Insider Trading and Corporate Board Reforms: International Evidence

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Abstract

We examine whether changes to corporate governance resulting from board reforms affect insider trading activities. While the connection between corporate governance and informed transactions has sparked the interests of both academics and practitioners, a lack of exogenous variation in governance has hampered inference. This paper employs a country's implementation of major governance reforms that capture shocks to board reforms for firms in 41 countries. Our difference-in-differences analysis shows a decline in insider trading activities and trading profit following the reforms. We find that decreased information asymmetry helps curb insider trading after board reforms. Rule-based reforms and reforms involving board and audit committee independence curtail insider trading while other types of reforms do not. The effect of board reforms on insider trading is more pronounced among countries with tighter public enforcement, more effective judicial systems, and higher financial reporting quality. Overall, our findings suggest that the governance mechanisms implemented in board reforms effectively discourage insider transactions. Our paper contributes to a growing literature on the implications of corporate governance mechanisms for financial markets and corporate management practices.

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1. Introduction

Do governance practices mitigate insider trading around the world? While the connection between corporate governance and insider transactions has sparked the interests of both academics and practitioners for several decades, a major limitation in the literature lies in the endogenous nature of both governance structures and informed transactions. Notably, there are large variations in the information environment and corporate governance practices across countries,[1] making the effect of governance practices on informed trading across the globe an important, yet thoroughly investigated research question.[2] In this study, we examine how the wave of worldwide corporate board reforms affects insider trading activities. This quasi-natural experiment comes with two important benefits. First, major board reforms provide variations in governance practices that are exogenous to firms' internal policies, allowing us to establish causal inferences between governance and insider trading if there are any. Second, country-level board reforms capture shocks to board governance for a universe sample of firms in 41 countries, providing greater variations in governance than any single country setting.

Following the issuance of the Cadbury Report in 1992, countries around the world have experienced several corporate board reforms which aim at enhancing corporate governance practices, such as improving board and audit committee independence and separating CEO and chairman positions (Kim and Lu (2013), Fauver, Hung, Li, and Taboada (2017)). Board reforms are major events that can affect the effectiveness of the corporate governance framework. The adoption of board reforms can influence several corporate practices (e.g., Fauver et al. (2017), Bae, El Ghoul, Guedhami, and Zheng (2021), Chen, Goyal, and Zolotoy (forthcoming)).

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[1] Denis and McConnell (2003) provide a review of international corporate governance and document notable differences in governance system across countries. Griffin, Kelly, and Nardari (2010) conduct a large empirical study across 56 countries and show significant variations in the informational environment in an international setting.

[2] Several studies (e.g., Betzer and Theissen (2009), Jagolinzer, Larcker, and Taylor (2011), Cziraki, De Goeij, and Renneboog (2014), Dai, Fu, Kang, and Lee (2016)) consider the link between governance rules and insider trading profit in one-country samples. However, the generation of results for international markets seems unfeasible due to significant differences in governance practices and regulatory regimes across countries.

Corporate governance mechanisms, such as those implemented in the worldwide board reforms, primarily aim at reducing agency problems (Denis and McConnell (2003), Hermalin and Weisbach (2003)).

Insider trading is a controversial practice that has received widespread attention from both academics and practitioners for decades (see, for example, Bhattacharya and Daouk (2002), Huddart and Ke (2007), Fernandes and Ferreira (2009), Jagolinzer, Larcker, and Taylor (2011), Cohen, Malloy, and Pomorski (2012), Jayaraman (2012), Dai, Parwada, and Zhang (2015), Rogers, Skinner, and Zechman (2016), and Mehta, Reeb, and Zhao (2021)).[3] The prominent criticisms voiced against insider transactions are that they exacerbate agency problems, harm investor confidence, and increase trading costs (e.g., Manove (1989), Moore (1990), Leland (1992)) and, therefore, should be governed and restricted (e.g., Werhane, (1989), Seyhun (1992), Bettis, Coles, and Lemmon (2000), Banerjee and Eckard (2001), Cline, Williamson, and Xiong (2021)). Supporters of insider trading, however, argue that it can improve the efficiency of financial markets and is a way for firms to compensate managers for their performance (e.g., Manne (1966), Fischel and Carlton (1982)). Motivated by the stream of research on insider trading and the emerging strand of literature on corporate board reforms, we argue that the adoption of board reforms should influence the monitoring roles of internal governance, thereby affecting insider trading activities. To the best of our knowledge, this study provides the first empirical investigation of the relation between board reforms and insider trading around the world. By doing so, we attempt to fill the inconvenient gap in the literature.

The effect of board reforms on insider trading remains unclear because of the mixed implications from the literature. On the one hand, the reinforcement of corporate governance practices following the reforms can be an essential factor in mitigating insider trading. Prior studies show that insiders, such as top executives and controlling shareholders, can trade strategically to make an abnormal profit based on

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[3] We collect the number of media mentions on "insider trading" from Factiva's global news database over the period of 1980 to 2018. Over the period of 2000-2018, on average, "insider trading" is mentioned in the media more than 300 times more frequently than in 1980. From the Web of Science, there are 1,428 articles with a focus on "insider trading" over the period 1980-2018.

the private information that they have superior access to (e.g., Aboody and Lev (2000), Ke, Huddart, and Petroni (2003), Piotroski and Roulstone (2005), Huddart and Ke (2007), Ravina and Sapienza (2010)). If the main focuses of board reforms are on improving board oversight, board functions, or fiduciary duties, these provisions should mitigate the agency-related frictions between corporate insiders and outside claimants and, hence, restrict corporate insiders from extracting private benefit, such as exploiting private information when trading (Fauver et al. (2017)). We call this the "monitoring hypothesis."

On the other hand, board reforms could motivate insider trading. A strand of the literature suggests that insider trading profit is considered to be a substitute benefit that insiders, such as CEOs and top managers, seek when they are restrained from extracting direct private benefits through their positions (Roulstone (2003), Banerjee and Eckard (2001), Cziraki, De Goeij, and Renneboog (2014), Cziraki and Gider (2021)). Opponents of board reforms can criticize that board reforms may unnecessarily push firms away from their optimal, equilibrium board practices (Fauver et al. (2017)). In this sense, board reforms may create an imbalance between insider trading profits and private benefits owned to managerial entrenchment. Specifically, the stronger corporate governance following the reforms could make it harder for insiders to reap their private benefits of control. As a result, they might have to rely on other substitute benefits, such as profits from insider trading, and, thus, be more likely to engage in insider trading activities. We call this hypothesis the "substitution hypothesis."

Furthermore, one may also argue that board reforms can be irrelevant to insider trading. For example, firms with strong corporate governance may not experience significant changes in their governance practices following the reforms. In addition, whether firms genuinely follow the governance framework required by the reforms remains an open question. For example, firms can strategically appoint directors who appear to be independent but, in fact, have personal relationships with the managers (Fauver et al. (2017)). In this sense, adopting board reforms would do no good for the firm's agency problems and should have no significant impact on insider trading

practices. We call this last hypothesis the "irrelevance hypothesis." Overall, the average effect of board reforms on insider trading is ambiguous and, therefore, urges a thorough empirical investigation.

To investigate the relation between board reforms and insider trading, we examine a comprehensive set of worldwide board reforms in 41 countries between 1999 and 2019. Our empirical methodology builds on recent studies that employ exogenous shocks that vary by time and location for identification to make casual inferences (e.g., Bertrand and Mullainathan (2003), Gormley and Matsa (2016), Fauver et al. (2017), Bourveau, Lou, and Wang (2018)). We create treatment and control groups using indicator variables based on the timing of adoption of board reforms by countries. Using a difference-in-differences research design, we find that, on average, insider trading activities decrease following the adoption of the reforms. Specifically, trading activities such as the number of insider transactions, trading volume, and traded values, as well as trading profits, significantly drop after the reforms. The decline of insider trading activities after the reforms is economically significant. On average, the number of insider transactions significantly reduces by 24.3% after board reforms become effective. A myriad set of sensitivity analyses suggest that our results are consistent across different proxies for insider trading measures, various model specifications, and estimation windows.

We further examine cross-sectional variations in the relation between board reforms and insider trading. First, we investigate whether the documented effect varies across different implementation approaches. We find that the effects of board reform on insider trading are more pronounced for compulsory reforms than voluntary reforms. Second, we consider the reforms that cover three components, including board independence, audit committee independence, and CEO-Chairman separation, and examine whether there are any cross-sectional differences among these three categories. Our empirical results suggest that reforms that involve board and audit committee independence significantly affect insider trading, while there are no significant changes in insider transactions following the reforms that require the separation of the CEO and Chairman positions.

Third, we consider the moderating role of country-level characteristics on the relation between board reforms and insider trading. Previous studies suggest that a country's legal system, including regulation of insider trading, can be an explaining factor for its financial system, capital market development, and investor protection (e.g., Bebchuk and Fershtman (1994), La Porta, Lopez-de-Silanes, Shleifer, and Vishny (1997), La Porta and Lopez-de-Silanes (1998), Bhattacharya and Daouk (2002)). Therefore, we expect that the effects of board reforms on insider trading could vary across countries with different legal origins. Our results suggest that the effects of board reforms on insider trading are more pronounced for common law countries and countries with more rigorous legal systems, such as higher public enforcement, more investor protection, and more effective judiciary. In addition, we find that board reforms are more effective in restricting insider trading activities in countries with higher degrees of transparency, such as higher disclosure, higher corruption perception, and better financial reporting quality.

Although we document a robust relation between board reforms and insider trading using difference-in-differences analyses, there could be a concern that our findings can be driven by cofounding events or omitted time-varying variables. To address this concern, we adopt three additional approaches. *First*, in addition to the difference-in-differences model with fixed effects, we employ a first difference model to address unobserved heterogeneity in panel data. We find our results are robust. *Second*, we adopt a propensity-score matching approach to generate a matched sample of treatment and control firms surrounding board reforms (e.g., Dahya, McConnell, and Travlos (2002), Fauver et al. (2017)). We find the results from the matched sample are consistent with our baseline results. *Third*, we test the parallel trends assumption using three different analyses, including (*i*) dynamic difference-in-differences (DiD hereafter) estimations, (*ii*) placebo tests, and (*iii*) graphical analyses. The dynamic DiD results indicate no difference in insider trading between treatment and control firms before the first board reforms, confirming our parallel assumption of the DiD setting. The graphical analysis suggests that insider trading shows a similar pattern between treatment and control firms in the pre-reform period. We also conduct the placebo

test. Results from the placebo test show that there is no significant change in insider trading following the pseudo-reform years, thereby mitigating the concern that the documented effect of board reforms on insider trading is solely driven by omitted time-varying covariates. The consistent findings from dynamic DiD estimations and other assumption validation tests suggest that the parallel trends assumption for the efficacy of the DiD approach is satisfied and the documented effect of board reforms on insider trading is likely causal.

We further investigate the possible mechanism through which board reforms affect insider trading. To test whether decreased information asymmetry helps curb insider trading after a board reform, we examine the effect of board reforms on insider trading conditional on the pre-reform corporate information environment. Using accrual quality and bid-ask spread as proxies for information asymmetry, we find that a decline in insider trading is more pronounced among firms with high levels of pre-reform information asymmetry. These firms should benefit more from the reduction in information asymmetry induced by the reform, thereby resulting in a larger reduction in insider transactions.

We also report several additional results. *First*, we find both major and first reforms place significant impacts on insider transactions. Our results are, therefore, aligned with Fauver et al. (2017) in that the improvement in internal monitoring practices from both major and first reforms affects insiders' decisions. *Second*, we conduct country-level insider trading tests as our study centers on a country-level differential of board reforms. While using country-level difference-in-differences regressions leads to smaller sample size and loses the variations within each country, this conservative analysis can provide a simple and clear interpretation of our main findings. We consistently observe a decline in aggregated insider trading at the country level following the reforms. *Third*, we further investigate the effects of board reforms on financial markets by considering whether board reforms influence stock return synchronicity. We find that stock return synchronicity significantly increases after the reforms. The board reforms deter insider transactions, which, in turn, decreases the relative flow of firm-specific information into stock prices. Our findings,

therefore, confirm the linkage between insider trading and stock return informativeness as suggested in Piotroski and Roulstone (2004). *Fourth*, we consider whether litigation risk affects the effects of board reforms on insider trading. Consistent with the notion that higher litigation risk deters insider trading (e.g., Huddart, Ke, and Shi (2007), Billings and Cedergren (2015)), we find that the decline in insider trading, as driven by an improvement in governance practices following the reforms, is more pronounced among firms that are less exposed to litigation concerns. *Fifth* and finally, to rule out the possibility that our results can be driven by confounding events such as changes in the institutional environment rather than changes in corporate governance, we restrict our sample period to five years before and after the reform (i.e., [-5, +5] sample). We find our documented effects of board reforms on insider trading are robust.

Our study makes several contributions to the literature. First, it contributes to the stream of research that examines the influence of corporate governance on firm outcomes and practices. Early studies, for example, provide mixed implications regarding the effect of corporate governance on firm value (e.g., Gompers, Ishii, and Metrick (2003), Core, Guay, and Rusticus (2006), Bhagat and Bolton (2008), Johnson, Moorman, and Sorescu (2009)). Recent studies utilize the exogenous board governance regulation changes and find significant effects of board reforms on firm value (Fauver et al. (2017)), corporate pay-out and capital structure decisions (Bae et al. (2021), Ben-Nasr, Boubaker, and Sassi (2021)), crash risk (Hu, Li, Taboada, and Zhang (2020)), cash holding (Chen, Guedhami, Yang, and Zaynutdinova (2020)), the pricing of IPOs (Chen, Goygal and Zolotoy (forthcoming)), cross-listing activities (Liao, Tsang, Wang, and Zhu (forthcoming)), and tax avoidance (Li, Maydew, Willis, and Xu (2022)). In this paper, we go one step further by examining whether the adoption of board reforms affects the trading activities of corporate insiders. To the best of our knowledge, our paper is the first to investigate the impact of an exogenous shift in governance practices on insider trading around the world. By showing that board reforms influence informed transactions and stock return informativeness, our

paper suggests that board reforms have significant implications for financial markets and corporate management.

Second, as a relation between governance structure and insider trading is often jointly determined, previous studies that attempt to provide causal inferences on corporate governance and firm outcomes tend to be restricted by endogeneity concerns. By employing an exogenous shock to firms' board governance practices that offer an ideal regulatory shock for identification, we can establish the causal effects of board governance regulation changes on insider trading. Our study, therefore, sheds further light on the relation between governance and insider trading by providing comprehensive evidence from a large sample of firms around the world. Furthermore, while previous studies tend to rely on the regulation changes in a single country sample,[4] the cross-country setting in this study comes with several benefits. *First*, it allows us to investigate the effect of governance practices on insider transactions in a broader context where we can incorporate the moderating effects of country-level institutions into our consideration. *Second*, the staggered adoption of board reforms that vary by time and country for identification enables the generation of documented findings for international markets. In doing so, we respond to Fidrmuc, Korczak, and Korczak (2013), who call for future research that enriches our understanding of determinants of the magnitude and frequency of insider transactions across different institutional settings, which are under-investigated in the literature.

The remainder of this paper is organized as follows. In Section 2, we present the background and empirical prediction. We describe the sample, data, and descriptive statistics in Section 3, and report the empirical results in Section 4. Section 5 concludes the paper.

2. Background and empirical predictions

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[4] Cziraki, De Goeij, and Renneboog (2014), for example, employ a shock in corporate governance regulation to investigate how it affects insider trading profit in the Dutch market.

Following the issuance of the Cadbury Report and the Greenbury Report in the 1990s, countries all over the world have recognized the importance of a strong corporate governance mechanism. Over the past few decades, several reforms have been initiated by both developed and emerging countries to enhance the effectiveness of corporate governance practices across several dimensions, such as board functions, fiduciary duty, audit, and managerial accountability.[5] Among others, board-related reforms are the central axis of the reforms in many countries worldwide given their role as a fundamental governance mechanism. Reforms related to board governance include practices that aim to enhance board composition and board roles, such as board independence and chairman-CEO separation.

