Mandatory CEO Non-Duality, Managerial Agency, and Shareholder Value*

Rinaldo Guimarães† Giuseppe Trevisan‡

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Abstract

This paper estimates the effect of a mandatory CEO non-duality-oriented governance reform on shareholder value. Three key findings emerge. Firstly, regression discontinuity results show a positive market reaction to the announcement of the regulation. Secondly, difference-in-differences estimates reveal long-lasting positive impacts on shareholder value following the duality rupture. Lastly, after the splitting event, firms trade off external for internal financing and experience increases in investments and profits. The findings suggest these impacts stem from mitigating managerial agency issues and underscore that coercive rules may have distinct effects compared to recommendation-based regulations in contexts with weak shareholder protection institutions.

JEL codes: G32, G38, G41, O16.
Keywords: CEO duality, mandatory regulation, shareholder value, financing policies.

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†Universidade Federal do Rio de Janeiro, e-mail: rinaldo.guimaraes@ufrj.br
‡Universidade Federal de Pernambuco, e-mail: giuseppe.trevisan@ufpe.br

1 Introduction

Over the past decades, CEO duality—the accumulation of CEO and chairman titles—has been one of the core discussions and most contentious issues in corporate governance among scholars and market regulators. The controversies emerge from the agency-efficiency dichotomous views about the role played by the dual CEO and the non-obvious implications for shareholders' wealth yielding from the separation. Many institutional efforts have been made worldwide to discourage the accumulation of titles in an attempt to improve board governance, such as the recommendations by the Cadbury Committee Report and the Sarbanes–Oxley Act for separation in U.K.- and U.S.-listed firms. Contrary to general expectations, empirical evidence from those countries suggests that companies that opt for a non-dual structure may eventually be worth about the same or even less than duality firms (e.g., Dahya et al., 2009; Dey et al., 2011). Fauver et al. (2017) raise the possibility of differential impacts on firm value between rule-based (mandatory compliance) and comply-or-explain (non-binding) governance reforms, which might vary according to the local context. In this sense, a question of great interest for stakeholders is whether regulations that prohibit the CEO-chairman combination would affect shareholder value differently, and under circumstances other than those of countries with strong investor protection and more developed financial systems. However, literature regarding the impacts of mandatory rules is scant since these are particularly rare. The goal of this paper is to address this gap.

There are two key reasons that make examining the implications of such a regulatory framework compelling. Firstly, in countries with relatively poor shareholder protection, the implementation of a regulation mandating the separation of CEO-chairman positions could arguably result in favorable repercussions on firm value, particularly among those firms with a higher concentration of common shareholders. These shareholders, given their heightened concern with agency issues and greater interest in the company's long-term success, are more likely to perceive such regulatory mechanism as robust and reliable for enhancing corporate governance. By curbing power concentration and augmenting board oversight over the CEO's actions, the regulation might foster greater trust among shareholders in the transparency and accountability of the companies, thereby facilitating the pursuit of value-maximizing strategies and deterring managerial opportunism (Jensen and Meckling, 1976; Jensen, 1986). Notably, low-quality investor protection institutions tend to be more prevalent in countries operating under the civil law system (La Porta et al., 1998, 2008), which form a substantial part of the global stock markets in terms of economic relevance. Nevertheless, there remains a dearth of corresponding empirical evidence. Secondly, the recent paper by Baker et al. (2022) highlights econometric issues in previous study estimating the effects of corporate governance reforms worldwide, thus questioning the reliability of conventional wisdom regarding the impacts of governance regulations and, consequently, making it an unresolved matter. Given these circumstances, it is crucial to present novel empirical evidence regarding the consequences of mandatory reforms.

The Brazilian institutional background provides a unique opportunity to study the impacts of such a regulatory change and overcome challenges concerning endogeneity issues. The Brazilian Stock Exchange (B3) 2011 regulatory reform called for a mandatory separation of the CEO-chairman titles in firms listed in special governance segments, which primarily consist of firms with a majority of common shareholders. For companies that previously maintained a dual-role structure, a limited timeframe was granted to ensure compliance with the new regulation. By recreating a setting where firms' governance preferences are controlled, the reform offers a natural experiment that allows us to compare duality firms that eventually separate positions with a group of rule-targeted firms that compulsorily kept their non-dual leadership structure and study the short- and long-term effects of a coercive regulation on dimensions of shareholder value.

In the first part of the paper, we examine the stock market reaction to the announcement of the reform. To mitigate the role of confounding factors related to the rule banning CEO duality[1] and

[1]The reform was accompanied by other governance terms that required mandatory and immediate compliance. Section 2 provides more detailed information about the regulation, and Section 4 describes in detail our strategy to disentangle the influence of those additional amendments.

other stocks' time trends, we combine regression discontinuity (RD) in time[2] and difference-in-discontinuity-based designs exploiting the rich temporal and cross-sectional variation of our data. In particular, our identification strategy compares daily abnormal returns near the date of disclosure of the reform to those on the same day in the previous year, separately for firms with separate titles and duality firms at the time of the announcement. The local RD estimates indicate that stockholders positively reacted to the mandatory ban on CEO duality. We further show that results are robust to a variety of empirical tests, which include employing different econometric specifications and alternate samples, and checking the role of autoregression processes. The observed stock returns responsiveness is consistent with an inability of investors to anticipate the new information in the disclosed corporate regulatory directives (Binder, 1985).

Our findings on the short-run effects of regulation dissent from previous evidence provided by Larcker et al. (2011), which document negative market reactions to the announcement of miscellaneous corporate governance regulations in the U.S. and suggest existing governance arrangements to be value-maximizing contracts between agents and principals. Conversely, our results corroborate predictions from the managerial power perspective of governance. Since the announcement of the reform arguably speaks to shareholders' expectations, the positive market response to the rule prohibiting the accumulation of titles is consistent with the idea of principals perceiving potential or actual managerial agency in the duality structure. In addition, it suggests the effects of corporate regulations may depend on the institutional environment and on the type of reform—i.e., mandatory- or recommendation-based.

The second part of the analysis focuses on the effects of the actual separation of CEO-chairman roles. For identification purposes, we exploit the coercive change in duality structure and the fact that some firms were compelled to maintain their pre-mandate non-dual status, thus serving as a pure control group. This approach is similar to other studies investigating the effects of

[2] Unlike the standard RD design, this setting uses time as the assignment variable, thus exploiting a temporal shock. A non-exhaustive list of examples using RD in time in different contexts includes Gallego et al. (2013); Anderson (2014).

governance provisions on firms' performance (Duchin et al., 2010) and firm risk (Anginer et al., 2018). Our empirical strategy consists of estimating a staggered adoption difference-in-differences-based (DD) regression model with firm and industry-time fixed effects. Firm fixed effects are crucial to controlling for unobserved firm-constant characteristics that may have affected the timing of compliance, while the interaction between industry and calendar quarter fixed effects flexibly control for specific sector time-varying homogeneous shocks to firms. To overcome the bias inherent in the standard estimation approach in staggered adoption designs, we employ novel methodological advances in the difference-in-differences framework that address the estimation issues stemming from the heterogeneity in treatment effects and in the timing of events.[3]

In contrast to prior related studies that use variation in duality structure stemmed from rules that recommend the separation, we find that the mandatory duality split yields positive effects on shareholder value. We estimate robust large responses on abnormal returns and market capitalization. Our baseline specification shows an average treatment effect in the order of 36.6% and 54.6%, averaged over a 3–6-year horizon relative to the split event. Interestingly, the analysis of dynamic treatment effects reveals the change in firm value is not immediate following the governance shift, albeit persistent over time. The delayed treatment responses, when considered alongside the stock market's reaction to the disclosure of the rule and our analysis of effect heterogeneity—which shows significant impacts among larger companies—, strongly indicate that the duality rupture indeed led to actual corporate policy changes that are agency-motivated (Jensen and Meckling, 1976). We further closely examine these changes.

The major concern with the validity of our results pertains to the potential endogenous timing of duality splits within the prescribed time window for firms to comply. To address this issue, we have conducted several checks that robustly support the credibility of our findings. Firstly, our event study

[3]The bias, which extends to both single-coefficient and dynamic DD approaches, arises from an inappropriate weighting scheme to calculate treatment effects in a DD design with staggered roll-out. See Roth et al. (2023) for a review of the latest developments in DD estimation methods and detailed discussion on issues embedded in the standard estimation framework.

analysis provides compelling evidence of non-anticipation effects and supports the parallel trends assumption, mitigating concerns about anticipatory behaviors influencing the results. Secondly, we rigorously examine the effects of the duality split across various subgroups, including late changers and early compliers, as well as different types of companies, ruling out strategic selection biases as the primary drivers of our findings. Thirdly, we rule out the possibility that treatment effects could be driven by firms in which compliance coincides with the end of the CEO's period of mandate, thus excluding potential endogenous behavior originating from shareholders' expectations regarding the upcoming split. Furthermore, we discard the role of other potential firm-level confounders, such as news releases, board meetings, and general shareholder announcements. To enhance the robustness of our findings, we include controls for trends in other observable variables and conduct permutation tests. In conclusion, the cumulative evidence from our empirical checks strongly supports a causal link between the duality rupture and shareholder value.

Guided by theories that recognize debt as a corrective device for managers' actions (Jensen, 1986; Stulz, 1990; Zwiebel, 1996), we shed light on the mechanisms driving the responses in shareholder value by evaluating the effect of duality rupture on capital structure and internal financing policies of firms. The underlying rationale is that a value-maximizing corporate governance provision should mitigate the reliance on external funding as a disciplining tool, thereby allowing internal cash flows to finance the firm's investments. Consistent with theoretical predictions, we find a clear trade-off between the two funding dimensions. On average, treated firms decrease financing loans and leverage while increasing their disposable cash and retained earnings. Notably, we observe a temporal alignment between the new leverage policy, retained earnings, and the increase in shareholder value, supporting a direct link between these factors. The dynamic effects are robust to alternative estimation methods, and we additionally find support for the assumptions of common-trends and non-anticipation effects. In order to strengthen the mechanism analysis, we provide evidence of an indirect impact of the financing policy on shareholder value in the spirit of

Keele et al. (2015).[4] Additionally, our findings indicate that the change in financing policies plays an important role in linking the governance provision to the observed enhancement of investments and profits. This provides support for the notion that the reallocation of financial resources in response to the governance reform fosters value-maximizing strategies and improves firm performance, driven by the mitigation of agency problems in management and the greater autonomy granted to the board of directors.

Finally, we examine and rule out several other potential channels that could explain the main results. In the pursuit of enhancing shareholder value, the new leadership configuration may seek alternative opportunities to maximize firm profits through changes in operations. However, we find no evidence of such changes. Additionally, we examine the implementation of additional governance measures, including improvements in monitoring capacity and adjustments in members' earnings (Chintrakarn et al., 2014). Yet, we find no significant responses related to these corporate provisions. Furthermore, our analysis excludes the possibility that the increase in value is driven by stock manipulation, wherein CEOs could utilize stock buybacks to artificially inflate the company's value.

Our study provides a threefold contribution to the existing literature. First, we add to the extensive body of research on CEO duality by showing that a mandatory regulatory intervention aimed at enhancing governance parameters can lead to distinct outcomes compared to non-coercive mechanisms. In this sense, context seems to play a crucial role in the effectiveness of reforms. In environments with relatively weak investor protection institutions, the external validity of recommended-based regulations on this matter may not produce the same effects. The applicability and generalizability of such regulations to different settings and jurisdictions could be limited due

[4] The authors develop a causal framework where the impact of an intervention on an outcome may be mediated by a third variable affected by the treatment. Their potential outcome framework introduces the Sequential Ignorability Assumption, which states that: a) conditioned on baseline characteristics, the potential outcome and the potential mediator are independent from the treatment assignment; b) conditioned on both treatment assignment and predetermined characteristics, the potential outcome is independent from the potential mediator variable. This design allows to differentiate indirect effects (representing the posited channel for why the treatment succeed) and direct effects (the remaining explanations for the observed treatment effect) of an exogenous shock.

to varying levels of legal enforcement, corporate governance practices, and shareholder rights protection. In these contexts, recommended-based approaches may lack the necessary mechanisms to ensure compliance and enforce accountability, potentially leading to suboptimal outcomes. This could provide an alternative explanation for why Fauver et al. (2017) do not document significant effects, as most sampled countries experience CEO non-duality-oriented reforms under a comply-or-explain approach. Thus, mandatory regulations may play a more significant role in promoting good governance practices and protecting shareholder interests from managerial opportunistic behavior.

Our work also offers a plausible causal interpretation to the relationship between CEO non-duality and shareholder value, a widely researched topic that has been plagued by endogeneity concerns in previous studies. The lack of an exogenous non-duality-oriented source of variation may help explain part of the mixed findings in prior related studies that relied on quasi-experimental variation (Yang and Zhao, 2014) or correlational designs.[5] Moreover, our study endorses the concerns highlighted by Baker et al. (2022) regarding potential estimation issues in DD designs with staggered roll-outs, wherein results could be sensitive to the estimation method due to the presence of negative weights and/or treatment effect heterogeneity when averaging treatment effects. Therefore, our study advances existing knowledge by assessing causality in the relationship of interest and by demonstrating the crucial role played by the type and context of regulatory reform.

Finally, our work contributes to the corporate governance literature by providing evidence that links agency theory with market responses to CEO duality structure, while also shedding light on the role of several potential channels. Prior research explores the correlation between CEO duality and capital structure decisions (García and Herrero, 2021) and cash holding (Masulis and Mobbs, 2011). However, our study offers causal evidence that underscores a significant connection between non-duality structure and changes in corporate financing decisions, which in turn mediate the effects on firm value. Our findings indicate that the CEO non-duality structure grants greater autonomy to

[5]For instance, see Rechner and Dalton (1989); Baliga et al. (1996); Brickley et al. (1997); Worrel et al. (1997); Dalton et al. (1998); Davidson et al. (2001); Iyengar and Zampelli (2009); Ballinger and Marcel (2010); Quigley and Hambrick (2012).

the board in managing debt and cash, acting as a potential substitute for debt as a corrective tool to mitigate agency issues. These results align with previous evidence on the impacts of other corporate governance reform in similar context (Arping and Sautner, 2010), where targeted firms respond by reducing their corporate leverage.

In addition to this introduction, the paper is structured as follows. Section 2 provides key background information on the Brazilian Stock Exchange and the regulatory reform explored in this study. Section 3 describes the databases and the measurement of the variables. Sections 4 and 5 present the results concerning the market reaction to the disclosure of the regulation and the impacts of the actual rupture in duality structure. Section 6 discusses potential channels and Section 7 concludes. Additional robustness and falsification tests are provided in the Internet Appendix.

2 Institutional Framework

2.1 The Brazilian Stock Exchange

B3 was founded in the late 1800s and it is currently the most important stock market in Latin America and a world's top twenty in terms of market capitalization. By the end of 2011, B3's total capitalization was about R$ 2.4 trillion and around 580 thousand investors were trading shares of 370 firms. During the COVID-19 pandemic, B3 achieved its largest historical level in market value (R$ 5.2 trillion) and attracted a significant number of new traders, reaching 3.8 million individual investors by 2021 (B3, 2020).

Amid many corporate scandals and collapse events in the main financial markets over the late 1980s and early 1990s, B3's commitment towards improvements on firms' accountability standards led to the creation of three distinct listing segments in 2000. These special segments required companies to reach peculiar governance levels. Ranked from top to bottom in accordance with the stringency of the rules, the created segments were: New Market (NM), Corporate Governance Level

2 (L2) and Corporate Governance Level 1 (L1).[6] All listed firms in special segments have to disclose comprehensive quarterly financial information and must have a free float of 25% of the capital. Only companies listed on the NM segment must offer a 100% tag along obligation and exclusively issue common shares, which grant their holders the right to vote in shareholders' meetings and participate in the profits distributed by the company in the form of dividends. However, as a result of the stringent governance standards mandated for participation in these segments, approximately 77% (91%) of all (NM and L2) traded shares are common stocks.

Enrolling in (exclusively) one of these segments is voluntary and companies are allowed to interchange or delist. Yet, some key regulatory requirements may guarantee firms' permanence by their joining. The delisting process involves a public tender offer—only for NM and L2—and the shareholders' approval at an extraordinary general assembly. In practice, these costly terms make firm turnover a rare event.

