firms must have, by the end of calendar year 2021, three female directors if a board comprises six or more directors, two female directors if a board comprises five directors, and one female director if there are four or less directors on a board. Using data on board size and directors' gender, we find that, at the time of the adoption of the quota law, 88.9% of all California firms do not comply with the mandated quota that would apply to their current board size (2021 requ. failed (d)). To comply with the quota, California firms would have to appoint on average 1.7 (median: 2.0) female directors (# missing female directors). The variable Shortfall (%) scales the number of female directors required for compliance with the 2021 quota requirements by board size and shows that on average 23.5% (median: 25%) of directors on the board need to be replaced by female directors to achieve quota compliance. As the quota's introduction is staged, we use variations of the variable # missing female directors in some robustness tests. First, we consider the number of missing female directors for compliance with the 2019 quota requirements (# missing female directors (2019)), a dummy variable equal to one if a firm currently has no female director, and zero otherwise. Second, we use the number of missing female directors for compliance with the 2021 quota requirements, assuming that the 2019 requirements have been met (# missing female directors (2021)). All variables considered so far assume that quota compliance will be achieved by replacing male by female directors, leaving board size constant. An alternative way to comply with the quota would be to add female directors to the existing board, increasing board size. This increase in board size, however, can increase the number of female directors required for quota compliance for firms with less than six directors. Hence, our final variation of this measure is the number of female directors necessary to fulfill the quota requirements, assuming that female directors will be added to the board without any directors being replaced (# missing female directors (exp)).[11]

In some of our multivariate regressions below, we require a proxy variable for the importance of labor market frictions that would result from the introduction of a board gender quota in other states than California. Since non-California firms do not have a gender quota in place, we assume that they would be subject to the California quota when computing our proxies for frictions these firms would face. Indeed, discussions about gender quotas at the senates of other U.S. states show that the outlined proposals are very similar to SB 826 (see, for example, New Jersey's Bill S3469).

2.3 Construction of matched control sample

To obtain treatment and control groups that are similar in terms of observable and thus presumably unobservable firm characteristics, we choose, for each of our 458 treatment firms headquartered in California, three non-California-headquartered firms that share the same primary two-digit SIC code and are closest in terms of total assets. A firm in the control sample may be a matched control firm for more than one treatment firm, but is included only once in the control sample. The resulting matched sample comprises 1,238 firms, 458 (37.0%) in the treatment group and 780 in the control group. Descriptive statistics for the matched control firms are displayed in Panel B of Table 1. Tests for differences between the treatment and control samples in Panel C show that our matching procedure results in a control sample of non-California firms with board characteristics that are very similar to those of the treated sample of California firms. For example, the average (median) fraction of female directors at California firms and firms of the matched sample is nearly identical and amounts to 14.8% (14.3%). Furthermore, there are no significant differences in board size, the number of missing female directors, and the fraction of firms who do not fulfill the quota requirements yet. This is important as treatment and control firms should not systematically differ with respect to female board representation, and thus (anticipated) quota compliance prior to the treatment assignment for our analysis to be valid. Note also, that the control sample is not dominated by one state or a small number of states: The largest fraction of control firms is headquartered in Massachusetts (13.9%), followed by New York (10.8%) and Texas (10.5%). In

[11] Our results remain qualitatively unchanged if we use these alternative versions of the number of missing female directors variable when computing the Shortfall (%) variable, which is the number of missing female directors scaled by board size.

total, the control group includes firms that are headquartered in 43 different states and the D.C.[12]

3 The impact of the gender quota on stock returns

To analyze firms' stock price reaction to the introduction of the quota, we regress different abnormal return measures on a dummy variable equal to one if a firm's headquarters are located in the state of California, and zero otherwise. The event date is Monday, October 1st, 2018, the first trading day after the public announcement of the adoption of the quota by the Governor's office.

The identifying assumption central to a causal interpretation of such a difference-in-differences analysis is that treated and control firms share parallel trends before the onset of treatment, i.e., the introduction of the quota on September 30th 2018. In Table 2, we provide evidence in support of the parallel trends assumption: The results show that market-adjusted returns leading up to the quota adoption estimated over five different pre-event windows do not differ between the California and non-California firms.

Table 3 reports results from regressions of three different abnormal return measures, estimated over different event windows that range from two to five days in length, on the California-headquarter dummy variable and control variables. The set of control variables includes the natural logarithm of total assets as a proxy for firm size, return on assets as a proxy for firms' profitability, and the market-to-book ratio as a proxy for firms' valuation level. All these variables may serve as proxies for how competitive firms are in the director labor market and thus be related to the announcement returns associated with the quota's introduction. We control for these variables in all subsequent regressions. Results in column (1) show that cumulative abnormal returns for the first two event days after the announcement are 0.70% lower for California firms than for the matched non-California control firms. The coefficient on the California dummy variable is significant at the 5% level.[13] In comparison, Greene, Intintoli, and Kahle (2019) find a negative announcement return of -1.2% for California firms in a univariate setting. Similarly, Hwang, Shivdasasni, and Simintzi (2019) find a negative announcement return of -1.4% for California firms using the port-

[12] In a robustness test reported in Table OA.3 and discussed in Section 2.3 in the Online Appendix, we reestimate our main event study tests using the entire set of non-California-headquartered firms as control group and find very similar results.

[13] We obtain economically identical results if we run a classical difference-in-differences regression using daily abnormal returns as dependent variable and a post dummy, a California dummy, and an interaction term between the two. For the results, see Section 2.4 and Table OA.4 in the Online Appendix.

folio approach of Eckbo, Nygaard, and Thorburn (2021).[14] However, these papers do not look at non-California firms in their main tests. Note, however, that the constant in our regression is also negative and statistically significant. This means that non-California firms react to the California gender quota, too. Specifically, they experience a negative announcement return of -1.90% on average upon the adoption of the quota in California. Accounting for the relative underperformance of California versus non-California firms of -0.70%, the negative announcement return of California firms is thus -2.60% on average. Based on the mean market capitalization, the mean reduction in shareholder value amounts to $104.51 million for non-California and $328.31 million for California firms, respectively. Results remain statistically significant and coefficients on the California dummy are even larger when we extend the event window to a symmetric three-day event window (column (2)), or a symmetric five-day event window (column (3)). In our further analysis, we use the regression model in column (1) as our baseline specification.

The results in Table 3 suggest that California firms react more strongly to the adoption of the board gender quota in California than non-California firms. However, even though non-California firms are not directly affected by the quota, they also show negative and significant announcement returns. A straightforward explanation would be that some of the non-California firms in our control sample are affected by the quota law in California through direct trading relationships. To test whether direct trading relationships drive the spillover effects documented in Table 3, we proceed as follows. First, we exclude all firms from the control sample that disclose at least one of the treated California sample firms as principal customer. To identify principal customers, we follow Cohen and Frazzini (2008) and collect the names of all principal customers from the Compustat Segments database. Results are reported in Panel A of Table OA.5 in the Online Appendix and are virtually identical to those reported in Table 3. Second, we exclude matched control firms in geographic proximity to California as these firms should be more likely to directly interact with California firms. Specifically, we exclude all firms with headquarters in neighboring states (AZ, OR, and NV) from the control sample. Again, we find the results to remain virtually unchanged. Finally, we additionally exclude firms with headquarters in the five states that are neighbors to California's three direct neighbors (CO, ID, NM, UT, and WA). Results of this more restrictive

[14] Using the same approach as Eckbo, Nygaard, and Thorburn (2021) and Hwang, Shivdasasni, and Simintzi (2019), we find a negative announcement effect of -2.13% over a two-day event window.

test are reported in Panel B of Table OA.5 in the Online Appendix. Again, the results remain virtually unchanged when compared to those in Table 3. Hence, the results from these robustness tests suggest that direct trading relationships to California firms are unlikely to drive the negative spillover effects documented in Table 3.

Another potential concern with our analysis arises from the fact that we study the market reaction of firms to one single event. Specifically, all firms in the treatment group, i.e., the firms headquartered in California in our sample, are treated at the same date on October 1st, 2018. Such a single event date may result in contemporaneous cross-correlation of (abnormal) stock returns (e.g., Brown and Warner, 1985). To address this concern, we apply the portfolio sorts approach proposed by Eckbo, Nygaard, and Thorburn (2021). Details of this analysis are provided in Section 2.6 of the Online Appendix. Results are reported in Table OA.6 and are very similar to those in Table 3.

As Eckbo, Nygaard, and Thorburn (2021) point out, it is important to include all major quota-related news events that increase the likelihood of a quota law in an analysis of changes in firm value. Therefore, in Table OA.7, we conduct tests using other potential event dates, such as the State Senate and Assembly votes (see Section 2.7 of the Online Appendix).[15] All coefficients on the California-headquarter indicator variable are insignificant for alternative event dates.

Finally, in Figure 3, we compare the economic magnitude of our results to other, potentially value-relevant events. To this end, we run the same 2-day event window test for each of the 250 trading days in the estimation window and compare the magnitude of estimated two-day CARs to those observed in response to the quota's adoption announcement. We find that only 13 (5.2%) of the slope estimates in the estimation window are smaller than -0.70%, and only six (2.4%) of intercept estimates in the estimation window are smaller than -1.90%, suggesting that announcement returns in response to the quota introduction are economically meaningful and constitute rare events.

[15] SB 826 first passed the Senate on May 31st, 2018, (22:6:11 votes) and the Assembly on August 29th, 2018 (41:13:26 votes). The Senate passed the amendments made by the Assembly on August 30th, 2018 (23:8:9 votes).

4 Why do shareholders react so strongly to the adoption of the gender quota?

Even though it is still unclear whether the California gender quota is constitutional or will be withdrawn, we find large and significantly negative announcement effects in response to its introduction for California as well as non-California firms. This suggests that the quota is perceived as binding by investors due to, for example, reputational concerns and public pressure. While frictions in the director labor market may explain the negative reaction of affected firms to the introduction of a gender quota (see Ahern and Dittmar (2012), Greene, Intintoli, and Kahle (2019), and Hwang, Shivdasasni, and Simintzi (2019))[16], the negative reaction of firms in other states of the U.S. is particularly surprising as they do not face any material changes to their organizational structure due to the California gender quota.

Indeed, we find that the negative reaction of California firms is at least partly explained by frictions in the director labor market. First, we show in Figure 4 that the demand for female directors in California increases substantially after the introduction of the gender quota. Specifically, we compute the change in mean female board representation relative to the adoption of the quota on September 30$^{\text{th}}$, 2018, in percentage points on a daily basis from July 1$^{\text{st}}$, 2018, to March 31$^{\text{st}}$, 2019, using data from BoardEx. The solid lines show changes in female board representation for the treatment sample (in black) and the industry- and size-matched control sample (in grey). We find that California firms increase female board representation more strongly than matched firms in the control sample starting two weeks after the adoption of the quota law until the end of our sample period in March 2019.[17] This effect is stronger for firms in need of more female directors to fulfill the quota.

Second, we augment our baseline regressions in Table 3 by alternative firm-specific measures related to quota compliance and interaction terms between these measures and the California-

[16] Ahern and Dittmar (2012) provide evidence in support of a friction-based explanation of the value loss associated with the introduction of the board gender quota in Norway. Greene, Intintoli, and Kahle (2019) and Hwang, Shivdasasni, and Simintzi (2019) conduct similar tests as Ahern and Dittmar (2012) and find evidence in support of a friction-based explanation of the negative announcement effect associated with the introduction of the board gender quota in California.

[17] In Table OA.8, we provide results of a difference-in-differences regression with results discussed in Section 3 of the Online Appendix. These tests mirror what is shown in Figure 4 and indicate that the differences in the increase in female board representation between the treated California firms and the non-California control firms turn significant already two months after the introduction of the gender quota.

headquarter dummy variable. The eight alternative proxy variables for the female director gap we use are explained in Section 2.2 and include the variables used by Greene, Intintoli, and Kahle (2019) and Hwang, Shivdasasni, and Simintzi (2019). Results are reported in Table OA.9 and explained in detail in the Online Appendix.[18] In short, our analysis supports findings by Ahern and Dittmar (2012), Greene, Intintoli, and Kahle (2019), and Hwang, Shivdasasni, and Simintzi (2019), suggesting that frictions in the director labor market partly explain the negative reaction of California firms to the introduction of a gender quota. However, across all these regressions we continue to observe a significantly negative announcement effect for both California and non-California firms that remains unexplained by the friction-based measures. In fact, the regression constant ranges from -1.69% to -2.57% and is statistically significant at the 1% level across all specifications. The California-headquarter dummy is mostly negative, but often insignificant once labor market frictions are accounted for. For example, when looking at our baseline measure of frictions, Shortfall (%), we find a negative and significant announcement return of -2.33% for both California- and non-California-headquartered firms, which becomes more negative for California firms, the larger the shortfall in female directors for quota compliance. The magnitude of this remaining effect for California firms and non-California firms is remarkable given that the quota may be withdrawn and non-California firms are not directly affected in the first place.

Therefore, we conjecture that investors' expectations regarding government interventions must have changed fundamentally in response to the introduction of the gender quota and that these expectations lead to the negative announcement returns we observe. In line with this view, previous literature shows that uncertainty associated with possible changes in government policy has negative implications for firm values (e.g., Pástor and Veronesi (2012; 2013); Brogaard and Detzel (2015); Kelly, Pástor, and Veronesi (2016); Koijen, Philipson, and Uhlig (2016); Brogaard, Dai, Ngo, and Zhang (2020)). We hypothesize that the adoption of the gender quota signals California's general willingness to legislate non-economic values. Shareholders' disapproval of the governments' attempt

[18]The eight measures of frictions in the director labor market reported in Table OA.9 are all demand-based. An alternative approach to measure such frictions is to look at the supply side, i.e., the pool of potential female director candidates to be appointed to the boards of firms facing a quota requirement. We test a number of supply-based measures of frictions in the director labor market, including the number of female directors that currently serve on boards of other firms with headquarters in close proximity or dummy variables equal to one for firms in close proximity to either the state boarder or a major airport as both may allow firms to tap potential female director candidates from neighboring states or from across the entire country. However, we find these measures to be dominated by demand-based measures. These analyses are explained in the Online Appendix with results reported in Table OA.10.

to legislate non-economic values may then result in the value losses we observe. Since California has a reputation for being a pioneer in introducing new legislation, shareholders of firms located in other states may expect similar legislation in their states. As stated by SEC commissioner Hester Peirce in her speech at the 2018 Annual SEC conference: "Nothing that happens in California stays in California." (Peirce (2018)).[19]

To test this conjecture, we first examine whether firms that are more vulnerable to regulatory interventions react more strongly to the quota's adoption. To identify these firms, we use the Economic Policy Uncertainty (EPU) index of Baker, Bloom, and Davis (2016) and follow a procedure similar to that in Koijen, Philipson, and Uhlig (2016) and Akey and Lewellen (2017). Specifically, we estimate the same market model regression used in the calculation of the daily ARs and add the daily change of the EPU index as a second explanatory variable. The coefficient estimate on the daily change in the EPU index provides a measure of the firm's sensitivity to changes in the regulatory environment that is not reflected in the market return. Following Akey and Lewellen (2017), we classify a firm as policy-sensitive if the coefficient on the change in the daily EPU index is significant (i.e., has a p-value below 0.1). According to this definition, in our matched sample comprising 1,238 firms, 138 firms (11.1%) are policy-sensitive, out of which 52 (37.7%) firms are headquartered in California. A potential concern with using the EPU index is that it may correlate with our measures for the female director gap. Hence, in Table OA.11 in the Online Appendix, we explore the determinants of being a policy-sensitive firm. We find that none of the eight proxies for the female director gap that we use is correlated with the variable indicating policy sensitivity, muting the concern that the effect of a firm's policy sensitivity captures the effect from frictions in the director labor market.

Figure 5 shows daily changes of the EPU index during the 250-day estimation window that precedes the adoption of the board gender quota in California. The red vertical line indicates the day on which the gender quota in California was announced. We observe a large positive change in the EPU index on this day, which is in line with the view that investors' uncertainty regarding economic policies across the entire U.S increased fundamentally after the California gender quota

[19] See, for example, Tood S. Purdum, *The Nation: Golden Rules; As California Goes, so Goes the Country?*, The New York Times, Sept. 21, 2003, Kate Conger and Noam Scheiber, *California's Contractor Law Stirs Confusion Beyond the Gig Economy*, The New York Times, Sept. 11, 2019, and Christine Mai-Duc and Lauren Weber, *It Isn't Just Uber: California Prepares for New Gig Worker Rules... and Confusion*, The Wall Street Journal, Dec. 17, 2019.

was adopted. Only 11 days (4.4% of the observations) are associated with larger positive changes of the EPU index, indicating that the gender quota had a meaningful impact on investors' economic policy uncertainty.

In the next step, we run regressions including the same set of control variables as in Table 3, but augmented with the policy sensitivity measure and its interaction with the California-headquarter dummy variable. Results are reported in column (1) of Table 4. They show a negative and significant regression constant of -1.88%, an insignificant coefficient of -0.48% on the California-headquarter dummy, and a negative and significant coefficient of -1.92% on the interaction term between the California-headquarter and policy-sensitivity dummies. Hence, after controlling for the policy sensitivity of a firm, announcement returns of California firms are not statistically different from those of non-California firms. Moreover, policy-sensitive California firms underperform other California firms by an economically meaningful -1.92%. Notably, all sample firms, independent of whether they are located in California or elsewhere, experience highly significant negative returns of -1.88%. Hence, our results support our hypothesis that shareholders' expectations regarding future legislation of non-economic values contributes to the negative announcement returns observed around the quota adoption.

