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Revisiting board independence mandates: Evidence from director reclassifications

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Abstract

We provide causal evidence on the effects of mandated board independence. We compare firms that replace existing non-independent directors to firms that retain these directors by reclassifying them as independent. Reclassification eligibility, being largely predetermined, offers quasi-exogenous variation in compliance strategies. We show that firms required to replace insiders perform worse post-mandate, driven by increased operational costs and reduced labor efficiency. Boards of non-reclassifying firms retain fewer former employees and replace them with directors more likely to join monitoring-focused committees, emphasizing the shift from advising to monitoring. Overall, these findings suggest that firm-specific director expertise contributes materially to performance and is consistent with pre-mandate board compositions optimized to balance benefits of enhanced monitoring against costs of reduced advisory capacity. We rule out alternative explanations, including adjustment costs due to director turnover and co-option. Our study underscores the importance of allowing firms' flexibility in governance structures and cautions against uniform mandates.

Keywords: corporate governance; board of directors; independent boards; independence mandates.
JEL classifications: G30, G34, G38, K20.

1. Introduction

A central question in corporate governance is the role of board independence in balancing monitoring and advisory functions. Independent directors are often viewed as better monitors of CEO behavior, given their lack of company ties. However, their effectiveness as advisors may be limited by a lack of firm-specific knowledge (Raheja 2005; Adams and Ferreira 2007). This tradeoff suggests that governance structures evolve endogenously to balance these roles based on the firm's contracting environment.[1] In contrast, another view posits that governance structures are prone to capture by entrenched CEOs, reducing board oversight, enabling rent-seeking, and exacerbating agency problems.[2] Identifying exogenous variation in board independence to disentangle these views is an ongoing challenge for empirical work. Regulatory reforms mandating board independence provide a natural setting to study these effects.[3]

[1] See also, Hermalin and Weisbach (1988); Hermalin and Weisbach (1998); Hermalin and Weisbach (2003); and Adams, Hermalin, and Weisbach (2010).
[2] E.g., Bebchuk and Fried (2003) and Coles, Daniel, and Naveen (2014).
[3] E.g., Cadbury commission reforms in the UK (see Dahya, McConnell, and Travlos 2002; Dahya and McConnell 2007) and studies related to the passage of SOX (see, e.g., Chhaochharia and Grinstein 2007; Duchin, Matsusaka, and Ozbas 2010).

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This article develops a novel approach to measure the impact of the 2002 NYSE and NASDAQ board independence mandates, which required a majority of independent directors and fully independent key committees.[4] We exploit specific provisions of the exchange mandates defining board independence, which allowed firms to comply in one of two ways: (1) by replacing non-independent directors with new independent directors or (2) by "reclassifying" certain non-independent directors as independent if they met specific criteria. These reclassifications, often based on actions predating the mandates, created a quasi-random assignment of compliance strategies among non-compliant firms, categorized as reclassifiers or non-reclassifiers. This unique setting allows us to isolate the consequences of mandated board independence on firm performance, addressing the endogeneity of director selection. To evaluate the implications of these mandates, we employ a triple-difference (DDD) methodology that compares outcomes for reclassifying and non-reclassifying non-compliant firms, using compliant firms as a control group. This approach goes beyond the standard comparison of non-compliant versus compliant firms in the literature by exploiting quasi-exogenous variation within non-compliant firms to mitigate selection concerns.[5]

Our key insight is that reclassifications provide an opportunity to test competing views of board structure: (1) the optimal tradeoff view and (2) the entrenchment view. If pre-mandate board compositions were optimal, reflecting a tradeoff between directors that specialize in monitoring and advising—with non-independent directors chosen for their unique skills and firm-specific knowledge to maximize shareholder value—then non-reclassifying firms, unable to retain these directors, should underperform post-mandate. This underperformance would stem from the greater extent to which they need to reconfigure their optimized pre-mandate boards.[6] Conversely, if pre-mandate boards were suboptimal, captured by entrenched CEOs, then reclassifying firms, which retained these directors, should exhibit relatively worse performance post-mandate.

Our main tests provide statistically significant evidence that non-reclassifiers underperformed similar non-compliant firms that met the independent board mandate by reclassifying directors. The magnitude of these differences is economically meaningful: we estimate that profitability (ROA) of non-reclassifiers declined by 2.7 percentage points relative to comparable reclassifiers following the mandate. This result highlights the tangible costs associated with the loss of insider knowledge and relationships within non-reclassifying boards. These findings are robust to the inclusion of fixed effects that absorb industry–year shocks to profitability, additional time-varying controls for board turnover, and other determinants of ROA. We obtain similar results when using profit margin as the outcome variable. Overall, our findings suggest that compliance with the mandate impedes the performance of firms targeted by the regulations, particularly for those unable to leverage the flexibility provided by reclassification.

Decomposing the DDD test into its [AQ]difference-in-differences (DD) sub-components reveals that the relative underperformance stems from a decline in profitability among non-compliant non-reclassifying firms. In contrast, no measurable effect of the mandate is observed within the subsample of reclassifying firms, where non-compliant firms retained insider directors. These findings align with the view that pre-mandate boards were on

[4] The committees are the audit, compensation, nominating, and governance committees. We provide details in Section 2 and Online Appendix 0A.1.

[5] Furthermore, because all firms are impacted by the mandate simultaneously, our tests are not subject to issues highlighted by recent advances in econometrics examining difference-in-difference designs with heterogeneous treatment timing. See, e.g., Baker, Larcker, and Wang (2022); Sun and Abraham (2021); and Roth et al. (2023).

[6] Indeed, Chhaochharia and Grinstein (2009) find that 13 percent of firms complied exclusively through reclassification. While director reclassifications have been noted in academic work before, for example in Chhaochharia and Grinstein (2009); Duchin, Matsusaka, and Ozbas (2010); and Guthrie, Sokolowsky, and Wan (2012a), we are the first to exploit them to identify causal estimates of the impact of independence mandates.

average optimally composed within our sample.[7] Additional tests restricted to non-compliant firms further confirm that the underperformance is concentrated among non-reclassifiers, underscoring that the observed differences are not driven by changes within the compliant group.

We conduct four tests to validate our main findings. First, we confirm that reclassification is largely predetermined by events preceding the mandates.[8] Second, we verify the parallel trends assumption, showing balanced pre-treatment levels and growth rates of the outcome variable, with dynamic estimates revealing no pre-mandate performance differences but a growing gap post-2003. Third, recognizing that prior studies in the field, such as Chhaochharia and Grinstein (2007) and Guthrie, Sokolowsky, and Wan (2012b), were found to be sensitive to outliers, we repeat our main tests 10,000 times, each time dropping 1 percent of sample firms. These tests confirm that outliers do not drive our results. Finally, falsification tests assuming the mandate's treatment began in 2001 show no differential effect, demonstrating that the observed impact is concentrated post-rule change.

The decline in performance at non-reclassifying firms is primarily driven by a reduction in labor efficiency relative to reclassifying firms. We document this through statistically and economically significant relative increases in SG&A expenses, as well as decreases in sales and profits per employee for non-reclassifying firms. Additionally, these firms show a reduced focus on process innovation, as reflected in fewer mentions of process optimization efforts within their 10-K filings. However, sales growth is statistically marginally higher for non-reclassifying firms, suggesting that part of the relative decline in operational performance may stem from a strategic shift in corporate priorities. Furthermore, we find limited evidence that differences in working capital management contribute to the observed performance disparities, and we show that the results are not driven by inferior M&A decisions or changes in risk-taking behavior.

Our examination of director-level characteristics highlights how flexibility in board composition might play a role in the labor efficiency channel documented above. Non-reclassifying firms complied with the mandate by reducing their representation of non-independent directors, particularly former employees with deep firm-specific knowledge.[9] Additionally, newly appointed directors were 20 percent more likely to serve on monitoring-focused committees, such as the audit committee, emphasizing monitoring over advisory functions. The reduced flexibility in board appointments at non-reclassifying firms also constrained their pool of potential directors.[10] Comparing reclassified directors to others, we find they hold significantly more equity, have longer board tenure, and possess more extensive firm employment experience, underscoring their substantial firm-specific knowledge and long-term alignment. These shifts in director roles and characteristics are likely contributing factors for the observed declines in labor efficiency at non-reclassifying firms, highlighting the costs imposed by governance reforms, particularly for firms unable to leverage the flexibility afforded by reclassification.

[7] While it would be interesting to explore heterogeneity for the main estimates—e.g., to understand which kinds of firms might have benefited from the mandate—the sample size does not allow for well-powered tests on a fourth difference.

[8] Section 3 highlights the predetermined nature of the ability to reclassify. In most cases, it relates to the decision of a director to retire as an employee prior to the mandates came into effect. Section 2.2 discusses how endogeneity in reclassification eligibility would impact our results.

[9] Our results provide meaningful, albeit indirect, evidence regarding the role of these directors. Directly demonstrating this effect is challenging due to the opaque nature of board decision-making processes. However, the influence of employee directors has been previously documented in European contexts (see Ginglinger, Megginson, and Waxin 2011).

[10] Linck, Netter, and Yang (2009) show that in the context of the passage of SOX reforms, the supply of independent directors did not match the increased demand. Knyazeva, Knyazeva, and Masulis (2013) argue that the depth of the pool of candidates for directorship in the geographic proximity of the firm affects the quality of their board due to search costs. Indeed, in the post-SOX era, firms hired independent board members from further away and Alam et al. (2018) show that this is associated with a drop in financial reporting quality.

We examine several alternative mechanisms that might explain our core results but find no evidence to support them. One possibility is that increased director turnover at non-reclassifying non-compliant firms leads to adjustment costs, such as learning frictions, where new directors require time to acquire firm-specific knowledge. However, our results remain unchanged when we directly control for board turnover. Furthermore, we find that both types of firms add new directors at similar rates, suggesting comparable adjustment costs. These findings highlight that the key distinction between the two compliance pathways central to our empirical design is not the amount of turnover, but rather the types of directors who exit, remain, or join the board. We also consider whether non-reclassifying boards become more co-opted (e.g., Coles, Daniel, and Naveen 2014). However, we find no evidence of differentially higher board appointments or increases in CEO pay, which is a primary prediction of the co-option hypothesis.

Finally, we investigate whether the performance benefits of reclassification were reflected in market valuations. Using the methodology of Chhaochharia and Grinstein (2007), we construct portfolios corresponding to the four groups[11] in our DDD test and evaluate their performance relative to a four-factor model over the period from November 1, 2001, to October 31, 2002, which captures key dates during the development of the new exchange rules. Our analysis reveals no statistically significant difference in performance between the portfolios of reclassifying and non-reclassifying firms. This finding suggests that the market may not have fully anticipated the operational advantages of reclassification or that these benefits were offset by the relatively lower sales growth exhibited by reclassifying firms in the post-mandate period. We revisit this result later in the article and highlight the econometric challenges that complicate its interpretation.