2.2 The 2011 Reform

Regulatory reforms may occasionally be imposed on the special segments in such a way that prescribed corporate governance provisions must be aligned with practices adopted internationally. To date, this has happened four times: 2006, 2011, 2017, and 2021. As a result of prior restricted assembly held strictly with company representatives from firms listed in these segments, the 2011 reform implemented significant changes in governance parameters and made other adjustments in existing rules. The amendments included the revision of contractual texts and terms, new sanctions and prohibitions, changes in minimum requirements for trading shares, and other measures to protect minor investors. All terms discussed and their respective approval status became publicly available from the disclosure of the regulation, as of May 10th, 2011. Table 1 outlines the most important proposals for each listing segment. The approved terms required mandatory and immediate

[6] In 2012, two other segments were created (B+ and B+ Level 2) to provide opportunity for small and medium-sized firms to gradually enter the stock market.

compliance. Effectively, 80% of the listed companies participated in the secret balloting.

Table 1 About Here

Among the approved proposals, companies were prohibited from accumulating CEO-chairman titles. This outcome was directly determined by the presence of non-duality firms at the time. In particular, they represented the majority of companies (79%) and 83% of these firms voted in favor of the amended proposal to ban duality. Companies with a duality structure prior to the regulatory shift had a three-year deadline to comply with the norm. In addition, they had to amend their bylaws and add such a requirement to guide future elections. Exceptional cases would be analyzed by B3 to allow a longer period. Corporations use the minutes of their assemblies as their means of publicizing any governance change, which is the primary vehicle for outsiders to learn about the change in leadership structure.[7]

As regards proposals that are common to the three segments, only two amendments were rejected by the assembly audience. With a 67% score, the first rejected amendment relates to the mandatory establishment of a statutory audit committee with at least one independent counselor. The other amendment that was rejected relates to board independence. In particular, 60% of the assembly audience voted against the increase of ten percentage points (from 20% to 30%) in independent counselors. Exclusively applicable to the NM segment, the assembly audience also rejected a proposal related to conditional terms for public offers of share acquisitions by a two-third majority vote.

[7]In some cases, the change in the dual-title structure can coincide with the end of the incumbent CEO mandate. We further discuss this issue in the robustness section and show that results are not driven by firms meeting such a requirement.

3 Data Sources and Variables

We use data from three distinct sources. The first source is B3's website, which contains historical information on (de-)listing dates of publicly traded corporations as well as data on their leadership structure and operating industry. The second source is the Brazilian Securities and Exchange Commission's website (*Comissão de Valores Mobiliários*, CVM), in which we can retrieve companies' year of foundation as well as yearly information on external auditors, type of stockholders, compensation of the members of the board, and members' family ties. The third source is Economatica, which is a leading private company in data intelligence services in the financial area. This rich dataset incorporates detailed content on stocks and accounting figures from firms operating in more than 45 countries.

3.1 CEO Duality

To identify duality firms at the time of the 2011 reform, we hand-collected information from the companies' reference form (RF). The RF is an official document containing a detailed description of firms' main activities, capital structure, securities issued, dates of election of board members, and significant financial data. All publicly traded firms are required by CVM to a mandatory yearly disclosure of their RF, which is publicly available on the B3 website. We extract the information in two steps. First, we analyze each firm-year RF to identify whether the CEO has also served as chairman by using their unique Individual Taxpayer Number (*Cadastro de Pessoa Física*, CPF). The CEO is traditionally elected at the board of directors meeting, and board members are usually elected at the shareholders meeting, while the chairman may be defined at either of these meetings. Based on these considerations, in the second step we check the date of disclosure of election results recorded on the RF, which are retrieved from the minutes of the assemblies. Accordingly, we are able to identify the precise moment in which a firm changed its management structure and thus determine the treatment period for each company.

Among the 139 non-financial firms in the special listing segments,[8] 20% had dual leadership prior to the 2011 reform. All CEOs are men in duality firms. The left graph in Figure 1 depicts trends in the splitting of titles over the years (black triangles) for those companies. Structural changes occurred in nine distinct quarter-year moments over a four-year period, thus exhibiting considerable variation over time. The right graph depicts the temporal distribution of firms from each industry achieving regulatory compliance. Most firms switched to non-duality in the two early quarters of 2014 and only one exceptional case complied in the following year.[9] This descriptive evidence highlights the enforcement of the regulation. In order to place a broader perspective on duality behavior, the graph additionally reports percentages of duality firms for the Taiwan and U.S. stock markets. As compared to Taiwan, Brazil presents similar pre-regulatory rates. As emerging economies, both countries contrast with U.S. levels.

FIGURE 1 ABOUT HERE

3.2 Stocks and Financial Statements

The data provided by Economatica collect quarterly information from financial statements, quarterly market capitalization (market cap), daily stock prices (opening, closing, average, highest, and lowest prices), and the daily number of shares. The dataset covers the time span from 2008 to 2017. We merge this database with both CVM and B3 data using the firm's unique 14-digit identifier (*Cadastro Nacional de Pessoas Jurídicas*, CNPJ). This feature ensures we can perfectly match all firm's information from the different sources.

To assess shareholder value, we use abnormal returns and market cap. We calculate quarterly-level abnormal returns as the residuals from a log-linear regression of the daily closing prices on the daily market return, controlling for firm and date fixed effects. To compute the residuals, we include

[8] We exclude listed firms operating in the financial sector (20 firms) as they are subject to different regulations.

[9] We further provide robustness to our findings by not taking into account this exceptional case and by interchangeably excluding groups of late compliers.

observations within our period of analysis (i.e. 2008–2017). In the last step, data are collapsed by each firm-quarter cell (averages). Market cap is the logarithm of the number of shares outstanding multiplied by the stock price. When investigating very short-run market responses, we use daily abnormal returns. We focus on market-based variables to reflect a cleaner measure of stockholders' foresight on firm value.

To further investigate potential channels, we derive several accounting-based proxies for financing activities, investments, return on assets, and operating performance. Together with market cap, these variables are winsorized at the 1st and 99th percentiles of their sample distribution to alleviate the influence of outliers. Table A.1 of the Internet Appendix provides detailed variable definitions.

4 How Do Shareholders Respond to the Mandatory CEO Non-Duality Regulation?

We begin by examining the market response to the disclosure of the regulation, in order to understand how shareholders perceive the accumulation of CEO and chairman titles. To do so, we employ a regression discontinuity design. In our context, however, assessing causal interpretation with a RD approach is not straightforward. First, as discussed in Section 2, the reform established multiple governance changes concurrent with abolishing duality. Second, not approving two important governance-content proposals—related to board independence and establishing an internal audit committee—makes it more difficult to disentangle shareholders' perception of the amendment prohibiting the accumulation of positions and other provisions. We thus implement as our main approach a RD model that consists of an amplified version of the standard RD design, which exploits both cross-sectional and time variation and thus combines RD- and DD-based frameworks. However, unlike most applications that explore quasi-experimental variation near the threshold of a non-temporal running variable, our setting uses a temporal cutoff to define treatment and control groups (RD in time).

4.1 Difference-in-Discontinuity Model

Based on Larcker et al. (2011), we argue that the disclosure of the 2011 reform generates a plausible exogenous variation in expectations of existing governance practices. However, a simple comparison between the outcome before and after the event is likely to yield a biased estimate. The endogeneity arises from stocks' distinct time trends and heterogeneous unobserved daily shocks to stock returns. In addition, interpreting the consequences of the regulatory shift in terms of market reactions to future duality rupture is not obvious, since the disclosure of the reform comprises other approved governance provisions plus the rejected ones. To address endogeneity issues, we take advantage of the high-frequency firm-level data and the sharp discontinuity in the awareness of the regulatory shift to examine shareholders' response in a boundary around the event. As the regulation is mandatory for all firms but some present a different leadership structure at the time of the event, we evaluate the effects separately for duality and non-duality firms to better understand shareholder expectations regarding the upcoming duality split.

The inclusion of time trends and fixed effects enforces that we are capturing the response in abnormal returns to the regulatory shift free from short-seasonal confounders. Yet, differences in yearly seasonal shocks may be influential to identification. We follow Picchetti et al. (2024) and improve our RD in time specification by taking the difference in the outcome in the previous year on the same date of the disclosure, thus identifying the difference of the difference (in year) in the abnormal returns (ΔY_{it}). The identifying assumptions are that abnormal returns would not discontinuously shift on the date of disclosure of the reform in the absence of the regulation and that confounding effects are time-invariant.

We begin by obtaining daily abnormal returns, calculating the residuals based on the following first-difference specification:

$$\Delta stockprice_{ia,t} = \alpha_0 + \alpha_1 \Delta marketindex_{a,t} + u_{ia,t}, \tag{1}$$

in which $\Delta stockprice_{ia,t} = log(stockprice_{ia,t}) - log(stockprice_{ia,t-1})$ and $\Delta marketindex_{a,t} = log(market$-$index_{a,t}) - log(marketindex_{a,t-1})$ for the stock i at day t in year a. To obtain the residuals, we estimate the coefficients by standard OLS using an estimation window of 80 trading days around the announcement of the reform using data from 2008 to 2011.

We then model the relationship of interest for all firms (j = *all*), and firms segregated by duality (j = *dual*) and non-duality (j = *non-dual*) as the following difference-in-discontinuity regression form:

$$\Delta Y_{it}^{j} = \alpha_0^j + \alpha_1^j Disclosure_t + \alpha_2^j Disclosure_t \times h(t) + h(t) + \theta_{wm} + \epsilon_{it}^{j}, \tag{2}$$

where ΔY_{it}^{j} represents the difference in abnormal return between years 2011 and 2010 of stock i at day t. $Disclosure_t$ is a dummy variable assuming value "1" for the entire period from the date of disclosure of the regulation, and zero otherwise. $h(t)$ is a first-order polynomial time trend centralized in zero, which takes the disclosure date as the reference point. The interaction between the polynomial with $Disclosure_t$ flexibly controls for differential time trends in abnormal returns at each side of the temporal cutoff. θ_{wm} are weekday-month fixed effects and are included to eliminate short-term seasonal shocks to abnormal returns. The coefficient α_1^j represents the net effect of the disclosure of the regulation, which captures both the difference in abnormal returns between years and the pre-post difference at the cutoff. Namely, we compare the yearly difference in daily abnormal returns around the disclosure event in 2011 to those around the same day in the previous year, thus allowing us to control for seasonal shocks and unobserved heterogeneity at the firm level. As a benchmark, we restrict our estimation sample to observations within a bandwidth of 25 days around the disclosure date to ensure we are capturing very short-term responses. Furthermore, we consider companies listed in the NM and L2 segments since duality firms are found only in these groups.

To test the plausibility of the time-invariant confounding effects assumption, we explore pre-regulation years and check consistency in the coefficients across time. We use data within a bandwidth of 6 days[10] around the disclosure date and run the following regression model:

$$Y_{ia,t} = \theta_i + \sum_{a=\{2008,2009,2010\}} \gamma_{-k} Disclosure_t \theta_a h(t) + \sum_{a=\{2008,2009,2010\}} \rho_{-k} \theta_a h(t) + \theta_{wm} + u_{it}, \quad (3)$$

where Y_{it} are abnormal returns obtained from Equation 1, θ_i is stock fixed effect, and θ_a is a vector of dummies for each pre-reform year a. We also incorporate weekday-month fixed effects to account for contextual adjustments. We test the null hypothesis: $H_0 : \gamma_{2008} = \gamma_{2009} = \gamma_{2010}$.

Building on theoretical foundations from agency theory, we expect shareholders to positively react to governance regulations seeking to undermine potential conflicts between principals and agents. However, the direction of α_1^{all} is not clear. For instance, if stockholders put more (less) weight on the rejection of the two amendments to the detriment of the approved ones, or if, on net, the existing governance structures represent value-maximizing contracts, then $\alpha_1^{all} < 0$ ($\alpha_1^{all} > 0$). Importantly, we expect the magnitudes of α_1^j to differ between duality and non-duality firms. The hypothesis is that, as all firms are subjected to the same new rules, companies with dual leadership will face better stockholder expectations regarding the upcoming mandatory structural change. Thus, we expect $\alpha_1^{non-dual} < \alpha_1^{dual}$. In this framework, therefore, any (local) observed differences in market reaction across the two groups can plausibly be attributed to the difference in expectations regarding the upcoming split of titles.

For estimation, we employ the standard OLS method instead of using nonparametric estimation. In our setting, the temporal running variable is also characterized as a discrete variable, which means that we observe a mass of units at each point of its distribution due to the longitudinal data structure. As shown by Dong (2015), standard RD estimation is not recommended in designs with discrete forcing variables because the coefficient is not nonparametrically identified. Moreover, usual manipulation tests are also not applicable due to the inherent density of the running variable.

[10]Following Picchetti et al. (2024)'s guidelines, we employ the optimal bandwidth proposed by Calonico et al. (2014) for estimation, which is narrower than our baseline used for the main results. In the Internet Appendix, we demonstrate that our main findings remain robust across various bandwidth choices.

To provide reliability for our diff-in-disc estimates, we follow the guidelines of Hausman and Rapson (2018) and check the robustness of the results by experimenting different polynomial orders and fixed effects, by using alternate samples and windows for estimation, as well as specifications that check the role of autoregression processes. These results are discussed in Section B.1 of the Internet Appendix. For inference, we use heteroskedasticity-robust standard errors.[11]

4.2 Short-term Market Reactions

Figure 2 plots difference-in-discontinuities for abnormal returns over an eighty-window days around the disclosure date for all firms (left panel) and separately for firms presenting non-duality structure at the time (middle panel) and for those that will eventually separate positions (right panel). The red vertical lines highlight the range of the effective observations used in our estimation sample. The dots represent the dependent variable averaged within three bins of day in 2011, while the respective solid lines represent the predicted values from local polynomials estimated separately on either side of the cutoff. According to the first graph, returns appear to fall discontinuously following the disclosure of the reform. In particular, companies with separate titles at the time of the announcement seem to be driving the negative reactions, as stock returns from duality firms show a smooth transition at the cutoff date.

FIGURE 2 ABOUT HERE

In Table 2, we formally estimate the discontinuities using Equation 2 and apply local regression methods for comparison. We also assess the sensitivity of estimates by using second-order polynomials to avoid misspecification issues commonly associated with higher-order polynomials

[11] Kolesár and Rothe (2018) show that, in contrast to common practices in designs with a discrete assignment variable, employing a running variable clustering-approach is likely to generate incorrect confidence intervals, thus leading to inaccurate statistical significance of the estimated impact. Alternatively to the "honest way" of estimating confidence intervals proposed by those authors, implementing heteroskedastic-robust standard errors appears to be a second-best solution.

in RD models, as discussed by Gelman and Imbens (2019). The coefficients confirm the patterns outlined above. Our estimates of α_1^{all} in columns (1)–(3) point to a negative effect of the disclosure on abnormal returns, which appears to be driven by firms with separate roles, as shown in columns (5)–(7). In the last columns, the treatment effects for duality firms are estimated with considerable noise and exceed those for firms with separate titles in magnitude. The diff-in-disc coefficients are robust to different estimation methods and polynomial orders. In column (4), we formally test the difference between the estimates of α_1^{dual} and $\alpha_1^{non-dual}$ using the full sample, interacting the treatment variable with an indicator for duality firms, and find evidence supporting our hypothesis.

TABLE 2 ABOUT HERE

To validate our main findings, we assess whether time-varying confounders may be influencing our main estimates. In Table 3, we estimate Equation 3 for the full sample and separately for non-dual and duality firms. The results show that the magnitudes of $\hat{\gamma}_a$ are highly consistent across regressions. Furthermore, the Wald test for coefficient equality in pre-treatment periods confirms that our diff-in-disc design is appropriate for estimating the effects of the disclosure of regulation on market reactions.

TABLE 3 ABOUT HERE

The results indicate that the net effect of the announcement of the reform is negative. Importantly, we note the magnitudes are much less expressive (and non-significant) for companies that will further experience the separation of the CEO-chairman roles. That is, stockholders have better future expectations with regard to the upcoming duality rupture. The short-term market reaction provides a first hint towards the perception of potential or actual agency conflicts inherent in the entrenched management structure.

5 The Effect of CEO Non-Duality on Shareholder Value

5.1 Sample and Identification Strategy

In this section, we investigate the effects of the actual change in leadership structure. To identify the impact of non-duality on dimensions of shareholder value, we leverage the temporal variation in duality splits among firms resulting from the mandatory separation mandated by the B3 regulatory reform. In particular, we exploit the fact that some firms separated CEO-chairman titles at heterogeneous moments in time, while other rule-targeted companies mandatorily maintained their non-dual board structure, thus serving as an empirical counterfactual group.

