

Online Appendix

“Global Board Reforms and the Pricing of IPOs”

In this online appendix, we report the results of the robustness checks discussed in Section 3.2 of the paper. Table OA1 presents the regression results of a number of sensitivity tests. For brevity, we only report the coefficient of *Board reform*. Control variables and fixed effects are included in all regressions but are not tabulated.

First, as with other related studies, a potential concern in our setting is that our results may stem from potential confounding effects of regulatory changes other than board reforms. To alleviate this concern, we conduct three tests. In the first test, we control for non-board reforms, as international governance reforms typically included both board and non-board reforms, and in some countries both reform types were implemented concurrently (Fauver et al. 2017). To that end, we construct *Non-board reform*, a dummy variable equal to one for IPOs taking place during or after the year when non-board reforms became effective in the country of issuance, and zero otherwise, and include this variable as an additional control in the baseline regression. The data used to construct *Non-board reform* is obtained from Fauver et al. (2017).

In the second test, we control for the adoption of International Financial Reporting Standards (IFRS). Prior studies (e.g., Hong et al. 2014) provide evidence consistent with IFRS adoption reducing information asymmetry in the IPO process. To control for potential effects of IFRS adoption, we construct *IFRS adoption*, which is a dummy variable equal to one for IPOs taking place during or after the year when IFRS were adopted in the country of issuance, and zero otherwise. We then modify our baseline regression model to include *IFRS adoption* as an additional control. We obtain the IFRS adoption years from Hong et al. (2014).

In the third test, we control for the passage of international takeover laws. International takeover laws enhance managerial discipline (Lel and Miller 2015), which in turn could curb managerial opportunism in IPO firms and, therefore, reduce IPO underpricing (Brennan and

Franks 1997; Ljungqvist and Wilhelm 2003). To examine whether the passage of antitakeover laws is behind our main results, we construct *Takeover law*, a dummy variable equal to one for IPOs taking place during or after the year when takeover laws became effective in the country of issuance, and zero otherwise, and include it as an additional control in the baseline regression model. Detailed data on international takeover laws are obtained from Lel and Miller (2015).

The results of these three tests are reported in Panel A of Table OA1, which shows that the coefficient of *Board reform* remains significantly negative in all three tests, re-affirming that our baseline results are not an artefact of confounding regulatory changes.

Second, we consider the possibility that the documented effect of board reforms on IPO first-day return reflects changes in the composition of IPO firms in the post-reform period. To alleviate this concern, we restrict our analysis to IPOs taking place within one year before or after the implementation of board reforms and construct a propensity score-matched sample. We calculate propensity scores by regressing *Board reform* against the IPO-level characteristics in Equation (1) (i.e., *Firm size*, *Profitability*, *Leverage*, *Asset turnover*, *Market-to-book*, and *Bookbuilding*), as well as industry and country fixed effects using probit. We then match each IPO taking place within one-year pre-reform period with the one taking place within one-year post-reform period that has the closest propensity score, and regress *IPO first-day return* against *Board reform* and controls. The results of this estimation are reported in Panel B of Table OA1, which shows that the coefficient of *Board reform* remains significantly negative, reinforcing the robustness of our findings.

Third, we examine the sensitivity of our baseline findings to alternative methods of accounting for IPOs that take place in the board reform years. The issuance dates of some of the IPOs taking place in the board reform year may be before the exact implementation dates of the

reforms, which could introduce some degree of noise into our *Board reform* measure. We conduct two tests to examine this issue. In the first test, we focus on a subsample of IPOs in countries in which the exact board reform dates are available from Kim and Lu (2013).¹ We then define *Board reform* as a dummy variable equal to one for IPOs taking place after the major board reform date in the country of issuance, and zero otherwise, and use this refined measure to estimate our baseline regression model. In the second test, we repeat our baseline analysis after excluding IPOs that take place in the board reform years from the full sample. We report the results of this analysis in Panel C of Table OA1, which shows that the coefficient of *Board reform* remains significantly negative in both tests.