To make sure that frictions in the director labor market do not cofound our results, we re-run the regression in column (1) of Table 4 and add proxy variables for the frictions a given firm would face from the introduction of the gender quota and interaction terms between the friction proxy variables and the California-headquarter dummy variable. We again use the eight alternative proxy variables for the female director gap explained in Section 2.2 and used in our analysis on the importance of frictions explained above (detailed in the Online Appendix with results reported in Table OA.9). Note that these variables include those used by Greene, Intintoli, and Kahle (2019) and Hwang, Shivdasasni, and Simintzi (2019). Results from a regression using our main proxy variable for frictions faced by firms upon introduction of the quota, Shortfall (%), are reported in column (2) of Table 4. Results from seven additional proxy variables that are meant to capture frictions in the director labor market are reported in columns (3) to (9).

Across all eight specifications, the coefficient on the interaction term between the California-headquarter dummy and the dummy variable indicating policy-sensitive firms remains negative and significant. Thus, in addition to the negative reaction due to frictions, as indicated by the significant coefficient on the interaction term between the California-headquarter dummy and the eight alternative friction proxies, California firms that are more sensitive to regulatory policies react more strongly to the gender quota announcement.

Again, we do not find evidence that frictions in the director labor market explain the negative reaction of non-California firms, as indicated by the insignificant coefficient on the friction variable itself. Therefore, we now turn to a more detailed analysis of what is driving the negative announcement returns of non-California firms.

5 Spillover effects to non-California firms

If the introduction of a mandatory gender quota in California raises concerns that other states follow suit, firms headquartered in states that are more likely to follow California and pass such laws should react more negatively to the introduction of a gender quota in California. The negative reaction could be driven by concerns that a given state would follow up on California's lead and introduce a gender quota which may be costly for firms to fulfill. It could also be driven by more general concerns that non-economic values may be legislated in a given state and lead to additional costs for firms.

In the following, we restrict the sample to all matched non-California firms and examine whether announcement returns are more negative in states that are more likely to follow California's legislation (Section 5.1). Then, we try to disentangle whether spillover effects to non-California firms are driven by expectations regarding gender quotas specifically, or by more general expectations that non-economic values may be turned into legislation (Section 5.2).

5.1 As California goes, so goes the nation?

California has often been a leader in introducing new regulation, which is subsequently adopted by other states. We hypothesize that states that followed California in the past are also likely to follow California in the future by, e.g., adopting either a gender quota or other regulation imposing non-economic values on firms. Hence, our first set of proxies are meant to capture the extent to which states have followed California's legislation in the past. Based on the presumption that states with an already high regulatory density are more likely to follow California's legislative lead, our second set of proxy variables consists of two alternative regulatory indices. Our final proxy for a state's likelihood to impose legislation of non-economic values on firms is a state's political orientation. Given the opposing views of the Democratic and Republican parties on regulatory issues more generally, we expect democratic states to be more likely to adopt potentially value-impairing regulation.

Legislators in several states proposed board gender quotas with requirements very similar to the quota law introduced in California . These states are Illinois, Massachusetts, New Jersey, New York, and Washington.[20] As our first proxy for the likelihood to follow California's legislation, we define a dummy variable, Impending quota (d), equal to one for firms headquartered in one of these states. This variable serves as our first proxy for the propensity that a state follows California in either introducing a gender quota or other value-impairing regulation. We then run regressions of two-day cumulative abnormal returns on the impending quota dummy and the same set of control variables as used in Tables 3 and 4. Results are reported in column (1) of Table 5. As expected, the coefficient on Impending quota (d) is negative and significant, suggesting that firms headquartered in states that are considering the introduction of a board gender quota react more negatively to the passing of Bill 826 in California than firms headquartered in other states.

California is well-known to be a leader in environmental laws and policies within the U.S. One prominent example of California's leadership in setting high environmental standards is the co-foundation of the United States Climate Alliance (jointly with New York and Washington), a bipartisan coalition of states and other U.S. territories that are committed to upholding the objectives of the 2015 Paris Agreement on climate change within their borders and meeting or exceeding the targets of the federal Clean Power Plan. The Alliance was formed on June 1st, 2017, by the governors of the three founding states following President Donald Trump's announcement that he had decided to withdraw the U.S. from the Paris Agreement. Until September 30th, 2018, thirteen other states had joined the Alliance. Our second proxy for a state's propensity to follow California's legislative lead is a dummy variable that is equal to one for firms headquartered in the other 15 states that had joined the United States Climate Alliance by the time of the adoption of

[20] See, for example, Anastasia Boden, *Setting quotas on women in the boardroom is probably unconstitutional. It also doesn't work*, Los Angeles Times, Jul. 8, 2019, and Laura Weiss, *California board diversity mandate spreads to other states, Washington*, Roll Call, Jul. 19, 2019.

the gender quota law in California, and zero otherwise (the states are listed in Appendix A). As a second proxy for how similar other states are to California in terms of implementing progressive environmental laws, we define a dummy variable that is equal to one for firms headquartered in states that have implemented comprehensive Greenhouse Gas (GhG) reduction policies and zero otherwise. As comprehensive GhG reduction policies, we consider statutory GhG reduction requirements, statutory GhG reporting requirements, market-based policies, or a participation in the Transportation & Climate Initiative (TCI). Besides California, eighteen states have such policies in place (for details see Appendix A). Results from regressions using these two environmental policy variables as proxies for how likely it is that a given state follows California in introducing non-economic laws are reported in columns (2) and (3) of Table 5. The coefficients on both these proxy variables are negative and significant, suggesting that firms with headquarters outside California but in states with a similar attitude towards progressive environmental laws experience more negative announcement returns around the adoption of the gender quota in California.

In the next step, we look at states that followed California in introducing minimum wage laws. Such state laws aim at raising the minimum wage above the currently applicable federal rate of \$7.25 per hour. California has been a leader in the expansion of minimum wage laws. On April 4^{th}, 2015, California's Governor Jerry Brown signed Senate Bill 3 into law, which prescribes a minimum wage of \$12 per hour, including staged increases to \$15 per hour by January 1, 2022. In the wake of the California minimum wage law, several states have moved to raise their state minimum wage above the federal minimum as well. Hence, a minimum wage at the state level that exceeds the federal minimum wage of \$7.25 per hour may be not only an indicator of a higher probability of following California in adopting progressive laws, but also a signal that this state is willing to impose costs on companies to fulfill non-economic goals. We define a state's propensity to follow California's legislative lead as the difference in dollars between the state-level minimum wage per hour and the federal minimum wage per hour. For an overview of the minimum wage policies of all U.S. states, see Panel A of Figure OA.1 in the Online Appendix.[21] Results in column (4) of Table 5 show that firms headquartered in states with a higher gap between the state-level and federal minimum wages react significantly more negatively to the introduction of the quota in California.

[21]The number of observations is reduced because we disregard firms headquartered in states that do not define a minimum wage (Alabama, Louisiana, Mississippi, New Hampshire, South Carolina, and Tennessee). In alternative tests, we assume the federal minimum wage of \$7.25 for these states and find slightly stronger results.

We also examine whether returns are stronger for firms headquartered in states that have followed California's cannabis legalization policies. In 1996, California was the first state to legalize the use of cannabis for medical purposes when voters approved Proposition 215 – the Compassionate Use Act of 1996. At the time of the adoption of the gender quota, the use of cannabis for medicinal purposes was legal in 32 states and the District of Columbia. In 2016, California voters approved the Adult Use of Marijuana Act through Proposition 64, which legalized cannabis for recreational use. Ten jurisdictions followed. For each state, we compute a Cannabis legalization score, which is a count variable equal to zero if any type of cannabis use is considered illegal. It is equal to one if cannabis is legal for medical purpose, but laws restrict the allowable concentration of tetrahydrocannabinol (THC), the main psychoactive component of cannabis, equal to two if the use of cannabis is legal for medical purposes without restrictions on THC-levels, equal to three if the recreational consumption of cannabis is illegal, but has been decriminalized, and equal to four if recreational consumption of cannabis is legal. Hence, the index captures the extent to which state law contradicts federal law, under which cannabis is as a Schedule 1 drug, which prohibits all use. For an overview of the cannabis policy of all U.S. states, see Panel B of Figure OA.1 in the Online Appendix. Results are reported in column (5) of Table 5. The coefficient on the cannabis-legalization score is negative and statistically significant at the 5% level.

Our next two proxies for a state's likelihood to follow California's legislation are regulatory indices provided by two libertarian think tanks, the John Locke Foundation and the Cato Institute. We would expect that states with an already high regulatory density are more likely to follow California's legislative lead and introduce further (value-reducing) regulation in the future. The John Locke Foundation uses 27 provision in six different categories (Land Use, Labor Market, Utilities, Occupations, Tort, and Insurance Regulations) to compute a "regulatory freedom ranking" for each U.S. state. The last available issue of this ranking is from 2015. Our second measure of states' regulatory environments is developed by the Cato Institute. Their index was issued in 2018 and is based on 2016 data. Their regulatory index encompasses 50 provisions in seven categories (Labor Regulation, Health Insurance, Occupational Licensing, Eminent Domain, Liability System, Land and Environment Regulation, and Utility Deregulation). We use the John Locke Foundation ranking and the Cato index to compute two regulatory score variables. To this end, we group all 50 states into quintiles according to their regulatory ranking and index values, respectively, and assign a score of one to firms headquartered in states with the least restrictive regulation and five for firms in states with the strictest regulation. An overview of state-level regulatory scores is provided in Panels C and D of Figure OA.1 in the Online Appendix. In columns (6) and (7) of Table 5, we show that firms with headquarters outside California but in states with a higher regulatory density experience more negative announcement returns around the adoption of the gender quota in California, but only the coefficient on the second regulatory score is significant.

5.2 Non-California firms: Non-economic values in general vs. gender quotas

Next, we decompose the negative announcement returns to non-California firms into the part that is due to concerns of impending legislation of non-economic values and the part resulting from expectations regarding costly labor market frictions associated with the introduction of a board gender quota.[22] We estimate regressions including the same set of control variables as in Table 5. The dependent variable is two-day CARs at quota adoption and the main independent variables are our measures of a state's propensity to follow California's legislative lead from Table 5.

To discriminate between the return effect that results from impending legislation of non-economic values and the return effect that results from anticipated frictions due to an anticipated gender quota, we additionally include the dummy for policy-sensitive firms, the number of missing female directors scaled by board size (Shortfall (%)), and interaction terms between these two variables and the probability that a firm's headquarter state follows California. These interactions allow for a differential impact of the quota announcement on policy-sensitive firms and on firms that face larger labor market frictions if a similar gender quota was introduced in a given state. A negative and significant loading on the interaction term between the proxy variable for the probability to follow California and the dummy for policy-sensitive firms would indicate that a significant part of the negative spillover effect results from concerns of impending value-reducing regulation in states that are more likely to follow California's lead. In contrast, a negative and significant interaction

[22]Note that in our spillover setting, there is an alternative channel through which frictions, such as search costs and a deterioration of board quality imposed by the gender quota, may affect our results: California firms may attempt to attract female directors from the boards of non-California companies to be able to comply with the quota. To control for a potential effect resulting from expectations of female directors being hired away from the board of control sample firms by California firms, we alternatively augment the regressions by fixed effects for the number of female directors on the board (ranging from zero to six in our sample). Including these fixed effects leaves our results unchanged.

term between the proxy variable for the probability to follow California and Shortfall (%) suggests that a significant part of the spillover effects is due to concerns of frictions in the director labor market that would result from these states also adopting a board gender quota.

Results are reported in Table 6. Consistent with our expectations, we find seven of the eight interaction terms between the proxy variable for the probability to follow California in legislative issues and the dummy for policy-sensitive firms to be negative and significant at the 10% level or higher. In contrast, the coefficient on Shortfall (%) and its interaction with the proxies for the propensity to add value-reducing regulations are insignificant across all eight columns, suggesting that the negative spillover effects observed at non-California firms are likely to be due to more general expectations regarding future legislation of non-economic values, rather than a more specific expectation that a gender quota may be introduced.[23]

Overall, our results suggest that the negative spillover effects to non-California firms are more likely to be the result of investors' general expectations regarding future legislation of non-economic values than being the result of an anticipated gender quota.

6 Discussion and conclusion

Several studies show that stock prices of companies becoming subject to gender quotas suffer significant losses when women are added to the board. For example, Ahern and Dittmar (2012) show significantly negative announcement returns of Norwegian firms to the introduction of a gender quota, arguing that frictions in the director labor market caused the significant drop in the stock prices. This reasoning has recently been questioned by Eckbo, Nygaard and Thorburn (2021), who argue that the pool of qualified female directors was large enough to avoid significant shareholder-borne costs.

In this paper, we document that the introduction of a gender quota in California led to similar stock market reactions as in other countries: We find robust and significantly negative announcement returns of California firms, which are partly explained by frictions in the director labor market. Interestingly, however, we also find that stock prices of non-California firms react negatively to the

[23]To test the robustness of these results with respect to the choice of the friction measure, we reestimate Table 6 replacing our baseline measure, Shortfall (%), by the other seven proxy variables for frictions on the director labor market. Results are reported in Table OA.12, Panels A to G and are very similar to those reported in Table 6.

California gender quota, even though these firms are not subject to an immediate quota.

We contribute to the debate on stock price reactions to adoptions of gender quotas by proposing a new channel through which gender quotas affect firm value negatively: We argue that shareholders take the introduction of a gender quota as a broader signal that future legislation of non-economic values will follow and find evidence consistent with this view, i.e., policy-sensitive firms in California and in states that are likely to follow California's legislative lead react strongest to the quota announcement. Our results show that even if the direct costs associated with the quota itself are negligible or non-existent, large negative stock price reactions can occur. In other words, the substantial negative stock market reactions to the quota's adoption that we document might have little to do with the fact that companies are forced to add women to their boards but might be attributable to the simple fact that shareholders do not appreciate legislation that forces companies to change their organizational structure to achieve non-monetary goals. Thus, even a cost-neutral policy may have adverse effects on stock prices if expected costs arise from a perceived shift towards stakeholder value maximization. In times of climate change and a stronger ESG orientation of firms, these expectations may be purely rational and not necessarily driven by gender bias

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Figure 1: Female board representation in listed U.S. firms

This figure shows female board representation in all listed U.S. firms included in the Russell 3000 index from 2008 to 2018 as well as female board representation in subsets of large firms included in the Russell 1000 index and small firms included in the Russell 2000 index. Index membership is determined as of the annual index constitution date using data from Russell's website. Board data from BoardEx as of the annual index constitution date is used to estimate female board representation.

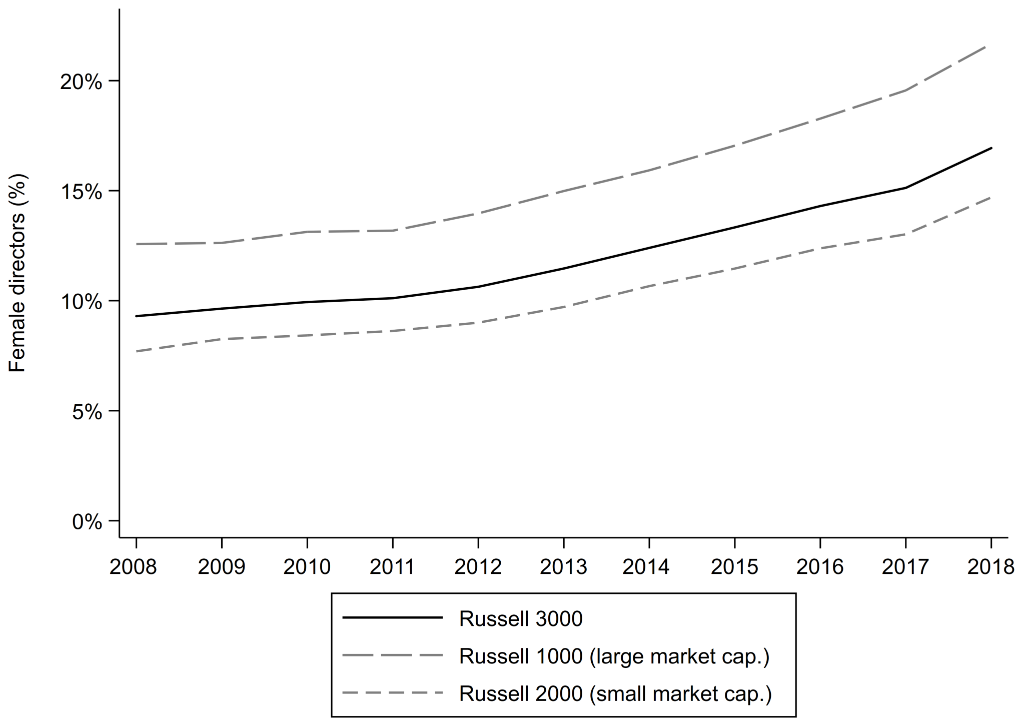


Figure 2: Distribution of newspaper coverage over time

This figure displays the distribution of newspaper coverage if we run article searches using Factiva for two different search terms, "California female board quota" (black line) and "California Senate Bill 826" (gray line), allowing for variations, e.g., "SB 826". The figure displays the weekly fraction of articles that contain these search terms during the time period from December 1, 2017, to November 30, 2018.

