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Board Governance and Investment Sensitivity to Stock Price: International Evidence

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Abstract

This article examines the effect of board governance on investment efficiency. I use the staggered enactment of board reforms in 41 countries as a shock to board structure that exogenously improves the quality of board oversight of managers. I find that investment-Q sensitivity improves by roughly half post-reform. This effect is more pronounced for firms that are more exposed to the reforms or when external governance mechanisms are less likely to discipline managers. These findings suggest that increased board oversight strengthens managers' incentives to make investment decisions that are more in line with their firms' growth opportunities.

I. Introduction

The Q theory of investment predicts that Tobin's Q, a measure of growth opportunities, is a sufficient statistic for investment behavior (Hayashi (1982)). Yet, the efficiency of investment decisions can be compromised by agency conflicts. The problems of free cash flow (Jensen (1986), Stulz (1990)) and underinvestment (Amihud and Lev (1981), Hirshleifer and Thakor (1992)) can cause a firm's investment to deviate from the optimal level implied by Tobin's Q, leading to lower investment efficiency. Strengthening board governance, however, is often regarded by many financial economists, institutional investors, and regulators as effective in reducing agency problems. If so, does stronger board governance improve investment efficiency? In this study, I investigate whether and how a board structure that emphasizes the independence of both the board and its major committees can influence investment efficiency as measured by investment-Q sensitivity.

A board structure that emphasizes independence is well suited to protect the interests of outside shareholders, as independent directors can effectively monitor managers and reduce managerial discretion (Fama and Jensen (1983)). If there is excess free cash flow, a self-interested manager has the incentive to make investments beyond the optimal level to gain private benefits through "empire building."

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Conversely, conservative investment policies may be optimal for risk-averse CEOs with concerns about their careers or wealth diversification. In either case, independent boards can discipline self-serving managers so that they make investment decisions that are less guided by the extraction of private benefits and more in line with their firms' growth opportunities as reflected in the stock prices.

However, skeptics argue that forcing boards to become more independent does not necessarily make firms' investments more efficient. A firm's current board structure likely reflects its optimal choice after considering all of the relevant factors, and any change may therefore be unnecessary and potentially harmful. For example, Adams and Ferreira (2007) predict that too much emphasis on board independence may be harmful, as the CEO will be reluctant to share information with a board that is too independent, thus making board monitoring less effective. Additionally, independent directors can be too conservative and may impose constraints on their firms' investment policies, as they have limited access to firm-specific information and their monetary incentives are less tied to earnings growth. Thus, greater board independence may not bring expected monitoring benefits and may result in investment decisions that are less related to firm fundamentals.

These two opposing views suggest that the effect of strengthening board governance on investment efficiency is theoretically ambiguous and should ultimately be empirically examined. However, establishing a causal link is challenging because corporate boards are determined endogenously (Hermalin and Weisbach (2003), Adams, Hermalin, and Weisbach (2010)).[1] Thus, any observed relationship between proxies for board governance and investment efficiency may be due to reverse causality or to other attributes of firms that drive investment efficiency. I address this issue by using the staggered adoption of board reforms in 41 countries as a shock to board structure that exogenously increases the quality of managers' board oversight (Fauver, Hung, Li, and Taboada (2017)). The reforms require more independent directors to be on boards and audit committees, and the separation of the roles of the board chairperson and CEO, leading to more independent board structures and thus potentially more effective monitoring.[2]

My empirical framework, which is designed to overcome the endogeneity problem, is based on difference-in-differences (DiD) regressions that include both firm and year-fixed effects. This enables the identification of post-reform changes in investment-Q sensitivity for firms in reform-adopting countries relative to those located in countries with no board reforms during the same time period. Using a large sample of 162,136 firm-year observations over the 1993–2012 period, the results point to a substantial improvement in investment efficiency following board reforms. The investment-Q sensitivity increases by 56 percentage points post-reform. This finding is not driven by any particular country or industry, changes

[1]For example, a negative correlation between board independence and firm performance may occur due to reverse causality. After experiencing poor performance, firms are likely to appoint independent directors to their boards. Conversely, successful firms are likely to hire nonindependent directors, as CEOs have a greater influence on board nominations if they achieve good performance. Graham, Kim, and Leary (2020) provide evidence consistent with these effects.

[2]These reforms are legislative or regulatory interventions undertaken by the state, regulatory agencies, or stock exchanges in a country's corporate governance practices. Table IA1 in the Supplementary Material presents detailed information about the characteristics of these reforms.

in the sample composition around the reforms, or non-board reform components. Importantly, the improvement in investment-Q sensitivity is not present in the years leading up to the reforms and materializes only after the reforms become effective. The placebo test results confirm that the findings are unlikely to be driven by alternative factors unrelated to board reforms. Additional results show that of the three board-related reform components (board independence, audit committee and auditor independence, and chairman and CEO separation), board independence is the main driver of increased investment efficiency.

I present two sets of results that support a causal interpretation of my findings. In the first test, I examine the reform effect conditional on the extent to which firms are exposed to the reforms. Regardless of whether I measure reform exposure using pre-reform board attributes or actual changes in board attributes around the reforms, I find that higher levels of exposure are associated with larger increases in investment-Q sensitivity post-reform. The second test is motivated by the idea that if the reforms enable corporate boards to monitor more effectively, then the reform effect should be smaller when managers are already subject to close monitoring by established external governance mechanisms. Consistent with this prediction, I find smaller improvements in post-reform investment-Q sensitivity for firms with more long-term and less short-term institutional ownership, those in industries with greater product market competition, or those from countries with stronger investor protections.

In an additional test, I investigate the implications of post-reform increased investment efficiency for operating performance. I find that firms that more closely link their investments to stock prices post-reform subsequently achieve better operating performance. This further supports my evidence and suggests that greater board oversight makes managers invest more efficiently by selecting better-quality projects.

This study makes several important contributions to the literature. Early research investigating the effect of board independence on firm performance offers mixed results. While some studies find a positive effect (e.g., Byrd and Hickman (1992), Cotter, Shivdasani, and Zenner (1997)), others find no effect (e.g., Baysinger and Butler (1985), Bhagat and Black (2002)). These studies generally fail to account for the endogeneity of board structure (Hermalin and Weisbach (2003), Adams et al. (2010)). Only recently has research been focusing on a causal relation. Examples of recent papers using shock-based settings to investigate the value of independent boards include Nguyen and Nielson (2010), Falato, Kadyrzhanova, and Lel (2014), Guo and Masulis (2015), Masulis and Zhang (2019), and Ellis, Guo, and Mobbs (2021).[3] Adding to this literature, I use board reforms as an exogenous shock to board structure to investigate the impact of the degree of independence of the board and its major committees on investment efficiency.

This study also adds to the growing literature on board reforms, which have been shown to improve firm value (Fauver et al. (2017)), increase dividend payouts (Bae, El Ghoul, Guedhami, and Zheng (2021)), increase leverage (Driss, El Ghoul, Guedhami, and Wald (2023)), reduce cash holdings (Chen, Guedhami, Yang, and Zaynutdinova (2020)), lower crash risk (Hu, Li, Taboada, and Zheng (2020)), and

[3]Masulis (2020) provides an excellent review of this literature.

decrease IPO underpricing (Chen, Goyal, and Zolotoy (2022)). My article complements this literature by showing that board reforms improve investment efficiency.[4]

Finally, my article contributes to and extends recent studies on the sources of investment sensitivity to stock price. I provide evidence suggesting that managerial incentives are an important determinant of such sensitivity. Mclean, Zhang, and Zhao (2012) find that investment-Q sensitivity is higher in countries with stronger investor protection laws. While this study argues that external governance facilitates access to capital, I focus on how internal governance facilitates managerial learning. Several studies provide evidence of managerial learning from stock prices (Chen, Goldstein, and Jiang (2007), Foucault and Frésard (2012), and Edmans, Jayaraman, and Schneemeier (2017)). They argue that managers use increasingly informative prices to obtain new information, assuming managerial incentives are fixed. My article differs from these studies in that it shows that stronger boards incentivize managers to increasingly obtain information from stock prices, holding price informativeness fixed.

The remainder of this article proceeds as follows: Section II describes the data and summary statistics and then presents the research design. I present and discuss the results in Section III, and Section IV concludes the article.

II. Sample Construction, Summary Statistics, and Research Design

A. Sample Construction and Summary Statistics

In constructing the sample, I first select 41 countries that adopted major board reforms between 1998 and 2007, as in Fauver et al. (2017). Table IA1 in the Supplementary Material provides information on the board aspects that are affected by the reforms and the approach used to enforce them in each of the sample countries. I collect accounting data on firms headquartered in these countries from the Compustat North America and Compustat Global databases. Following the literature (e.g., Chen and Chen (2012), Peters and Taylor (2017)), I exclude financials (SIC codes 6000–6999), utilities (SIC codes 4900–4999), governmental and quasi-governmental enterprises (SIC codes 9000 and above), and firms with less than $1 million in total assets from the sample. To mitigate the impact of confounding events, I restrict the sample to a $[-5, +5]$ year window around year 0, the year immediately prior to the first year the reform is in effect. I perform several robustness tests using alternative samples and find that the results are not sensitive to the above data filters. The Appendix provides a list of all of the variables along with their definitions. I winsorize firm-level continuous variables at the top and bottom

[4]Hu et al. (2020) find that board reforms reduce crash risk, partly due to better investment efficiency. I ask a different research question, which leads to different results. In addition to the various robustness results, I show that investment efficiency improves, particularly for firms with weak board governance pre-reform, those that switch to a more independent board structure immediately after the reforms, or those that are less exposed to external governance forces. Additionally, I examine the implications of post-reform improvements in investment efficiency and show that they translate into better operating performance.

TABLE 1

Descriptive Statistics by Country and Major Reform Years

For each country in the sample, Table 1 presents the number and percentage of firm-year observations and firms and major reform years (Fauver et al. (2017)). The sample is composed of 162,136 firm-year observations representing 27,616 unique firms from 41 unique countries over the 1993–2012 period.