The worldwide board reforms are expected to have significant impacts on strengthening long-term economic performance and improving the function of the international financial markets (see, for example, the 2009 Latin American Corporate Governance Roundtable[6] and OECD (2011) report). Given their economic and regulatory significance, board reforms also influence various firm-level characteristics. In addition, changes in the corporate governance environment due to the worldwide governance reforms are exogenous to firms' policies and are considered as a powerful setting for academics to study the impact of the reforms on corporate outcomes. Fauver et al. (2017) employ this shocked-based research design to investigate the causal effect of board-related reforms on firm performance. The authors document that firm value increases following the adoption of board reforms. Following their inspiring work, recent studies examine whether the adoption of board reforms would affect various aspects of corporate behavior, such as equity volatility, tax avoidance, stock price crash risk, debt choice, and IPO pricing (Gagnon and Jeanneret (2020), Hu et al. (2020), Ben-Nars, Boubaker, and Sassi (2021), Chen et al. (forthcoming), Li et al. (2022)). However, little is known about how the board reforms affect the trading decisions of corporate insiders. In this study, we aim to address this

inconvenient void by examining the impact of worldwide board reforms on insider trading activities.

Insider trading refers to the usage of corporate private or insider information for personal trading purposes and is often viewed unfavorably. Previous studies on insider trading suggest that insiders can take advantage of their private information to earn abnormal profits (Seyhun (1986), Lakonishok and Lee (2001), Piotroski and Roulstone (2005)). Insider trading can impose significant costs to the firms and their outside shareholders, such as adverse selection and litigation risk. As a consequence, insider trading has been governed and constrained in many countries (e.g., Seyhun, 1986; Bettis, Coles, and Lemmon (2000), Hillier and Marshall (2002), Jagolinzer, Larcker, and Taylor (2011), Agrawal and Nasser (2012); Fidrmuc, Korczak, and Korczak (2013), Agrawal and Cooper (2015), Aitken, Cumming, and Zhan (2015), Brochet (2019), Cline, Williamson, and Xiong (2021)).[7] Outside shareholders often rely on several governance measures to mitigate insider trading and protect their benefits. Previous studies suggest that proper board and ownership structures lead to more effective monitoring (Weisbach (1988), Agrawal and Mandelker (1990), Healy and Wahlen (1999), Klein (2002)). Strong corporate governance, therefore, is expected to restrain the exploitation of private information in insider trading activities (Dai et al. (2016)). However, establishing a causal relationship between corporate governance rules and insider trading is often challenging because of the endogeneity issues. Board reforms, with their exogenous nature, provide a neat setting for us to examine the effect of corporate governance on insider trading.

There are opposite predictions regarding the relationship between corporate governance and insider trading, supported by two competing hypotheses. The monitoring hypothesis predicts that strong corporate governance discourages insider trading activities and diminishes insider trading profits (Fidrmuc, Goergen, and Renneboog (2006), Betzer and Theissen (2009)). Under the monitoring hypothesis, the

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[7] Examples of the restrictions on insider trading in the U.S. include Rule 10b-5 of the Securities Exchange Act of 1934, the Insider Trading and Securities Fraud Enforcement Act, and the Stock Enforcement Remedies and Penny Stock Reform Act.

adoption of board reforms may limit the opportunities for insider trading. The improvement in board oversight due to the reforms can help to mitigate agency conflicts and strengthen internal monitoring and discipline (Weisbach (1988)). As a result, executives and controlling shareholders can be encouraged to refrain from exploiting their private information to make a profit from insider trading.

The substitution hypothesis, in contrast, conjectures that insiders at firms with strong corporate governance gain more profit from insider trading activities (Cziraki, De Goeij, and Renneboog (2014), Cziraki and Gider (2021)). Under this hypothesis, insider trading profit is considered as the substitute benefits that the insiders, such as CEOs and top managers, seek when they are not able to extract direct private benefits from their company. Examples of attractive private benefits include using company resources for private purposes and CEO entrenchment. If this hypothesis holds, the enaction of board reforms is expected to have a positive relationship with insider trading.

The two competing hypotheses, however, are not mutually exclusive. Board reforms could have both negative and positive impacts on insider trading, and it could be possible that the two opposite effects cancel each other out, leaving no significant impact on insider trading. The effect of board reforms on insider trading, therefore, is ambiguous. We propose the following hypothesis in its null form:

H1: The adoption of board reforms does not affect insider trading activities.

We further investigate whether the impact of board reform on insider transactions varies depending on different circumstances. Aboody and Lev (2000) show greater information asymmetry between insiders and investors leads to more insider transactions based on insiders' information advantage. Hu, Li, Taboada, and Zhang (2020) suggest that board reforms reduce stock price crash risk by improving information transparency. Therefore, we conjecture that firms with high levels of pre-reform information asymmetry should benefit more from the reduction in information asymmetry induced by the reform, thereby resulting in a more significant decrease in insider transactions. Our second hypothesis is, thus, in its alternative form as follows:

H2: The effects of board reform on insider trading are more pronounced among firms with high information asymmetry.

Recent empirical studies on board reforms document that the effects of board reforms on a firm's behavior vary across different types of reforms. For example, Fauver et al. (2017) find that firm values only increase following the reforms related to board and audit committee independence, and the effect disappears in the reforms involving chairman and CEO separation. Different reforms' implementation approaches, such as mandatory compliance versus a more flexible approach where firms can choose not to comply with the reform (for legitimate reasons), may affect firms' outcomes differently (Fauver et al. (2017), Ben-Nasr et al. (2021), Chen et al. (forthcoming)). In addition, a body of literature documents that country-level characteristics such as regulations and the legal system have a significant impact on market efficiency, thus potentially influencing insider trading (e.g., Bebchuk and Fershtman (1994), La Porta et al. (1997), La Porta and Lopez-de-Silanes (1998), Bhattacharya and Daouk (2002), Fidrmuc, Korczak, and Korczak (2013)). Therefore, we expect that the relationship between board reforms and insider trading is not uniform across different reform types and country-level institutions. Our third hypothesis is as follows:

H3a: Board reforms affect insider trading differently across different reform types.

H3b: Board reforms affect insider trading differently across different country-level institutions.

3. Sample and variables

3.1. Data and sample

We obtain the data for this study from several data sources. First, we source global insider transaction data from the 2iQ Global Insider Trading database, which provides the most complete insider transaction historical data for all insiders. This database contains over 10 million individual transactions across 59 countries, with an

average history across all regions of around 14 years.[8] The full global coverage of the 2IQ database allows us to conduct a comprehensive study on insider trading across the globe, which remains under-investigated due to data unavailability.[9] Following the market microstructure literature (e.g., Frankel and Li (2004), Cohen, Malloy, and Pomorski (2012)), we screen the transactions for open market purchases and sales by insiders and exclude all other types of transactions, including awards, options, trades with corporations, and other transactions. We follow Alldredge and Cicero (2015) and exclude insider trades that are characterized as routine because these trades are unlikely to be motivated by the information and, hence, are less informative about a firm's prospects. We follow Cohen, Malloy, and Pomorski (2012)'s approach and define routine insider trading. Specifically, we define a routine trader as an insider who placed a trade in the same calendar month for at least three consecutive years. Non-routine, or opportunistic, insiders are the remaining insiders. [10]

Second, the information on board reforms is obtained from Fauver et al. (2017), which originally source the governance reform information from the World Bank, European Corporate Governance Institute, local exchange regulators, and previous studies such as Kim and Lu (2013). This dataset contains board reforms and the years in which the reforms are adopted. When there are multiple board reforms in a country, the dataset reports both the first board reform year and the major board reform year. In our analysis, we investigate the effect of the reforms on insider trading for both the first and major reform years separately.

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[8] Other studies that employ 2iQ Global Insider Trading data include, for example, Dardas and Guttler (2011) who studies the informativeness of directors' dealing reports; Jin, Livnat, and Zhang (2013) who consider how insider trading affects market responses to analyst forecast revisions; Chowdhury, Mollah, and Al Farooque (2018) who examine the earning manipulation practices of insiders; and Bourveau, Brochet, Ferri, and Sun (2021) who examine the impact of say-on-pay on insider trading.

[9] 2IQ collects insider transaction data from financial and regulatory authorities and includes legal insider trading data only. The legal insider transaction data is important for our research setting as the association between corporate board reform and legal insider trading remains unclear. Several studies have attempted to investigate illegal insider trading in the U.S. using proprietary data (see, for example, Ahern (2017), Ahern (2020), and Patel and Putnins (2022)). The illegal insider transaction data for international market is, however, not publicly available. We thank 2iQ, especially Kevin Adams, Patrick Hable, and Nikolay Vasilev for providing and helping us understand the 2IQ database.

[10] As part of the robustness tests, we consider all insider transactions as reported in the 2IQ database and find our main findings are robust.

We obtain firm-level financial data from the Compustat Global and North America database, and stock return information for U.S. and international markets from the Capital IQ daily database. Daily returns are computed based on the daily close prices adjusted for the daily total return factors and daily adjustment factors (e.g., Fahlenbrach, Rageth, and Stulz (2021)). We source country characteristics and governance data from several sources. Specifically, we source country-level characteristics from the World Bank's World Development Indicators, the International Monetary Fund (IMF), and La Porta and Lopez-de-Silanes (1998). We source the anti-director rights index from Djankov, La Porta, Lopez-de-Silanes, and Shleifer (2008). We obtain the public enforcement index, and the efficiency of the judiciary index, which captures the strength of a country's legal enforcement, from La Porta, Lopez-de-Silanes, and Shleifer (2006). We also source the investor protection index and the information disclosure requirement, which represents business transparency, from La Porta Lopez-de-Silanes, and Shleifer (2006). We obtain country-level financial reporting quality from Bushman, Piotroski, and Smith (2004). Our final sample consists of 118,728 firm-year observations spanning 41 countries from January 2000 to December 2019.[11]

We report information on the board reforms, including the major reform year, the first reform year, and a number of firm-year observations in Table 1. The number of firm-year observations varies across the countries in our sample, with the U.S., Canada, and China accounting for the greatest percentage of firm-year observations (64.59%).[12] Among the 41 countries in our sample, the U.K. is the first country that implemented board reforms (in 1992), while Italy is the one that most recently issued board reform policies (in 2006). We present a summary of the country-level characteristics in Appendix A1.

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[11] The choice of the sample period is based on the data availability. Our sample begins in 2000 as the global insider transaction data before 2000 is not complete. Our sample ends in 2019, which is the latest version of global insider data available at the time of writing the paper.

[12] As part of the sensitivity analyses, we exclude firms from the U.S., Canada, and China from the sample. We report the results for these tests in Appendix A6. We find that our documented findings are not driven by the overrepresentation by firms that are domiciled in these three countries.

3.2. Key variables

Our dependent variable is insider trading, and we source insider trading data from the 2iQ Global Insider Trading database. Motivated by the theory of informed trading (e.g., Kyle (1985), Huddart, Hughes, and Levine (2001)) that trade quantities are an important choice variable of insiders, we construct three measures of firm-level insider transactions, including the number of insider trades *(#TRADE)*, trading volume (*TVOLUME*), and traded values of insider trades (*TVALUE*). Specifically, *#TRADE* is the natural logarithm of one plus the total number of insider trades in a year. *TVOLUME* is the natural logarithm of one plus the total shares traded by insiders in a year. *TVALUE* is the natural logarithm of one plus the traded value in a year.

Our main independent variables are the board reform indicators. Specifically, *POST* is a dummy variable that takes a value of one starting from the year that the reform in a country became effective and zero otherwise. To ensure that our results are not sensitive to how we construct the *POST* variable, we include several variations of *POST*, including *POST1*, *POST2*, and *POST3*, which equals one if the insider trading takes place after the adoption of one, two, and three components of the reforms, respectively.

We follow the previous literature (e.g., Lakonishok and Lee (2001), Piotroski and Roulstone (2005)) and include a number of firm-level control variables that can be related to insider transactions. Specifically, we control for firm size (*LOGSIZE*), which is the natural logarithm of a firm's total assets. To control for firm growth potentials, we include the book-to-market ratio (*BMRATIO*), measured as the ratio of the market value of assets to the book value of assets. To account for a firm's capital structure, we use leverage (*LEVERAGE*), measured as total debt divided by total assets. We also include return on assets (*ROA*), which is earnings before interest and taxes divided by total assets, to control for a firm's profitability. All continuous variables are

winsorized at the top and bottom 1% to mitigate the effect of outliers. We provide the details of our variable descriptions in Table 2.

[Please Insert Table 2 About Here]

3.3. Descriptive statistics

We report the descriptive statistics (Panel A) and correlation matrix (Panel B) for our final samples in Table 2. Table 3's results show that the means of the number of insider trades, trading volume, and traded values ((in logarithm) are 10.54, 6.73, and 7.75, respectively. Regarding the control variables, the average firm in our sample has a firm size (in logarithm form) of 6.43, a debt-to-asset ratio of 22.01%, and a return-on-asset ratio of -7.66%. These summary statistics are consistent with previous international studies on insider trading (e.g., Bris (2005), Fidrmuc, Korczak, and Korczak (2013), Denis and Xu (2013)).

[Please Insert Table 3 About Here]

4. Empirical findings

4.1. Baseline regression

Our empirical methodology builds on recent studies that employ exogenous shocks that vary by time and location for identification to make casual inferences (e.g., Bertrand and Mullainathan (2003), Fauver et al. (2017)). Specifically, we investigate the impact of board reforms on insider trading using the difference-in-differences (DiD) estimation. Our regression specification is as follows:

(1) $Insider_{i,t} = \beta_0 + \beta_1 Post + Controls_{i,t} + \delta_i + \phi_i + \varepsilon_{i,t}$

where $Insider_{i,t}$ refers to three measures of insider trading of firm i during year t; $Control_{i,t}$ is the vector of firm characteristic variables; δ_i and ϕ_i are firm fixed-effects and year fixed-effects, respectively. We include firm fixed effects and year fixed effects to control for firm-specific heterogeneity and time-specific unobservable factors that

may be related to insider transactions. Since board reforms are implemented at the country level, we cluster standard errors by country in the baseline regressions. This conservative clustering method accounts for potential time-varying correlations in omitted variables that may affect different firms within the same country (Bertrand, Duflo, and Mullainathan (2004)).

We regress insider trading measures on *POST*, which takes a value of one starting from the year the reform in a country became effective and zero otherwise. Regarding the proxies for insider trading, we employ three different measures, including the number of insider trades *(#TRADE)*, traded values (*TVALUE*), and trading volumes (*TVOLUME*). The coefficient β_1 is our difference-in-differences estimate, which captures the average effect of board reforms for the treatment group relative to the control group. We report the results of our baseline analyses in Panels A, B, and C of Table 4, respectively. We consider the effect of the reforms on insider trading for two types of reforms: major reforms (columns (1) to (5)); and first reform (columns (6) to (10)).

[Please Insert Table 4 About Here]

Our results consistently show that the coefficient estimate on *POST* is negative and statistically significant at the 1% level, suggesting that insider trading activities are significantly attenuated after the reforms. The negative relation between board reforms and insider trading persists for both major and first reforms. To ensure that our results are not driven by cofounding events, we use several alternative variations to classify the period as pre- or post-reform, including *POST1*, *POST2,* and *POST3*, which equals one if the countries have passed at least one, two, or all three components of the reforms, respectively. Our empirical results consistently reject the null hypothesis H1 and suggest that the adoption of board reforms would curtail insider trading, which is consistent with the monitoring hypothesis. The magnitude of the reduction in insider trading is economically large. Panel A's Model (2) implies that the number of insider transactions significantly reduces by 24.3% (= -2.56/ 10.54)

on average after board reforms become effective. Similarly, trading volumes and traded values reduce by 24.2% and 25.4%, respectively, following the board reforms.

Regarding control variables, insiders at large firms and growth firms are more likely to be engaged in insider trading activities. In addition, insider trading is more common in profitable firms. Overall, the signs of coefficients of the control variables are generally consistent with those reported in previous studies (e.g., Rozeff and Zaman (1988), Lakonishok and Lee (2001), Frankel and Li (2004)).

4.2. Effect of board reforms on insider trading: Additional analyses

We supplement the baseline regression results in Table 4 with several additional tests to ensure that our results are not sensitive to specific model specifications or estimation approaches. We report the results for these tests in Table 5 and the Online Appendix.

First, we augment our baseline models in Table 4 with a range of country-level characteristics to ensure that our documented findings are not driven by any country's idiosyncratic features. The additional controls include the gross national product (*GNP*), GNP growth, trade ratio (*TRADE RATIO*) measured as the sum of export and import scaled by GDP, market turnover (*MARKET TURNOVER*) as measured by traded stocks scaled by market capitalization, foreign direct investment (*FDI*), an indicator indicating if a country is a developed or a developing country (*DEVELOPING*), and an indicator indicating if a country's legal origin is civic or common law (*CIVIC LAW*).