We make three contributions to the literature. First, we contribute to the broader debate on whether board independence, and board composition more generally, should be viewed as optimized, "window dressing," or "entrenched," as described by Duchin, Matsusaka, and Ozbas (2010). Our study provides new quasi-experimental evidence that directly addresses these perspectives. The findings suggest that pre-mandate boards effectively balanced the monitoring-advising tradeoff, aligning with the optimization view. This conclusion challenges the assumption that inside directors inherently reflect or exacerbate agency problems, showing instead that firms maintaining a composition closer to their pre-mandate structure outperformed those that made more significant changes for compliance purposes.[12]

Second, we add to the debate on the efficacy of governance mandates—particularly those emphasizing board independence—as tools to improve firm outcomes. While prior work finds mixed valuation effects for firms impacted by independence requirements and the contemporaneous SOX reforms (e.g., Chhaochharia and Grinstein 2007; Litvak 2007; Zhang 2007), evidence on real effects remains limited. Duchin, Matsusaka, and Ozbas (2010) use an IV strategy based on compliance and find that the mandates' performance effects depend on the cost of acquiring firm-specific knowledge needed for decision-making. Our study extends their work by leveraging variation in compliance strategies to provide cleaner identification. The results reinforce the notion that even well-intentioned standards can impose costs when they disregard firms' pre-existing governance arrangements.[13]

[11] Non-compliant reclassifiers and non-reclassifiers, as well as compliant reclassifiers and non-reclassifiers.

[12] To be clear, our findings do not imply that boards cannot be captured by CEOs or serve as irrelevant "window dressing" at times.

[13] There is a significant parallel debate on board mandates related to gender quotas. Greene, Intintoli, and Kahle (2020) and Hwang, Shivdasani, and Simintzi (2021) examine gender mandates in California and find negative effects, attributed to restrictions in the supply of directors. Similarly, Ahern and Dittmar (2012) report negative effects of gender representation quotas in Norway. However, Eckbo, Nygaard, and Thorburn (2022) challenge these findings, arguing that such quotas increased the supply of qualified directors on certain measurable dimensions. Ferrari et al. (2022) support this view in their analysis of an Italian mandate. In our setting—

Third, our article contributes to ongoing policy and practitioner debates about board composition and the value of insiders. Governance advisors frequently recommend maximizing independence to enhance oversight and public trust, and policymakers have adopted similar logic in crafting governance standards. These trends have materially reshaped boards over the past 75 years: The proportion of independent directors at large US companies grew from 20 percent in the 1950s, to 35 percent in 1980, reached 70 percent by 2000, and now stands at approximately 85 percent.[14] However, our study links changes in board composition to operational efficiency and labor productivity, emphasizing that insiders can provide strategic and informational benefits that enhance firm performance. This evidence cautions policymakers and governance advisors against universal prescriptions for "good governance" and broadens the conversation to encourage more flexible, context-sensitive approaches to board design.

2. Methodology

This section provides background on the exchange mandates that provide identification for our tests before introducing and discussing the main specification in the article.

2.1 Exchange mandate background

Following the corporate scandals that preceded Sarbanes-Oxley (SOX), the NYSE and NASDAQ each proposed and passed a mandate in the fall of 2002 requiring that the board of directors of listed firms be comprised of a majority of "independent" directors. Additionally, the mandates require fully independent audit, compensation, nominating, and governance committees.[15] The goal of the mandates was to encourage the creation of boards that were more effective in monitoring executives.

According to the NYSE, a director is considered independent if two criteria are satisfied. First, the board must affirm that no material relationship exists between the firm and the board member.[16] Second, in the past three years, neither the director nor any immediate family member has been an employee of the firm or its auditor(s), received more than $100,000 outside of compensation for the directorship or prior services, been in an interlocked board with an executive, or had outside employment with a firm that did business with the firm of more than one million dollars or 2 percent of the other company's gross revenues.[17] This second provision, which we will call the "lookback provision," is important for interpreting our tests as exogenous, as will be discussed in greater detail below.

The most common approach to comply with the mandate is to replace non-independent directors with newly appointed independent directors. However, the ability to reclassify a director from non-independent to independent allows a board to become more compliant with the mandate requirements *without* changing board membership. For example, suppose the director also served as a consultant to the firm and received compensation of $150,000 for this work. According to NYSE Rule 303A.02 (see Supplementary Appendix OA.1), this would be considered a material relationship. For this director to be reclassified as independent, the consulting relationship would need to end and 3 years to pass.

Reclassifications are prevalent in the data—35 percent of firms in our sample reclassify at least one director as independent after 2001. Moreover, these reclassifications are plainly visible in DEF-14A filings that firms submit to the SEC. For example, John W. Murrey III stopped

large US firms in the early 2000s—Linck, Netter, and Yang (2009) document a reduction in the supply of directors due to changes introduced by SOX and exchange mandates.

[14] These figures are pulled from Gordon (2006) and research reports from Spencer Stuart.

[15] Chhaochharia and Grinstein (2007) present a thorough and concise summary of the events that precipitated SOX and the exchange mandates.

[16] A partial list of such relationships: commercial, industrial, banking, consulting, legal, accounting, charitable, and familial.

[17] Supplementary Appendix contains the full NYSE rule. The NASDAQ rule is similar.

providing legal services to Dixie Group in 2000 and the 2003 DEF-14A filing listed him among the independent directors. In 2005, Emmis Communications Corp. added Greg Nathanson to its roster of independent directors "because on that date three years will have elapsed since he was employed by Emmis." The next year, Emmis reported another reclassification, noting that "We expect Mr. Leventhal to qualify as an independent director on June 25, 2005, because on that date three years will have elapsed since Mr. Leventhal's brother-in-law ceased to be one of Emmis' executive officers."

2.2 Methodology

The idealized test of the effect of board independence on performance would randomly treat a subset of firms with an increase in board independence by changing members of their board. The randomly untreated firms whose board independence is unchanged would then be the control group, and a simple comparison of how firm performance changed after the mandate would produce an estimate of the effect of board independence.

Our empirical design allows us to get close to this idealized test by exploiting the fact that reclassification of directors is quasi-exogenous. As described in the previous section, the board mandates are written such that the ability to reclassify a director as independent is mostly predetermined. Namely, the 3-year lookback provision allows some firms that took certain actions *before* the rules were proposed to keep an inside director on the board by reclassifying them as independent *after* the rules were implemented.[18]

The intuition behind the test is as follows: Imagine two otherwise identical firms, each with three independent directors ("I") and five non-independent directors ("N"). Both receive a mandate from the exchange requiring that at least half of the board consist of independent directors. Assume the optimal board size for these firms is eight. Consequently, neither firm complies with the exchange's listing mandate in its current state.

To achieve compliance, the first firm removes an N director and appoints a new I director, increasing the independence of the board. In contrast, the second firm complies by reclassifying one of its N directors as an I director, leaving the board's actual composition unchanged. While both firms now meet the exchange mandate, only the first firm has made a substantive change to its board composition.

The relative change in performance between the two firms depends on the rationale behind their original $3-I/5-N$ composition and the relative value of the reclassified N director at the second firm versus the newly hired I director at the first firm. If we assume that boards were optimally composed prior to the mandate, the first firm's performance would likely decline as it is required to deviate from its previous equilibrium by replacing an N director with an I director.

There is a second key distinction between these two firms that could drive differences in performance: the newly appointed director at the first firm may require time to familiarize themselves with the firm. We address this alternative explanation in two ways in our empirical analysis. First, we show that director turnover rates in the data are very similar across the two types of firms. Second, we directly control for director turnover in our empirical specifications.

To implement our DDD model, we need to define three variables. First, we divide firms into *NonCompliant* and *Compliant*. Non-compliant firms ($NC_i = 1$) are those whose board in 2001 did not comply with the exchange mandates. Specifically, non-compliance means that a firm has less than 50 percent independent directors or any of its committees is not fully independent. Second, we divide all firms into *Reclassifiers* and *Non-Reclassifiers*. Reclassifiers ($R_i = 1$) are firms that have at least one director reclassified from non-independent to independent after 2001. For reclassifiers, the number of board changes required to comply with the mandate is either eliminated or reduced substantially. In contrast, non-reclassifiers that have not complied with the mandate must either appoint independent directors and/or remove

[18] Section 3 provides more details on the predetermined nature of reclassification in our sample. We also argue later and find supporting evidence that endogenous selection into reclassification does not drive our results.

insider directors to comply. Third, because the mandates go into effect in the latter half of 2002, we set 2003 as the year in which the mandates' constraints begin, and thus P_t is equal to one beginning in 2003 and zero otherwise.

We use these variables to estimate the parameter of interest empirically by specifying the following DDD model:

$$y_{i,j,t} = \beta_1 P_t \cdot R_i \cdot NC_i + \beta_2 P_t \cdot R_i + \beta_3 P_t \cdot NC_i + \phi_i + \phi_{j,t} + u_{i,j,t} \quad (1)$$

where the unit of observation is a firm–year (i, t)). The outcome(s) of interest are performance metrics ROA and Profit Margin.

We include fixed effects for firms (ϕ_i) and year-by-industry ($\phi_{j,t}$, where industry j is defined as SIC2 classifications).[19] By incorporating year-by-industry fixed effects, we effectively control for potential common shocks at the year–industry level.

Standard errors are clustered at the firm level to account for within-firm serial correlation. While double clustering on firm and time dimensions is theoretically appealing, it is less suitable for our dataset due to both practical and statistical considerations.[20] As a robustness check, we re-estimated our main specifications with double clustering, and the results were consistent with our main findings. These robustness tests are available upon request.

In a model with two time periods, one can show that

$$\beta_1 = E[(\Delta y_{NC=1,R=1} - \Delta y_{NC=0,R=1}) - (\Delta y_{NC=1,R=0} - \Delta y_{NC=0,R=0})] \quad (2)$$

where Δ denotes a time difference (see Olden and Møen 2022). This expression is useful for two reasons. First, it clearly expresses the DDD estimator as the difference between two DD estimators and builds intuition for our test design. In particular, our empirical approach is similar to separately estimating a counterfactual performance for each subgroup (reclassifiers and non-reclassifiers) of non-compliant firms via DD. The counterfactual firms come from the pool of compliant firms, and we then take the difference of the resulting estimates.[21] That is, β_1 produces an estimate of the effect of the rule on reclassifying non-compliant firms [the first term in equation (2)] relative to non-reclassifying non-compliant firms [the second term in equation (2)]. A positive estimate of β_1 would indicate that reclassifying firms outperformed non-reclassifying firms following the passage of the rules. Conversely, a negative estimate of β_1 would indicate that the performance of firms was improved by replacing insider directors with outside and independent board members. This approach is precisely what is required to map the idealized test to empirical data.