	No. of Obs.		Firms		Major Reforms
Country	*N*	%	*N*	%	Year
Argentina	290	0.18	50	0.18	2001
Australia	7,903	4.87	1,631	5.91	2004
Austria	580	0.36	110	0.40	2004
Belgium	815	0.50	124	0.45	2005
Brazil	1,046	0.65	226	0.82	2002
Canada	9,490	5.85	1,836	6.65	2004
Chile	602	0.37	101	0.37	2001
China	5,950	3.67	1,327	4.81	2001
Colombia	92	0.06	16	0.06	2001
Czech Republic	91	0.06	20	0.07	2001
Denmark	905	0.56	142	0.51	2001
Egypt	141	0.09	49	0.18	2002
Finland	1,020	0.63	141	0.51	2004
France	4,720	2.91	832	3.01	2003
Germany	4,472	2.76	779	2.82	2002
Greece	1,035	0.64	230	0.83	2002
Hong Kong	6,077	3.75	848	3.07	2005
Hungary	152	0.09	22	0.08	2003
India	3,927	2.42	887	3.21	2002
Indonesia	2,277	1.40	325	1.18	2007
Israel	401	0.25	98	0.35	2000
Italy	1,717	1.06	247	0.89	2006
Japan	23,984	14.79	3,376	12.22	2002
Malaysia	5,202	3.21	833	3.02	2001
Mexico	576	0.36	92	0.33	2001
Netherlands	1,288	0.79	192	0.70	2004
Norway	1,344	0.83	250	0.91	2005
Pakistan	798	0.49	156	0.56	2002
Peru	361	0.22	63	0.23	2005
Philippines	821	0.51	119	0.43	2002
Poland	760	0.47	174	0.63	2002
Portugal	357	0.22	68	0.25	2001
Singapore	3,443	2.12	553	2.00	2003
South Korea	2,092	1.29	421	1.52	1999
Spain	915	0.56	133	0.48	2006
Sweden	2,722	1.68	430	1.56	2006
Switzerland	1,559	0.96	222	0.80	2002
Thailand	2,614	1.61	407	1.47	2002
Turkey	683	0.42	180	0.65	2002
UK	9,729	6.00	1,713	6.20	1998
USA	49,185	30.34	8,193	29.67	2003
All countries	162,136	100.00	27,616	100.00	–

1% to reduce the effects of outliers. I lag all of the control variables by 1 year. The baseline sample consists of 162,136 firm-year observations for 27,616 unique firms from 41 countries over the 1993–2012 period.

Table 1 shows the number and percentage of firm-year observations and sample firms, as well as the major reform year for each country included in the sample. U.S. firms represent nearly 30% of the sample firms, and Japanese firms also constitute a large proportion.

Table 2 presents the descriptive statistics for the variables in Panel A and the correlation coefficient estimates of the variables of main interest in Panel B. Investment (INVEST) is positively correlated with an indicator variable for the post-reform period (POST) and with Tobin's Q, and it is negatively correlated with cash flow (CF).

TABLE 2

Summary Statistics and Correlations

Panel A of Table 2 presents the number, mean value, standard deviation, 25th percentile, median value, and 75th percentile of the variables used in the regressions. All variables are as defined in the Appendix. Panel B reports the pairwise Pearson correlation coefficients among variables used in the baseline regressions. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. The sample is composed of 162,136 firm-year observations representing 27,616 unique firms from 41 unique countries over the period of 1993 to 2012.

Panel A. Summary Statistics

	N	Mean	Std. Dev.	p25	Median	p75
Dependent Variables						
INVEST	162,136	0.077	0.208	−0.012	0.029	0.112
CAPX	155,800	0.067	0.098	0.015	0.036	0.077
CAPX_RD	155,800	0.102	0.141	0.023	0.056	0.120
CAPX_RD_SGA	144,067	0.360	0.358	0.132	0.255	0.460
ASSETS_GR	162,136	0.126	0.495	−0.058	0.038	0.171
SALES_GR	147,513	0.161	0.486	−0.024	0.069	0.206
ROA	152,488	0.028	0.263	0.021	0.082	0.137
Board Reform Variables						
POST	162,136	0.620	0.485	0.000	1.000	1.000
POST (board independence)	162,136	0.485	0.500	0.000	0.000	1.000
POST (audit committee and auditor independence)	162,136	0.599	0.490	0.000	1.000	1.000
POST (chairman and CEO separation)	162,136	0.166	0.372	0.000	0.000	0.000
POST (non-board components)	162,136	0.417	0.493	0.000	0.000	1.000
GRACE	162,136	0.077	0.266	0.000	0.000	0.000
Firm-Level Explanatory Variables						
Q	162,136	1.873	1.946	0.960	1.266	1.965
CF	162,136	0.025	0.240	0.014	0.068	0.123
SIZE	162,136	4.669	2.093	3.234	4.571	5.988
LEVERAGE	162,136	0.229	0.218	0.037	0.191	0.352
TANGIBILITY	162,136	0.303	0.231	0.110	0.259	0.446
CASH	162,136	0.174	0.197	0.036	0.103	0.232
AGE	162,136	2.160	0.752	1.792	2.197	2.639
CROSSLIST	112,951	0.110	0.313	0.000	0.000	0.000
MOST_SENSITIVE	162,136	0.284	0.451	0.000	0.000	1.000
LEAST_SENSITIVE	162,136	0.335	0.472	0.000	0.000	1.000
MOST_IMPACTED_EXANTE	33,089	0.686	0.668	0.000	1.000	1.000
MOST_IMPACTED_EXPOST	28,980	0.139	0.385	0.000	0.000	0.000
IO_HI	71,324	0.514	0.499	0.000	1.000	1.000
LTIO_HI	71,324	0.516	0.499	0.000	1.000	1.000
STIO_HI	71,324	0.514	0.499	0.000	1.000	1.000
Industry-level explanatory variables						
HHI_LO	159,737	0.834	0.372	1.000	1.000	1.000
Country-Level Explanatory Variables						
ITL	162,136	0.942	0.233	1.000	1.000	1.000
GDP_GR	162,136	3.340	2.656	1.779	3.086	4.487
CPI	162,136	2.232	3.353	1.156	2.188	2.853
ADRI_HI	152,906	0.294	0.455	0.000	0.000	1.000

Panel B. Pearson Correlation Coefficients (N = 162,136)

	INVEST	POST	Q	CF
INVEST	1			
POST	0.010***	1		
Q	0.322***	−0.033***	1	
CF	−0.030***	0.016***	−0.235***	1

B. Research Design

To test the impact of board reforms on investment-Q sensitivity, I exploit the staggered adoption of board reforms across countries and use a DiD approach. I measure the difference in the post-reform change in investment-Q sensitivity between firms located in countries with board reforms and those in countries with no such reforms during the same years. I implement this using the following regression model:

$$(1)\quad \mathrm{INVEST}_{i,j,t} = \beta_1 \mathrm{POST}_{j,t} + \beta_2 Q_{i,j,t-1} + \beta_3 \mathrm{POST}_{j,t} \times Q_{i,j,t-1} + \beta_4 \mathrm{FIRMCTRL}_{i,j,t-1} + \beta_5 \mathrm{COUNTRYCTRL}_{j,t-1} + \mathrm{FIRM}_i + \mathrm{YEAR}_t + \varepsilon_{i,j,t},$$

where $\mathrm{INVEST}_{i,j,t}$ is an investment measure for firm i in country j in year t; $\mathrm{POST}_{j,t}$ is an indicator variable for the post-reform period; $Q_{i,j,t-1}$ is Tobin's Q; $\mathrm{FIRMCTRL}_{i,j,t-1}$ is a set of firm-level control variables; $\mathrm{COUNTRYCTRL}_{j,t-1}$ is a set of country-level control variables; FIRM_i is the firm-fixed effects; and YEAR_t is the year-fixed effects.

In the baseline specification, the dependent variable, $\mathrm{INVEST}_{i,j,t}$, is defined as change in property, plant, and equipment plus change in inventory plus R&D expense, scaled by lagged total assets. The key variable, $\mathrm{POST}_{j,t}$, equals 1 beginning in the year the board reform becomes effective, and 0 otherwise. The normalized stock price variable, $Q_{i,j,t-1}$, is defined as the book value of total assets minus the book value of equity plus the market value of equity, scaled by the book value of total assets. Following the literature (e.g., Asker, Farre-Mensa, and Ljungqvist (2015)), I control for the following firm-level variables: cash flow to asset (CF), the natural logarithm of market value of equity (SIZE), total debt to assets (LEVERAGE), cash to assets (CASH), and the natural logarithm of firm age in years (AGE). I also control for the GDP growth rate (GDP_GR) and inflation rate (CPI). My main interest is the impact of country-level shocks to governance resulting from board reforms, so I cluster standard errors at the country level, as recommended by Petersen (2009).

The specification of equation (1) is an augmented version of the standard investment equation used in the literature (Fazzari, Hubbard, and Petersen (1988), Baker, Stein, and Wurgler (2003)), with $\mathrm{POST}_{j,t}$ and its interaction with $Q_{i,j,t-1}$ added to the regression model. The treatment (control) group consists of firms located in countries after (before) their board reforms become effective. The inclusion of firm and year-fixed effects enables the within-firm and within-year change in investment-Q sensitivity between treatment and control firms to be identified. Thus, the coefficient β_3 identifies the average residual change in investment-Q sensitivity around the reform years for treatment firms relative to benchmark firms. If board reforms improve (reduce) investment-Q sensitivity, then I should expect β_3 to be positive (negative).

III. Empirical Results

A. Board Reforms and Investment-Q Sensitivity

Table 3 reports the estimation results of equation (1). Column 1 gives the results without the variable POST and its interaction with Q. Consistent with the literature (e.g., Baker et al. (2003), Chen et al. (2007)), I find that investment is positively and significantly related to both Q and CF. Smaller firms or firms with more cash or less debt invest more. Investment is negatively related to firm age, although the relation is statistically weak. Finally, I find that a higher GDP growth or inflation rate is associated with a higher level of investment. In column 2, I add the variable POST and find that firms increase their investments following the reforms.