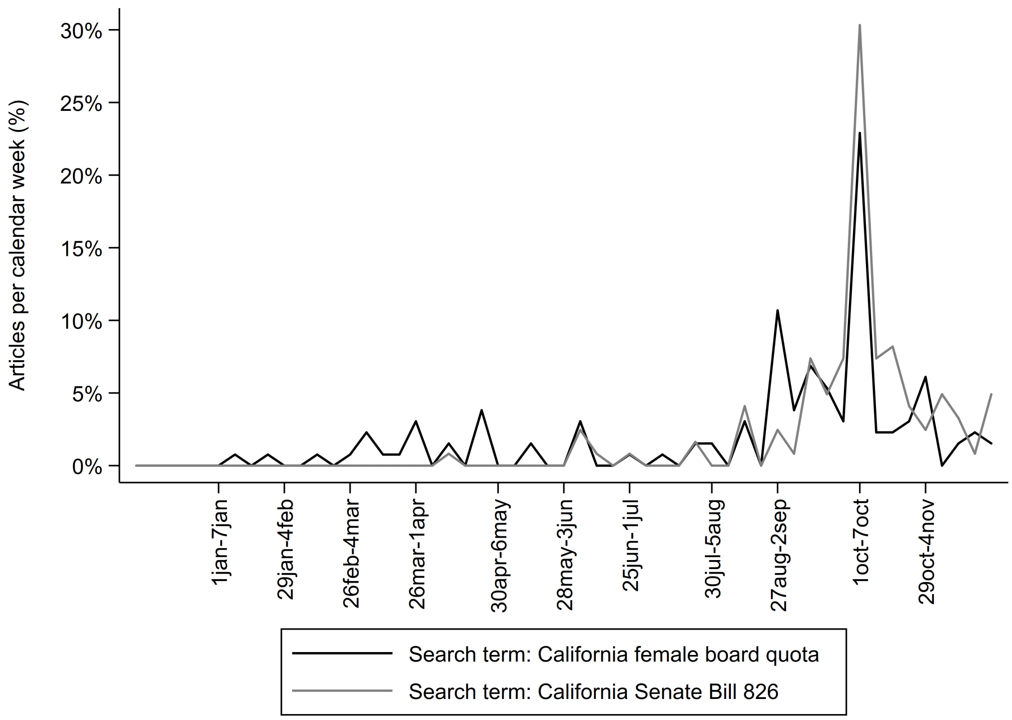
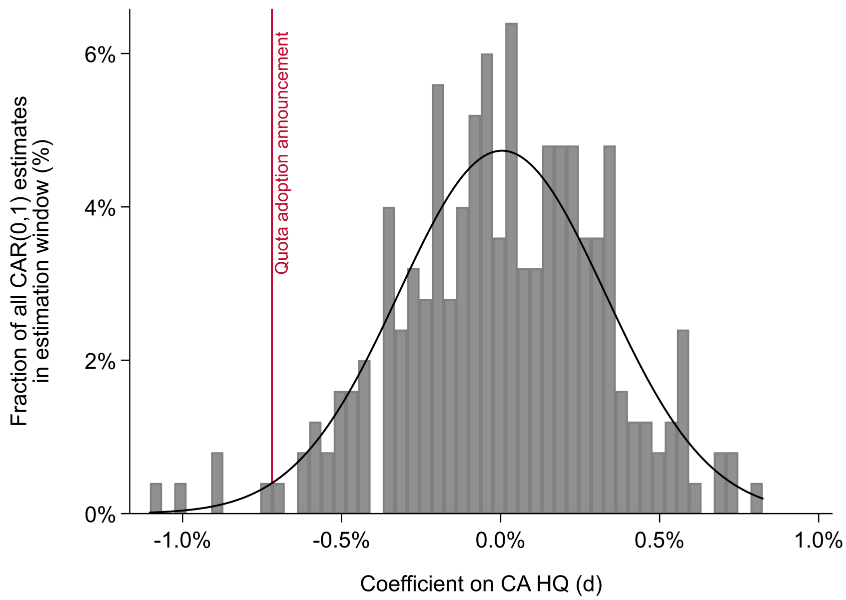


Figure 3: Frequency distribution of event study coefficient estimates and placebo event study coefficients

This figure displays the distribution of the coefficients (in gray bars) for the California-headquarter dummy (Panel A) and for the intercept (Panel B) resulting from a rolling estimation of our baseline event study, as reported in column (1) of Table 3, using each of the 250 days in the estimation window as placebo events well as the effective event date (October 1, 2018) as event day. In Panel A, six of the 250 placebo event studies (2.4%) have a lower coefficient estimate than the true event study using the first two trading days after the adoption of the board gender quota in California. In Panel B, 13 of the 250 placebo event studies (5.2%) generate a lower intercept than the adoption of the board gender quota in California.

Panel A: Frequency distribution of coefficients on California-headquarter dummy



Panel B: Frequency distribution of intercepts

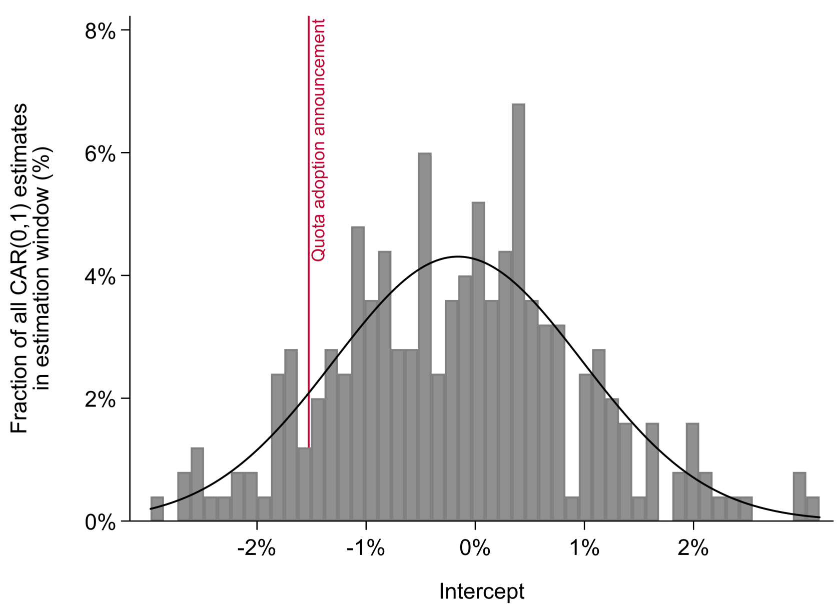


Figure 4: Changes in female board representation around quota adoption

This figure shows daily changes in female board representation for the time period July 1, 2018, to March 31, 2019, relative to the adoption of the quota on September 30, 2018. Solid lines show percentage point changes for a sample of California-headquartered firms (in black) and a sample of industry- and size-matched non-California-headquartered control firms (in grey). Dashed lines show changes in percentage points for subsamples of California-headquartered firms (in black) and non-California-headquartered firms (in grey) firms that miss more than one female director to fulfill California's board quota at the quota's adoption date. The red vertical line indicates the date the law was signed by the Governor (September 30, 2018). The sample comprises all firms in Compustat with a data entry within one calendar year before September-end 2018, excluding utility and financial firms (SIC codes 4940-4949 and 6000-6999, respectively), firms with missing information on the state in which it is headquartered, firms headquartered outside the U.S., firms with negative book value of equity, firms with missing financial control variables (total assets, market-to-book ratio, ROA), firms that only list American Depository Receipts, and firms without a listing on NYSE, AMEX, or NASDAQ. We also require at least 125 daily return observations during the 250-day estimation window that ends September 21, complete return data for the entire five-day event window around the event date (October 1), and availability of board data from BoardEx. For each firm headquartered in California, we draw the three closest size-matched non-California-headquartered firms in the same two-digit SIC code industry as control firms. A firm in the control sample may be a matched control firm for more than one treatment firm, but is included only once in the control sample. Board data is obtained from BoardEx.

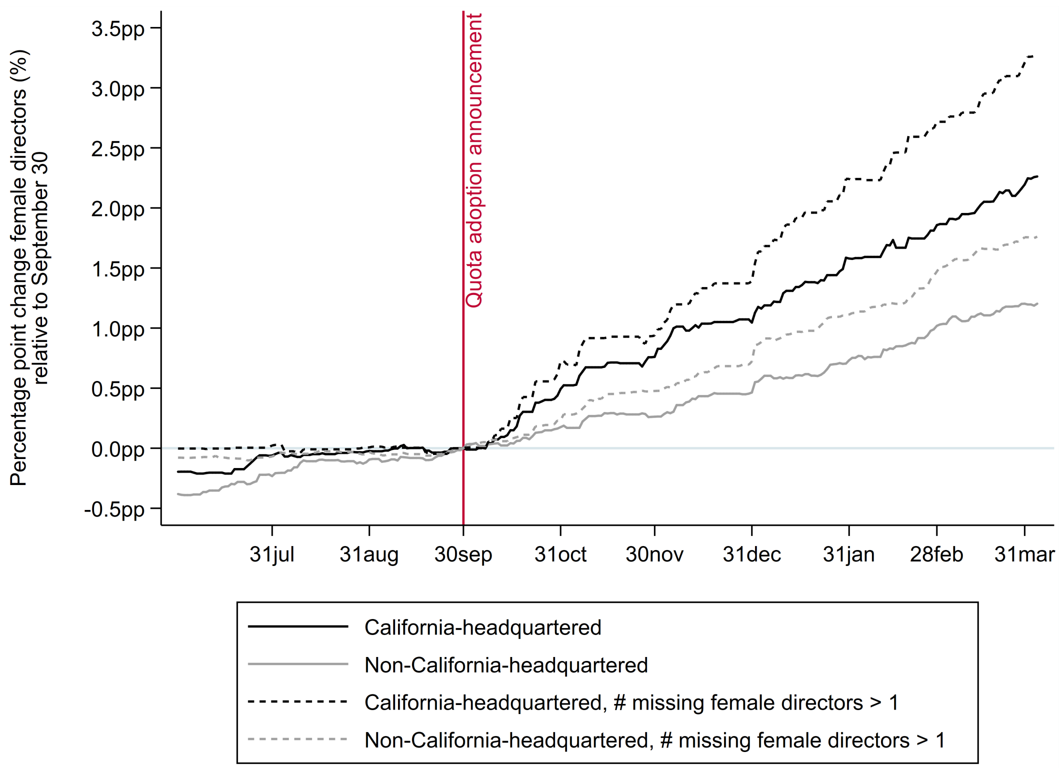


Figure 5: Frequency distribution of daily changes in the economic policy uncertainty index

This figure shows the frequency distribution of daily changes in the economic policy uncertainty (EPU) from Baker, Bloom, and Davis (2016) during the 250-day estimation window that ends six trading days before the event date (September 21, 2018). During the estimation window, eleven of the 250 observations (4.4%) have a higher change in EPU than the adoption of the board gender quota in California (October 1, 2018).

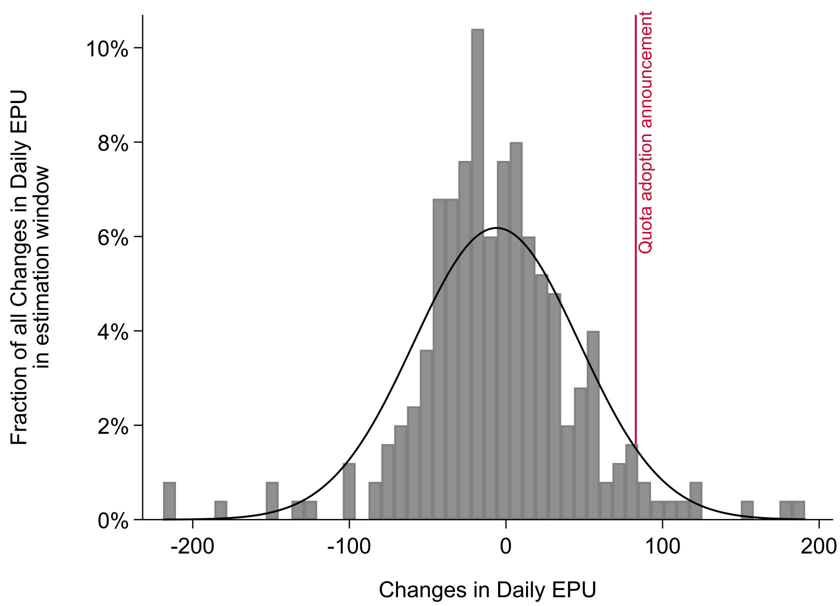


Table 1: Descriptive statistics

Panel A reports descriptive statistics for the sample of firms subject to California's board quota. A firm enters this sample if it is headquartered in California and has a reporting date within one calendar year before the quota's adoption announcement (September 30, 2018) in Compustat. We exclude utility and financial firms (SIC codes 4940-4949 and 6000-6999, respectively), firms with negative book value of equity, firms with missing financial control variables (total assets, market-to-book ratio, ROA), firms that only list American Depository Receipts, and firms without a listing on NYSE, AMEX, or NASDAQ. We also require at least 125 daily return observations during the 250-day estimation window that ends September 21, complete return data for the entire five-day event window around the event date (October 1), and availability of director data from BoardEx. Panel B reports descriptive statistics for the matched control sample. To construct this control sample, we draw, for each firm headquartered in California, the three firms closest in size that are active in the same two-digit SIC code industry, are headquartered in another U.S. state or the D.C., and pass the same sample selection criteria as outlined above. A firm in the control sample may be a matched control firm for more than one treatment firm, but is included only once in the control sample. Panel C reports results of tests for differences in means and medians between the sample of California-headquartered firms (Panel A) and the sample of matched non-California control firms (Panel B). Detailed variable definitions are in Appendix A.

Panel A: California-headquartered firms

Firm characteristic	Mean	P25	Median	P75	SD	N
Board size	7.664	6.000	8.000	9.000	1.891	458
Shortfall (%)	0.235	0.125	0.250	0.375	0.155	458
# missing female directors	1.683	1.000	2.000	2.000	0.927	458
# missing female directors (exp)	1.817	1.000	2.000	3.000	0.982	458
# missing female directors (2019)	0.297	0.000	0.000	1.000	0.457	458
# missing female directors (2021)	1.386	1.000	2.000	2.000	0.720	458
2021 requ. failed (d)	0.889	1.000	1.000	1.000	0.315	458
Female directors (%)	0.146	0.000	0.143	0.222	0.124	458
# female directors	1.199	0.000	1.000	2.000	1.061	458

Panel B: Industry- and size-matched control firms

Firm characteristic	Mean	P25	Median	P75	SD	N
Board size	7.801	6.000	8.000	9.000	2.031	780
Shortfall (%)	0.227	0.111	0.250	0.333	0.156	780
# missing female directors	1.632	1.000	2.000	2.000	0.946	780
# missing female directors (exp)	1.779	1.000	2.000	3.000	1.007	780
# missing female directors (2019)	0.290	0.000	0.000	1.000	0.454	780
# missing female directors (2021)	1.342	1.000	2.000	2.000	0.751	780
2021 requ. failed (d)	0.864	1.000	1.000	1.000	0.343	780
Female directors (%)	0.148	0.000	0.143	0.222	0.124	780
# female directors	1.249	0.000	1.000	2.000	1.125	780

Panel C: Tests for differences

	Differences (CA HQ (d) = 1 – CA HQ (d) = 0)				
	Mean	SE	t-value	Median	z-value
Board size	-0.138	0.12	-1.18	-0.000	-1.09
Shortfall (%)	0.009	0.01	0.96	0.000	0.84
# missing female directors	0.051	0.06	0.93	0.000	0.83
# missing female directors (exp)	0.037	0.06	0.63	0.000	0.52
# missing female directors (2019)	0.007	0.03	0.27	0.000	0.27
# missing female directors (2021)	0.044	0.04	1.01	0.000	0.86
2021 requ. failed (d)	0.025	0.02	1.25	0.000	1.25
Female directors (%)	-0.002	0.01	-0.25	-0.000	-0.24
# female directors	-0.05	0.06	-0.77	-0.000	-0.51

Table 2: Run-up return tests

This table reports differences in cumulative abnormal returns between California-headquartered firms and U.S. non-California-headquartered matched control firms for different event windows that predate the quota adoption announcement. We also report results from t-tests against zero for different cumulative abnormal return measures for the subsample of firms headquartered in California (CA HQ (d) = 1) and for the matched control sample comprising firms headquartered in any other U.S. state but California (CA HQ (d) = 0). Daily abnormal returns are computed as the observed return minus the predicted return from a market model regression estimated over a 250-day estimation window that ends on the trading day before the begin of our regular estimation window. The construction of the matched control sample is described in detail in the caption of Table 1. All cumulative abnormal return measures are winsorized at the 1st and 99th percentiles. *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively. Detailed variable definitions are in Appendix A.

	CA HQ (d) = 1			CA HQ (d) = 0			Differences	
	Mean	SE	N	Mean	SE	N	Mean	SE
CAR (-250,-1)	-2.48%	3.53%	436	-2.64%	2.79%	727	0.17%	4.52%
CAR (-200,-1)	2.77%	3.00%	437	1.46%	2.30%	729	1.32%	3.78%
CAR (-150,-1)	0.53%	2.38%	437	2.63%	1.85%	730	-2.10%	3.01%
CAR (-100,-1)	-5.62%***	1.81%	437	-3.34%**	1.45%	730	-2.28%	2.34%
CAR (-50,-1)	-5.76%***	1.15%	437	-4.88%***	0.88%	730	-0.87%	1.45%

Table 3: Market reaction to the quota's adoption announcement

This table reports differences in abnormal returns to the announcement of the adoption of the gender quota in California between California-headquartered and matched non-California-headquartered control firms. Each column shows results from a pooled ordinary least squares regression of an abnormal return measure on a dummy variable set equal to one if a firm is headquartered in California (CA HQ (d)). Across columns, we vary the length of the event window. Daily abnormal returns are computed as the observed return minus the predicted return from a market model regression estimated over a 250-day estimation window that ends on Friday, September 21. The construction of the matched sample is described in detail in the caption of Table 1. All abnormal return measures are winsorized at the 1st and 99th percentiles. Heteroscedasticity-consistent standard errors are reported in parentheses. *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively. Detailed variable definitions are in Appendix A.