Second, to ensure that our documented findings are not sensitive to model specification selections, we estimate the effects of board reform on insider trading on different model estimations. Specifically, we follow Bertrand and Mullainathan (2003) and adopt high-dimension fixed effect models. Specifically, we use industry × year fixed effects and country × year fixed effects. The adoption of high-dimension fixed effects accounts for time-, industry-, and country-invariant factors that could be associated with insider trading activities and vary at a yearly frequency (e.g., Bertrand

and Mullainathan (2003), Gormley and Matsa (2016), Bourveau, Lou, and Wang (2018)). We also follow Petersen (2009) and use robust standard errors clustered by firm. We report the results for these tests in Table 5. Panels A, B, and C of Table 5 report the results when the number of insider trades (#*TRADE*), trading volume (*TVOLUME*), and trading values of insider transactions (*TVALUE*) are dependent variables of interest, respectively. We find our results are robust.

Third, as part of the sensitivity analyses, we repeat our baseline models in Table 4 using all insider transactions collected by the 2IQ database. We report the results for these tests in Appendix A3 and consistently find that board reforms place significant impacts on insider transactions.

Forth, given our study centers on a country-level differential of board reforms, we conduct country-level insider trading tests. While using country-level difference-in-differences regressions leads to a smaller sample size and loses the variations within each country, this conservative analysis can provide a simple and clear interpretation of our main findings. The dependent variable of interest is country-level insider trading, measured by the total number of insider transactions, trading value, and trading value in each country year. We include country fixed effects and year fixed effects to control for country-specific heterogeneity and time-specific unobservable factors that may be related to insider transactions. We report the results of these tests in Appendix A4. We consistently observe a decline in aggregated insider trading at the country level following the reforms.

Overall, the results from Table 4, Table 5, and Appendices A3 and A4 consistently suggest that the effects of board reforms on trading activities of corporate insiders are robust to alternative measures of insider transactions and not sensitive to specific model estimations.

[Please Insert Table 5 About Here]

4.3. First difference model results

In addition to the difference-in-differences model with fixed effects, we further employ a first difference model to mitigate unobserved heterogeneity in panel data. Specifically, we examine a re-estimation of our baseline results in Table 4 after taking the first differences of all continuous variables. We report the results for these tests in Panel A of Table 6.

Panel A's results consistently show that the coefficient estimate on *POST* is negative and statistically significant at the 1% level, suggesting that insider trading activities are significantly attenuated after the reforms. In addition, we find our results are robust across different model specifications, alternative measures of insider trading, and alternative board reform indicators.

[Please Insert Table 6 About Here]

4.4. Propensity-score-matching results

We next adopt a propensity-score matching approach to generate a matched sample of treatment and control firms surrounding board reforms. Since the staggered board reforms involve different event years, we follow Fauver et al. (2017) and employ a cohort matching strategy where an observation may serve as both a control and a treatment for different treatment events. Specifically, we first identify treatment firms as firms incorporated in countries that adopted board reforms. For each treatment sample, we identify a seven-year window surrounding the year in which the treated firms adopt the board reform. Control firms are firms without any board reforms in this window. We match one treatment firm to one control firm in the reform year.[13] To derive a close match, we use propensity scores with the nearest neighbor matching with replacement on all control variables in our baseline model in Table 4. With the cohort matched sample, we run the following model.

(2) $\quad Insider_{i,t} = \beta_0 + \beta_1 Post_t \times Treat_t + \beta_2 Post_t + \beta_3 Treat_t + \beta_1 Controls_{i,t} + \delta_i + \varepsilon_{i,t}$

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[13] The results are similar if we match one treatment firm to three nearest neighbor control firms as suggested in Li et al. (2022).

where *Post* is an indicator that equals 1 for both treated and control firms beginning in the year in which a board reform is passed and 0 otherwise. *Treat* is an indicator that equals 1 for firms incorporated in a country that adopts a board reform and 0 otherwise. *Post × Treat,* our variable of interest, captures the change for treatment firms, relative to the change for control firms. We estimate equation (1) using the matched treatment-control firms and report the results for these tests in Panel B of Table 6.[14]

We find the results from the matched sample are consistent with our baseline results. The coefficients of the variable of interest, *Post × Treat*, are negative and statistically significant across different model specifications. Consistent with the previous results, results from the matched sample suggest a decline in insider trading activities and trading profit following the board reforms.

4.5. Entropy-balancing weighted results

While the propensity score matching aims to minimize the difference in the propensity score across treatment and control firms, it may ignore the balance in moments of covariates. We, therefore, rerun our regressions using the entropy-balancing weighted method developed by Hainmueller (2012). The key advantage of entropy balancing is it reweights each observation of the control group to ensure the balance in the moments (e.g., mean or variance) of all covariates between treatment and control firms. The entropy balancing approach overcomes several drawbacks of the propensity score matching method, such as its statistical inferences being less sensitive to design choices, and is recently employed in the accounting literature (e.g., Bonsall and Miller, 2017; Joshi, 2020; McMullin and Schonberger, 2020; Ham, Kaplan, and Utke, 2022). In our analysis, we run the entropy balancing to balance the mean and variance of the covariates between the two groups. We report the results for the entropy-balancing weighted regressions in Panel C of Table 6. We find the results of the entropy-balancing weighted regressions are consistent. The coefficients of the

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[14] In untabulated results (for brevity), we find the treated firms and their matched control firms have comparable characteristics, suggesting that the matching improves the comparability of the treatment and control firms.

interaction terms, *Post × Treat*, are negative and statistically significant with different specifications. These results lend further supportive evidence that the board reforms reduce insider transactions.

4.5. *Validation of Parallel Trends Assumption: Dynamic Difference-in-difference, Graphical Analysis, and Placebo Test*

The parallel trends assumption is essential in the difference-in-difference research design. It assumes that treatment and control firms would have exhibited similar insider trading trends in the pre-reform period (without exogenous shocks) (Abadie, 2005). We test for the parallel trends assumption using three approaches, including (*i*) dynamic difference-in-differences (DiD) estimations, (*ii*) placebo tests, and (*iii*) graphical analyses.

First, we employ the following dynamic DiD regression framework to identify the exact timing of the treatment effect:

(3) $Insider_{i,t} = a + \beta_1\ Year\ (-1)_{i,t} + \beta_2\ Year\ (0)_{i,t} + \beta_3\ Year\ (+1)_{j,t} + Controls_{i,t} + \delta_i + \phi_i + \varepsilon_{i,t}$

where *Insider*$_{i,j,t}$ refers to three measures of insider trading of firm *i* during year *t*. Our sample period is from year -2 to year +2 relative to the adoption year of the first board reform. *Year (-1)*$_{i,t}$ and *Year (0)*$_{i,t}$ is an indicator that equals one if year *t* is one year before the adoption year or the adoption year of the first board reform of the country where firm *i* incorporated. *Year (+1)*$_{i,t}$ is an indicator that equals one if year *t* is the 1st year onward after the adoption year of the first board reform of the country. The reference year is the year *-2* relative to the reform year. We also include control variables, firm-fixed effects, and year-fixed effects as in the baseline regressions. Appendix A2 reports the results of these analyses.

We find that the estimated coefficients of *Year (-1)*$_{i,t}$ are nonsignificant, indicating no difference in insider trading between treatment and control firms before the first board reforms. It confirms our parallel assumption of the DiD setting. More importantly, we find that the treatment effect of the first board reforms appears since

the adoption year and more clearly in the first year onward after the board reform. For instance, coefficients of *Year (0)$_{i,t}$* are negative and significant for *TVOLUME* only, whereas coefficients of *Year (1+)$_{i,t}$* are negative and significant for all measures of insider trading. In terms of coefficient magnitudes, we find that estimated coefficients β_3 are larger than estimated coefficients β_2. The results suggest that the board reforms gradually take effect after the reform year and firms need some time to comply with the new board requirements. Thus, we observe the negative effect of board reforms on insider trading more clearly from the first year onwards after the board reforms.

Second, we adopt placebo tests to mitigate concerns that our documented findings are spuriously driven by features of the underlying data instead of exogenous board reforms. Specifically, we conduct a series of placebo tests in which we randomly assign a board reform year for each country in the sample and conduct our baseline regression estimation based on pseudo board reform years. Specifically, for each country, we randomly sample a year during our sample period, which is not the true reform year, and treat it as a pseudo reform year. We repeat this process 1,000 times to get 1,000 pseudo estimated coefficients. We report the placebo tests in Panel A (for major reforms) and Panel B (for first reforms) in Figure 1. In each panel, the upper density plots show the estimated coefficients of the pseudo board reform on the number of insider trades.[15] In lower tables, we report the mean value and a set of percentiles from the distribution of pseudo board reform coefficient estimates.

Figure 1's results suggest that the coefficient of true board reform reported lies at the very left tail of the empirical distribution of the pseudo board reform coefficients. Thus, the placebo tests suggest no significant change in insider trading following the pseudo-reform years, thereby mitigating the concern that the documented effects of board reforms on insider trading are solely driven by omitted time-varying covariates.

[Please Insert Figure 1 About Here]

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[15] For brevity, we report the results for the number of insider trades only. Other measures of insider transactions such as the trading volume and traded values provide qualitatively similar findings, which are untabulated for brevity but available upon request.

Third and finally, another way to test the parallel trends assumption is to use a graphical analysis as described by Li et al. (2022). Specifically, we plot the mean of each insider trading measure (i.e., *#TRADE*, *TVOLUME*, and *TVALUE)* of the two groups over the pre- and post-reform years (e.g., Gormley and Matsa, 2011; Bourveau et al., 2018; Li et al., 2022). We use the propensity-score-matched sample to identify the control firms in this analysis (details in Section 4.4). Figure 2 illustrates that the treatment firms and the control firms follow a similar pattern in insider trading during the pre-reform years until year 0 when the reform takes place in the treatment firms' countries. After the major board reforms, their insider trading began to diversify. Hence, the parallel trends assumption of the DiD research design is valid.

[Please Insert Figure 2 About Here]

Taken together, the consistent findings from dynamic DiD estimations and other assumption validation tests suggest that the parallel trends assumption for the efficacy of the DiD approach is satisfied and the documented effect of board reforms on insider trading is most likely causal.

4.6. Board reforms, information asymmetry, and insider trading

We further investigate the possible mechanism through which board reforms affect insider trading. Hu *et al.* (2020) suggest that board reforms reduce stock price crash risk by improving information transparency. We, therefore, consider whether decreased information asymmetry helps curb insider trading after board reforms. To test this possibility, we examine the effect of board reforms on insider trading conditional on the pre-reform corporate information environment. We conjecture that firms with high levels of information asymmetry should benefit more from the reduction in information asymmetry as induced by the reform, thereby resulting in a larger reduction in insider transactions.

To investigate the information asymmetry channel, we employ two common proxies for information asymmetry, including earnings quality and bid-ask spreads. Poor earnings quality is associated with higher information asymmetry (e.g., Aboody,

Hughes, and Liu (2005), Francis, LaFond, Olsson, and Schipper (2005), Bhattacharya, Desai, and Venkataraman (2013)), while lower bid-ask spreads are associated with greater transparency (e.g., Diamond and Verrecchia (1991), Armstrong, Core, Taylor, and Verrecchia (2011), Lang, Lins, and Maffett (2012), Armstrong, Core, and Guay (2014)). To capture earnings quality, we follow Hutton, Marcus, and Tehranian (2009) and Kim and Zhang (2016) and employ information opacity, measured as a three-year moving sum of absolute discretionary accruals, where discretionary accruals are estimated with the modified Jones (1991) model, following Dechow, Sloan, and Sweeney (1995). To measure bid-ask spreads, we use the natural logarithm of the average of daily effective spread in a given year, where daily effective spread is measured as two times the absolute value of trading price minus the bid-ask midpoint, all divided by the trading price (e.g., Daske, Hail, Leuz, and Verdi (2008), Anderson, Duru, and Reeb (2009), Bhattacharya et al. (2013)). As an alternative measure, we employ quoted spread, measured as the natural logarithm of the average of daily quoted spread in a given year, where daily quoted spread is measured as two times the absolute value of ask price minus bid price, all divided by ask price plus bid price (e.g., Pham (2020)). To capture high levels of information asymmetry before the reforms, for each measure of information asymmetry, we create an indicator (*OPACITY*, *HIGH_ESPREAD*, or *HIGH_QSPREAD*) that equals one if this measure is larger than the country's average value in the pre-reform year and zero otherwise. We then rerun the baseline models and include an interaction between *POST* and each of the information asymmetry indicators. We report the results of these tests in Table 7.

We find that the coefficients on the interaction term between *POST* and information asymmetry measures are negative and statistically significant across different model estimations. The results suggest that a decline in insider trading is more pronounced among firms with high levels of pre-reform information asymmetry. Thus, decreased information asymmetry contributes to explaining a reduction in insider trading after a board reform.

[Please Insert Table 7 About Here]

4.7. Variations in reform types and reform approaches

Having established the relation between board reforms and insider trading, we further investigate whether this relationship varies with different types of board reform. First, we examine how the reform implementation enables the effects of board reforms on insider trading. Some reforms in our sample are rule-based, which require mandatory compliance from firms, while others are comply-or-explain, which give firms an option to provide the reasons why they choose not to comply with the governance codes listed in the reforms.[16] We rerun our baseline regression separately for the two groups of mandatory and voluntary reforms and report the results for these tests in Columns (1) and (2) of Table 8, respectively.[17]

Our results suggest that rule-based reforms significantly reduce insider trading, while comply-or-explain reforms do not. One plausible explanation for this finding is that insider trading itself is a restricted regulatory activity; therefore, the strong enforcement of the rule-based reforms will be more effective in influencing insider trading than a more flexible approach in comply-or-explain reforms.

Second, we investigate how major components of board reforms affect insider trading. We zoom in on three components of board practices, including board independence, audit committee independence, and CEO-Chairman separation. As the three major components serve different purposes, the effect of board reforms on insider trading could vary across different reform types.[18] We report the regression results for each of these categories in Columns (3) to (5) of Table 8.

The results show that insider trading significantly drops after introducing reforms that involve board and audit committee independence. However, we document no significant evidence for the relation between insider trading and the

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[16] We classify the reform approaches as rule-based and comply-or-explain following Fauver et al. (2017).

[17] For brevity, we only report the results for the major reforms using the number of insider trades as a measure of insider trading. In untabulated results, we find consistent results for the first reforms and for other measures of insider trading (i.e., traded value and trading volume).

[18] For example, recommendations on greater board independence and the separation of the CEO and chairman positions aim at enhancing board oversight.

reforms which require the separation of the CEO and Chairman positions. Our results are aligned with Fauver et al. (2017), which suggest that the reforms that involve more representation from outsiders can lead to a greater impact on corporate value and insiders' decisions.

[Please Insert Table 8 About Here]

4.8. The moderating effect of country-level institutions

We next explore the moderating effects of countries' institutional features on the effects of board reforms and insider trading. Previous studies suggest that a country's legal system can be a determinant factor for its financial system, capital market development, and investor protection (e.g., Bebchuk and Fershtman (1994), La Porta et al. (1997), La Porta and Lopez-de-Silanes (1998), Bhattacharya and Daouk (2002)). Therefore, we expect that the effects of board reforms and insider trading could vary across countries with different regulatory regimes. To test this prediction, we run several subsample analyses. We first divide our country sample into two groups of "High" and "Low" according to a specific country-level characteristic. We then repeat the baseline regressions for each of the two groups separately. Following the prior literature (e.g., La Porta et al. (1997), La Porta and Lopez-de-Silanes (1998), Fidrmuc, Korczak, and Korczak (2013)), we consider several country-level institutions, including the degree of the anti-director index, the effectiveness of the judicial system, the stringency of public enforcement, and the legal system origin. In general, countries in a "High" group have a more rigorous legal system and better legal obedience. We report the sub-sample analysis for each of the country institutions in Columns (1) to (4) of Table 9 Panel A.

Panel A of Table 9's results suggest that the effects of board reforms on firm-level insider trading are more pronounced in countries with a more rigorous legal system, such as higher public enforcement, more investor protection, and a more effective judicial system. A possible explanation for our results is that a rigorous legal system would better enhance the effectiveness of the reforms. As a result, board

reforms show a stronger effect in countries with a better legal environment. Regarding the countries' legal origins, the results from Column (4) of Table 9 suggest that the documented effect is more pronounced in common law countries. Prior studies document that common law countries have better shareholder protection and corporate governance compared to civil law countries (La Porta et al. (1997)). Our results are, therefore, consistent with the previous literature and suggest that a better corporate governance mechanism in common law countries would make it easier for firms in these countries to implement the changes in the board reforms, leading to a more significant decline in insider transactions.