TABLE 3

Board Reforms and Investment-Q Sensitivity

Table 3 reports regression results, where the dependent variable is investment (INVEST). In column 1, the explanatory variables are Tobin's Q, cash flow (CF), firm size (SIZE), leverage (LEVERAGE), cash (CASH), age (AGE), GDP growth rate (GDP_GR), and inflation rate (CPI). Column 2 adds POST, a dummy variable that equals 1 beginning in the year a major board reform becomes effective in a country, and 0 otherwise. Column 3 adds the interaction POST × Q. All variables are as defined in the Appendix. Year and firm-fixed effects are included in all regressions. Standard errors are clustered at the country level. *t*-statistics are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	1	2	3
POST		0.015***	−0.010*
		(2.900)	(−1.773)
Q	0.029***	0.029***	0.025***
	(14.598)	(14.726)	(20.036)
POST × Q			0.014***
			(5.961)
CF	0.024***	0.023***	0.025***
	(3.584)	(3.469)	(4.290)
SIZE	−0.014***	−0.014***	−0.016***
	(−5.635)	(−5.589)	(−6.018)
LEVERAGE	−0.147***	−0.147***	−0.155***
	(−9.005)	(−8.990)	(−10.662)
CASH	0.187***	0.187***	0.182***
	(6.304)	(6.315)	(6.337)
AGE	−0.011	−0.011	−0.014*
	(−1.574)	(−1.571)	(−1.737)
GDP_GR	0.003**	0.002**	0.003**
	(2.637)	(2.357)	(2.687)
CPI	0.002**	0.002***	0.002***
	(2.536)	(2.858)	(2.834)
Year-fixed effects	Yes	Yes	Yes
Firm-fixed effects	Yes	Yes	Yes
No. of obs.	162,136	162,136	162,136
Countries	41	41	41
Adj. R^2	0.344	0.345	0.348

To examine my research question, I add the interaction POST × Q in column 3 and find that its coefficient estimate is positive and significant. The evidence is consistent with the hypothesis that stronger board governance improves investment efficiency. Investment-Q sensitivity increases by 0.014/0.025 = 56% after the reforms. This magnitude is economically significant and larger than the 38% improvement in investment-Q sensitivity following the enactment of insider trading laws (Edmans et al. (2017)).

B. Robustness Tests

1. Alternative Samples

In this section, I address sample composition issues and report the estimation results of regressions using alternative samples in Panel A of Table 4. First, my evidence may be driven by U.S. firms because they represent roughly 30% of the sample, as shown in Table 1. To address this concern, I exclude U.S. firms from the sample. I further exclude Canadian firms because they typically have strong economic ties with their U.S. counterparts. I then reestimate equation (1) based on this reduced sample and report the estimation results in column 1. Investment-Q

TABLE 4

Robustness Tests

Table 4 examines the robustness of the effect of board reforms on investment-Q sensitivity. In Panel A, each column reports the estimation results of a replication of the regression specification in column 3 of Table 3 using a different sample or estimation method. Column 1 uses a sample excluding the U.S. and Canada. Column 2 uses a sample excluding Japan. Column 3 uses weighted least squares (WLS) as an alternative estimation method to OLS. Column 4 uses a matched sample constructed as described in the text. Column 5 uses a sample excluding firms with extreme ownership structures. Column 6 uses a sample that excludes all firms that appear only in the pre- or post-reform period. Column 7 uses a sample of industrial firms only. Column 8 uses a sample excluding non-board reforms. Column 9 uses a sample that excludes firms with less than $10 million in total assets. Column 10 uses a sample restricted to [−3, +3]. Column 11 uses the full sample over the 1990–2015 period (i.e., unrestricted to [−5, +5]). Column 12 uses a sample of first board reforms. In Panel B, each column reports the estimation results of a replication of the regression specification in column 3 of Table 3 using alternative dependent variables, additional control variables, or year dummy variables around the reforms. Column 1 uses capital expenditures (CAPX) as a dependent variable. Column 2 uses capital expenditures plus R&D expense (CAPX_RD) as a dependent variable. Column 3 uses capital expenditures plus R&D expense plus selling, general, and administrative expense (CAPX_RD_SGA) as a dependent variable. Column 4 uses assets growth (ASSETS_GR) as a dependent variable. Column 5 controls for POST × CF. Column 6 controls for ITL × Q, where ITL is an indicator variable for the enactment of an insider trading law. Column 7 controls for CROSSLIST × Q, where CROSSLIST is an indicator for cross-listing on a U.S. exchange. Column 8 controls for GRACE × Q, where GRACE is an indicator variable for the reform grace period. Column 9 uses the following year dummy variables: YEAR −1, YEAR 0, YEAR 1, and YEAR 2+, which equal 1 if the firm is headquartered in a country that will adopt a board reform in 2 years, will adopt a board reform in 1 year, adopts a board reform in the current year, and adopted a board reform 1 or more years ago, respectively. Each of these indicator variables is interacted with Q. The estimated coefficients on other controls (SIZE, LEVERAGE, CASH, AGE, GDP_GR, and CPI) are unreported for brevity. All variables are as defined in the Appendix. Year and firm-fixed effects are included in all regressions. Standard errors are clustered at the country level. *t*-statistics are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A. Alternative Samples and Estimation Methods

	Excluding the USA and Canada	Excluding Japan	WLS	Matched Sample	Excluding Special Ownership Structures	Restricted Sample	Industrials Only	Excluding Nonboard Reforms	Excluding Small Firms	[−3, +3]	Full Sample	First Board Reforms
	1	2	3	4	5	6	7	8	9	10	11	12
POST	−0.008	−0.012*	−0.021***	0.002	−0.009	−0.013**	−0.011*	−0.010	−0.007	−0.015**	−0.001	−0.015**
	(−1.282)	(−1.946)	(−3.342)	(0.371)	(−1.205)	(−2.585)	(−1.749)	(−1.074)	(−1.299)	(−2.463)	(−0.252)	(−2.658)
Q	0.018***	0.025***	0.025***	0.025***	0.017***	0.024***	0.030***	0.027***	0.021***	0.022***	0.028***	0.024***
	(6.895)	(20.785)	(59.696)	(6.455)	(9.646)	(21.955)	(17.831)	(5.849)	(19.894)	(16.278)	(14.744)	(19.870)
POST × Q	0.013***	0.014***	0.015***	0.011***	0.012***	0.013***	0.011***	0.020***	0.011***	0.015***	0.006**	0.014***
	(4.643)	(6.171)	(10.352)	(3.270)	(3.618)	(11.456)	(5.253)	(4.991)	(3.233)	(5.055)	(2.059)	(6.819)
CF	0.041***	0.024***	0.019***	0.034	0.049***	0.032***	0.033***	0.019*	0.065***	0.027***	0.062***	0.034***
	(3.994)	(4.397)	(9.860)	(1.679)	(4.520)	(4.653)	(2.757)	(2.130)	(8.804)	(3.407)	(5.845)	(5.288)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	103,461	138,152	162,136	26,827	76,460	121,415	83,925	51,021	147,762	103,791	377,072	150,734
Countries	39	40	41	41	20	41	41	11	41	41	41	41
Adj. R^2	0.232	0.340	0.425	0.542	0.191	0.305	0.414	0.310	0.335	0.386	0.298	0.349

(continued on next page)

TABLE 4 (continued)
Robustness Tests

Panel B. Other Robustness Tests

	Alternative Dependent Variables				Additional Control Variables				Reform Timing
	CAPX	CAPX_RD	CAPX_RD_SGA	ASSETS_GR	INVEST	INVEST	INVEST	INVEST	INVEST
	1	2	3	4	5	6	7	8	9
POST	0.001 (0.768)	−0.001 (−0.294)	−0.004 (−0.372)	−0.009 (−0.536)	−0.008 (−1.379)	−0.009* (−1.764)	−0.009 (−1.525)	−0.008 (−0.933)	
Q	0.008*** (12.021)	0.018*** (12.734)	0.054*** (11.916)	0.106*** (34.504)	0.025*** (19.806)	0.008*** (3.744)	0.020*** (5.603)	0.024*** (23.828)	0.025*** (26.761)
POST × Q	0.003** (2.177)	0.005*** (3.003)	0.018*** (5.293)	0.051*** (8.204)	0.013*** (6.006)	0.014*** (6.071)	0.016*** (5.018)	0.014*** (5.323)	
CF	0.009*** (7.894)	0.003 (0.708)	−0.083*** (−5.185)	0.072*** (4.273)	0.043*** (3.298)	0.025*** (4.329)	0.032*** (3.754)	0.025*** (4.666)	0.025*** (4.810)
POST × CF					−0.034** (−2.562)				
ITL						−0.078*** (−10.202)			
ITL × Q						0.017*** (6.170)			
CROSSLIST × Q							0.006*** (2.999)		
GRACE								−0.002 (−0.225)	
GRACE × Q								0.004 (0.871)	
YEAR −1									0.007 (0.804)
YEAR 0									0.005 (0.380)

(continued on next page)

TABLE 4 (continued)

Robustness Tests

Panel B. Other Robustness Tests (continued)

	Alternative Dependent Variables				Additional Control Variables				Reform Timing
	CAPX	CAPX_RD	CAPX_RD_SGA	ASSETS_GR	INVEST	INVEST	INVEST	INVEST	INVEST
	1	2	3	4	5	6	7	8	9
YEAR 1									−0.003
									(−0.206)
YEAR 2+									0.003
									(0.200)
YEAR −1 × Q									−0.002
									(−0.759)
YEAR 0 × Q									0.003
									(0.696)
YEAR 1 × Q									0.015***
									(3.041)
YEAR 2+ × Q									0.014***
									(4.843)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	155,800	155,800	144,067	162,136	162,136	162,136	112,951	162,136	162,136
Countries	41	41	41	41	41	41	40	41	41
Adj. R^2	0.555	0.622	0.745	0.234	0.348	0.348	0.276	0.348	0.348

sensitivity increases post-reform, as indicated by the positive and significant coefficient estimate on POST × Q. Second, I exclude Japan, the second major contributor to the sample, and still find evidence of a post-reform improvement in investment-Q sensitivity (column 2). Third, I use a weighted least squares (WLS) regression, where each observation is weighted by the reciprocal of the number of observations for the corresponding country. Column 3 shows that the post-reform increase in investment-Q sensitivity remains, and thus the evidence is not limited to countries with a large representation in the sample.