Second, equation (2) highlights the identifying assumption behind the test. While the DDD test is akin to the difference of two DD estimates, our empirical design only needs one—not two—parallel trend assumption to hold: The relative performance of reclassifiers and non-reclassifiers within the subset of non-compliant firms must trend the same as the relative performance of reclassifiers and non-reclassifiers among compliant firms.[22] We

[19] The fixed effects absorb P_t, R_i, and NC_i and unlisted interaction terms.

[20] First, the inclusion of year-by-industry fixed effects in our model absorbs common shocks along the time dimension, as highlighted by Abadie et al. (2023). This approach reduces the need for additional clustering along the time axis, consistent with Hansen (2007), who notes that clustering is unnecessary when fixed effects capture group-level shocks. Second, the small time-series dimension in our dataset (T = 7) introduces severe limitations for double clustering, as demonstrated by recent research. Monte Carlo simulations, such as those conducted by Chiang, Hansen, and Sasaki (2024), reveal that double clustering with small T results in underestimated standard errors and undercoverage of confidence intervals, leading to unreliable inference. Similar challenges have been observed in earlier work, including Thompson (2011). For these reasons, we opted to use firm-level clustering, which aligns with best practices for datasets with limited time observations.

[21] Indeed, the difference-in-difference results of Table 5 recover the DDD coefficient in Table 3.

[22] Because equation 2 can be rearranged by swapping the roles of R and NC, there is an additional parallel trend assumption that would identify our parameter. The relative performance of *non-compliant and compliant firms* within the subset of reclassifying firms must trend the same as the relative performance of *non-compliant and compliant firms* among non-reclassifying firms.

present information on pre-treatment balance across both R_i and NC_i in Section 3.4 to explore this assumption in our setting and assess the case for including controls in our baseline tests.[23] Additionally, we will present event-study plots of our main tests, following the recommendations in Roth et al. (2023).

While reclassification eligibility is mostly predetermined, it may not be exogenous. In particular, firms that value insiders on the board might be more likely to have directors that can be reclassified. This type of sample selection would lead to specific and testable outcomes. First, the treatment effect on reclassifying firms [the first term of equation (2)] would be zero if they could retain valuable insiders on the board, as doing so would reduce any detrimental impact of the mandate. Second, this form of endogeneity would imply that firms that do not reclassify directors—but instead replace their insiders on the board—do so because these insiders are of low value to them. Thus, there would be no treatment effect of the mandate on non-reclassifying firms [the second term of equation (2)]. Third, combining these two statements, β_1 would be negative. However, we find direct evidence against each of the three in Section 4.1. We conclude that endogenous assignment in reclassification eligibility is not driving our results.

3. Data

This section discusses the data underlying the main tests. We first discuss the method used to classify which director–years are independent. This crucial step allows us to measure the variables in our main specification. We then outline the sample construction and describe the sample. Finally, we assess the pre-mandate balance between the groups of firms in our baseline models.

3.1 Director-level data

We obtain data on board members from 1999 to 2006 from Institutional Shareholder Services (ISS). The ISS Directors Legacy database contains information about directors of approximately 1,500 of the largest firms in each year.

Our goal is to define which directors are independent in a way that matches the *exchanges*' definitions. This process is described in full in the Supplementary Appendix OA.2, and we outline it here. We start with ISS's classification of directors as independent, affiliated, or employee. However, ISS uses a more stringent definition of director independence than the exchanges. Thus, we use adjustments from Guthrie, Sokolowsky, and Wan (2012a) to approximate the rules of the exchanges as closely as possible, given the data available from ISS.[24]

Supplementary Table OA-1 shows a breakdown of how many firms are *NonCompliant* and/or *Reclassifying*. Across all firms in the ISS data where we can define both variables, 442 firms do not comply with the mandate, 423 firms reclassify at least one director after the rules are announced, and 203 non-compliant firms use reclassification. After applying the sample filters used in our main test (and described in the next section), those numbers are 199, 196, and 96, respectively.[25]

Finally, to assess the predetermined nature of reclassification, we analyze in more detail the ninety-four non-compliant reclassifying firms. For this subset of firms, we find 128 directors that were reclassified as independent during the post-mandate period (2003–2006). Notably, 70 percent (90/128) of those directors are reclassified based on decisions taken prior to the rule proposal. The retirement of director employees is the dominant reason for reclassification—it

[23] Olden and Møen (2022) clarify that under this parallel trend assumption, β_1 identifies the average treatment effect on the treated.

[24] The Supplementary Appendix describes additional manual checks we conduct to verify the data on director independence and reclassification.

[25] Approximately 10 percent of firm–years reclassify a director as newly independent and 35 percent of firms reclassify at least one director during the post-mandate period.

accounts for more than half of reclassified directors and 80 percent retire during the pre-mandate period (02 or before).

3.2 Firm–year sample

The main sample comprises firm–years in the CRSP-Compustat Merged (CCM) database from 1999 to 2006. Table 1 provides details on the order and impact of our sample filters. First, we drop observations where total nominal assets or sales are less than five million, and observations where total debt (DLC plus DLTT) exceeds total assets (AT). Second, the firm–year must be listed on the NYSE or NASDAQ exchange, which we obtain using the monthly CRSP stock file. Third, we require firms to have at least three non-missing observations of ROA in each of the pre- and post-periods. Fourth, we drop firms classified as utilities or financial entities (SIC codes 4900–4949 and 6000–6999). Fifth, we require the firm to have defined values for NC_i and R_i. This requirement means that the firm must be in the ISS database in at least 2001 (to define NC_i) and 2002 (to define R_i). Finally, we limit the sample to firms near the 50 percent independence cutoff in 2001 by keeping firms that could cross the threshold with less than three director replacements.[26] After these filters, our final sample contains 4,387 firm years, of which 4,337 make it into our baseline specifications due to the inclusion of fixed effects.

Table 2 presents the definitions and summary statistics for the firm–year sample used in the baseline models of Table 4.[27] All variables are winsorized at the 1 percent tails to mitigate the influence of outliers. Firm performance is assessed using ROA and Profit Margin, both of which have average and median values of 14 percent and exhibit slight right skewness. Since the ISS dataset focuses on S&P 1500 firms, the reported statistics are consistent with those observed in other finance studies of large firms. The log of nominal assets is 6.92 for the average firm–year. Book leverage is 19 percent, and approximately 6 percent and 4 percent of assets are allocated to CAPX and R&D, respectively. *NonCompliant* firms account for 36 percent of the firm–years in the sample, while *Reclassifiers* also represent

Table 1. Sample selection criteria.

This table reports the order and sample selection criteria used throughout the article. We begin with yearly observations (1999–2006) from the CRSP-Compustat Merged (CCM) database. Assets and sales used in step 2 are nominal. We obtain the historical exchange from the monthly CRSP stock file. The SIC code, from CCM, is *SICH* where available and *SIC* otherwise. Section 2.2 provides more details on the definition of firms as reclassifiers (R_i) and/or non-compliant (NC_i).

	Step	*N*	Number of firms
1	CRSP-Compustat Merged (CCM) fiscal year 1999–2006	55,577	10,774
2	Assets and Sales $\geq$ \$5m, book leverage ≤ 1	51,680	10,148
3	NYSE and NASDAQ listed firm–years	46,602	9,248
4	Firm has at least 3 non-missing ROA in pre and post	27,863	3,574
5	Drop SIC 49 and 60-69	21,581	2,791
6	In ISS database and R_i and NC_i defined	7,205	912
7	Less than 3 director replacements from 50% board independence in 2001	4,387	556
8	In baseline regression (due to fixed effects)	4,337	551

[26] Specifically, we require $|N - I|/2 < 3$ in 2001. For example, a firm with three independent directors (I) and seven non-independent directors (N) could replace two of the N directors with new I directors and has a board that is now majority independent.

[27] We manually collect data on 392 firm–years where lagged board controls are not available.

Table 2. Summary statistics for main regression sample.
This table reports summary statistics for the main regression sample in Table 4. The sample comprises firm–year observations from 1999 to 2006. See Table 1 for more details on the sample. Column 1 names the variable, column 2 defines the variable, and column 3 lists the data source. All variables are winsorized at the 1 percent tails. Section 2.2 provides more details on the definition of NC_i (*NonCompliant*) and R_i (*Reclassifier*).

Variable	Definition	N	Mean	SD	5th perc.	Median	95th perc.
Outcome variables (reported in *t*)							
ROA_t	oibdp/at	4,337	0.14	0.10	−0.01	0.14	0.30
Profit Margin	oibdp/sale	4,337	0.14	0.18	−0.01	0.14	0.40
Lagged control variables (reported in $t-1$)							
NC_i	Section 2.2	4,337	0.36	0.48	0.00	0.00	1.00
R_i	Section 2.2	4,337	0.36	0.48	0.00	0.00	1.00
Log(Assets)	Log(at)	4,291	6.92	1.27	5.01	6.78	9.25
Book Leverage	(dlc+dltt)/at	4,291	0.19	0.17	0.00	0.18	0.50
CAPX/Assets	capx/at	4,258	0.06	0.05	0.01	0.04	0.17
R&D/Assets	xrd/at	4,291	0.04	0.06	0.00	0.00	0.16
Board Size	No. of directors	4,286	8.11	2.29	5.00	8.00	12.00
Board Independence	% of directors classified as independent per exchange definition	4,286	0.70	0.14	0.43	0.71	0.88
Incoming Directors	Incoming/Board Size	4,286	0.09	0.12	0.00	0.00	0.33
Outgoing Directors	Outgoing/Board Size	4,286	0.09	0.12	0.00	0.00	0.33

36 percent of observations.[28] Over the full sample, boards have an average size of 8.11 directors, 70 percent of whom are independent as defined by exchange standards. This average masks a significant evolution in board independence over the sample period, which is described at length in the next subsection. Finally, the ratio of incoming directors, as well as outgoing directors, to board size is 9 percent on average. However, the medians for both ratios are zero, indicating that in a given year the median board does not experience any turnover.

3.3 Evolution of boards

To provide important context for our tests, figure 1 illustrates how boards in our sample evolved during the sample period for the different groups in our DDD design. The red lines highlight the non-compliant firms, offering a visual distinction that is central to our analysis.

Panel (1) shows that board size remains relatively stable across all groups. This indicates that compliance with regulatory mandates was achieved primarily through director replacement rather than through significant additions of independent directors or removals of non-independent ones.

Panels (2) and (3) focus on director independence, highlighting a notable distinction between two definitions of independence: those set by the exchanges and those set by ISS, a prominent corporate governance advisory firm. Panel (2) shows that reclassification allowed non-compliant firms to rapidly increase their independence, as defined by the exchanges, following the introduction of mandates.

In contrast, panel (3), which applies ISS's more stringent criteria (e.g., not classifying former employees as independent), reveals that reclassifying firms consistently exhibit the lowest ISS independence ratios throughout the sample period. This emphasizes that

[28] Supplementary Appendix Table OA-2 shows that firms reclassify a non-independent director as newly independent in approximately 10 percent of firm–year observations.