[Please Insert Table 9 About Here]

We further consider how a country's information environment enables the effects of board reforms on insider transactions. La Porta, Lopez-de-Silanes, and Shleifer (2006) document that laws that mandate the disclosure of material information, such as corporate ownership structure or profit, can benefit the stock markets by reducing uncertainty and enhancing investors' confidence to invest. Several studies suggest that the corporate information environment is an important determinant of insiders' activities (e.g., Aboody and Lev (2000), Frankel and Li (2004), Huddart and Ke (2007), Jagolinzer, Larcker, and Taylor (2011)). Given the notable differences in the informational environment across the globe (Griffin, Kelly, and Nardari (2010)), we test how the information environment affects the board reform effects. To capture the country's information environment, we use several proxies following the literature, including the disclosure requirement index (La Porta, Lopez-de-Silanes, and Shleifer (2006)), corruption perception index (Transparency International (2019)), and financial reporting quality (Bushman, Piotroski, and Smith (2004)). Specifically, the disclosure requirement index, or the index of disclosure, captures six components, including prospectus, compensation, shareholders, inside ownership, contract irregular, and transactions (La Porta, Lopez-de-Silanes, and Shleifer (2006)). The corruption perception index ranks countries by how corrupt their public services are perceived to be (Transparency International (2019)). The corporate reporting quality measure capture 90 items in companies' annual reporting that fall

into seven categories, including general information, income statements, balance sheets, funds flow statement, accounting standards, stock data, and special items (Bushman, Piotroski, and Smith (2004)). We define the High and Low groups based on the sample median of the variable distribution. We then repeat the baseline regressions for each group separately and report the results for these tests in Panel B of Table 9.

Table 9's results suggest that the effects of board reforms on insider transactions are more pronounced among countries with higher degrees of transparency, such as higher disclosure requirements, higher corruption perception, and higher financial reporting quality.

4.9. Board reforms and insider trading profit

Trading profit is one of the drivers of insider transactions (e.g., Seyhun (1986)). In this section, we examine the effects of board reforms on insider trading profitability. Following Jagolinzer, Larcker, and Taylor (2011), Gao, Lisic, and Zhang (2014), and Dai, Parwada, and Zhang (2015), we calculate trading profit as the alpha from the three-factor model estimated over the 180 days following each transaction.[19] The trade-specific profit is defined as:

(4) $R_i - R_f = a + \beta_1 (R_{mkt} - R_f) + \beta_2 SMB + \beta_3 HML + \varepsilon$

where R_i is the daily return to firm i; R_f and R_{mkt} are the daily risk-free interest rate and market return, respectively; SMB and HML are the size and book-to-market factors;[20] and a ($-a$) is the average daily risk-adjusted return to insider purchases (sales). As Jagolinzer, Larcker, and Taylor (2011) note, this approach to estimating trading profitability comes with two important benefits. First, estimating average daily abnormal returns reduces the biases in statistical tests of long-run buy-and-hold

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[19] Following the insider trading literature, we compute abnormal returns over a six-month horizon because insiders can be penalized for profits from trades that are made less than 180 days subsequent to prior trades (e.g., Ravina and Sapienza (2010), Jagolinzer, Larcker, and Taylor (2011), Jagolinzer, Larcker, Ormazabal, and Taylor (2020)).

[20] We thank Kenneth French for sharing the data.

returns. Second, computing trade-day specific risk-adjusted returns allows us to control for differences in risk across transactions and, hence, provides a trade-specific measure of insider trading profitability.[21]

We regress the insider trading profit on *POST*, which takes a value of one starting from the year that the reform in a country became effective and zero otherwise. Consistent with previous analyses, we include a number of firm-level variables, as in the baseline models. We follow Bertrand and Mullainathan (2003) and adopt high-dimension fixed effect models. Specifically, we use firm × trading day fixed effects and country × trading day fixed effects. The adoption of high-dimension fixed effects accounts for time-, firm-, and country-invariant factors that could be associated with insider trading activities and vary at a daily frequency (e.g., Bertrand and Mullainathan (2003)). Similarly, we cluster standard errors by country in the baseline regressions as board reforms are implemented at the country level. This conservative clustering method accounts for potential time-varying correlations in omitted variables that may affect different firms within the same country (Bertrand, Duflo, and Mullainathan (2004)). We report the results for these tests in Table 10. We consider the effect of the reforms on insider trading profitability for two types of reforms: major reforms (columns (1) to (4)); and first reforms (columns (5) to (10)).

Table 10's results suggest that trading profits significantly reduce after the reforms, and the results hold for different measures of board reforms and alternative model estimations. Thus, our results indicate that the improvement in internal monitoring practices following the board reforms contributes to a lower insider trading profitability. Our results, therefore, provide a generalization of the findings of the previous studies on one-country samples that governance matters for insiders' gains.

[Please Insert Table 10 About Here]

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[21] We thank Daniel Taylor for sharing the SAS code to estimate trade-specific insider trading profit used in Jagolinzer, Larcker, and Taylor (2011).

4.8. Board reforms and stock return synchronicity

We further investigate the effects of board reforms on financial markets by considering whether board reforms influence stock return synchronicity. Following the seminal work of Roll (1988), stock return synchronicity has become an attention-grabbing topic of accounting and finance research. Piotroski and Roulstone (2004), for example, document that insider trading can reduce the stock return synchronicity by conveying the firm-specific information flow into stock prices. Morck, Yeung, and Yu (2000) show that stronger protection of private property may promote informed arbitrage and capitalizes more firm-specific information into stock prices. If board reforms deter insider trading as our documented results so far, we expect it may have an impact on the stock return synchronicity.

We estimate the firm-specific measure of stock return synchronicity following the approach of Morck et al. (2000) and Durnev, Morck, Yeung, and Zarowin (2003). Specifically, we regress firm stock weekly returns on the corresponding market returns in a calendar year. For non-US firms, we further control for the US market returns in the model to incorporate the possibility that stock prices in other economies are influenced by the U.S. market (Morck et al., 2000). From the model, we obtain the coefficient of determination or R^2 and define stock return synchronicity as $SYNCH = log(R^2 / (1 - R^2))$. The log transformation yields an unbounded continuous variable and a more normal distribution for stock return synchronicity measure. A higher value of synchronicity indicates that the firm stock returns are closely tied to the market returns and reflect less firm-specific information. We then replace insider trading with *SYNCH* in our main regression setting to investigate whether board reforms influence the stock return synchronicity. Table 11 reports the regression results for these tests.

We find that stock return synchronicity significantly increases after the reforms, and the results hold for different measures of board reforms and alternative model estimations. The results suggest that the board reforms reduce firm-specific information in treatment firms' stock returns. The board reforms deter insider transactions, which, in turn, decreases the relative flow of firm-specific information

into stock prices. Our results, therefore, only confirm the linkage between insider trading and stock return informativeness as suggested in Piotroski and Roulstone (2004) but they also deepen our understanding of the effect of board reforms on financial markets.

[Please Insert Table 11 About Here]

4.9. Alternative sample to address confounding effects

So as to conduct a comprehensive investigation on the effects of the wave of worldwide corporate board reforms on insider trading, we further consider alternative samples. Specifically, to rule out the possibility that our results can be driven by confounding events such as changes in the institutional environment rather than changes in corporate governance, we restrict our sample period to five years before and after the reform. We report the results for these tests in Table 12.[22]

Table 12's results suggest that our documented effects of board reforms on insider trading are robust to alternative window estimation. In addition, we consider whether the degree of litigation risk affects the effects of board reforms on insider trading.[23] We report the results of these tests in Appendix A5. Consistent with the notion that higher litigation risk deters insider trading (e.g., Billings and Cedergren (2015)), we find that the decline in insider trading, as driven by an improvement in governance practices following the reforms, is more pronounced among firms that are less exposed to litigation concerns.

Furthermore, we consider subsample analyses. We find consistent results after excluding countries with extensive observations in the sample. We exclude firms in the U.S., Canada, and China, which account for about 65% of the total firm-year

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[22] For brevity, we report the results for the number of insider trade only. In untabulated results, we find that other measures of insider transactions such as the trading volume and traded values provide qualitatively similar findings.

[23] Following Chen, Gul, Veeraraghavan, and Zolotoy (2015), we define a firm as being one with high litigation risk if it belongs to one of the following industries with SIC code between 2833 and 2838; 3570 and 3577; 3600 and 3674; 5200 and 5961, 7370 and 7374, and 8731 and 8734.

observations. We report these results in Appendix A6 and find our results are neither sensitive to alternative sampling nor driven by countries of the overrepresentation firms. [24]

[Please Insert Table 12 About Here]

5. Conclusions

We study the effects of worldwide board reforms on the trading decisions of corporate insiders in a large sample of firms in 41 countries. Employing a difference-in-differences analysis, we find that insider trading activities and trading profitability significantly drop after the reforms. The decline of insider trading activities after the reforms is economically significant. On average, the number of insider transactions significantly reduces by 24.3% after board reforms become effective. We find that the effects of board reform on insider trading are more pronounced for compulsory reforms than voluntary reforms. In addition, the reforms that involve board and audit committee independence significantly affect insider trading while there are no significant changes in insider transactions following the reforms that require the separation of the CEO and Chairman positions. We find that decreased information asymmetry helps curb insider trading after board reforms. Considering the moderating roles of a range of country-level institutions, we observe that the effects of board reforms on insider trading are more pronounced for common law countries and countries with higher public enforcement, more investor protection, and more effective judiciary systems. Furthermore, we find that board reforms are more effective in restricting insider trading activities in countries with higher degrees of disclosure requirements, corruption perception, and financial reporting quality. Our

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[24] We further test the robustness of our results using three additional analyses. First, we re-estimate our baseline models after excluding some countries where board reforms were implemented prior to 2000 (e.g., the U.K., Spain, or Indian). Second, we limit our sample from 1990 to 2012, as in Fauver et al. (2017). Third, as the staggered difference-in-differences (DiD) estimations can be biased in the present of delayed or hetero treatment effects (e.g., Baker, Larcker, and Wang, 2021; Barrios, 2021), we consider a stacked DiD approach, following Cengiz, Dube, Lindner, and Zipperer (2019) and Goldstein, Yang, and Zuo (2022). Consistent across these additional analyses, we find our results (untabulated for brevity) are robust.

study provides more insights into the relation between governance and insider trading by adopting a natural experiment that allows us to establish a causal inference and provide comprehensive evidence from a large sample of firms around the world. Future studies can consider our research design to explore further the impact of governance practices on the economic decisions of other corporate stakeholders (e.g., employees, creditors, suppliers, and tax regulators). Given the magnitude and robustness of our results, this presents a potentially fruitful avenue for future studies.

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Table 1
Sample distribution

The table reports the board reform information. Columns (1) and (2) present the year when the major reform and the first reform year become effective, respectively. The sample covers 118,728 firm-year observations from 41 countries between 2000 and 2019.

Country	Major Board Reform Year (1)	First Reform Year (2)	Number of firm-years (3)	Frequency (%) (4)
Australia	2004	2003	5,091	4.29
Austria	2004	2002	221	0.19
Belgium	2005	1998	430	0.36
Brazil	2002	2002	504	0.42
Canada	2004	2004	15,424	12.99
Switzerland	2002	2002	971	0.82
Chile	2001	2001	105	0.09
China	2001	2001	7,816	6.58
Czech Republic	2001	2001	19	0.02
Germany	2002	2002	1,502	1.26
Denmark	2001	2001	406	0.34
Egypt	2002	2002	171	0.14
Spain	1998	1998	452	0.38
Finland	2003	2003	564	0.48
France	2001	2001	2,537	2.14
UK	1992	1992	6,732	5.67
Greece	1999	1999	878	0.74
Hong Kong	2005	2005	868	0.73
Hungary	2003	2003	49	0.04
Indonesia	2000	2000	545	0.46
India	1998	1998	4,251	3.58
Israel	2000	2000	449	0.38
Italy	2006	2006	893	0.75
South Korea	1999	1999	3,132	2.64
Malaysia	2001	2001	2,347	1.98
Netherlands	1997	1997	931	0.78
Norway	2005	2005	724	0.61
Pakistan	2002	2002	302	0.25
Philippines	2002	2002	438	0.37
Poland	2002	2002	865	0.73
Portugal	1999	2002	184	0.15
Singapore	2003	2003	1,442	1.21
Sweden	2005	2005	1,843	1.55
Thailand	2002	2002	1,316	1.11
Turkey	1999	1999	867	0.73
US	2003	2003	53,459	45.02
Total			118,728	100%

Table 2
Variable Descriptions

Variables	Descriptions	Sources
Board reform variables		
POST	An indicator variable equal to one starting the year (t = 1) in which the board reform becomes effective in the country and zero otherwise.	Fauver et al. (2017)
POST1	An indicator variable equal to one starting the year (t = 1) in which at least one component of board reform becomes effective in the country and zero otherwise.	Fauver et al. (2017), Bae et al. (2021)
POST2	An indicator variable equal to one starting the year (t = 1) in which at least two components of board reform become effective in the country and zero otherwise.	Fauver et al. (2017), Bae et al. (2021)
POST3	An indicator variable equal to one starting the year (t = 1) in which all three components of board reform become effective in the country and zero otherwise.	Fauver et al. (2017), Bae et al. (2021)
Insider trading variables		
#TRADE	Natural logarithm of one plus total number of insider trades in a year.	2iQ Global
TVOLUME	Natural logarithm of one plus total shares traded by insiders in a year.	2iQ Global
TVALUE	Natural logarithm of one plus total value traded by insiders in a year.	2iQ Global
Firm-level variables		
LOGSIZE	Natural logarithm of total assets (in millions of US dollars).	Compustat
BMRATIO	Natural logarithm of book assets divided by market capitalization.	Compustat
LEVERAGE	Sum of current debt and long-term debt, divided by total assets (in percentage).	Compustat
ROA	Net income divided by total assets (in percentage).	Compustat
OPACITY	An indicator shows if information opacity measure is larger than the country average value in the pre-reform year. Information opacity is measured as a three-year moving sum of the absolute value of discretionary accruals, where discretionary accruals are estimated with the	Compustat, Hutton et al. (2009), Dechow et al. (1995).

	modified Jones (1991) model, following Dechow et al. (1995).	
HIGH_ESPREAD	An indicator shows if the effective bid-ask spread is larger than the country average value in the pre-reform year. We measure effective spreads as the natural logarithm of the average of daily effective spread in a year, where daily effective spread is measured as two times the absolute value of trading price minus bid-ask midpoint, all divided by trading price.	Compustat
HIGH_QSPREAD	An indicator shows if the quoted spread is larger than the country average value in the pre-reform year. We measure quoted spreads as the natural logarithm of the average of daily quoted spread in a year, where daily quoted spread is measured as two times the absolute value of ask price minus bid price, all divided by ask price plus bid price.	Compustat
SYNCH	Stock return synchronicity, measured following Morck et al. (2000) and Durnev et al. (2003).	Morck et al. (2000), CRSP, Compustat
Country characteristics		
GNP Per Capita	Natural log of GNP per capita.	WDI World Bank
GNP Growth Rate	Annual growth rate in GNP.	WDI World Bank
(Export + Import)/GDP	Sum of export and import, scaled by GDP (in percentage).	WDI World Bank
Liquidity	Traded stocks / Market capitalization.	WDI World Bank
FDI	Net foreign direct investment / GDP.	WDI World Bank
Developing	Developing country indicator.	IMF
Civil law	An indicator for civil law (French, German, or Scandinavian countries).	La Porta et al. (1998)

Table 3
Descriptive statistics

The table reports the descriptive statistics for the global inside trading sample covering 117,728 firm-year observations across 41 countries spanning 2000 through 2019. Table 2 provides detailed descriptions of the variables.

