K V Lins, L Roth, H Servaes and A Tamayo
Sexism, culture, and firm value: evidence from the Harvey Weinstein scandal and the #MeToo movement
Article

Sexism, Culture, and Firm Value: Evidence from the Harvey Weinstein Scandal and the #MeToo Movement

KARL V. LINS,* LUKAS ROTH,[†] HENRI SERVAES,[‡] AND ANE TAMAYO[§]

ABSTRACT

During the revelation of the Harvey Weinstein scandal and the reemergence of the #MeToo movement, firms with a nonsexist corporate culture, proxied

*University of Utah; [†]University of Alberta and ECGI; [‡]London Business School, CEPR, and ECGI; [§]London School of Economics and CEPR

1

by having women among the five highest-paid executives, earn excess returns of 1.3% relative to firms without female top executives. These returns are driven by changes in investor preferences toward firms with a nonsexist culture. Institutional ownership increases in firms with a nonsexist culture after the Weinstein/#MeToo events, particularly for investors with larger holdings and investors with a lower ESG focus ex ante. Firms without female top executives improve gender diversity after these events, particularly in more sexist states and in industries with few women executives. Our evidence attests to the value of having a nonsexist corporate culture and indicates that changes in societal norms toward women are permeating into capital markets and corporations.

JEL codes: M14, J16, G12, G30

Keywords: culture; sexism; gender equality; #MeToo; valuation; returns; investor preferences; institutional ownership; ESG

1. Introduction

In this paper, we assess the valuation effect of an important aspect of corporate culture: gender equality. Gender equality is at the forefront of sustainable economic development (see, e.g., the UN Sustainable Development Goal #5: "To achieve gender equality and empower all women and girls"), yet the extent to which the market values a gender-equal, nonsexist corporate culture remains largely unexplored.[1] Our goal is to shed light on this matter by determining whether and, importantly, why investors respond to changes in societal attitudes toward women.

To explore this question, we exploit the public revelation of the numerous sexual harassment allegations against Harvey Weinstein and the subsequent resurgence of the #MeToo movement. Together, these events rapidly brought to light the extent to which sexual harassment and gender discrimination were prevalent in business, while elucidating that such egregious behavior would no longer be condoned. Our premise is that as a result of this shock, investors and society as a whole re-evaluated the importance of having a nonsexist culture,[2] potentially shifting investors' nonmonetary preferences toward nonsexist firms and leading to meaningful price effects. By leveraging these events, we are able to study the economically important, yet empirically elusive, role of nonmonetary preferences in asset pricing and capital allocation (Pastor, Stambaugh, and Taylor [2021, 2022]; Pedersen, Fitzgibbons, and Pomorski [2021]).

---

[1] The Council of Europe defines sexism as "any act, gesture, visual representation, spoken or written words, practice, or behavior based upon the idea that a person or a group of persons is inferior because of their sex, which occurs in the public or private sphere, whether online or offline." As such, sexism encompasses different degrees of severity—from inappropriate language and remarks to sexual harassment and assault.

[2] We use survey data from the General Social Survey and the World Values Survey to substantiate this claim (see section 3.1 for further details).

Our identification strategy is related to that of Billings, Klein, and Shi [2022], who also study whether the market values a nonsexist corporate culture using the same shock. Billings, Klein, and Shi [2022] document superior returns for firms with at least three female directors around a series of #MeToo-related news events, and negative returns for firms with no female directors. They do not investigate what drives these return differentials, however. In contrast, we conduct an analysis of the mechanisms behind the market reaction, which allows us to determine whether social norms regarding sexism do indeed shape investor preferences and, hence, corporate valuation. Furthermore, our analyses also seek to identify whether these preferences are monetary or nonmonetary.

To capture a nonsexist corporate culture, we build on the O'Reilly and Chatman [1996] definition of corporate culture as "a set of norms and values that are widely shared and strongly held throughout the organization."[3] We posit that the way in which female employees are treated in an organization reflects such norms and values, and argue that the extent to which women have significant leadership positions inside the firm is a strong signal of a nonsexist culture. A firm that has women among its top executives is less likely to have a culture that tolerates sexism[4]; if such a culture were present, it is improbable that a woman would have risen to the top in the first place given the well-documented "glass ceiling."[5] Consistent with this view, survey evidence by the Rockefeller Foundation and GlobalStrategy-Group [2017] shows that one of the main hurdles to women achieving top leadership positions is the culture of the corporation itself, particularly the so-called "boys club" attitude in the workplace. Moreover, having a woman in the firm's C-suite increases equality in the organization by reducing the gender pay gap (Tate and Yang [2015], Kunze and Miller [2017], Dong [2022]). Similarly, the pivotal role of female leadership in building a culture of gender equality has been highlighted in a study by the World Economic Forum [2017] on attitudes toward women in the workplace. The title of the press release accompanying the study succinctly summarizes its conclusion: "The key to closing the gender gap? Putting more women in charge."

We start our empirical analysis by examining the stock price response over various time periods surrounding the Weinstein allegations and the subsequent rise of the #MeToo movement for U.S. firms covered by the Execucomp database. To capture the presence of women in significant leadership positions, we focus on whether one or more of a firm's five highest-paid

---

[3] The O'Reilly and Chatman [1996] definition of culture is also adopted by Guiso, Sapienza, and Zingales [2015a] and Graham, Grennan, Harvey, and Rajgopal [2022].

[4] We do not contend that all firms lacking top female executives have a sexist corporate culture (or vice versa); rather our proxy is a *relative* metric—on average, firms with women at the top are *less likely* to have a sexist corporate culture.

[5] Although several factors help in explaining the glass ceiling, there is a residual that is unaccounted for and as argued by Bertrand [2018] "sexism should be high on the list to name that residual" (p. 228).

executives is a woman. We find that, relative to firms without female top executives, companies with at least one woman among their five highest-paid executives earned excess returns of 1.3% during the revelation of the Weinstein scandal and the reemergence of the #MeToo movement. This return differential is largely driven by negative excess returns accruing to firms without female top executives.

Motivated by the literature on gender diversity at the board level and the work of Billings, Klein, and Shi [2022], we also explore whether female leadership is required for this valuation effect to obtain or whether the presence of women on the board suffices. We find that the effect of female directors is fully subsumed by the presence of female leadership. Thus, in our setting, the market values the presence of women in top corporate leadership roles more than their presence on the board of directors. This finding supports the premise that corporate culture is largely driven by C-suite executives (e.g., Deloitte [2016], World Economic Forum [2017], Graham et al. [2022]).

Next, we turn to the drivers of the return evidence that we document, which is crucial for understanding whether investors are simply repricing sexual misconduct risk or, as we argue, social norms regarding sexism are shaping investor preferences. The notion that changes in investors' ESG preferences can affect the pricing of stocks stems from the asset pricing models of Pastor, Stambaugh, and Taylor [2021] and Pedersen, Fitzgibbons, and Pomorski [2021]. Pastor, Stambaugh, and Taylor [2021], in particular, argue that sustainable assets have lower expected returns (and, hence, a lower cost of equity) in equilibrium partly because investors enjoy holding them. Sustainable assets nevertheless outperform when the ESG factor, which captures investors' tastes for sustainable holdings, receives a positive shock. We argue that the Weinstein/#MeToo event is exactly this type of shock and, thus, expect the associated changes in social norms to shift investor preferences toward companies with a nonsexist culture, resulting in meaningful price effects.

To test whether the investor preference mechanism is indeed driving the positive excess stock returns, we focus on institutional investors because these are sophisticated investors who own and vote the bulk of the world's capital and because information on their portfolio holdings is available on a quarterly basis. Furthermore, several recent studies have highlighted the importance of institutional investors in driving firms' ESG performance (e.g., Dyck et al. [2019], Krueger, Sautner, and Starks [2020], Stroebel and Wurgler [2021]).[6] We first examine changes in institutional investor demand for firms with and without top female executives after the

---

[6] For example, in Stroebel and Wurgler [2021]'s survey of academics, professionals, and public sector regulators, 52% and 48% of the respondents viewed carbon taxes and institutional investors, respectively, as the most important forces for reducing firms' climate risk exposures and/or carbon footprints. Only 5% of the respondents thought individual investors were among the most important factors.

Weinstein/#MeToo events and find larger increases in institutional ownership for firms with female leaders. This result holds for institutional ownership in aggregate, as well as at the individual institutional investor level where we can control for time-varying investor-level determinants of portfolio composition. We further find that the increase in ownership is more pronounced for investors who had less of a preference for sustainable assets (as measured by their portfolio ESG scores) prior to the events. These are exactly the types of investors whose preferences we would expect to change the most; because high-ESG investors already displayed a preference toward sustainable assets, changes in societal attitudes surrounding the Weinstein/#MeToo events are less likely to elicit changes in their preferences.

Second, we analyze whether these changes in portfolio composition translate into actual increases in the ESG scores of the investors' portfolios and find this to be the case: investors with low-ESG preferences exhibit the highest improvements in their portfolios' ESG scores after the events. Moreover, we show that this result is not just a consequence of improvements in the ESG scores of the firms they already held, but due to active rebalancing of their holdings toward higher ESG stocks.

Third, we explore whether, in response to the reduced demand from institutional investors or in anticipation of further reductions, firms with low gender equality cater to investor preferences by increasing gender diversity after the Weinstein and #MeToo events (see Pastor, Stambaugh, and Taylor [2021]). Using a variety of metrics, we document larger improvements in gender diversity in firms without female leaders. Additionally, we find increases in gender diversity in firms operating in industries with fewer women in executive positions and in more sexist states, which suggests that shortages in the labor market for female executives and/or unwillingness of women to work in these industries or states are not the main driver of the return patterns we document.

We also examine several other alternative explanations for the stock price reaction we document. First, we investigate whether our findings could be due to increased litigation risk for firms with a more sexist corporate culture. We find this not to be the case for four reasons: (1) despite a significant increase in sexual harassment revelations after the start of #MeToo (see Borelli-Kjaer, Schack, and Nielsson [2021]), we find few cases of actual litigation; (2) post-Weinstein/#MeToo, we find no evidence of changes in bond yield spreads for firms with no female top executives relative to other firms, thereby ruling out risk-based explanations for the valuation changes; (3) the changes in equity ownership are substantially larger for long-term investors than for transient investors; a risk-based explanation would apply to all investor types; and (4) the effect we document is simply too large to be explained by litigation. We note that, despite these four arguments against a litigation risk explanation, it is possible that *long-term* equity investors expected litigation problems that did not materialize.

Second, we investigate whether our findings could stem from a belief that other stakeholders will reward firms with greater gender diversity (e.g., by buying their products). According to this explanation, investors would also increase their demand for firms with female leadership, but the preferences would be monetary. We find no evidence of changes in operating performance for firms with female leadership relative to other firms in the two years after the events, indicating that the driver of excess returns is a shift in investors' tastes for nonsexist companies rather than customers' tastes for products produced by them. We recognize, however, that actual improvements in operating performance may take longer to materialize (Grennan [2019]).

Third, we explore whether our results are driven by undervaluation. In particular, firms with fewer female top executives may have refrained from investing resources to improve gender diversity prior to the Weinstein and #MeToo events if firms with a nonsexist corporate culture were undervalued. To investigate this possibility, we study whether investors underreact more to earnings news of firms with top female executives prior to the Weinstein event, and whether this underreaction gets corrected after the events. We find no evidence in support of this conjecture.

In sum, we show that the revelation of the extent to which sexism was prevalent in corporations elicited investor reactions and changes in firm policies. Low-ESG institutional investors actively rebalanced their portfolios toward firms with female leaders, leading to meaningful price effects. In the two years after the events, firms with female leaders did not exhibit increases in operating performance or decreases in bond yield spreads relative to firms without female leaders. *Holistically*, these findings are most consistent with changes in investors' nonmonetary preferences and indicate that societal attitudes toward women are permeating into capital markets and corporations. However, we acknowledge that it is difficult to exclude the possibility that investors changed their positions under the belief that diversity would have a financial impact. This belief could either be mistaken or the financial impact could materialize only in the long run, and not be captured by our tests. As such, both nonmonetary and monetary factors could explain the stock returns that we document.

Our paper makes several contributions. First, we examine in detail the *mechanisms* behind the revaluation of firms with a less sexist corporate culture, thereby advancing our knowledge of how, and why, societal attitudes about sexism impact capital markets. As discussed previously, Billings, Klein, and Shi [2022] find that firms with more female directors earned higher returns around a series of #MeToo-related news events, but they do not analyze the source of these returns. Our results are most consistent with changes in investors' nonmonetary preferences driving higher returns. As such, we provide validation for the models of Pastor, Stambaugh, and Taylor [2021] and Pedersen, Fitzgibbons, and Pomorski [2021], in which investors' tastes for ESG stocks is one prominent channel that can impact valuation. We recognize, however, that in other settings risk- and/or cash

flow-based explanations (which these models also allow for) could contribute to improved valuations of firms with a nonsexist corporate culture. For instance, in private equity, Calder-Wang and Gompers [2021] find that hiring more female partners improves deal flow and financial performance. Our findings also highlight the importance of public information dissemination in altering investor preferences and driving their responses to ESG issues.[7]

Second, our work adds to the literature assessing the impact of female leadership in corporations by documenting its valuation and performance effects. Prior studies show that women in leadership positions: (1) create corporate cultures with greater gender equality (e.g., as captured by reductions in the wage gap, see Tate and Yang [2015], Kunze and Miller [2017], and Dong [2022]); (2) display more conservative investing and financing policies (Huang and Kisgen [2013], Faccio, Marchica, and Mura [2016]); and (3) experience fewer operations-related lawsuits (Adhikari, Agrawal, and Malm [2019]). We contribute to this literature by showing that female leadership has a positive impact on shareholder value when investors reassess the salience of having a nonsexist corporate culture.[8] Further, this impact stems from investors' nonmonetary preferences, and not from differences in expected cash flows, which enhances our understanding of the way in which investors place value on female leadership. In addition, the fact that subsequent increases in diversity at the executive level *do not* have a negative impact on corporate performance contravenes the argument that women are underrepresented in executive leadership positions due to labor market shortages of qualified women (Bertrand, Goldin, and Katz [2010]).

Third, we contribute to the literature on the value of corporate culture in general (e.g., Guiso, Sapienza, and Zingales [2015a], Grennan [2019], Graham et al. [2022]) by attributing valuation effects to an increasingly relevant aspect of culture: the extent to which gender equality exists in firms and society. Academic evidence on the value of a "good" corporate culture itself is inconclusive, largely due to difficulties in defining and measuring culture and in attributing causality. By focusing on an unequivocal shock to sexism, we move closer to identifying the causal effects of corporate culture (see Guiso, Sapienza, and Zingales [2015a]). Furthermore, we show that corporate culture can also add value through investors' nonmonetary preferences without necessarily affecting short-term cash flows. Finally, the fact that the effects we uncover stem mainly from female leadership inside the

---

[7] See Christensen, Hail, and Leuz [2021] for a discussion of investor preferences and the real effects of CSR reporting.