The sample consists of a unbalanced panel of the same firms listed in the NM and L2 special segments used in the analysis of Section 4, observed in the period ranging from 2008 to 2017. We impose this restriction to keep apart from the influence of other reforms and to sufficiently investigate anticipatory and dynamic effects, resulting in a total of 4,155 firm-quarter observations. Table 4 presents summary statistics for stock returns, financial figures, and several characteristics separately for control and treated firms. The numbers clearly indicate firms that will eventually separate positions are remarkably distinct from companies in the control group. Evidently, these differences mix both selection and causal effects. In addition, Table A2 in the Internet Appendix shows that treated firms disproportionately operate in the consumer goods industry and are relatively younger than their counterparts.

TABLE 4 ABOUT HERE

To overcome selection problems, we employ a difference-in-differences research design. The underlying DD identifying assumption is that treatment and control groups exhibit parallel trends in the absence of treatment—which does not require firm outcomes to have the same pre-treatment levels. Consider a dependent variable y_{ft} for firm f observed at time t. Denote by Q_{ft} the number of periods relative to the firm's structural change in leadership. We model the relationship of interest as follows:

$$y_{ft} = \sum_{q \in \mathbb{Z},\ q \neq -1} \beta_q \mathbb{1}\{Q_{ft} = q\} + \theta_f + \theta_{st} + \varepsilon_{ft}. \tag{4}$$

The term θ_f denotes firm fixed effect to account for time-invariant unobserved firm-level heterogeneity and θ_{st} is the interaction of industry and calendar quarter fixed effects, which absorbs any confounding sector-specific shocks to enterprises over time. The relative quarters to treatment event are defined in a set of integers $q \in \mathbb{Z}$, where $q = 0$ indicates the quarter of the duality split event and the baseline omitted period is $q = -1$. The binary variables $\mathbb{1}\{Q_{ft} = q \mid q \in \mathbb{Z}_+\}$ represent the treatment lags and $\mathbb{1}\{Q_{ft} = q \mid q \in \mathbb{Z}^*_-\}$ are the treatment leads. In some specifications, we include the interaction between time-constant firm characteristics (such as listing segment and year of foundation) and a linear time trend. These variables are not necessary for identification but are used solely to check robustness of the results and enhance the precision of estimates. Lastly, ε_{ft} is an idiosyncratic term. Coefficients $\beta_{q \geq 0}$ are the treatment effects, which measure changes in the outcome q quarters after the duality split, and $\beta_{q<-1}$ capture anticipation impacts. The parameters of interest are thus identified using within-variation in the outcome of eventually treated firms compared to the within-variation of never-treated firms at each relative time period from compliance to the rule. We also present results averaging effects over all post-treatment periods using the following specification:

$$y_{ft} = \beta \text{Duality-split}_{ft} + \theta_f + \theta_{st} + \varepsilon_{ft}, \tag{5}$$

where $\text{Duality-split}_{ft}$ is a dummy indicating all quarters from the compliance to the rule, and the remaining variables are the same as described in Equation 4.

Recent methodological advancements in the staggered DD literature underscore potential pitfalls when estimating the parameter of interest using standard designs. The tacit homogeneity assumption in the plain vanilla approach is likely to render a biased estimate of the treatment effect in staggered adoption designs due to a misleading weighting process. The intuition is that the estimator erroneously picks both units treated over a period of time and baseline units treated earlier in time to derive cohort weights used to average out treatment effects. This delivers spurious comparisons between treated and control groups and generates an unreliable point estimate. This weighting issue may arise even in dynamic specifications in the presence of heterogeneous effects and differential timing, thus violating strict exogeneity and providing distorted coefficients as a consequence. Fortunately, the presence of a never-treated group allows us to deal with part of the issue created by the negative weights. To mitigate the remaining concerns, we implement as our baseline estimation method the finite-sample efficient estimator developed by Borusyak et al. (2024) (BJS henceforth), which is robust to heterogeneous treatment effects across groups and periods, and adjusts for inaccurate weighting as well. To draw inferences on point estimates, we use firm-level clustered standard errors.

While both static and dynamic treatment coefficients are estimated using treated and untreated observations with a particular weighting scheme, the BJS's imputation method employs only untreated units (never treated and not-yet treated observations) in periods before the onset of treatment to estimate the parameters and (indirectly) evaluate both non-anticipatory effects and parallel trend assumptions. We model the potential outcome of untreated observations as

$$y_{ft} = \sum_{q \in \{-13,-2\}} \alpha_q \mathbb{1}\{Q_{ft} = q\} + \theta_f + u_{ft}, \tag{6}$$

where θ_f is a unit fixed effect. α_q represent the coefficients of the leads. In order to investigate anticipation effects, we test if the coefficients $\alpha_{q \in \mathbb{Z}^*_-}$ converge statistically to zero. As indicated by the authors, to test for parallel trends assumption we execute a joint test over the null $\alpha = 0$. To improve statistical power, we follow the authors' recommendation by using fewer leads than the full extent possible and thus avoiding a few sparse events in the very extremes of the relative periods.

For the sake of readability, we present estimates within the window $q \in [-13, 15]$, thus allowing us to check anticipatory effects up to roughly three and a half years before treatment—thus assuming away anticipation behavior up to fourteen periods before the treatment event—and evaluate dynamic effects up to four years ahead. This normalization seems reasonable, as virtually all firms switch their leadership structures up to three years following the announcement of the rule.

To strengthen the robustness of our estimation approach, we show event study results obtained by the methods of de Chaisemartin and D'Haultfœuille (2020) (CH henceforth) and Callaway and Sant'Anna (2021) (CS henceforth), which also accounts for heterogeneous treatment impacts across periods and groups as well. Besides the underlying particularities of the estimation procedure, the BJS approach distinguishes itself from the other methods by using a stronger identifying assumption. Namely, it imposes parallel trends for all groups and time periods instead of relying only on post-treatment parallel trends. In the staggered context, CH and CS estimators differ basically in the weighting scheme.

5.2 Effects of Duality Rupture

Figure 3 plots the event study estimates of duality rupture on our measures of shareholder value from the fully-dynamic model in Equation 4—baseline pre-treatment coefficients are estimated as in Equation 6. The impacts reflect differences in outcomes of treated firms relative to the (omitted) quarter immediately before the split in positions. BJS baseline estimates are depicted in orange dots together with their respective confidence intervals.

FIGURE 3 ABOUT HERE

Some interesting patterns emerge from the graphs. As market participants already expect companies to adjust their leadership structure as a result of the reform, they could e.g. foresee potential gains (or losses) before firms' compliance, or they could obtain privileged information about the upcoming split and thus heterogeneously affect stock performance in advance. In the same vein, if the timing of leadership change is correlated to unobserved determinants of shareholder value over which insiders have control, one would expect anticipation behavior from stockholders.

The figure suggests that this is not the case. We find strong support for the assumptions of non-anticipation effects and parallel trends for both outcomes in the pre-treatment period, which can be seen by the large confidence intervals for the treatment leads coefficients and the non-significant Wald joint test over those parameters.[12] In the period following the separation, abnormal returns and market cap of treated firms increase relative to the control group. However, impacts do not occur immediately after the switch to non-duality, but progressively increase over time. Despite the fact that treatment effects kink about one year after the event, the estimates are statistically significant starting around two years later, reaching their maximum values in the third year. To check robustness, we reestimate all parameters of interest implementing alternate methods. The coefficients of the leads and lags derived from the CH and CS approaches are remarkably similar to our main estimates.

The evidence from the graphs strengthens the notion that non-duality constitutes a value-enhancing governance policy, but the separation *per se* does not fully explain the change in patterns. We note that the delay in treatment impacts and the comparison of treated firms with a pure control group endorse that responses are not solely a consequence of firms complying with the regulation. Therefore, it is reasonable to expect that shareholders would react positively to actual policy changes made possible through the timely shift in board autonomy. The delayed large-sized dynamic treatment effects suggest that alternative channels related to the structural change are at play other than shareholders' optimistic expectations regarding the new management structure. We return to discuss the mechanisms in detail in Section 6.

Table 5 presents the baseline DD effects averaged over all quarters after duality rupture, as demonstrated in Equation 5. Columns (1) and (5), which comprise our baseline model, show that abnormal returns and market cap significantly increase by 36.6% and 54.6%, respectively. The following columns show that the magnitudes of the impacts are virtually unaffected when including

[12] We also note that, if the other approved terms in the 2011 reform disproportionately affected shareholder value by the time of the enactment, one should observe a significant pre-trend in outcomes of treated firms relative to the pure control group before the duality split event.

control variables.[13] Figure A1 of the Internet Appendix reports point estimates derived from the CH and CS estimators and confirms that baseline static treatment effects are robust to alternative estimation methods. In columns (3) and (7), we explore a different specification by switching industry-time fixed effects for the interaction between industry and year fixed effects and adding calendar quarter dummies (time fixed effects), and obtain similar point estimates.

TABLE 5 ABOUT HERE

In the remaining columns of Table 5, we test the sensitivity of the results using a different sample. Even finding support for the common-trends assumption, one may still be concerned with the comparability of treated and control firms in terms of their baseline characteristics to evaluate treatment effects. In order to check for this potential issue, we calculate propensity scores to match similar units based on their covariates and rerun our baseline DD model. The one-to-one closest match procedure reduces the number of treated firms to 24, as we do not find common support in the control group for some units. Even though we lose approximately 60% of observations, we obtain qualitatively the same results without sacrificing statistical power.

In Figure A2 of the Internet Appendix, we reestimate treatment impacts using subsamples in which we progressively exclude firms with shorter periods of data.[14] Dashed lines represent the confidence interval for the baseline estimates (solid vertical lines) reported in Table 5, while dots depict estimates for each subsample. In our main sample, 72% of the companies have a balanced panel. All point estimates fall within the inner region of the intervals and are generally of similar

[13]Table A3 in the Internet Appendix shows that inference for our main results is robust to alternate standard errors clustering options.

[14]One way of evaluating the role of treatment timing and effect heterogeneity in single-DD coefficient estimates is by applying the Decomposition Theorem developed by Goodman-Bacon (2021). The theorem demonstrates that "forbidden comparisons," which refer to comparisons between earlier- to later-treated and/or later- to earlier-treated pairs, are typically problematic in staggered DD designs. The diagnostic test assumes a balanced panel, which does not apply to our context. Baker et al. (2022) emphasize the importance of showing the fraction of never-treated units when the panel is unbalanced. In this regard, we scrutinize our results, using this test as an alternative check suggested by the authors to validate our findings.

magnitude, indicating that coefficients derived from the unbalanced panel in our baseline approach are unlikely to be driven by sample selection issues.

As closing prices may not be representative of the stock price volatility observed during the trading day, we next exploit different ways of deriving stock returns. In Table A4 in the Internet Appendix, we replace daily closing prices by mean, open, minimum, and maximum prices, and we recalculate quarterly abnormal returns. Obtaining virtually identical coefficients to our baseline findings, the results confirm that CEO non-duality leads to a positive response in all these proxies.

5.3 Threats to Identification

The previous section shows empirical support for CEO non-duality affecting shareholder value. While presenting evidence that supports parallel trends, we cannot fully eliminate certain concerns regarding the possibility of selection into the treatment. For example, in a strategic fashion, some firms could rapidly commit to the norm (or even delay commitment) in order to obtain the best possible results and thus endogenously influence shareholder value through the timing of compliance. In this section, we evaluate other potential threats to our empirical strategy and try to alleviate those emerging issues.

In absolute numbers, few were the duality firms at the time of the reform. This might raise concerns about the role of particular groups of firms in driving our findings. Given the limited number of cross-sectional treated units, the observed positive market responses could be driven by firms strategically splitting at a similar time—especially early and late changers—or by a renowned/specific group of companies. To address these potential problems, we perform leave-one-out tests, which consist of re-estimating the treatment effects for sample subsets. Panel A of Figure 4 reports results of this exercise when removing all treated corporations complying in the same treatment quarter and keeping all control firms. The vertical axis shows the average DD estimate, while the horizontal axis displays in chronological order each treatment quarter dropped from the regression. We obtain point estimates that are very similar to our main findings when removing these firms, and standard errors are just marginally affected due to a loss in statistical power. We note that in the second-to-last treatment quarter, where estimates are somewhat larger, fourteen treated firms from several distinct operating sectors are excluded from the estimation sample. Overall, results are very robust. This exercise provides additional support against potential endogeneity stemming from a strategic delay in compliance by firms invalidating our results.

FIGURE 4 ABOUT HERE

Panel B of Figure 4 replicates the leave-one-out analysis, this time excluding both treated and control firms operating in a specific industry from the sample. The horizontal axis indicates the industry dropped from the regression. Across subsamples, the DD coefficients exhibit an overall flat behavior, and their precision does not substantially differ from those in our main results. We conclude that our findings are not driven by specific groups of companies.

As discussed in Section 2, the key source for market participants' awareness of firms' compliance is the disclosed minutes of the assemblies. However, they have knowledge from past minutes about the beginning and (expected) ending period of the mandate of each member of the board and management layer. Because structural changes can coincide with the end of the dual CEO mandate, it might be the case that stockholders anticipate behavior due to the expectation arising from the possible change. Therefore, companies that surprised investors in this regard likely provided the most reliable shock to expectations. In our context, 40% of the treated firms changed to non-duality unexpectedly. Figure A3 in the Internet Appendix shows results when running the event study specifically for this subsample of treated units—while maintaining control firms. The magnitudes of the coefficients for both outcomes are very similar to our baseline results and are significant only in quarters after compliance with the norm. This suggests that shareholders' ability to predict the split of positions has little or no influence on our main findings.

In Section B.2 of the Internet Appendix, we carry out additional robustness and falsification tests.

We show evidence against non-parallel trends to alternate placebo strategy, test for the inclusion of additional control variables related to potential coinciding news releases, and perform permutation tests. In sum, the evidence shown in this section endorses our identifying assumptions by showing that potential endogenous issues related to the timing of duality splits are unlikely to affect the interpretation of our findings and strongly suggests estimated effects to be both reliable and causal.

5.4 Effect Heterogeneity

The robust average impacts we have documented so far indicate a positive influence of CEO non-duality. However, these effects may conceal interesting heterogeneity across firms. To gather additional evidence on the consequences of separating the CEO and chairman positions, we explore heterogeneous treatment effects by considering different firm dimensions. In particular, we focus on certain characteristics that can shed light on the role of agency in shareholder responses.

Columns (1) and (2) of Table 6 report the DD estimates for abnormal returns and market cap across firms of different sizes. To classify firms into groups, we divide the sample into larger and smaller companies based on a median split of total assets observed in the years prior to the enactment of the regulation. The results show the effects are significantly larger for large corporations, whereas smaller ones do not exhibit a response to the treatment event. These patterns align with the findings of Palmon and Wald (2002); Goergen et al. (2020), as larger firms typically face higher agency costs and are thus expected to benefit the most from the separation. In columns (3) and (4), we examine the effects on firms with different levels of market experience. In this analysis, we perform a median split based on the firm's year of foundation and divide the sample into older and younger companies. The rationale for this approach is that as the scale of the firm tends to be positively related to its market experience, agency issues are expected to be more pronounced in older enterprises, which become more complex over time. Consequently, treatment effects should be more pronounced in this case. While the magnitudes of the estimates support these predictions, showing that both stock returns and market value are more responsive in older companies, we cannot confirm that the responses are statistically different from zero.

TABLE 6 ABOUT HERE

As suggested by Lee and Masulis (2009), poor accounting information quality tends to increase information asymmetry for outside investors, which, in turn, is expected to exacerbate agency problems. In the last two columns, we estimate treatment effects using split samples based on the median levels of pre-reform discretionary accruals, calculated following the method developed by Kothari et al. (2005).[15] The findings reveal that firms with higher pre-reform accrual levels experience stronger effects, supporting our argument that agency issues play a role in our main results.

Taken together, the findings are in line with the notion that our baseline results may be influenced by managerial agency problems inherent in the duality structure. In the next section, we examine miscellaneous measures implemented by the new board configuration that may explain the observed patterns.

6 Potential Mechanisms

6.1 Corporate Financing Policies

Free cash flow theory recognizes the role of outside funding in shaping agents' behavior to prevent cash mismanagement and preserve principals' wealth (Jensen, 1986; Stulz, 1990). Notwithstanding, the decision regarding internal funding may also stem from managerial agency issues. Analyzing the trade-off between external and internal financing is crucial for understanding firm investment decisions, especially in the context of corporate governance provisions. We now turn our attention to investigate the consequences of the duality splits on the capital structure and internal cash of

[15]The Internet Appendix provides details on the calculation of discretionary accruals.

firms.