Fourth, I use a matched sample to reduce the possibility that differences in firm characteristics confound my results. In building this sample, I require that candidate control firms have the same 2-digit SIC code and belong to a country with similar economic development status (classified as either an emerging or developed economy) as treatment firms. I also use firm characteristics prevailing in the year immediately prior to the reform year and require that candidate control firms be in the same firm size quintile, Q quintile, and CF quintile as treatment firms. I then break any ties based on the smallest difference in firm size. To maximize the number of matched firms and due to the staggered nature of the reforms, I restrict the matched sample to observations taking place within 1 year before or after the reforms become effective. Column 4 reports the estimation results based on this sample. The estimated coefficient on POST × Q is positive and significant, suggesting that the improvement in investment-Q sensitivity post-reform is unlikely to be driven by pre-reform differences in firm characteristics.

Fifth, the presence of firms with extreme ownership structures in the sample could alter the evidence because in some countries such firms can be exempt from complying with the reform mandates or because their boards are likely to have much less influence over their decisions. To identify firms with extreme ownership structures, I use ownership data from i) Claessens, Djankov, and Lang (2000) who provide information on corporate ownership in nine East Asian countries, and ii) Faccio and Lang (2002) who provide corporate ownership information in 13 Western European countries.[5] I then drop from the sample i) firms for which the largest shareholder controls over 10% of the votes, ii) firms with dual-class shares, iii) firms with pyramid ownership structures, and iv) family-controlled firms. Column 5 reports the regression results based on this reduced sample. The estimated coefficient on POST × Q is positive and significant, suggesting that the evidence on the post-reform improvement in investment-Q sensitivity is robust to excluding firms with special ownership structures.

Sixth, I exclude all firms that appear only in the pre- or post-reform period to test whether changes in sample composition around the reforms drive my results. Column 6 shows that the evidence is robust to using this alternative sample. Seventh, other studies of investment-Q sensitivity (e.g., Fazzari et al. (1988), Chen and Chen (2012)) limit their sample to manufacturing firms. To contrast my results with theirs, I reestimate equation (1) using a sample that excludes non-manufacturing

[5]I restrict the analysis to the 20 countries in my sample that are also covered by these two studies. These countries are: Austria, Belgium, Finland, France, Germany, Hong Kong, Indonesia, Italy, Japan, Malaysia, Norway, Philippines, Portugal, Singapore, South Korea, Spain, Sweden, Switzerland, Thailand, and the U.K.

firms. Column 7 shows that the post-reform increase in investment-Q sensitivity persists with this alternative sample. Eighth, I address the concern that reforms unrelated to corporate boards may lead to my findings by excluding firms from countries in which reforms have components unrelated to corporate boards. Column 8 indicates that the post-reform increase in investment-Q sensitivity is greater after excluding reforms with non-board components, suggesting that their presence weakens rather than drives my evidence.

Ninth, I drop firms with total assets of less than $10 million and find consistent evidence, as shown in column 9. Tenth, I restrict the sample to include only observations within 3 years before or after year 0 to further reduce the potential effect of confounding events and find similar evidence (column 10). Eleventh, the evidence is consistent when using a sample without imposing a $[-5, +5]$ year window around year 0 (column 11). Finally, I redefine POST to indicate the time period following the first board reforms and test whether my evidence remains robust when using first rather than major board reforms. The first board reforms are defined as the earliest in countries with multiple reforms (see Table IA1 in the Supplementary Material). Column 12 indicates that my evidence also holds when using first board reforms.

2. Alternative Dependent Variables

The evidence suggests a post-reform increase in investment-Q sensitivity, where investment is defined as change in property, plant, and equipment plus change in inventory plus R&D expense, scaled by lagged total assets. I consider alternative measures of investment and explore variations in their sensitivity to price around the reforms.

Panel B of Table 4 reports the estimation results of equation (1), where the dependent variable is capital expenditures scaled by lagged total assets (CAPX) in column 1; capital expenditures plus R&D expense, scaled by lagged total assets (CAPX_RD) in column 2; capital expenditures plus R&D expense plus selling, general, and administrative expense,[6] scaled by lagged total assets (CAPX_RD_SGA) in column 3; and total assets growth (ASSETS_GR) in column 4.

Column 1 shows that the estimated coefficient on POST $\times$ Q is positive and significant at the 5% level. Column 2 shows that adding R&D expense to the investment measure yields a larger and more significant post-reform increase in investment-Q sensitivity. This stronger result is not unexpected, as firms tend to invest less in physical assets and more in intangible assets over time (Kahle and Stulz (2017), Stulz (2020)).[7] Column 3 shows that including selling, general, and administrative expenses in the investment measure further strengthens the post-reform increase in investment-Q sensitivity. In column 4, I use assets growth as an alternative measure of investment and find consistent evidence. Taken together, these results are in line with the finding of Peters and Taylor (2017) that Q explains total investment better than physical investment.

[6]Selling, general, and administrative expense is part of total investment because it can be viewed as capital investment-like expenditures for developing intangible assets (e.g., Mclean et al. (2012), Peters and Taylor (2017)).

[7]Kahle and Stulz (2017) find that for U.S. firms, capital expenditures (R&D expenditures) decreased (increased) from 10% (3.4%) in the 1975–1996 period to 5.91% (6.1%) in the 1997–2015 period.

3. Additional Controls

In columns 5–8 in Panel B of Table 4, I consider additional control factors. Other studies suggest that Q is measured with error and that cash flow provides information about growth opportunities beyond that reflected in stock prices (Gomes (2001), Alti (2003)). Thus, it is important to check the robustness of the evidence by controlling for changes in investment–cash flow sensitivity around the reforms. Column 5 reports the estimation results of equation (1) with the interaction POST × CF as an additional control. I find that the coefficient estimate on POST × Q remains positive and significant, so my evidence is not affected by variations in investment–cash flow sensitivity around the reforms. The estimated coefficient on POST × CF is negative and significant, suggesting a weakened relationship between cash flow and investment post-reform. To the extent that a positive investment–cash flow sensitivity reflects capital constraints (e.g., Fazzari et al. (1988), (2000), Hubbard (1998)), a less positive investment–cash flow sensitivity post-reform is consistent with the notion that board reforms help to alleviate these constraints. These findings are consistent with those of Mclean et al. (2012), who show that investment-Q sensitivity is higher and investment–cash flow sensitivity is lower in countries with stronger investor protection.

Edmans et al. (2017) find that the enforcement of insider trading laws across countries, a shock that exogenously increases the amount of new information that managers can obtain from their stock prices, improves investment-Q sensitivity. If these laws coincide with board reforms, then the observed post-reform increase in investment-Q sensitivity may be confounded by this effect. To address this concern, I control for ITL, an indicator variable for the enforcement of insider trading laws as identified by Fernandes and Ferreira (2009), and its interaction with Q. Column 6 shows that the coefficient estimate of ITL × Q is positive and significant, indicating that investment-Q sensitivity improves after insider trading laws become effective. Importantly, the coefficient on POST × Q remains positive, significant, and economically large, and thus the identified board reform effect is unrelated to variations in the information environment caused by insider trading laws.

Foucault and Frésard (2012) provide evidence that cross-listed firms in the U.S. experience an increase in their investment-Q sensitivity, as cross-listing provides more new private information to managers from their stock prices, which they can then use when making capital allocation decisions.[8] I control for this cross-listing effect by interacting Q with CROSSLIST, an indicator variable of whether a firm is cross-listed on a U.S. exchange.[9] Column 7 shows that cross-listed firms have higher investment-Q sensitivity than those that never cross-list, as indicated by the positive and significant coefficient estimate on CROSSLIST × Q. The estimated coefficient on POST × Q remains positive and significant, so the post-reform increase in investment-Q sensitivity is independent of the cross-listing effect documented by Foucault and Frésard (2012).

[8]Lins, Strickland, and Zenner (2005) show that investment–cash flow sensitivity decreases for emerging markets firms that cross-list on a U.S. exchange as cross-listing can provide better access to capital.

[9]In this test, I drop U.S firms from the sample and focus on the incremental impact of Q for cross-listed firms relative to firms that never cross-list. I do not include the variable CROSSLIST in the regression because there are no within-firm variations in this variable, and I include firm-fixed effects.

Governance reforms typically include a grace period before implementation is mandatory. To account for this, I include the variable GRACE and its interaction with Q in the regression, where GRACE is a dummy variable that is set equal to 1 during the reform grace period. Column 8 shows that the coefficient estimate on GRACE × Q is insignificant, suggesting that investment-Q sensitivity remains unchanged during the grace period. Importantly, the estimated coefficient on POST × Q is positive and significant, indicating that investment-Q sensitivity increases after the reform becomes effective.

4. Dynamic Pattern of Investment-Q Sensitivity Around Board Reforms

In this section, I test and rule out reverse causality by examining the timing of changes in investment-Q sensitivity around board reforms. A causal interpretation of the DiD results would require the investment-Q sensitivity of treatment firms to evolve similarly to that of control firms over time in the absence of board reforms (Roberts and Whited (2013)). If reverse causality drives my evidence, I should observe an improvement in the investment-Q sensitivity of treatment firms prior to the reforms.

Column 9 in Panel B of Table 4 reports the estimation results of equation (1) after replacing POST with YEAR -1, YEAR 0, YEAR 1, and YEAR 2+, which are equal to 1 if the firm is headquartered in a country that will adopt a board reform in 2 years, will adopt a board reform in 1 year, adopts a board reform in the current year, and adopted a board reform 1 or more years ago, respectively, and 0 otherwise. Consistent with the causal interpretation, I find that the post-reform increase in investment-Q sensitivity is not part of a long-term trend, but instead only occurs after the reforms become effective. The estimated coefficients on the interaction terms YEAR $-1 \times$ Q and YEAR $0 \times$ Q are insignificant, indicating that there are few pre-reform differences in investment-Q sensitivity between the treatment and control firms. By contrast, firm investment in countries that adopt the reforms becomes more sensitive to stock prices after the reforms take effect, as indicated by the positive and significant estimated coefficients on the interactions YEAR $1 \times$ Q and YEAR $2+ \times$ Q. These results suggest that board reforms lead to an improvement in investment-Q sensitivity.