Dependent variable:	CAR(0,1)	CAR(-1,1)	CAR(-2,2)
	(1)	(2)	(3)
CA HQ (d)	-0.70**	-0.74**	-1.14***
	(0.27)	(0.33)	(0.41)
ln(Total assets)	0.09	0.14*	0.12
	(0.07)	(0.08)	(0.10)
ROA	-0.72	-0.41	-2.21**
	(0.53)	(0.66)	(0.89)
MTB	-0.03*	-0.03	-0.05
	(0.02)	(0.02)	(0.03)
Constant	-1.90***	-2.23***	-1.31*
	(0.50)	(0.58)	(0.69)
R^2	0.01	0.01	0.02
N	1,238	1,238	1,238

Table 4: Policy sensitivity and quota announcement returns

This table reports results from pooled ordinary least squares regressions of two-day cumulative abnormal returns (CARs) on a dummy variable set equal to one if a firm is headquartered in California (CA HQ (d)), a proxy for a firm's vulnerability to future regulation, and an interaction term between the latter variable and the California-headquarter dummy. The proxy for a firm's vulnerability to future regulation is a dummy variable set equal to one if a firm's stock returns in the 250-day estimation window depend significantly on changes of the daily Economic Policy Uncertainty index of Baker, Bloom, and Davis (2016), zero otherwise. Regressions reported in columns (2) to (9) additionally include variables that capture the frictions a firm encounters when complying with the California board gender quota and interaction terms between these variables and the California-headquarter dummy. Daily abnormal returns are computed as the observed return minus the predicted return from a market model regression estimated over a 250-day estimation window that ends on Friday, September 21. Cumulative abnormal returns are winsorized at the 1st and 99th percentiles. The construction of the matched sample is described in detail in the caption of Table 1. Each regression includes the control variables from Table 3 (ln(Total assets), ROA, and MTB). Heteroscedasticity-consistent standard errors are reported in parentheses. *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively. Detailed variable definitions are in Appendix A.

Dependent variable:	CAR(0,1)								
Friction proxy =	-	Shortfall (%)	# missing female directors	# missing female directors (exp)	# missing female directors (2019)	# missing female directors (2021)	2021 requ. failed (d)	Female directors (%)	# female directors
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
CA HQ (d)	-0.48	0.52	0.56	0.60	-0.34	0.68	1.81**	-1.07**	-1.17**
	(0.29)	(0.43)	(0.51)	(0.51)	(0.32)	(0.56)	(0.72)	(0.47)	(0.46)
CA HQ (d) × Policy sensitive firm (d)	-1.92**	-1.85**	-1.89**	-1.86**	-1.87**	-1.90**	-1.84**	-1.86**	-1.87**
	(0.75)	(0.74)	(0.75)	(0.75)	(0.74)	(0.74)	(0.74)	(0.75)	(0.74)
Policy sensitive firm (d)	0.22	0.16	0.19	0.18	0.19	0.21	0.20	0.18	0.19
	(0.50)	(0.49)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
CA HQ (d) × Friction proxy		-4.36**	-0.62**	-0.61**	-0.51	-0.84**	-2.61***	3.95*	0.56**
		(1.73)	(0.29)	(0.27)	(0.62)	(0.37)	(0.77)	(2.14)	(0.24)
Friction proxy		1.58	0.10	0.22	0.57	-0.03	0.73	-1.94	-0.23
		(1.15)	(0.18)	(0.17)	(0.39)	(0.23)	(0.46)	(1.34)	(0.16)
Constant	-1.88***	-2.26***	-1.91***	-2.29***	-2.25***	-1.69**	-2.53***	-1.66***	-1.66***
	(0.51)	(0.70)	(0.68)	(0.73)	(0.58)	(0.66)	(0.72)	(0.53)	(0.53)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.02
N	1,238	1,238	1,238	1,238	1,238	1,238	1,238	1,238	1,238

Table 5: Spillover tests: The propensity to follow California's legislative lead

This table reports results from pooled ordinary least squares regressions of two-day cumulative abnormal returns (CARs) on state-level variables that serve as a proxy for the likelihood that a firm headquartered in a state becomes subject to future regulation. Across columns, we vary the future regulation likelihood proxy: In column (1), we use a dummy set equal to one for firms that are headquartered in states that are likely to introduce a board gender quota. In columns (2) and (3), we use dummy variables set equal to one for states that implemented similarly strict environmental laws as California, either by joining the United States Climate Alliance or by implementing comprehensive Greenhouse Gas (GhG) reduction policies. In column (4), we use state-level minimum wages. In column (5), we use a measure that quantifies the extent to which a state has followed California in legislative issues in the past proxied with the similarity in Cannabis legalization. In columns (6) and (7), we use two alternative regulatory scores with higher values indicating higher regulatory density. In column (8), we use the fraction of votes obtained by the Democratic Party in the 2016 Presidential Election. The sample comprises only non-California-headquartered firms. The construction of this sample is described in detail in the caption of Table 1. Daily abnormal returns are computed as the observed return minus the predicted return from a market model regression estimated over a 250-day estimation window that ends on Friday, September 21. Cumulative abnormal returns are winsorized at the 1^{st} and 99^{th} percentiles. Each regression includes the control variables from Table 3 (ln(Total assets), ROA, and MTB). Heteroscedasticity-consistent standard errors are reported in parentheses. *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively. Detailed variable definitions are in Appendix A.

Dependent variable:	CAR(0,1)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Impending quota (d)	-0.83**							
	(0.35)							
U.S. Climate Alliance (d)		-0.87***						
		(0.33)						
GhG reduction policies (d)			-0.66**					
			(0.33)					
Excess minimum wage ($)				-0.22**				
				(0.09)				
Cannabis legalization score					-0.29**			
					(0.14)			
Regulatory score (JL)						-0.32**		
						(0.13)		
Regulatory score (Cato)							-0.17	
							(0.13)	
% votes Democrats								-3.93**
								(1.93)
Constant	-1.23**	-1.01	-1.14*	-1.07*	-0.76	-0.37	-0.90	0.40
	(0.62)	(0.63)	(0.63)	(0.64)	(0.71)	(0.73)	(0.74)	(1.09)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01
N	780	780	780	756	780	778	778	780

Table 6: Frictions vs. legislating non-economic values

This table reports results from pooled ordinary least squares regressions of two-day cumulative abnormal returns (CARs) on state-level variables that serve as a proxy for the likelihood that a firm headquartered in a state becomes subject to future legislation, labeled X, a proxy for a firm's vulnerability to future regulation, the number of missing female directors a firm needs to appoint under the current board size in order to match the requirements postulated by California's board gender quota scaled by board size, as well as interaction terms between the future regulation likelihood proxy (X) and the proxy for a firm's vulnerability to future regulation and the number of missing female directors scaled by board size. The proxy for a firm's vulnerability to future regulation is a dummy variable set equal to one of a firm's stock returns in the 250-day estimation window depend significantly on changes of the daily Economic Policy Uncertainty Index of Baker, Bloom, and Davis (2016), zero otherwise. Across columns, we vary the future regulation likelihood proxy (X) as indicated above each column: In column (1), we use a dummy set equal to one for firms that are headquartered in states that are likely to introduce a board gender quota. In columns (2) and (3), we use dummy variables set equal to one for states that implemented similarly strict environmental laws as California, either by joining the United States Climate Alliance or by implementing comprehensive Greenhouse Gas (GhG) reduction policies. In column (4), we use state-level minimum wages. In column (5), we use a measure that quantifies the extent to which a state has followed California in legislative issues in the past proxied with the similarity in Cannabis legalization. In columns (6) and (7), we use two alternative regulatory scores with higher values indicating higher regulatory density. In column (8), we use the fraction of votes obtained by the Democratic Party in the 2016 Presidential Election. The sample comprises only non-California-headquartered firms. The construction of this sample is described in detail in the caption of Table 1. Daily abnormal returns are computed as the observed return minus the predicted return from a market model regression estimated over a 250-day estimation window that ends on Friday, September 21. Cumulative abnormal returns are winsorized at the 1st and 99th percentiles. Each regression includes the control variables from Table 3 (ln(Total assets), ROA, and MTB). Heteroscedasticity-consistent standard errors are reported in parentheses. *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively. Detailed variable definitions are in Appendix A.

Dependent variable:	CAR(0,1)							
X =	Impending quota (d)	U.S. Climate Alliance (d)	GhG reduction policies (d)	Excess minimum wage ($)	Cannabis legalization score	Regulatory score (JL)	Regulatory score (Cato)	% votes Democrats
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
X	-0.54	-0.97*	0.03	-0.27**	-0.45**	-0.23	-0.05	-2.67
	(0.54)	(0.52)	(0.52)	(0.13)	(0.22)	(0.19)	(0.19)	(3.11)
Policy sensitive firm (d)	0.78	1.14	1.19	1.24*	1.94	1.06	2.65*	7.19**
	(0.63)	(0.73)	(0.78)	(0.66)	(1.19)	(1.37)	(1.48)	(3.04)
X × Policy sensitive firm (d)	-1.79*	-1.66*	-1.94**	-0.51**	-0.69*	-0.27	-0.69*	-13.84**
	(0.93)	(0.97)	(0.95)	(0.24)	(0.42)	(0.39)	(0.37)	(5.74)
Shortfall (%)	1.38	0.56	2.20	0.18	-1.66	2.13	2.03	1.12
	(1.45)	(1.61)	(1.46)	(1.77)	(2.93)	(2.90)	(3.32)	(6.00)
X × Shortfall (%)	-0.29	1.33	-1.97	0.50	1.05	-0.23	-0.18	0.42
	(2.18)	(2.09)	(2.15)	(0.54)	(0.89)	(0.79)	(0.87)	(11.68)
Constant	-1.93**	-1.60*	-2.09**	-1.59*	-0.91	-1.33	-1.96*	-0.86
	(0.87)	(0.90)	(0.87)	(0.94)	(1.11)	(1.06)	(1.03)	(1.75)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.02	0.02	0.02	0.02	0.02	0.02	0.01	0.02
N	780	780	780	756	780	778	778	780

Appendix A: Variable definitions

This table reports variable definitions of all variables used in the paper as well as their data sources. Database mnemonics are in italics (if available).

Variable	Definition	Source
CAR(t_1,t_2)	Cumulative abnormal return, estimated as the sum of daily (unwinsorized) abnormal returns from t_1 to t_2 where October 1, 2018 marks the event date. Daily abnormal returns are calculated as the observed return minus a predicted return. The predicted return is estimated using a market model regression where daily returns (adjusted for distributions and stock splits) are regressed on daily value-weighted index returns over a 250-day estimation window that ends six trading days prior to the event (September 21). At least 125 daily observations with non-missing stock and index return data are required. Winsorized at the 1st and 99th percentiles.	Compustat
CA HQ (d)	Dummy variable equal to one if a firm is headquartered (*state*) in California as of September-end 2018, zero otherwise.	Compustat
Policy sensitive firm (d)	Dummy variable equal to one if the firm's stock returns in the 250-day estimation window that ends six trading days prior to the event (September 21) depend significantly (p-value < 0.1) on changes (in percent) of the daily Economic Policy Uncertainty (EPU) index of Baker, Bloom, and Davis (2016) when controlling for daily value-weighted market returns, zero otherwise. The daily EPU index relies on the Newsbank database and is computed as a scaled daily number of articles that appeared in around 1,500 U.S. newspapers and include the triple 'uncertainty' or 'uncertain'; 'economic' or 'economy'; and one of the following policy terms: 'congress', 'deficit', 'Federal Reserve', 'legislation', 'regulation' or 'white house' (including variants).	Baker, Bloom, and Davis (2016)
Board size	Number of directors on the board.	BoardEx
# missing female directors	Number of female directors necessary to fulfill the 2021 female director requirements of SB 826 assuming that board size stays constant, that is, three minus the current number of female directors if board size is six or more, two minus the current number of female directors if board size is five, and one minus the current number of female directors if board size is four or less.	BoardEx
# missing female directors (2019)	Number of female directors necessary to fulfill the 2019 female director requirements of SB 826, that is, one if the current number of female directors is zero and zero otherwise.	BoardEx
# missing female directors (2021)	Number of female directors necessary to fulfill the 2021 female director requirements of SB 826 assuming that board size stays constant and that the 2019 requirement of having one female director has been met, that is, two minus the current number of female directors if board size is six or more, one minus the current number of female directors if board size is five, and zero if board size is four or less.	BoardEx
# missing female directors (exp)	Number of female directors necessary to fulfill the 2021 female director requirements of SB 826 assuming that female directors will be added to the board, which increases board size and can trigger the requirement to add additional female directors. For instance, if a firm currently has no female director and board size if four, the addition of a new female director increases board size to five. Having a board size of five requires the firm to add another female director, which increases board size to six. Having a board size of six requires the firm to add another female director. For current board size of four, this measure would therefore be three (rather than one if one assumes that firms exchange female directors for male directors).	BoardEx
Shortfall (%)	# missing female directors scaled by board size.	BoardEx
2021 requ. failed (d)	Dummy variable equal to one if a firm fails to comply with the 2021 female director requirements of SB 826, that is, a firm does not have three female directors if board size is six or more, two female directors if board size is five, and one female director if board size is four or less, zero otherwise.	BoardEx
Female directors (%)	Fraction of directors on the board that are female.	BoardEx
# female directors	Number of directors on the board that are female.	BoardEx
Total assets	Total assets (*at*).	Compustat

ROA	Operating income before depreciation (*oibdp*) scaled by total assets (*at*), winsorized at the 1st and 99th percentiles.	Compustat
MTB	Market value equity ($csho \times prcc\_f$) scaled by book value equity (*ceq*), winsorized at the 1st and 99th percentiles.	Compustat
Impending quota (d)	Dummy variable equal to one for firms headquartered in states that are likely to follow California in introducing a board gender quota (MA, IL, NJ, NY, and WA), zero otherwise.	Newspaper articles
U.S. Climate Alliance (d)	Dummy variable equal to one for firms headquartered in states that had joined the United States Climate Alliance by the time of the adoption of the gender quota law in California, zero otherwise. The United States Climate Alliance is a bipartisan coalition of states and other U.S. territories that are committed to upholding the objectives of the 2015 Paris Agreement on climate change within their borders and meeting or exceeding the targets of the federal Clean Power Plan. The Alliance was formed on June 1, 2017 by the governors of the three founding states following President Donald Trump's announcement that he had decided to withdraw the U.S. from the Paris Agreement. Until September 30, 2018, CO, CT, DE, HI, MA, MD, MN, NC, NJ, OR, RI, VA, and VT had joined the three founding states CA, WA, and NY.	U.S. Climate Alliance, Newspaper articles
GhG reduction policies (d)	Dummy variable equal to one for firms headquartered in states that have implemented comprehensive Greenhouse Gas (GhG) reduction policies, zero otherwise. As comprehensive GhG reduction policies, we consider statutory reduction requirements, statutory reporting requirements, market-based policies, or the Transportation & Climate Initiative (TCI). Besides CA, the following states have such policies in place: CT, DE, HI, IA, MA, MD, ME, MN, NH, NJ, NV, NY, OR, PA, RI, VA, VT, and WA.	NCSL
Excess minimum wage ($)	Difference in dollars between the state and federal minimum wage per hour, estimated as the state-wide minimum wage per hour less the federal wage of $7.25 per hour. For the spatial distribution of the state-level minimum wages, see Panel A in Figure OA.1 in the Online Appendix.	NCSL
Cannabis legalization score	Score variable that is equal to zero if any type of cannabis use is considered illegal, equal to one if the use of cannabis for medical purpose is legal but laws restrict the allowable concentration of tetrahydrocannabinol (THC), the main psychoactive component of cannabis, equal to two if the use of cannabis is legal for medical purposes, equal to three if the recreational consumption of cannabis is illegal, but has been decriminalized, and equal to four if the recreational consumption of cannabis is legal. For the spatial distribution of this variable, see Panel B in Figure OA.1 in the Online Appendix.	State-level legislative websites
Regulatory score (JL)	Score variable ranging from one to five with low scores indicating little regulation and high scores high regulation. We group all 50 states and the D.C. into quintiles according to their regulatory ranking and assign a score of one to firms headquartered in states with the least restrictive regulation and five for firms in states with the strictest regulation.The ranking is based on 27 provisions in six categories (Land Use, Labor Market, Utilities, Occupations, Tort, and Insurance Regulations). For the spatial distribution of this variable, see Panel C in Figure OA.1 in the Online Appendix.	John Locke Foundation
Regulatory score (Cato)	Score variable ranging from one to five with low scores indicating little regulation and high scores high regulation. We group all 50 states and the D.C. into quintiles according to their regulatory index values and assign a score of one to firms headquartered in states with the least restrictive regulation and five for firms in states with the strictest regulation. The index is based on 50 provisions in seven categories (Land-use Freedom, Health Insurance Freedom, Labor Market Freedom, Lawsuit Freedom, Occupational Freedom, Miscellaneous Regulatory Freedom, and Cable and Telecommunications). For the spatial distribution of this variable, see Panel D in Figure OA.1 in the Online Appendix.	Cato Institute
% votes Democrats	Fraction of votes obtained by Democratic Party in 2016 Presidential Election in the state where a company's headquarter is located.	Politico

Online Appendix to: As California goes, so goes the nation? Board gender quotas and the legislation of non-economic values

May 18, 2021

1 Introduction

The purpose of this Online Appendix is to provide details and results of additional tests briefly mentioned in the paper "As California goes, so goes the nation? Board gender quotas and the legislation of non-economic values". In Section 2, we discuss alternative specifications of our baseline difference-in-differences estimations of the quota announcement effect reported in the paper, including tests that address the potential concern that our results are biased due to contemporaneous cross-correlation of (abnormal) stock returns. Results of these tests are reported in Tables OA.1 to OA.7. Section 3 describes the results from an analysis of changes in female board representation around the adoption of the quota. The results are reported in Table OA.8. Section 4 provides tests of the importance of frictions in the director labor market in explaining the negative announcement returns to the quota's adoption. These tests use eight alternative demand-based measures and three supply-based measure that are all meant to proxy for frictions in the director labor market. Results are reported in Tables OA.9 and OA.10. Section 5 examines the determinants of firms' policy sensitivity. The results are reported in Table OA.11. The analysis described in Section 6 tests the robustness of results reported in Table 6 of the paper, which aims at disentangling the friction-based explanation of the negative quota effect from the explanation based on the legislation of non-economic values, to the choice of the (demand-based) friction measure. The results are reported in Table OA.12. Finally, in Section 7, we discuss the spatial distribution of some of the state-level proxies for the probability to follow California's legislative lead, as displayed in Figure OA.1, and explain how we construct these variables.