[8] Recent evidence also indicates that policies aimed at attracting more women to the workplace in general either through maternity benefits (Liu, Makridis, Ouimet, and Simintzi (2023)) or state-level Paid Family Leave Acts (Bennett et al. [2021]) can be value enhancing.

firm, and not from female board membership,[9] suggests that in order to improve the culture of the corporation, additional focus should be placed on factors that facilitate women obtaining top-executive positions and not just positions at the board level.

2. Data

Under SEC regulations, companies are required to disclose detailed information regarding the remuneration of the CEO, the CFO, and the three other most highly paid officers. We gather these data for the most recent fiscal year prior to October 1, 2017, from the Execucomp database, which covers the S&P 1500 firms.[10] We drop executives for whom Execucomp's 'rank' variable is missing. We also drop firms for which Execucomp reports fewer than five top-compensated executives per firm. To capture the extent of gender equality within a firm, we compute the fraction of these executives that are women (*Fraction Top-5 Women*) and create a dummy variable that is equal to one if at least one woman is among the highest-paid executives (*Indicator Top-5 Women*), and zero otherwise. Finally, we combine these data with daily stock returns from the CRSP database for the three-month period starting in September 2017, more than one month before the first allegations against Harvey Weinstein were made. After dropping firms with missing return data (due to delisting as a result of mergers, going private transactions, etc.), we obtain a sample of 1,436 firms.[11]

Table 1 contains summary statistics on the firms in our sample. More than 60% of the firms have no women among the highest-paid executives, and less than 10% of the top-5 executives in our sample are women. In firms with at least one female executive, women comprise 25.1% of the top-5 executives, indicating that most of these firms have just one woman among its leaders. Furthermore, in our sample, only 5% of the sample firms have a female CEO.

Table 1 also contains summary statistics on our sample firms' financial characteristics, measured at the end of the most recent fiscal year prior to October 1, 2017. Firms with at least one female executive are broadly similar to those with no female executives in terms of size, cash holdings, Tobin's *q*, and average investments. However, they have higher profitability.

For our sample firms, we also gather data on board composition from BoardEx, based on the most recent proxy statements filed before October

---

[9] See, for example, Adams and Ferreira [2009], Adams and Funk [2012], Ahern and Dittmar [2012], Kim and Starks [2016], Bertrand et al. [2019], and Billings, Klein, and Shi [2022], for articles investigating the role of women on the board.

[10] We employ the most recent version of the Execucomp database (January 2024) because the gender of some executives is misclassified in older versions (those prior to 2020). We verify that the gender classification is correct by comparing the gender to the executive's name prefix and to the actual name, and, when in doubt, the profile of the executive.

[11] Including delisted firms until the delisting date has no material effect on our results.

TABLE 1
Summary Statistics

	Full Sample (N = 1,436)			At Least One Female Executive (N = 557)			No Female Executives (N = 879)			Test of Differences (p-values)	
	Mean (1)	Median (2)	SD (3)	Mean (4)	Median (5)	SD (6)	Mean (7)	Median (8)	SD (9)	Mean (10)	Median (11)
Fraction Top-5 Women	0.097	0.000	0.139	0.251	0.200	0.107	0.000	0.000	0.000		
Indicator Top-5 Women	0.388	0.000	0.487	1.000	1.000	0.000	0.000	0.000	0.000		
Female CEO	0.050	0.000	0.218	0.129	0.000	0.336	0.000	0.000	0.000		
Fraction Board Women	0.172	0.167	0.110	0.208	0.200	0.123	0.149	0.143	0.096	(<0.01)	(<0.01)
Log (Total Assets)	8.399	8.309	1.703	8.393	8.267	1.758	8.403	8.341	1.669	(0.92)	(0.70)
Cash	0.127	0.075	0.144	0.129	0.079	0.146	0.125	0.074	0.143	(0.61)	(0.84)
Leverage	0.291	0.273	0.235	0.287	0.271	0.240	0.293	0.276	0.233	(0.60)	(0.53)
Tobin's q	1.967	1.598	1.268	1.998	1.594	1.405	1.947	1.601	1.173	(0.48)	(0.62)
Investment	0.037	0.025	0.044	0.037	0.028	0.036	0.036	0.022	0.048	(0.53)	(<0.01)
Profitability	0.115	0.110	0.113	0.127	0.117	0.102	0.108	0.106	0.119	(<0.01)	(<0.01)

Fraction Top-5 Women is the fraction of female executives among the five highest-paid executives of the company. *Indicator Top-5 Women* is a dummy variable that equals one if a firm has at least one female executive among the five highest-paid executives, and zero otherwise. *Female CEO* is a dummy variable that equals one if the CEO is a woman, and zero otherwise. These data are from Execucomp. We drop executives for which Execucomp's "rank" variable is missing. We drop firms for which Execucomp reports fewer than five top executives per firm. *Fraction Board Women* is the fraction of female directors on the firm's board. The data are from BoardEx. *Log (Total Assets)* is the logarithm of total assets. *Cash* is cash and cash equivalents divided by total assets. *Leverage* is the sum of short- and long-term debt divided by total assets. *Tobin's q* is calculated as (total assets − book value of equity + market value of equity) / total assets. *Investment* is capital expenditures divided by total assets. *Profitability* is profit from operations divided by total assets. These data are from Compustat and the variables are measured at the end of the most recent fiscal year prior to October 1, 2017. The last two columns show *p*-values of mean comparison tests (using a *t*-test) and median comparison tests (using a Wilcoxon rank-sum test) between the two subsamples.

1, 2017. As we do for the highest-paid executives, we compute the fraction of board members that are women (*Fraction Board Women*). Across our sample, 17% of all board members are women and 87% of all firms have at least one woman on the board (untabulated). Compared with the statistics for top female executives, these numbers show that a woman is three times more likely to be on a corporate board than in the top-5 executive team. Firms with female executives have a higher fraction of women on the board (20.8%) than firms without female executives (14.9%).

3. Gender (in)Equality and Firm Value

3.1 FEMALE LEADERSHIP: FIRM-LEVEL RESULTS

We start by studying whether firms with female leadership, our proxy for having a nonsexist corporate culture, earned higher stock returns during the two days in which the Harvey Weinstein sexual assaults were first widely reported in the media, on October 5 and 6, 2017, and during the start of the #MeToo movement.[12] Although further allegations were made against Weinstein in the weeks after October 6, the notion that harassment in the workplace could be pervasive and systematic gained strong momentum on October 15, 2017, when actress Alyssa Milano encouraged spreading the hashtag #MeToo in an attempt to draw attention to the widespread occurrence of sexual assault and harassment. In the subsequent days, Google searches for the terms "#MeToo" and "sexual harassment in the workplace" hit an all-time high, and other prominent leaders in business and society were accused of sexual misconduct in the workplace.[13]

To assess whether firms with female leadership earned excess returns around these events, we estimate the following panel regression of raw

---

[12] Using Factiva, we verify that there are no news stories in any of the major media outlets covering the terms "Harvey Weinstein" and either "harassment" or "assault" over the period from September 1, 2017, through October 4, 2017. On October 5, 2017, there were 72 stories and on October 6, 2017, there were 144, indicating that these two trading days are key to identifying the stock price response to the Weinstein announcement.

[13] To illustrate that the #MeToo movement did indeed change societal attitudes toward women, we use survey data. For instance, the General Social Survey reports on agreement/disagreement with the following question: "It is much better for everyone involved if the man is the achiever outside the home and the woman takes care of the home and family." Over the period 2014–2016, 29% of respondents agreed or strongly agreed with this statement versus 22% over the period 2018–2021. The difference is highly significant, and there is no pre-trend when we look at changes from 2012 to 2014. The World Values Survey asks whether: "An essential characteristic of democracy is that women have the same rights as men." The response was on a 10-point scale, but we focus on the percentage of the respondents that rank this as 10, indicating that it is absolutely essential. In the 2010–2014 survey wave, this percentage was 51.5%, while in 2017–2022 it was 56.8%. The difference is again highly significant and not a continuation of a trend.

daily stock returns over the three-month period from September 1, 2017, through November 30, 2017:

$$R_{i,t} = \alpha_i + \beta_t + \gamma_1 \text{Female}_i \times \text{Weinstein Event}_t + \gamma_2 \text{Female}_i \times \text{\#MeToo Event}_t + \varepsilon_{i,t}, \quad (1)$$

where *Female$_i$* is one of our two female leadership variables (*Fraction Top-5 Women* or *Indicator Top-5 Women*); *Weinstein Event$_t$* is a time dummy equal to one on October 5 and 6, 2017, and zero otherwise; and *#MeToo Event$_t$* is a time dummy equal to one for the two-week event window starting on October 16 (the first trading day after the #MeToo tweet) and ending on October 27, and zero otherwise. As shown in figure 1, the number of news stories on Factiva mentioning variations of the term "#MeToo" drops markedly after this date; hence, we assume that investors would have incorporated the information on the severity of the problem by then.[14] The model is estimated with firm (α_i) and time (day) (β_t) fixed effects, and the standard errors are double clustered by firm and time. The firm fixed effects control for all time-invariant firm characteristics. Thus, by keeping the estimation period relatively short and including firm fixed effects, we alleviate the need to include controls for factor loadings, firm financials, and the female leadership proxies themselves. Our coefficients of interest are the interaction terms of the female leadership proxies and the event dummies (γ_1 and γ_2), which measure the change in the stock market's assessment of the importance of having a nonsexist culture.

Models 1 and 2 of panel A of table 2 contain the results. In model 1, we use the interactions of the event dummies with *Fraction Top-5 Women* as the explanatory variables, while in model 2, we use the interactions with *Indicator Top-5 Women*. The coefficient estimates are positive and statistically significant for three out of the four interactions, indicating that, when the Weinstein scandal and #MeToo movement unfolded, firms with female top executives earned excess returns relative to firms without women among their highest-paid executives.

To assess whether the female leadership effect on returns persists or is temporary in nature (and reverses in subsequent weeks), we further augment equation (1) by interacting the female leadership proxies with a dummy variable for the period in between the Weinstein and #MeToo event windows (October 9–13, 2017) and the one-month period after the #MeToo event window (October 30 to November 30, 2017). Models 3 and 4 of panel A of table 2 display these results. We find no evidence of return reversals in the week after the Weinstein announcement or for the month after the #MeToo event window, while the statistical and economic significance of the main findings increases relative to models 1 and 2. In terms of economic significance, the coefficient estimates in model 4 imply that firms with one or more top-5–compensated female executives earned an excess

---

[14] When we split this two-week event window into two separate one-week windows, we find positive and significant stock price effects in both weeks.
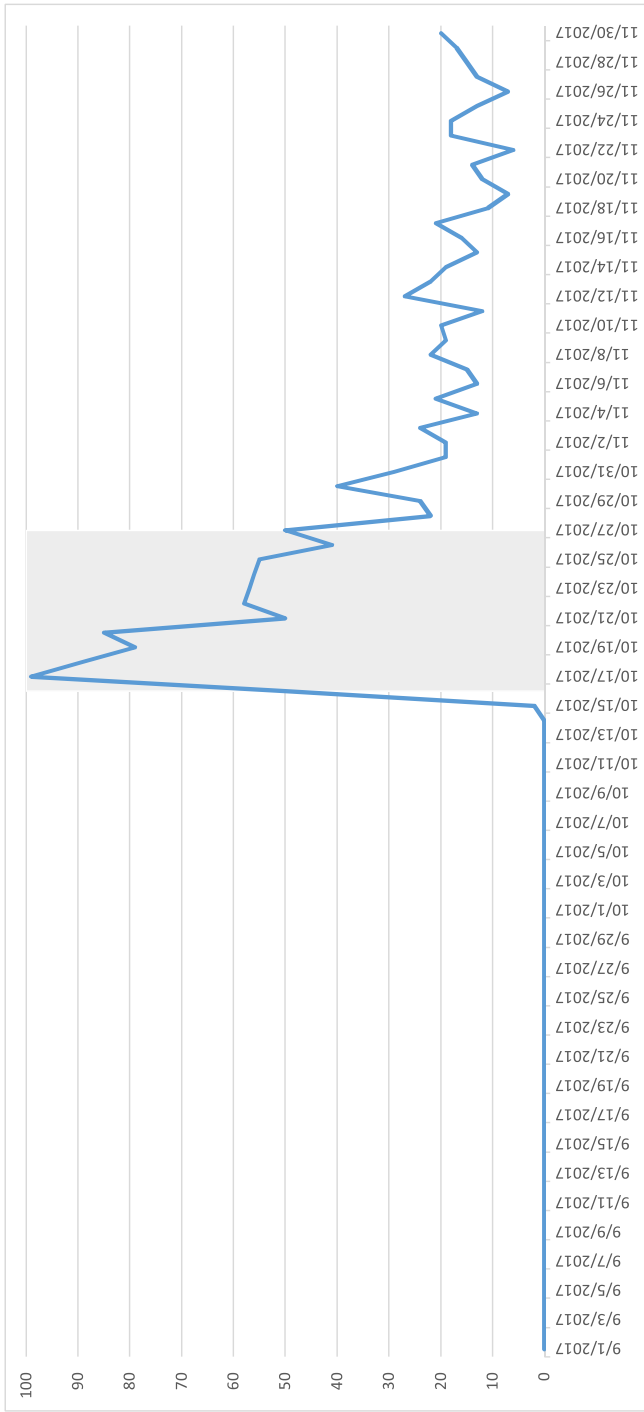
Fig. 1.—#MeToo Press Frequency. This figure shows the number of daily press articles mentioning the terms: Me Too, MeToo, #Me Too, and #MeToo in the Factiva database over our sample period. We read each article or heading to verify that it is indeed related to the #MeToo movement. The shaded area contains the dates we include in our #MeToo event window.