5. Placebo Test

The reform timing results suggest that there is little evidence of pre-reform trends in investment-Q sensitivity. To further address the concern that alternative forces unrelated to board reforms may lead to my findings, I use a placebo test design similar to that of Bae et al. (2021) and undertake a DiD approach with placebo reforms, in which pseudo-adoption years are chosen at random. For each of the 41 countries included in the sample, I draw a placebo-reform year at random from a uniform distribution between 1990 and 2015, and construct POST_PSEUDO, a dummy variable that equals 1 beginning in the pseudo-reform year, and 0 otherwise. I then estimate equation (1) on these placebo reforms and repeat the estimation 1,000 times, each time drawing a placebo reform for each of the 41 countries at random. I aim to generate a distribution of simulated coefficient and *t*-statistic estimates when the reforms have no effect. Thus, I construct a new dependent variable, INVEST_PSEUDO, which does not consider the reform

TABLE 5

Placebo Test

Table 5 shows the results of a placebo test. In column 1, I report the actual coefficient and *t*-statistic estimates, which are identical to those in column 3 of Table 3. I report the mean values of the coefficients and *t*-statistics estimated using simulated data in column 2 and their distribution in columns 3–11. The simulation-based estimates are obtained as follows. First, for each sample country, I randomly assign the country's board reform to one of the years from 1990 to 2015, and construct POST_PSEUDO, a dummy variable that equals 1 beginning in the pseudo-reform year, and 0 otherwise. Second, I reestimate equation (1) using an alternative dependent variable that excludes the reform effect and then replacing POST with POST_PSEUDO. I repeat this 2-step process 1,000 times. The dependent variable I use is the original dependent variable, INVEST, adjusted in the years the reform is in effect. For each sample firm and each post-reform year, I subtract from INVEST the difference between the mean value of INVEST in the years the reform is in effect and the mean value of INVEST in the years the reform is not in effect. The estimated coefficients on other controls (SIZE, LEVERAGE, CASH, AGE, GDP_GR, and CPI) are unreported for brevity. All variables are as defined in the Appendix. Year and firm-fixed effects are included in all regressions. Standard errors are clustered at the country level. *t*-statistics are in parentheses.

	Actual Estimate	Simulation-Based Estimate	Distribution of Coefficient and *t*-Statistic Estimates Based on Pseudo-Reform Years								
		Mean	1%	5%	10%	25%	50%	75%	90%	95%	99%
	1	2	3	4	5	6	7	8	9	10	11
POST_PSEUDO	−0.010	−0.003	−0.015	−0.012	−0.010	−0.006	−0.003	0.000	0.003	0.005	0.009
	(−1.773)	(−0.721)	(−4.055)	(−2.944)	(−2.334)	(−1.508)	(−0.658)	(0.136)	(0.840)	(1.312)	(2.361)
Q	0.025	0.022	0.017	0.020	0.020	0.021	0.022	0.023	0.024	0.025	0.026
	(20.036)	(14.582)	(4.932)	(5.753)	(6.437)	(7.829)	(15.489)	(19.792)	(23.231)	(25.277)	(31.438)
POST_ PSEUDO × Q	0.014	0.002	−0.004	−0.002	−0.002	−0.000	0.002	0.004	0.005	0.006	0.007
	(5.961)	(1.120)	(−2.366)	(−1.284)	(−0.798)	(−0.082)	(0.866)	(2.065)	(3.476)	(4.540)	(6.115)
CF	0.025	0.050	0.049	0.049	0.049	0.049	0.049	0.050	0.050	0.050	0.050
	(4.290)	(6.106)	(5.816)	(5.888)	(5.933)	(6.023)	(6.088)	(6.197)	(6.290)	(6.343)	(6.444)

effect. For each sample firm and each post-reform year, I subtract from INVEST the difference between the mean values of INVEST in the years the reform is in effect and in the years the reform is not in effect. Through this approach, I can answer the following questions: if 1,000 researchers investigate the effects of various fictitious reforms in my data, would they find a significant overall result even if these reforms have no effect? How does the distribution of the simulation-based effects compare with the effects detected using genuine reforms?

Table 5 displays the placebo test results, which show that randomly selected reform dates yield no statistical significance, as expected. The mean value of the simulation-based estimated coefficients on POST_PSEUDO × Q is insignificant (0.002, *t*-statistic = 1.120) and is well below the corresponding actual coefficient estimate (0.014, *t*-statistic = 5.961). The distribution of the simulated coefficient estimates suggests that the likelihood of obtaining the post-reform increase in investment-Q sensitivity of 0.014 at random is less than 1%. Thus, the null hypothesis that the estimated magnitude of the reform effect is random can be rejected based on the 1% significance level. In terms of the distribution of the simulated *t*-statistic estimates, I obtain a *t*-statistic estimate of 5.961 at random in less than 5% of cases and can thus reject the null hypothesis that the estimated significance of the reform effect is false based on the 5% significance level.

C. Analysis of Reform Components and Approaches

The reforms I study in this article involve different components (board independence, audit committee and auditor independence, and chairman and CEO separation), as shown in Table IA1 in the Supplementary Material. To assess the contribution of each component to my findings, I reestimate equation (1), where

I replace POST with POST (board independence), POST (audit committee and auditor independence), or POST (chairman and CEO separation). The dummy variable POST (board independence) is set equal to 1 starting in the year when a reform involving board independence becomes effective. The dummy variables POST (audit committee and auditor independence) and POST (chairman and CEO separation) are similarly defined. I further control for the effect of concurrent non-board reforms by adding POST (non-board components), a dummy variable indicating the time period subsequent to reforms with additional non-board components, and its interaction with Q.

Panel A of Table 6 reports the estimation results. Column 1 shows that the estimated coefficient on POST (non-board components) × Q is insignificant, indicating that reforms with additional non-board components have no incremental impact beyond those involving board components. Importantly, the estimated

TABLE 6

Analysis of Components and Approaches of Board Reforms

Table 6 examines the effects of board reform components on investment-Q sensitivity using the full sample in Panel A, rules-based reform sample in Panel B, and comply-or-explain reform sample in Panel C. The dependent variable is investment (INVEST). The variable POST (board independence) is a dummy variable set equal to 1 beginning in the year a major board reform involving board independence becomes effective in a country, and 0 otherwise. The variables POST (audit committee and auditor independence), POST (chairman and CEO separation), and POST (non-board components) are similarly defined. The estimated coefficients on other controls (SIZE, LEVERAGE, CASH, AGE, GDP_GR, and CPI) are unreported for brevity. All variables are as defined in the Appendix. Year and firm-fixed effects are included in all regressions. Standard errors are clustered at the country level. *t*-statistics are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A. Full Sample

	1	2	3	4	5
POST	−0.011				
	(−1.006)				
Q	0.024***	0.024***	0.025***	0.025***	0.024***
	(20.370)	(20.630)	(21.155)	(19.236)	(21.730)
POST × Q	0.021***				
	(3.384)				
POST (board independence)		−0.029***			−0.049***
		(−3.416)			(−3.914)
POST (board independence) × Q		0.024***			0.026***
		(4.782)			(3.264)
POST (audit committee and auditor independence)			−0.010		0.014*
			(−0.966)		(1.788)
POST (audit committee and auditor independence) × Q			0.019***		−0.001
			(2.905)		(−0.192)
POST (chairman and CEO separation)				0.004	0.022*
				(0.231)	(1.721)
POST (chairman and CEO separation) × Q				0.011	−0.002
				(1.330)	(−0.242)
POST (non-board components)	−0.001	0.010	−0.002	−0.015	0.014
	(−0.073)	(0.834)	(−0.175)	(−1.197)	(1.046)
POST (non-board components) × Q	−0.009	−0.012*	−0.007	0.008	−0.011
	(−1.241)	(−1.964)	(−0.919)	(1.443)	(−1.513)
CF	0.026***	0.026***	0.025***	0.025***	0.026***
	(4.416)	(4.396)	(4.432)	(4.387)	(4.378)
Other controls	Yes	Yes	Yes	Yes	Yes
Year-fixed effects	Yes	Yes	Yes	Yes	Yes
Firm-fixed effects	Yes	Yes	Yes	Yes	Yes
No. of obs.	162,136	162,136	162,136	162,136	162,136
Countries	41	41	41	41	41
Adj. R^2	0.348	0.348	0.348	0.347	0.349

(continued on next page)

TABLE 6 (continued)

Analysis of Components and Approaches of Board Reforms

	1	2	3	4	5
Panel B. Rules-Based Reform Sample					
POST	−0.011				
	(−0.797)				
Q	0.024***	0.025***	0.025***	0.025***	0.024***
	(23.416)	(23.671)	(23.641)	(24.308)	(22.140)
POST × Q	0.023**				
	(2.873)				
POST (board independence)		−0.046***			−0.046**
		(−3.644)			(−2.300)
POST (board independence) × Q		0.030***			0.020**
		(8.086)			(2.347)
POST (audit committee and auditor independence)			−0.013		0.015*
			(−0.935)		(1.743)
POST (audit committee and auditor independence) × Q			0.023**		0.001
			(2.873)		(0.318)
POST (chairman and CEO separation)				−0.035***	−0.003
				(−3.211)	(−0.121)
POST (chairman and CEO separation) × Q				0.030***	0.010
				(6.614)	(0.899)
POST (non-board components)	−0.008	0.015	−0.007	−0.026***	0.008
	(−0.502)	(1.336)	(−0.456)	(−3.131)	(0.373)
POST (non-board components) × Q	−0.009	−0.015***	−0.009	0.013***	−0.007
	(−1.089)	(−3.798)	(−1.093)	(4.614)	(−0.757)
CF	0.021***	0.022***	0.021***	0.022***	0.022***
	(4.274)	(3.898)	(4.277)	(3.990)	(3.963)
Other controls	Yes	Yes	Yes	Yes	Yes
Year-fixed effects	Yes	Yes	Yes	Yes	Yes
Firm-fixed effects	Yes	Yes	Yes	Yes	Yes
No. of obs.	107,973	107,973	107,973	107,973	107,973
Countries	19	19	19	19	19
Adj. R^2	0.408	0.408	0.408	0.408	0.409
Panel C. Comply-or-Explain Reform Sample					
POST	−0.011				
	(−1.167)				
Q	0.022***	0.022***	0.023***	0.024***	0.022***
	(6.615)	(6.868)	(7.234)	(8.558)	(7.047)
POST × Q	0.018***				
	(5.494)				
POST (board independence)		−0.014			−0.078**
		(−1.350)			(−2.493)
POST (board independence) × Q		0.017***			0.036*
		(4.954)			(1.990)
POST (audit committee and auditor independence)			−0.002		0.058*
			(−0.157)		(2.042)
POST (audit committee and auditor independence) × Q			0.012**		−0.019
			(2.370)		(−1.145)
POST (chairman and CEO separation)				0.004	0.015
				(0.314)	(1.553)
POST (chairman and CEO separation) × Q				0.008	−0.002
				(1.451)	(−0.284)
POST (non-board components)	0.007	0.008	0.001	−0.002	0.010
	(0.677)	(0.794)	(0.057)	(−0.224)	(0.997)
POST (non-board components) × Q	−0.005	−0.004	−0.000	0.004	−0.004
	(−0.828)	(−0.648)	(−0.014)	(0.638)	(−0.712)
CF	0.033***	0.033***	0.033***	0.033***	0.033***
	(3.824)	(3.813)	(3.809)	(3.901)	(3.716)
Other controls	Yes	Yes	Yes	Yes	Yes
Year-fixed effects	Yes	Yes	Yes	Yes	Yes
Firm-fixed effects	Yes	Yes	Yes	Yes	Yes
No. of obs.	54,163	54,163	54,163	54,163	54,163
Countries	22	22	22	22	22
Adj. R^2	0.225	0.224	0.224	0.224	0.225