2 Alternative difference-in-differences specifications

2.1 Confounding events: Controlling for the effect of SB 822

As explained in Section 2.1 in the paper, California's governor decided on a total of 183 bills over the same weekend that he signed the board gender quota into law. One single bill dominated the board gender quota (SB 826) in terms of media coverage: the net neutrality law (SB 822). Here we set out to test whether this law potentially affects our results. To this end, we first identify firms that are most affected by the law. Specifically, we define a dummy variable, Telecom, which is equal to one for firms with a two-digit SIC code of 48 (Communications) and for firms which are members of the US Telecom Trade Group. Second, we rerun our tests in Table 3 of the paper, augmented with the Telecom dummy variable and an interaction term between the California headquarter dummy and the Telecom dummy. The results are reported in Table OA.1. The coefficients on the California headquarter dummy variable and the regression constant remain very similar to those reported in Table 3. Moreover, the coefficient on the interaction term between the California headquarter dummy and the Telecom dummy is insignificant across all three specifications. Hence, we conclude that the announcement returns to the board gender quota are not affected by the net neutrality law.

2.2 Equally-weighted index returns as a proxy for the market return

In our baseline specification, we use the return of a value-weighted market index consisting of all sample firms as a proxy for the market return to estimate daily abnormal returns. In this subsection, we test how our main results change if we use the return of an equally-weighted market index to estimate daily abnormal returns. To this end, we rerun the event study with this alternative market return proxy, reestimate all abnormal return measures, and replicate Table 3. Table OA.2 reports the results. We continue to find a negative and significant coefficient on the California-headquarter dummy variable in all three columns as well as a negative and statistically significant intercept in two of the three columns. Economically, we find that the estimates are somewhat reduced compared to the those reported in Table 3, but still economically meaningful. For instance, using an equally-weighted (value-weighted) market index as a proxy for the market return, we find that California-headquartered firms underperform matched non-California-headquartered control firms by 0.54% (0.74%) over the two days following the adoption of the quota law. Taken together, the results discussed in this subsection indicate that the choice of the market return proxy does not alter our conclusions.

2.3 Alternative control sample

In this subsection, we show that our main difference-in-differences results, reported in Table 3 and discussed in Section 3 of the paper, remain unaffected when we use all non-California firms that pass the sample selection criteria as control firms. Recall that in all regressions in the paper, we use a control sample constructed by drawing for each of our 458 treatment firms headquartered in California three non-California firms that share the same primary two-digit SIC and are closest in terms of total assets. In the paper, we match with replacement, i.e., a firm in the control sample may serve as a matched control firm to more than one treatment firm, but we include every control firm only once in the sample, resulting in a sample that comprises 1,238 firms, 458 in the treatment group and 780 in the control group. In Table OA.3, we report results from re-estimating our baseline specification reported in Table 3 of the paper using all non-California firms that pass our sample selection procedure explained in Section 2.1 of the paper as a control group. The results remain qualitatively unchanged compared to those reported in Table 3 using the matched sample.

2.4 Difference-in-differences estimates with treatment- and post dummies

In this subsection, we conduct a difference-in-differences analysis using a treatment dummy and a post-treatment dummy. To this end, we estimate OLS regressions of daily abnormal returns (ARs) on a dummy variable which is equal to one if a firm is headquartered in California (CA HQ (d)) and zero otherwise, and a dummy variable which is equal to one for observations measured after the implementation of the quota (Post (d)) and zero for observations measured before the implementation of the quota. We also add an interaction term between these two variables. The results obtained when using a four-day event window with two pre-treatment (September 27 and 28) and two post-treatment (October 1 and 2) observations per sample firm are reported in column (1) of Table OA.4. The difference-in-differences estimator, i.e., the coefficient on CA HQ (d) × Post (d), is negative and significant at the 5% level. In terms of economic magnitude, the coefficient estimate suggests a two-day abnormal return of California firms that is 0.72% lower than that of non-California firms (0.36% per post-treatment day), a number very similar to the two-day abnormal return difference of -0.70% reported in column (1) of Table 3 of the paper. Moreover, the coefficient of -0.63% on the Post (d) dummy variable, which is significant at the 1% level, suggests that both California and non-California firms significantly underperform the market in the two days following the announcement of the quota's adoption. The results in column (2) show that the inclusion of a set of firm-level control variables leaves the results virtually unchanged. In column (3), we add firm fixed effects to the specification in column (1) to control for unobservable heterogeneity at the firm-level that is time-invariant. Note that the firm fixed effects absorb all firm-level covariates from column (2), including the treatment dummy, CA HQ (d), as these variables are time-invariant over the four-day sample period used in this analysis. While the significance level is somewhat reduced, the estimates remain economically unchanged.

2.5 Excluding firms with trading relations with California firms

The results in Table 3 of the paper show negative and significant spillover effects of the adoption of the board gender quota in California to non-California firms. One possible explanation for these spillover effects, which we test empirically in Section 5 of the paper, is that other states are expected to follow California's legislative lead and introduce a board gender quota or other value-reducing regulation as well. A straightforward alternative explanation would be hat some of the non-California firms in our control sample are affected by the law in California through direct trading relationships. To test whether direct trading relationships drive the spillover effects documented in Table 3, we conduct two robustness tests. First, we exclude all firms from the control sample that disclose at least one of the treated California sample firms as principal customer. To identify principal customers, we follow (Cohen and Frazzini, 2008) and collect the names of all principle customers from the Compustat Segments database as of the most recent financial year-end prior to the quota's adoption announcement. Regulation SFAS No. 131 requires firms to disclose the identity of any customer representing more than 10% of total reported sales. Using string-matching based on company names, we find 37 matched control firms disclosing at least one California-headquarter sample firm as principal customer (4.7% of 780 matched control firms). Results of this robustness test are reported in Panel A of Table OA.5 and are virtually identical to those reported in Table 3 of the paper. Second, we exclude firms in geographic proximity to California as these firms may be more likely to directly interact with California firms. Specifically, we exclude all firms with headquarters in the three neighboring states (AZ, OR, and NV) plus the five states that are neighbors to California's direct neighbors (CO, ID, NM, UT, and WA). Results are reported in Panel B of Table OA.5. These results again remain virtually unchanged when compared to those in Table 3 of the paper. Hence, the results from these robustness tests suggest that direct trading relationships to California firms are unlikely to drive the spillover effects documented in Table 3.

2.6 Calendar-time portfolio analysis

In this subsection, we conduct robustness tests that address concerns arising from the fact that we study the market reaction of firms to one single event. Specifically, all firms in the treatment group, i.e., the firms headquartered in California in our sample, are treated at the same date, October 1, 2018. Such a single event date may result in contemporaneous cross-correlation of (abnormal) stock returns. To address this concern, we follow Eckbo, Nygaard, and Thorburn (2021) and form equally-weighted calendar time portfolios of all California firms, our treatment sample, and all non-California size- and industry-matched control firms, our control sample. We then estimate the portfolios' daily abnormal returns by estimating the following time-series regression over a sample that includes all observations from the 250-day estimation window, which ends on September 21, and the observations from the respective event window:

$$r_t = \alpha + ARd_t + \beta r_{wt} + \epsilon_t$$

where r_t is the daily equally-weighted return of the portfolio of all Californian (or size- and industry matched control) firms in excess of the daily 1-month U.S. treasury bill rate. Alternatively, to analyze differences in abnormal returns between Californian and non-Californian firms, we define r_t as the daily difference in portfolio returns of Californian and non-Californian firms. r_{wt} is the daily value-weighted market index return in excess of the daily 1-month U.S. treasury bill rate. As a proxy for the market return, we use the return of a self-computed, value-weighted portfolio consisting of all sample firms. d_t is a dummy variable that takes a value of one for each day in the event window, and zero otherwise. AR is the average daily abnormal portfolio return over the event days. Hence, estimates for the cumulative abnormal returns are obtained by multiplying the obtained coefficient for AR by the number of days in the event window. For instance, to obtain the two-day CAR(0,1), d_t takes a value of one in the two-day event window that includes the first two trading days after the quota came into effect. The two-day cumulative abnormal return, CAR(0,1), is then computed as 2 $\times$ AR.

The results are reported in Table OA.6. As in Table 3 of the paper, we use three alternative abnormal return measures, estimated over event windows that range from two to five days in length. Consistent with results reported in the paper, we find announcement returns to the introduction of the quota to be significantly more negative for California firms, as shown in the last two columns of the table. Also consistent with results reported in Table 3 of the paper, we find non-California firms to react negatively to the quota's adoption in California as well. In terms of economic magnitude, the results obtained here are similar to those reported in Table 3 of the paper. For instance, we find the two-day cumulative abnormal return, which includes the event day and the day after (CAR(0,1)), to be -1.42% for non-California firms. California firms react even more negatively: Their two-day announcement return is -2.13%. The difference between California and non-California firms of -0.70% is identical to the estimate reported in column (1) of Table 3 in the paper. All these estimates remain statistically significant at the 5% level or higher. Hence, accounting for potential contemporaneous cross-correlation resulting from a single event does not materially affect our results.[1]

2.7 Alternative event dates

Our event study results focus on the date on which the Governor of California signed the quota law. As Eckbo, Nygaard, and Thorburn (2021) point out, it is important to include all major quota-related news events that increase the likelihood of a quota law in an analysis of changes in firm value. Therefore, we reestimate our difference-in-differences analysis for other potential quota-related event dates. These are the day of the introduction of the law (January 3) and the day after, the day of the successful Senate vote (May 31) and the day after, and a three-day event window that includes the day of the Assembly vote (August 29), the day of the second Senate vote (August 30), and the day after. Results are reported in Table OA.7. All three coefficients on the California-headquarter indicator variable are insignificant, suggesting that the market reaction to the California gender quota was confined to the days after the Governor signed the law. The choice of our event window is further justified by the patterns reported in Figure 2 in the paper, which shows the distribution of the newspaper coverage of the gender quota in California.

[1] A potential concern of event studies with a single event is that calendar day anomalies, such as the day-of-the-week or day-of-the-month effects, affect our abnormal return estimation. To address such concerns, we reestimate our portfolio sorts analysis including a Monday dummy variable and a first trading day of the month dummy variable as our event day is Monday, October 1, 2018. We find our results to remain virtually unchanged.

3 Changes in female board representation around the adoption of the quota

To analyze the pattern displayed in Figure 4 in the paper in a regression framework, we estimate regressions at the firm-month level with the fraction of female directors on the board as dependent variable over the period September 2018 to March 2019. Hence, we obtain a firm-month panel containing up to seven monthly observations per firm. We then regress the fraction of female directors on the board on dummy variables for the month of observation, omitting September 2018, and interaction terms between the California-headquarter dummy and the month dummy variables. To account for time-invariant unobserved heterogeneity at the firm level, we add firm fixed effects. The coefficients on the month dummies indicate the percentage points by which female board representation has changed, on average across all sample firms, in the respective month compared to the introduction of the quota at the end of September 2018. The coefficients on the interaction terms between the month dummy and California-headquarter dummies are the difference-in-differences estimators, that is, the average percentage points difference in the change of female board representation of California firms relative to the control firms at the end of a given month. If the quota law already had a statistically significant impact on female board representation at firms headquartered in California, the difference-in-differences estimators are expected to be significantly positive.

Results in column (1) of Table OA.8 show that female board representation at California firms indeed increased relative to the sample of non-California control firms. At the end of October, one month after the quota's introduction, the difference amounts to 0.26 percentage points, which is statistically significant at the 10% level. Two months after the quota's introduction, the difference increases to 0.39 percentage points, which is statistically significant at the 5% level. The difference continues to grow monotonically and amounts to 0.97 percentage points in March 2019, which is statistically significant at the 1% level. Compared to the average annual growth rate of female board representation, which amounts to 0.8% in the Russell 3000 index over the period 2008-2018 (see Figure 1 in the paper), this quota-induced increase in female board representation over a mere six-month time period is therefore economically meaningful.

Next, we test whether firms under more pressure to appoint female directors respond stronger to the introduction of the quota. In column (2), we, therefore, retain only firms in the sample that need at least one female director to comply with the quota at adoption announcement, and, in column (3), we retain only firms that need at least two female directors. Consistent with our expectations and descriptive evidence provided in Figure 4 of the paper, we find that the coefficients on the interaction terms between the California-headquarter dummy and the month dummy variables increase monotonically from column (1) to column (3). California-headquartered firms that require one (two) female directors to comply with the quota on average increased female board representation by 1.15 (1.48) percentage points relative to the control firms six months after the quota's adoption. In the first three columns, the coefficients on all month-end dummy variables, which capture the general time trend, are positive and significant, suggesting that both California and non-California firms significantly increased female board representation in the months after the adoption of the quota – but as the difference-in-differences estimates show, California firms even more so.

The public debate around female board representation often emphasizes the number of firms without any female director on the board to stress the most extreme cases of gender inequality.[2] The goal of our next test is to ascertain whether the new gender legislation has helped female directors to break into all-male boardrooms or whether the increase in female board representation documented in columns (1) to (3) is mostly driven by firms that already have at least one female director on the board and add additional female directors after the quota's adoption. Column (4) reports results obtained from estimating the regression in column (1) and replacing the dependent variable with a dummy variable that is equal to one if a firm has no female director on the board at the end of a given month. The coefficients on all six difference-in-difference estimators are negative and statistically significant at the 5% level or higher. They suggest that the fraction of California firms without a female director on the board has gone down by roughly 6% relative to the matched control firms by March-end 2019. Hence, California's gender quota indeed seems to have induced some firms without any female director to appoint at least one. Moreover, these results indicate that California firms move towards fulfilling the first threshold stipulated by SB 826, that is, that all firms headquartered in California have to have at least one female director by the end of the

[2] See, for instance, Joanna S. Lublin, *Why Breaking Into the Boardroom Is Harder for Women*, The Wall Street Journal, Feb. 2, 2018, and Vanessa Fuhrmans, *The Last All-Male Board on the S&P 500 Is No Longer*, The Wall Street Journal, Jul. 24, 2019.

year 2019.

In summary, these results imply that California's female board quota, although argued to be lacking teeth, is in fact taken seriously by firms: It triggered a significant increase in female board representation and a significant reduction in the number of firms without any female director on the board already within six months after adoption of the quota.

4 Can frictions in the director labor market explain the negative announcement returns to the quota's adoption?

To examine whether frictions in the director labor market drive the negative announcement returns of California and non-California firms to the quota's adoption, we regress two-day cumulative abnormal returns (CARs) on a dummy variable that is equal to one if a firm is headquartered in California (CA HQ (d)), a firm-specific characteristic related to quota compliance, interaction terms between these two, and the set of control variables from Table 3. Results from using the eight alternative proxy variables for frictions in the director labor market, as detailed in Section 2.2 of the paper, are reported in Table OA.9. We find seven of the eight coefficients on the interaction terms between the friction measures and the California-headquarters dummy to exhibit the expected sign and to be statistically significant at the 10% level or higher. Thus, California firms facing a larger friction, i.e., firms with a larger female director gap on their board, indeed react stronger to the gender quota announcement. In contrast, such frictions do not seem to play a role for non-California firms, as indicated by the insignificant coefficients on the friction variables themselves. Most important, the regression constant remains negative and statistically significant across all specifications, indicating an economically large negative announcement return for both California and non-California firms that remains unexplained by the friction-based measures.

All of these measures of frictions in the director labor market are based on the quota-induced demand for female directors. Alternatively, we consider the supply side, i.e., the pool of potential female director candidates to be appointed to the boards of firms facing a quota requirement. Inspired by Knyazeva, Knyazeva, and Masulis (2013), who construct a measure of local director supply, we resort to BoardEx data and compute a related measure of local female director supply. For each sample firm, we count the number of female directors that currently serve on boards of other firms with headquarters within a 100-mile distance from the sample firm's headquarters, scaled by the number of other firms within the same distance, and define a dummy variable that is equal to one for those firms that have a supply of female directors larger than the median supply in our sample. As an alternative measure of female director supply, we compute the distance of a firm's headquarter to the California state border and define a dummy variable equal to one if the headquarter is located within 100 miles from the border. This measure follows the idea that California firms close to the border can more easily tap potential female director candidates from neighboring states, that are not subject to a gender quota. As a third supply-based measure, we compute the distance between a firm's headquarter and the nearest major airport, as classified by the Federal Aviation Administration (FAA), and define a dummy variable equal to one if the headquarter is located within 50 miles from the nearest airport. This measure follows the notion that the preferred mode of travel for executives is via airplane, making it easier for firms located nearby major airports to hire female directors from all over the U.S.

We then reestimate the regression specification as reported in column (1) of Table OA.9 augmented with the supply-based measures and an interaction term between the supply-based measures and the California-headquarter indicator. The results are reported in Table OA.10 and show that the coefficient on the interaction term between the California-headquarter dummy variable and the supply-based measures is positive across all three specifications, but statistically significant in column (3) only. These findings provide some suggestive evidence that the negative valuation effect associated with the quota's adoption is reduced for California firms if there is a larger supply of potential female director candidates. The coefficient on the interaction term between the California-headquarters dummy variable and Shortfall (%) is negative and significant at the 5% level or higher in all three specifications. Hence, supply-based measures generally seem not to matter much, or do be dominated by demand-based measures.

5 Determinants of policy sensitivity

To check whether firms' policy sensitivity is significantly related to our measures of frictions in the director labor market, we regress the dummy variable whether a firm is policy sensitive on the proxy variables for frictions in the director labor market and firm-level covariates. The results of these regressions are reported in Table OA.11. In column (1), we employ a firm's size (logarithm of total assets), profitability (ROA), and valuation level (market-to-book ratio) as explanatory variables. We find that larger firms are less likely to be policy sensitive. In columns (2) to (9), we consecutively add our eight measures of the female director gap on the board as additional explanatory variables. We find coefficients on these friction proxies that are very close to zero, while firm size continues to be a significant determinant of whether a firm is policy sensitive. These insignificant coefficients on the friction proxies suggest that policy sensitive firms do not differ from their non-policy sensitive counterparts when it comes to female board representation or the female director gap. This finding mutes the concern that the effect of policy sensitivity captures the effect of restrictions imposed on firms by the quota.