TABLE 2

Shareholder Value and Female Leadership

Panel A: Regression Results

Female Variable =	Daily Stock Returns			
	Fraction Top-5 Women (1)	Indicator Top-5 Women (2)	Fraction Top-5 Women (3)	Indicator Top-5 Women (4)
Female Variable ×				
Oct 5–6	0.638***	0.148***	0.751***	0.184***
	(<0.01)	(<0.01)	(<0.01)	(<0.01)
Oct 9–13			−0.185	−0.004
			(0.46)	(0.94)
Oct 16–27	0.273**	0.055	0.386***	0.091**
	(0.04)	(0.15)	(0.01)	(0.03)
Oct 30–Nov 30			0.292	0.080*
			(0.13)	(0.09)
Firm Fixed Effects	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes
N	90,468	90,468	90,468	90,468
Adjusted R^2	0.052	0.052	0.052	0.052

Panel B: Cumulative Abnormal Returns

	Average CARs		
	Weinstein (October 5–6) (1)	#MeToo (October 16–27) (2)	Weinstein and #MeToo (October 5–6 and 16–27) (3)
With Top-5 Women	−0.104	0.105	0.002
	(0.33)	(0.71)	(1.00)
Without Top-5 Women	−0.422***	−0.404**	−0.826***
	(<0.01)	(0.05)	(<0.01)
Difference	0.318***	0.509	0.828**
	(0.01)	(0.14)	(0.03)

This table provides stock price reaction results around the Weinstein and #MeToo events for firms with and without top-5 female executives. Panel A shows regression estimates of daily stock returns on interaction terms of female × event and firm and time fixed effects. The female variables are *Fraction Top-5 Women*, which is the fraction of female executives among the five highest-paid executives of the company; and *Indicator Top-5 Women*, which is a dummy variable that equals one if a firm has at least one female executive among the five highest-paid executives, and zero otherwise. The event variables (e.g., Oct 5–6) are dummy variables that equal one for all trading days during a specific event window, and zero otherwise. The female variables are measured at the end of the most recent fiscal year prior to October 1, 2017. The sample period is September 1, 2017, to November 30, 2017. Firms with missing returns during the sample period are dropped. Standard errors are double clustered by firm and time (trading day). Panel B shows average cumulative abnormal returns around the Weinstein and #MeToo events for firms with and without top-5 female executives. Abnormal returns are computed as market-model residuals. The model parameters are estimated using daily returns over the period from September 2016 to August 2017. The data are from Execucomp and CRSP. *p*-values are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

return of 0.37% on October 5 and 6 (calculated as: 0.184 × 2 days) and an additional 0.91% during the 10 trading days starting on October 16 (calculated as: 0.091 × 10 days).

The regressions reported in panel A of table 2 employ the firms' raw returns as the dependent variable and include firm and time fixed effects. Thus, we are comparing the firms' returns during the various event windows to the firms' returns outside of the event windows, after adjusting for market movements, thereby implicitly assuming that returns outside of the event window are "normal." To ensure that our findings are robust to alternative methods of computing abnormal returns, we employ two variations to the above methodology. First, we replace the raw returns with market-model–adjusted returns, where the market model is estimated using daily returns over the period September 1, 2016, through August 31, 2017, with the CRSP value–weighted index as the market proxy. This approach ensures that our findings are not due to firms with (without) female leadership possibly experiencing abnormally low (high) returns outside the event windows. Second, in our base-case model, we include an interaction term between the firm fixed effect and the market return. This approach accounts for differences across firms' sensitivities to market movements during the estimation period. Both alternative approaches yield results that are economically and statistically very similar to our base-case specifications (see table OA-1 of the online appendix).

Finally, to ensure that our findings are robust to alternative research designs, we calculate cumulative abnormal returns around the event windows based on the market model and find similar results. This alternative methodology also allows us to compute the effects for firms with and without female leadership separately. The results are displayed in panel B of table 2. Abnormal returns based on the market model are significantly negative for firms without female leaders, with a combined effect over the Weinstein/#MeToo windows of -0.83% ($p < 0.01$), and insignificant for firms with female leadership. The difference between the two of 0.83% is highly significant ($p = 0.03$).

Overall, the evidence reported in table 2 provides strong support for our conjecture that a nonsexist corporate culture is valuable—firms with women in top leadership positions earned positive excess returns relative to other firms when the importance of having a nonsexist culture increased around the Weinstein scandal and the emergence of the #MeToo movement.

We also investigate whether the benefits of having a woman in a top-5 leadership position are further enhanced when the CEO is a woman. A woman holding the highest position in a firm is arguably a strong indicator of a nonsexist culture. However, because only 5% of firms have a female CEO, a female CEO effect may be hard to detect empirically. To test for a CEO effect, we reestimate our base-case regression models but include a standalone female CEO indicator and either a variable capturing the fraction of women holding any of the remaining top-4 executive positions or an indicator if there is at least one woman among the remaining top-4 executives. The results (reported in table OA-2 of the online appendix) suggest that the valuation benefits of having a woman in the top

management team stem from all top-5 positions and not just the CEO position.

In further analyses, we estimate models where we allow the effect of female leadership on stock returns during the event windows to depend on the level of sexism in the firm's industry and in the state where it is head-quartered. We find that the positive effect of female leadership is particularly strong in industries with few women in executive positions, in more sexist states, and in states with a higher gender wage gap (see section 5.4 for the definition of these variables). These findings indicate that there is an important interaction between firm culture and the industry/societal culture in which the firm operates and that both can have an effect on firm value (see Guiso, Sapienza, and Zingales [2006], [2015b]). These results are reported in the online appendix tables OA-3 and OA-4.

3.2 ALTERNATIVE PROXIES FOR FIRM-LEVEL SEXISM AND CORPORATE CULTURE

One potential concern with our firm-level proxy of sexism is that some firms with a sexist culture may hire women in executive positions as a token in order to be perceived as being nonsexist. While there could be cases of tokenism, we believe that it is highly unlikely that firms would hire women as highly paid top executives, with associated decision-making authority, if they did not believe that they were competent. Nonetheless, to further alleviate this concern and, more generally, the concern that the lack of women among the top-5 executives may not capture firm-level sexism, we investigate whether our results continue to hold using two alternative proxies for firm-level sexism.

First, we examine whether our results persist when female leadership is measured at the level below the C-suite. To construct this alternative proxy, we obtain from the BoardEx database the profiles of the senior management of the organization at the Vice President level for the most recent fiscal year prior to October 1, 2017. This allows us to focus on managers who rank below a firm's C-suite but are likely to have senior leadership responsibilities. As we do for our primary female leadership variables, we compute the fraction of women among a firm's senior management (*Fraction Senior Management*) and also construct an indicator variable that equals one if a firm has at least one woman in a senior management position, and zero otherwise (*Indicator Senior Management*). Using these alternative measures of female leadership, we reestimate our regression models. Table OA-5 of the online appendix reports the results. The coefficients on both measures are positive and significant during the Weinstein and #MeToo event windows, indicating that when the importance of having a nonsexist culture increased, market participants also placed a higher value on firms with greater female leadership below the top-executive level.

Second, we employ a measure of sexism based on employee comments made on the Glassdoor website. Glassdoor is an employer review and recruiting website that contains company reviews from current and former

employees for 600,000 companies worldwide. We analyze individual reviewer's comments made under the Glassdoor "negative feedback" field for the years 2015 to 2016 for all U.S. companies with stock return data available on the CRSP database over the three-month period starting on September 1, 2017. Firms with less than 10 feedback reviews are removed from the analysis, yielding a sample of 1,920 companies. We flag each negative feedback if it contains keywords that are related to a sexist corporate culture, such as sexist, sexism, sexual harassment, misogyny, boys' club, etc. We create a dummy variable, *Glassdoor Negative Feedback*, which equals one if more than 10% of the negative feedback comments refer to these keywords, and zero otherwise. We then reestimate our models employing this dummy variable to capture whether the firm's culture is sexist or not. These findings, which are reported in table OA-6 of the online appendix, confirm our prior results that firms with a less sexist corporate culture outperformed other firms by 2.1% during the Weinstein and #MeToo event windows.

We also assess whether the Weinstein and #MeToo events led investors to reassess the value of corporate culture more broadly. To do so, we rely on the culture and values rating given to companies by their employees as provided by *Glassdoor*. Table OA-7 in the online appendix presents the results when we replace the female leadership measure with the *Glassdoor Culture* variable. We find that *Glassdoor Culture* is positively and significantly related to returns during both the Weinstein and #MeToo event windows. These results are consistent with broader corporate culture also being valued more highly during these periods.[15]

3.3 WOMEN ON THE BOARD

Much of the literature on gender diversity in corporate leadership has focused on the board of directors, and outside directors in particular, rather than on the executive team (see, e.g., Adams and Ferreira [2009], Adams and Funk [2012], Ahern and Dittmar [2012]). Prior work documents that female board members enhance a board's skill sets, which may increase board efficiency (see, e.g., Kim and Starks [2016]). Moreover, Matsa and Miller [2011] find that firms with female directors are more likely to recruit female executives, and Table 1 shows that female leadership is positively related to female board membership. Thus, it is possible that, as emphasized by Billings, Klein, and Shi [2022], the benefits of having a nonsexist culture originate at the board level.

To explore this conjecture, we investigate whether top female leadership is needed for the positive valuation effects of a nonsexist culture to materialize or whether the presence of women on the board suffices. We augment our baseline models with additional interactions between the relevant event windows and the fraction of female board

---

[15] We also find that firms with more negative feedback comments related to sexism have a lower overall culture and values rating.

TABLE 3
Shareholder Value and Female Directors

Female Variable =	Daily Stock Returns			
	Fraction Top-5 Women (1)	Indicator Top-5 Women (2)	Fraction Top-5 Women (3)	Indicator Top-5 Women (4)
Fraction Board Women ×				
Oct 5–6	−0.149	−0.052	0.042	0.135
	(0.26)	(0.67)	(0.81)	(0.40)
Oct 9–13			0.209	0.122
			(0.51)	(0.73)
Oct 16–27	0.034	0.078	0.224	0.266
	(0.92)	(0.83)	(0.51)	(0.45)
Oct 30–Nov 30			0.378	0.390
			(0.16)	(0.16)
Female Variable ×				
Oct 5–6	0.647***	0.139***	0.725***	0.169***
	(<0.01)	(<0.01)	(<0.01)	(<0.01)
Oct 9–13			−0.218	0.002
			(0.29)	(0.97)
Oct 16–27	0.299***	0.064*	0.377***	0.094**
	(0.01)	(0.10)	(<0.01)	(0.02)
Oct 30–Nov 30			0.220	0.066*
			(0.16)	(0.09)
Firm Fixed Effects	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes
N	85,743	85,743	85,743	85,743
Adjusted R^2	0.053	0.053	0.053	0.053

This table shows regression estimates of daily stock returns on interaction terms of *Fraction Board Women* × event, female × event and firm and time fixed effects. *Fraction Board Women* is calculated as the fraction of directors on the firm's board that are female. The female variables are *Fraction Top-5 Women*, which is the fraction of female executives among the five highest-paid executives of the company; and *Indicator Top-5 Women*, which is a dummy variable that equals one if a firm has at least one female executive among the five highest-paid executives, and zero otherwise. The event variables (e.g., Oct 5–6) are dummy variables that equal one for all trading days during a specific event window, and zero otherwise. The female variables are measured at the end of the most recent fiscal year prior to October 1, 2017. The sample period is from September 1, 2017, to November 30, 2017. The data are from CRSP, Execucomp, and BoardEx. Firms with missing returns during the sample period are dropped. Standard errors are double clustered by firm and time (trading day), and *p*-values are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

members.[16] The findings are reported in table 3. We continue to find that our measures of female executives (*Fraction Top-5 Women* and *Indicator Top-5 Women*) have a positive and significant effect on stock returns during the Weinstein and #MeToo event periods. However, the fraction of female board members has no incremental effect on returns over these periods.[17]

---

[16] Because 87% of our sample firms have at least one woman on the board, our tests concentrate solely on the fraction of female board members and not the presence of a woman on the board.

[17] Billings et al. [2022] show that firms with at least three women on the board over 2012 to 2016 earned positive returns during 37 selected days over the period October 2017 to May 2018 as the #MeToo movement unfolded. Our results indicate that over our sample period,

These results suggest that when the importance of having a nonsexist corporate culture increases, value creation stems from having women in top-executive positions rather than having additional female board members. Given that there are far more female directors than female top executives (see table 1), the presence of female executives is likely a stronger indicator of a nonsexist culture than the presence of female board members. This finding is consistent with Bertrand et al. [2019], who show that female board quotas in Norway did not lead to increased female labor participation or improved career outcomes for women in the affected companies.

3.4 ADDITIONAL ROBUSTNESS TESTS

We conduct several robustness tests to ensure that our results are not driven by confounding events and/or sample characteristics other than female leadership. First, while it is unlikely that firms with (without) female top executives would be announcing higher (lower) earnings or dividends exactly during the Weinstein and #MeToo event windows, we also verify that our results hold after controlling for the days on which dividends, earnings, mergers or restructurings are announced, and the following day (panel A of table OA-9 of the online appendix).

Second, we verify that our results are unaffected when we exclude firms from each of the Fama–French 49 industries one at a time. We also control for three industries simultaneously—healthcare, medical equipment, and pharmaceuticals—which may have been affected by the removal of certain Obamacare subsidies and/or the opioid crisis being declared a public health emergency during our sample period. Our results continue to hold (panel B of table OA-9 of the online appendix).

Third, we have reestimated our base-case model using a matched sample approach, where we match each firm with female leadership to a firm without female leadership closest in total assets within the same three-digit SIC code (moving to the two-digit, and one-digit SIC codes if no match is available). We conduct this matching approach both with and without replacement. Again, our results are unaffected (table OA-10 of the online appendix).

4. *Investor Preferences*

In this section, we study whether changes in investor preferences can explain the returns that we document. This analysis is motivated by the recent asset pricing models of Pastor, Stambaugh, and Taylor [2021] and

---

which covers both the Weinstein event and the start of the #MeToo movement, the impact of women on the board gets subsumed by the presence of women in leadership positions. Additionally, because female leadership is positively related to female board membership, we have also reestimated these models without the female leadership variables to assess the standalone valuation effect of female board membership during our event windows. We find positive excess returns during the Weinstein event (table OA-8 of the online appendix).

Pedersen, Fitzgibbons, and Pomorski [2021], in which changes in investor preferences for ESG performance can lead to positive (negative) abnormal returns for high (low) ESG stocks. In our context, the public revelation of the Weinstein scandal and the subsequent reemergence of the #MeToo movement increased the salience of gender equality, which is an important component of ESG. We predict that this increased salience will lead investors to switch their preferences toward greater gender equality, thereby increasing their demand for firms with a more gender-equal culture. The actual and anticipated changes in demand, in turn, result in the stock price response we document.

These models also predict firm responses to accommodate changes in investor preferences. As such, we expect that at least some of the firms with a more sexist culture will try to mitigate these effects by hiring more women executives.[18] However, because it takes time to adjust and because not all firms adjust, firms with a more sexist culture still experience a negative stock price response, on average.