coefficient on POST × Q is positive and significant, suggesting that board-related components increase investment-Q sensitivity. In columns 2–4, I investigate the contribution of each board-related component and find evidence of an increase in investment-Q sensitivity following reforms involving board independence or audit committee and auditor independence but not separation of CEO and board chair positions.[10] In column 5, I include all three dummy variables corresponding to the board components being examined in the same regression to test the incremental impact of each. The estimated coefficient on POST (board independence) × Q is positive and significant, whereas those on the interactions POST (audit committee and auditor independence) × Q and POST (chairman and CEO separation) × Q are insignificant. Controlling for the effects of other reform components, board independence increases investment-Q sensitivity. Conversely, once the effect of board independence is accounted for, other board components have little impact on investment-Q sensitivity. Thus, of the three board-related components, board independence is the most important one. This finding is consistent with the governance literature in that board independence represents a major and effective corporate governance mechanism (e.g., Guo and Masulis (2015), Masulis and Zhang (2019), and Ellis et al. (2021)).

I then investigate how my findings differ with different reform approaches. As shown in Table IA1 in the Supplementary Material, reforms can be enforced using either a rules-based (e.g., the Sarbanes-Oxley Act (SOX) and updated stock exchange listing rules in the U.S.) or a comply-or-explain (e.g., the U.K. Cadbury Report) approach. The rules-based approach makes compliance mandatory, whereas the comply-or-explain approach is more flexible and allows companies to choose to explain why they do not comply. I repeat the regression analysis in Panel A of Table 6 using the rules-based and comply-or-explain reform subsamples and report the estimation results in Panels B and C of Table 6, respectively. In Panel B, I find that rules-based board reforms increase investment-Q sensitivity (column 1). Results in columns 2–4 suggest that each board component plays a role in increased investment-Q sensitivity. However, as shown in column 5, once all three board components are considered simultaneously, the effect of board independence persists, while other components no longer have a significant effect. Results in Panel C of Table 6 offer a similar conclusion for comply-or-explain reforms. Thus, regardless of the legal regime being adopted, board independence appears to be the main driver of increased investment efficiency.

[10]Related papers (e.g., Fauver et al. (2017), Bae et al. (2021), and Chen et al. (2022)) use 2003 as the first effective year for both components (board independence and audit committee and auditor independence) of the U.S. reform. Although the updated listing rules were approved by the Securities and Exchange Commission (SEC) in late 2003, compliance with the board independence requirements became mandatory only in 2004. Note that firms with staggered boards had until 2005 to meet the new exchange listing requirements on directors. To account for this, I reestimate a regression similar to that used in column 2 assuming that the U.S. reform board independence component became effective starting in 2004 (instead of 2003, as shown in Table IA1 in the Supplementary Material). In unreported results, I find that the estimated coefficient on the interaction POST (board independence) × Q remains positive and significant, suggesting that the evidence is robust to using this alternative start date. I thank the referee for suggesting this test.

D. Role of Firm Internal Governance

I examine the reform effect conditional on firm-level board attributes to further assess the causal interpretation of my findings. If firm investment becomes more sensitive to stock price in response to the stronger monitoring imposed by board reforms, then the increase in investment-Q sensitivity should be greater for firms that are more likely to benefit from enhanced monitoring. I use two measures to capture the extent to which firms are affected by the reforms. My first measure, MOST_IMPACTED_EXANTE, is a variable capturing weak boards, for which the monitoring of CEOs is likely to be poor prior to the reforms. The variable is a score incremented by 1 if i) the proportion of independent directors is less than or equal to 50%; ii) the proportion of independent audit committee members is less than or equal to 50%; or iii) the CEO is the board chair, all of which are measured in the year immediately prior to the reform (year 0). Consequently, this measure takes on values between 0 and 3, with higher values indicating less pre-reform monitoring and thus higher exposure to and benefits from the reforms.

However, one shortcoming of this measure may be that firms with weak boards may ultimately fail to comply with the governance changes mandated by the reforms, possibly due to the lack of enforcement in some countries. Thus, I use a more direct measure reflecting an actual change in firm board structure around the reforms. My second measure, MOST_IMPACTED_EXPOST, is a score incremented by 1 if i) the proportion of independent directors is less than or equal to 50% in year 0 and greater than 50% in year 2; ii) the proportion of independent audit committee members is less than or equal to 50% in year 0 and greater than 50% in year 2; or iii) the CEO is the chairman in year 0 and is not the chairman in year 2. Thus, this score variable takes on values between 0 and 3, with higher values indicating better compliance with the governance changes in the reforms.

Table 7 reports the estimation results of equation (1), which I extend by including a triple interaction term POST × Q × MOST_IMPACTED_EXANTE in column 1 and POST × Q × MOST_IMPACTED_EXPOST in column 2. In this setup, the coefficient on POST × Q measures the post-reform change in investment-Q sensitivity for firms with little exposure to the reforms, whereas the coefficients on the triple interaction terms reflect the incremental changes in investment-Q sensitivity for firms that are likely to be most affected by the reforms. Consistent with my prediction, I find that greater exposure to the reforms is associated with a larger increase in investment-Q sensitivity. In column 1, I estimate that the incremental improvement in investment-Q sensitivity for firms with one weak board attribute amounts to 0.003, which represents a 0.003/0.008 = 38% increase in investment-Q sensitivity relative to firms with little exposure to the reforms. Column 2 provides consistent and stronger evidence, as expected. Firms that had one weak board attribute prior to the reforms and that subsequently strengthened this board attribute post-reform experienced an additional improvement in investment-Q sensitivity of 0.005, which translates into a 0.005/0.008 = 63% relative increase in investment-Q sensitivity. Overall, these results provide direct evidence that strongly supports a causal interpretation of the reform effect.

TABLE 7

Board Reforms, Internal Governance, and Investment-Q Sensitivity

Table 7 examines the effect of board reforms on investment-Q sensitivity conditional on the strength of internal governance. The dependent variable is investment (INVEST). The interaction POST × Q is further interacted with the variable MOST_IMPACTED_EXANTE in column 1 and MOST_IMPACTED_EXPOST in column 2. MOST_IMPACTED_EXANTE is a score variable ranging from 0 to 3. The variable is incremented by 1 if i) the proportion of independent directors is less than or equal to 50%; ii) the proportion of independent audit committee members is less than or equal to 50%; or iii) the CEO is the chairman, all of which are measured in year 0, the year immediately prior to the reform year. MOST_IMPACTED_EXPOST is a score variable ranging from 0 to 3. The variable is incremented by 1 if i) the proportion of independent directors is less than or equal to 50% in year 0 and higher than 50% in year 2; ii) the proportion of independent audit committee members is less than or equal to 50% in year 0 and higher than 50% in year 2; or iii) the CEO is the chairman in year 0 and is not the chairman in year 2. The estimated coefficients on other controls (SIZE, LEVERAGE, CASH, AGE, GDP_GR, and CPI) are unreported for brevity. All variables are as defined in the Appendix. Year and firm-fixed effects are included in all regressions. Standard errors are clustered at the country level. *t*-statistics are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	1	2
POST	−0.004	−0.007
	(−0.600)	(−1.065)
Q	0.031***	0.026***
	(66.142)	(67.406)
POST × Q	0.008***	0.008***
	(3.284)	(3.996)
POST × Q × MOST_IMPACTED_EXANTE	0.003**	
	(2.129)	
POST × MOST_IMPACTED_EXANTE	−0.008**	
	(−2.267)	
Q × MOST_IMPACTED_EXANTE	−0.006***	
	(−7.983)	
POST × Q × MOST_IMPACTED_EXPOST		0.005***
		(4.878)
POST × MOST_IMPACTED_EXPOST		−0.012***
		(−3.940)
Q × MOST_IMPACTED_EXPOST		−0.006***
		(−3.462)
CF	0.029***	0.027***
	(4.131)	(4.671)
Other controls	Yes	Yes
Year-fixed effects	Yes	Yes
Firm-fixed effects	Yes	Yes
No. of obs.	33,089	28,980
Countries	34	34
Adj. R^2	0.385	0.385

E. Role of External Governance Mechanisms

My evidence implies better internal monitoring of CEOs due to changes in board structure, and thus, suggests a higher level of investment-Q sensitivity following board reforms. However, when the external governance mechanisms are stronger, this effect should be weaker, as I expect to observe some substitution between internal and external governance mechanisms. To test this prediction, I augment equation (1) with an interaction term between POST × Q and measures of the effectiveness (or lack thereof) of external governance forces. I use four measures: i) a dummy variable indicating that the anti-director rights index of Spamann (2010) is above the cross-country median, and 0 otherwise (ADRI_HI); ii) a dummy variable indicating institutional ownership in year 0 is above the cross-firm median, and 0 otherwise (IO_HI); iii) a dummy variable indicating long-term (short-term) institutional ownership in year 0 is above the cross-firm median, and 0 otherwise

(LTIO_HI (STIO_HI)); and iv) a dummy variable indicating the Herfindahl–Hirschman Index in year 0 is below the cross-industry median, and 0 otherwise (HHI_LO).