6 Using alternative friction measures when disentangling labor market frictions and the legislation of non-economic values

To test the robustness of our results reported in Table 6 of the paper with respect to the choice of the friction measure, we reestimate Table 6 replacing our baseline measure, Shortfall (%), by the other seven proxy variables for frictions in the director labor market. The results are reported in Table OA.11, Panels A to G. The results are similar to those reported in Table 6 with the majority of coefficients on the interaction term between the proxy variable for the probability to follow California in legislative issues and the dummy for policy-sensitive firms being negative and significant at the 10% level or higher across all seven panels. In contrast, only two of 56 interaction terms (3.6%) between the proxy variable for the probability to follow California in legislative issues and the proxy variables for frictions in the director labor market are significant at the 10% level or higher. These results again are consistent with our presumption that the negative spillover effects observed at non-California firms are more likely to be the result of general expectations regarding future legislation of non-economic values than more specific expectations that a gender quota may be introduced.

7 Spatial distribution of state-level proxies for the probability to follow California's lead

In this section, we discuss the spatial distribution of some of our proxy variables for the states' propensity to follow California's legislative lead. These distributions are graphically displayed in Figure OA.1. Moreover, we explain how we construct these proxy variables.

Panel A provides an overview of the minimum wage policies of all U.S. states and the D.C. Six states do not set their own minimum wages but refer to the federal minimum wage of $7.25 per hour. Three states even set minimum hourly wages below the federal rate, which implies that the federal rate applies. Fourteen states define the minimum hourly wage to be equal to the federal rate of $7.25. Twenty-eight states and the D.C. set minimum hourly wages in excess of the federal rate, out of which 14 states set hourly minimum wages between $7.25 and $10 and 13 states and the D.C. set hourly minimum wages in excess of $10. Note that the hourly minimum wage set at the state-level can vary within a state. Oregon, for instance, prescribes a minimum wage per hour of $10.5 for non-urban counties and of $12 for the Portland Metropolitan Area. In such cases, we use the lower minimum wage. We also disregard minimum wages set at the municipality or city-level as prevalent, for example, in Berkeley ($15). In the paper, we use the difference between the state-level minimum wage and the federal rate as a proxy for the state's propensity to follow California in legislative issues. As of September-end 2018, California is tied with Massachusetts and Washington for second place with a $12 minimum wage per hour. The D.C. ranks first ($13.25).

Panel B provides an overview of the cannabis policy of all U.S. states and the D.C. as of the board gender quota's adoption date (September 30, 2018). In the paper, we make use of an index that captures the extent to which cannabis consumption is legalized in a given state. It is equal to zero if any type of cannabis use is considered illegal (two states, very light grey), equal to one if cannabis is legal for medical purpose, but laws restrict the allowable concentration of tetrahydrocannabinol (THC), the main psychoactive component of cannabis, (15 states, light grey) equal to two if the use of cannabis is legal for medical purposes without restrictions on THC-levels (13 states; grey), equal to three if the recreational consumption of cannabis is illegal, but has been decriminalized (10 states; dark grey), and equal to four if the recreational consumption of cannabis is legal (10 states and the D.C.; black). If a state legalized the use of cannabis for medical treatment without restrictions on THC-levels, the numbers in parentheses indicate the year in which it was legalized. California was the first state to legalize Cannabis for medical purpose (in 1996) and is among those states which legalized Cannabis consumption completely.

Panels C and D display the distribution of the regulatory score variables across all 50 U.S. states. The two regulatory scores employed in our paper are based on regulatory indices provided by two libertarian U.S. think tanks, the John Locke Foundation and the Cato Institute. The John Locke Foundation uses 27 provision in six different categories (Land Use, Labor Market, Utilities, Occupations, Tort, and Insurance Regulations) to compute a "regulatory freedom ranking" for each U.S. state. The last available issue of this ranking is from the year 2015. The Cato Institute's index was issued in 2018 and is based on 2016 data. The index encompasses 50 provisions in seven categories (Labor Regulation, Health Insurance, Occupational Licensing, Eminent Domain, Liability System, Land and Environment Regulation, and Utility Deregulation). We use the John Locke Foundation ranking and the Cato index to compute two alternative regulatory score variables. To this end, we group all states and the D.C. into quintiles according to their regulatory index values, and assign a score of one to firms headquartered in states with the least restrictive regulation and five for firms in states with the strictest regulation. Panel C shows the spatial distribution of these scores based on the index using John Locke Foundation's data and Panel D using the Cato Institute's. As expected, California is among the most heavily regulated states in both panels.

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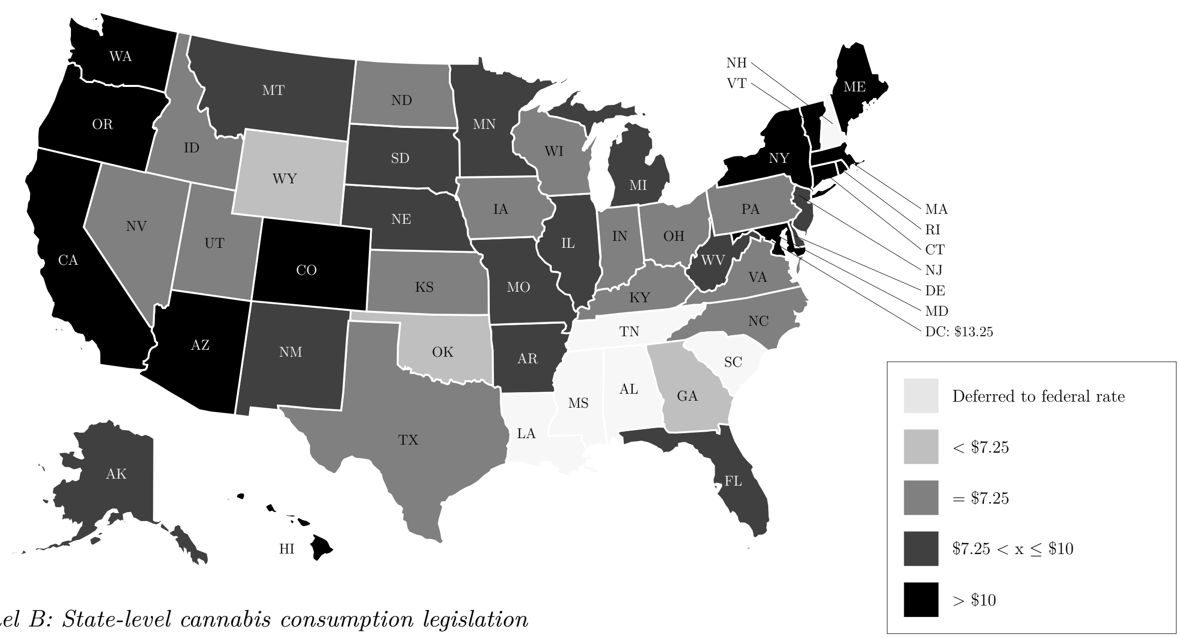
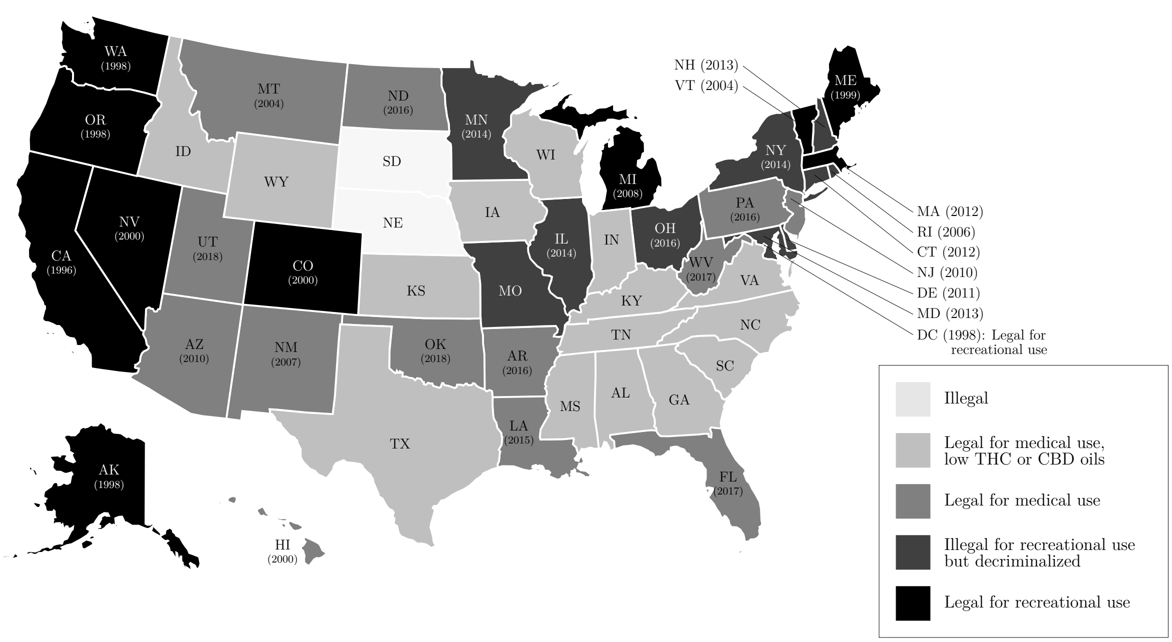
Figure OA.1: Distribution of state-level proxies for the probability to follow California's lead

Panel A of this figure shows the state-level minimum wage per hour for each state and the D.C. Panel B shows the legislation governing cannabis consumption for each state and the D.C. and, if applicable, the year in which cannabis was legalized for medical use. Panels C and D show the regulatory scores for each state. For a detailed description of the construction of these variables, see Section 6 of this Online Appendix.

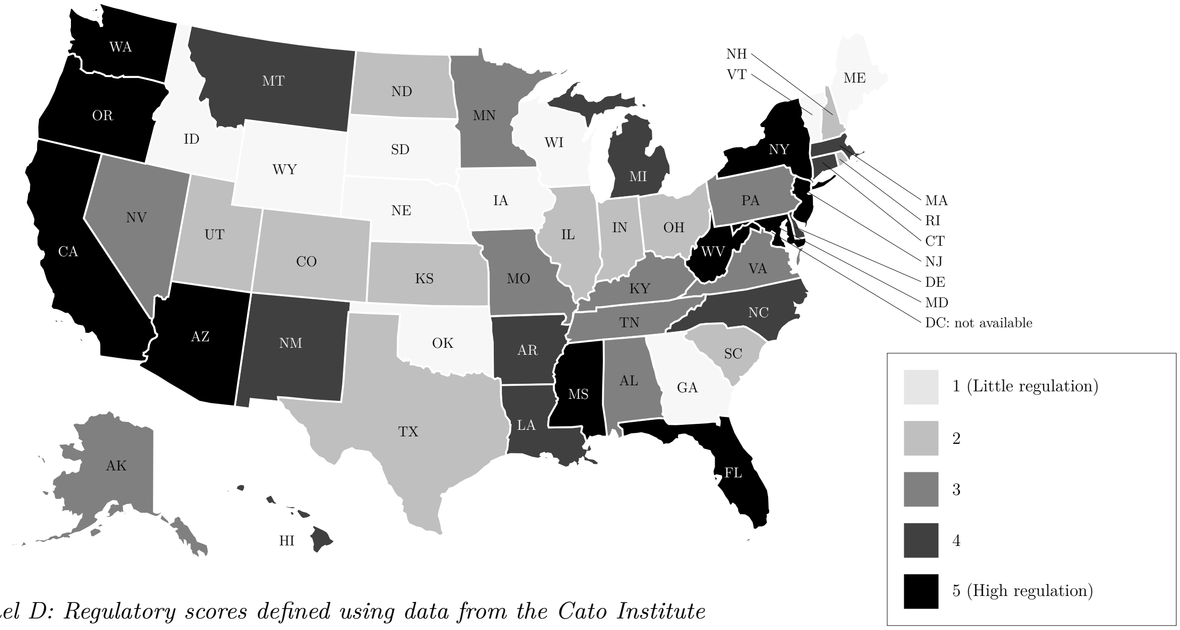
Panel A: State-level minimum wages per hour



Panel B: State-level cannabis consumption legislation



Panel C: Regulatory scores defined using data from the John Locke Foundation



Panel D: Regulatory scores defined using data from the Cato Institute

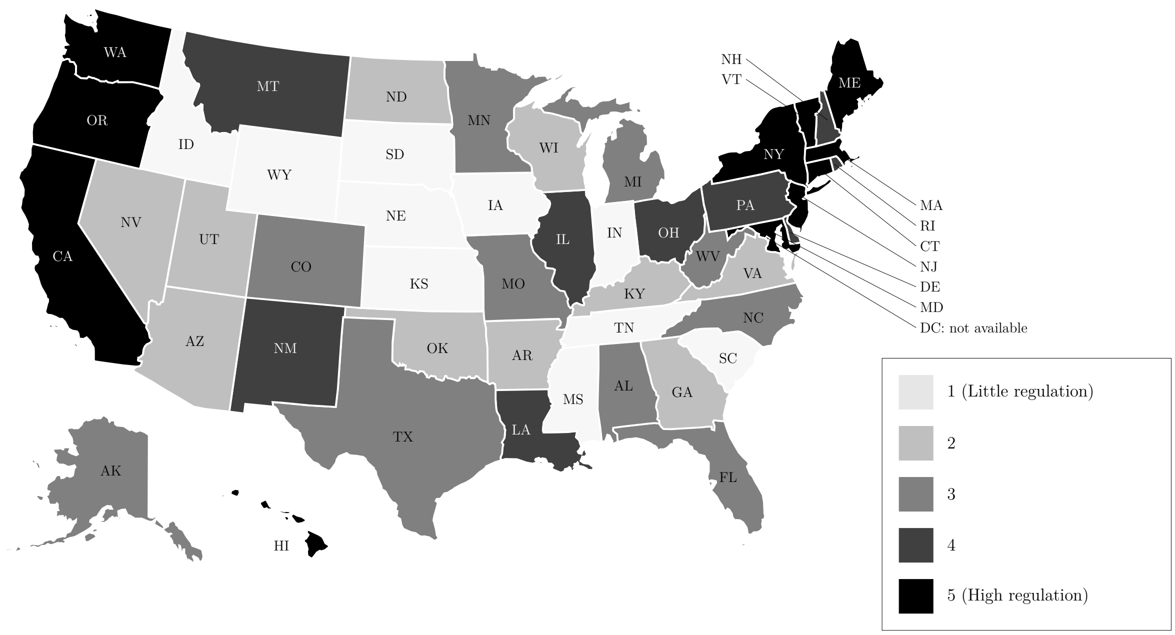


Table OA.1: Robustness tests: Controlling for the introduction of SB 822

This table reports robustness tests of the results shown in Table 3 in the paper by controlling for the potential impact of the contemporaneous introduction of SB 822 (Net Neutrality Law). Specifically, each column reports results from a pooled ordinary least squares regression of an abnormal return measure on a dummy variable set equal to one if a firm is headquartered in California (CA HQ (d)), a dummy variable equal to one for firms with a two-digit SIC code 48 (Communications) and for firms which are members of the U.S. Telecom Trade Group (Telecom (d)), and an interaction term between the two dummy variables. Daily abnormal returns are computed as the observed return minus the predicted return from a market model regression estimated over a 250-day estimation window that ends on Friday, September 21. Cumulative abnormal returns are winsorized at the 1st and 99th percentiles. The construction of the matched sample is described in detail in the caption of Table 1 of the paper. Heteroscedasticity-consistent standard errors are reported in parentheses. *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively. Detailed variable definitions are in Appendix A of the paper and of this Online Appendix.

Dependent variable:	CAR(0,1)	CAR(-1,1)	CAR(-2,2)
	(1)	(2)	(3)
CA HQ (d)	-0.67**	-0.70**	-1.10***
	(0.28)	(0.33)	(0.42)
CA HQ (d) × Telecom (d)	-1.45	-1.75	-2.18
	(1.08)	(1.25)	(1.75)
Telecom (d)	1.08**	1.65***	2.35***
	(0.46)	(0.63)	(0.90)
ln(Total assets)	0.09	0.14*	0.12
	(0.07)	(0.08)	(0.10)
ROA	-0.73	-0.42	-2.22**
	(0.53)	(0.66)	(0.89)
MTB	-0.03*	-0.03	-0.05
	(0.02)	(0.02)	(0.03)
Constant	-1.92***	-2.25***	-1.34*
	(0.50)	(0.58)	(0.70)
R^2	0.01	0.01	0.02
N	1,238	1,238	1,238

Table OA.2: Robustness tests: Equally-weighted returns as market return proxy

This table reports robustness tests of the results shown in Table 3 in the paper. Specifically, it reports differences in abnormal returns around the announcement of the adoption of the gender quota in California between California-headquartered and matched non-California-headquartered control firms. Each column shows results from a pooled ordinary least squares regression of an abnormal return measure on a dummy variable set equal to one if a firm is headquartered in California (CA HQ (d)). Across columns, we vary the length of the event window. In our baseline regression shown in Table 3 of the paper, we use a value-weighted index return as a proxy for the market return. In this table, we use an equally-weighted index return as a proxy for the market return. All abnormal return measures are winsorized at the 1^{st} and 99^{th} percentiles. Heteroscedasticity-consistent standard errors are reported in parentheses. *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively. Detailed variable definitions are in Appendix A of the paper.