Variable	Observation	Mean	Std. Dev.	P25	P50	P75
Insider trading measure						
#TRADE	118,728	10.54	8.97	0.00	16.47	18.38
TVOLUME	118,728	6.73	5.97	0.00	9.04	11.87
TVALUE	118,728	7.75	6.80	0.00	10.57	13.78
Major reform indicators						
POST	118,728	0.73	0.44	0.00	1.00	1.00
POST3	118,728	0.20	0.40	0.00	0.00	0.00
POST2	118,728	0.71	0.45	0.00	1.00	1.00
POST1	118,728	0.73	0.44	0.00	1.00	1.00
First reform indicators						
POST	118,728	0.69	0.46	0.00	1.00	1.00
POST3	118,728	0.15	0.36	0.00	0.00	0.00
POST2	118,728	0.69	0.46	0.00	1.00	1.00
POST1	118,728	0.69	0.46	0.00	1.00	1.00
Firm-level controls						
LOGSIZE	118,728	6.43	2.68	4.59	6.35	8.05
BMRATIO	115,962	-0.01	1.06	-0.67	0.00	0.65
LEVERAGE (%)	118,455	22.01	22.85	1.54	17.54	33.83
ROA (%)	117,521	-7.66	40.09	-4.70	2.79	7.05

Table 4

Effect of Board Reforms on Insider Trading

This table shows the results of difference-in-differences regressions of insider trading on board reforms. The dependent variable is insider trading, measured by the number of insider transactions (#*TRADE*), trading value (*TVALUE*), and trading value (*TVOLUME*). Our full sample consists of 118,728 firm-year observations for 23,320 unique firms from 41 countries during the 2000 – 2019 period. Models (1) to (5) report the results for the major reform, while Models (6) to (10) report the results for the first reform. In Models 1 and 2, *POST* is a dummy variable that equals 1 beginning in the year a major board reform becomes effective in the country, and 0 otherwise. Models 3 to 5 provide additional results on the effects of the breadth of board reforms on insider trading. *POST3* is a dummy variable that equals 1 beginning in the year the major reform becomes effective in countries that have passed all three reform components, and 0 otherwise. *POST2* is a dummy variable that equals 1 beginning in the year the major reform becomes effective in countries that have passed at least two reform components, and 0 otherwise. *POST1* is a dummy variable that equals 1 beginning in the year the reform becomes effective in countries that have passed at least one reform component, and 0 otherwise. All variables are defined in Table 2. Year and firm fixed effects are included in all regressions. *t*-statistics are reported beneath each coefficient estimate in parentheses, with standard errors clustered at the country level. Significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

Panel A: Number of Insider Transactions (#*TRADE*) as insider trading measure

	Major reforms					First reforms				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
POST	-2.3200***	-2.5617***				-2.1602***	-2.4104***			
	(-3.41)	(-3.89)				(-3.04)	(-3.54)			
POST3			-2.5840***					-2.5276***		
			(-3.73)					(-3.61)		
POST2				-2.5617***					-2.4104***	
				(-3.89)					(-3.54)	
POST1					-2.5617***					-2.4104***
					(-3.89)					(-3.54)
LOGSIZE		0.3164***	0.3181***	0.3164***	0.3164***		0.3158***	0.3181***	0.3158***	0.3158***
		(5.36)	(5.31)	(5.36)	(5.36)		(5.38)	(5.27)	(5.38)	(5.38)
BMRATIO		-0.7282***	-0.7296***	-0.7282***	-0.7282***		-0.7274***	-0.7286***	-0.7274***	-0.7274***
		(-7.31)	(-7.31)	(-7.31)	(-7.31)		(-7.30)	(-7.29)	(-7.30)	(-7.30)
LEVERAGE		-0.0015	-0.0015	-0.0015	-0.0015		-0.0015	-0.0015	-0.0015	-0.0015

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
		(-1.06)	(-1.07)	(-1.06)	(-1.06)		(-1.07)	(-1.07)	(-1.07)	(-1.07)
ROA		0.0041***	0.0040***	0.0041***	0.0041***		0.0041***	0.0041***	0.0041***	0.0041***
		(6.15)	(6.13)	(6.15)	(6.15)		(6.08)	(5.98)	(6.08)	(6.08)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
S.E. clustered by country	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	118,728	114,579	114,579	114,579	114,579	118,728	114,579	114,579	114,579	114,579
Adjusted R-squared	0.4592	0.4678	0.4678	0.4678	0.4678	0.4591	0.4677	0.4677	0.4677	0.4677

Panel B: Trading value (*TVALUE*) as insider trading measure

	Major reforms					First reforms				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
POST	-1.7503***	-1.9722***				-1.6411***	-1.8614***			
	(-3.17)	(-3.65)				(-2.87)	(-3.33)			
POST3			-1.9518***					-1.9156***		
			(-3.33)					(-3.06)		
POST2				-1.9722***					-1.8614***	
				(-3.65)					(-3.33)	
POST1					-1.9722***					-1.8614***
					(-3.65)					(-3.33)
LOGSIZE		0.3725***	0.3736***	0.3725***	0.3725***		0.3721***	0.3737***	0.3721***	0.3721***
		(8.72)	(8.61)	(8.72)	(8.72)		(8.76)	(8.54)	(8.76)	(8.76)
BMRATIO		-0.7954***	-0.7965***	-0.7954***	-0.7954***		-0.7948***	-0.7958***	-0.7948***	-0.7948***
		(-13.30)	(-13.25)	(-13.30)	(-13.30)		(-13.26)	(-13.21)	(-13.26)	(-13.26)
LEVERAGE		-0.0001	-0.0001	-0.0001	-0.0001		-0.0001	-0.0001	-0.0001	-0.0001
		(-0.12)	(-0.13)	(-0.12)	(-0.12)		(-0.13)	(-0.13)	(-0.13)	(-0.13)
ROA		0.0035***	0.0035***	0.0035***	0.0035***		0.0035***	0.0035***	0.0035***	0.0035***
		(8.46)	(8.49)	(8.46)	(8.46)		(8.32)	(8.25)	(8.32)	(8.32)
Firm fixed effects	Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes	Yes
S.E. clustered by country	Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes	Yes

Observations	118,728	114,579	114,579	114,579	114,579	118,728	114,579	114,579	114,579	114,579
Adjusted R-squared	0.4675	0.4795	0.4795	0.4795	0.4795	0.4674	0.4794	0.4794	0.4794	0.4794

Panel C: Trading volume (*TVOLUME*) as insider trading measure

	Major reforms					First reforms				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
POST	-1.4525***	-1.6295***				-1.3688**	-1.5437***			
	(-2.79)	(-3.16)				(-2.56)	(-2.94)			
POST3			-1.5567***					-1.5334**		
			(-2.80)					(-2.58)		
POST2				-1.6295***					-1.5437***	
				(-3.16)					(-2.94)	
POST1					-1.6295***					-1.5437***
					(-3.16)					(-2.94)
LOGSIZE		0.1327***	0.1332***	0.1327***	0.1327***		0.1324***	0.1333***	0.1324***	0.1324***
		(2.95)	(2.94)	(2.95)	(2.95)		(2.93)	(2.91)	(2.93)	(2.93)
BMRATIO		-0.3912***	-0.3920***	-0.3912***	-0.3912***		-0.3907***	-0.3914***	-0.3907***	-0.3907***
		(-7.79)	(-7.82)	(-7.79)	(-7.79)		(-7.78)	(-7.79)	(-7.78)	(-7.78)
LEVERAGE		0.0002	0.0002	0.0002	0.0002		0.0002	0.0002	0.0002	0.0002
		(0.18)	(0.17)	(0.18)	(0.18)		(0.17)	(0.17)	(0.17)	(0.17)
ROA		0.0019***	0.0019***	0.0019***	0.0019***		0.0019***	0.0019***	0.0019***	0.0019***
		(3.26)	(3.29)	(3.26)	(3.26)		(3.23)	(3.24)	(3.23)	(3.23)
Firm fixed effects	Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes	Yes
S.E. clustered by country	Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes	Yes
Observations	118728	114579	114579	114579	114579	118728	114579	114579	114579	114579
Adjusted R-squared	0.4293	0.4364	0.4363	0.4364	0.4364	0.4293	0.4363	0.4363	0.4363	0.4363

Table 5

Effect of Board Reforms on Insider Trading: Additional Analyses

This table shows the results of difference-in-differences regressions of insider trading on board reforms using different model specifications. The dependent variable is insider trading, measured by the number of insider transactions *(#TRADE)*, trading value (*TVALUE*), and trading value (*TVOLUME*). In Models (1) and (2), *POST* is a dummy variable that equals 1 beginning in the year a major board reform becomes effective in the country, and 0 otherwise. Models (2) to (4) provide additional results on the effects of the breadth of board reforms on insider trading. Model (5) reports the results when additional country-level characteristics are included in the model. *POST3* is a dummy variable that equals 1 beginning in the year the major reform becomes effective in countries that have passed all three reform components, and 0 otherwise. *POST2* is a dummy variable that equals 1 beginning in the year the major reform becomes effective in countries that have passed at least two reform components, and 0 otherwise. *POST1* is a dummy variable that equals 1 beginning in the year the reform becomes effective in countries that have passed at least one reform component, and 0 otherwise. All variables are defined in Table 2. Significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

Panel A: Number of Insider Transactions (#*TRADE*) as insider trading measure

	Dependent variable: Insider Trading measure				
	(1)	(2)	(3)	(4)	(5)
POST	-2.2635***				-4.6218***
	(-9.43)				(-3.64)
POST3		-2.2564***			
		(-4.97)			
POST2			-2.2649***		
			(-6.26)		
POST1				-2.2635***	
				(-9.43)	
LOGSIZE	1.1813***	1.1762***	1.1733***	1.1813***	1.2012***
	(53.23)	(52.76)	(52.68)	(53.23)	(50.93)
BMRATIO	-1.1055***	-1.1214***	-1.1142***	-1.1055***	-1.1831***
	(-26.10)	(-26.38)	(-26.25)	(-26.10)	(-25.88)
LEVERAGE	-0.0037**	-0.0037**	-0.0037**	-0.0037**	-0.0014
	(-1.97)	(-1.97)	(-1.98)	(-1.97)	(-0.72)
ROA	0.0117***	0.0118***	0.0119***	0.0117***	0.0119***
	(10.29)	(10.35)	(10.39)	(10.29)	(10.06)
GNP	1.3880***	1.3985***	1.4306***	1.3880***	-1.1581
	(7.63)	(6.86)	(7.38)	(7.63)	(-0.98)
GNP GROWTH	-0.0805	-0.0557	-0.0654	-0.0805	0.1702*
	(-1.50)	(-1.03)	(-1.22)	(-1.50)	(1.73)
TRADE RATIO					0.0252***
					(2.65)
MARKET TURNOVER					0.0100**
					(2.26)
FDI					-0.0482**
					(-2.34)
DEVELOPING					-5.1144**
					(-2.17)

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	(1)	(2)	(3)	(4)	(5)
CIVIC LAW					2.1644
					(1.63)
Industry × Year fixed effects	Yes	Yes	Yes	Yes	Yes
Country × Year fixed effects	Yes	Yes	Yes	Yes	Yes
S.E. clustered by firm	Yes	Yes	Yes	Yes	Yes
Observations	105,903	105,903	105,903	105,903	88,973
Adjusted R-squared	0.2351	0.2338	0.2341	0.2351	0.2287

Panel B: Trading value (*TVALUE*) as insider trading measure

	Dependent variable: Insider Trading measure				
	(1)	(2)	(3)	(4)	(5)
POST	-1.7047***				-3.6334***
	(-9.08)				(-3.69)
POST3		-2.0309***			
		(-5.80)			
POST2			-1.8028***		
			(-6.51)		
POST1				-1.7047***	
				(-9.08)	
LOG_SIZE	1.1330***	1.1308***	1.1273***	1.1330***	1.1663***
	(67.43)	(66.95)	(66.77)	(67.43)	(65.76)
BM_RATIO	-1.1458***	-1.1599***	-1.1526***	-1.1458***	-1.2446***
	(-36.34)	(-36.66)	(-36.48)	(-36.34)	(-36.90)
LEVERAGE	-0.0010	-0.0010	-0.0010	-0.0010	0.0012
	(-0.73)	(-0.73)	(-0.75)	(-0.73)	(0.82)
ROA	0.0088***	0.0088***	0.0089***	0.0088***	0.0090***
	(10.91)	(10.94)	(11.01)	(10.91)	(10.73)
GNP	1.2770***	1.3632***	1.3333***	1.2770***	-1.0584
	(9.16)	(8.78)	(9.03)	(9.16)	(-1.16)
GNP GROWTH	-0.0084	0.0132	0.0034	-0.0084	0.1518**
	(-0.20)	(0.32)	(0.08)	(-0.20)	(2.01)
TRADE RATIO					0.0228***
					(3.05)
MARKET TURNOVER					0.0095***
					(2.77)
FDI					-0.0419**
					(-2.50)
DEVELOPING					-5.0235***
					(-2.77)
CIV_COM					1.7022*
					(1.65)
Industry × Year fixed effects	Yes	Yes	Yes	Yes	Yes
Country × Year fixed effects	Yes	Yes	Yes	Yes	Yes
S.E. clustered by firm	Yes	Yes	Yes	Yes	Yes
Observations	105,903	105,903	105,903	105,903	88,973
Adjusted R-squared	0.2614	0.2603	0.2604	0.2614	0.2621

Panel C: Trading volume (*TVOLUME*) as insider trading measure

	Dependent variable: Insider Trading measure				
	(1)	(2)	(3)	(4)	(5)
POST	-1.7360***				-1.8198**
	(-11.03)				(-2.11)
POST3		-1.2631***			
		(-4.12)			
POST2			-1.2469***		
			(-5.26)		
POST1				-1.7360***	
				(-11.03)	
LOGSIZE	0.7565***	0.7502***	0.7485***	0.7565***	0.7661***
	(50.06)	(49.31)	(49.23)	(50.06)	(48.01)
BMRATIO	-0.6888***	-0.6980***	-0.6939***	-0.6888***	-0.7661***
	(-23.92)	(-24.10)	(-24.01)	(-23.92)	(-25.01)
LEVERAGE	-0.0010	-0.0010	-0.0010	-0.0010	0.0013
	(-0.77)	(-0.78)	(-0.79)	(-0.77)	(0.99)
ROA	0.0031***	0.0032***	0.0033***	0.0031***	0.0037***
	(3.94)	(4.08)	(4.11)	(3.94)	(4.52)
GNP	0.4897***	0.3874***	0.4001***	0.4897***	-1.5918**
	(4.09)	(2.95)	(3.20)	(4.09)	(-1.99)
GNP GROWTH	0.0015	0.0163	0.0108	0.0015	0.0486
	(0.04)	(0.45)	(0.30)	(0.04)	(0.72)
TRADE RATIO					0.0041
					(0.62)
MARKET TURNOVER					0.0079***
					(2.65)
FDI					-0.0434***
					(-3.00)
DEVELOPING					-4.0087**
					(-2.52)
CIV_COM					-0.5411
					(-0.60)
Industry × Year fixed effects	Yes	Yes	Yes	Yes	Yes
Country × Year fixed effects	Yes	Yes	Yes	Yes	Yes
S.E. clustered by firm	Yes	Yes	Yes	Yes	Yes
Observations	105,903	105,903	105,903	105,903	88,973
Adjusted R-squared	0.1908	0.1886	0.1888	0.1908	0.1914

Table 6

Board Reform and Insider Trading: First Difference Model, Propensity-Score Matching, and Entropy-Balancing

This table shows the results of regressions of insider trading on board reforms using three approaches. In Panel A, we employ a first difference model to mitigate unobserved heterogeneity in panel data. In Panel B, we adopt a propensity-score matching approach to generate a matched sample of treatment and control firms surrounding board reforms. In Panel C, we use the entropy-balancing weighted method developed by Hainmueller (2012). The dependent variable is insider trading, measured by the number of insider transactions *(#TRADE)*, trading value *(TVALUE)*, and trading value *(TVOLUME)*. All variables are defined in Table 2. Significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