Although investor preferences are not directly observable, we test two implications of these models. First, we examine whether institutional investors increase their ownership in firms with a nonsexist culture after the event. Second, we analyze whether firms cater to investor preferences by increasing gender diversity.

4.1 CHANGES IN INSTITUTIONAL OWNERSHIP

Institutions are sophisticated investors and the largest owners of the companies in our sample, with average ownership prior to the events exceeding 82%. Thus, the preferences of these investors will have the largest impact on the demand for stocks. In addition, as institutions have to disclose their portfolio holdings on a quarterly basis, we are able to study changes in their holdings around our events. If changes in institutional investor preferences cause the documented stock price effects, we would expect a relative increase in their shareholdings of firms with a nonsexist culture after the events.

To test this conjecture, we obtain data on institutional investor holdings from the FactSet Ownership database over the period 2016 to 2019. The ownership data are collected from quarterly Form 13F filings with the SEC, which are mandatory for all institutional investors with at least $100 million in assets under management. For each firm and quarter, we measure the aggregate ownership of all institutional investors. In addition, to assess whether institutions with larger stakes play a more important role, we aggregate the holdings of investors that hold at least 0.25%, 1%, and 5% of a firm's shares.

---

[18] In other words, the change in preferences shifts the equilibrium and firms subsequently adjust their policies to reflect these changed preferences. The relative increase in value of firms with a nonsexist culture also points to a small decrease in their cost of capital relative to firms with a sexist culture.

We estimate changes in aggregate institutional ownership after the Weinstein/#MeToo events using the following regression model:

$$
\begin{aligned}
Institutional\,Ownership_{i,t} = \quad & \alpha_i + \beta_t + \lambda_1 Female_i \times Post_t \\
& + \lambda_2 \log\left(Market\,Equity_{i,t}\right) + \varepsilon_{i,t},
\end{aligned} \tag{2}
$$

where $Institutional\,Ownership_{i,t}$ is the aggregate institutional ownership of firm i over quarter t; and $Post_t$ is a dummy set equal to zero for the quarters before the event and one for the quarters after the event. We also include firm and time (quarter) fixed effects and the firm's market capitalization as a control, and double cluster the standard errors by firm and time.[19]

Panel A of table 4 reports the results of these tests. We find that institutional investors significantly increased their holdings of firms with a nonsexist culture relative to other firms after the Weinstein/#MeToo events, which supports the notion that the stock price reaction documented previously is related to changes in investor preferences. In models 1 and 2, the coefficient estimates on the $Female \times Post$ interactions are statistically significant with p-values of 0.06 and 0.02, respectively. When we investigate aggregate holdings of institutional investors with a stake in a firm greater than a particular threshold level ($\geq 0.25\%$, $\geq 1\%$, and $\geq 5\%$), the results are statistically stronger ($p < 0.05$) except for the 5% cutoff and larger in magnitude (models 3–6). The findings are also economically important; based on the models that use the indicator variables (models 2, 4, 6, and 8), ownership by institutional investors increases by about one percentage point in firms with a nonsexist culture relative to firms with a sexist culture. Although this change may appear modest, it is not—our models include time and firm fixed effects, so the overall increasing trend in institutional ownership, as well as any firm-specific component in ownership, have been removed from this analysis. Thus, the basis for comparison is no change at all.

To confirm that the changes documented in panel A of table 4 are due to the events we study and are not the continuation of a general trend, we verify the parallel trends assumption by replacing the post-female interaction dummy with interaction dummies for each quarter in the sample. We remove the interaction for the quarter prior to the events (third quarter of 2017), so that all coefficients reflect the change in institutional ownership for firms with a nonsexist culture relative to that quarter. The results are displayed in panel B of table 4. None of the pre-event coefficients are significant, while all of the post-event coefficients are significant. We also note that the magnitude of the effect increases over time, suggesting that

---

[19] In this specification as well as all subsequent models that measure changes in characteristics over time, we remove the period that includes October 2017, our event period; the only exceptions are the models that investigate parallel trends in which we capture the event period by a separate dummy. Our results are very similar if we include the event period in the post-event window.

TABLE 4

Changes in Total Institutional Ownership Surrounding the Weinstein and #MeToo Events

Panel A: Institutional Ownership

	Institutional Ownership							
	Total		Holdings ≥ 0.25%		Holdings ≥ 1%		Holdings ≥ 5%	
Female Variable =	Fraction Top-5 Women	Indicator Top-5 Women	Fraction Top-5 Women	Indicator Top-5 Women	Fraction Top-5 Women	Indicator Top-5 Women	Fraction Top-5 Women	Indicator Top-5 Women
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Female Variable × Post	2.963*	1.097**	3.508**	1.216**	3.714**	1.263**	2.258	0.789*
	(0.06)	(0.02)	(0.04)	(0.02)	(0.03)	(0.02)	(0.14)	(0.09)
Log (Market Equity)	6.957***	6.958***	5.267***	5.267***	2.925***	2.924***	0.952	0.952
	(<0.01)	(<0.01)	(<0.01)	(<0.01)	(0.01)	(0.01)	(0.14)	(0.14)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	19,809	19,809	19,809	19,809	19,809	19,809	19,809	19,809
Adjusted R^2	0.872	0.872	0.872	0.872	0.849	0.849	0.771	0.771

(*Continued*)

TABLE 4—*(Continued)*

Panel B: Parallel Trends

	Institutional Ownership	
Female Variable =	Fraction Top-5 Women (1)	Indicator Top-5 Women (2)
Event time dummy interactions		
Female variable × $D_{\leq Q4, 2016}$	1.065	0.290
	(0.33)	(0.36)
Female variable × $D_{Q1, 2017}$	0.200	0.003
	(0.71)	(0.98)
Female variable × $D_{Q2, 2017}$	−0.649	−0.183
	(0.16)	(0.21)
Female variable × $D_{Q4, 2017}$	1.720***	0.542***
	(<0.01)	(<0.01)
Female variable × $D_{Q1, 2018}$	2.821***	0.895***
	(<0.01)	(<0.01)
Female variable × $D_{Q2, 2018}$	3.289***	1.032***
	(<0.01)	(<0.01)
Female variable × $D_{Q3, 2018}$	4.180***	1.191***
	(<0.01)	(<0.01)
Female variable × $D_{\geq Q4, 2018}$	3.472***	1.341***
	(0.01)	(<0.01)
Log (Market Equity)	6.834***	6.835***
	(<0.01)	(<0.01)
Firm Fixed Effects	Yes	Yes
Time Fixed Effects	Yes	Yes
N	21,140	21,140
Adjusted R^2	0.875	0.875

(Continued)

TABLE 4—*(Continued)*

This table presents regression results of institutional ownership around the Weinstein and #MeToo events. Panel A shows regression estimates of institutional ownership on female × *Post*, equity market capitalization, and firm and time (year-quarter) fixed effects. *Post* is a dummy variable equal to zero for the quarters 2016q1 to 2017q3 and equal to one for the quarters 2018q1 to 2019q4. The dependent variables, in percent, are measures of total institutional ownership (columns 1 and 2) and total institutional ownership of institutions holding at least 0.25%, 1%, and 5% of a firm's equity (columns 3–8). The ownership data are obtained from the FactSet Ownership database, which is compiled from quarterly Form 13F filings with the SEC. Panel B displays regression results of institutional ownership on time dummies interacted with the female variables, equity market capitalization, and firm and time (year-quarter) fixed effects. Each time dummy is equal to one for a particular quarter-year period, and zero otherwise. The time dummy interaction for the quarter before the Weinstein/#MeToo events (i.e., 2017q3 or $t = -1$) is excluded from the regressions. The female variables are *Fraction Top-5 Women*, which is the fraction of female executives among the five highest-paid executives of the company; and *Indicator Top-5 Women*, which is a dummy variable that equals one if a firm has at least one female executive among the five highest-paid executives, and zero otherwise. The data are from Execucomp and FactSet. Standard errors are double clustered by firm and time, and *p*-values are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

the change in institutions' preferences for firms with a nonsexist culture is
not only permanent but has gradually become more pronounced.

To determine whether the documented change in ownership is due to
firms with or without female leadership, we estimate separate effects for
both groups. We find an increase in institutional ownership for both sets
of firms, reflecting the overall trend in institutional ownership over time.
However, only the increase for firms with female leadership is significant.

We also study ownership at the individual institutional investor level to
gain more insight into the importance of changes in demand by institu-
tional investors. Such an analysis allows us to include investor by time fixed
effects ($\lambda_{j,t}$) to control for any time-varying factors that could affect a spe-
cific investor's demand at a given point in time. In particular, we estimate
the following regression:

$$
\begin{aligned}
\textit{Institutional Ownership}_{i,j,t} = \quad & \alpha_i + \lambda_{j,t} + \gamma_1 \textit{Female}_i \times \textit{Post}_t \\
& + \gamma_2 \log\left(\textit{Market Equity}_{i,t}\right) + \varepsilon_{i,j,t},
\end{aligned}
\tag{3}
$$

where *Institutional Ownership*$_{i,j,t}$ is the individual ownership of institution *j*
in firm *i* over quarter *t*; and the remaining variables are as defined pre-
viously. In this specification, we triple cluster the standard errors by firm,
institution, and time.

The findings are reported in table 5. The coefficients on each of the in-
teraction terms are highly significant, indicating that institutional investors
increase their relative ownership of firms with female leadership. Not
surprisingly, the effect is bigger for larger institutions. Whereas the average
institution increases its relative ownership by 0.5 basis points in firms with
female leaders (model 2), institutions with holdings above 5% increase
their relative holdings in firms with female leadership by 13.5 basis points
(model 8).

Next, to provide additional support for the investor preference explana-
tion, we investigate whether the changes in ownership depend on investor
preferences for firms with a nonsexist culture prevailing before the events.
We posit that the Weinstein/#MeToo events should have a *smaller* impact on
the holdings of institutions that previously revealed a stronger preference
for high-ESG stocks in their portfolios, insofar as gender diversity is an im-
portant component of ESG. Such investors already displayed preferences
for high ESG stocks and, as such, changes in general societal preferences
toward gender equality are not expected to change their preferences sub-
stantially. Conversely, the events should have a *larger* impact on investors
that were less concerned about ESG/gender diversity prior to the events—
these are the investors whose preferences are more likely to have changed.

To capture institutional investor preferences, we follow Gantchev, Gi-
annetti, and Li [2022] and compute, for each institutional investor, the
weighted average ESG score of its portfolio holdings prior to the event,

TABLE 5
Changes in Investor-Level Institutional Ownership Surrounding the Weinstein and #MeToo Events

	Institutional Ownership							
	Total		Holdings ≥ 0.25%		Holdings ≥ 1%		Holdings ≥ 5%	
Female Variable =	Fraction Top-5 Women	Indicator Top-5 Women	Fraction Top-5 Women	Indicator Top-5 Women	Fraction Top-5 Women	Indicator Top-5 Women	Fraction Top-5 Women	Indicator Top-5 Women
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Female Variable × Post	0.018***	0.005***	0.043**	0.012**	0.108***	0.034**	0.320*	0.135**
	(<0.01)	(<0.01)	(0.02)	(0.02)	(0.03)	(0.02)	(0.07)	(0.03)
Log (Market Equity)	−0.025***	−0.025***	−0.062***	−0.062***	−0.098*	−0.098*	−0.080	−0.080
	(<0.01)	(<0.01)	(0.01)	(0.01)	(0.06)	(0.06)	(0.70)	(0.70)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Investor × Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	10,064,779	10,064,779	1,632,014	1,632,014	561,114	561,114	85,461	85,461
Adjusted R^2	0.550	0.550	0.535	0.535	0.503	0.503	0.451	0.451

This table presents regression results of investor-level institutional ownership around the Weinstein and #MeToo events on female × Post, equity market capitalization, and firm and investor × time (year-quarter) fixed effects. *Post* is a dummy variable equal to zero for the quarters 2016q1 to 2017q3 and equal to one for the quarters 2018q1 to 2019q4. The dependent variables, in percent, are measures of investor-level institutional ownership (columns 1 and 2) and investor-level institutional ownership of institutions holding at least 0.25%, 1%, and 5% of a firm's equity (columns 3–8). The ownership data are obtained from the FactSet Ownership database, which is compiled from quarterly Form 13F filings with the SEC. The female variables are *Fraction Top-5 Women*, which is the fraction of female executives among the five highest-paid executives of the company; and *Indicator Top-5 Women*, which is a dummy variable that equals one if a firm has at least one female executive among the five highest-paid executives, and zero otherwise. The data are from Execucomp and FactSet. Standard errors are clustered by firm, investor, and time, and *p*-values are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

using the scores from the Refinitiv ESG database.[20] We compute the mean of this metric across the four quarters from December 2016 to September 2017 to obtain a holdings-based ESG score for each investor. Next, we divide investors into terciles based on their portfolio ESG scores and investigate whether changes in holdings after the Weinstein/#MeToo events are more pronounced for investors that had relatively low-ESG portfolios before these events occurred. To conduct this analysis, we estimate equation (3) separately for high- and low-ESG investors.

Panel A of table 6 contains the results. Consistent with our prediction, low-ESG investors increase their holdings in firms with a nonsexist culture significantly more than high-ESG investors. In fact, the entire change in holdings is driven by low-ESG investors. These findings further support the investor preference explanation for the return results documented in Table 2.

In panels B and C of table 6, we confirm that these results also hold when we narrow our focus to two components of the Refinitiv ESG metric that are particularly relevant in light of the Weinstein/#MeToo events: the Social pillar score and the Workforce score. To that end, we divide institutional investors into terciles using their holdings-based Social pillar or Workforce scores. We continue to find that the investors from the lowest terciles on either score are the ones that change their holdings of firms with a non-sexist culture the most, consistent with the notion that investors with less of a focus on ESG issues prior to the Weinstein/#MeToo events are the ones making the most significant adjustments to their portfolios.

We also investigate whether these changes in institutional ownership by low-ESG investors lead to meaningful changes in the ESG scores of their portfolios. To do so, we first estimate a regression of the portfolio ESG score of each investor over the period 2016 to 2019 on investor and time fixed effects, and an interaction term between the low-ESG dummy and a post-event dummy that is equal to one for the quarters ending March 2018 to December 2019, and zero otherwise (the event quarter ending December 2017 is removed from the analysis). Column 1 of table 7 reports the results. We find that, compared with high-ESG investors, low-ESG investors significantly improve the ESG scores of their portfolios. The average pre-event ESG portfolio score of low-ESG investors is 42.7. Thus, the coefficient of 4.296 (column 1) represents a relative increase of 10.1%.