I expect the reform effect to be weaker in countries in which the institutional environments favor investor protection. Institutional ownership is considered an effective governance mechanism for mitigating information asymmetry and agency problems (Shleifer and Vishny (1986)). Recent research distinguishes between long-term and short-term institutional investors and argues that unlike short-term investors, long-term investors can effectively monitor managers and reduce managerial myopia through active engagement, shareholder proposals, and discussions with management (Gaspar, Massa, and Matos (2005), Chen, Harford, and Li (2007), and McCahery, Sautner, and Starks (2016)). Thus, I expect the reforms to be less effective for firms that already benefit from more long-term institutional investors. Giroud and Mueller (2010), (2011) find that firms in noncompetitive industries benefit more from good governance than those in competitive industries because competition reduces managerial slack. Based on this, I expect the reform effect to be weaker for firms facing stronger product market competition measured by a Herfindahl–Hirschman Index value that is below the cross-industry median.

Table 8 provides evidence in support of the above predictions. In column 1, the estimated coefficient on POST × Q × ADRI_HI is negative and significant, suggesting that the post-reform increase in investment-Q sensitivity is smaller in countries where shareholders enjoy better protection. Column 2 shows that overall institutional ownership has little impact on the reform effect, as indicated by the insignificant coefficient estimate on POST × Q × IO_HI. However, when dividing total institutional ownership into long-term and short-term, I find very different results (column 3). The reform effect is weaker for firms with more long-term investors, as indicated by the negative and significant estimated coefficient on POST × Q × LTIO_HI, and stronger for firms with more short-term investors, as indicated by the positive and significant estimated coefficient on POST × Q × STIO_HI. All else equal, the investment-Q sensitivity of an average firm with high short-term and low long-term institutional ownership increases post-reform by approximately twice ((0.014 + 0.006)/(0.014–0.005)) as much as that of an average firm with low short-term and high long-term institutional ownership. Thus, the reforms appear to be less (more) valuable when institutional investors conduct (fail to conduct) more monitoring. In column 4, the coefficient estimate on POST × Q × HHI_LO is negative and significant, implying that stronger product market competition weakens the reform effect. These results suggest that board reforms are less effective when alternative governance mechanisms are in place, as these ensure the proper monitoring of managers. Conversely, the reforms are more effective when these mechanisms fail to discipline a firm's management.[11]

[11]Collectively, the results strongly support the notion that increased board oversight improves investment efficiency. I further consider and test three alternative non-mutually exclusive explanations of my findings. First, I consider whether an increase in investment-Q sensitivity is partially due to improved access to capital but find no evidence that financially constrained firms making greater changes in board governance have better access to capital following board reforms. Second, I find little evidence of improved information disclosure quality following board reforms, so it is unlikely that variations in firm information environments partially explain the post-reform increase in investment-Q sensitivity. Third, I investigate whether variations in stock price informativeness (e.g., Chen et al.

TABLE 8

Board Reforms, External Governance, and Investment-Q Sensitivity

Table 8 examines the effect of board reforms on investment-Q sensitivity conditional on the strength of external governance mechanisms. The dependent variable is investment (INVEST). The interaction variable POST × Q is further interacted with ADRI_HI in column 1, IO_HI in column 2, LTIO_HI and STIO_HI in column 3, and HHI_LO in column 4. The variable ADRI_HI is a dummy variable indicating that the anti-director rights index of Spamann (2010) is above the cross-country median, and 0 otherwise. The variable IO_HI is a dummy variable indicating that institutional ownership in year 0 (the year immediately prior to the reform year) is above the cross-firm median, and 0 otherwise. The variable LTIO_HI (STIO_HI) is a dummy variable indicating that long-term (short-term) institutional ownership in year 0 is above the cross-firm median, and 0 otherwise. The variable HHI_LO is a dummy variable indicating that the Herfindahl–Hirschman Index in year 0 is below the cross-industry median, and 0 otherwise. The estimated coefficients on other controls (SIZE, LEVERAGE, CASH, AGE, GDP_GR, and CPI) are unreported for brevity. All variables are as defined in the Appendix. Year and firm-fixed effects are included in all regressions. Standard errors are clustered at the country level. *t*-statistics are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	1	2	3	4
POST	−0.016***	−0.005	−0.003	−0.007
	(−2.896)	(−0.853)	(−0.443)	(−0.663)
Q	0.025***	0.023***	0.023***	0.018***
	(23.164)	(8.377)	(8.405)	(6.099)
POST × Q	0.017***	0.014***	0.014***	0.023***
	(8.881)	(12.181)	(11.891)	(5.817)
POST × Q × ADRI_HI	−0.013***			
	(−4.581)			
POST × ADRI_HI	0.024***			
	(3.553)			
Q × ADRI_HI	−0.007**			
	(−2.427)			
POST × Q × IO_HI		0.000		
		(0.080)		
POST × IO_HI		−0.019***		
		(−3.682)		
Q × IO_HI		0.002		
		(0.930)		
POST × Q × LTIO_HI			−0.005***	
			(−3.189)	
POST × LTIO_HI			−0.003	
			(−0.668)	
Q × LTIO_HI			0.005***	
			(3.059)	
POST × Q × STIO_HI			0.006**	
			(2.244)	
POST × STIO_HI			−0.022***	
			(−4.891)	
Q × STIO_HI			−0.004	
			(−1.298)	
POST × Q × HHI_LO				−0.010*
				(−1.785)
POST × HHI_LO				−0.006
				(−0.486)
Q × HHI_LO				0.007**
				(2.564)
CF	0.025***	0.027***	0.027***	0.024***
	(4.206)	(5.715)	(5.802)	(4.326)
Other controls	Yes	Yes	Yes	Yes
Year-fixed effects	Yes	Yes	Yes	Yes
Firm-fixed effects	Yes	Yes	Yes	Yes
No. of obs.	152,906	71,324	71,324	159,737
Countries	36	39	39	41
Adjusted R^2	0.358	0.362	0.362	0.350

(2007)) play a role in my findings. Although stock price nonsynchronicity of firms making greater changes in board governance increases following board reforms, I find no evidence that firms with the least informative prices pre-reform achieve the greatest improvement in investment-Q sensitivity. The results of these tests are unreported for the sake of brevity.

F. Implications for Firm Operating Performance

In this section, I investigate the implications of my findings for firm operating performance. If board reforms are effective in improving the structure of corporate boards and increasing board oversight, then managers should identify and invest in better-quality (more profitable) projects following the reforms. Thus, I expect firms to achieve better operating performance post-reform. I test this prediction by estimating the following regression model:

$$(2)\quad \text{PERFORMANCE}_{i,j,t,t+1} = \alpha_1 \text{POST}_{j,t} + \alpha_2 \text{CONTROLS}_{i,j,t-1} + \text{FIRM}_i + \text{YEAR}_t + \varepsilon_{i,j,t},$$

where $\text{PERFORMANCE}_{i,j,t,t+1}$ is an average operating performance measure between year t and year $t+1$ for firm i in country j; $\text{POST}_{j,t}$ is an indicator variable for the post-reform period; and $\text{CONTROLS}_{i,j,t-1}$ is a set of control variables. I measure operating performance by sales growth rate (SALES_GR) or return on assets (ROA). The control variables are SIZE, LEVERAGE, TANGIBILITY, and CASH. The variable TANGIBILITY is property, plant, and equipment to assets, and the other variables are defined as above. If operating performance improves following the reforms, then α_1 should be positive.

Table 9 reports the regression results using the dependent variable SALES_GR in column 1 and ROA in column 3. The estimated coefficients on POST are both positive and significant, suggesting that firms achieve better operating performance post-board reform. These results are consistent with Fauver et al. (2017), who find that firms' profitability, measured by their profit margins or returns on assets, improves following the reforms.

I further examine this performance effect and investigate the link between post-reform investment-Q sensitivity and performance. A positive link will further support the monitoring channel. I follow the approach first proposed by Durnev (2010) and later used by Foucault and Frésard (2012) to estimate firm-level variations in investment-Q sensitivity. I reestimate equation (1) after dropping the interaction POST × Q from the regression and keep the residuals for each post-reform firm-year observation. I drop the term that captures the post-reform effect from the regression so that the residuals will pick up this effect and thus can be interpreted as abnormal changes in firm investment-Q sensitivity. Positive (negative) residuals indicate that firm investment is more (less) closely linked to the stock price after the reform. I then construct a dummy variable MOST_SENSITIVE (LEAST_SENSITIVE) that equals 1 if the residuals are positive (negative), and 0 otherwise. Finally, I estimate a regression model similar to equation (2) after replacing POST with MOST_SENSITIVE and LEAST_SENSITIVE. The estimated regression model is as follows:

$$(3)\quad \text{PERFORMANCE}_{i,j,t,t+1} = \gamma_1 \text{MOST\_SENSITIVE}_{j,t} + \gamma_2 \text{LEAST\_SENSITIVE}_{j,t} + \gamma_3 \text{CONTROLS}_{i,j,t-1} + \text{FIRM}_i + \text{YEAR}_t + \varepsilon_{i,j,t},$$

where all of the variables are defined as before. If the reform-induced improvement in investment-Q sensitivity is associated with better capital allocation, then γ_1 should be more positive than γ_2.