When there is moral hazard, the issuance of debt may serve as a mechanism to align managers' actions with shareholders' interests, curbing undesirable behaviors such as diverting funds for personal gain or investing in low-return projects. Consequently, an increase in debt may act as a deterrent against managers' predatory spending tendencies. By implementing improved corporate governance structures, it is expected that such provisions would mitigate the need for external financing as a disciplining tool. As a result, internal financing sources (e.g. retained earnings and cash reserves) become fundamental for sustaining ongoing operations and seizing potential investment opportunities. Drawing on the aforementioned theoretical works and empirical evidence provided by Arping and Sautner (2010) concerning the impact of governance mechanisms on financing policies,[16] we expect that firms undergoing the separation of chairman and CEO positions engage in a trade-off between external and internal financing.

Columns (1)–(3) of Table 7 reveal that firms respond to the treatment event by reducing their liabilities. Treated firms significantly reduce their leverage by approximately 28 percentage points following the separation of CEO-chairman positions compared to the control group. This negative average effect does not appear to be particularly driven by either long-term or short-term leverage strategies, indicating that non-duality induces a sharp move in financial obligations as well. Furthermore, Column (4) illustrates a decrease of 6 percentage points in financing loans, supporting the notion that companies adopt risk-averse behavior by reducing their reliance on external funding. Conversely, columns (5) and (6) reveal an increase in internal financing sources. Specifically, we find a statistically significant increase in retained earnings, rising by 15 percentage points, and in disposable cash, increasing by 1.35 log points for firms transitioning to a non-dual leadership structure compared to the control firms. This huge effect on disposable cash is

[16]The paper analyzes the implementation of a governance code in the Netherlands to evaluate causal effects on firms' leverage and debts, comparing treated companies to a control group of firms outside the country. The Dutch corporate regulation includes measures affecting board members' remuneration packages, board size, and the independence of board members. Although the code mandates that the incumbent chairman should not be a former member of the management board, it does not specifically evaluate the merits of dual-leadership.

particularly meaningful given that treated firms do not show a significant change in their total assets as compared to their counterparts (column 7). This suggests that treated firms adopt a less risky position, potentially shielding future investments against cash constraints. With these financial policy restructurings, treated firms may be better positioned to invest in profitable projects and growth opportunities, ultimately leading to increased investments and higher profits. Columns (8)-–(10) of Table 7 indicate a positive impact on investment growth and return on assets. The increase in profitability generates a greater capacity for firms to self-finance their businesses, which may lessen their need for external financing. Altogether, these patterns indicate a clear-cut change in the financial policy direction of firms after implementing the non-duality structure.

Table 7 About Here

Results presented in Section 5.2 demonstrate that the impact on shareholder value takes some time to materialize following the governance change. Considering the possibility that changes in firms' financing policies are a driving mechanism behind the main findings, it is crucial to explore the timing of effects on abnormal returns and market cap in relation to the unfolding of financial policies. To investigate the dynamics of changes in funding directives, we employ event study-based design using the three different estimators. The top-left graph in Figure 5 shows that the reduction in firm leverage becomes statistically significant only from the eighth quarter onward relative to the treatment event. The positive effect on retained earnings (top-right graph) appears to be temporally aligned, suggesting that treated firms are enhancing their ability to meet external obligations by retaining more of their earnings. The bottom-left graph shows that, compared to their counterparts, treated firms experience a statistically significant increase in cash reserves in the quarter immediately following the duality split, suggesting they are converting more liquid financial instruments into cash and cash equivalents. Notably, this effect persists over subsequent periods. Despite a temporary reduction in total assets, overall asset variations do not seem to account for these results.

FIGURE 5 ABOUT HERE

In Figure 6, we estimate the dynamic effects on investments and firm profitability. Three quarters after the duality split, the impact on firm investments (top graph) begins to grow progressively over time. The increase in return on assets (bottom graph) emerges approximately one year after the event and, overall, remains sustained in the subsequent quarters. Accordingly, the graphs indicate that the timing of responses on investments and profits aligns with the changes in financing policy.

FIGURE 6 ABOUT HERE

In Table A5 of the Internet Appendix, we incorporate disposable cash, retained earnings, and leverage as control variables in our baseline regressions. The objective of this analysis is to assess the extent to which the observed variation in shareholder value attributed to the treatment event is influenced by changes in corporate financing policy. Assuming the validity of sequential ignorability (Keele et al., 2015), if changes in financing policies mediate the effects on shareholder value, including these variables as controls in the main specification should attenuate the treatment effect of the duality split. The results indicate that while internal financing plays a pivotal role in absorbing a portion of the treatment effects, the primary driver of changes in shareholder value stems from shifts in leverage. The robust relationship between financing variables and the duality rupture is further supported by Table A6 in the Internet Appendix, which presents results across alternate specifications.

We first highlight the enduring nature of the impacts observed in both dimensions. The effects on disposable cash and leverage persist over an extended period, indicating a sustained influence of the governance reform on firms' financial policies. Of particular interest is the temporal alignment between the documented effects in Figure 3 and the patterns observed in firm leverage, which essentially mirror the effects on shareholder value. This provides suggestive evidence of a connection between the redirection of financial strategies and shareholder value, initiated by the governance change. Furthermore, it reinforces the significance of changes in leverage in explaining a substantial portion of the variation in outcomes, as demonstrated in Table A5 of the Internet Appendix. Additionally, Figures 5 and 6 corroborate the robustness of the baseline event study results across different estimation methods, providing strong support for the validity of the non-anticipation effects and parallel trends assumptions.

The observed changes in funding dimensions imply that CEO non-duality acts as a disciplining mechanism for managerial actions, replacing the corrective role traditionally attributed to debt, ultimately affecting firm value. The observed time lag in shareholder value responses aligns with the timing of the firm's capital restructuring process, which is reasonably expected to take time for implementation due to factors such as varying contract durations and debt renegotiations.

6.2 Other Potential Channels

If a new leadership structure triggers changes in other governance provisions, shareholders might react to the introduction of new arrangements and, as a result, update their expectations. To investigate this possibility, we assess the impact of non-duality on corporate governance practices by using proxies for monitoring capacity, members income, and family relationships. In addition, we examine strategic stock price manipulation and changes in firm's ownership structure. Since the CVM website provides such information only on an annual basis,[17] we interpret the results from this analysis with caution.

In columns (1) and (2) of Table 8, we investigate whether the duality split changes the number of external auditors and whether firms switch to a Big Four auditor. The estimated coefficients are

[17] Data used to identify auditing firms, members compensation, family ties, and type of stockholder are available only from 2010 on.

of small magnitude, and none of these measures show statistically significant effects, indicating that firms do not strengthen supervision through changes in independent auditing services. Subsequently, we examine whether the new governance arrangement impacts shareholder value by analyzing changes in the earnings of directors and counselors (Page, 2018). The variables represent the logarithm of quarterly average earnings for both board members and directors, respectively. Although the effects in columns (3) and (4) demonstrate a positive response, these differences do not reach statistical significance.

Table 8 About Here

Next, we examine the responses concerning the number of members with familial ties to the CEO. To construct this variable, we identify any individual within the same company who is connected to the CEO through marriage, kinship, or family relationship.[18] The negative sign of the coefficient (column 5) suggests a trend toward reducing family relationships within the firm's leadership. However, the estimate is not statistically different from zero, indicating that the presence of CEO's family links in influential positions may not necessarily pose a threat to shareholder interests. In column (6), we investigate the impact on treasury stock held by the firm. The purpose of this analysis is to assess whether new management might manipulate stock value by implementing stock buybacks, potentially contributing to the increase in shareholder wealth. The result shows a negative and imprecise effect. If anything, the direction of the effect does not support the idea of stock manipulation. Lastly, in column (7), we explore the effects on the share of legal entity stockholders within firms. Given that legal entities include both corporate and institutional investors, who may have significant influence on firm value (Ruiz-Mallorquí and Santana-Martín, 2011), the new governance provision could have led to incentives for ownership restructuring. However, the non-significant estimate suggests that the presence of influential stockholders does not significantly impact firm value.

[18] Positions within the same company encompass roles on the board or any shareholders with ownership stakes. Table A.1 in the Internet Appendix provides a more comprehensive description of the variable definitions.

Finally, the non-duality structure might enhance shareholder value by implementing miscellaneous strategies that directly maximize owners' wealth. This could involve pursuing cost minimization and operational changes within the company. We explore these possibilities in the last columns of Table 8, estimating the effects on sales expenses, tax liabilities, and net revenues. However, our analysis reveals no significant evidence of firms reducing operational costs and tax liabilities or experiencing improvements in sales as a result of the non-duality structure.

7 Conclusion

The prevailing consensus in the existing empirical literature suggests a negative or neutral effect on shareholder value when the CEO-chairman titles are separated based on recommendations from governance rules. However, little is known about the role of mandatory regulations in this matter. In this paper, we revisit the topic and estimate the short- and long-term effects of a compulsory change in CEO duality structure in an emerging economy context.

Employing regression discontinuity and difference-in-differences techniques, our study reveals that corporate governance regulations aimed at eliminating CEO duality can lead to an increase in shareholder value. While shareholders initially view the forthcoming separation of CEO duality favorably upon the announcement of the reform, their actual response to the split appears to be contingent upon effective changes in the firm's corporate directives. Specifically, our results indicate that shifts in financing strategies play a pivotal role in driving the observed enhancement in shareholder value. These patterns emerge even as firms remain unaffected by other governance provisions. The findings are particularly informative given the limited understanding of both the causal effects of CEO non-duality and the underlying causal chains, especially in contexts where investor protection institutions may be weak or limited in their effectiveness. Additionally, leveraging more granular financial statement data, our study explores the role that the timing of financing strategy changes plays in shaping shareholder value, providing greater clarity on the mechanisms at work following the governance transition.

This study provides novel insights for the debate on the value and efficacy of mandatory governance regulations. The results underscore the importance of carefully designed regulations and the need to consider context-specific factors when implementing such reforms. Ultimately, these findings provide valuable guidance for policymakers seeking to enhance investor confidence and promote shareholder welfare in settings with institutional challenges.

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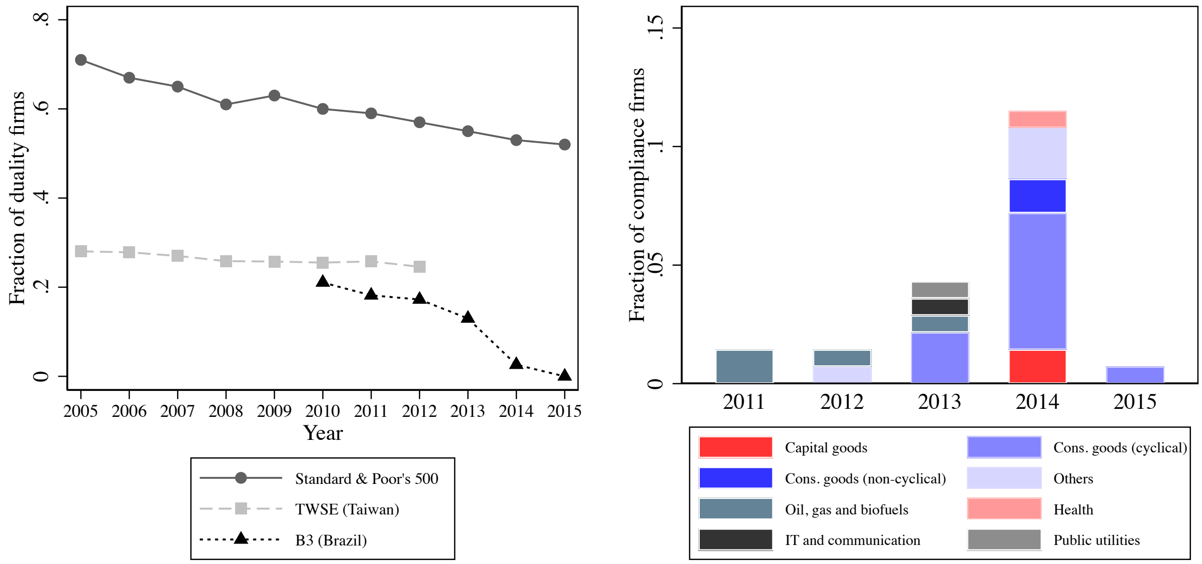

Figure 1: Trends in Separation of CEO-Chairman Titles

Notes: This figure presents the separation of the CEO-chairman positions over time. In the left graph, the vertical axis represents the share of companies with a duality structure. The black triangles represent the evolution across non-financial firms listed in B3 (in special segments). The gray squares represent the evolution across non-financial firms listed in the Taiwan Stock Exchange and Taipei Exchange (2005-2012) (data from Hsu et al. (2021)), while the gray circles represent the evolution across firms listed in Standard & Poor's (S&P) 500 (2005-2015) (data from Spencer Stuart Board Index (2015)). The right graph presents the distribution of compliant firms over the years, categorized by their respective industries.

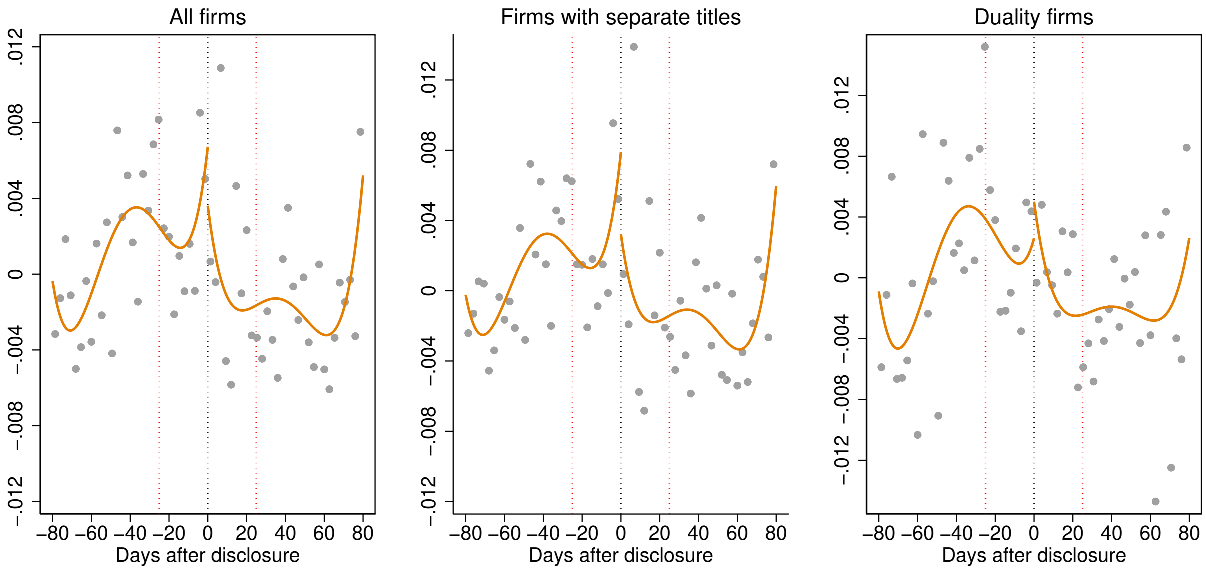

Figure 2: Difference-in-Discontinuities for Abnormal Returns around the Day of Disclosure of the Reform.

Notes: This figure plots abnormal returns in a window of eighty days around the disclosure date of the reform for all firms (left panel), for firms presenting non-duality structure (middle panel), and for duality firms (right panel). The red vertical lines highlight the range with the effective observations used in the estimation sample (bw = 25). The black dots represent the average of the dependent variable within three bins of day and the respective black solid lines represent the predicted values from 4th-order local polynomials estimated separately on either side of the cutoff.