These changes in investor ESG scores could, of course, be driven by improvements in the ESG scores of their portfolio firms. To verify that they do indeed reflect active portfolio rebalancing, we recompute the investor ESG

---

[20] As in Gantchev et al. [2022], the weights are based on all firms with ESG measures available on Refinitiv, and if more than half (by value) of the portfolio firms have no ESG measures reported in Refinitiv, the score is set to zero. Our results continue to hold if we use the score based on all available firms in Refinitiv, even if they constitute less than half of the portfolio. They are also unchanged if we remove institutions for which we cannot obtain ESG scores for at least half of their holdings.

T A B L E 6

Changes in Investor-Level Institutional Ownership Surrounding the Weinstein and #MeToo Events: Splits by Investor ESG Preferences

Panel A: Investor ESG Preferences Measured with the Overall ESG Score

	Institutional Ownership			
	Fraction Top-5 Women		Indicator Top-5 Women	
Female Variable =	Low-ESG Preference Investors (1)	High-ESG Preference Investors (2)	Low-ESG Preference Investors (3)	High-ESG Preference Investors (4)
Female Variable × Post	0.032***	0.001	0.009***	0.000
	(<0.01)	(0.63)	(<0.01)	(0.61)
Log (Market Equity)	−0.066***	−0.006**	−0.066***	−0.006**
	(<0.01)	(0.02)	(<0.01)	(0.02)
Firm Fixed Effects	Yes	Yes	Yes	Yes
Investor × Time Fixed Effects	Yes	Yes	Yes	Yes
N	3,109,772	3,131,573	3,109,772	3,131,573
Adjusted R^2	0.424	0.354	0.424	0.354
p-value of low − high	(<0.01)		(<0.01)	

Panel B: Investor ESG Preferences Measured with the Social Pillar Score

	Institutional Ownership			
	Fraction Top-5 Women		Indicator Top-5 Women	
Female Variable =	Low-ESG Preference Investors (1)	High-ESG Preference Investors (2)	Low-ESG Preference Investors (3)	High-ESG Preference Investors (4)
Female Variable × Post	0.030***	0.001	0.008***	0.000
	(<0.01)	(0.52)	(<0.01)	(0.57)
Log (Market Equity)	−0.065***	−0.007**	−0.065***	−0.007**
	(<0.01)	(0.01)	(<0.01)	(0.01)
Firm Fixed Effects	Yes	Yes	Yes	Yes
Investor × Time Fixed Effects	Yes	Yes	Yes	Yes
N	3,123,414	3,131,163	3,123,414	3,131,163
Adjusted R^2	0.426	0.345	0.426	0.345
p-value of low − high	(<0.01)		(<0.01)	

(*Continued*)

T A B L E   6—*(Continued)*

Panel C: Investor ESG Preferences Measured with the Workforce Score

Female Variable =	Institutional Ownership			
	Fraction Top-5 Women		Indicator Top-5 Women	
	Low-ESG Preference Investors (1)	High-ESG Preference Investors (2)	Low-ESG Preference Investors (3)	High-ESG Preference Investors (4)
Female Variable × Post	0.031***	0.002	0.008***	0.001
	(<0.01)	(0.16)	(<0.01)	(0.22)
Log (Market Equity)	−0.065***	−0.009***	−0.065***	−0.009***
	(<0.01)	(<0.01)	(<0.01)	(<0.01)
Firm Fixed Effects	Yes	Yes	Yes	Yes
Investor × Time Fixed Effects	Yes	Yes	Yes	Yes
N	3,127,221	3,129,013	3,127,221	3,129,013
Adjusted R^2	0.427	0.320	0.427	0.320
p-value of low − high	(<0.01)		(<0.01)	

This table presents regression results of investor-level institutional ownership around the Weinstein and #MeToo events on female × *Post*, equity market capitalization, and firm and investor × time (year-quarter) fixed effects, grouped by investors' ex-ante ESG preferences using three metrics from Refinitiv: Overall ESG score (panel A), Social pillar score (panel B), and Workforce score (panel C). For each institutional investor, we separately compute the weighted average Overall ESG score, Social pillar score, and Workforce score of its holdings using each of the four quarters prior to the Weinstein/#MeToo events (2016q4 to 2017q3). For each quarterly computation, if more than half (by value) of the portfolio firms have no score for the metric reported in Refinitiv, the score is set to zero. We sort institutional investors into terciles based on the four-quarter average of each of their respective ex ante ESG preference scores and discard the middle tercile (i.e., we use the *Low-ESG Preference Investors* and *High-ESG Preference Investors* terciles). *Post* is a dummy variable equal to zero for the quarters 2016q1 to 2017q3 and equal to one for the quarters 2018q1 to 2019q4. The dependent variable, in percent, is the investor-level institutional ownership in a given firm and quarter. The ownership data are obtained from the FactSet Ownership database, which is compiled from quarterly Form 13F filings with the SEC. The female variables are *Fraction Top-5 Women*, which is the fraction of female executives among the five highest-paid executives of the company; and *Indicator Top-5 Women,* which is a dummy variable that equals one if a firm has at least one female executive among the five highest-paid executives, and zero otherwise. The data are from Execucomp, FactSet, and Refinitiv. Standard errors are clustered by firm, investor, and time, and *p*-values are reported in parentheses. *** and ** denote statistical significance at the 1% and 5% levels, respectively.

scores holding firm-level ESG scores constant at their pre-event levels (using the four-quarter average prior to the events) and reestimate the above regression. Thus, any changes we document using this alternative metric must be due to changes in portfolio holdings. The results presented in column 2 of table 7 suggest that 56% of the improvements in the ESG scores (2.421 vs. 4.296) of low- versus high-ESG investors are due to changes in portfolio composition. We repeat these analyses for the Social pillar and the Workforce scores in columns 3–6. As before, low-tercile investors exhibit larger improvements in their Social pillar and Workforce scores (columns 3 and 5), and these improvements are primarily (82% and 88%, respectively) driven by portfolio rebalancing (columns 4 and 6). These results also suggest that investors do not expect all firms to make (sufficient) changes in ESG/gender diversity, which is consistent with the stock price reaction that we document.

TABLE 7
Changes in Institutional Investor-Level ESG Scores Surrounding the Weinstein and #MeToo Events

Portfolio Firms' ESG Scores =	Investor Level Overall ESG Score		Investor Level Social Pillar Score		Investor Level Workforce Score	
	Time-Varying (1)	Constant (2)	Time-Varying (3)	Constant (4)	Time-Varying (5)	Constant (6)
Low-ESG Preference Investor × Post	4.296***	2.421***	3.347***	2.746***	3.275***	2.873***
	(<0.01)	(<0.01)	(<0.01)	(<0.01)	(<0.01)	(<0.01)
Investor Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
N	40,061	40,061	40,125	40,125	40,057	40,057
Adjusted R^2	0.822	0.855	0.814	0.850	0.809	0.842

This table presents regression results of institutional investor-level ESG scores around the Weinstein and #MeToo events on investors' ex ante ESG preferences × *Post*, and investor and time (year-quarter) fixed effects. We use three different metrics from Refinitiv to measure investor-level ESG scores as the dependent variable: Overall ESG score, Social pillar score, and Workforce score. In columns 1, 3, and 5 (*Time-varying*), for each of the three ESG scores separately, we measure the weighted average investor-level ESG score using the investor's quarterly holdings and the portfolio firms' *quarterly* ESG scores. In columns 2, 4, and 6 (*Constant*), for each of the three ESG scores separately, we hold the portfolio firms' ESG scores *constant* and measure the weighted average investor-level ESG score using the investor's quarterly holdings and the average portfolio firms' ESG scores over the four quarters prior to the Weinstein/#MeToo events (2016q1 to 2017q3). For each quarterly computation, if more than half (by value) of the portfolio firms have no score for the metric reported in Refinitiv, the score is set to zero. To measure an investor's ex ante ESG preference, we separately compute the weighted average Overall ESG score, Social score, and Workforce score of its holdings using each of the four quarters prior to the Weinstein/#MeToo events (2016q4 to 2017q3). For each quarterly computation, if more than half (by value) of the portfolio firms have no score for the metric reported in Refinitiv, the score is set to zero. We sort institutional investors into terciles based on each of their respective ex ante ESG preference score and discard the middle tercile. *Low-ESG Preference Investors* is a dummy variable that is equal to one for investors in the bottom (low) tercile, and zero for investors in the top (high) tercile. *Post* is a dummy variable equal to zero for the quarters 2016q1 to 2017q3 and equal to one for the quarters 2018q1 to 2019q4. The ownership data are obtained from the FactSet Ownership database, which is compiled from quarterly Form 13F filings with the SEC. The data are from FactSet and Refinitiv. Standard errors are clustered by investor and time, and *p*-values are reported in parentheses. *** denotes statistical significance at the 1% level.

Finally, we conduct two tests to confirm that the institutions that are changing their ownership positions are indeed reacting to the (lack of) female leadership in the firm rather than simply chasing returns. In our first test, we run a "horse race" between the female leadership measures and quarterly market-adjusted stock returns. If institutions are just trend chasers, increasing their holdings of firms with female leadership because they performed well, then their holdings will be explained by stock returns, and not by the leadership characteristics of the firm. Therefore, we include the firm's contemporaneous quarterly market-adjusted return as an additional explanatory variable in the models explaining individual institutional ownership. Panel A of table 8 contains the results. The change in institutional ownership is significantly related to the firm's market-adjusted return, consistent with the findings of Grinblatt, Titman, and Wermers [1995]. However, the effect of female leadership remains significant for both measures of female leadership, and the magnitude of the coefficient is virtually unchanged from table 5. Thus, investors are indeed responding to measures of the firm's leadership and not just chasing returns.

In the second test, we split institutional investors into two groups depending on their holdings of each stock over the 15 quarters prior to October 2017. Those who held the stock every single quarter are classified as long-term investors, while those who did not are deemed transient investors. The assumption is that transient investors are more likely arbitrageurs whose preferences are less likely to be affected by changes in social norms. Long-term investors, on the other hand, are dedicated institutions whose preferences are more likely to respond to changes in social norms. In panel B of table 8, we display the change-in-ownership results for both groups and show that the relation between the change in ownership and our measure of female leadership is substantially larger for long-term investors than for transient investors, consistent with the view that the preferences of dedicated investors are more relevant in this context. We further verify that the changes in ownership of long-term investors are primarily driven by institutions with low-ESG preferences (see table OA-11 in the online appendix).

Combined, the results reported in this section indicate that institutional investors rebalance their portfolios toward stocks with more female leadership, particularly if the investors did not have an ESG focus prior to the Weinstein/#MeToo events, and that these changes lead to improvements in the ESG scores of their portfolios. Moreover, the changes are more substantial for long-term investors. These results are consistent with changes in investor preferences driving the stock return results that we document.

4.2 CHANGES IN FIRMS' GENDER DIVERSITY

Next, we investigate an additional implication of the investor preference hypothesis, which is that firms with fewer female leaders will cater to investor preferences by increasing gender diversity. To measure gender diversity, we employ three metrics obtained from the Refinitiv ESG database for

TABLE 8

Changes in Investor-Level Institutional Ownership Surrounding the Weinstein and #MeToo Events: Additional Tests

Panel A: Controlling for Returns

	Institutional Ownership	
	Fraction Top-5 Women	Indicator Top-5 Women
Female Variable =	(1)	(2)
Female Variable × Post	0.018***	0.005***
	(<0.01)	(<0.01)
Return	0.037***	0.037***
	(<0.01)	(<0.01)
Log (Market Equity)	−0.027***	−0.027***
	(<0.01)	(<0.01)
Firm Fixed Effects	Yes	Yes
Investor × Time Fixed Effects	Yes	Yes
N	10,064,779	10,064,779
Adjusted R^2	0.550	0.550

Panel B: Splits by Long-Term and Transient Investors

	Institutional Ownership			
	Fraction Top-5 Women		Indicator Top-5 Women	
	Long-Term Investors	Transient Investors	Long-Term Investors	Transient Investors
Female Variable =	(1)	(2)	(3)	(4)
Female Variable × Post	0.017**	0.014***	0.005**	0.003**
	(0.05)	(0.01)	(0.02)	(0.03)
Log (Market Equity)	−0.009	−0.028***	−0.009	−0.028***
	(0.38)	(<0.01)	(0.39)	(<0.01)
Firm Fixed Effects	Yes	Yes	Yes	Yes
Investor × Time Fixed Effects	Yes	Yes	Yes	Yes
N	3,629,241	6,432,488	3,629,241	6,432,488
Adjusted R^2	0.624	0.379	0.624	0.379
p-value of Long-Term − Transient	(0.12)		(0.04)	

This table provides additional tests for changes in investor-level institutional ownership surrounding the Weinstein and #MeToo events. Panel A shows regression results of investor-level institutional ownership around the Weinstein and #MeToo events on female × *Post*, *Returns*, equity market capitalization, and firm and investor × time (year-quarter) fixed effects. Panel B presents regression results of investor-level institutional ownership around the Weinstein and #MeToo events on female × *Post*, equity market capitalization, and firm and investor × time (year-quarter) fixed effects for long-term and transient investors. We define long-term investors as those invested in a particular firm's stock every quarter for the 15 quarters from 2014q1 to 2017q3. The other investors are considered transient investors. The dependent variable, in percent, is the investor-level institutional ownership in a given firm and quarter. The ownership data are obtained from the FactSet Ownership database, which is compiled from quarterly Form 13F filings with the SEC. The female variables are *Fraction Top-5 Women*, which is the fraction of female executives among the five highest-paid executives of the company; and *Indicator Top-5 Women*, which is a dummy variable that equals one if a firm has at least one female executive among the five highest-paid executives, and zero otherwise. *Post* is a dummy variable equal to zero for the quarters 2016q1 to 2017q3 and equal to one for the quarters 2018q1 to 2019q4. *Return* is the quarterly stock return minus the contemporaneous CRSP value-weighted market return. The data are from Execucomp, FactSet, and CRSP. Standard errors are clustered by firm, investor, and time, and *p*-values are reported in parentheses. *** and ** denote statistical significance at the 1% and 5% levels, respectively.

the 2013 to 2020 period: (1) the Refinitiv *Diversity Score*, which measures a firm's commitment and effectiveness toward maintaining a gender diverse workforce and board member cultural diversity; it ranges from 0 to 100 with higher values indicating greater gender diversity; (2) *Executive Member Gender Diversity*, which measures the fraction of women among a firm's executives; and (3) *Policy Diversity and Opportunity*, which is a dummy variable equal to one if the firm has a policy to drive diversity and equal opportunity, and zero otherwise.