Panel A: First Difference Model

	Dependent variable: Δ Insider Trading											
	Number of Insider Transactions (*#TRADE*)				Trading volume (*TVOLUME*)				Trading value (*TVALUE*)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
POST	-1.2452***				-0.8645***				-1.0468***			
	(-7.98)				(-8.51)				(-8.78)			
POST3		-1.5058***				-0.9759***				-1.2408***		
		(-5.54)				(-5.25)				(-5.98)		
POST2			-1.2452***				-0.8645***				-1.0468***	
			(-7.98)				(-8.51)				(-8.78)	
POST1				-1.2452***				-0.8645***				-1.0468***
				(-7.98)				(-8.51)				(-8.78)
ΔLOGSIZE	0.0919	0.0933	0.0919	0.0919	-0.0343	-0.0336	-0.0343	-0.0343	0.0419	0.0430	0.0419	0.0419
	(0.76)	(0.78)	(0.76)	(0.76)	(-0.31)	(-0.30)	(-0.31)	(-0.31)	(0.42)	(0.43)	(0.42)	(0.42)
ΔBMRATIO	-0.1987***	-0.1993***	-0.1987***	-0.1987***	-0.0520	-0.0524	-0.0520	-0.0520	-0.2094***	-0.2099***	-0.2094***	-0.2094***
	(-5.69)	(-5.73)	(-5.69)	(-5.69)	(-1.34)	(-1.36)	(-1.34)	(-1.34)	(-6.66)	(-6.71)	(-6.66)	(-6.66)
ΔLEVERAGE	-0.0008	-0.0008	-0.0008	-0.0008	0.0012	0.0012	0.0012	0.0012	0.0006	0.0006	0.0006	0.0006
	(-0.57)	(-0.57)	(-0.57)	(-0.57)	(0.99)	(0.98)	(0.99)	(0.99)	(0.48)	(0.47)	(0.48)	(0.48)
ΔROA	-0.0016**	-0.0016**	-0.0016**	-0.0016**	-0.0014**	-0.0014**	-0.0014**	-0.0014**	-0.0005	-0.0005	-0.0005	-0.0005
	(-2.30)	(-2.31)	(-2.30)	(-2.30)	(-2.20)	(-2.21)	(-2.20)	(-2.20)	(-0.69)	(-0.70)	(-0.69)	(-0.69)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	77,252	77,252	77,252	77,252	77,252	77,252	77,252	77,252	77,252	77,252	77,252	77,252
Adjusted R-squared	0.0217	0.0218	0.0217	0.0217	0.0199	0.0199	0.0199	0.0199	0.0248	0.0249	0.0248	0.0248

Panel B: Regression analysis for Propensity-Score Matching

	(1)	(2)	(3)	(4)	(5)	(6)
	#TRADE	TVOLUME	TVALUE	#TRADE	TVOLUME	TVALUE
Treat × Post	-5.3534***	-3.8415***	-4.3448***	-3.9758***	-2.7995***	-3.3262***
	(-7.14)	(-7.03)	(-7.51)	(-5.99)	(-5.70)	(-6.46)
Post	0.0524	-0.6583	-0.2460	-0.0677	-0.4658	-0.2399
	(0.08)	(-1.46)	(-0.51)	(-0.13)	(-1.16)	(-0.57)
Treat	4.5880***	3.5225***	3.2196***			
	(6.24)	(6.57)	(5.67)			
LOGSIZE	0.1104***	0.1217***	0.3362***	-0.1831	-0.1577*	0.0667
	(2.67)	(4.04)	(10.56)	(-1.52)	(-1.77)	(0.72)
BMRATIO	-1.0151***	-0.6016***	-1.0691***	-0.7521***	-0.4500***	-0.7933***
	(-10.15)	(-8.25)	(-13.85)	(-6.10)	(-4.93)	(-8.30)
LEVERAGE	-0.0116***	-0.0071**	-0.0062*	-0.0085	-0.0037	-0.0054
	(-2.70)	(-2.26)	(-1.87)	(-1.56)	(-0.93)	(-1.27)
ROA	0.0159***	0.0097***	0.0115***	0.0049*	0.0030	0.0035*
	(7.24)	(6.10)	(6.83)	(1.89)	(1.55)	(1.76)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	No	No	No	Yes	Yes	Yes
Observations	7,925	7,925	7,925	7,907	7,907	7,907
Adjusted R-squared	0.0877	0.0685	0.1128	0.3537	0.3211	0.3648

Panel C: Regression analysis for Entropy-Balancing Weighted

	(1)	(2)	(3)	(4)	(5)	(6)
	#TRADE	*TVOLUME*	*TVALUE*	*#TRADE*	*TVOLUME*	*TVALUE*
Treat × Post	-2.6335***	-2.1271***	-1.9428***	-1.6385***	-1.3439***	-1.3179***
	(-5.62)	(-6.86)	(-5.49)	(-4.02)	(-4.87)	(-4.27)
Post	-0.7790*	-0.7819***	-0.5598*	-0.8419**	-0.4622**	-0.6349**
	(-1.93)	(-2.93)	(-1.84)	(-2.45)	(-1.99)	(-2.44)
Treat	5.3377***	3.2922***	3.7027***			
	(11.52)	(10.74)	(10.58)			
LOGSIZE	0.8581***	0.5583***	0.9169***	-0.8575***	-0.6684***	-0.3437***
	(49.58)	(48.75)	(70.13)	(-18.13)	(-20.87)	(-9.60)
BMRATIO	-2.2743***	-1.3323***	-2.0460***	-0.9701***	-0.6028***	-1.0358***
	(-58.87)	(-52.12)	(-70.11)	(-18.75)	(-17.21)	(-26.45)
LEVERAGE	0.0144***	0.0075***	0.0080***	-0.0003	-0.0042***	-0.0059***
	(9.65)	(7.61)	(7.06)	(-0.17)	(-3.22)	(-4.06)
ROA	0.0129***	0.0053***	0.0140***	-0.0093***	-0.0057***	-0.0017
	(10.52)	(6.51)	(15.20)	(-6.66)	(-6.02)	(-1.62)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	No	No	No	Yes	Yes	Yes
Observations	52,810	52,810	52,810	52,609	52,609	52,609
Adjusted R-squared	0.2086	0.1712	0.2392	0.4674	0.4162	0.4861

Table 7
Channel analyses

This table shows the results of difference-in-differences regressions of insider trading on board reforms conditional on the pre-reform corporate information environment. To capture the corporate information environment, we use three measures of information asymmetry, including (*i*) information opacity (Hutton et al., 2009), (*ii*) effective bid-ask spreads, and (*iii*) quoted spreads. To capture high levels of information asymmetry before the reforms, for each measure of information asymmetry, we create an indicator (*OPACITY*, *HIGH_ESPREAD*, or *HIGH_QSPREAD*) that equals one if this measure is higher than the country's average value in the pre-reform year and zero otherwise. *POST* is a dummy variable that equals 1 beginning in the year a major board reform becomes effective in the country, and 0 otherwise. All variables are defined in Table 2. Year fixed effects and firm fixed effects are included in all regressions. *t*-statistics are reported beneath each coefficient estimate in parentheses, with standard errors clustered at the country level. Significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

	(1) #TRADE	(2) #TRADE	(3) #TRADE	(4) TVOLUME	(5) TVOLUME	(6) TVOLUME	(7) TVALUE	(8) TVALUE	(9) TVALUE
POST	-3.1688***	-2.5629***	-2.3678***	-2.0714***	-1.7029***	-1.5854***	-2.4101***	-1.9969***	-1.8512***
	(-4.71)	(-4.86)	(-4.39)	(-3.33)	(-3.47)	(-3.11)	(-3.90)	(-4.09)	(-3.72)
POST × OPACITY	-1.3356***			-0.6371**			-1.0212***		
	(-2.64)			(-2.28)			(-2.94)		
POST × HIGH_ESPREAD		-1.6659***			-0.8892***			-1.2995***	
		(-2.89)			(-2.67)			(-3.02)	
POST × HIGH_QSPREAD			-2.2401***			-1.2384***			-1.7267***
			(-4.51)			(-4.19)			(-4.47)
LOGSIZE	0.1509***	0.1284**	0.1273**	-0.0285	-0.0387	-0.0393	0.2868***	0.2765***	0.2756***
	(4.20)	(2.41)	(2.41)	(-1.26)	(-1.32)	(-1.34)	(9.32)	(6.83)	(6.87)
BMRATIO	-0.7147***	-0.7919***	-0.7952***	-0.3742***	-0.4170***	-0.4189***	-0.8720***	-0.9156***	-0.9182***
	(-10.66)	(-6.67)	(-6.52)	(-12.15)	(-7.86)	(-7.62)	(-18.49)	(-13.46)	(-13.08)
LEVERAGE	0.0019	0.0026	0.0026	0.0018	0.0021	0.0021	0.0018	0.0017	0.0017
	(0.38)	(0.64)	(0.64)	(0.52)	(0.70)	(0.70)	(0.49)	(0.63)	(0.64)
ROA	0.0070***	0.0078***	0.0079***	0.0042***	0.0045***	0.0046***	0.0057***	0.0062***	0.0063***

	(6.17)	(5.12)	(5.16)	(3.84)	(3.65)	(3.74)	(6.15)	(5.39)	(5.48)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
S.E. clustered by country	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	36,716	42,180	42,180	36,716	42,180	42,180	36,716	42,180	42,180
Adjusted R-squared	0.3934	0.3930	0.3932	0.3777	0.3733	0.3735	0.4381	0.4335	0.4337

Table 8
Board Reforms and Insider Trading: Components and Implementation Approaches of Reforms

This table examines whether the effects of board reforms on insider trading vary across board reform characteristics. Columns (1) and (2) report the results for different reform implementations. Rule-based reforms require mandatory compliance from firms, while comply-or-explain reforms give firms an option to provide the reasons why they choose not to comply with the governance codes listed in the reforms. Columns (3) to (5) report the model estimation across major reform components. Three components of board practices include board independence, audit committee independence, and CEO-Chairman separation. The dependent variable is the insider trading measure. For brevity, we report the results for the number of insider trades only. *POST* is a dummy variable that equals 1 beginning in the year a major board reform becomes effective in the country, and 0 otherwise. All variables are defined in Table 2. Year and firm fixed effects are included in all regressions. *t*-statistics are reported beneath each coefficient estimate in parentheses, with standard errors clustered at the country level. Significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

	(1) Rule-based	(2) Comply-or-explain	(3) Board Independence	(4) Audit Committee Independence	(5) CEO-Chairman Reform
POST	-2.6015***	-3.0809	-2.5556***	-2.5598***	-3.2522
	(-4.80)	(-1.21)	(-3.85)	(-3.87)	(-1.54)
LOGSIZE	0.2557***	0.4363**	0.3261***	0.3214***	0.2967***
	(3.64)	(2.20)	(5.53)	(5.44)	(3.71)
BMRATIO	-0.7540***	-0.5873***	-0.7280***	-0.7334***	-0.9794***
	(-6.66)	(-5.45)	(-7.15)	(-7.15)	(-6.63)
LEVERAGE	-0.0011	0.0009	-0.0010	-0.0011	0.0028
	(-0.76)	(0.21)	(-0.76)	(-0.80)	(1.64)
ROA	0.0041***	0.0052	0.0039***	0.0041***	0.0050***
	(10.51)	(1.66)	(6.01)	(6.24)	(3.96)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
S.E. clustered by country	Yes	Yes	Yes	Yes	Yes
Observations	86,992	275,87	110,215	111,068	32,549
Adjusted R-squared	0.4790	0.3735	0.4674	0.4670	0.4179

Table 9

The moderating effect of country-level institutions

This table shows the results of difference-in-differences regressions of insider trading on board reforms conditional on country-level institutions. In Panel A, we consider four external governance mechanisms, including (i) the degree of the anti-director index, (ii) the effectiveness of the judicial system, (iii) the stringency of public enforcement, and (iv) legal origin. Panel B shows the results of difference-in-differences regressions of insider trading on board reforms conditional on the country's information environment. To capture the country's information environment, we use three measures, including the disclosure requirement index (La Porta, Lopez-de-Silanes, and Shleifer, 2006), corruption perception index (Transparency International, 2019), and financial reporting quality (Bushman, Piotroski, and Smith, 2004). We define high and low groups based on the sample median of the variable distribution. The dependent variable is the insider trading measure. For brevity, we report the results for the number of insider trades only. *POST* is a dummy variable that equals 1 beginning in the year a major board reform becomes effective in the country, and 0 otherwise. All variables are defined in Table 2. Year and firm fixed effects are included in all regressions. *t*-statistics are reported beneath each coefficient estimate in parentheses, with standard errors clustered at the country level. Significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

Panel A: The moderating effect of investor protection and public enforcement

	(1)		(2)		(3)		(4)	
	Anti-director index		Effective Judiciary		Public enforcement		Legal origin	
	High	Low	High	Low	High	Low	Civil Law	Common Law
POST	-3.3552**	0.9482	-2.8511***	-0.5086	-3.4042***	1.2801	1.0678	-3.5467***
	(-2.01)	(0.61)	(-4.65)	(-0.44)	(-7.02)	(1.16)	(1.03)	(-7.62)
LOGSIZE	0.2722***	0.4395***	0.3192***	0.4266**	0.2836***	0.9181***	0.6287***	0.2892***
	(4.01)	(6.75)	(4.93)	(2.53)	(5.02)	(3.45)	(3.10)	(4.48)
BMRATIO	-0.8634***	-0.6395***	-0.7232***	-0.7681***	-0.7217***	-0.6117**	-0.6336***	-0.7505***
	(-5.74)	(-18.25)	(-6.43)	(-4.14)	(-6.85)	(-2.43)	(-3.14)	(-6.12)
LEVERAGE	0.0001	-0.0027*	-0.0008	-0.0165**	-0.0011	-0.0075	-0.0113*	-0.0001
	(0.04)	(-1.82)	(-0.60)	(-2.19)	(-0.80)	(-1.09)	(-1.84)	(-0.08)
ROA	0.0047***	0.0031***	0.0041***	0.0017	0.0045***	-0.0055*	-0.0029	0.0045***
	(3.49)	(5.00)	(5.82)	(0.16)	(6.49)	(-1.79)	(-0.65)	(5.93)

Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
S.E. clustered by country	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	42,456	72,123	98,405	16,174	101,879	12,700	17,703	88,222
Adjusted R-squared	0.0191	0.0552	0.4590	0.3893	0.4652	0.4091	0.4101	0.4367

Panel B: The moderating effect of countries' information environment

	(1) Disclose requirement		(2) Corruption perception		(3) Financial Reporting Quality	
	High	Low	High	Low	High	Low
POST	-2.6173***	-0.5720	-3.0176***	3.3170	-2.9232***	0.2794
	(-3.80)	(-1.52)	(-5.21)	(1.40)	(-4.79)	(0.22)
LOGSIZE	0.2991***	0.6925**	0.2802***	0.5429***	0.3090***	0.5839***
	(5.03)	(2.38)	(3.60)	(3.58)	(4.92)	(3.00)
BMRATIO	-0.7063***	-1.1923***	-0.7762***	-0.6031***	-0.7173***	-0.8533***
	(-6.85)	(-3.81)	(-6.00)	(-4.27)	(-6.47)	(-4.15)
LEVERAGE	-0.0011	-0.0071	-0.0004	-0.0156**	-0.0014	-0.0084
	(-0.84)	(-0.59)	(-0.17)	(-2.28)	(-1.00)	(-0.83)
ROA	0.0044***	-0.0074	0.0045***	-0.0067	0.0041***	-0.0001
	(6.46)	(-1.07)	(5.06)	(-1.12)	(5.82)	(-0.01)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
S.E. clustered by country	Yes	Yes	Yes	Yes	Yes	Yes
Observations	107,662	6,917	85,941	22,749	99,920	14,659
Adjusted R-squared	0.4682	0.3757	0.4463	0.4497	0.4600	0.3802

Table 10
Board Reforms and Insider Trading Profit

This table shows the results of difference-in-differences regressions of insider trading on board reforms on insider trading profit. The dependent variable is insider trading profit, measured as the alpha from the three-factor model estimated over the 180 days following each transaction (e.g., Jagolinzer, Larcker, and Taylor, 2011). In Models (1), POST is a dummy variable that equals 1 beginning in the year a major board reform becomes effective in the country, and 0 otherwise. Models (2) to (4) provide additional results on the effects of the breadth of board reforms on insider trading. POST3 is a dummy variable that equals 1 beginning in the year the major reform becomes effective in countries that have passed all three reform components, and 0 otherwise. POST2 is a dummy variable that equals 1 beginning in the year the major reform becomes effective in countries that have passed at least two reform components, and 0 otherwise. POST1 is a dummy variable that equals 1 beginning in the year the reform becomes effective in countries that have passed at least one reform component, and 0 otherwise. All variables are defined in Table 2. Firm-, trading day-, and country-fixed effects are included in all regressions. t-statistics are reported beneath each coefficient estimate in parentheses, with standard errors clustered at the country level. Significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

	Major reforms				First reforms			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
POST	-0.8801**				-1.4727***			
	(-2.14)				(-4.29)			
POST3		-1.1021***				-1.3854***		
		(-3.23)				(-4.49)		
POST2			-0.8013**				-1.4727***	
			(-2.04)				(-4.29)	
POST1				-0.8801**				-1.4727***
				(-2.14)				(-4.29)
LOG_SIZE	-0.1121**	-0.1130**	-0.1083**	-0.1121**	-0.1276***	-0.1119**	-0.1276***	-0.1276***
	(-2.53)	(-2.49)	(-2.39)	(-2.53)	(-3.00)	(-2.43)	(-3.00)	(-3.00)
BM_RATIO	0.2669*	0.2790*	0.2618	0.2669*	0.2470	0.2759*	0.2470	0.2470
	(1.73)	(1.83)	(1.69)	(1.73)	(1.56)	(1.79)	(1.56)	(1.56)