Dependent variable:	CAR(0,1)	CAR(-1,1)	CAR(-2,2)
	(1)	(2)	(3)
CA HQ (d)	-0.54**	-0.58*	-1.06***
	(0.27)	(0.32)	(0.41)
ln(Total assets)	0.17**	0.22***	0.17*
	(0.07)	(0.08)	(0.10)
ROA	-1.32**	-0.98	-2.43***
	(0.52)	(0.65)	(0.89)
MTB	-0.03	-0.02	-0.04
	(0.02)	(0.02)	(0.03)
Constant	-1.06**	-1.41**	-0.95
	(0.48)	(0.57)	(0.69)
R^2	0.01	0.01	0.02
N	1,238	1,238	1,238

Table OA.3: Robustness tests: All non-California firms as control firms

This table reports robustness tests of the results shown in Table 3 in the paper. Specifically, it reports differences in abnormal returns around the announcement of the adoption of the gender quota in California between California-headquartered and non-California-headquartered control firms. Each column shows results from a pooled ordinary least squares regression of an abnormal return measure on a dummy variable set equal to one if a firm is headquartered in California (CA HQ (d)). Across columns, we vary the length of the event window. In our baseline regression shown in Table 3 of the paper, the matched control sample is constructed by drawing, for each California-headquartered sample firm, the three closest firms in terms of size that are active in the same two-digit SIC code industry. In this table, we use all non-California-headquartered firms as control firms that pass the sample selection criteria outlined in Section 2.1 of the paper. All abnormal return measures are winsorized at the 1^{st} and 99^{th} percentiles. Heteroscedasticity-consistent standard errors are reported in parentheses. *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively. Detailed variable definitions are in Appendix A of the paper.

Dependent variable:	CAR(0,1)	CAR(-1,1)	CAR(-2,2)
	(1)	(2)	(3)
CA HQ (d)	-0.78***	-0.76***	-1.07***
	(0.23)	(0.27)	(0.33)
ln(Total assets)	0.16***	0.21***	0.13**
	(0.04)	(0.05)	(0.06)
ROA	-0.64	-0.35	-1.94***
	(0.45)	(0.52)	(0.67)
MTB	-0.02**	-0.03**	-0.05***
	(0.01)	(0.01)	(0.02)
Constant	-2.29***	-2.58***	-1.36***
	(0.32)	(0.36)	(0.45)
R^2	0.02	0.02	0.02
N	2,454	2,454	2,454

Table OA.4: Robustness tests: Difference-in-differences estimations using daily abnormal returns

This table reports results from pooled ordinary least squares regressions of daily abnormal returns (ARs) on a dummy variable set equal to one if a firm is headquartered in California (CA HQ (d)), a dummy set equal to one for observations measured after the implementation of the quota (Post (d)), and an interaction term between these two variables. The regression reported in column (2) additionally includes financial controls while the regression reported in column (3) additionally includes firm fixed effects. The sample comprises four daily abnormal stock return observations per firm, two before and two after the introduction of the quota. Daily abnormal returns are computed as the observed return minus the predicted return from a market model regression estimated over a 250-day estimation window that ends on Friday, September 21. Daily abnormal returns and all financial ratios are winsorized at the 1^{st} and 99^{th} percentiles. The construction of the matched sample is described in detail in the caption of Table 1 of the paper. All regressions include an intercept, which is not shown for brevity. Standard errors, reported in parentheses, are clustered at the firm level. *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively. Detailed variable definitions are in Appendix A of the paper.

Dependent variable:	AR(t)		
	(1)	(2)	(3)
CA HQ (d) × Post (d)	-0.36**	-0.36**	-0.36*
	(0.18)	(0.18)	(0.20)
Post (d)	-0.63***	-0.63***	-0.63***
	(0.11)	(0.11)	(0.13)
CA HQ (d)	-0.02	-0.02	
	(0.11)	(0.12)	
ln(Total assets)		0.03	
		(0.02)	
ROA		0.00	
		(0.18)	
MTB		-0.00	
		(0.01)	
Firm FE	No	No	Yes
R^2	0.02	0.02	0.27
N	4,952	4,952	4,952
Firms	1,238	1,238	1,238

Table OA.5: Robustness tests: Excluding control firms with trading relations with California firms

This table reports robustness tests of the results shown in Table 3 in the paper. Specifically, it reports differences in abnormal returns around the announcement of the adoption of the gender quota in California between California-headquartered and non-California-headquartered control firms. Each column shows results from a pooled ordinary least squares regression of an abnormal return measure on a dummy variable set equal to one if a firm is headquartered in California (CA HQ (d)). As in our baseline regression shown in Table 3 of the paper, the matched control sample is constructed by drawing, for each California-headquartered sample firm, the three closest firms in terms of size that are active in the same two-digit SIC code industry. In Panel A, we drop matched control firms that disclose at least one of the treated California sample firms as principal customer in Compustat's Segments database (Cohen and Frazzini, 2008). In Panel B, we drop matched control firms that are headquartered in AZ, CO, ID, NM, NV, OR, UT, or WA. All abnormal return measures are winsorized at the 1st and 99th percentiles. Heteroscedasticity-consistent standard errors are reported in parentheses. *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively. Detailed variable definitions are in Appendix A of the paper.

Panel A: Excluding control firms that disclose a California-headquartered firm as principal customer

Dependent variable:	CAR(0,1)	CAR(-1,1)	CAR(-2,2)
	(1)	(2)	(3)
CA HQ (d)	-0.72***	-0.75**	-1.14***
	(0.28)	(0.33)	(0.41)
ln(Total assets)	0.09	0.14*	0.12
	(0.07)	(0.08)	(0.10)
ROA	-0.79	-0.44	-2.23**
	(0.53)	(0.66)	(0.90)
MTB	-0.03*	-0.03	-0.05
	(0.02)	(0.02)	(0.03)
Constant	-1.90***	-2.21***	-1.30*
	(0.51)	(0.59)	(0.70)
R^2	0.01	0.01	0.02
N	1,201	1,201	1,201

Panel B: Excluding control firms headquartered in AZ, CO, ID, NM, NV, OR, UT, or WA

Dependent variable:	CAR(0,1)	CAR(-1,1)	CAR(-2,2)
	(1)	(2)	(3)
CA HQ (d)	-0.70**	-0.77**	-1.23***
	(0.28)	(0.33)	(0.41)
ln(Total assets)	0.10	0.14*	0.12
	(0.07)	(0.08)	(0.10)
ROA	-0.78	-0.48	-2.36***
	(0.53)	(0.66)	(0.89)
MTB	-0.03	-0.03	-0.05
	(0.02)	(0.02)	(0.03)
Constant	-1.95***	-2.20***	-1.26*
	(0.50)	(0.58)	(0.70)
R^2	0.01	0.01	0.02
N	1,214	1,214	1,214

Table OA.6: Robustness tests: Accounting for the cross-sectional dependence of returns

This table reports cumulative abnormal stock returns for two portfolios, one comprising California-head-quartered firms (CA HQ (d) = 1) and the other comprising a sample of industry- and size-matched non-California-headquartered firms (CA HQ (d) = 0). The estimate for the daily abnormal return (AR) is obtained from estimating the following regression:

$$r_t = \alpha + ARd_t + \beta r_{wt} + \epsilon_t$$

where r_t is the daily equally-weighted portfolio return of all portfolio firms in excess of the 1-month U.S. treasury bill rate, d_t is a dummy variable set equal to one for observations in the event window and zero for observations in the estimation window, an r_{wt} is the daily value-weighted market index return in excess of the 1-month U.S. treasury bill rate. As a proxy for the market return, we use the return of a self-computed, value-weighted market index consisting of all sample firms. The regression is estimated over a sample that includes all observations from the 250-day estimation window that ends on September 21 and the event window. Estimates for the cumulative abnormal returns are obtained by multiplying the obtained coefficient for AR by the number of days in the event window. Differences in abnormal returns between California-headquartered and non-California-headquartered firms are obtained from estimating the regression above but with the dependent variable being the daily difference in portfolio returns of California-headquartered and non-California-headquartered firms. The construction of the matched sample is described in detail in the caption of Table 1 of the paper. *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively. Detailed variable definitions are in the Appendix of the paper.

	CA HQ (d) = 1			CA HQ (d) = 0			Differences	
	Mean	SE	N	Mean	SE	N	Mean	SE
CAR (0,1)	-2.13%***	0.34%	458	-1.42%**	0.28%	780	-0.70%**	0.17%
CAR (-1,1)	-2.10%**	0.28%	458	-1.38%**	0.23%	780	-0.72%*	0.14%
CAR (-2,2)	-1.59%	0.22%	458	-0.70%	0.18%	780	-0.89%*	0.11%

Table OA.7: Robustness tests: Alternative event dates

This table reports results from pooled ordinary least squares regressions of cumulative abnormal returns (CARs) on a dummy variable set equal to one if a firm is headquartered in California (CA HQ (d)) and a set of financial control variables. In column (1), CARs are estimated over a two-day event window that includes the day of the introduction of the law (January 3) and the day after. In column (2), CARs are estimated over a two-day event window that includes the day of the successful Senate vote (May 31) and the day after. In column (3), CARs are estimate over a three-day event window that includes the day of the Assembly vote (August 29), the day of the second Senate vote (August 30), and the day after. Daily abnormal returns are computed as the observed return minus the predicted return from a market model regression estimated over a 250-day estimation window that ends six days before the event. Cumulative abnormal returns and all financial ratios are winsorized at the 1st and 99th percentiles. The construction of the matched sample is described in detail in the caption of Table 1 of the paper. All regressions include an intercept, which is not shown for brevity. Heteroscedasticity-consistent standard errors are reported in parentheses. *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively. Detailed variable definitions are in Appendix A of the paper and of this Online Appendix.

Dependent variable:	CAR(Jan. 3, Jan. 4)	CAR(May 31, Jun. 1)	CAR(Aug. 29, Aug. 31)
Event(s):	Law introduced	Successful Senate vote	Successful Assembly vote and second Senate vote
	(1)	(2)	(3)
CA HQ (d)	0.19	0.36	0.40
	(0.26)	(0.22)	(0.29)
ln(Total assets)	-0.12	-0.19***	-0.28***
	(0.07)	(0.06)	(0.08)
ROA	-0.70	-0.11	-1.67***
	(0.54)	(0.49)	(0.55)
MTB	-0.01	0.02	-0.00
	(0.02)	(0.02)	(0.01)
R^2	0.01	0.02	0.05
N	1,194	1,232	1,237

Table OA.8: Female board representation around the quota's adoption
This table reports results from pooled ordinary least squares regressions of different board characteristics on a dummy variable set equal to one if a firm is headquartered in California (CA HQ (d)), month dummy variables, and interaction terms between the California-headquarter dummy variable and the month dummy variables. For each California-headquartered firm and the sample of control firms, we compute board characteristics for the end of September (the base month) as well as for the end of October 2018 to March 2019. In column (1), we use the fraction of directors on the board that are female as the dependent variable. In column (2), we restrict the sample to firms that require at least one additional female director to fulfill the quota at the quota's adoption date, and in column (3), we restrict the sample to firms that require at least two additional female directors to fulfill the quota at the quota's adoption date. In column (4), we use the sample from column (1) but replace the dependent variable with a dummy variable set equal to one if a firm at the end of a given month has no female director on the board. The construction of the matched sample is described in detail in the caption of Table 1 of the paper. All regressions include firm fixed effects and an intercept, which is not shown for brevity. Standard errors, reported in parentheses, are clustered at the firm level. *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively. Detailed variable definitions are in Appendix A of the paper.

Dependent variable:	Female directors (%)			No female (d)
Sample:	Full	# missing female directors > 0	# missing female directors > 1	Full
	(1)	(2)	(3)	(4)
CA HQ (d) × October-end (d)	0.26*	0.35**	0.48**	-0.02**
	(0.16)	(0.17)	(0.23)	(0.01)
CA HQ (d) × November-end (d)	0.39**	0.47**	0.51*	-0.02**
	(0.19)	(0.21)	(0.28)	(0.01)
CA HQ (d) × December-end (d)	0.51**	0.59**	0.67**	-0.03***
	(0.23)	(0.25)	(0.32)	(0.01)
CA HQ (d) × January-end (d)	0.76***	0.88***	1.10***	-0.05***
	(0.26)	(0.29)	(0.38)	(0.01)
CA HQ (d) × February-end (d)	0.77***	0.86***	1.15***	-0.05***
	(0.29)	(0.32)	(0.42)	(0.01)
CA HQ (d) × March-end (d)	0.97***	1.15***	1.48***	-0.06***
	(0.33)	(0.35)	(0.46)	(0.02)
October-end (d)	0.20**	0.19**	0.24**	-0.00
	(0.08)	(0.09)	(0.12)	(0.00)
November-end (d)	0.29***	0.32***	0.47***	-0.01
	(0.10)	(0.11)	(0.14)	(0.00)
December-end (d)	0.48***	0.53***	0.76***	-0.01*
	(0.13)	(0.14)	(0.18)	(0.01)
January-end (d)	0.73***	0.79***	1.15***	-0.02***
	(0.14)	(0.15)	(0.20)	(0.01)
February-end (d)	1.02***	1.14***	1.57***	-0.02***
	(0.17)	(0.18)	(0.23)	(0.01)
March-end (d)	1.19***	1.33***	1.77***	-0.03***
	(0.19)	(0.20)	(0.27)	(0.01)
Firm FE	Yes	Yes	Yes	Yes
R^2	0.05	0.06	0.09	0.03
N	8,559	7,472	4,945	8,559
Firms	1,238	1,079	712	1,238

Table OA.9: Frictions in the director labor market and quota announcement returns

This table reports robustness tests in which test for the effect of the frictions in the director labor market on announcement returns around the quota adoption in California. Specifically, we report results from pooled ordinary least squares regressions of two-day cumulative abnormal returns (CARs) on a dummy variable set equal to one if a firm is headquartered in California (CA HQ (d)), a proxy for the demand of female candidates, as well as interaction terms between the California-headquarter indicator variable and the female director demand proxy. The proxies for the demand for female directors are the same as in columns (2) to (9) of Table 4 in the paper. Daily abnormal returns are computed as the observed return minus the predicted return from a market model regression estimated over a 250-day estimation window that ends on Friday, September 21. Cumulative abnormal returns are winsorized at the 1^{st} and 99^{th} percentiles. The construction of the matched sample is described in detail in the caption of Table 1 of the paper. Heteroscedasticity-consistent standard errors are reported in parentheses. *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively. Detailed variable definitions are in Appendix A of the paper and of this Online Appendix.

Dependent variable:	CAR(0,1)							
Friction proxy =	Shortfall (%)	# missing female directors	# missing female directors (exp)	# missing female directors (2019)	# missing female directors (2021)	2021 requ. failed (d)	Female directors (%)	# female directors
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CA HQ (d)	0.33	0.36	0.42	-0.56*	0.49	1.66**	-1.30***	-1.39***
	(0.43)	(0.51)	(0.51)	(0.30)	(0.56)	(0.71)	(0.46)	(0.44)
CA HQ (d) × Friction proxy	-4.43**	-0.63**	-0.62**	-0.51	-0.86**	-2.68***	4.07*	0.57**
	(1.73)	(0.29)	(0.27)	(0.62)	(0.38)	(0.77)	(2.15)	(0.24)
Friction proxy	1.67	0.11	0.23	0.60	-0.02	0.75	-2.02	-0.24
	(1.16)	(0.18)	(0.17)	(0.40)	(0.23)	(0.46)	(1.34)	(0.16)
Constant	-2.33***	-1.97***	-2.35***	-2.30***	-1.73***	-2.57***	-1.69***	-1.69***
	(0.69)	(0.67)	(0.72)	(0.57)	(0.66)	(0.71)	(0.52)	(0.52)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.02	0.02	0.02	0.01	0.02	0.02	0.01	0.02
N	1,238	1,238	1,238	1,238	1,238	1,238	1,238	1,238

Table OA.10: Demand vs. supply of female directors

This table reports robustness tests in which we disentangle the effect of the demand and supply of female directors on announcement returns around the quota adoption in California. Specifically, the table report results from pooled ordinary least squares regressions of two-day cumulative abnormal returns (CARs) on a dummy variable set equal to one if a firm is headquartered in California (CA HQ (d)), the number of missing female directors scaled by board size as a proxy for the demand of female candidates, as well as interaction terms between the California-headquarter indicator variable and the female director demand proxy. In addition, in each column, we add a different measures that proxies for the local supply of female directors as well as interaction terms between the California-headquarter indicator variable and the female director supply proxy. Daily abnormal returns are computed as the observed return minus the predicted return from a market model regression estimated over a 250-day estimation window that ends on Friday, September 21. Cumulative abnormal returns are winsorized at the 1^{st} and 99^{th} percentiles. The construction of the matched sample is described in detail in the caption of Table 1 of the paper. Heteroscedasticity-consistent standard errors are reported in parentheses. *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively. Detailed variable definitions are in Appendix A of the paper and of this Online Appendix.

Dependent variable:	CAR(0,1)		
	(1)	(2)	(3)
CA HQ (d)	0.12	0.23	-2.62**
	(0.50)	(0.44)	(1.32)
CA HQ (d) × Shortfall (%)	-4.24**	-4.77***	-4.47***
	(1.75)	(1.72)	(1.73)
Shortfall (%)	1.62	1.74	1.69
	(1.17)	(1.16)	(1.16)
CA HQ (d) × Local female supply > median (d)	0.84		
	(0.58)		
Local female supply > median (d)	0.04		
	(0.33)		
CA HQ (d) × Border < 100 miles (d)		0.75	
		(0.57)	
Border < 100 miles (d)		-0.08	
		(0.25)	
CA HQ (d) × Airport < 50 miles (d)			3.08**
			(1.30)
Airport < 50 miles (d)			-0.53
			(0.44)
Constant	-2.26***	-2.41***	-1.86**
	(0.73)	(0.69)	(0.76)
Controls	Yes	Yes	Yes
R^2	0.02	0.02	0.02
N	1,238	1,238	1,238

Table OA.11: Determinants of policy sensitivity

This table reports results from pooled ordinary least squares regressions of a dummy indicating firm's that are vulnerable to future regulation on variables that capture the frictions a firm encounters when complying with the California board gender quota. The proxy for a firm's vulnerability to future regulation is a dummy variable set equal to one if a firm's stock returns in the 250-day estimation window depend significantly on changes of the daily Economic Policy Uncertainty index of Baker, Bloom, and Davis (2016), zero otherwise. Daily abnormal returns are computed as the observed return minus the predicted return from a market model regression estimated over a 250-day estimation window that ends on Friday, September 21. Cumulative abnormal returns are winsorized at the 1st and 99th percentiles. The construction of the matched sample is described in detail in the caption of Table 1. Heteroscedasticity-consistent standard errors are reported in parentheses. *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively. Detailed variable definitions are in Appendix A.