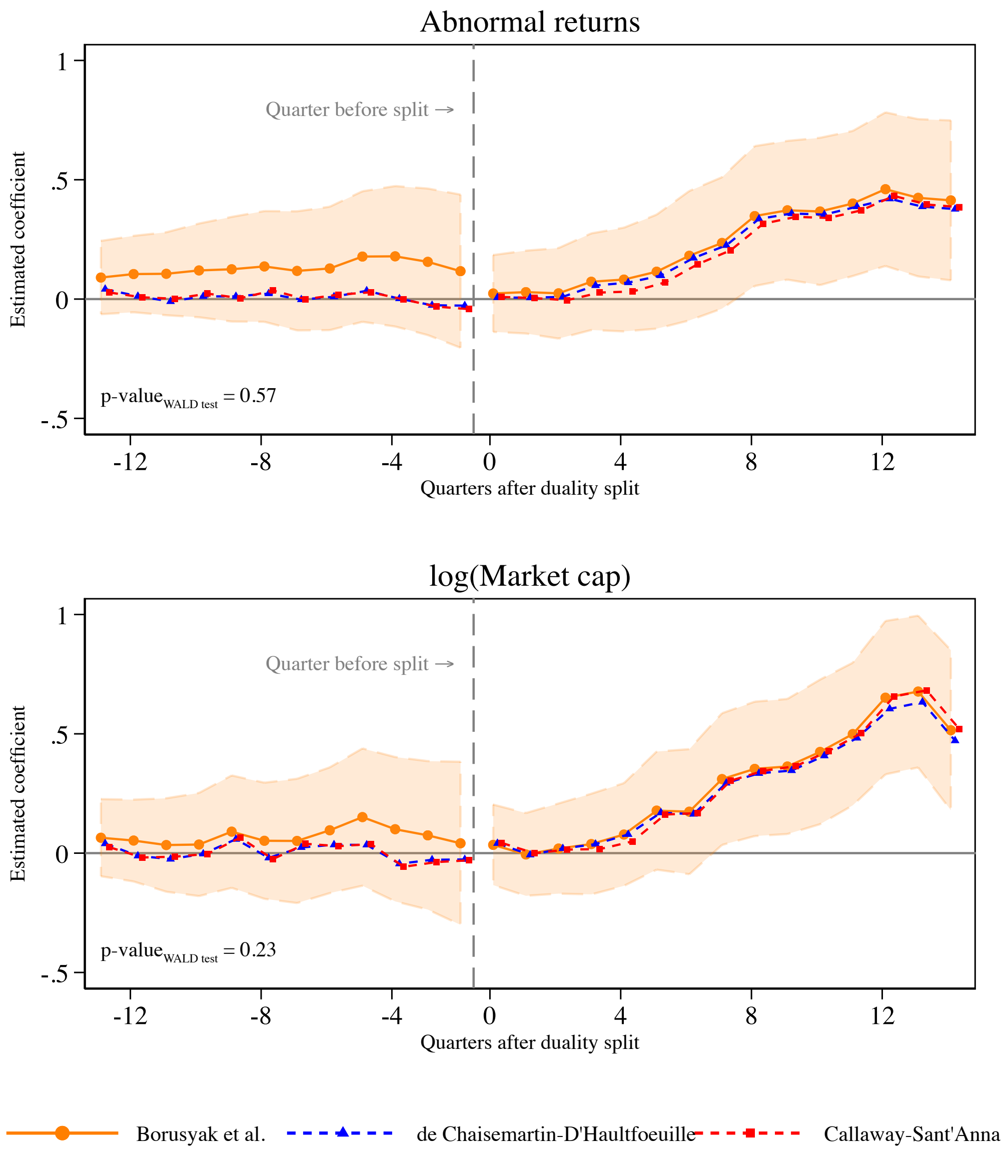

Figure 3: Dynamic Effects of CEO Non-Duality on Shareholder Value

Notes: The panels plot estimates from the fully-dynamic event study model (as defined in Equation 4) for abnormal returns and market cap. The omitted period is $q = -1$. The orange dots represent baseline coefficients obtained from the estimation method of Borusyak et al. (2024) and the shaded areas represent 90% confidence intervals. The blue triangles (red squares) represent coefficients obtained from the estimation method of de Chaisemartin and D'Haultfœuille (2020) (Callaway and Sant'Anna (2021)). The p-value of the Wald joint test over pre-treatment coefficients is reported in the graphs.

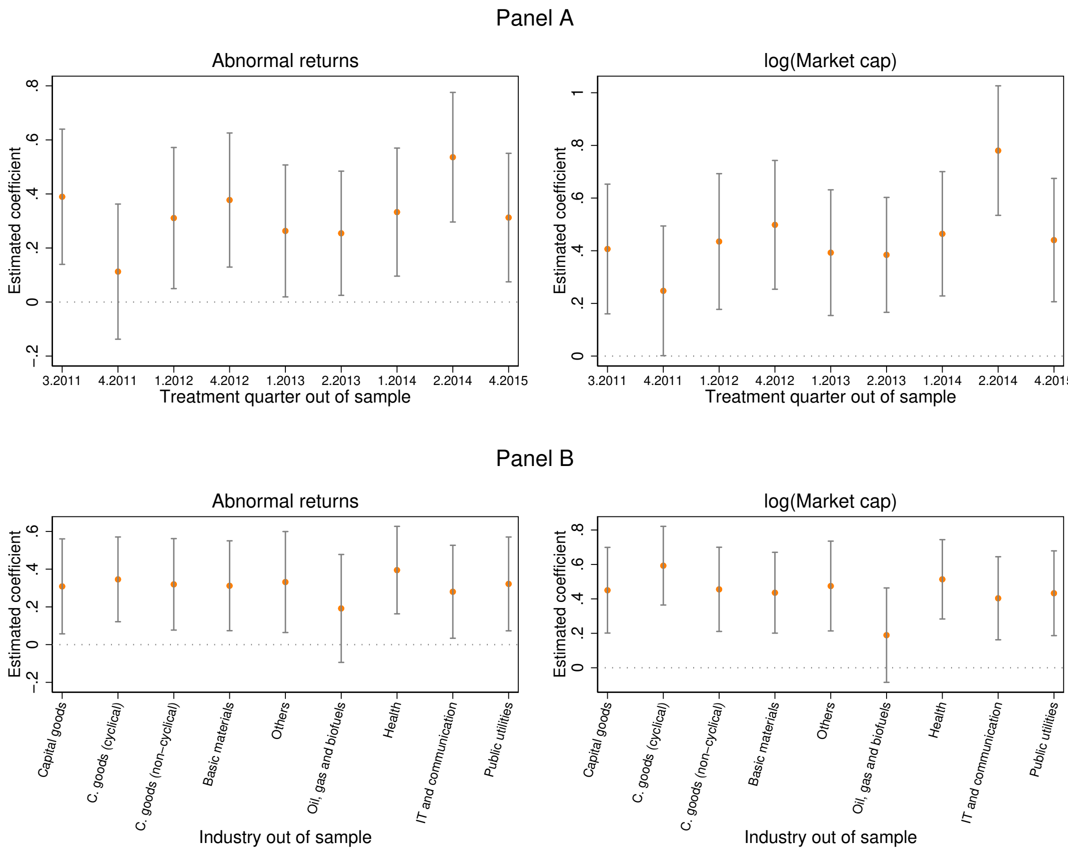

Figure 4: Leave-one-out Tests

Notes: This figure shows point estimates from the static difference-in-differences model (as defined in Equation 5) for different leave-one-out tests. Coefficients are obtained from the estimation method of Borusyak et al. (2024). Panel A plots estimates when removing treated firms at each treatment quarter from the sample. The numbers on the horizontal axis represent, in chronological order, each treatment quarter dropped from the sample. Panel B plots estimates when removing firms from specific industries at each regression. The horizontal axis informs the industry left out from the sample. The vertical lines represent 90% confidence intervals.

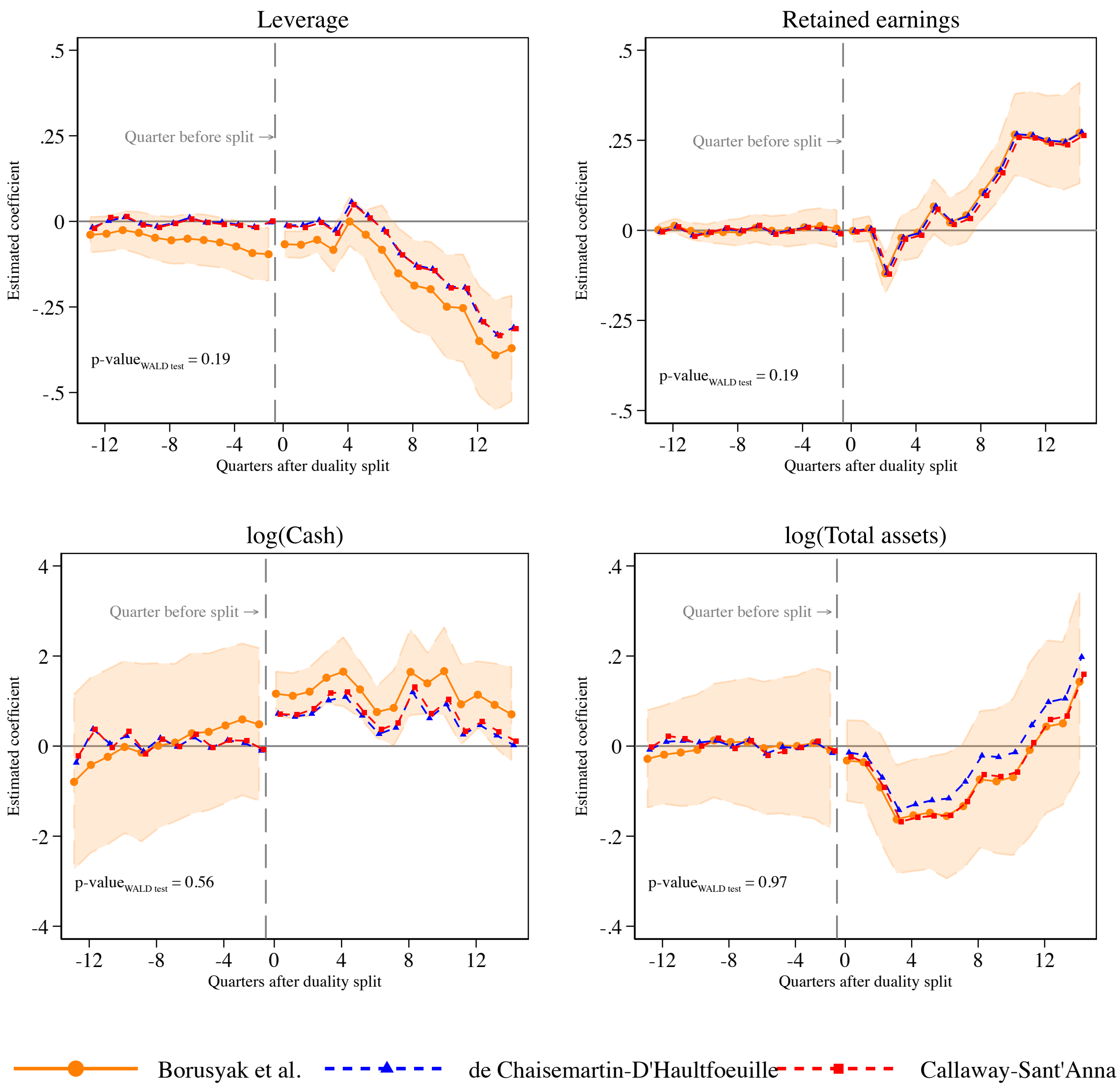

Figure 5: Dynamic Effects of CEO Non-Duality on Financing Activities

Notes: The panels plot estimates from the fully-dynamic event study model (as defined in Equation 4) for total leverage, cash and total assets. The omitted period is $q = -1$. The orange dots represent baseline coefficients obtained from the estimation method of Borusyak et al. (2024) and the shaded areas represent 90% confidence intervals. The blue triangles (red squares) represent coefficients obtained from the estimation method of de Chaisemartin and D'Haultfœuille (2020) (Callaway and Sant'Anna (2021)). The p-value of the Wald joint test over pre-treatment coefficients is reported in the graphs.

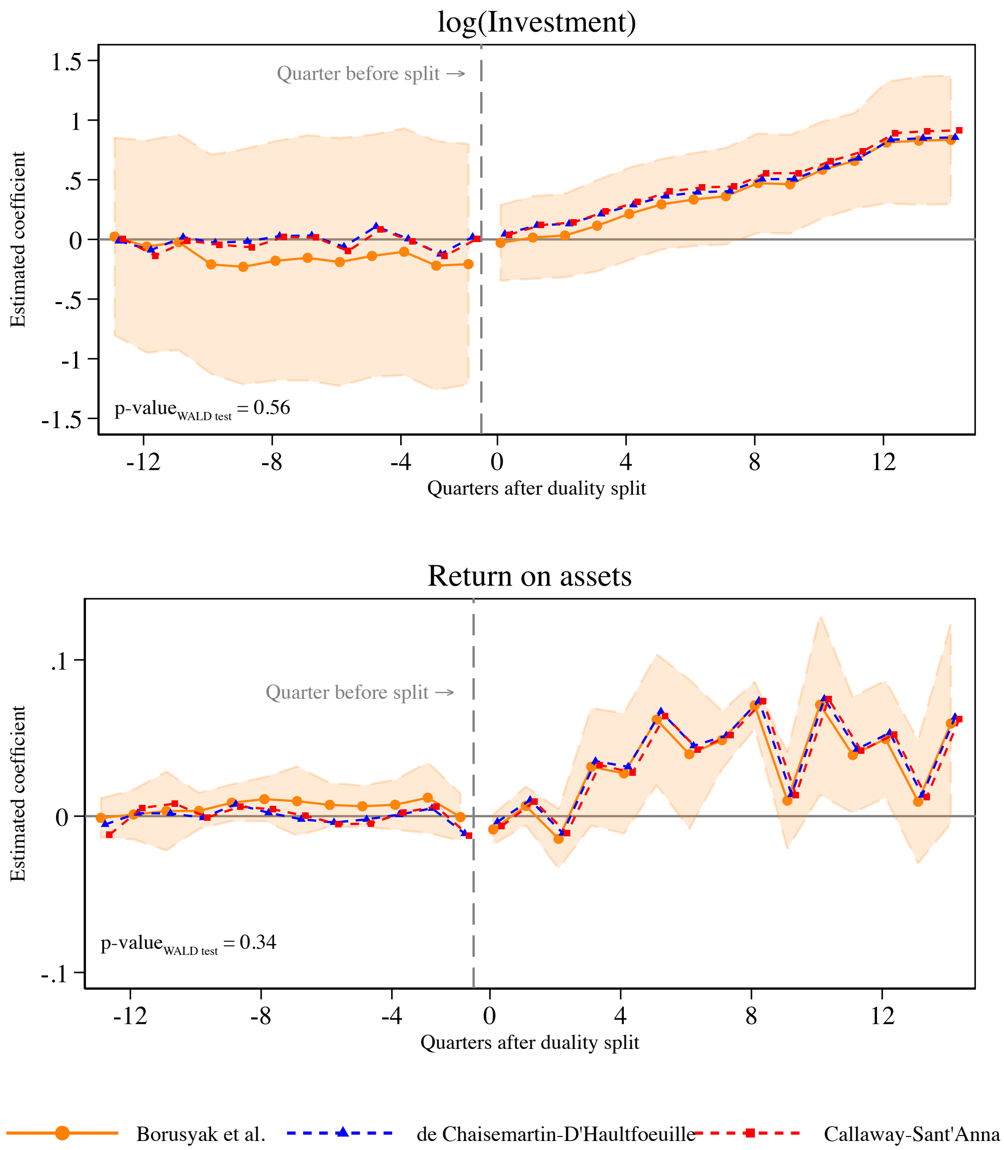

Figure 6: Dynamic Effects of CEO Non-Duality on Investment and Profits

Notes: The panels plot estimates from the fully-dynamic event study model (as defined in Equation 4) for investments and return on assets. The omitted period is $q = -1$. The orange dots represent baseline coefficients obtained from the estimation method of Borusyak et al. (2024) and the shaded areas represent 90% confidence intervals. The blue triangles (red squares) represent coefficients obtained from the estimation method of de Chaisemartin and D'Haultfœuille (2020) (Callaway and Sant'Anna (2021)). The p-value of the Wald joint test over pre-treatment coefficients is reported in the graphs.

Table 1: 2011 Reform - Proposals and Approval Status

Proposal	Segment	Status
Ban on CEO-chairman accumulation of positions.	NM, L2, and L1	Approved
Establishment of an internal audit committee composed of a minimum of three members, with at least one independent counselor.	NM, L2, and L1	Not approved
Increase of independent counselors from 20% to 30% (at least 20% of independent counselors for L1).	NM, L2, and L1	Not approved
Securities trading policy.	NM, L2, and L1	Approved
Code of conduct by managers.	NM, L2, and L1	Approved
Prohibition of establishing a qualified quorum.	NM and L2	Approved
Ban on the clause that prevents voting favorably or imposes onerous burden on shareholders.	NM and L2	Approved
Obligation of the board to manifest itself about any public takeover bid related to shares issued by the company.	NM and L2	Approved
Ban on the limitation of voting rights to 5% of the share capital, except for cases of privatization auctions, limits required by law, or specific regulations related to the firm's operating sector.	NM and L2	Approved
Mandatory public takeover bid if reaching 30% of shareholding acquisition.	NM	Not approved

Notes. This table summarizes the most relevant proposals in the 2011 Reform for each of B3's Special Listing Segments, together with their status of approval.