To study changes in diversity, we estimate diff-in-diff regressions of the diversity measures on an interaction term between our female measures and a *Post* dummy that is zero for the years 2013 to 2016 and one for the years 2018 to 2020; we also include firm and time (year) fixed effects (2017 is removed because it is the event year).

Panel A of table 9 reports the results. For all diversity measures, firms with fewer female top executives improve gender diversity more than other firms after the Weinstein and #MeToo events. The results are also economically significant. For example, the estimates in column 2 suggest that firms without female top executives improve their average *Diversity Score* by 8.7% relative to firms with female top executives (calculated as: coefficient estimate of 1.509 / average *Diversity Score* of 17.3 for firms with no female top executive before the Weinstein/#MeToo events).[21] Despite these changes, we note that firms that did not have female leaders prior to the events continue to be less diverse than firms that did; thus, they do not fully catch up. For example, in 2020, firms with and without female leaders prior to the Weinstein event have average *Executive Member Gender Diversity* of 23.1% and 14.1%, respectively.

One potential concern with this analysis is that these changes might have taken place irrespective of the Weinstein/#MeToo events. In particular, it could be that firms with fewer female top executives started changing their policies well before these events; as such, convergence was already happening and the Weinstein/#MeToo events were not necessary to elicit changes. To address this concern, we conduct a parallel trends analysis by replacing the post dummy with dummies for the periods before and after the events. We restrict this analysis to the *Executive Member Gender Diversity* and *Policy Diversity and Opportunity* measures because the *Diversity Score* measure is available only for two years prior to the event (the analysis for this latter metric is reported in table OA-13 of the online appendix). Our findings, which are reported in panel B of table 9, indicate that there are no differences in pre-event trends for the *Policy Diversity and Opportunity* measure. For the *Executive Member Gender Diversity* measure, we do find a pre-trend, but it is the opposite of convergence: compared with 2016, the omitted year, firms with female leadership increased *Executive Member Gender Diversity* relative

---

[21] Table OA-12 of the online appendix shows that the results hold for shorter estimation periods around the events (2014–2020 and 2015–2019), and do not hold for placebo event dates (2013 and 2014).

TABLE 9

Changes in Gender Diversity Measures Surrounding the Weinstein and #MeToo Events

Panel A: Relative Changes in Gender Diversity

Female Variable =	Diversity Score		Executive Member Gender Diversity		Policy Diversity and Opportunity	
	Fraction Top-5 Women (1)	Indicator Top-5 Women (2)	Fraction Top-5 Women (3)	Indicator Top-5 Women (4)	Fraction Top-5 Women (5)	Indicator Top-5 Women (6)
Female Variable × Post	−6.218**	−1.509**	−5.790	−2.105*	-0.129*	-0.037*
	(0.03)	(0.02)	(0.23)	(0.07)	(0.08)	(0.07)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
N	3,713	3,713	6,815	6,815	6,823	6,823
Adjusted R^2	0.758	0.758	0.688	0.689	0.613	0.613

Panel B: Parallel Trend Regressions

Female Variable =	Executive Member Gender Diversity		Policy Diversity and Opportunity	
	Fraction Top-5 Women (1)	Indicator Top-5 Women (2)	Fraction Top-5 Women (3)	Indicator Top-5 Women (4)
Event Time Dummy Interactions				
Female Variable × D_{2013}	−14.918***	−3.205***	0.019	−0.001
	(<0.01)	(<0.01)	(0.80)	(0.98)
Female Variable × D_{2014}	−12.294***	−2.479***	0.092	0.026
	(<0.01)	(<0.01)	(0.12)	(0.15)
Female Variable × D_{2015}	−4.306**	−0.875*	−0.004	0.003
	(0.03)	(0.06)	(0.94)	(0.85)
Female Variable × D_{2017}	−1.575	−0.903**	−0.021	−0.001
	(0.34)	(0.02)	(0.61)	(0.93)
Female Variable × D_{2018}	−7.708***	−2.287***	−0.066	−0.023*
	(<0.01)	(<0.01)	(0.24)	(0.08)
Female Variable × D_{2019}	−13.779***	−3.803***	−0.117*	−0.037**
	(<0.01)	(<0.01)	(0.08)	(0.03)
Female Variable × D_{2020}	−16.971***	−4.541***	−0.222***	−0.046*
	(<0.01)	(<0.01)	(<0.01)	(0.08)
Firm Fixed Effects	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes
N	8,083	8,083	8,092	8,092
Adjusted R^2	0.713	0.712	0.653	0.653

Panel C: Changes in Gender Diversity for Firms With and Without Female Leadership

	Diversity Score (1)	Executive Member Gender Diversity (2)	Policy Diversity and Opportunity (3)
With Top-5 Women × Post	2.083**	1.748*	0.047**
	(0.03)	(0.08)	(0.03)
Without Top-5 Women × Post	3.588**	3.250***	0.085***
	(0.02)	(<0.01)	(<0.01)
Difference	−1.505**	−1.502***	−0.038***
	(0.04)	(<0.01)	(<0.01)

(*Continued*)

TABLE 9—*(Continued)*

This table presents regression results of gender diversity measures around the Weinstein and #MeToo events. Panel A shows regression estimates of diversity measures on female × *Post*, and firm and time (year) fixed effects. Panel B displays regression results of diversity measures on time dummies interacted with the female variables for the years 2013 to 2020, and firm and time (year) fixed effects. Each time dummy is equal to one for a particular year, and zero otherwise. The time dummy interaction for the year before the Weinstein/#MeToo events (i.e., 2016 or $t = -1$) is excluded from the regressions. Panel C shows coefficient estimates of regressions of measures of gender diversity on *With Top-5 Women × Post* and *Without Top-5 Women × Post* surrounding the Weinstein/#MeToo events. *Post* is a dummy variable equal to zero for the years 2013 to 2016, and equal to one for the years 2018 to 2020. The gender diversity measures are obtained from the Refinitiv ESG database over the period 2013 to 2020. *Diversity Score* measures a company's commitment and effectiveness toward maintaining a gender-diverse workforce and board member cultural diversity. The score is compiled by Refinitiv and ranges from 0 to 100 with higher values indicating greater diversity and is available from 2016 to 2020. *Executive Member Gender Diversity* is the fraction of females among a firm's executives. *Policy Diversity and Opportunity* is a dummy variable equal to one if the firm has a policy to drive diversity and equal opportunity, and zero otherwise. In panels A and B, the female variables are *Fraction Top-5 Women, which* is the fraction of female executives among the five highest-paid executives of the company; and *Indicator Top-5 Women,* which is a dummy variable that equals one if a firm has at least one female executive among the five highest-paid executives, and zero otherwise. In panel C, *With Top-5 Women* is a dummy variable that equals one if a firm has at least one female executive among the five highest-paid executives, and zero otherwise. *Without Top-5 Women* is a dummy variable that equals one if a firm has no female executive among the five highest-paid executives, and zero otherwise. In all regressions, the standalone *With Top-5 Women* variable is included but not reported. The data are from Execucomp and Refinitiv. Standard errors are double clustered by firm and time, and *p*-values are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

to firms without female leadership (the coefficient becomes less negative as the gap between the two sets of firms widens). After the event period, the gap declines as firms without female leadership add female executives. Thus, firms with fewer female executives start catching up only in the post-event period.

To further illustrate which group of firms contributes most to the change in relative diversity, we also report changes in the three diversity measures separately for firms with and without female leaders prior to the Weinstein event. As documented in panel C of table 9, both sets of firms improve their diversity; however, the change is significantly larger for firms without women in leadership positions.

In our final test, we relate the abnormal returns (documented in section 3.1) to the changes in diversity for firms without female top leaders. The expected sign of this relation is unclear because the stock price response is the combination of two effects: (1) the relative decline in the price of firms without female top executives resulting from changes in investor preferences; and (2) the anticipated improvements in gender diversity; that is, among firms without female leadership, those expected to change the most should experience the smallest relative stock price declines, thereby undoing some of the first effect. Of course, these tests require a substantial amount of foresight by capital markets because investors have to anticipate which of the firms without female leadership will be making at least some changes. Overall, we find evidence of a positive relation between abnormal returns and changes in diversity when diversity is measured by the *Diversity Score* or by having a *Policy on Diversity and Opportunity,* but not when measured by *Executive Member Gender Diversity* (table OA-14 in the online

appendix). Thus, the negative reaction for firms without female leadership is partly offset by the anticipation of improvements in diversity for some firms.[22]

5. Alternative Mechanisms

In this section, we examine several alternative explanations for the stock price reaction we document.

5.1 LITIGATION RISK

First, we investigate whether the return results documented in table 2 could be explained by increased litigation risk in firms with a potentially more sexist corporate culture. We note, however, that the change in value for the average firm without female top executives (compared with firms with female top executives) around the Weinstein and #MeToo events is $215 million (1.3% return differential multiplied by the average firm market capitalization of $16.5 billion); this seems very high for it to be a reflection of the expected increase in legal costs, fines, and possible private settlements.[23]

Nonetheless, we test the litigation risk hypothesis using Audit Analytics, which tracks firms' filings with the SEC of lawsuits that have a *potential material financial impact*, and focus on lawsuits filed in the Civil Rights–Jobs category as these will contain Equal Employment Opportunity Commission (EEOC) violation allegations. For all the firms in our sample, there are only 11 potentially material lawsuits disclosed over the period January 2018 to December 2020 (three in firms with a top-5 female, eight in firms without one), which indicates that increased litigation risk is not a likely explanation.[24] We acknowledge that we cannot observe private settlements; however, even if they amount to large sums, they are unlikely to account for the sizable valuation effects that we report.[25]

---

[22] Note that this relation does not depend on whether the abnormal returns are due to changes in nonmonetary preferences or due to expected improvements in performance as a result of improving executive gender diversity. Both could lead to a smaller stock price decline for those firms without female leadership that are expected to increase executive team diversity the most.

[23] Although the actual litigation costs for any particular firm that is sued or settles privately may well be higher than $215 million, this amount corresponds to the average expected cost if every single sample firm were forced to defend a lawsuit or to settle privately. For example, if only 5% of the sample firms are targeted and thus incur litigation costs, this would imply an average fine of $4.3 billion for these firms, which we believe is implausibly high.

[24] We find similar results when we compile lawsuits in the Civil Rights–Jobs category using the Federal Judicial Center Civil Integrated Database that covers all lawsuits filed in Federal courts.

[25] It is possible that we find little evidence of increased litigation because firms with fewer female top executives improve gender diversity after the Weinstein/#MeToo events as documented in table 9. However, the lawsuits filed shortly after the Weinstein/#MeToo events

It is possible, of course, that the market expected much more litigation than actually took place and/or large associated reputational losses. We test this conjecture by looking at bond yield spreads. If the market anticipated increased litigation or reputational risk, the yield spreads of affected companies should have increased in the post-Weinstein/#MeToo period. To investigate this possibility, we obtain bond yield data for our sample firms from the Enhanced Historic TRACE database over the two-year period spanning the events (October 2016 to September 2018). We combine these with Treasury yields from the Federal Reserve to calculate the yield spreads of the firms in our sample, computed as the difference between the bond yield and the Treasury yield matched by maturity. We then estimate a diff-in-diff regression of monthly yield spreads on an interaction term between our female measures and a *Post* dummy that is zero for the 12 months prior to October 2017 and one for the next 12 months. In all models, we include control variables (as in Correia, Richardson, and Tuna [2012] and Amiraslani et al. [2023]), and firm and time (month) fixed effects.

Table 10 reports the results of these tests. In none of the models is the female leadership effect different after the Weinstein/#MeToo events, which does not support the increased risk argument. To ensure that there are no short-run increases in spreads that get reversed subsequently, we also estimate these regressions with our female measures interacted with separate dummies for every month in the post-event period (October 2017–September 2018). None of the interactions are significant (not tabulated). Finally, we also test for a yield spread effect if we limit the sample to firms with noninvestment grade bonds, which are most affected by downside risk changes, but find no relative changes in spreads for this subsample either (not tabulated).[26]

These yield spreads analyses also rule out the possibility that the market anticipated higher litigation costs, but that the firms took action to counteract this risk, leading to no observable change in the number of lawsuits.

---

likely reflect misdeeds that occurred prior to the event. As such, the small number of material lawsuits filed is unlikely to be due to improvements in diversity after these events.

[26] In principle, the investor preference hypothesis could also apply to the debt markets. As such, the lack of differences in yield spreads could be interpreted as contradicting this hypothesis. There are at least two reasons, however, why we believe that the lack of a result is not a cause for concern. First, ESG investing in the bond market was very limited in 2017. For example, according to the *Financial Times*, in 2017, ESG funds attracted only 1.3% of net annual flows to bond funds. Second, even if bond investors have preferences for firms with female leadership, they are also constrained by the need for diversification. Our equity sample consists of 1,436 firms, of which 376 firms have at least one female executive. However, less than 30% of these firms have bonds outstanding (298 firms without and 113 firms with female leadership). Moreover, most bond investors will already specialize in certain credit risks (investment grade versus noninvestment grade), thereby further reducing the investable universe. Using data from the eMAXX database, we verify that institutional ownership of bonds does not change around the Weinstein/#MeToo events.