Dependent variable:	Policy sensitive firm (d)								
Friction proxy =	-	Shortfall (%)	# missing female directors	# missing female directors (exp)	# missing female directors (2019)	# missing female directors (2021)	2021 requ. failed (d)	Female directors (%)	# female directors
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Friction proxy		0.04	0.01	0.00	0.00	0.01	0.01	-0.06	0.00
		(0.07)	(0.01)	(0.01)	(0.02)	(0.01)	(0.03)	(0.07)	(0.01)
ln(Total assets)	-0.01**	-0.01**	-0.01**	-0.01**	-0.01***	-0.01**	-0.01***	-0.01**	-0.01**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
ROA	0.03	0.03	0.03	0.03	0.03	0.03	0.03	0.03	0.03
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
MTB	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Constant	0.18***	0.18***	0.17***	0.19***	0.20***	0.17***	0.19***	0.20***	0.20***
	(0.05)	(0.05)	(0.04)	(0.05)	(0.04)	(0.04)	(0.05)	(0.03)	(0.03)
R^2	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01
N	1,238	1,238	1,238	1,238	1,238	1,238	1,238	1,238	1,238

Table OA.12: Frictions vs. legislating non-economic values: Other friction proxies

This table reports robustness tests of Table 6 in the paper by alternating the variable that captures the frictions a firm encounters when complying with the California board gender quota across panels. Specifically, each panel reports results from pooled ordinary least squares regressions of two-day cumulative abnormal returns (CARs) on state-level variables that serve as a proxy for the likelihood that a firm headquartered in a state becomes subject to future legislation, labeled X, a proxy for a firm's vulnerability to future regulation, a proxy for the frictions a firm encounters when complying with the California board gender quota, as well as interaction terms between the future regulation likelihood proxy (X) and the proxy for a firm's vulnerability to future regulation and the friction proxy. The proxy for a firm's vulnerability to future regulation is a dummy variable set equal to one if a firm's stock returns in the 250-day estimation window depend significantly on changes of the daily Economic Policy Uncertainty Index of Baker, Bloom, and Davis (2016), zero otherwise. Across columns, we vary the future regulation likelihood proxy (X) as indicated above each column: In column (1), we use a dummy set equal to one for firms that are headquartered in states that are likely to introduce a board gender quota. In columns (2) and (3), we use dummy variables set equal to one for states that implemented similarly strict environmental laws as California, either by joining the United States Climate Alliance or by implementing comprehensive Greenhouse Gas (GhG) reduction policies. In column (4), we use state-level minimum wages. In column (5), we use a measure that quantifies the extent to which a state has followed California in legislative issues in the past proxied with the similarity in Cannabis legalization. In columns (6) and (7), we use two alternative regulatory scores with higher values indicating higher regulatory density. In column (8), we use the fraction of votes obtained by the Democratic Party in the 2016 Presidential Election. The sample comprises only non-California-headquartered firms. The construction of this sample is described in detail in the caption of Table 1 of the paper. Daily abnormal returns are computed as the observed return minus the predicted return from a market model regression estimated over a 250-day estimation window that ends on Friday, September 21. Cumulative abnormal returns are winsorized at the 1^{st} and 99^{th} percentiles. Each regression includes the control variables from Table 3 (ln(Total assets), ROA, and MTB). Heteroscedasticity-consistent standard errors are reported in parentheses. *, **, and ***, indicate statistical significance at the 10%, 5%, and 1% level, respectively. Detailed variable definitions are in Appendix A.

Panel A: # missing female directors

Dependent variable:	CAR(0,1)							
X =	Impending quota (d)	U.S. Climate Alliance (d)	GhG reduction policies (d)	Excess minimum wage ($)	Cannabis legalization score	Regulatory score (JL)	Regulatory score (Cato)	% votes Democrats
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
X	-0.28	-0.68	0.58	-0.24	-0.49*	-0.05	0.06	-0.22
	(0.62)	(0.60)	(0.60)	(0.15)	(0.25)	(0.22)	(0.22)	(3.66)
Policy sensitive firm (d)	0.79	1.14	1.20	1.25*	1.99*	1.05	2.66*	7.21**
	(0.63)	(0.73)	(0.78)	(0.66)	(1.20)	(1.37)	(1.48)	(3.02)
X × Policy sensitive firm (d)	-1.75*	-1.63*	-1.87**	-0.51**	-0.70*	-0.26	-0.68*	-13.81**
	(0.93)	(0.98)	(0.95)	(0.24)	(0.42)	(0.39)	(0.37)	(5.69)
# missing female directors	0.14	0.04	0.36	-0.06	-0.43	0.54	0.42	0.83
	(0.22)	(0.24)	(0.24)	(0.25)	(0.43)	(0.46)	(0.49)	(0.99)
X × # missing female directors	-0.22	-0.01	-0.63*	0.04	0.17	-0.14	-0.10	-1.54
	(0.35)	(0.33)	(0.34)	(0.09)	(0.14)	(0.13)	(0.13)	(1.97)
Constant	-1.56*	-1.27	-1.92**	-1.18	-0.29	-1.42	-1.88*	-1.59
	(0.85)	(0.88)	(0.86)	(0.90)	(1.11)	(1.08)	(1.07)	(1.92)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.02	0.02	0.02	0.02	0.02	0.02	0.01	0.02
N	780	780	780	756	780	778	778	780

Panel B: # missing female directors (exp)

Dependent variable:	CAR(0,1)							
X =	Impending quota (d)	U.S. Climate Alliance (d)	GhG reduction policies (d)	Excess minimum wage ($)	Cannabis legalization score	Regulatory score (JL)	Regulatory score (Cato)	% votes Democrats
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
X	-0.36	-0.81	0.34	-0.24*	-0.49*	-0.14	0.05	-0.47
	(0.62)	(0.60)	(0.59)	(0.15)	(0.25)	(0.22)	(0.22)	(3.45)
Policy sensitive firm (d)	0.80	1.13	1.22	1.23*	1.93	1.06	2.65*	7.15**
	(0.63)	(0.73)	(0.78)	(0.66)	(1.19)	(1.37)	(1.47)	(2.99)
X × Policy sensitive firm (d)	-1.77*	-1.62*	-1.96**	-0.50**	-0.68	-0.27	-0.68*	-13.72**
	(0.93)	(0.97)	(0.94)	(0.24)	(0.42)	(0.39)	(0.37)	(5.63)
# missing female directors	0.22	0.12	0.38*	0.06	-0.28	0.44	0.46	0.75
(exp)	(0.21)	(0.24)	(0.22)	(0.24)	(0.40)	(0.43)	(0.45)	(0.85)
X × # missing female	-0.15	0.08	-0.42	0.05	0.16	-0.08	-0.08	-1.17
directors (exp)	(0.33)	(0.31)	(0.32)	(0.08)	(0.13)	(0.12)	(0.12)	(1.68)
Constant	-1.94**	-1.62*	-2.22**	-1.58	-0.71	-1.56	-2.24**	-1.87
	(0.91)	(0.94)	(0.90)	(0.97)	(1.16)	(1.11)	(1.06)	(1.80)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.02	0.02	0.02	0.02	0.02	0.02	0.01	0.02
N	780	780	780	756	780	778	778	780

Panel C: # missing female directors (2019)

Dependent variable:	CAR(0,1)							
X =	Impending quota (d)	U.S. Climate Alliance (d)	GhG reduction policies (d)	Excess minimum wage ($)	Cannabis legalization score	Regulatory score (JL)	Regulatory score (Cato)	% votes Democrats
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
X	-0.58	-0.69*	-0.33	-0.18*	-0.27*	-0.23	-0.08	-2.84
	(0.41)	(0.39)	(0.38)	(0.10)	(0.16)	(0.15)	(0.15)	(2.39)
Policy sensitive firm (d)	0.80	1.13	1.27	1.20*	1.90	1.10	2.75*	7.30**
	(0.63)	(0.72)	(0.79)	(0.65)	(1.18)	(1.37)	(1.47)	(3.06)
X × Policy sensitive firm (d)	-1.80*	-1.63*	-2.04**	-0.50**	-0.68	-0.28	-0.71*	-14.03**
	(0.92)	(0.97)	(0.95)	(0.24)	(0.42)	(0.39)	(0.36)	(5.78)
# missing female directors (2019)	0.44	0.35	0.52	0.31	-0.15	1.10	0.57	-0.09
	(0.49)	(0.52)	(0.53)	(0.55)	(0.90)	(1.04)	(1.15)	(2.01)
X × # missing female directors (2019)	-0.09	0.07	-0.22	0.05	0.22	-0.19	-0.03	1.01
	(0.78)	(0.75)	(0.75)	(0.19)	(0.30)	(0.29)	(0.30)	(3.98)
Constant	-1.66**	-1.48**	-1.68**	-1.58**	-1.20	-1.05	-1.60*	-0.51
	(0.71)	(0.71)	(0.72)	(0.74)	(0.81)	(0.82)	(0.83)	(1.32)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.02	0.02	0.02	0.02	0.02	0.02	0.01	0.02
N	780	780	780	756	780	778	778	780

Panel D: # missing female directors (2021)

Dependent variable:	CAR(0,1)							
X =	Impending quota (d)	U.S. Climate Alliance (d)	GhG reduction policies (d)	Excess minimum wage ($)	Cannabis legalization score	Regulatory score (JL)	Regulatory score (Cato)	% votes Democrats
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
X	-0.21	-0.64	0.78	-0.23	-0.48*	-0.08	0.09	0.93
	(0.67)	(0.65)	(0.65)	(0.15)	(0.27)	(0.24)	(0.24)	(3.78)
Policy sensitive firm (d)	0.80	1.15	1.20	1.26*	2.00*	1.08	2.65*	7.27**
	(0.63)	(0.74)	(0.79)	(0.67)	(1.21)	(1.37)	(1.48)	(3.04)
X × Policy sensitive firm (d)	-1.73*	-1.63*	-1.86*	-0.51**	-0.70*	-0.27	-0.68*	-13.90**
	(0.94)	(0.98)	(0.95)	(0.24)	(0.42)	(0.39)	(0.37)	(5.73)
# missing female directors	0.06	-0.05	0.40	-0.19	-0.61	0.46	0.45	1.36
(2021)	(0.29)	(0.31)	(0.31)	(0.31)	(0.54)	(0.58)	(0.63)	(1.28)
X × # missing female	-0.32	-0.03	-0.92**	0.05	0.19	-0.16	-0.14	-2.85
directors (2021)	(0.47)	(0.44)	(0.45)	(0.11)	(0.18)	(0.17)	(0.17)	(2.60)
Constant	-1.31	-1.04	-1.80**	-0.94	-0.07	-1.07	-1.69	-1.89
	(0.83)	(0.86)	(0.84)	(0.86)	(1.10)	(1.10)	(1.09)	(1.92)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.02	0.02	0.02	0.02	0.02	0.02	0.01	0.02
N	780	780	780	756	780	778	778	780

Panel E: 2021 requ. failed (d)

Dependent variable:	CAR(0,1)							
X =	Impending quota (d)	U.S. Climate Alliance (d)	GhG reduction policies (d)	Excess minimum wage ($)	Cannabis legalization score	Regulatory score (JL)	Regulatory score (Cato)	% votes Democrats
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
X	-0.32	-0.92	0.53	-0.24	-0.46	-0.35	0.01	-0.18
	(0.86)	(0.81)	(0.81)	(0.19)	(0.34)	(0.29)	(0.29)	(4.71)
Policy sensitive firm (d)	0.81	1.13	1.25	1.19*	1.84	1.10	2.63*	7.25**
	(0.63)	(0.74)	(0.79)	(0.66)	(1.19)	(1.39)	(1.49)	(2.98)
X × Policy sensitive firm (d)	-1.79*	-1.58	-1.99**	-0.47**	-0.64	-0.28	-0.68*	-13.91**
	(0.93)	(0.98)	(0.95)	(0.24)	(0.42)	(0.39)	(0.37)	(5.62)
2021 requ. failed (d)	0.76	0.44	1.21**	0.44	-0.24	0.37	1.05	2.10
	(0.54)	(0.64)	(0.55)	(0.64)	(1.14)	(1.12)	(1.16)	(2.51)
X × 2021 requ. failed (d)	-0.34	0.28	-1.10	0.09	0.29	0.08	-0.12	-2.89
	(0.93)	(0.88)	(0.89)	(0.21)	(0.36)	(0.32)	(0.32)	(5.05)
Constant	-2.18**	-1.74*	-2.56***	-1.84*	-0.98	-1.06	-2.29*	-2.26
	(0.87)	(0.96)	(0.86)	(0.95)	(1.30)	(1.26)	(1.18)	(2.35)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.02	0.02	0.02	0.02	0.02	0.02	0.01	0.02
N	780	780	780	756	780	778	778	780

Panel F: Female directors (%)

Dependent variable:	CAR(0,1)							
X =	Impending quota (d)	U.S. Climate Alliance (d)	GhG reduction policies (d)	Excess minimum wage ($)	Cannabis legalization score	Regulatory score (JL)	Regulatory score (Cato)	% votes Democrats
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
X	-0.69	-0.42	-0.76	-0.09	-0.01	-0.25	-0.07	-2.56
	(0.58)	(0.56)	(0.57)	(0.14)	(0.23)	(0.21)	(0.22)	(2.98)
Policy sensitive firm (d)	0.79	1.14	1.22	1.23*	1.92	1.13	2.70*	7.19**
	(0.63)	(0.73)	(0.79)	(0.65)	(1.19)	(1.38)	(1.48)	(3.05)
X × Policy sensitive firm (d)	-1.80*	-1.65*	-1.97**	-0.50**	-0.68	-0.29	-0.70*	-13.83**
	(0.93)	(0.97)	(0.95)	(0.24)	(0.42)	(0.39)	(0.37)	(5.74)
Female directors (%)	-1.69	-0.56	-2.65	-0.31	2.29	-1.01	-1.25	-1.54
	(1.68)	(1.87)	(1.70)	(2.04)	(3.37)	(3.47)	(3.65)	(6.88)
X × Female directors (%)	0.56	-1.73	2.32	-0.46	-1.32	-0.18	-0.09	0.09
	(2.67)	(2.58)	(2.60)	(0.64)	(1.07)	(0.95)	(0.97)	(13.53)
Constant	-1.26*	-1.27*	-1.11*	-1.38**	-1.49*	-0.59	-1.22	-0.27
	(0.66)	(0.65)	(0.67)	(0.68)	(0.84)	(0.92)	(0.98)	(1.54)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.02	0.02	0.02	0.02	0.02	0.02	0.01	0.02
N	780	780	780	756	780	778	778	780

Panel G: # female directors

Dependent variable:	CAR(0,1)							
X =	Impending quota (d)	U.S. Climate Alliance (d)	GhG reduction policies (d)	Excess minimum wage ($)	Cannabis legalization score	Regulatory score (JL)	Regulatory score (Cato)	% votes Democrats
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
X	-0.65	-0.34	-0.66	-0.09	-0.05	-0.24	-0.07	-2.34
	(0.57)	(0.55)	(0.56)	(0.14)	(0.22)	(0.21)	(0.22)	(3.00)
Policy sensitive firm (d)	0.81	1.16	1.23	1.23*	1.90	1.11	2.69*	7.18**
	(0.63)	(0.74)	(0.79)	(0.66)	(1.19)	(1.38)	(1.48)	(3.05)
X × Policy sensitive firm (d)	-1.79*	-1.64*	-1.98**	-0.50**	-0.67	-0.29	-0.69*	-13.76**
	(0.93)	(0.98)	(0.95)	(0.24)	(0.42)	(0.39)	(0.37)	(5.76)
# female directors	-0.20	-0.05	-0.28	-0.06	0.19	-0.09	-0.15	-0.10
	(0.19)	(0.21)	(0.19)	(0.24)	(0.39)	(0.37)	(0.41)	(0.79)
X × # female directors	0.04	-0.26	0.20	-0.06	-0.13	-0.03	-0.01	-0.17
	(0.30)	(0.28)	(0.29)	(0.07)	(0.12)	(0.11)	(0.11)	(1.57)
Constant	-1.39**	-1.44**	-1.27*	-1.49**	-1.53*	-0.77	-1.36	-0.51
	(0.67)	(0.66)	(0.68)	(0.68)	(0.85)	(0.92)	(1.01)	(1.57)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.02	0.02	0.02	0.02	0.02	0.02	0.01	0.02
N	780	780	780	756	780	778	778	780

Appendix: Variable definitions

This table reports variable definitions and data sources of all variables used in this Online Appendix but not in the paper.

Variable	Definition	Source
Telecom (d)	Dummy variable equal to one if a firm is active in the two digit SIC code 48 (Communications) or is a member in the U.S. Telecom Trade Group and zero otherwise.	Compustat
Local female supply $>$ median (d)	Dummy variable equal to one if a firm's headquarter is located in an area with above sample median supply of female candidates. To compute the local supply of female candidates, we geocode each sample firm's ZIP code and count the number of female directors on the boards of all other sample firms within a 100-mile distance around the sample firm and scale this number by the number of other sample firms within the same distance. For firms that do not have another sample firm within a 100-mile distance around their headquarter, we set the local supply of female candidates to zero.	Compustat, BoardEx, ZIP code-latitude longitude table
Border $<$ 100 miles (d)	Dummy variable equal to one if a firm's headquarter is located within 100 miles from the boarder of CA and zero otherwise.	Compustat
Airport $<$ 50 miles (d)	Dummy variable equal to one if a firm's headquarter is located within 50 miles of the next major airport (Large or medium hubs, as defined by the Federal Aviation Administration (FFA) for the calendar year 2017 based on passenger enplanement) and zero otherwise.	Compustat, FAA