Table 2: Difference-in-Discontinuity Estimates for Abnormal Returns on the Day of Disclosure of the Reform

	All firms				Firms with separate titles			Duality firms		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$Disclosure_t$	-0.004**	-0.004**	-0.009***	-0.005**	-0.005**	-0.006**	-0.009***	0.001	-0.000	-0.008
	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)	(0.004)	(0.006)
$Disclosure_t \times Duality_f$				0.007*						
				(0.004)						
Effect. observations	5,394	5,287	5,394	5,394	4,206	4,122	4,206	1,188	1,165	1,188
Polynomial	Linear	Linear	Quad.	Linear	Linear	Linear	Quad.	Linear	Linear	Quad.
Weekday-month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Method	OLS	Local reg.	OLS	OLS	OLS	Local reg.	OLS	OLS	Local reg.	OLS

Notes. This table reports estimated discontinuities on daily abnormal returns on the day of announcement of the 2011 reform obtained from standard RD and difference-in-discontinuities approaches. Results are presented segregated for all firms, firms with separate titles (non-dual firms), and duality firms. Each column shows results from a separate regression. Columns (1), (4) and (7) show the standard regression discontinuity estimates and Columns (2)–(3), (5)–(6), and (8)–(9) show difference-in-discontinuity estimates. The last column reports the p-value from the statistical test of the difference between the coefficient reported in column (6) with the coefficient in column (9) for the models with weekday-month fixed effects. Estimation samples include daily observations within a 20-day bandwidth for firms listed in NM and L2 segments. Heteroskedasticity robust standard errors are in parentheses. ***, **, * represent statistical significance at the 1%, 5% and 10% levels, respectively.

Table 3: Validity Test for Time-Invariant Confounders

	All firms (1)	Firms with separate titles (2)	Duality firms (3)
($\hat{\gamma}_1$) $Disclosure_t \times \theta_{2010} \times h(t)$	-0.002***	-0.002***	-0.003**
	(0.001)	(0.001)	(0.001)
($\hat{\gamma}_2$) $Disclosure_t \times \theta_{2009} \times h(t)$	-0.001**	-0.002**	-0.000
	(0.001)	(0.001)	(0.001)
($\hat{\gamma}_3$) $Disclosure_t \times \theta_{2008} \times h(t)$	-0.001*	-0.001*	-0.001
	(0.001)	(0.001)	(0.001)
$\theta_{2010} \times h(t)$	0.001***	0.001**	0.001
	(0.000)	(0.000)	(0.001)
$\theta_{2009} \times h(t)$	0.001*	0.001**	-0.001
	(0.000)	(0.000)	(0.001)
$\theta_{2008} \times h(t)$	0.000	0.000	0.001
	(0.000)	(0.000)	(0.001)
Constant	-0.000	0.001	-0.004*
	(0.001)	(0.002)	(0.003)
Observations	3,965	3,120	845
Firm FE	Yes	Yes	Yes
Weekday-month FE	Yes	Yes	Yes
R-squared	0.031	0.029	0.054
Wald-test ($H_0 : \gamma_1 = \gamma_2 = \gamma_3$)			
(p-value)	0.14	0.41	0.11

Notes. This table presents RD estimates for the validity check of time-invariant confounders. Each column shows results from a separate regression. The sample is restricted to observations within the optimal bandwidth, estimated using the procedure of Calonico et al. (2014). Standard errors (in parentheses) are calculated with a firm-level cluster. ***, **, * represent statistical significance at the 1%, 5% and 10% levels, respectively.

Table 4: Descriptive Statistics

	Control firms (N = 82)			Treated firms (N = 28)			Diff. of means
	Mean	S.D.	Obs.	Mean	S.D.	Obs.	(p-value)
Panel A: Stocks and financial figures							
Abnormal returns	0.001	0.666	3,106	0.002	0.814	1,049	0.97
Market cap (R$ billion)	4.710	6.849	3,106	3.654	5.380	1,049	0.00
Total assets (R$ billion)	6.242	9.464	3,100	4.559	4.890	1,049	0.00
Cash (R$ billion)	0.415	0.702	3,100	0.292	0.458	1,049	0.00
Leverage	0.431	0.363	3,100	0.440	0.350	1,049	0.49
Short-term	0.202	0.213	3,100	0.184	0.201	1,049	0.02
Long-term	0.219	0.201	3,100	0.252	0.206	1,049	0.00
Financing loans	0.223	0.203	3,099	0.217	0.170	1,049	0.45
Investment	0.112	0.155	3,018	0.093	0.152	1,021	0.00
Return on assets	0.019	0.110	3,100	0.011	0.122	1,049	0.05
Sale expenses (R$ billion)	0.157	0.380	3,100	0.067	0.169	1,049	0.00
Tax liability (R$ billion)	0.044	0.107	3,100	0.020	0.030	1,049	0.00
Revenues (R$ billion)	1.474	3.259	3,100	0.776	1.238	1,049	0.00
Panel B: Other characteristics							
External auditors	1.613	0.578	2,481	1.551	0.592	857	0.01
Big four	0.501	0.500	2,481	0.471	0.499	857	0.14
Average earnings							
Directors (R$ million)	0.186	0.096	2,944	0.221	0.114	970	0.00
Counselors (R$ million)	0.043	0.039	2,932	0.048	0.046	974	0.00
# of family members	0.077	0.267	2,533	0.430	0.495	881	0.00
Treasury stock	0.116	0.195	2,481	0.174	0.208	857	0.00
Legal entities	0.280	0.294	2,481	0.332	0.282	857	0.00

Notes. This table reports descriptive statistics for the variables used in our analysis segregated by firms with separate titles prior to regulation (control firms) and duality firms that will eventually split positions (treated firms) during the observed period. The sample includes firms in NM and L2 special segments. Details on variables definitions are presented in Table A.1.

Table 5: Effects of CEO Non-Duality on Shareholder Value

	Abnormal returns				log(Market cap)			
	Full sample	Full sample	Full sample	Matched sample	Full sample	Full sample	Full sample	Matched sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Duality-split$_{ft}$	0.312**	0.303**	0.285**	0.534**	0.436***	0.438***	0.411***	0.520**
	(0.144)	(0.142)	(0.141)	(0.244)	(0.142)	(0.144)	(0.138)	(0.238)
Effect magnitude (%)	36.6%				54.6%			
Observations	4,155	4,155	4,155	1,689	4,155	4,155	4,155	1,689
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-time FE	Yes	Yes	No	Yes	Yes	Yes	No	Yes
N. of control / treated firms	82 / 28	82 / 28	82 / 28	24 / 24	82 / 28	82 / 28	82 / 28	24 / 24
Covariates	No	Yes	No	Yes	No	Yes	No	Yes
Time FE	No	No	Yes	No	No	No	Yes	No
Industry-year FE	No	No	Yes	No	No	No	Yes	No

Notes. This table reports difference-in-differences estimates of the impacts of CEO duality rupture (as displayed in Equation 5) on shareholder value, obtained from the estimation method of Borusyak et al. (2024). Each column shows results from a separate regression. Columns (1) and (5) present treatment effects without control variables, while columns (2) and (6) add control variables (interactions between a linear time trend with year of foundation and listing segment). Columns (3) and (7) use time (quarter) fixed effect and industry-year fixed effect, instead of baseline industry-time fixed effect. Columns (4) and (8) employ a one-to-one matched sample obtained by propensity score matching technique. Variables used in the matching include: year of foundation, operating segment, and industry. Table A.1 provides detailed variable definitions. All specifications include firm fixed effect. The table also reports the total number of treated and control firms used in each regression. Baseline means for market cap are in R$ billions. The sample includes firms listed in the NM and L2 special segments. Standard errors (in parentheses) are calculated with a firm-level cluster. ***, **, * represent statistical significance at the 1%, 5% and 10% levels, respectively.

Table 6: Heterogeneous Effects of CEO Non-Duality on Shareholder Value by Type of Firm

	Firm size		Firm age		Earnings management	
	Larger	Smaller	Older	Younger	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Abnormal returns						
Duality-split$_{ft}$	0.793***	-0.015	0.275	0.056	0.546**	-0.077
	(0.253)	(0.203)	(0.259)	(0.176)	(0.214)	(0.212)
Observations	1,956	2,063	1,975	2,020	2,002	2,035
Panel B: log(Market cap)						
Duality-split$_{ft}$	0.779***	0.003	0.260	0.105	0.786***	-0.096
	(0.229)	(0.208)	(0.247)	(0.173)	(0.205)	(0.195)
Observations	1,956	2,063	1,975	2,020	2,002	2,035
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-time FE	Yes	Yes	Yes	Yes	Yes	Yes
N. of control / treated firms	40 / 11	40 / 15	38 / 14	42 / 12	37 / 15	43 / 11
Covariates	No	No	No	No	No	No

Notes. This table reports difference-in-differences estimates of the impacts of CEO duality rupture (as displayed in Equation 5) on shareholder value stratified by pre-reform levels of firm size, firm age, and pre-reform discretionary accruals levels obtained from the estimation method of Borusyak et al. (2024). Columns (1)–(2) show results for larger and smaller firms, columns (3)–(4) show results for older and younger companies, and columns (5)–(6) display results for firms with high and low information asymmetry. The definition of larger and smaller firms is based on a median split of total assets observed in the years prior to the 2011 reform. The definition of older and younger firms is based on a median split of year of foundation. High- and low-information asymmetry firms are defined based on a median split of pre-reform levels of discretionary accruals. All specifications include firm and industry-time fixed effects. The sample includes firms listed in the NM and L2 special segments. Standard errors (in parentheses) are calculated with a firm-level cluster. ***, **, * represent statistical significance at the 1%, 5% and 10% levels, respectively.

Table 7: Effects of CEO Non-Duality on Financing Policies, Investment, and Profits

	Leverage									
	Total	Short-term	Long-term	Financing loans	Retained earnings	log(Cash)	log(Total assets)	log(Invest.)	Return on assets	Adjusted ROA
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Duality-split$_{ft}$	-0.277***	-0.095***	-0.132***	-0.057***	0.154***	1.352***	0.069	0.357*	0.054***	0.045***
	(0.068)	(0.029)	(0.036)	(0.022)	(0.044)	(0.393)	(0.084)	(0.211)	(0.017)	(0.015)
Observations	4,149	4,149	4,149	4,149	4,148	4,149	4,148	4,039	4,149	4,149
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N. of control / treated firms	82 / 28	82 / 28	82 / 28	82 / 28	82 / 28	82 / 28	82 / 28	82 / 28	82 / 28	82 / 28
Covariates	No	No	No	No	No	No	No	No	No	No

Notes. This table reports difference-in-differences estimates of the impacts of CEO duality rupture (as displayed in Equation 5) on proxies of financing policies, obtained from the estimation method of Borusyak et al. (2024). Each column shows results from a separate regression. Cash holding and total assets are log-linearized, while the remaining outcomes represent the relative proportion of debt and financial obligations measures to total assets. Table A.1 provides detailed variable definitions. All specifications include firm and industry-time fixed effects. The sample includes firms listed in the NM and L2 special segments. Standard errors (in parentheses) are calculated with a firm-level cluster. ***, **, * represent statistical significance at the 1%, 5% and 10% levels, respectively.

Table 8: Effects of CEO Non-Duality on Alternate Governance Provisions and Operating Performance

	Monitoring		Board earnings		Family ties					
	External auditors	Big four	Counselors	Directors	# of members	Treasury stock	Legal entities	Sale expenses	Tax liability	Revenues
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Duality-split$_{ft}$	-0.069	0.061	0.037	0.097	-0.095	-0.015	0.026	-0.081	-0.471	-0.020
	(0.076)	(0.062)	(0.092)	(0.136)	(0.066)	(0.027)	(0.041)	(0.480)	(0.481)	(0.025)
Observations	3,330	3,330	3,882	3,662	3,414	3,330	3,330	4,149	4,149	4,149
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N. of control / treated firms	81 / 28	81 / 28	82 / 28	82 / 28	82 / 28	81 / 28	81 / 28	82 / 28	82 / 28	82 / 28
Covariates	No	No	No	No	No	No	No	No	No	No

Notes. This table reports difference-in-differences estimates of the impacts of the CEO duality rupture (as displayed in Equation 5) on proxies for monitoring, members compensation, family ties, treasury stock, stockholder type (legal entities), and operating performance, obtained from the method of estimation of Borusyak et al. (2024). Each column shows results from a separate regression. Table A.1 provides detailed variable definitions. All specifications include firm and industry-time fixed effects. The sample includes firms listed in the NM and L2 special segments. Standard errors (in parentheses) are calculated with a firm-level cluster.

For Online Publication

Internet Appendix for "Mandatory CEO Non-Duality, Managerial Agency, and Shareholder Value"

A Additional Figures and Tables

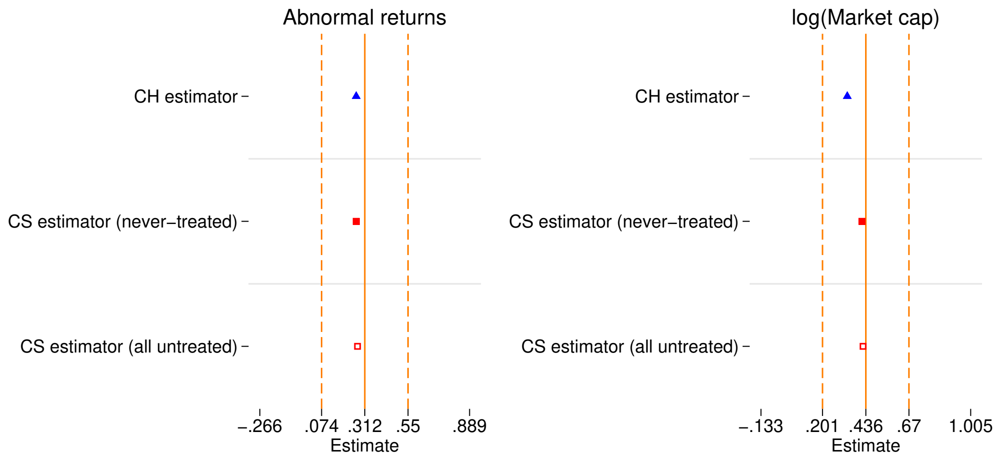

Figure A1: Static Difference-in-Differences Estimates – Robustness to Alternate Estimators

Notes: This figure shows point estimates from the static difference-in-differences model (as defined in Equation 5) for different estimation methods. The vertical solid lines show the baseline estimates obtained from the estimation method of Borusyak et al. (2024) showed in columns (2) and (6) of Table 5, and the vertical dashed lines represent their respective 90% confidence intervals. The blue triangles represent the estimates obtained by the method of de Chaisemartin and D'Haultfœuille (2020). The solid (hollow) red squares represent the estimates obtained from the method of Callaway and Sant'Anna (2021) using only never treated firms (all untreated firms) in the control group.

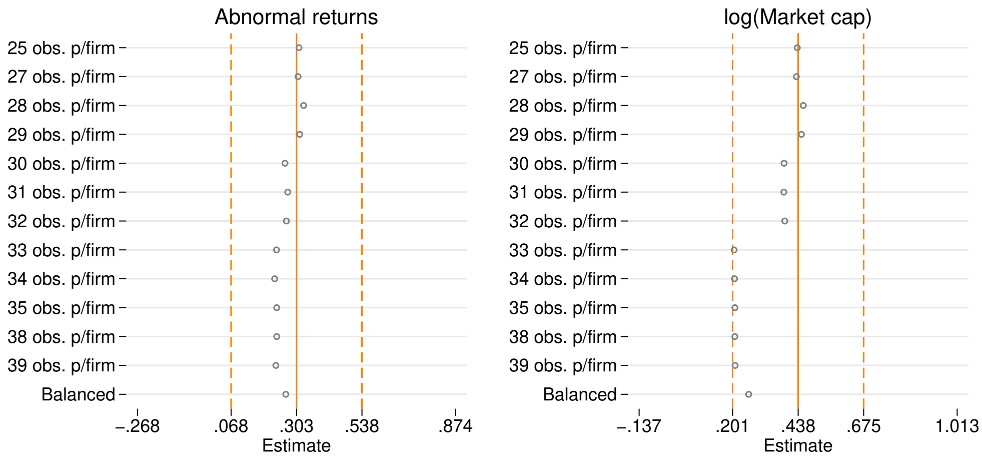

Figure A2: Static Difference-in-Differences Estimates – Different Subsamples

Notes: This figure shows point estimates from the static difference-in-differences model (as defined in Equation 5) for different subsamples. Coefficients are obtained from the estimation method of Borusyak et al. (2024). The vertical solid lines show the baseline estimates obtained in columns (2) and (6) of Table 5, and the vertical dashed lines represent their respective 90% confidence intervals. The vertical axis indicates the minimum number of firm-quarter observations considered in the estimation sample — the smaller the number indicated on the axis, the more unbalanced the panel is (but the larger the sample size). The balanced panel includes all firms with 40 observations per quarter.