Fourth, we modify our baseline model to include an array of additional IPO-level and economy-wide controls to mitigate the potential problem of omitted variables. Following prior literature (Ritter 1984; Demers and Joos 2007; Chemmanur and Yan 2017; Çolak et al. 2017), we include *Cash* to control for firm's financial slack, *Advertising* to control for firm's visibility, *Firm age* to control for firm's life cycle, *Price revision* to control for partial adjustment effect in IPO prices, and *Float* to control for the percentage of shares issued to the public. We also control for the economy-wide conditions and regulatory environment by including *Hot issue market*, *Rule of law*, *Political stability*, *Regulatory quality*, *Corruption perception*, and *GDP per capita*. Specifically, *Cash* is defined as cash and short-term investments divided by total assets of the IPO firm at the time of listing. *Advertising* is defined as advertising expenses divided by sales of the IPO firm at the time of listing. *Float* is defined as number of common shares issued to the public divided by total number of outstanding shares. *Firm age* is defined as log transformation of one

¹ These countries are Argentina, Australia, Austria, Belgium, Brazil, Canada, Chile, China, Finland, France, Germany, Hong Kong, India, Italy, Malaysia, Mexico, Netherlands, Norway, Poland, Singapore, South Korea, Spain, Sweden, Switzerland, the U.K., and the U.S.

plus the difference in years since the firm was established up to the IPO year. *Hot issue market* is the average initial return for IPOs issued during the three months prior to IPO month. *Rule of law* is country specific index for the extent to which agents have confidence in and abide by the rules of society based on LaPorta et al. (1998). *Price revision* is defined as the difference between the IPO offer price and the mid-point of the initial filing range, divided by the mid-point of the initial filing range. *Political stability* and *Regulatory quality* are country-specific political stability index and regulatory quality index based on the Worldwide Governance Indicators (WGI) project. *Corruption perception* is country-specific corruption perception index based on Transparency International. *GDP per capita* is country-specific GDP per capita. The sample size of this test is reduced to 7,975 IPOs due to the limited availability of data required to construct these additional controls. The results of this estimation are reported in Panel D of Table OA1 and show that including these additional controls does not affect our main results.

Fifth, we examine whether our findings are robust to alternative time windows used to measure IPO initial returns. Ljungqvist (2007) points out that it may take some time before aftermarket prices balance supply and demand in less developed capital markets or those with daily volatility limits. Thus, the stock return over the first trading day may not fully capture IPO underpricing. To address this issue, we follow prior research (e.g., Ellul and Pagano 2006; Lin et al. 2013) and measure IPO initial returns over one week and two weeks after the listing date. The results of this analysis are reported in Panel E of Table OA1 and show that the coefficient of *Board reform* remains significantly negative for both alternative return measures.

Sixth, we examine whether our results hold when using the year of the first board reforms as the implementation year instead of the year of major board reforms (Bae et al. 2020). To that end, we define *First board reform* as a dummy variable equal to one for IPOs taking place during

or after the year when the first board reform became effective in the country of issuance, and zero otherwise, and use this variable in our baseline regression model. Panel F of Table OA1 shows that using this alternative approach does not alter our main results.

Seventh, we examine the possibility that our results are spuriously caused by features of the underlying data. To alleviate this concern, we run a series of placebo tests in which we randomly assign a board reform year for each country in our sample and perform our baseline regression using pseudo-board reform years. We repeat the process 1,000 times, thereby generating 1,000 coefficients for pseudo-*Board reform*. We report the mean value and a set of percentiles from the corresponding empirical distributions of pseudo-*Board reform* coefficient estimates in Panel G of Table OA1. The results show that the coefficient of true *Board reform* reported in Column (3) of Table 3 lies at the very left tail of the empirical distribution of the pseudo-*Board reform* coefficients, suggesting that our core findings are unlikely to result from spurious correlations in our data.