LEVERAGE	0.0028	0.0025	0.0029	0.0028	0.0030	0.0025	0.0030	0.0030
	(1.38)	(1.25)	(1.44)	(1.38)	(1.60)	(1.28)	(1.60)	(1.60)
ROA	0.0018	0.0015	0.0019	0.0018	0.0012	0.0019	0.0012	0.0012
	(0.52)	(0.43)	(0.54)	(0.52)	(0.36)	(0.56)	(0.36)	(0.36)
Firm × Trading Day fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country × Trading Day fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
S.E. clustered by country	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	206,077	206,077	206,077	206,077	206,077	206,077	206,077	206,077
Adjusted R-squared	0.0113	0.0114	0.0113	0.0113	0.0118	0.0116	0.0118	0.0118

Table 11

Board Reforms and Stock Return Synchronicity

This table shows the results of difference-in-differences regressions of stock return synchronicity on board reforms. The dependent variable is stock return synchronicity, measured as $SYNCH = log(R^2/(1 - R^2))$; higher synchronicity indicates firm stock returns are closely tied to market returns and reflect less firm-specific information. Specifically, R^2 is the R-squared (or the coefficient of determination) from a market model that we regress stock returns on the market returns (e.g., Piotroski and Roulstone, 2004). For non-US firms, we further control the US market returns in the market model. In Models (1), *POST* is a dummy variable that equals 1 beginning in the year a major board reform becomes effective in the country, and 0 otherwise. Models (2) to (4) provide additional results on the effects of the breadth of board reforms on insider trading. *POST3* is a dummy variable that equals 1 beginning in the year the major reform becomes effective in countries that have passed all three reform components, and 0 otherwise. *POST2* is a dummy variable that equals 1 beginning in the year the major reform becomes effective in countries that have passed at least two reform components, and 0 otherwise. *POST1* is a dummy variable that equals 1 beginning in the year the reform becomes effective in countries that have passed at least one reform component, and 0 otherwise. All variables are defined in Table 2. Firm and year fixed effects are included in all regressions. *t*-statistics are reported beneath each coefficient estimate in parentheses, with standard errors clustered at the country level. Significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

	Major reforms				First reforms			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
POST	0.3547***				0.2995***			
	(3.02)				(2.84)			
POST3		0.2197***				0.2139***		
		(3.44)				(4.49)		
POST2			0.3547***				0.2995***	
			(3.02)				(2.84)	
POST1				0.3547***				0.2995***
				(3.02)				(2.84)
LOGSIZE	0.1082***	0.1082***	0.1082***	0.1082***	0.1082***	0.1082***	0.1082***	0.1082***
	(3.18)	(3.18)	(3.18)	(3.18)	(3.18)	(3.18)	(3.18)	(3.18)
BM_RATIO	-0.0997**	-0.0996**	-0.0997**	-0.0997**	-0.0995**	-0.0995**	-0.0995**	-0.0995**
	(-2.69)	(-2.69)	(-2.69)	(-2.69)	(-2.69)	(-2.69)	(-2.69)	(-2.69)
LEVERAGE	0.0012***	0.0012***	0.0012***	0.0012***	0.0012***	0.0012***	0.0012***	0.0012***
	(4.48)	(4.49)	(4.48)	(4.48)	(4.50)	(4.51)	(4.50)	(4.50)
ROA	0.0002	0.0002	0.0002	0.0002	0.0002	0.0002	0.0002	0.0002

	(0.57)	(0.57)	(0.57)	(0.57)	(0.56)	(0.56)	(0.56)	(0.56)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
S.E. clustered by country	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	64,773	64,773	64,773	64,773	64,773	64,773	64,773	64,773
Adjusted R-squared	0.0356	0.0356	0.0356	0.0356	0.0356	0.0355	0.0356	0.0356

Table 12
Alternative sample to address confounding effects

This table shows the results of difference-in-differences regressions of insider trading on board reforms based on alternative samples to address confounding effects. We follow Fauver, Hung, Li, and Taboada, 2017) and restrict our sample period to five years before and after the reform (i.e., [-5, +5] sample). The dependent variable is the insider trading measure. For brevity, we report the results for the number of insider trades only. In Model (1), *POST* is a dummy variable that equals 1 beginning in the year a major board reform becomes effective in the country, and 0 otherwise. Models (2) to (4) provide additional results on the effects of the breadth of board reforms on insider trading. *POST3* is a dummy variable that equals 1 beginning in the year the major reform becomes effective in countries that have passed all three reform components, and 0 otherwise. *POST2* is a dummy variable that equals 1 beginning in the year the major reform becomes effective in countries that have passed at least two reform components, and 0 otherwise. *POST1* is a dummy variable that equals 1 beginning in the year the reform becomes effective in countries that have passed at least one reform component, and 0 otherwise. Panels A and B report the results for major reforms and first reforms, respectively. All variables are defined in Table 2. Year and firm fixed effects are included in all regressions. *t*-statistics are reported beneath each coefficient estimate in parentheses, with standard errors clustered at the country level. Significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

Panel A: Major reforms

	Dependent variable: Insider trading measure			
	(1)	(2)	(3)	(4)
POST	-2.3573***			
	(-5.34)			
POST3		-2.3284***		
		(-3.99)		
POST2			-2.3573***	
			(-5.34)	
POST1				-2.3573***
				(-5.34)
LOGSIZE	0.1447	0.1455	0.1447	0.1447
	(0.87)	(0.86)	(0.87)	(0.87)
BMRATIO	-0.4725***	-0.4779***	-0.4725***	-0.4725***
	(-7.88)	(-7.91)	(-7.88)	(-7.88)
LEVERAGE	-0.0044	-0.0044	-0.0044	-0.0044
	(-1.26)	(-1.25)	(-1.26)	(-1.26)
ROA	0.0008	0.0008	0.0008	0.0008
	(0.40)	(0.40)	(0.40)	(0.40)
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
S.E. clustered by country	Yes	Yes	Yes	Yes
Observations	32,034	32,034	32,034	32,034
Adjusted R-squared	0.5290	0.5291	0.5290	0.5290

Panel B: First reforms

	Dependent variable: Insider trading measure			
	(1)	(2)	(3)	(4)
POST	-2.2759***			
	(-5.04)			
POST3		-2.0630***		
		(-3.88)		
POST2			-2.2759***	
			(-5.04)	
POST1				-2.2759***
				(-5.04)
LOGSIZE	0.0662	0.0663	0.0662	0.0662
	(0.44)	(0.43)	(0.44)	(0.44)
BMRATIO	-0.4177***	-0.4234***	-0.4177***	-0.4177***
	(-8.51)	(-8.34)	(-8.51)	(-8.51)
LEVERAGE	-0.0057*	-0.0057*	-0.0057*	-0.0057*
	(-1.89)	(-1.86)	(-1.89)	(-1.89)
ROA	0.0006	0.0006	0.0006	0.0006
	(0.32)	(0.32)	(0.32)	(0.32)
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
S.E. clustered by country	Yes	Yes	Yes	Yes
Observations	29,952	29,952	29,952	29,952
Adjusted R-squared	0.5270	0.5269	0.5270	0.5270

Figure 1
Placebo Test

Panel A. Pseudo Major Reform

Panel A presents the estimated coefficients of the Pseudo Major Reform on the number of insider trades. Specifically, for each country, we randomly sample a year between 2000 to 2020 (which is based on the availability of the insider trade data), which is not the true major reform year, and treat it as a pseudo major reform year. We then repeat the regression analysis as in Models (2) - (5) of Panel A of Table 4 (baseline). We repeat this process 1,000 times to get 1,000 pseudo estimated coefficients. Finally, we plot the density plot of the pseudo estimated coefficients for *POST*, *POST3*, *POST2*, and *POST1* in Panels A, B, C, and D, respectively. For comparison, we draw the vertical line (i.e., dashed blue lines) of true estimated coefficients from Panel A of Table 4 (baseline). The below table presents the summary statistics of estimates of board reform effects based on pseudo-reform years.
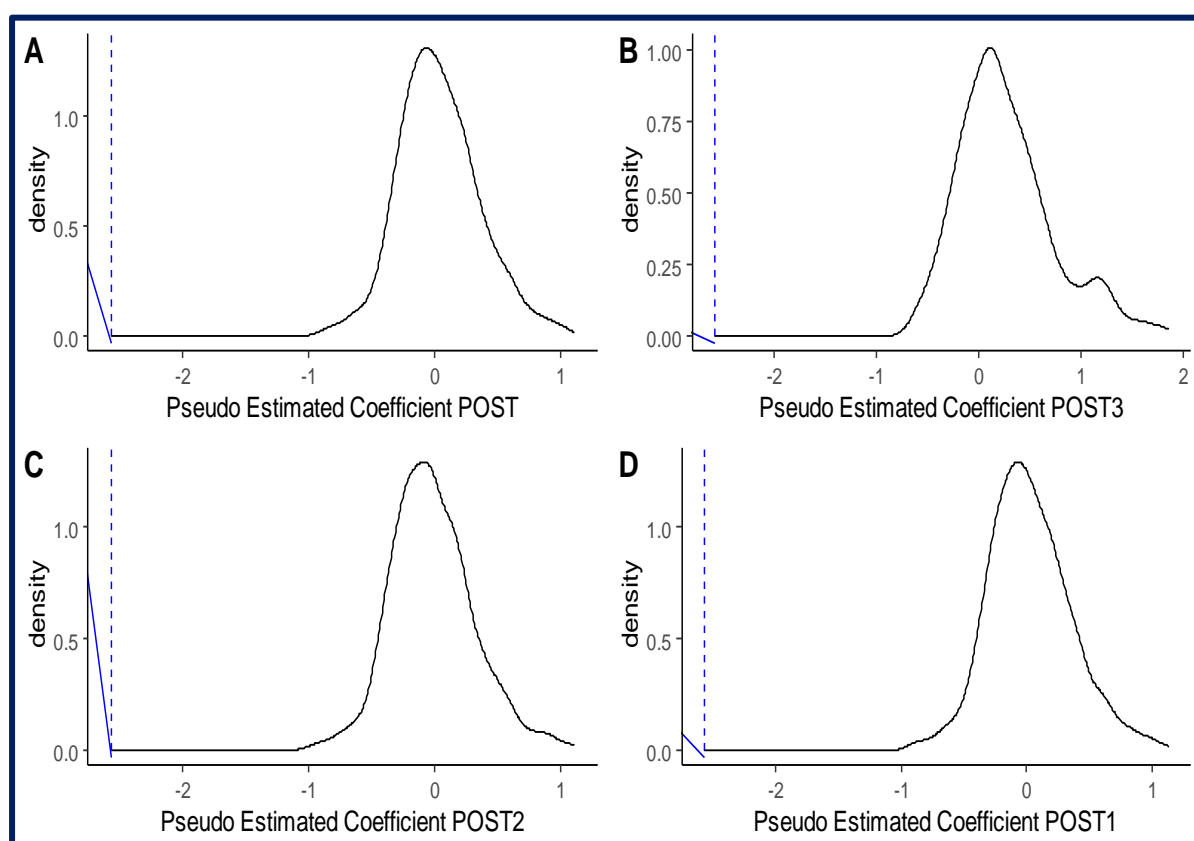
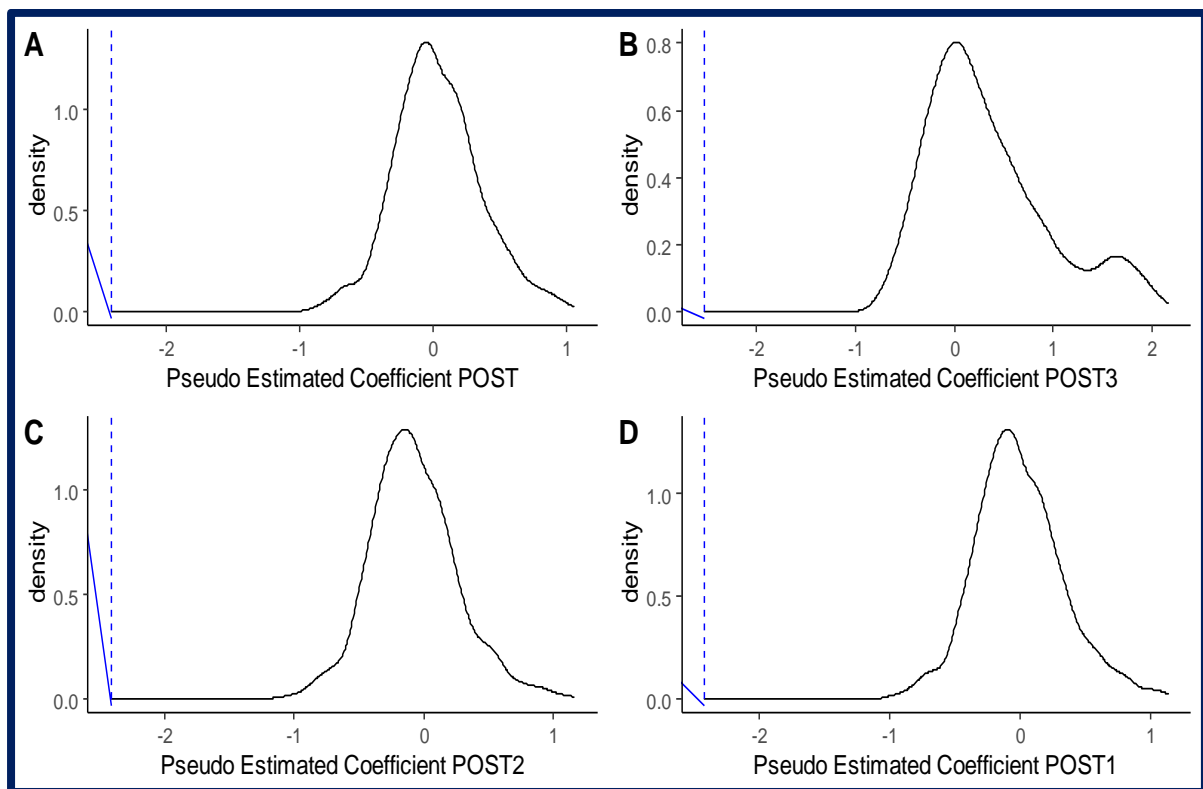| Major Reform	Actual	Observation	Mean	S.D.	P25	P50	P75
POST	-2.5617	1,000	0.0404	0.3229	-0.1783	0.0141	0.2334
POST1	-2.5617	1,000	0.0302	0.3289	-0.1935	0.0011	0.2268
POST2	-2.5617	1,000	-0.0189	0.3287	-0.2397	-0.0479	0.1700
POST3	-2.5840	1,000	0.2689	0.4700	-0.0611	0.1870	0.5086

Panel B: Pseudo First Reform

Panel B presents the estimated coefficients of the Pseudo First Reform on the number of insider trades. Specifically, for each country, we randomly sample a year between 2000 to 2020 (which is based on the availability of the insider trade data), which is not the true first reform year, and treat it as a pseudo first reform year. We then repeat the regression analysis as in Models (2) - (5) of Panel A of Table 4 (baseline model). We repeat this process 1,000 times to get 1,000 pseudo estimated coefficients. Finally, we plot the density plot of the pseudo estimated coefficients for *POST*, *POST3*, *POST2*, and *POST1* in Panels A, B, C, and D, respectively. For comparison, we draw the vertical line (i.e., dashed blue lines) of true estimated coefficients from Panel A of Table 4 (baseline). The below table presents the summary statistics of estimates of board reform effects based on pseudo-reform years.