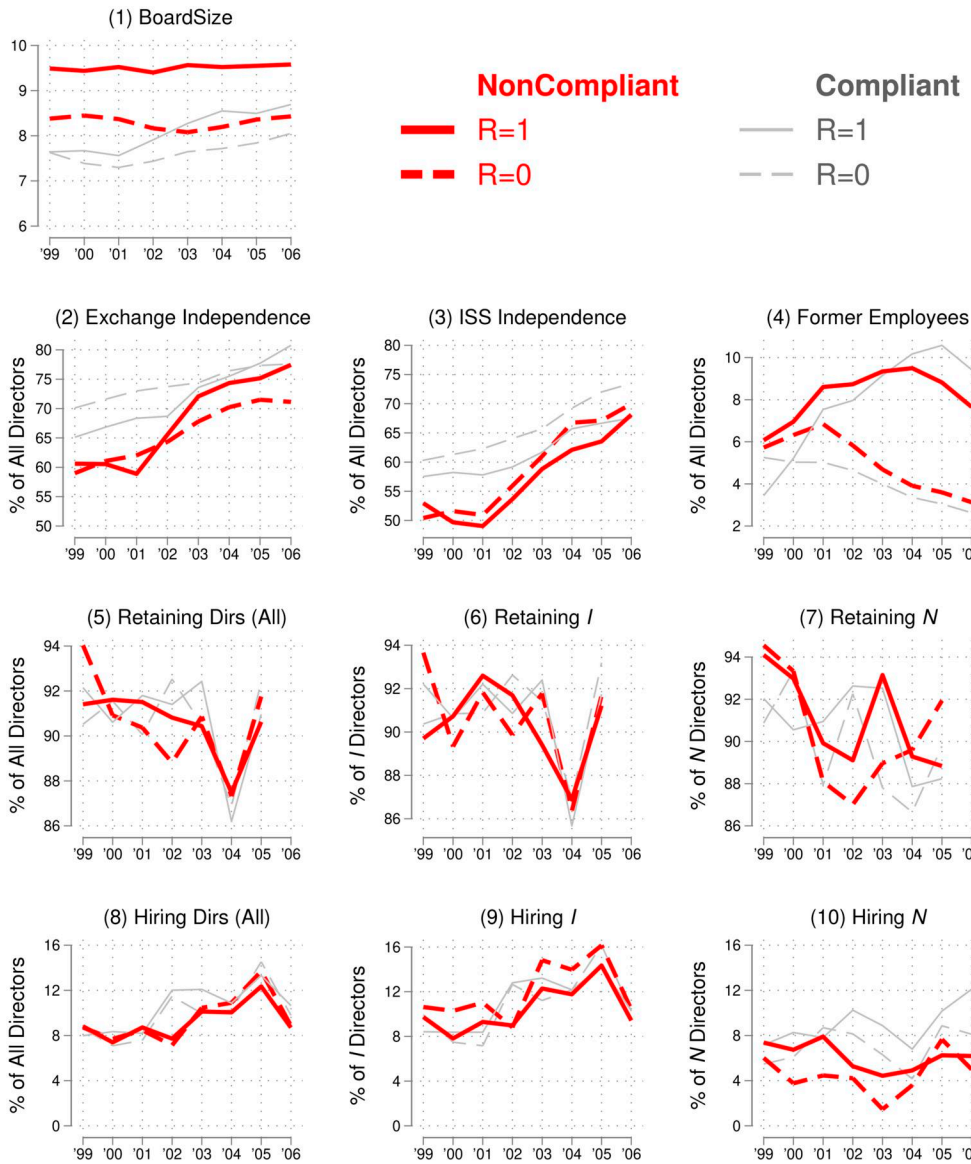

Figure 1. Evolution of boards.

This figure illustrates how the average board composition evolves over the sample period from 1999 to 2006. The data include all directors in our sample (see Table 1 for more details). The sample is divided into four groups of firms: compliant reclassifiers, compliant non-reclassifiers, non-compliant reclassifiers, and non-compliant non-reclassifiers, a subdivision used consistently throughout the article. *N* refers to directors classified as non-independent, while *I* refers to directors classified as independent. Directors are classified as *I* or *N* based on criteria that align as closely as possible with the rules of the exchanges. See Section 3.1 and Supplementary Appendix OA.2 for additional details. In Panel (3), we show director independence as defined by ISS, which applies more stringent criteria than the exchanges' definitions. Unless otherwise specified, "Board Independence" throughout the article refers to classifications based on exchange rules.

reclassifying firms tend to have directors with prior or existing ties to the firm, such as employment, consulting, or other relationships.

To illustrate this point, panel (4) focuses on former employees, showing that reclassifying firms are significantly more likely to have former employees on their boards over time compared to non-reclassifying firms. These differences became statistically significant after 2002, coinciding with the implementation of the mandates that allowed for the reclassification of certain directors.[29]

The panels in the bottom two rows provide insights into director turnover and appointments. While a single reclassification could bring many non-compliant firms into compliance without further compositional changes, turnover patterns suggest a more nuanced dynamic. Indeed, panel (5) (director retention) and panel (8) (director appointment) reveal overall director turnover rates that are nearly identical across reclassifying and non-reclassifying firms. However, the types of directors appointed differ significantly. Non-reclassifying firms focus on retaining and appointing independent directors, as defined by the exchanges, as shown in panels (6) and (9). In contrast, reclassifying firms tend to retain and appoint relatively more non-independent directors, often including former employees, as shown in panels (7) and (10).

Despite similar levels of director turnover and appointment rates, reclassifying firms' boards increasingly includes more former employees and a higher proportion of directors whom ISS does not classify as independent in the latter half of the sample period. These compositional differences are key to understanding the mechanisms underlying our main results.

3.4 Pre-mandate balance

We conduct several tests to assess the validity of the parallel trends assumption and determine whether the inclusion of additional controls in our main specification is warranted.[30] In this section, we test for balance in both levels and growth rates for the key outcome variables and potential controls during the pre-treatment period (1999–2002). We run OLS regressions for a given covariate $y_{i,t}$, across dimensions $D_i \in \{NC_i, R_i\}$, using both level and growth rate specifications $F(\cdot) \in \{y_{i,j,t}, \frac{y_{i,j,t} - y_{i,j,t-1}}{y_{i,j,t-1}}\}$:

$$F(y_{i,j,t}) = \alpha + \beta D_i + \phi_j + \varepsilon_{i,j,t} \tag{3}$$

where firm i is in industry j and ϕ_j is SIC3 fixed effects.

Table 3 presents β from each of these tests. The first two columns compare *NonCompliant* firms to *Compliant* firms. The last two columns compare firms defined as *Reclassifiers* to firms that do not use reclassification. For the main outcome variables we examine (*ROA* and *Profit Margin*), the subsamples exhibit good balance and parallel trends, as our tests do not detect a statistical difference in levels or growth rates across either dimension of comparison.

Among control variables, we detect statistical differences in four variables' levels (columns (1) and (3)). Non-compliant firms are 24.9 percent larger and carry 2.8 percent more leverage, while reclassifying firms are 46.1 percent larger.[31] Additionally, boards of non-compliant firms (column 1) and reclassifying firms (column 3) are larger and less independent. As with DD models, Olden and Møen (2022) show that in DDD models, introducing controls can bias estimates. However, controls can be worthwhile for two reasons: they can increase the precision of estimates and they can lead to conditional parallel trends.

[29] Statistical significance is shown in Table 10 and is described in more detail later in the article.
[30] Following the recommendations of Olden and Møen (2022), Roth et al. (2023), and many others.
[31] These percentages come from exponentiating the *Log*(*Assets*) coefficient, minus 1.

Table 3. Ex-ante balance and parallel trends.
This table reports tests for balance in level and growth paths among key covariates in the pre-treatment period of 1999–2002, along two dimensions of comparison. For each covariate $y_{i,t}$, each dimension $D_i \in \{NC_i, R_i\}$, and each $F(\cdot) \in \left\{ y_{i,t}, \frac{y_{i,t}-y_{i,t-1}}{y_{i,t-1}} \right\}$, we run an OLS regression specified by:

$$F(y_{i,t}) = \alpha + \beta D_i + \phi_j + \varepsilon_{i,j,t}.$$

where ϕ_j is SIC3 industry fixed effects. In this table, we report β for each combination of $y_{i,t}$, D_i, and $F(\cdot)$. Columns (1) and (3) examine balance in *levels* (i.e., $F(y_{i,t})$ is $y_{i,t}$), and columns (2) and (4) examine balance in *percentage growth* (i.e., $F(y_{i,t})$ is $(y_{i,t} - y_{i,t-1})/y_{i,t-1}$). Columns 1 and 2 compare non-compliant firms to compliant firms (D_i is NC_i). Columns 3 and 4 compare reclassifying firms to non-reclassifying firms (D_i is R_i). The sample comprises firm–year observations from 1999 to 2002, but otherwise is as described in Table 1. All variables are defined in Table 2, and Section 2.2 provides more details on the definition of NC_i and R_i. Standard errors are clustered by firm. *t*-Statistics are reported in parentheses. The symbols ***, **, and * indicate statistical significance at the 1 percent, 5 percent, and 10 percent levels, respectively.

$F(y_{i,t})$:	$y_{i,t}$	$\frac{y_{i,t}-y_{i,t-1}}{y_{i,t-1}}$	$y_{i,t}$	$\frac{y_{i,t}-y_{i,t-1}}{y_{i,t-1}}$
D_i:	NC_i	NC_i	R_i	R_i
$y_{i,t}$	(1)	(2)	(3)	(4)
Outcome variables				
ROA	0.001	0.041	0.006	0.043
	(0.10)	(0.75)	(0.72)	(0.74)
Profit Margin	0.005	0.054	0.001	0.034
	(0.38)	(0.99)	(0.05)	(0.59)
Control variables				
Log(Assets)	0.222**	−0.002	0.359***	−0.002
	(1.97)	(−0.70)	(2.88)	(−0.73)
Book Leverage	0.028**	−0.079	−0.009	0.185
	(2.05)	(−0.37)	(−0.65)	(0.81)
CAPX/Assets	0.005	−0.027	−0.002	0.017
	(1.47)	(−1.07)	(−0.62)	(0.65)
R&D/Assets	−0.005	−0.000	−0.003	−0.034
	(−1.15)	(−0.01)	(−0.69)	(−1.28)
Board Size	1.116***	−0.005	1.049***	0.007
	(5.63)	(−0.78)	(5.57)	(1.06)
Board Independence	−0.090***	0.011*	−0.049***	0.000
	(−6.86)	(1.75)	(−4.08)	(0.05)
Incoming Directors	−0.005	−0.023	0.001	0.010
	(−0.82)	(−0.38)	(0.23)	(0.15)
Outgoing Directors	0.006	0.060	−0.001	0.050
	(0.96)	(0.80)	(−0.24)	(0.67)

Both channels might be important in this setting. Thus, our central results will include tests with and without controls. Furthermore, we will present estimates in event-study plots that indicate the parallel trends assumption is satisfied.