[Insert Table OA1 here]

As discussed in the paper, our baseline analysis is conducted using a pooled sample of IPOs in multiple countries and in different industries. Hence, a potential concern is that our main results may be limited to a specific country or industry. To examine this issue, we conduct two tests. In the first test, we perform country-by-country regression analysis. To ensure the appropriate degrees of freedom, we conduct regressions for the 23 countries in our sample with at least 100 IPOs. We estimate our baseline model in Equation (1) for each country and report the results in Panel A of Table OA2. For brevity, we only report the coefficient of *Board reform*. Control variables, industry fixed effects, and year fixed effects are included in all regressions but are not tabulated. Although the sample size is significantly reduced, the coefficient of *Board reform* is

negative and significant at the 10% level or better in 17 out of 23 countries, suggesting that our main findings are not limited to IPOs in a particular country. In the second test, we conduct an industry-by-industry regression analysis using the Fama- French 12 industry classification. The results of this analysis are reported in Panel B of Table OA2. Analogous to Panel A of the table, we only report the coefficient of *Board reform*. Control variables, country fixed effects and year fixed effects are included in all regressions. Of the 12 industry-specific regressions, the coefficient of *Board reform* is negative and significant at the 10% level or better in 10 regressions, suggesting that our main findings are not limited to IPOs from a particular industry.

[Insert Table OA2 here]

Lastly, we conduct analysis of the timing of board reforms. While board reforms constitute an exogenous shock to board practices at the IPO firm-level, these reforms are not necessarily exogenous at the country level in terms of their timing, as the decision to implement reforms could be influenced by the prevailing economy and market conditions. A potential issue that could impact our inferences is that regulators' decision to implement board reforms in a specific country could be partly influenced by the performance indicators of the IPO market, such as the average first-day return of the country's IPOs.

To examine this issue, we follow prior literature (e.g., Kroszner and Strahan 1999) and use proportional hazard models to model the time until reform implementation. For each country in each year up to the board reform year, we calculate time to board reform (*Time to reform*)—the dependent variable in proportional hazard models—as the log transformation of one plus the number of years between a particular year and the board reform year. The explanatory variable of interest in the models is *Country first-day return*, calculated as the average first-day return across all IPOs for each country in each year. Control variables include *No. of IPOs*, *Market return*,

Political stability, Regulatory quality, Corruption perception, GDP per capita, GDP per capita growth, Market size, and Market turnover. Column (1) of Table OA3 reports the estimates of the Cox proportional hazard model and Column (2) those of the Weibull model. In both columns, the coefficient of *Country first-day return* is statistically insignificant (smallest p -value = 0.207), thereby providing no evidence that the timing of board reform is related to country-level IPO first-day return. These results re-affirm that board reforms are plausibly exogenous in our setting.

[Insert Table OA3 here]

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Appendix OA: Variable Definitions

Variable	Definition
Additional variables in Table OA1	
<i>One-week return</i>	IPO closing price at the end of the first week minus offer price, divided by offer price.
<i>Two-week return</i>	IPO closing price at the end of the first two weeks minus offer price, divided by offer price.
<i>First board reform</i>	Dummy variable equal to one for IPOs taking place during or after the year of first board reform in the country of issuance, and zero otherwise.
Additional variables in Table OA3	
<i>Time to reform</i>	Log transformation of one plus the number of years between a particular year and the board reform year.
<i>Country first-day return</i>	Average first-day return across all IPOs for each country in each year.
<i>No. of IPOs</i>	Log transformation of the number of IPOs for each country in each year.
<i>Market return</i>	Annual return on value-weighted benchmark index.
<i>Political stability</i>	Country-specific political stability index based on the Worldwide Governance Indicators (WGI) project.
<i>Regulatory quality</i>	Country-specific regulatory quality index based on the Worldwide Governance Indicators (WGI) project.
<i>Corruption perception</i>	Country-specific corruption perception index based on Transparency International.
<i>GDP per capita</i>	Country-specific Gross Domestic Product per capita.