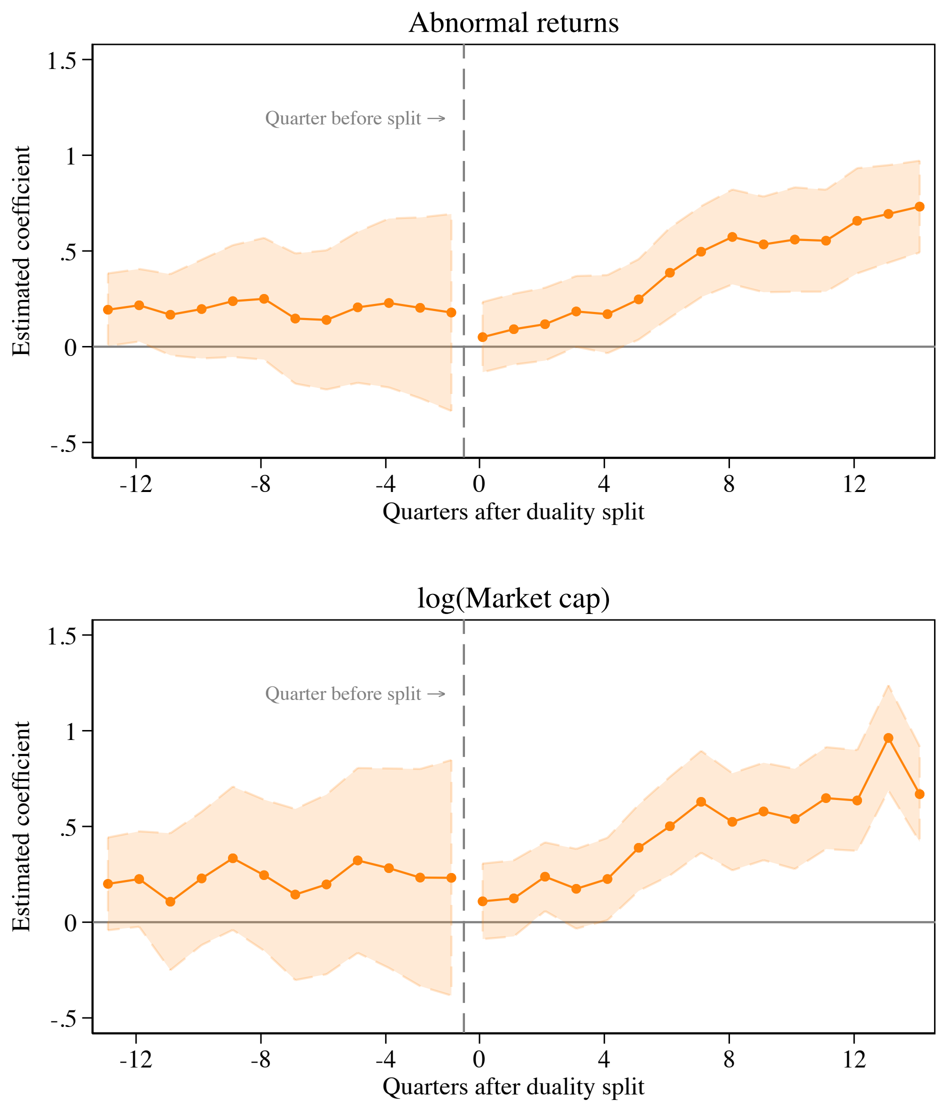

Figure A3: Event Study Results Excluding Firms Expected to Split

Notes: This figure plots point estimates from the fully-dynamic event study model (as defined in Equation 4) on shareholder value, excluding firms in which compliance coincides with the end of the CEO period of mandate. The control group includes all firms listed in the NM and L2 special segments presenting separate titles prior to the regulation. The coefficients are obtained from the estimation method of Borusyak et al. (2024) and the shaded areas represent 90% confidence intervals.

Table A.1: Variables Definition

Variable	Description
Abnormal returns	Quarterly-averaged residuals from a log-linear regression of the daily closing prices on market return, controlled by firm and date fixed effects.
Market cap	Logarithm of the number of shares outstanding multiplied by stock price.
Leverage	Book value of total liabilities, divided by book value of total assets.
Short-term leverage	Book value of short-term liabilities, divided by book value of total assets.
Long-term leverage	Book value of long-term liabilities, divided by book value of total assets.
Financing loans	Book value of total debt, divided by the book value of total assets.
Cash	Logarithm of cash and cash equivalents.
Total assets	Logarithm of the book value of total assets.
Retained earnings	Cumulative profits after accounting for dividends paid to shareholders, divided by the book value of total assets.
Investment	Logarithm of property, plant and equipment, divided by the book value of total assets in $t-1$.
Return on assets	Net profit divided by the book value of total assets.
Adjusted ROA	Earnings before interest and taxes divided by the book value of total assets.
Sale expenses	Logarithm of the costs related to sale activities.
Tax liability	Logarithm of the total taxes owed to the Fiscal Authority from income tax, self-employment tax, and capital gains tax.
Revenues	Logarithm of the gross revenue minus discounts or allowances.
External auditors*	Total number of auditing companies providing services to the listed firm.
Big four*	Dummy variable indicating if the listed firm is audited by a Big Four.
Average earnings*	
Directors	Logarithm of the quarterly-average earnings of directors.
Counselors	Logarithm of the quarterly-average earnings of counselors.
# of family members*	Dummy variable indicating if any member has a family connection with the CEO in the same company by way of marriage, kinship, or family relationship (including parents, children, siblings, grandparents, and grandchild).
Treasury stock*	Share held as treasury stock.
Legal entities*	Share of firms and institutional investors holding stock in the listed firm.

Notes. This table presents the definition of market- and accounting-based variables. * information available from 2010 onwards.

Table A2: Firm Baseline Characteristics

	Control firms (N = 82)		Treated firms (N = 28)		
	Mean	Obs.	Mean	Obs.	Diff. (p-value)
Year of foundation	1984	3,106	1987	1,049	0.00
Listing segment					
New market	0.896	3,106	0.933	1,049	0.00
L2 governance	0.104	3,106	0.067	1,049	0.00
Industry					
Capital goods	0.231	3,106	0.068	1,049	0.00
Consumer goods (cyclical)	0.251	3,106	0.427	1,049	0.00
Consumer goods (non-cyclical)	0.124	3,106	0.076	1,049	0.00
Basic materials	0.062	3,106	0	1,049	0.00
Oil, gas, and biofuels	0.010	3,106	0.141	1,049	0.00
Health	0.062	3,106	0.076	1,049	0.10
IT and communication	0.023	3,106	0.038	1,049	0.01
Public utilities	0.141	3,106	0.029	1,049	0.00
Others	0.095	3,106	0.145	1,049	0.00

Notes. This table reports summary statistics for firm's year of foundation, listing segment, and operating industry by firms with separate titles prior to regulation (control firms) and duality firms that will eventually split positions (treated firms) during the observed period. The sample includes firms in NM and L2 special segments.

Table A3: Robustness to Alternative Clustering of Standard Errors

	Abnormal returns			log(Market cap)		
	(1)	(2)	(3)	(4)	(5)	(6)
SE	(0.087)	(0.080)	(0.028)	(0.078)	(0.074)	(0.028)
p-value	0.000	0.000	0.000	0.000	0.000	0.000
Observations	4,155	4,155	4,155	4,155	4,155	4,155
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-time FE	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	No	No	No	No	No
Cluster type	Industry	Ind.-cohort	Ind.-time	Industry	Ind.-cohort	Ind.-time

Notes. This table reports standard errors (in parentheses) and p-values derived from alternate clustering methods (industry, industry-cohort, and industry-time) for the difference-in-differences estimates of the impacts of CEO duality rupture on shareholder value (as displayed in columns 2 and 6 of Table 5). All specifications include firm and industry-time fixed effects. The sample includes firms listed in the NM and L2 special segments.

Table A4: Effects of CEO Non-Duality on Different Proxies of Abnormal Returns

	Mean price (1)	Opening price (2)	Min. price (3)	Max. price (4)
Duality-split$_{ft}$	0.313**	0.313**	0.314**	0.313**
	(0.144)	(0.144)	(0.145)	(0.144)
Observations	4,155	4,155	4,155	4,155
Firm FE	Yes	Yes	Yes	Yes
Industry-time FE	Yes	Yes	Yes	Yes
Covariates	No	No	No	No

Notes. This table reports difference-in-differences estimates of the impacts of CEO duality rupture (as defined in Equation 5) on alternate proxies of shareholder value. The abnormal returns are calculated using stocks' mean prices, open prices, minimum prices, and maximum prices instead of closing prices. All specifications include firm and industry-time fixed effects. The sample includes firms listed in the NM and L2 special segments. Standard errors (in parentheses) are calculated with a firm-level cluster.

Table A5: Effects of CEO Non-Duality on Shareholder Value Controlling for Financing Policy Measures

	Abnormal returns			log(Market cap)		
	(1)	(2)	(3)	(4)	(5)	(6)
Duality-split$_{ft}$	0.067	-0.025	0.066	0.208*	0.095	0.131
	(0.107)	(0.131)	(0.105)	(0.110)	(0.124)	(0.107)
Observations	4,148	4,149	4,148	4,148	4,149	4,148
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-time FE	Yes	Yes	Yes	Yes	Yes	Yes
Covariates						
Retained earnings	Yes	No	Yes	Yes	No	Yes
log(Cash)	Yes	No	Yes	Yes	No	Yes
Leverage	No	Yes	Yes	No	Yes	Yes

Notes. This table reports difference-in-differences estimates of the impacts of CEO duality rupture on shareholder value (as displayed in Equation 5) controlling for capital structure measures, obtained from the method of estimation of Borusyak et al. (2024). Each column shows results from a separate regression. Columns (1) and (4) show estimates including cash holding as controls, and columns (2) and (5) include leverage. Columns (3) and (6) include both variables. All specifications include firm and industry-time fixed effects. The sample includes firms listed in the NM and L2 special segments. Standard errors (in parentheses) are calculated with a firm-level cluster.

Table A6: Robustness of the Effects of CEO Non-Duality on Financing Policy Measures

	Leverage				Retained earnings				log(Cash)			
	Main estim.	Full sample	Full sample	Matched sample	Main estim.	Full sample	Full sample	Matched sample	Main estim.	Full sample	Full sample	Matched sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Duality-split$_{ft}$	-0.277***	-0.277***	-0.278***	-0.233**	0.154***	0.155***	0.147***	0.149*	1.352***	1.327***	1.279***	1.560**
	(0.068)	(0.069)	(0.068)	(0.115)	(0.044)	(0.045)	(0.043)	(0.090)	(0.393)	(0.388)	(0.365)	(0.652)
Observations	4,149	4,149	4,149	1,685	4,148	4,148	4,148	1,685	4,149	4,149	4,149	1,685
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-time FE	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes
N. of control / treated	82 / 28	82 / 28	82 / 28	24 / 24	82 / 28	82 / 28	82 / 28	24 / 24	82 / 28	82 / 28	82 / 28	24 / 24
Covariates	No	Yes	No	No	No	Yes	No	No	No	Yes	No	No
Time FE	No	No	Yes	No	No	No	Yes	No	No	No	Yes	No
Industry-year FE	No	No	Yes	No	No	No	Yes	No	No	No	Yes	No

Notes. This table reports difference-in-differences estimates of the impacts of CEO duality rupture (as displayed in Equation 5) on financing policy measures, obtained from the estimation method of Borusyak et al. (2024). Each column shows results from a separate regression. Columns (1) and (5) present treatment effects without control variables, while columns (2) and (6) add baseline controls (interactions between a linear time trend with year of foundation and listing segment). Columns (3) and (7) use time (quarter) fixed effect and industry-year fixed effect, instead of baseline industry-time fixed effect. Columns (4) and (8) employ a one-to-one matched sample obtained by propensity score matching technique. Variables used in the matching include: year of foundation, operating segment, and industry. Table A.1 provides detailed variable definitions. All specifications include firm fixed effect. The table also reports the total number of treated and control firms used in each regression. The sample includes firms listed in the NM and L2 special segments. Standard errors (in parentheses) are calculated with a firm-level cluster.

B Ancillary Results

B.1 RD Robustness Checks

In this section, we examine the sensitivity of the diff-in-disc estimates documented in the main paper. Results are shown separately for all firms, firms with separate titles, and duality firms.

In Table B.2, we check the robustness of results by experimenting different sample configurations for the group of non-duality firms. Columns (1) and (4) exclude firms from the L2 segment, thus considering the remaining listed companies in NM special segment. As is apparent, the magnitudes are virtually the same as compared to results reported in Table 2. In columns (2) and (5), we keep firms in the L2 segment and exclude those from the basic materials sector, since no duality firm is found in this industry. The coefficients remain indistinguishable from our baseline results. Additionally, estimates are unchanged when L2 companies are excluded from this sample (columns (3) and (6)), indicating that both segment and operating industry do not drive our results.

Table B.3 reports estimates using models that include alternate controls and polynomial order of the running variable. In columns (1), (4), and (7), we include weekday fixed effect and month fixed effect together with industry-year fixed effects to account for potential non-linearities in these period levels and different trends in sector activities. As is shown, results are very stable to controlling for these interaction terms. In the next columns, we extend our model by incorporating weekday and month fixed effects. The resulting estimates remain precise and virtually unchanged. Finally, we obtain similar results when adding pre-reform covariates to our baseline model, which include total assets, total liabilities, market capitalization, and total shares. In sum, point estimates remain non-sensitive to these exercises.

Since we are interested in obtaining a local short-run market reaction, it is reasonable to only consider observations relatively close to the treatment event. Thus, one concern to the reliability of our diff-in-disc results could be related to the *ad hoc* choice of the estimation window. In Figure

B.1, we report estimates for our main model using several alternative windows for estimation, exploiting both narrower and wider ranges. As expected, patterns suggest that utilizing smaller or larger bandwidths does not alter our conclusions. In sum, the effects remain both quantitatively and qualitatively similar to our main findings.

Finally, we address the concern about the role of autoregression on the estimation of causal effects in RD in time designs. Specifically, if the outcome presents an autoregressive component, not considering this process would lead to a misspecification issue. In Table B.4, we present results using the difference between 2011 and 2010 in actual daily abnormal returns (instead of its first-difference) as the dependent variable and thus augment Equation 2 demonstrated in the main paper by incorporating both firm fixed effects and autoregressive processes. These processes include up to four lags of the dependent variable. In all models and for all types of firms, the coefficient of the first lag of the outcome is close to unity and highly significant, indicating the presence of an AR(1) process. This result confirms the importance to consider the autoregressive component. Importantly, the table also shows that diff-in-disc coefficients are very stable even if other lags of the outcome are included. We underscore that these estimates are virtually the same as those in Table 2, in which we consider the first-difference in abnormal returns as the dependent variable.

The findings in this section support that estimated short-term market reactions are likely to represent local causal effects.

B.2 Additional Robustness to DD Results

Borusyak, Jaravel and Spiess (2021) observe that, even ruling out anticipatory effects, the parallel trajectories assumption can still be violated. To provide alternate formal test for this assumption, we take a second-best solution by following the de Chaisemartin and D'Haultfœuille (2020) placebo strategy. The procedure consists in pretending a placebo treatment in a period prior to the true event and estimating the treatment effects. Figure B.2 reports the results using $t - 8$ quarters before the actual treatment event and inspecting post-treatment differences obtained from our baseline estimation procedure. Findings show that neither outcomes respond to the fake treatment.

In Table B.1, we explore information about news releases and firm reports to check for the possibility of other confounding events. Specifically, we control for the total number of releases and reports of different contents made by firms, the total number of board meetings, and the number of announcements made to shareholders, all in a quarterly basis. As is apparent, adding these characteristics barely changes point estimates. The models in columns (3) and (6) incorporate all variables. Their inclusion does not harm the precision of the estimates and the coefficients remain virtually intact. These results discard the role of such events in driving our main findings.

In order to verify whether results are derived purely by chance, we execute a placebo test according to the Fisher randomization test. That is, we randomly generate false treatment quarters for each firm and then run Equation 5. By repeating this step 500 times, we draw the empirical null distribution of the placebo effects for the third and sixth quarter lags. Figure B.3 depicts the estimated cumulative distribution of placebo dynamic effects together with the real estimates (vertical lines) reported in Table 5 of the main manuscript for each outcome. The image of the real estimate on the vertical axis represents the estimated p-value. P-values derived from the permutation test are analogous to those from baseline results, thus strengthening the existence of a positive relationship between shareholder value and duality rupture.