TABLE 9

Implications for Firm Operating Performance

Table 9 reports the results of OLS regressions of operating performance measures on POST in columns 1 and 3 and measures of post-reform changes in investment-Q sensitivity in columns 2 and 4. Columns 1 and 2 use 2-year average sales growth (SALES_GR) as a dependent variable. Columns 3 and 4 use 2-year average return on assets (ROA) as a dependent variable. The variable MOST_SENSITIVE (LEAST_SENSITIVE) is a dummy variable that equals 1 if a firm experiences a relatively large (small) increase in its investment-Q sensitivity post-reform, and 0 otherwise. A detailed description of the construction of these variables is provided in the text. The *F*-test results at the bottom of the table in columns 2 and 4 are for whether the coefficients on MOST_SENSITIVE and LEAST_SENSITIVE are equal. The control variables SIZE, LEVERAGE, TANGIBILITY, and CASH are lagged by one period. All variables are as defined in the Appendix. Year and firm-fixed effects are included in all regressions. Standard errors are clustered at the country level. *t*-statistics are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	SALES_GR	SALES_GR	ROA	ROA
	1	2	3	4
POST	0.053***		0.007*	
	(4.942)		(1.847)	
MOST_SENSITIVE		0.119***		0.013***
		(10.039)		(3.052)
LEAST_SENSITIVE		−0.002		0.002
		(−0.117)		(0.484)
SIZE	−0.014***	−0.014***	0.006***	0.006***
	(−3.303)	(−3.379)	(5.298)	(5.280)
LEVERAGE	−0.078***	−0.088***	−0.009	−0.010*
	(−3.473)	(−4.489)	(−1.506)	(−1.693)
TANGIBILITY	0.104***	0.166***	0.010	0.016
	(2.726)	(4.166)	(0.534)	(0.794)
CASH	0.484***	0.531***	−0.070*	−0.066
	(10.277)	(10.978)	(−1.753)	(−1.626)
Year-fixed effects	Yes	Yes	Yes	Yes
Firm-fixed effects	Yes	Yes	Yes	Yes
No. of obs.	147,513	147,513	152,488	152,488
Countries	41	41	41	41
Adj. R^2	0.310	0.320	0.748	0.748
F-test: MOST_SENSITIVE = LEAST_SENSITIVE		63.19		9.655
P-Value		0.000		0.003

Columns 2 and 4 of Table 9 report the estimation results, which are consistent with the above prediction. The estimated coefficients on MOST_SENSITIVE are positive and significant, whereas those on LEAST_SENSITIVE are insignificant. The differences in coefficients are statistically significant, as shown by the *F*-test results at the bottom of the table. Thus, of the firms exposed to board reforms, those that more closely link their investment to stock prices subsequently achieve better performance.

IV. Conclusion

This study contributes to the governance literature by providing causal evidence for the effect of better quality board governance on investment efficiency. To address endogeneity concerns, I use a DiD research design with the staggered enactment of board reforms in 41 countries as a source of exogenous variation in the effectiveness of board monitoring. The reforms increase the incentives for monitoring by requiring more independent directors on the board and its audit committee and the separation of the CEO and chair positions.

My evidence supports the hypothesis that stronger board governance increases investment efficiency. I find a significant improvement in investment-Q sensitivity

post-reform. The effect withstands several robustness tests and is present only after the reforms become effective. Consistent with a causal interpretation, the effect is greater for firms that are more likely to benefit from board monitoring or those that are less likely to be disciplined by external governance forces. The effect cannot be explained by variations in the degree of financial constraints or price informativeness. Together, these findings suggest that improving the quality of board oversight through regulatory changes can address agency problems and motivate managers to consider stock price information when making investment decisions.

Appendix. Variable Definitions

This Appendix gives the definitions of the variables used in the article. Where applicable, Compustat variable codes are given in parentheses.

Dependent Variables

INVEST: Change in property, plant, and equipment (PPENT) plus change in inventory (INVT) plus R&D expense (XRD), all scaled by lagged total assets (AT). Missing values of XRD are replaced by 0. Source: Compustat NA and Compustat Global.

CAPX: Capital expenditures (CAPX) scaled by lagged total assets (AT). Source: Compustat NA and Compustat Global.

CAPX_RD: Capital expenditures (CAPX) plus R&D expense (XRD), all scaled by lagged total assets (AT). Missing values of XRD are replaced by 0. Source: Compustat NA and Compustat Global.

CAPX_RD_SGA: Capital expenditures (CAPX) plus R&D expense (XRD) plus selling, general, and administrative expense (XSGA), all scaled by lagged total assets (AT). Missing values of XRD are replaced by 0. Source: Compustat NA and Compustat Global.

ASSETS_GR: Change in total assets scaled by lagged total assets (AT). Source: Compustat NA and Compustat Global.

SALES_GR: Change in sales scaled by lagged sales (SALE). Source: Compustat NA and Compustat Global.

ROA: Operating income before depreciation (OIBDP) scaled by total assets (AT). Source: Compustat NA and Compustat Global.

Board Reform Variables

POST: Dummy variable set equal to 1 beginning in the year a major board reform becomes effective in a country, and 0 otherwise. Source: Fauver et al. (2017).

POST (board independence): Dummy variable set equal to 1 beginning in the year a major board reform involving board independence becomes effective in a country, and 0 otherwise. Source: Fauver et al. (2017).

POST (audit committee and auditor independence): Dummy variable set equal to 1 beginning in the year a major board reform involving audit committee and auditor independence becomes effective in a country, and 0 otherwise. Source: Fauver et al. (2017).

POST (chairman and CEO separation): Dummy variable set equal to 1 beginning in the year a major board reform involving chairman and CEO separation becomes effective in a country, and 0 otherwise. Source: Fauver et al. (2017).

POST (non-board components): Dummy variable set equal to 1 beginning in the year a major board reform involving non-board components becomes effective in a country, and 0 otherwise. Source: Fauver et al. (2017).

GRACE: Dummy variable set equal to 1 during the grace period of a major board reform, and 0 otherwise. The grace period starts in the year in which the statutes are passed or regulations are published (the year in parentheses in column 2 of Table IA1 in the Supplementary Material) and ends in the year immediately prior to the reform first effective year (the year in column 2 of Table IA1 in the Supplementary Material). Source: Fauver et al. (2017).

YEAR −1: Dummy variable set equal to 1 if the firm is headquartered in a country that will adopt a major board reform in 2 years, and 0 otherwise. Source: Fauver et al. (2017).

YEAR 0: Dummy variable set equal to 1 if the firm is headquartered in a country that will adopt a major board reform in 1 year, and 0 otherwise. Source: Fauver et al. (2017).

YEAR 1: Dummy variable set equal to 1 if the firm is headquartered in a country that adopts a major board reform in the current year, and 0 otherwise. Source: Fauver et al. (2017).

YEAR 2+: Dummy variable set equal to 1 if the firm is headquartered in a country that adopted a major board reform 1 or more years ago, and 0 otherwise. Source: Fauver et al. (2017).

POST_PSEUDO: Dummy variable set equal to 1 beginning in a pseudo-reform year, and 0 otherwise. Pseudo-reform years are randomly selected from the sample years, 1993–2012. Source: Author's calculations.

Firm-Level Explanatory Variables

Q: Book value of total assets (AT) minus book value of equity (CEQ) plus market value of equity (PRCC_F × CSHO), all scaled by book value of total assets (AT). Source: Compustat NA and Compustat Global.

CF: Income before extraordinary items (IB) plus R&D expense (XRD) plus depreciation and amortization (DP), all scaled by lagged total assets (AT). Missing values of XRD are replaced by 0. Source: Compustat NA and Compustat Global.

SIZE: Natural logarithm of market value of equity expressed in millions of U.S. dollars. Market value of equity is shares outstanding (CSHO) times closing share price (PRCC_F). Source: Compustat NA and Compustat Global.

LEVERAGE: Long-term debt (DLTT) plus debt in current liabilities (DLC), all scaled by total assets (AT). Source: Compustat NA and Compustat Global.

TANGIBILITY: Property, plant, and equipment (PPENT) scaled by total assets (AT). Source: Compustat NA and Compustat Global.

CASH: Cash and short-term investments (CHE) scaled by total assets (AT). Source: Compustat NA and Compustat Global.

AGE: Natural logarithm of the number of years since a firm appears in Compustat. Source: Compustat NA and Compustat Global.

CROSSLIST: Dummy variable set equal to 1 for firm-year observations with a non-missing CIK, and 0 otherwise. Source: Compustat NA and Compustat Global.

MOST_SENSITIVE: Dummy variable set equal to 1 for firms experiencing a relatively large increase in investment sensitivity to price post-reform, and 0 otherwise. Identification is based on post-reform firm-year observations with positive residuals from estimating equation (1) without the interaction term POST × Q. Source: Author's calculations.

LEAST_SENSITIVE: Dummy variable set equal to 1 for firms experiencing a relatively small increase in investment sensitivity to price post-reform, and 0 otherwise. Identification is based on post-reform firm-year observations with negative residuals from estimating equation (1) without the interaction term POST × Q. Source: Author's calculations.

MOST_IMPACTED_EXANTE: Score variable ranging from 0 to 3. The variable is incremented by 1 if i) the proportion of independent directors is less than or equal to 50%, ii) the proportion of independent audit committee members is less than or equal to 50%, or iii) the CEO is the chairman, all of which are measured in year 0, the year immediately prior to the reform year. Source: BoardEx, ISS, and ASSET4.

MOST_IMPACTED_EXPOST: Score variable ranging from 0 to 3. The variable is incremented by 1 if i) the proportion of independent directors is less than or equal to 50% in year 0 and higher than 50% in year 2, ii) the proportion of independent audit committee members is less than or equal to 50% in year 0 and higher than 50% in year 2, or iii) the CEO is the chairman in year 0 and is not the chairman in year 2. Source: BoardEx, ISS, and ASSET4.

IO_HI: Dummy variable set equal to 1 if a firm's institutional ownership in year 0 is above the cross-firm median, and 0 otherwise. Source: FactSet.

LTIO_HI: Dummy variable set equal to 1 if institutional ownership of a firm's long-term investors in year 0 is above the cross-firm median, and 0 otherwise. Long-term institutional ownership is calculated following Gaspar et al. (2005). Source: FactSet.

STIO_HI: Dummy variable set equal to 1 if institutional ownership of a firm's short-term investors in year 0 is above the cross-firm median, and 0 otherwise. Short-term institutional ownership is calculated following Gaspar et al. (2005). Source: FactSet.

Industry-Level Explanatory Variables

HHI_LO: Dummy variable set equal to 1 if the Herfindahl–Hirschman Index (HHI) in year 0 is below the cross-industry median, and 0 otherwise. HHI is computed as the sum of squared market shares in a given country-industry-year cell. Market shares are computed based on firm sales (SALE). Industries are defined using 1-digit SIC codes. Source: Compustat NA and Compustat Global.

Country-Level Explanatory Variables

ITL: Dummy variable set equal to 1 after the enactment of an insider trading law in a country, and 0 otherwise. Source: Fernandes and Ferreira (2009).

GDP_GR: Annual percentage growth rate of GDP. Source: World Development Indicators (WDI)

CPI: Annual percentage inflation rate. Source: World Development Indicators (WDI).

ADRI_HI: Dummy variable set equal to 1 if the anti-director rights index of Spamann (2010) is above the cross-country median, and 0 otherwise. Source: Spamann.

Supplementary Material

To view supplementary material for this article, please visit http://doi.org/10.1017/S0022109022001211.

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