Table OA1. Board Reforms and IPO First-day Returns: Robustness Tests

This table presents the results of robustness tests for the relationship between board reforms and IPO first-day returns. For brevity, the table only reports the coefficients of *Board reform*. Our baseline sample consists of 17,066 IPOs across 38 countries spanning the period 1990 to 2016. The regressions are performed by OLS, with *t*-statistics computed using standard errors robust to heteroskedasticity and clustering at the industry-year level. Control variables in Equation (1), constant, industry fixed effects based on Fama-French 12-industry classification, year of listing fixed effects, and country of listing fixed effects are included in all the regressions. Variable definitions are presented in the Appendix.

Panel A: Control for potential confounding events		
(1) Control for non-board reform		
Dependent variable:	<i>First-day return</i>	
	Co-eff.	<i>t</i> -stat.
<i>Board reform</i>	-0.156	-3.03
Observations	17,066	
Adjusted R ²	0.192	
(2) Control for IFRS adoption		
Dependent variable:	<i>First-day return</i>	
	Co-eff.	<i>t</i> -stat.
<i>Board reform</i>	-0.209	-3.47
Observations	13,563	
Adjusted R ²	0.180	
(3) Control for takeover laws		
Dependent variable:	<i>First-day return</i>	
	Co-eff.	<i>t</i> -stat.
<i>Board reform</i>	-0.149	-2.71
Observations	17,027	
Adjusted R ²	0.195	
Panel B: Propensity score-matched sample around board reform years		
Dependent variable:	<i>First-day return</i>	
	Co-eff.	<i>t</i> -stat.
<i>Board reform</i>	-0.226	-2.11
Observations	1,040	
Adjusted R ²	0.158	
Panel C: Alternative methods of accounting for IPOs in board reform years		
(1) Subsample of IPOs in countries with board reform dates available		
Dependent variable:	<i>First-day return</i>	
	Co-eff.	<i>t</i> -stat.
<i>Board reform</i>	-0.163	-2.32
Observations	13,655	
Adjusted R ²	0.172	
(2) Exclude the board reform year		
Dependent variable:	<i>First-day return</i>	
	Co-eff.	<i>t</i> -stat.
<i>Board reform</i>	-0.132	-1.95
Observations	16,604	
Adjusted R ²	0.191	

Panel D: Additional controls		
Dependent variable:	<i>First-day return</i>	
	Co-eff.	<i>t</i> -stat.
<i>Board reform</i>	-0.172	-2.44
Observations	7,975	
Adjusted R ²	0.206	
Panel E: Alternative measures of IPO initial return		
(1) IPO return over one week after listing		
Dependent variable:	<i>One-week return</i>	
	Co-eff.	<i>t</i> -stat.
<i>Board reform</i>	-0.083	-3.61
Observations	12,024	
Adjusted R ²	0.212	
(2) IPO return over two weeks after listing		
Dependent variable:	<i>Two-week return</i>	
	Co-eff.	<i>t</i> -stat.
<i>Board reform</i>	-0.064	-2.25
Observations	12,024	
Adjusted R ²	0.153	
Panel F: Alternative definition of board reform years		
Dependent variable:	<i>First-day return</i>	
	Co-eff.	<i>t</i> -stat.
<i>First board reform</i>	-0.151	-3.28
Observations	17,066	
Adjusted R ²	0.191	
Panel G: Placebo test		
	<i>Pseudo-Board reform</i>	
	Co-eff.	
Minimum	-0.045	
1%	-0.034	
5%	-0.023	
10%	-0.019	
20%	-0.013	
25%	-0.010	
30%	-0.007	
40%	-0.004	
Median	0.000	
60%	0.004	
70%	0.008	
75%	0.010	
80%	0.013	
90%	0.019	
95%	0.024	
99%	0.031	
Maximum	0.037	
Mean	0.000	
Co-eff. of <i>Board reform</i> in Column (3) of Table 3 in the paper	-0.157	

Table OA2. Board Reforms and IPO First-day Returns: Country-and Industry-Level Regressions

This table presents country- and industry-level regression results for the relationship between board reforms and IPO first-day returns. Our baseline sample consists of 17,066 IPOs across 38 countries spanning the period 1990 to 2016. In Panel A, we only include countries for which we have at least 100 IPOs over the sample period. The regressions are performed by OLS, with *t*-statistics computed using standard errors robust to heteroskedasticity. Control variables, constant, and fixed effects are included in all the regressions but are not reported. Variable definitions are presented in the Appendix.