4. Results

This section presents the central results of the article. We start with the main DDD specification followed by a series of robustness tests. We then show tests exploring the mechanism for the main results and conclude by examining valuation impacts of the mandates.

Table 4. Main DDD results.
This table reports OLS estimates of a DDD model given by equation (1). The sample comprises firm–year observations from 1999 to 2006. See Table 1 for more details on the sample. All variables are defined in Table 2. FE denotes fixed effects. Unreported interaction terms of P_t, R_i, and NC_i are absorbed by the fixed effects. *t*-Statistics, clustered by firm, are reported in parentheses. The symbols ***, **, and * indicate statistical significance at the 1 percent, 5 percent, and 10 percent levels, respectively.

Independent variable:	ROA_t		$Profit\ Margin_t$	
	(1)	(2)	(3)	(4)
$P_t \times R_i \times NC_i$	0.027**	0.027**	0.044**	0.040*
	(2.21)	(2.25)	(1.98)	(1.91)
$P_t \times R_i$	−0.011	−0.009	−0.005	−0.002
	(−1.55)	(−1.24)	(−0.43)	(−0.13)
$P_t \times NC_i$	−0.012	−0.012	−0.015	−0.014
	(−1.41)	(−1.47)	(−1.12)	(−1.04)
$Log(Assets)_{t-1}$		−0.023**		−0.032
		(−2.18)		(−1.14)
$Book\ Leverage_{t-1}$		−0.005		−0.009
		(−1.56)		(−0.99)
$CAPX/Assets_{t-1}$		−0.003		0.004
		(−1.04)		(0.84)
$R\&D/Assets_{t-1}$		−0.002		−0.002
		(−0.32)		(−0.18)
$Board\ Size_{t-1}$		−0.006*		−0.009
		(−1.87)		(−1.48)
$Board\ Independence_{t-1}$		−0.003		−0.004
		(−1.14)		(−0.84)
$Incoming\ Directors_{t-1}$		0.001		−0.000
		(1.11)		(−0.09)
$Outgoing\ Directors_{t-1}$		−0.003***		−0.004**
		(−2.75)		(−2.05)
Firm FE	Yes	Yes	Yes	Yes
Year × Ind. FE	Yes	Yes	Yes	Yes
Observations	4,337	4,244	4,337	4,244
Adj. R^2	0.66	0.66	0.63	0.64
Avg. Y	0.14	0.14	0.14	0.14

4.1 Main results

Panel A of Table 4 presents the main DDD results from estimating equation (1). Column (1) shows that non-compliant reclassifiers outperformed similar non-compliant firms that did not use reclassifications to meet the mandates. Specifically, non-compliant reclassifiers have profitability (ROA) that is 2.7 percentage points higher following the rule change than comparable non-compliant firms that do not use director reclassification. The magnitude of this coefficient is economically meaningful—the average profitability in the sample is 14 percent—and suggests that the mandates hindered the performance of firms subject to the new requirements.

Column (3) examines profit margin. In line with the main finding, non-compliant reclassifiers have relatively larger profit margins after the mandates than similar non-reclassifying non-compliant (NRNC) firms. The magnitude is roughly double that of the ROA specification—4.4 percent.

Table 3 shows that there was some pre-mandate imbalance across subsamples split by reclassification. Thus, we repeat the baseline models but add additional time-varying controls because such controls can be used to obtain conditional parallel trends. It is important to note that the firm fixed effects already absorb the component of these variables that are

stable within the firm over our sample period. Nevertheless, we include specifications with additional controls to assess the robustness of our findings.

Columns (2) and (4) report results when we include these controls. The estimated coefficients on the triple interaction term are quantitatively similar to those found in columns (1) and (3). Linck, Netter, and Yang (2009) showed that Sarbanes–Oxley (SOX) compliance burdened smaller firms disproportionally. Importantly, any study with an ISS data requirement (such as ours) restricts the sample to large firms where compliance costs are more homogeneous. Nevertheless, the inclusion of size in our tests allows us to control for the possibility that our findings are driven by the heterogeneous impact of the SOX regulations.

Another possible confounder is that new directors need time to learn about the firm, and therefore, the firm might underperform for a period of time following their appointment. While the univariate comparisons in figure 1 showed that non-reclassifying firms have very similar levels of director turnover and hiring, it remains possible that the results in columns (1) and (3) reflect higher turnover for these firms. To control for this possibility, we include board turnover measures. We find that these board-level controls do not affect the triple interaction term, and hence we conclude that board turnover is not driving our main findings.

Figure 2 reports β_1 in event time for each of the four specifications in Table 4, along with 90% confidence intervals. We obtain these event-time estimates by interacting all terms in equation (1) with year fixed effects. Panel (1) shows the results of the baseline ROA model without controls. In the pre-mandate period, β_1 is not statistically different from zero in any year, which provides support for the parallel trend assumption. Following the mandates, the DDD term increases linearly for two years before flattening over the remaining sample. The red lines on the figure show the average of the β_1 in the pre- and post-mandate periods, and the difference between the two lines is approximately equal to the main effect reported in Table 4.

The remaining panels contain similar evidence for the remaining specifications. In each case, the pre-mandate coefficients are both economically and statistically close to zero, with estimates rising in the post-period through 2004 before leveling off. Overall, the year-by-year dynamics are supportive of a differential effect occurring after the introduction of exchange mandates between firms that can reclassify directors as independent and those that do not have such flexibility.

To understand which of the difference(s) in the DDD are driving the main results, Table 5 decomposes the DDD test into its DD sub-components by repeating the estimates on three subsamples. For each subsample, we estimate the applicable DD by excluding terms from equation (1) without variation in that subsample.

The key intuition of our empirical strategy is that reclassifying firms can keep their pre-mandate board relatively intact. As such, we expect no change in performance for non-compliant reclassifiers after the mandate. Columns (1) and (2) examine the subset of reclassifying firms and confirm this intuition; the DD coefficient ($P_t \times NC_i$) is not statistically significant for either outcome.

In contrast, we expect the performance impact of the mandate to come from firms compelled to alter their board relatively more. Thus, columns (3) and (4) restrict the sample to non-reclassifying firms only and report results consistent with this expectation: The DD coefficient ($P_t \times NC_i$) is negative for both outcomes and statistically significant.[32]

Finally, columns (5) and (6) limit the sample to non-compliant firms in order to directly compare the performance change of reclassifying firms to firms that do not reclassify directors. Intuitively, this mimics a test that directly compares "treatment" versus "placebo" firms. The results indicate that performance of non-compliant firms declined if they did not

[32] You can approximate the DDD term for ROA in Table 4 by subtracting column (3) from column (1): 0.009–(–0.016)≈0.027.

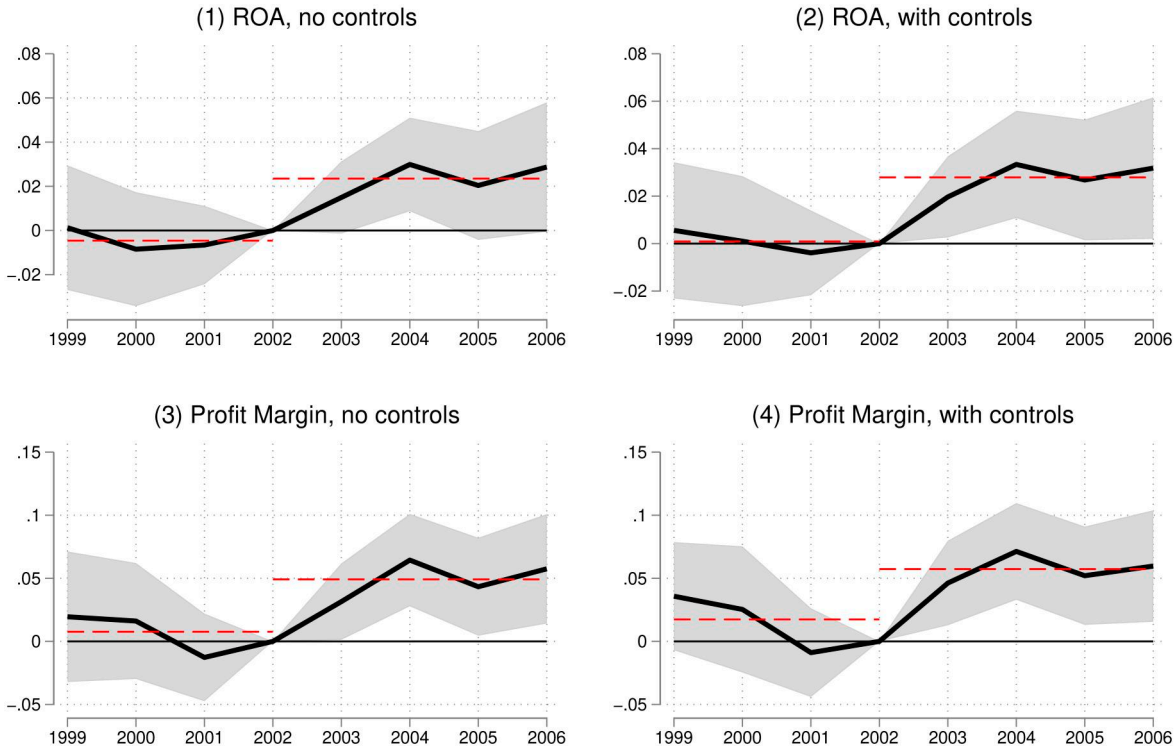

Figure 2. DDD estimates in event-time.

This figure reports β_1 from equation (1), estimated in event time. We obtain these event-time estimates by interacting all terms in equation (1) with year fixed effects. Each panel corresponds to the same numbered model in Table 4. The shaded areas display 90% confidence intervals. The dashed lines on the figure show the average of the β_1 in the pre- and post-mandate periods.

Table 5. DD specifications.
This table reports DD tests on three subsamples. For each subsample, we estimate the applicable DD by excluding terms from equation (1) without variation in that subsample. The sample comprises firm–year observations from 1999 to 2006. See Table 1 for more details on the sample. All variables are defined in Table 2. FE denotes fixed effects. T-statistics, clustered by firm, are reported in parentheses. The symbols ***, **, and * indicate statistical significance at the 1 percent, 5 percent, and 10 percent levels, respectively.