First Reform	Actual	Observation	Mean	S.D.	P25	P50	P75
POST	-2.4104	1,000	0.0324	0.3229	-0.1797	0.0065	0.2213
POST1	-2.4104	1,000	-0.0099	0.3373	-0.2292	-0.0418	0.1826
POST2	-2.4104	1,000	-0.0753	0.3358	-0.2996	-0.0989	0.1161
POST3	-2.5276	1,000	0.3409	0.6234	-0.1123	0.1991	0.6425

Figure 2

Validation of Parallel Trend Assumption

This figure tests the parallel trend assumption by comparing the mean of three insider trading variables between treatment firms (with board reforms) and control firms (using the propensity score matching). Insider trading measures include #*TRADE*, *TVOLUME*, and *TVALUE*. The treatment firms are firms incorporated in countries that adopted major board reforms. Control firms are firms matched using propensity score matching (details in Section 4.4). The sample period includes seven years surrounding each country's major board reform: three years before the board reform (from -3 to -1), the reform year (year 0), and three years after the board reform (from +1 to +3).
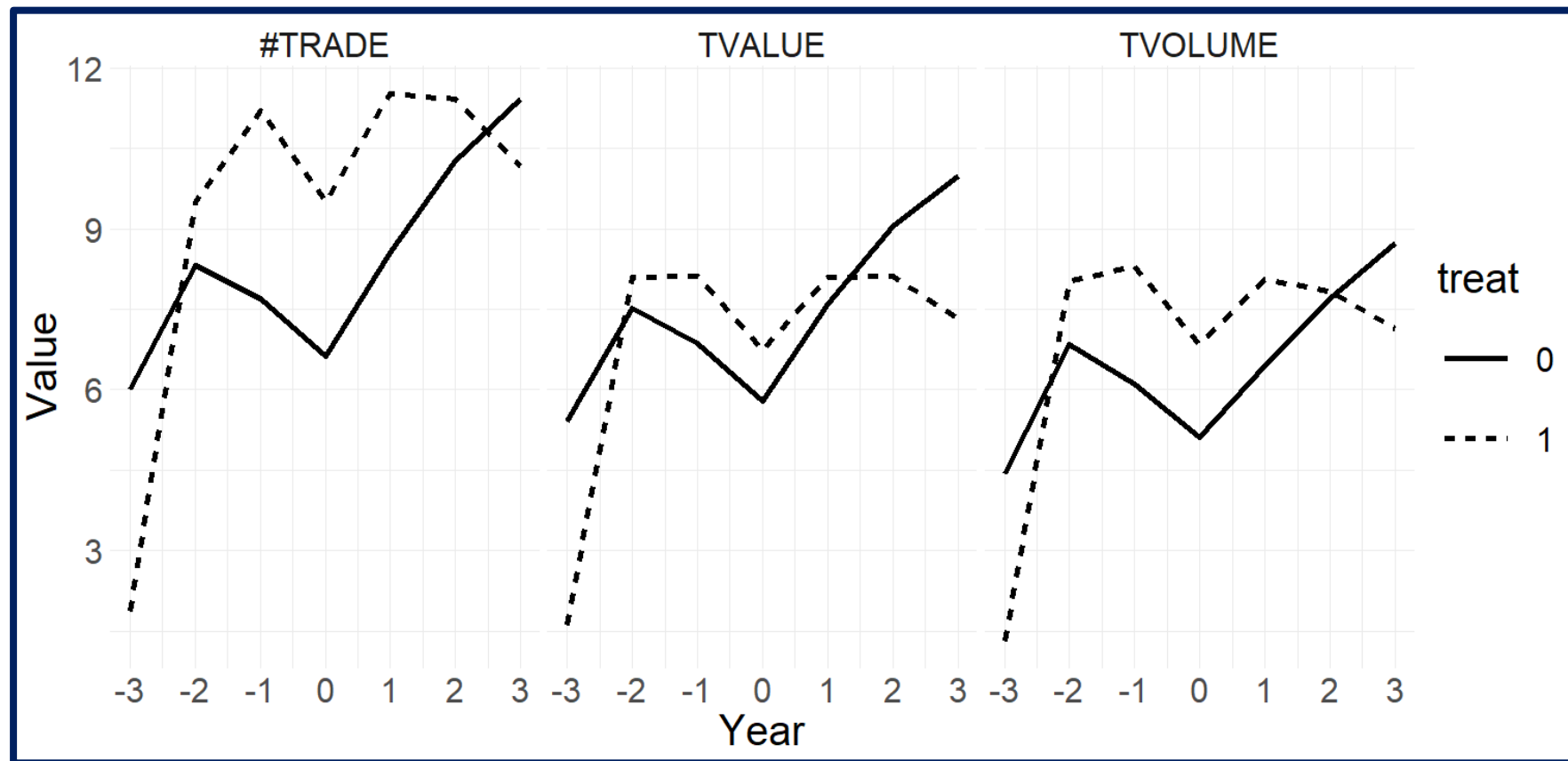
Appendix A1
Country characteristic

Appendix A1 reports the descriptive statistics of country-level attributes in our sample.

Country	Investor Protection	Insider trade restriction	Effective Judicial	Public enforce	Disclose	Accounting standard	Anti-director index	Corruption perception
Australia	0.78	5.59	10.00	0.90	0.75	75.00	4.00	1.00
Austria	0.10	4.83	9.50	0.17	0.25	54.00	2.50	1.00
Belgium	0.07	5.41	9.50	0.15	0.42	61.00	3.00	1.00
Brazil	0.44	3.72	5.75	0.58	0.25	54.00	5.00	0.00
Canada	0.96	5.55	9.25	0.80	0.92	74.00	4.00	1.00
Switzerland	0.30	4.67	10.00	0.33	0.67	68.00	3.00	1.00
Chile	0.61	4.16	7.25	0.60	0.58	52.00	4.00	1.00
China	-	3.45	-	-	-	-	1.00	0.00
Czech Republic	-	2.90	-	-	-	-	4.00	0.00
Germany	0.00	5.24	9.00	0.22	0.42	62.00	3.50	1.00
Denmark	0.36	6.00	10.00	0.37	0.58	62.00	4.00	1.00
Egypt	0.20	3.77	6.50	0.30	0.50	24.00	3.00	0.00
Spain	0.55	4.68	6.25	0.33	0.50	64.00	5.00	0.45
Finland	0.47	5.53	10.00	0.32	0.50	77.00	3.50	1.00
France	0.47	5.17	8.00	0.77	0.75	69.00	3.50	1.00
UK	0.78	5.85	10.00	0.68	0.83	78.00	5.00	1.00
Greece	0.32	3.41	7.00	0.32	0.33	55.00	2.00	0.00
Hong Kong	0.85	3.94	10.00	0.87	0.92	69.00	5.00	1.00
Hungary	-	3.81	-	-	-	-	2.00	0.00
Indonesia	0.51	3.56	2.50	0.62	0.50	-	4.00	0.00
India	0.77	3.53	8.00	0.67	0.92	57.00	5.00	0.00
Israel	0.59	4.39	10.00	0.63	0.67	64.00	4.00	0.12
Italy	0.20	4.38	6.75	0.48	0.67	62.00	2.00	0.00
South Korea	0.36	4.10	6.00	0.25	0.75	62.00	4.50	0.00
Malaysia	0.73	3.42	9.00	0.77	0.92	76.00	5.00	0.00
Netherlands	0.54	5.20	10.00	0.47	0.50	64.00	2.50	1.00
Norway	0.44	4.24	10.00	0.32	0.58	74.00	3.50	1.00
Pakistan	0.63	-	5.00	0.58	0.58	-	4.00	0.00
Philippines	0.81	3.48	4.75	0.83	0.83	65.00	4.00	0.00
Poland	-	3.88	-	-	-	-	2.00	0.00
Portugal	0.57	4.37	5.50	0.58	0.42	36.00	2.50	0.40
Singapore	0.77	5.58	10.00	0.87	1.00	78.00	5.00	1.00
Sweden	0.39	5.58	10.00	0.50	0.58	83.00	3.50	1.00
Thailand	0.37	3.29	3.25	0.72	0.92	64.00	4.00	0.01
Turkey	0.34	3.58	4.00	0.63	0.50	51.00	3.00	0.00
US	1.00	5.64	10.00	0.90	1.00	71.00	3.00	1.00

Appendix A2
Dynamic effects of board reforms

This table reports the dynamic difference-in-differences regression results on the effect of board reforms on insider trading. The dependent variable is insider trading, measured by the number of insider transactions (#TRADE), trading value (TVALUE), and trading value (TVOLUME). The sample period is from year -2 to year +2 relative to the adoption year of the first board reform. *Year (-1)*$_{i,t}$ and *Year (0)*$_{i,t}$ is an indicator that equals one if year *t* is one year before the adoption year or the adoption year of the first board reform of the country where firm *i* incorporated. *Year (+1)*$_{i,t}$ is an indicator that equals one if year *t* is the first year onward after the adoption year of the first board reform of the country. The reference year is the year -2 relative to the reform year. All variables are defined in Table 2. Year and firm fixed effects are included in all regressions. *t*-statistics are reported beneath each coefficient estimate in parentheses, with standard errors clustered at the country level. Significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

	(1)	(2)	(3)
	#TRADE	TVALUE	TVOLUME
Year (-1)	-0.9487	-0.6671	-0.3041
	(-0.72)	(-0.64)	(-0.31)
Year (0)	-2.9392*	-2.2172*	-1.5542
	(-2.06)	(-1.94)	(-1.44)
Year (1+)	-3.4521**	-2.5328**	-1.8422*
	(-2.31)	(-2.15)	(-1.70)
LOGSIZE	0.0945	0.1556	-0.0155
	(0.74)	(1.10)	(-0.09)
BMRATIO	0.0496	0.0302	0.1044
	(0.76)	(0.78)	(1.63)
LEVERAGE	-0.0013	-0.0022	0.0007
	(-0.25)	(-0.51)	(0.15)
ROA	-0.0030	-0.0023	-0.0013
	(-1.02)	(-1.01)	(-0.82)
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
S.E. clustered by country	Yes	Yes	Yes
Observations	14,254	14,254	14,254
Adj. R-squared	0.2011	0.2224	0.2074

Appendix A3
Alternative measures of insider trading: All insider transactions

This table shows the results of difference-in-differences regressions of insider trading on board reforms. The dependent variable is total insider trading, measured by the total number of insider transactions from the 2IQ database. Models (1) to (5) report the results for the major reform, while Models (6) to (10) report the results for the first reform. In Models 1 and 2, *POST* is a dummy variable that equals 1 beginning in the year a major board reform becomes effective in the country, and 0 otherwise. Models 3 to 5 provide additional results on the effects of the breadth of board reforms on insider trading. *POST3* is a dummy variable that equals 1 beginning in the year the major reform becomes effective in countries that have passed all three reform components, and 0 otherwise. *POST2* is a dummy variable that equals 1 beginning in the year the major reform becomes effective in countries that have passed at least two reform components, and 0 otherwise. *POST1* is a dummy variable that equals 1 beginning in the year the reform becomes effective in countries that have passed at least one reform component, and 0 otherwise. All variables are defined in Table 2. Year and firm fixed effects are included in all regressions. *t*-statistics are reported beneath each coefficient estimate in parentheses, with standard errors clustered at the country level. Significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

	Major reforms					First reforms				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
POST	-0.2850***	-0.3037***				-0.2067**	-0.2265**			
	(-4.90)	(-5.53)				(-2.07)	(-2.34)			
POST3			-0.2832***					-0.2062**		
			(-3.09)					(-2.51)		
POST2				-0.3037***					-0.2265**	
				(-5.53)					(-2.34)	
POST1					-0.3037***					-0.2265**
					(-5.53)					(-2.34)
LOGSIZE		0.0634***	0.0635***	0.0634***	0.0634***		0.0630***	0.0630***	0.0630***	0.0630***
		(8.24)	(8.12)	(8.24)	(8.24)		(8.19)	(7.98)	(8.19)	(8.19)
BMRATIO		-0.1455***	-0.1457***	-0.1455***	-0.1455***		-0.1454***	-0.1455***	-0.1454***	-0.1454***
		(-7.50)	(-7.49)	(-7.50)	(-7.50)		(-7.48)	(-7.45)	(-7.48)	(-7.48)
LEVERAGE		-0.0004**	-0.0004**	-0.0004**	-0.0004**		-0.0004**	-0.0004**	-0.0004**	-0.0004**

		(-2.18)	(-2.18)	(-2.18)	(-2.18)			(-2.18)	(-2.18)	(-2.18)	(-2.18)
ROA		0.0009***	0.0009***	0.0009***	0.0009***			0.0009***	0.0009***	0.0009***	0.0009***
		(7.56)	(7.69)	(7.56)	(7.56)			(7.66)	(7.85)	(7.66)	(7.66)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
S.E. clustered by country	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	118,736	114,587	114,587	114,587	114,587	118,736	114,587	114,587	114,587	114,587
Adjusted R-squared	0.0969	0.1134	0.1133	0.1134	0.1134	0.0965	0.1130	0.1130	0.1130	0.1130

Appendix A4
Country-level insider trading

This table shows the results of difference-in-differences regressions of country-level insider trading on board reforms. The dependent variable is country-level insider trading, measured by the total number of insider transactions (*#TRADE*), trading value (*TVALUE*), and trading value (*TVOLUME*) in each country year. *POST* is a dummy variable that equals 1 beginning in the year a major board reform becomes effective in the country, and 0 otherwise. Year and country fixed effects are included in all regressions. *t*-statistics based on robust standard errors are reported beneath each coefficient estimate in parentheses. Significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

	Dependent variable: Country-level Insider Trading		
	(1)	(2)	(3)
	#TRADE	*TVOLUME*	*TVALUE*
POST	-3.1543***	-1.7505*	-2.0029**
	(-2.81)	(-1.78)	(-2.01)
SIZE	-0.1046	0.0382	0.1471
	(-0.56)	(0.23)	(0.89)
BM	-0.5823*	-0.6347**	-0.5275*
	(-1.67)	(-2.08)	(-1.70)
LEV	-0.0059	-0.0042	-0.0064
	(-0.59)	(-0.47)	(-0.71)
ROA	0.0084	-0.0078	-0.0021
	(0.65)	(-0.70)	(-0.18)
Country fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	566	566	566
Adjusted R-squared	0.5434	0.5895	0.5678

Appendix A5
Litigation risk

This table shows the results of difference-in-differences regressions of insider trading on board reforms conditional on levels of litigation risk. Following Chen, Gul, Veeraraghavan, and Zolotoy (2015), we define a firm with high litigation risks if it belongs to one of the following industries with standard industry classification (SIC) codes between 2833 and 2838; 3570 and 3577; 3600 and 3674; 5200 and 5961, 7370 and 7374, and 8731 and 8734. The dependent variable is the insider trading measure. For brevity, we report the results for the number of insider trades only. *POST* is a dummy variable that equals 1 beginning in the year a major board reform becomes effective in the country, and 0 otherwise. All variables are defined in Table 2. Year and firm fixed effects are included in all regressions. *t*-statistics are reported beneath each coefficient estimate in parentheses, with standard errors clustered at the country level. Significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

Board reforms	Dependent variable: Insider trading measure	
	(1)	(2)
	High Litigation Risk	Low Litigation Risk
POST	-1.4113*	-2.8733***
	(-1.78)	(-4.31)
LOG_SIZE	0.4924***	0.2695***
	(6.80)	(5.40)
BM_RATIO	-0.6464***	-0.7469***
	(-6.99)	(-7.45)
LEVERAGE	0.0046	-0.0052**
	(1.33)	(-2.55)
ROA	0.0038*	0.0038**
	(1.73)	(2.59)
Firm fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
S.E. clustered by country	Yes	Yes
Observations	33,830	80,749
Adjusted R-squared	0.4689	0.4679

Appendix A6
Alternative Sample: Exclude firms from the U.S., Canada, and China

This table shows the results of difference-in-differences regressions of insider trading on board reforms after excluding firms from countries that account for large proportions of the sample. The dependent variable is the insider trading measure. For brevity, we report the results for the number of insider trades only. *POST* is a dummy variable that equals 1 beginning in the year a major board reform becomes effective in the country, and 0 otherwise. All variables are defined in Table 2. Year and firm fixed effects are included in all regressions. *t*-statistics are reported beneath each coefficient estimate in parentheses, with standard errors clustered at the country level. Significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

Board reforms	Dependent variable: Insider trading measure	
	(1)	(2)
	Exclude firms in U.S. and Canada	Exclude firms in U.S., Canada, and China
POST	-0.4170**	-0.4287**
	(-2.32)	(-2.28)
LOG_SIZE	0.0413**	0.0331*
	(2.13)	(1.80)
BM_RATIO	-0.0743***	-0.0802***
	(-4.53)	(-4.48)
LEVERAGE	-0.0003	-0.0000
	(-0.53)	(-0.05)
ROA	0.0007***	0.0008***
	(3.05)	(3.17)
Firm fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
S.E. clustered by country	Yes	Yes
Observations	44,683	37,520
Adjusted R-squared	0.4001	0.4002