Panel A. Country-level regressions

Country	<i>Board reform</i>	<i>Board reform</i>	Observations	Adjusted R ²
	Co-eff.	<i>t</i> -stat.		
Australia	0.055	0.66	1,083	0.031
Brazil	0.069	0.16	123	0.116
Canada	-0.179	-2.36	956	0.032
China	-0.398	-3.60	2,191	0.175
France	-0.019	-0.28	498	0.043
Germany	-0.084	-1.76	317	0.045
Greece	-0.369	-2.14	128	0.254
Hong Kong	-0.423	-2.16	756	0.165
India	-0.159	-1.78	1,242	0.086
Indonesia	-0.447	-1.67	315	0.035
Italy	-0.093	-0.68	174	0.103
Japan	-0.639	-7.90	1,983	0.214
Malaysia	-0.340	-2.66	672	0.103
Norway	-0.166	-0.33	104	0.201
Pakistan	-0.233	-2.20	100	0.280
Poland	0.180	0.61	255	0.130
Singapore	-0.058	-1.75	431	0.075
South Korea	-0.569	-2.34	865	0.084
Sweden	-0.153	-1.68	132	0.324
Thailand	-0.351	-1.70	457	0.085
Turkey	-0.395	-1.71	123	0.180
United Kingdom	-0.104	-1.93	1,204	0.070
United States	-0.316	-3.48	2,173	0.128

Panel B. Industry-level regressions

Industry	<i>Board reform</i>	<i>Board reform</i>	Observations	Adjusted R ²
	Co-eff.	<i>t</i> -stat.		
Consumer Non-Durables	-0.141	-2.00	1291	0.021
Consumer Durables	-0.220	-2.40	513	0.050
Manufacturing	-0.118	-2.28	1974	0.034
Energy	-0.182	-1.74	570	0.012
Chemical	0.025	0.24	574	0.054
Business Equipment	-0.088	-1.78	2563	0.040
Telecommunication	-0.211	-1.65	338	0.102
Utilities	0.042	0.29	273	0.088
Shops, Retail & Wholesale Units	-0.111	-2.18	1469	0.028
Healthcare	-0.137	-1.96	1265	0.016
Finance	-0.207	-4.77	2971	0.014
Others	-0.146	-3.60	3265	0.039

Table OA3. Timing of Board Reforms: Hazard Models

This table presents the results of the hazard models for the timing of board reforms. The observations in these models are at a country-year level. Our baseline sample consists of 17,066 IPOs across 38 countries spanning the period 1990 to 2016. The models are estimated using maximum likelihood, with z-statistics computed using standard errors robust to heteroskedasticity. Variable definitions are presented in the Appendix.

Dependent Variable:	Cox proportional hazard model		Weibull proportional hazard model	
	<i>Time to reform</i> (1)		<i>Time to reform</i> (2)	
	Co-eff.	z-stat.	Co-eff.	z-stat.
<i>Country first-day return</i>	0.042	1.10	0.045	1.25
<i>No. of IPOs</i>	0.060	1.15	0.048	0.95
<i>Market return</i>	0.464	0.80	0.470	0.69
<i>Political stability</i>	-0.231	-2.30	-0.217	-2.51
<i>Regulatory quality</i>	-0.758	-2.27	-0.649	-2.11
<i>Corruption perception</i>	0.109	1.51	0.080	1.22
<i>GDP per capita</i>	0.244	2.47	0.233	2.76
<i>GDP per capita growth</i>	0.002	0.11	-0.002	-0.12
<i>Market size</i>	0.673	4.79	0.636	4.08
<i>Market turnover</i>	0.087	0.83	0.086	0.79
Observations	375		375	
Wald chi2	47.03		45.79	