Subsample:	Reclassifiers only		Non-reclassifiers only		Non-compliant subsample	
Independent variable:	ROA_t (1)	Profit $Margin_t$ (2)	ROA_t (3)	Profit $Margin_t$ (4)	ROA_t (5)	Profit $Margin_t$ (6)
$P_t \times NC_i$	0.009	0.020	−0.016**	−0.022*		
	(1.20)	(1.60)	(−2.03)	(−1.86)		
$P_t \times R_i$					0.015	0.036**
					(1.64)	(2.41)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Ind. FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,664	1,664	2,951	2,951	1,658	1,658
Adj. R^2	0.65	0.66	0.62	0.62	0.60	0.57
Avg. Y	0.14	0.15	0.14	0.14	0.14	0.14

reclassify directors. Economically, the estimated impact on ROA is 1.5 percent in this specification, about 60 percent of the baseline finding.

4.2 Robustness

Chhaochharia and Grinstein (2009) examine CEO compensation using the same exchange mandates. Guthrie, Sokolowsky, and Wan (2012b) show their results are sensitive to two outliers among non-compliant firms. To test whether our study could suffer from a similar lack of robustness, we exclude five (approximately 1 percent) firms in the sample and re-run the specification in Table 4. We then repeat this 10,000 times. This test allows us to gauge whether our results are sensitive to outliers and is more conservative than a "leave-one-out" or "leave-two-out" test.

The results are reported in figure 3, which shows the resulting distribution of t-statistics for the DDD coefficient ($t(\beta_1)$) from equation (1) in the left column, and the β_1 coefficient values in the right column. Each row repeats the corresponding model from Table 4. The red vertical bars in the *t*-statistics panels represent the traditional significance cutoffs (i.e., 1.65 and 1.96, respectively). For each model (row), nearly all permutations show a *t*-statistic above the 10 percent cutoff, and a great majority are above the 5 percent cutoff.[33] Overall, we conclude that our main results are not driven by outlier firms.

Table 6 reports a falsification test as suggested by Roberts and Whited (2013). We repeat the baseline estimates but counterfactually assume the mandates' compliance period begins in 2001. Across all specifications, we find no differential effect of the mandate for reclassifying firms.

Our final robustness test redefines reclassifying firms to focus on those for whom reclassification plays a primary role in complying with the mandate. Specifically, we set $\tilde{R}_i$ equal to one if R_i is one and the number of independent directors appointed post-mandate by firm i is not in the top quartile of the distribution of appointments. We then substitute $\tilde{R}_i$ for R_i in equation (1). This alternative definition provides cleaner variation by isolating firms for which reclassification is a more predominant compliance mechanism, though it

[33] In model 3 (4), 0.01 percent (1 percent) of permutations have t-statistics below 1.65.

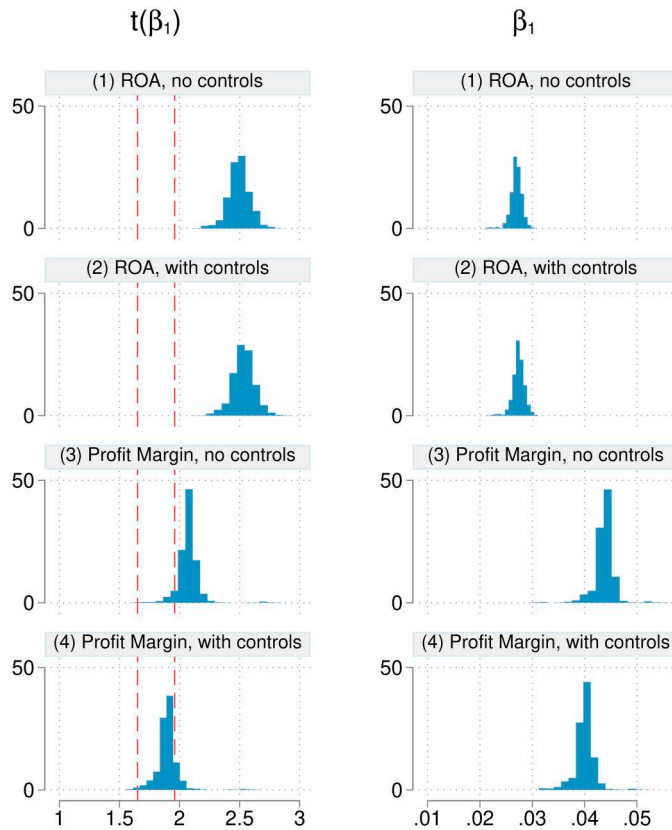

Figure 3. Sensitivity to outliers.

This figure presents a sensitivity analysis of the main results in Table 4. We exclude 1 percent of firms in the sample and re-estimate the models in the table and repeat this process 10,000 times. The figure displays the resulting distribution of t-statistics for the DDD coefficient ($t(\beta_1)$) from equation (1) in the left column and the β_1 coefficient values in the right column. The red vertical bars in the *t*-statistic figures represent the traditional significance cutoffs (i.e., 1.65 and 1.96, respectively). The independent variable of the regression associated with each subfigure is denoted above it, while the y-axis in each subfigure represents the frequency of outcomes within a specific range.

reduces statistical power due to the stricter cutoff. Despite this limitation, the test yields results that are quantitatively similar to our main findings.

4.3 Drivers of differences in operating performance

To understand the channel driving the relative underperformance of non-reclassifying firms, we decompose profit margins into components related to COGS, SG&A, and R&D.[34] Panel A of Table 7 presents point estimates showing declines across all three cost components for reclassifying firms post-mandate, with the reduction in SG&A margins reaching statistical significance. Importantly, because all our DDD results are framed in relative terms, the estimates also imply that non-reclassifying firms experienced relative increases in these operational cost components post-mandate. These results highlight the

[34] Compustat's SG&A data were missing for several firms in our sample. To address this, we manually reviewed the 10-K filings of these firms and supplemented the dataset with 266 additional firm–year observations for which SG&A values were consistent throughout the entire sample window. The updated data and accompanying documentation are included in the replication package for this article. Our results remain robust when using only the original Compustat data.

Table 6. Falsification tests.
This table contains falsification tests of the main results from Table 4. The sample comprises firm–year observations from 1999 to 2006. See Table 2 for more details on the sample. P_t^{2001} is an indicator as one beginning in 2001 and as zero before. All other variables are defined in Table 2. FE denotes fixed effects. Unreported interaction terms of P_t^{2001}, R_i, and NC_i are absorbed by the fixed effects. *t*-Statistics, clustered by firm, are reported in parentheses. The symbols ***, **, and * indicate statistical significance at the 1 percent, 5 percent, and 10 percent levels, respectively.

Independent variable:	ROA_t		$Profit\ Margin_t$	
	(1)	(2)	(3)	(4)
$P_t^{2001} \times R_i \times NC_i$	0.018	0.017	0.012	0.003
	(1.31)	(1.25)	(0.50)	(0.12)
$P_t^{2001} \times R_i$	−0.005	−0.002	0.015	0.021
	(−0.52)	(−0.19)	(0.99)	(1.30)
$P_t^{2001} \times NC_i$	−0.009	−0.009	0.004	0.008
	(−0.96)	(−0.97)	(0.27)	(0.54)
Controls	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes
Year × Ind. FE	Yes	Yes	Yes	Yes
Observations	4,337	4,244	4,337	4,244
Adj. R^2	0.66	0.66	0.63	0.64
Avg. Y	0.14	0.14	0.14	0.14

role of reduced efficiency, particularly in SG&A-related areas, in the operational underperformance of non-reclassifying firms post-mandate.

Panel B investigates the sources of the relative inefficiency in SG&A-related margins for non-reclassifying firms. Since SG&A expenses often include substantial labor costs, the observed changes likely reflect reduced employee productivity at these firms. Our findings indicate that profit and sales per employee improved significantly for reclassifying firms after the mandates, highlighting their relative ability to maintain operational efficiency. In the Supplementary Appendix, we also analyze employment levels and find no evidence that the difference in performance per employee is driven by changes in employment. This suggests that, in the post-mandate period, non-reclassifiers were relatively less effective at utilizing their existing workforce.

To build on this evidence, we incorporate measures that capture where a firm stands in the product life cycle. Abernathy and Utterback (1978) define the four stages of this cycle as product innovation, process innovation, maturity, and decline, and Hoberg and Maksimovic (2022) provide estimates of the fraction of a firm's product portfolio in each stage. The results are reported in columns (3)–(6) of Panel B, with column (4) revealing that reclassifying firms exhibit a statistically significant increase in discussions related to process innovation. Specifically, the process innovation measure increases when more paragraphs in a 10-K mention "cost(s)" or "expense(s)" alongside words describing types of operating expenditures (e.g., labor, employees, and wages). This finding highlights that non-reclassifying firms, by contrast, were less able to leverage process innovation as a means of improving operational efficiency.

Finally, Panel B reports a negative triple interaction term when sales growth is the outcome variable, indicating that non-reclassifying non-compliant firms experienced relatively faster sales growth in the post-mandate period. While this result is statistically significant only at the 10 percent level, it suggests that the relative inefficiency observed at non-reclassifiers may reflect a tradeoff, where higher sales growth comes at the expense of operational efficiency gains.

Table 7. Drivers of differences in operating performance.
This table contains tests exploring channels for the main results in Table 4. The sample comprises firm–year observations from 1999 to 2006. See Table 2 for more details on the sample. Independent variables are defined in Appendix Table A1. All other variables are defined in Table 2. FE denotes fixed effects. To conserve space, we only report the main coefficient. Avg. Y reports the average value of the dependent variable in each model. T-statistics, clustered by firm, are reported in parentheses. The symbols ***, **, and * indicate statistical significance at the 1 percent, 5 percent, and 10 percent levels, respectively.