B.3 Discretionary Accruals Estimation

To estimate our accrual-based variable used to split samples in our effect heterogeneity analysis, we use a version of the model proposed by Kothari et al. (2005). Since we cannot compute total accruals ($TA_{i,t}$) as in Kothari et al. (2005), we compute $TA_{i,t}$ based on Kim et al. (2017)'s model. Specifically, we run the following regression:

$$\frac{TA_{i,t}}{Assets_{i,t-1}} = \beta_0 + \beta_1 \frac{1}{Assets_{i,t-1}} + \beta_2 \frac{\Delta Rev_{i,t} - \Delta AR_{i,t}}{Assets_{i,t-1}} + \beta_3 \frac{PPE_{i,t}}{Assets_{i,t-1}} + \beta_4 ROA_{i,t-1} + \theta_t + \epsilon_{i,t}, \quad \text{(B.7)}$$

where $TA_{i,t}$ represents net income minus operating cash flows, $Assets_{i,t-1}$ denotes lagged total assets, $\Delta Rev_{i,t}$ ($\Delta AR_{i,t}$) stands for changes in revenues (accounts receivables) from $t-1$ to t, $PPE_{i,t}$ comprises property, plant, and equipment, and $ROA_{i,t-1}$ measures lagged return on assets. The specification includes quarter (θ_t) fixed effects as we may have cross-sectional limitations for each time period. Importantly, we run Equation B.7 separately for each industry.

Finally, we proxy discretionary accruals by using the absolute value of the predicted errors ($\hat{\epsilon}_{i,t}$). To categorize the samples into 'high' and 'low' levels of earnings management, we perform a median split based on pre-reform levels of this variable.

REFERENCES

Borusyak, Kirill, Xavier Jaravel, and Jann Spiess (2021). "Revisiting Event Study Designs, with an Application to the Estimation of the Marginal Propensity to Consume," Working Paper, SSRN.

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Kim, J., Y. Kim, and J. Zhou. (2017) "Languages and earnings management," *Journal of Accounting and Economics*, 63 (2), 288–306.

Kothari, S. P., A. J. Leone, and C. E. Wasley. (2005) "Performance matched discretionary accrual measures," *Journal of Accounting and Economics*, 39 (1), 163–197.

Figures

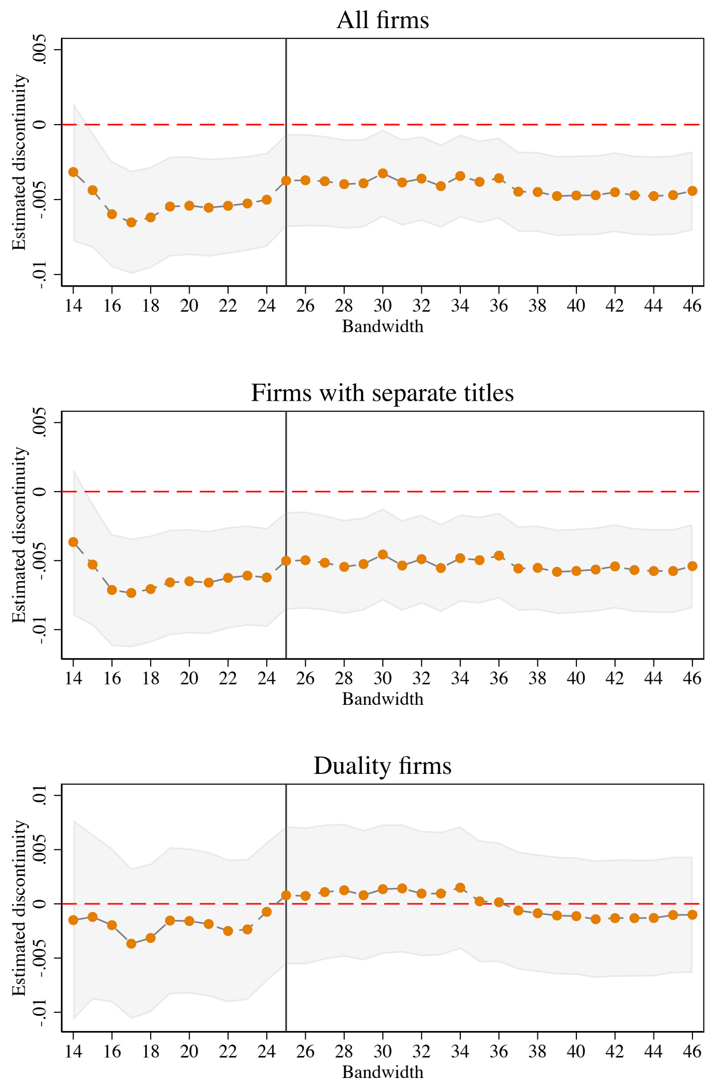

Figure B.1: Robustness of Difference-in-Discontinuities Estimates to Different Bandwidths

Notes: This figure plots difference-in-discontinuity estimates using different bandwidths. The vertical black lines indicate the bandwidth used in the main regressions. The shaded areas represent 90% confidence intervals.

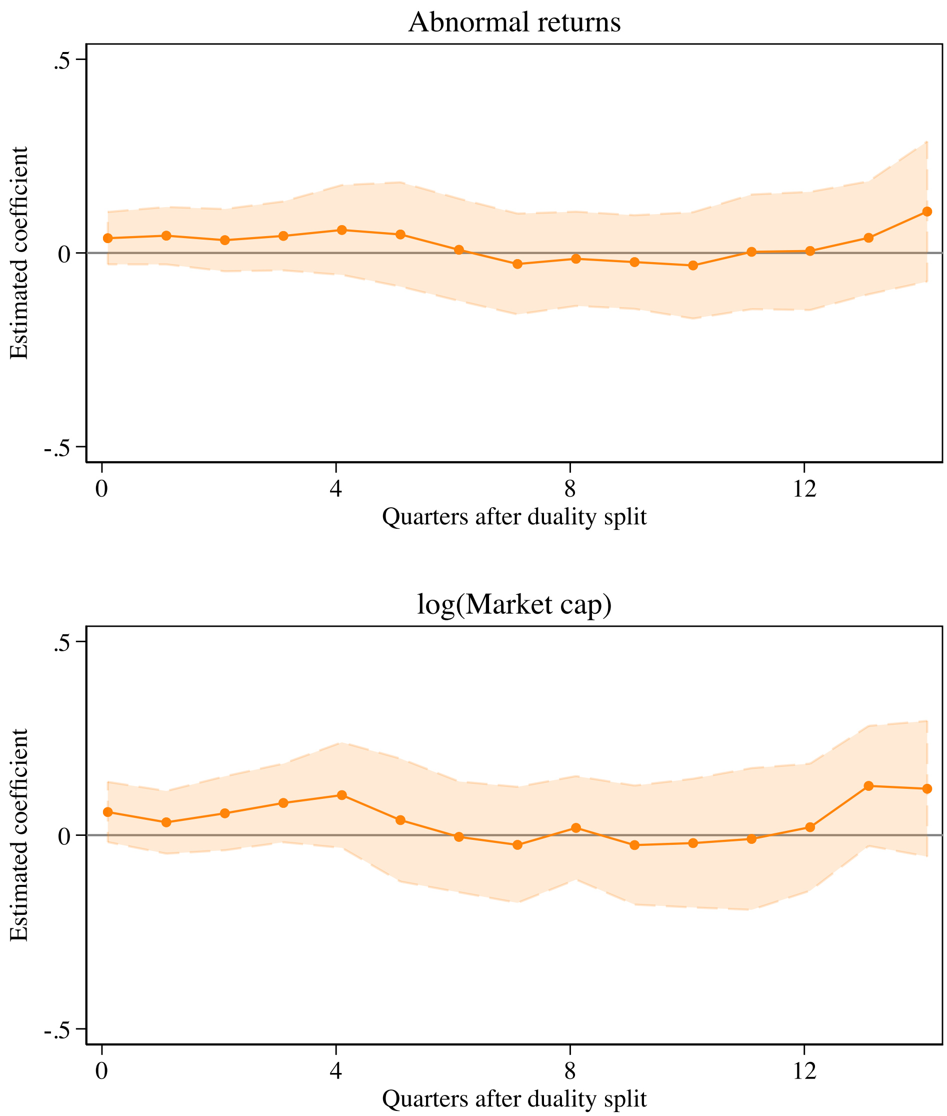

Figure B.2: Placebo Treatments

Notes: This figure plots the placebo point estimates from the dynamic event study model (as defined in Equation 4) for abnormal returns and market cap, obtained from the estimation method of Borusyak et al. (2024). Treatment is pretended to happen in *t*-8 quarters before the true treatment event. The shaded areas represent 90% confidence intervals.

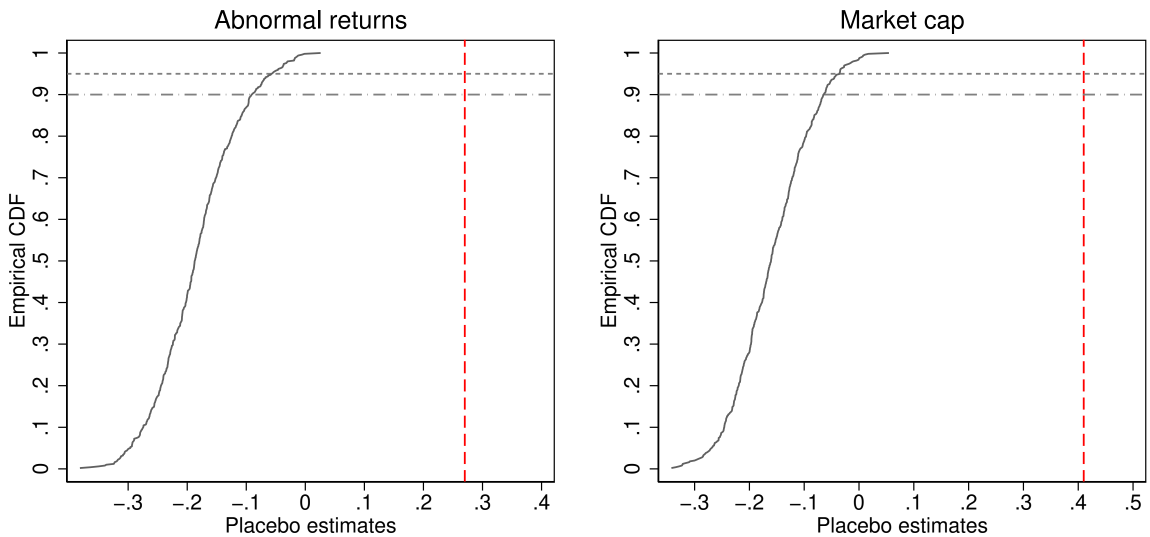

Figure B.3: Permutation Tests

Notes: This figure plots results from the permutation test with 500 replications. The procedure consists in randomly generating false treatment quarters for each firm and then running Equation 5. The curves represent the empirical cumulative distribution of the placebo effects. The vertical lines represent the real estimates obtained from the estimation method of de Chaisemartin and D'Haultfœuille (2020). The vertical axis denotes the estimated p-value.

Tables

Table B.1: Robustness of the Effects to Controlling for News Releases and Firm Reports

	Abnormal returns			log(Market cap)		
	(1)	(2)	(3)	(4)	(5)	(6)
Duality-split$_{ft}$	0.309**	0.303**	0.303**	0.434***	0.428***	0.427***
	(0.145)	(0.144)	(0.144)	(0.143)	(0.142)	(0.142)
Observations	4,147	4,147	4,147	4,147	4,147	4,147
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-time FE	Yes	Yes	Yes	Yes	Yes	Yes
Covariates						
News releases	Yes	No	No	Yes	No	No
Board meetings	No	Yes	No	No	Yes	No
Announcements to shareholders	No	No	Yes	No	No	Yes

Notes. This table reports difference-in-differences estimates of the impacts of CEO duality rupture on shareholder value (as displayed in Equation 5) obtained from the method of estimation of Borusyak et al. (2024). Each column shows results from a separate regression. Columns (1) and (4) control for the total number of news releases. Columns (2) and (5) control for the total number of board meetings. Columns (3) and (6) include the total number of announcements to shareholders. All specifications include firm and industry-period fixed effects. The sample includes firms listed in the NM and L2 special segments. Standard errors (in parentheses) are calculated with a industry-level cluster.

Table B.2: Robustness of Difference-in-Discontinuities Estimates to Alternate Samples

	All firms			Firms with separate titles		
	(1)	(2)	(3)	(4)	(5)	(6)
$Disclosure_t$	-0.003	-0.004*	-0.003	-0.004*	-0.005**	-0.004*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Observations	4,629	5,144	4,379	3,491	3,956	3,241
Polynomial	Linear	Linear	Linear	Linear	Linear	Linear
Weekday-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Excluding						
Segment L2	Yes	No	Yes	Yes	No	Yes
Basic materials sector	No	Yes	Yes	No	Yes	Yes

Notes. This table reports difference-in-discontinuity estimates for different samples. Results are shown pooling non-duality and duality firms (all firms), and non-duality companies only (firms with separate titles). Columns (1) and (5) include firms listed in the NM special segment only. Columns (2) and (6) use firms listed in NM and L2 segments but exclude companies operating in the basic materials sector. Columns (3) and (7) use firms in the NM segment only, but excluding firms from the basic materials sector. Columns (4) and (8) use a matched sample obtained from a propensity score matching (PSM). Variables used in the PSM include: total assets, total liabilities, year of foundation, operating segment, and number of shares. All specifications include weekday-month fixed effects and use a first-order polynomial of the running variable. Heteroskedasticity robust standard errors are in parentheses.

Table B.3: Robustness of Difference-in-Discontinuities Estimates to Alternate Controls

	All firms			Firms with separate titles			Duality firms		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$Disclosure_t$	-0.004**	-0.004**	-0.004**	-0.005**	-0.005**	-0.005**	0.001	0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)	(0.004)	(0.004)
Effect. observations	5,394	5,394	5,394	4,206	4,206	4,206	1,188	1,188	1,188
Polynomial	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear
Weekday-month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FE	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Weekday FE	No	Yes	No	No	Yes	No	No	Yes	No
Month FE	No	Yes	No	No	Yes	No	No	Yes	No
Covariates	No	No	Yes	No	No	Yes	No	No	Yes

Notes. This table reports difference-in-discontinuity estimates using alternate controls and the polynomial order of the running variable. Results are presented separately for all firms, non-duality companies (firms with separate titles), and for duality firms. Columns (1), (4), and (7) use a first-order polynomial and include interactions of industry and year fixed effects. Columns (2), (5), and (8) use second-order polynomials, and Columns (3), (6), and (9) add interactions of industry and year fixed effects to these models. All specifications include weekday-month, weekday, and month fixed effects. The sample includes firms listed in the NM and L2 special segments. Heteroskedasticity robust standard errors are in parentheses.

Table B.4: Robustness of Diff-in-Disc Estimates to Including Autoregressive Processes

	All firms			Firms with separate titles			Duality firms		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$Disclosure_t$	-0.003*	-0.003*	-0.003*	-0.004**	-0.004**	-0.004**	0.001	0.000	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)	(0.004)	(0.004)
ΔY_{t-1}	0.998***	0.956***	0.952***	0.998***	0.952***	0.947***	0.998***	0.976***	0.973***
	(0.001)	(0.017)	(0.017)	(0.001)	(0.019)	(0.019)	(0.003)	(0.035)	(0.035)
ΔY_{t-2}		0.042**	-0.033		0.046**	-0.032		0.022	-0.049
		(0.017)	(0.023)		(0.019)	(0.025)		(0.035)	(0.050)
ΔY_{t-3}			0.073***			0.073***			0.070
			(0.022)			(0.025)			(0.046)
ΔY_{t-4}			0.006			0.010			0.004
			(0.016)			(0.018)			(0.032)
Observations	5,394	5,392	5,386	4,206	4,205	4,201	1,188	1,187	1,185
Polynomial	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear
Weekday-month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes. This table reports difference-in-discontinuity estimates experimenting alternate autoregressive processes. Results are presented separately for all firms, non-duality companies (firms with separate titles), and for duality firms. All specifications include weekday-month fixed effects, firm fixed effects, and use a first-order polynomial of the running variable. The sample includes firms listed in the NM and L2 special segments. Heteroskedasticity robust standard errors are in parentheses.