TABLE 10
Bond Yield Spreads and Female Leadership

	Yield Spread			
Female Variable =	Fraction Top-5 Women (1)	Fraction Top-5 Women (2)	Indicator Top-5 Women (3)	Indicator Top-5 Women (4)
Female Variable × Post	−0.085	−0.022	−0.013	−0.007
	(0.68)	(0.89)	(0.77)	(0.86)
Illiquidity	0.830	1.071*	0.828	1.071*
	(0.27)	(0.08)	(0.27)	(0.08)
Duration	0.086***	0.085***	0.086***	0.085***
	(<0.01)	(<0.01)	(<0.01)	(<0.01)
Probability of Default	0.446**	0.384***	0.445**	0.384***
	(0.02)	(<0.01)	(0.02)	(<0.01)
Credit Rating	0.168***	0.170***	0.168***	0.170***
	(<0.01)	(<0.01)	(<0.01)	(<0.01)
Log (Offering Amount)	0.030	0.034*	0.030	0.034*
	(0.13)	(0.10)	(0.13)	(0.10)
Time-to-maturity	0.003	0.003	0.003	0.003
	(0.45)	(0.42)	(0.45)	(0.42)
Coupon	0.037***	0.036***	0.037***	0.036***
	(<0.01)	(<0.01)	(<0.01)	(<0.01)
Log (Market Equity)		0.171		0.171
		(0.11)		(0.11)
Profitability		−0.001		−0.001
		(0.99)		(0.99)
Inverse Leverage		−0.000***		−0.000***
		(0.01)		(<0.01)
Coverage 1		−0.262**		−0.262**
		(0.02)		(0.02)
Coverage 2		−0.019		−0.019
		(0.63)		(0.63)
Coverage 3		−0.004		−0.004
		(0.88)		(0.89)
Coverage 4		−0.066**		−0.066**
		(0.05)		(0.05)
Log (Volatility)		0.213**		0.213**
		(0.04)		(0.04)
Firm Fixed Effects	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes
N	34,273	32,971	34,273	32,971
Adjusted R^2	0.867	0.880	0.867	0.880

This table shows regression results of monthly bond yield spreads on female × *Post*, control variables, and firm and time (year-month) fixed effects. The dependent variable, *Yield Spread*, is calculated as the difference between the bond yield and the Treasury yield matched by maturity. The female variables are *Fraction Top-5 Women*, which is the fraction of female executives among the five highest-paid executives of the company; and *Indicator Top-5 Women*, which is a dummy variable that equals one if a firm has at least one female executive among the five highest-paid executives, and zero otherwise. The control variables are: *Illiquidity* (the Amihud illiquidity measure), *Duration* (modified duration), *Probability of Default* (probability that a firm will default on its obligations over the next 12 months), *Credit Rating* (conversion of credit ratings into rank variables; AAA = 1, …, C = 21), *Offering Amount* (face value of the bond issue), *Time-to-maturity* (time-to-maturity in months), *Coupon* (fixed coupon of the bond), *Market Equity* (equity market capitalization), *Profitability* (operating income before depreciation / net sales), *Inverse Leverage* ((market value of equity + book value of ST and LT debt) / (book value of ST debt + 0.5 × book value of LT debt),

(*Continued*)

T A B L E 1 0—*(Continued)*

Coverage 1 to 4 (interest coverage ratio groups; (operating income after depreciation + interest expense) / interest expense; sorted into four groups based on the following cutoffs: 0–5, 5–10, 10–20, 20–100, and ratios above 100 are truncated at 100), and *Volatility* (standard deviation of daily stock returns for the previous 252 trading days). The data are from CRSP, Execucomp, Compustat, TRACE, Mergent FISD, and the Credit Research Initiative of the Risk Management Institute at the National University of Singapore. The sample period is October 2016 to September 2018, covering 24 months surrounding the events. *Post* is a dummy variable equal to zero for the months prior to October 2017 and equal to one for the months after October 2017. Standard errors are double clustered by firm and time, and *p*-values are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Under this scenario, post-event yield spreads would have increased in anticipation of higher litigation risk, at least in the short term.[27]

The results on investor type discussed in the previous subsection, which indicate that the changes in ownership are caused by long-term dedicated investors, also do not support the litigation risk hypothesis. This hypothesis would imply that *all* investors reduce their relative holdings of firms more subject to litigation risk, that is, firms without female leadership, but we find smaller changes in relative ownership for transient investors.

Despite the lack of evidence on increased risk and litigation, it remains possible that the events we study increased the likelihood of future revelations of sexual harassment, which could lead to a negative stock price response, even if not accompanied by litigation. Billings, Klein, and Shi [2022] also allude to this possibility. However, the stock price reaction associated with the revelation of *actual* cases of sexual harassment is much smaller than the effect we document: Borelli-Kjaer, Schack, and Nielsson [2021] report average announcement returns of −1.5% for 199 cases of actual sexual harassment revealed over the period 2005 to 2018. Thus, given our return differential of about 1.3%, this explanation would have to imply that every single firm in our sample without women in top leadership positions (and none of the firms with women in top leadership positions) experienced allegations of sexual harassment post-Weinstein/#MeToo. This is clearly not plausible.[28]

5.2 IMPROVEMENTS IN OPERATING PERFORMANCE

The stock market returns that we document could stem from a belief that other stakeholders will reward firms with greater gender diversity (e.g., by buying their products). If this were the case, we should observe relatively stronger operating performance for firms with top female executives after the Weinstein and #MeToo events.

---

[27] We also find no significant increase in the relative probability of default in the post-event period for firms without female leaders.

[28] Using the dataset of Borelli-Kjaer et al. [2021], we find that, for our sample, 1.1% of the firms with female leadership experience at least one episode of sexual harassment post-Weinstein/#MeToo compared with 2.9% of the firms without female leadership. Although the difference between the two groups is statistically significant, the occurrence of such events is too low to explain the stock price effect we document.

To examine this conjecture, we compute changes in four metrics: operating income to sales, gross margin (defined as sales less cost of goods sold divided by sales), growth in sales relative to the same quarter in the previous year, and sales per employee (calculated as quarterly sales divided by the number of employees measured at the end of the fiscal year). These measures are computed using quarterly Compustat data over two periods surrounding our event window. The pre-period includes quarters ending between January 2016 and September 2017, and the post-period comprises quarters ending between January 2018 and December 2020. We estimate a diff-in-diff regression of each performance metric on the interaction of our measure of female leadership with a post-event dummy, which is zero for all quarters before October 2017, and one for quarters starting in January 2018. The model also includes the log of total assets to control for size, firm fixed effects to control for unobservable time-invariant firm characteristics, and time (year-quarter) × industry fixed effects to control for any time-varying industry performance.

The results are presented in table 11. Panel A reports results using *Fraction Top-5 Women* and panel B using *Indicator Top-5 Women* as explanatory variables. Both panels yield similar insights: there is no difference in the change in operating performance of firms with female leadership relative to other firms. This evidence suggests that our findings are unlikely to be caused by the anticipation of monetary payoffs, and, as such, further supports our interpretation that our findings are due to changes in investors' nonmonetary preferences. We recognize, however, that real effects may take longer to materialize or that the changes in diversity documented previously may have had a positive effect on the relative performance of firms with fewer female executives, thereby offsetting any enhanced operating performance of firms with more female executives. Moreover, it is possible that equity investors expected an increase in cash flows, which turned out not to happen. It is also important to note that these results do not imply that nonmonetary preferences dominate monetary preferences, only that changes in nonmonetary preferences also have stock price effects.

5.3 UNDERVALUATION OF FIRMS WITH A NONSEXIST CORPORATE CULTURE

Another alternative explanation is that firms with top female executives were undervalued prior to the events (see Pedersen, Fitzgibbons, and Pomorski [2021]); given this undervaluation, firms with fewer female top executives did not spend resources to improve gender diversity prior to the shock, as this would have reduced their valuation. The Weinstein/#MeToo shock reduced this undervaluation, consistent with the price changes documented in table 2.

To investigate this alternative explanation, we study whether investors underreacted to earnings news of firms with top female executives prior to Weinstein/#MeToo, and whether this underreaction got corrected afterward. If firms with female executives are systematically undervalued, we would ex-

TABLE 11

Operating Performance Surrounding the Weinstein and #MeToo Events

Panel A: Fraction Top-5 Women

	Operating Income to Sales (1)	Gross Margin (2)	Sales Growth (3)	Sales per Employee (4)
Fraction Top-5 Women × Post	−0.014	−0.005	0.016	−0.016
	(0.36)	(0.75)	(0.63)	(0.22)
Log (Total Assets)	0.044***	0.023***	0.182***	0.037***
	(<0.01)	(0.01)	(<0.01)	(<0.01)
Firm Fixed Effects	Yes	Yes	Yes	Yes
Time × Industry Fixed Effects	Yes	Yes	Yes	Yes
N	25,309	27,065	27,057	26,292
Adjusted R^2	0.718	0.871	0.313	0.944

Panel B: Indicator Top-5 Women

	Operating Income to Sales (1)	Gross Margin (2)	Sales Growth (3)	Sales per Employee (4)
Indicator Top-5 Women × Post	−0.003	−0.002	0.009	−0.006
	(0.45)	(0.52)	(0.31)	(0.16)
Log (Total Assets)	0.044***	0.023***	0.182***	0.037***
	(<0.01)	(0.01)	(<0.01)	(<0.01)
Firm Fixed Effects	Yes	Yes	Yes	Yes
Time × Industry Fixed Effects	Yes	Yes	Yes	Yes
N	25,309	27,065	27,057	26,292
Adjusted R^2	0.718	0.871	0.313	0.944

This table presents regressions of quarterly operating performance measures on interaction terms of female × *Post*, control variables, and firm and time (quarter) by industry fixed effects. The female variables are *Fraction Top-5 Women,* which is the fraction of female executives among the five highest-paid executives of the company (in panel A); and *Indicator Top-5 Women,* which is a dummy variable that equals one if a firm has at least one female executive among the five highest-paid executives, and zero otherwise (in panel B). *Post* is a dummy variable equal to zero for quarters ending between January 2016 and September 2017, and equal to one for quarters ending between January 2018 and December 2020. All operating performance measures are computed using quarterly Compustat data. *Operating Income to Sales* is quarterly operating income before depreciation divided by quarterly sales; *Gross Margin* is quarterly sales less cost of goods sold divided by quarterly sales; *Sales Growth* is growth in quarterly sales compared with the same quarter (q) of the prior year ($y − 1$) calculated as ($sales_{q,y}$ / $sales_{q,y−1}$) − 1; and *Sales per Employee* is quarterly sales divided by the number of employees measured at the end of the fiscal year. The female variables are measured at the end of the most recent fiscal year prior to January 1, 2016. The model also includes *Log(Total Assets)* to control for size, firm fixed effects to control for unobservable time-invariant firm characteristics, and time (year-quarter) by industry fixed effects to control for any time-varying industry performance. The data are from Execucomp and Compustat. All operating performance measures are winsorized at the 1st and 99th percentiles. Standard errors are double clustered by firm and time (fiscal-year-quarter), and *p*-values are reported in parentheses. *** and ** denote statistical significance at the 1% and 5% levels, respectively.

pect investors to underreact to positive earnings news insofar as they believe that these earnings are not going to persist in the future. Furthermore, this underreaction should be mitigated after the events. Using analyst consensus forecasts from IBES, we compute earnings surprises from January 2016 to December 2020 (dropping announcements made during October 2017)

as: (actual earnings – forecasted earnings) / stock price measured at the most recent IBES statistical period before the earnings announcement. Cumulative abnormal returns around the earnings announcements are computed using the market model for the period $t = -1$ to $t = +1$, where $t = 0$ is the earnings announcement date.[29] We then estimate a regression of the announcement return as a function of the earnings surprise, a post-event dummy, and the various interactions of the earnings surprise, the post-event dummy, and the female leadership measures. Firm fixed effects and time (year-quarter) × industry fixed effects are also included.

We are particularly interested in the coefficients on (1) the interaction between the earnings surprise and the female leadership measure, which indicates whether the earnings response is different for firms with female leaders; and (2) the triple interaction between the earnings surprise, female leadership, and the post-event dummy, which shows whether the earnings response for firms with female leaders changes after the Weinstein/#MeToo events. The results are displayed in table 12. Not surprisingly, both models indicate that announcement returns are higher for larger earnings surprises. However, none of the interaction terms of interest are significant, which implies that the stock price reaction to earnings surprises is not related to the presence of female top executives either in the pre- or post-event periods. These findings therefore do not support the undervaluation hypothesis.

5.4 CONSTRAINTS IN FEMALE LABOR SUPPLY

Hitherto, we have interpreted the lack of women in top leadership positions as a reflection of a sexist corporate culture and attributed our results to changes in investor preferences around the Weinstein/#MeToo events.

A potential concern with this interpretation is that our measure of female leadership may reflect supply constraints in certain industries rather than gender (in)equality, a factor often mentioned to explain the lack of women at the top. Thus, firms with fewer women executives do not have a more sexist culture, they simply cannot find or attract women qualified to take on these roles. The Weinstein and #MeToo events increase pressure from investors or society at large on firms to hire women and, faced by limited supply of female labor, particularly in some industries, firms without female leaders experience lower valuations. Although this conjecture has some appeal, we offer several arguments and pieces of evidence to refute it.

At a theoretical level, if the lack of female leadership is not an indication of a more sexist culture, then there is no compelling argument as to *why* stakeholders should "force" firms to hire more women at the top after these sexism-revealing events. We would have to rely on irrationality

---

[29] The market model is estimated over 120 trading days ending 20 trading days before the earnings announcement date, using the CRSP value-weighted index as the market proxy. The results are very similar when we calculate CARs over the $[0; +1]$ and $[-2; +2]$ windows.

TABLE 12
Earnings Response Coefficients Surrounding the Weinstein and #MeToo Events

Female Variable =	Cumulative Abnormal Returns [−1, +1]	
	Fraction Top-5 Women (1)	Indicator Top-5 Women (2)
Earnings Surprise	0.841***	0.727***
	(<0.01)	(<0.01)
Earnings Surprise × Post	0.044	−0.005
	(0.85)	(0.98)
Female Variable × Earnings Surprise	−0.808	0.101
	(0.66)	(0.84)
Female Variable × Post	0.010	0.003
	(0.36)	(0.32)
Female Variable × Earnings Surprise × Post	−0.597	0.042
	(0.80)	(0.95)
Firm Fixed Effects	Yes	Yes
Time × Industry Fixed Effects	Yes	Yes
N	6,599	6,599
Adjusted R^2	0.039	0.038

This table presents regression results of cumulative abnormal returns around annual earnings announcements on *Earnings Surprise*, female, *Post*, and their interaction terms. Cumulative abnormal returns around the earnings announcement are computed based on the market model for the period $t = −1$ to $t = +1$, where $t = 0$ is the earnings announcement date. The market model is estimated over 120 trading days ending 20 trading days before the earnings announcement date, using the CRSP value-weighted index as the market proxy. *Earnings Surprise* is calculated as (actual earnings − analyst consensus earnings forecast) / stock price measured at the end of the most recent IBES statistical period before the earnings announcement for earnings announcements during the January 2016 to December 2020 period. The female variables are *Fraction Top-5 Women*, which is the fraction of female executives among the five highest-paid executives of the company; and *Indicator Top-5 Women*, which is a dummy variable that equals one if a firm has at least one female executive among the five highest-paid executives, and zero otherwise. *Post* is a dummy variable equal to zero for earnings announcements made during January 2016–September 2017, and equal to one for earnings announcements made during November 2017–December 2020. All models include firm fixed effects and time (year-quarter) by industry fixed effects. The data are from CRSP, IBES, and Execucomp. All continuous variables are winsorized at the 1st and 99th percentiles (except for *Fraction Top-5 Women*). Standard errors are double clustered by firm and time, and *p*-values are reported in parentheses. *** denotes statistical significance at the 1% level.