Panel A: Decomposing profit margins

Independent variable:	COGS/Sale (1)	SGA/Sale (2)	R&D/Sale (3)
$P_t \times R_i \times NC_i$	−0.006	−0.034**	−0.005
	(−0.37)	(−2.25)	(−0.48)
Controls	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Year × Ind. FE	Yes	Yes	Yes
Observations	4,244	4,045	4,244
Adj. R^2	0.85	0.84	0.79
Avg. Y	0.60	0.27	0.07

Panel B: Employee productivity

			Life Cycle Measures				
Independent variable:	Prof/L (1)	Sale/L (2)	Product Innov. (3)	Process Innov. (4)	Maturity (5)	Product Discont. (6)	SalesGrth (7)
$P_t \times R_i \times NC_i$	0.018**	0.047**	−0.008	0.037*	−0.028	−0.002	−0.055*
	(2.39)	(2.19)	(−0.50)	(1.72)	(−1.34)	(−0.11)	(−1.65)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year × Ind. FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,195	4,195	4,198	4,198	4,198	4,198	4,244
Adj. R^2	0.87	0.92	0.76	0.76	0.66	0.54	0.32
Avg. Y	0.05	0.31	0.24	0.40	0.29	0.07	0.13

Panel C: Working capital management

	Turnover					
Independent variable:	Total Asset (1)	Fixed Asset (2)	Inventory (3)	AR (4)	AP (5)	CCC (6)
$P_t \times R_i \times NC_i$	0.045	0.855	0.950	4.852	−2.381*	−1.464
	(1.04)	(0.90)	(0.48)	(1.37)	(−1.74)	(−0.21)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Ind. FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,244	4,241	4,139	3,660	4,242	4,209
Adj. R^2	0.94	0.93	0.90	0.85	0.77	0.84

Consistent with the internal focus of reclassifiers, we find no evidence showing changes in M&A propensity or decision quality post-mandate. In the Supplementary Appendix, we investigate M&A activity using detailed SDC data on completed deals for all sample firms during the analysis window (1999–2006). Using the same DDD framework as in our main results, we test two dimensions of M&A activity: propensity and decision quality. We find no significant differences in M&A activity across groups on the extensive margin (whether a firm engaged in acquisitions) or the intensive margin (the number of acquisitions per year). Similarly, tests of decision quality, measured by the number of value-destroying acquisitions with negative cumulative abnormal returns (CAR) and accounting-based metrics like goodwill impairment, reveal no statistically significant differences.[35]

Panel C explores another potential channel through which firms might enhance their operational performance: working capital management. Columns (1)–(5) examine different measures of asset turnover as outcome variables, while column (6) focuses on the duration of the cash conversion cycle (CCC) defined in Appendix Table A1. The results provide tentative but weak evidence that working capital management may explain some of the observed disparities. Although the estimates suggest relative inefficiencies for non-reclassifiers across most dimensions, the statistical support for these findings is limited. Specifically, only the decrease (increase) in accounts payable turnover for reclassifying (non-reclassifying) firms is significant at the 10 percent level, highlighting a modest advantage for reclassifiers in this area.

Finally, we examine whether differences in risk-taking could account for the observed disparities in operating performance. To do so, we analyze six measures of risk: total risk (standard deviation of daily returns), systematic risk (CAPM beta), idiosyncratic risk (standard deviation of residual returns based on CAPM), book leverage (debt-to-assets), market leverage (debt divided by debt plus market capitalization), and cash (divided by assets) to assess precautionary savings. The results, reported in the Supplementary Appendix, reveal no statistically significant differences in risk-taking between reclassifying and non-reclassifying firms across any of these metrics. This indicates that differential risk-taking does not play a significant role in explaining the operational performance differences.

In summary, the primary channel behind the relative decline in performance observed at non-reclassifying firms stems from a differential in SG&A costs, driven by reduced labor efficiency. In Section 5, we examine director-level data to further investigate the mechanisms underlying this relationship.

4.4 Valuation

To gauge the market assessment of these mandates, we first adopt the event study design of Chhaochharia and Grinstein (2007) (hereafter CG2007). Following their methodology, we estimate four-factor regressions of daily portfolio returns, incorporating the factors from Fama and French (1993) and Carhart (1997), over the period from November 1, 2001, to October 31, 2002. This window spans nearly a year and encompasses multiple phases of the implementation of the new exchange rules.

Table 8 presents the results. Panel A focuses on equal-weighted portfolio returns, while Panel B examines value-weighted portfolio returns. For the convenience of readers, column (5) reports the main results from CG2007, which show the alpha of portfolios for the most- and least-compliant firms.[36] In column (1), we conduct an analogous test using our sample firms and our definition of compliance. While our sample differs due to the additional restrictions required for the DDD design and our reliance on exchange definitions of independence (rather than ISS definitions), the overall patterns remain similar.[37] Specifically, when examining equal-weighted portfolios, we find a positive but statistically insignificant annualized alpha of

[35] These results are consistent across various robustness checks, including an analysis of acquisition CARs and the fraction of value-destroying deals in firm–years with at least one acquisition.
[36] From Panel C in their Table IV.
[37] The exception is that we find a larger alpha for the value-weighted portfolio of compliant firms.

Table 8. Valuation.
This table shows the announcement effect of governance rules on different portfolios of firms, based on their compliance with the board independence provisions. We use the event study design of CG2007 and estimate four-factor regressions (Fama and French 1993; Carhart 1997) of daily portfolio returns from November 1, 2001, to October 13, 2002. The table below reports the alpha for various portfolios along with Newey–West standard errors using four lags. The sample comprises firms in the main results from Table 4. We form portfolios across each level (0/1) of compliance and across each level of reclassification (0/1). Column (4) and the last row of each panel examine long–short portfolios. The symbols ***, **, and * indicate statistical significance at the 1 percent, 5 percent, and 10 percent levels, respectively.

Panel A: Equal-weighted 4 factor alphas

Source:	This study				CG2007, Table IV, Panel C	
Subset:	All (1)	R = 1 (2)	R = 0 (3)	R = 1 – R = 0 (4)		All (5)
$NC = 1$	0.0006***	0.0005**	0.0007***	−0.0003	L	.0006***
	(0.0002)	(0.0002)	(0.0002)	(0.0003)		(0.0001)
$NC = 0$	0.0006**	0.0005	0.0006**	−0.0001	H	.0004**
	(0.0002)	(0.0003)	(0.0002)	(0.0002)		(0.0001)
$NC = 1 - NC = 0$	0.0000	−0.0000	0.0001	−0.0002	L-H	0.0002
	(0.0002)	(0.0003)	(0.0002)	(0.0004)		(0.0001)

Panel B: Value-weighted 4 factor alphas

Source:	This Study				CG2007, Table IV, Panel C	
Subset:	All (1)	R = 1 (2)	R = 0 (3)	R = 1 – R = 0 (4)		All (5)
$NC = 1$	0.0005*	0.0003	0.0008**	−0.0004	L	.0008***
	(0.0003)	(0.0004)	(0.0003)	(0.0004)		(0.0002)
$NC = 0$	0.0011***	0.0011**	0.0011***	−0.0000	H	0.0002
	(0.0003)	(0.0005)	(0.0004)	(0.0006)		(0.0002)
$NC = 1 - NC = 0$	−0.0006	−0.0007	−0.0003	−0.0004	L-H	.0006**
	(0.0004)	(0.0006)	(0.0005)	(0.0007)		(0.0002)

1 percent for a long–short portfolio that buys non-compliant firms and shorts compliant firms.[38] This effect is modest and directionally consistent with CG2007's finding of a 4 percent annualized alpha, albeit smaller in magnitude.

To evaluate whether the market recognized the potential benefits of reclassifying directors as independent under the exchange mandates, we construct portfolios that mirror our DDD analysis. Specifically, we form four portfolios based on the combination of compliance and reclassification, and report their performance in columns (2) and (3). We then compute returns for long–short portfolios that compare reclassifying and non-reclassifying firms within each compliance group (column (4)). The difference between these long–short portfolios, reported in the last row of column (4), provides a market-based analog to our DDD estimates. We find no significant differences using either equal- or value-weighted approaches, suggesting that the market did not price reclassification decisions during this period.

The lack of a clear market reaction should be interpreted with caution. The implementation of the exchange mandates unfolded over an extended period and was marked by multiple regulatory events, creating significant challenges for an event study approach (Kothari and Warner 2007). For instance, Zhang (2007), using different research design choices than CG2007, finds that abnormal returns are higher—rather than lower—for firms with

[38] The annualized alpha is computed as 252 times the daily long-short alpha.

better governance. Furthermore, in a regulatory environment that strongly emphasized greater board independence, the market may not have fully anticipated the potential value of retaining former insiders in board positions. Notably, non-compliant firms that reclassified directors remained the least independent boards in our sample based on ISS classifications (recall panel (3) of figure 1), even as they achieved superior operating performance.

We also investigate longer-term valuation effects. In the Supplementary Appendix, we estimate our DDD specification using Tobin's Q as the dependent variable. The coefficient on the triple interaction term is insignificant, aligning with our event study findings. A potential explanation for these null results emerges from our earlier analysis in Table 7, which explores the drivers of performance. Specifically, the (weakly) significant negative triple interaction effect on sales growth suggests that the operational improvements we document may have come at the expense of growth opportunities, a key driver of market-based valuations.

5. Mechanisms and alternative explanations

The previous section finds that reclassifying firms outperformed their non-reclassifying counterparts in the post-mandate period, primarily due to operational cost increases at non-reclassifying firms driven by reduced labor efficiency. In this section, we examine director-level data to better understand the mechanisms behind these differences. While we attribute the results to the benefits of retaining directors with firm-specific knowledge, other mechanisms could also be at play. We thus conclude by exploring alternative explanations for our central performance findings.

5.1 Director-level evidence

We begin by examining whether differences in director characteristics might help explain the disparities in operational performance between reclassifying and non-reclassifying

Table 9. Comparing reclassifying to non-reclassifying directors.
This table reports statistics about directors. The sample is all director–firm–year observations from 1999 to 2006 of firms in the main results in Table 4 for which ISS governance data are available. Variables are defined in Appendix Table A2. $r_{d,i,t}$ equals one if director d of firm i is reclassified as independent in year t. Column (1) reports the average for observations in which a director is reclassified. Column (2) reports the average for directors who never reclassify. The difference ("Diff") in the average between R and NR firms is tested using an unequal variances t-test. The symbols ***, **, and * indicate statistical significance at the 1 percent, 5 percent, and 10 percent levels, respectively.

	Subsample		
	$r_{d,i,t} = 1$ (1)	$(\sum_t r_{d,i,t}) = 0$ (2)	Diff (3)
Independent	1.00	0.71	0.29***
Fraction of firm shares held	2.15	1.12	1.03***
Former employee?	0.64	0.04	0.59***
On any of A/C/G/N committees	0.41	0.68	−0.28***
Audit committee	0.20	0.42	−0.23***
Compensation committee	0.17	0.41	−0.24***
Governance committee	0.12	0.22	−0.10***
Nominating committee	0.20	0.31	−0.11***
Directorship tenure	13.80	9.93	3.87***
Num. of outside board positions	0.68	0.74	−0.05
Low attendance	0.02	0.02	0.00

firms. Table 9 presents univariate comparisons of all director–firm–year observations for firms in the main results in Table 4, comparing reclassified and non-reclassified directors.

Specifically, column (1) of Table 9 reports characteristics of directors d of firm i in the year t they are reclassified as independent (which we denote as $r_{d,i,t} = 1$, using lowercase for the director-level variable), while column (2) reports characteristics of directors who are never reclassified on a given board ($(\sum_t r_{d,i,t}) = 0$). Column (3) presents the differences between these groups, along with t-test significance levels. The results show that reclassifying directors have similar attendance records and number of board positions, suggesting that the differences in operational performance are not driven by variations in attendance or general measures of director quality (to the extent that additional board positions serve as a proxy for quality). However, reclassifying directors differ across several key characteristics: they own more shares in the firm (2.1 percent vs. 1.1 percent), are far more likely to be former employees (65 percent vs. 4 percent), have approximately 25 percent more board experience, and are 28 percentage points less likely to serve on committees. These traits suggest that reclassifying directors have significant firm-specific knowledge and focus on advisory roles rather than monitoring responsibilities.[39]

The ability to reclassify directors reduces the number of changes non-compliant firms *need* to make to their boards to satisfy the independence mandate. Indeed, Chhaochharia and Grinstein (2009) observe that 13 percent of firms in their sample complied with the mandate exclusively through reclassification. To further support the hypothesis that reclassifying firms exhibit greater flexibility in meeting compliance requirements, we measure "net independent hires" as the number of independent directors added to the board minus those who leave during the post-mandate period.