(incorrect beliefs) whereby investors penalize firms without female leaders when the importance of having a nonsexist culture increases, *despite* these firms not having a more sexist culture. Alternatively, it could be that investors are fully rational but believe that other stakeholders (e.g., consumers) are not. Hence, the stock price underperformance of firms with no female leaders reflects society's incorrect perception of a sexist culture in such firms. If this were the case, the negative price reaction to the Weinstein and #MeToo events should be accompanied by lower subsequent operating performance, insofar as other stakeholders penalize companies without female representation at the top, and/or these firms are forced to hire female leaders when the pool of talent is limited. However, as reported in section 5.2, we do not find differences in the subsequent operating performance of firms with and without female leadership.

Notwithstanding these arguments, to further rule out the limited labor supply hypothesis, we present four pieces of evidence. First, if supply constraints are most binding in industries and/or states that have fewer women in executive positions, then firms operating in these industries/states would find it particularly difficult to attract female top executives in the post-Weinstein/#MeToo period as they seek to cater to increased preferences for firms with a nonsexist culture. To study this conjecture, we repeat our analysis of panel A of table 9 for the variable that directly captures the change in executive member gender diversity but limit the sample to firms in industries with few women in executive positions or sexist states. If supply constraints, rather than the culture of the firm, prevent firms from addressing gender imbalances in their executive teams, we would expect the post-Weinstein/#MeToo changes to be less significant in industries with fewer women in executive positions and in sexist states. In other words, firms in such industries and states would not be able to attract female executives because they are not available.

To compute the fraction of women in executive positions in each industry, we obtain data on the job patterns of women collected annually by the US EEOC from private employers with 100 or more employees or federal contractors with 50 or more employees. We use the nationally aggregated data at the six-digit NAICS code for 2015.[30] For each NAICS code, the EEOC reports the number of female and male employees in executive and senior officer positions. Because our sample firms are identified by SIC codes, we match the NAICS codes to four-digit SIC codes and compute the fraction of women in executive positions for each SIC code. Firms for which there is no match are dropped from this analysis.[31] We then define *Industries with Few Women in Executive Positions* as those industries for which this fraction is below the median.

To capture the attitude toward women at the state level, we employ two variables. The first one is state-level sexism obtained from Charles, Guryan, and Pan [2018]. They employ questions from the General Social Survey to determine whether an individual is sexist and average the survey responses across individuals in a specific state and across surveys to obtain a state-level measure.[32] *High Sexism States* are defined as those states with sexism scores above the median. The second variable is the state-level gender wage gap, which we calculate using data from the Current Population Survey for the years 2015 and 2016. This survey contains state-level data on

---

[30] We use 2015 data because the EEOC aggregates data at only the three-digit NAICS code or lower from 2016 onward.

[31] Alternatively, to avoid dropping firms that cannot be matched at the four-digit SIC code level, we match NAICS codes to three-digit, two-digit, and one-digit SIC codes, respectively, and repeat our analysis. Our findings are similar.

[32] Charles et al. [2018] combine responses to eight questions. For example, one question is whether respondents agree with the statement: "Women should take care of running their home and leave running the country up to men."

wages and many demographic characteristics. For each state, we estimate a regression of weekly pay on a female indicator variable, while controlling for various other variables that explain wages (age, education, occupation, manager position, race, metropolitan area, central city, suburbs, rural, industry, county, year, and month). The coefficient on the female indicator captures the difference in pay after controlling for observables; that is, it serves as an estimate of the gender pay gap. States with a gender wage gap above the median are defined as *High Gender Wage Gap* states.

In panel A of table OA-15, we report the results on changes in diversity for *Industries with Few Women in Executive Positions*, and for states with *High Sexism* or a *High Gender Wage Gap*. These findings contradict the labor supply constraint hypothesis.[33] The coefficients on the female variables in the post-Weinstein/#MeToo period are all negative and four out of six changes are statistically significant. Thus, firms with no female top executives improve their executive member diversity more than other firms after the Weinstein/#MeToo events, even in industries with fewer women executives or sexist states. Note that this result also contradicts the argument that women are available but unwilling to work for firms in sexist states or industries with a sexist culture. In panel B of table OA-15, we show the results for *Industries with Many Women in Executive Positions*, and for *Low Sexism States* or *Low Gender Wage Gap States*. For these subsets of firms, the effect of female leadership on post-event changes in diversity is weaker.

Second, we investigate another implication of the labor supply constraints hypothesis, which is that the pay of female executives increased post-Weinstein/#MeToo due to increased demand for their services. Based on a regression model of the log of salary and bonus as a function of gender, firm size, CEO and CFO dummies, and executive fixed effects, we find no evidence in support of this implication (table OA-17 of the online appendix).[34]

Third, the problem of limited female labor supply is most likely binding at the highest level of the organization. The analyses reported in section 3.2 show that our findings also hold using measures of female leadership below the C-suite or when we measure sexism using textual analysis based on employee reviews on Glassdoor. It is highly unlikely that these findings can be explained by the labor supply argument.

Fourth, we also reestimate our baseline models, but instead of including an indicator variable equal to one if at least one of the top-5 executives is female, we set it equal to one if the firm has more female top-5 executives than firms in its two-digit SIC code industry, thereby controlling for female

---

[33] The results for other measures of changes in diversity are reported in table OA-16 of the online appendix.

[34] In addition, in the post-Weinstein/#MeToo period, we also find no evidence that new female executives (those that enter Execucomp for the first time) are paid more than new male executives.

labor supply in the industry. Our findings persist for this alternative variable (table OA-18 in the online appendix).

6. Conclusion

This paper assesses whether a gender-equal, nonsexist corporate culture is valued by investors using events that brought to the forefront the extent to which sexism was prevalent in organizations. We show that firms that have women in their top leadership team—in which a corporate culture that tolerates sexism is less likely to be present—earn substantial excess returns relative to other firms during the days immediately following the revelation of the Harvey Weinstein scandal and the resurgence of the #MeToo movement.

We explore various mechanisms behind this revaluation effect: changes in investor preferences, increased downside risk due to litigation and expected revelations of sexual harassment, expected improvements in operating performance, resolution of undervaluation, and labor market frictions. The explanation consistent with a battery of tests is that the Weinstein/#MeToo events changed the preferences of investors for firms with a nonsexist corporate culture. Institutional investors increased their holdings of these firms, particularly when their focus prior to the events was less ESG-related. As a result, the ESG scores of their portfolios improved significantly after the events. We also document relative improvements in gender diversity after the events in firms that had fewer female executives prior to the events as these firms cater to investors' taste for firms with a nonsexist culture. Investigating in detail whether institutional investor preferences change because of their own beliefs or because of changes in their clients' beliefs is an interesting avenue for future work.

Taken together, our results show that the revelation of prevalent sexism in corporations elicited changes in investors' attitudes toward sexism, prompting corporations to improve gender diversity. As such, investors acted as a catalyst in advancing the United Nation's Sustainable Development Goal of achieving gender equality. We conclude that changes in societal attitudes toward women are filtering into capital markets in a material way and that changes in investors' nonmonetary preferences are an important mechanism through which this happens.

REFERENCES

ADAMS, R. B., and D. FERREIRA. "Women in the Boardroom and Their Impact on Governance and Performance." *Journal of Financial Economics* 94 (2009): 291–309.

ADAMS, R. B., and P. FUNK. "Beyond the Glass Ceiling: Does Gender Matter." *Management Science* 58 (2012): 219–35.

ADHIKARI, B. K.; A. AGRAWAL; and J. MALM. "Do Women Managers Keep Firms out of Trouble? Evidence from Corporate Litigation and Policies." *Journal of Accounting and Economics* 67 (2019): 202–25.

AHERN, K. R., and A. K. DITTMAR. "The Changing of the Boards: The Impact on Firm Valuation of Mandated Female Board Representation." *Quarterly Journal of Economics* 127 (2012): 137–97.

AMIRASLANI, H.; K. V. LINS; H. SERVAES; and A. TAMAYO. "Trust, Social Capital, and the Bond Market Benefits of ESG Performance." *Review of Accounting Studies* 28 (2023): 421–62.

BENNETT, B.; I. EREL; L. STERN; and Z. WANG. "Paid Leave Pays Off: The Effects of Paid Family Leave on Firm Performance." Working paper, European Corporate Governance Institute, 2021.

BERTRAND, M. "Coase Lecture – The Glass Ceiling." *Economica* 85 (2018): 205–31.

BERTRAND, M.; S. E. BLACK; S. JENSEN; and A. LLERAS-MUNEY. "Breaking the Glass Ceiling? The Effect of Board Quotas on Female Labour Market Outcomes in Norway." *Review of Economic Studies* 86 (2019): 191–239.

BERTRAND, M.; C. GOLDIN; and L. F. KATZ. "Dynamics of the Gender Gap for Young Professionals in the Financial and Corporate Sectors." *American Economic Journal: Applied Economics* 2 (2010): 228–55.

BILLINGS, M. B.; A. KLEIN; and Y. SHI. "Investors' Response to the #MeToo Movement: Does Corporate Culture Matter?" *Review of Accounting Studies* 27 (2022): 897–937.

BORELLI-KJAER, M., L. M. SCHACK; and U. NIELSSON. "#MeToo: Sexual Harassment and Company Value." *Journal of Corporate Finance* 67 (2021): 101875.

CALDER-WANG, S., and P. A. GOMPERS. "And the Children Shall Lead: Gender Diversity and Performance in Venture Capital." *Journal of Financial Economics* 142 (2021): 1–22.

CHARLES, K. K.; J. GURYAN; and J. PAN. "The Effects of Sexism on American Women: The Role of Norms vs. Discrimination." Working paper, Yale University, 2018.

CHRISTENSEN, H. B.; L. HAIL; and C. LEUZ. "Mandatory CSR and Sustainability Reporting: Economic Analysis and Literature Review." *Review of Accounting Studies* 26 (2021): 1176–248.

CORREIA, M.; S. RICHARDSON; and I. TUNA. "Value Investing in Credit Markets." *Review of Accounting Studies* 17 (2012): 572–609.

DELOITTE. "Global Human Capital Trends 2016." The New Organization: Different by Design, Deloitte University Press, 2016.

DONG, T. "Gender Salary Gap in the Auditing Profession: Trend and Explanations." *European Accounting Review*, 2022. 10.1080/09638180.2022.2113550.

DYCK, A.; K. V. LINS; L. ROTH; and H. F. WAGNER. "Do Institutional Investors Drive Corporate Social Responsibility? International Evidence." *Journal of Financial Economics* 131 (2019): 693–714.

FACCIO, M.; M. MARCHICA; and R. MURA. "CEO Gender, Corporate Risk-Taking, and the Efficiency of Capital Allocation." *Journal of Corporate Finance* 39 (2016): 193–209.

GANTCHEV, N.; M. GIANNETTI; and R. LI. "Does Money Talk? Divestitures and Corporate Environmental and Social Policies." *Review of Finance* 26 (2022): 1469–508.

GRAHAM, J. R.; J. GRENNAN; C. R. HARVEY; and S. RAJGOPAL. "Corporate Culture: Evidence from the Field." *Journal of Financial Economics* 146 (2022): 552–93.

GRENNAN, J. "A Corporate Culture Channel: How Increased Shareholder Governance Reduces Firm Value." Working paper, Duke University, 2019.

GRINBLATT, M.; S. TITMAN; and R. WERMERS. "Momentum Investment Strategies, Portfolio Performance, and Herding: A Study of Mutual Fund Behavior." *American Economic Review* 85 (1995): 1088–105.

GUISO, L.; P. SAPIENZA; and L. ZINGALES. "Does Culture Affect Economic Outcomes?" *Journal of Economic Perspectives* 20 (2006): 23–48.

GUISO, L.; P. SAPIENZA; and L. ZINGALES. "The Value of Corporate Culture." *Journal of Financial Economics* 117 (2015a): 60–76.

GUISO, L.; P. SAPIENZA; and L. ZINGALES. "Corporate Culture, Societal Culture, and Institutions." *American Economic Review: Papers & Proceedings* 105 (2015b): 336–39.

HUANG, J., and D. J. KISGEN. "Gender and Corporate Finance: Are Male Executives Overconfident Relative to Female Executives." *Journal of Financial Economics* 108 (2013): 822–39.

KIM, D., and L. STARKS. "Gender Diversity on Corporate Boards: Do Women Contribute Unique Skills?" *American Economic Review. Papers & Proceedings* 106 (2016): 267–71.

Krueger, P.; Z. Sautner; and L. T. Starks. "The Importance of Climate Risk for Institutional Investors." *Review of Financial Studies* 33 (2020): 1067–111.

Kunze, A., and A. Miller. "Women Helping Women? Evidence from Private Sector Data on Workplace Hierarchies." *Review of Economics and Statistics* 99 (2017): 769–55.

Liu, T.; C. Makridis; P. Ouimet; and E. Simintzi. "The Distribution of Non-Wage Benefits: Maternity Benefits and Gender Diversity." *Review of Financial Studies* 36 (2023): 194–234.

Matsa, D. A., and A. R. Miller. "Chipping Away at the Glass Ceiling: Gender Spillovers in Corporate Leadership." *American Economic Review, Papers & Proceedings* 101 (2011): 635–39.

O'Reilly, C. A., and J. A. Chatman. "Culture As Social Control: Corporations, Cults, and Commitment." In *Research in Organizational Behavior: An Annual Series of Analytical Essays and Critical Reviews*, edited by B. M. Staw and L. L. Cummings. Elsevier Science/JAI Press, vol. 18 (1996): 157–200.

Pastor, L.; R. F. Stambaugh; and L. A. Taylor. "Sustainable Investing in Equilibrium." *Journal of Financial Economics* 142 (2021): 550–71.

Pastor, L.; R. F. Stambaugh; and L. A. Taylor. "Dissecting Green Returns." *Journal of Financial Economics* 146 (2022): 403–24.

Pedersen, L. H.; S. Fitzgibbons; and L. Pomorski. "Responsible Investing: The ESG-Efficient Frontier." *Journal of Financial Economics* 142 (2021): 572–97.

Rockefeller Foundation, and GlobalStrategyGroup. "Women in Leadership: Tackling Corporate Culture from the Top," 2017.

Stroebel, J., and J. Wurgler. "What Do You Think about Climate Finance?" *Journal of Financial Economics* 142 (2021): 487–98.

Tate, G., and L. Yang. "Female Leadership and Gender Equity: Evidence from Plant Closure." *Journal of Financial Economics* 117 (2015): 77–97.

World Economic Forum. "The Global Gender Gap Report," 2017.