Our findings indicate that reclassifying non-compliant firms do not rely as much on external hiring of independent directors. Specifically, reclassifying non-compliant firms average 0.63 net independent hires, compared to 0.92 for non-reclassifying non-compliant firms. In the Supplementary Appendix, we also find that the proportion of reclassified directors serving on any one of the board committees increases monotonically around the reclassification year, from 30 percent 2 years before reclassification to 45 percent 2 years after. This pattern suggests that firms use reclassified directors to satisfy independence mandates at both the board and committee levels. As such, firms that use reclassification have post-mandate boards that more closely resemble their pre-mandate boards.

Combining this fact with our findings of better post-mandate performance for reclassifying non-compliant firms suggests that pre-mandate boards were well-functioning and that firms required to make more substantial changes to their boards to meet the independence mandates experienced subsequent underperformance.

To complement the analysis in Table 9, which compares reclassified and non-reclassified directors, Table 10 examines incoming and outgoing directors within non-compliant firms, comparing those at reclassifying versus non-reclassifying firms during the post-mandate period. The analysis includes all firm–director–year observations from 2003 to 2006 for non-compliant firms featured in the main results in Table 4 where ISS governance data are available.[40] Directors are separated into incoming and outgoing groups, and for each characteristic, we report the average for reclassifying (R) and non-reclassifying (NR) firms separately. The difference ("Diff") in averages between R and NR firms is tested using an unequal variances t-test.

The first three columns focus on incoming directors, allowing us to test whether reclassifying non-compliant firms (column 1) attract a different type of incoming director than non-compliant non-reclassifying firms (column 2) on average. As might be expected under

[39] The patterns reported are consistent across individual sample years and remain unchanged when restricting the sample to non-compliant firms, reclassifying firms, or their intersection.

[40] Observations corresponding to a firm's first or last year in the ISS data are excluded, as these do not allow for determination of whether directors are incoming or outgoing.

Table 10. Incoming and departing directors at non-compliant firms.
This table reports statistics about incoming and departing directors of non-compliant firms and compares firms that reclassify directors to those that do not during the post-mandate period. The sample is all director–firm–year observations from 2003 to 2006 of non-compliant firms in the main results in Table 4 for which ISS governance data are available. Section 2.2 describes how firms are classified as non-compliant and which firms are defined as a *Reclassifier* ("R") or a *NonReclassifier* ("NR"). Variables are defined in Appendix Table A2. We separately report averages for incoming directors in their first year on a board and departing directors in their last year on a board, for R and NR firms separately. We exclude observations that are a firm's first or last year in the ISS data as these observations do not allow us to determine whether directors are incoming or outgoing. The difference ("Diff") in the average between R and NR firms is tested using an unequal variances *t*-test. The symbols ***, **, and * indicate statistical significance at the 1 percent, 5 percent, and 10 percent levels, respectively.

Director-observation subsample:	**Incoming directors**			**Departing directors**		
Firm subsample:	**R** (1)	**NR** (2)	**Diff** (3)	**R** (4)	**NR** (5)	**Diff** (6)
Independent	0.87	0.88	−0.01	0.77	0.70	0.07*
Fraction of firm shares held	0.13	0.17	−0.04	0.72	0.90	−0.18
Former employee?	0.01	0.02	−0.00	0.13	0.09	0.04
On any of A/C/G/N committees	0.52	0.63	−0.11***	0.56	0.60	−0.05
Audit committee	0.32	0.40	−0.07**	0.31	0.38	−0.07*
Compensation committee	0.23	0.27	−0.04	0.31	0.34	−0.03
Governance committee	0.18	0.23	−0.06*	0.25	0.30	−0.05
Nominating committee	0.21	0.24	−0.03	0.30	0.33	−0.03
Directorship tenure	0.89	0.98	−0.09	9.81	9.63	0.18
Num. of outside board positions	0.58	0.46	0.12	0.70	0.61	0.09
Low attendance	0.01	0.01	0.01	0.04	0.02	0.02

the mandate, most incoming directors (88 percent) are independent, with no meaningful difference in this proportion between reclassifiers and non-reclassifiers.

Column (3) highlights several statistically significant differences between the two groups. The most notable is that non-reclassifying firms are approximately 20 percent more likely to appoint incoming directors to at least one of the committees specified by the exchanges' mandate (audit, compensation, governance, or nominating). Examining each committee individually, we find that incoming directors at non-reclassifying firms are also more likely to serve on audit and governance committees, with a relative increase in likelihood of about 20 percent in both cases. These findings suggest that non-reclassifying firms rely on new directors more heavily for monitoring roles than reclassifying firms.

The next three columns focus on outgoing directors, enabling us to test whether reclassifying non-compliant firms (column 4) have, on average, a different type of outgoing director compared to non-reclassifying non-compliant firms (column 5). Among non-compliant firms, the only statistically significant differences between the non-reclassifying and reclassifying firms are that departing directors of reclassifying firms are more likely to be independent and less likely to be on the audit committee. This finding, along with the differing traits and roles of incoming directors, underscores the greater flexibility reclassifying firms had in complying with the mandates.

To explore the implications of this flexibility, Table 11 aggregates director-level information to the board level, providing a broader view of how boards evolved over the sample period. We focus on non-compliant firms and compare reclassifying (R) and non-reclassifying (NR) firms. The table complements figure 1 by presenting statistical tests for each year as well as for the pre- and post-mandate periods.

Columns (1)–(6) examine two board characteristics likely impacted by the flexibility offered by reclassification: the number of former employees (insiders) on the board and director

Table 11. Boards of non-compliant firms over time.
This table reports statistics about the board of directors of non-compliant firms and compares firms that reclassify directors to those that do not. The sample is all firm–year observations from 1999 to 2006 of non-compliant firms in the main results in Table 4 for which ISS governance data are available. Section 2.2 describes how firms are classified as non-compliant and which firms are defined as a *Reclassifier* ("R") or a *NonReclassifier* ("NR"). In each year and for each variable, we report the average for R and NR firms. *Pre* refers to 1999–2002, *Post* refers to 2003–2006, and *Post* minus *Pre* computes the difference before and after the mandates take effect. The difference ("Diff") in the average between R and NR firms is tested using an unequal variances *t*-test. The symbols ***, **, and * indicate statistical significance at the 1 percent, 5 percent, and 10 percent levels, respectively.

	No. of former employees on board			Avg. director tenure			No. of directors added to board			No. of directors removed from board		
Subsample:	R (1)	NR (2)	diff (3)	R (4)	NR (5)	diff (6)	R (7)	NR (8)	diff (9)	R (10)	NR (11)	diff (12)
1999	0.57	0.47	0.10	9.23	10.40	−1.18	0.83	0.73	0.10	0.74	0.78	−0.04
2000	0.66	0.51	0.15	9.11	10.32	−1.21*	0.70	0.65	0.05	0.82	0.51	0.31**
2001	0.82	0.55	0.27**	9.18	9.94	−0.76	0.93	0.71	0.22	0.80	0.78	0.02
2002	0.84	0.44	0.40***	9.39	10.30	−0.91	0.72	0.58	0.14	0.83	0.81	0.03
2003	0.92	0.35	0.58***	9.67	10.25	−0.58	0.96	0.84	0.11	0.88	0.91	−0.03
2004	0.90	0.29	0.61***	9.48	10.11	−0.63	0.95	0.89	0.05	0.92	0.74	0.18
2005	0.84	0.27	0.57***	9.48	9.97	−0.49	1.19	1.15	0.04	1.18	1.04	0.14
2006	0.72	0.23	0.49***	9.66	10.15	−0.49	0.82	0.75	0.08	0.95	0.76	0.19
Pre:	0.72	0.49	0.23***	9.23	10.23	−1.00***	0.79	0.67	0.13*	0.80	0.72	0.08
Post:	0.85	0.28	0.56***	9.57	10.12	−0.55*	0.98	0.91	0.07	0.98	0.86	0.12
Post–Pre:	0.12*	−0.21***	0.33***	0.34	−0.11	0.45	0.19**	0.25***	−0.06	0.18**	0.15*	0.03

tenure. Among reclassifying firms, the average number of former employees on the board increased slightly, whereas it declined significantly for non-reclassifying firms. These differential changes are highly significant and confirm that reclassification enhances the ability to retain directors with firm-specific knowledge. Similarly, the average director tenure increased for reclassifying firms during the post-mandate period but decreased for non-reclassifiers, narrowing the tenure differential between the two groups by about half a year.

The results in this section underscore a key constraint imposed by the exchange mandate: retaining directors with firm-specific human capital on the board becomes more challenging post-mandate. By reclassifying former employees as independent directors, some firms are able to meet the mandate while preserving valuable institutional knowledge. Additionally, reclassification provides boards with greater flexibility in director appointments, retention, and committee assignments.

5.2 Alternative explanations

Building on the director-level evidence presented in Section 5.1, we next examine several alternative explanations for our central findings, focusing first on adjustment costs from director turnover, such as learning frictions.

The adjustment cost hypothesis posits that new directors may initially contribute less to the board as they acquire firm-specific knowledge, potentially causing temporary performance effects even if their long-term impact equals (or surpasses) that of the replaced director. If reclassifying firms experience less director turnover and thus face fewer adjustment costs, this could account for the observed performance disparities.

However, three pieces of evidence suggest that adjustment costs are unlikely to explain our results. First, figure 1 demonstrates that director hiring patterns are very similar across the different groups of firms. Second, if adjustment costs were the main driver of the results, we would expect performance effects to fade over time. Instead, the dynamic DDD estimates in figure 2 show no reversal in the third and fourth years of the post-mandate period. Third, our main results explicitly control for board turnover and hiring, further mitigating this concern.

Columns (7)–(12) of Table 11 formally test whether the hiring patterns observed in figure 1 are statistically different. The results indicate that turnover patterns are highly similar between reclassifying and non-reclassifying firms, with no statistically significant differences. For instance, in the post-mandate period (2003–2006), the average non-reclassifying board adds 0.89 directors annually and loses 0.83, while reclassifying boards add 0.96 directors and lose 0.93. These findings, consistent with figure 1, show that reclassifying firms do not experience systematically lower turnover.

Taken together, the results in Table 11 reinforce earlier evidence that reclassifying boards experience similar levels of director turnover and, consequently, comparable adjustment costs. Thus, our findings do not support the adjustment cost hypothesis.

The second alternative explanation we explore is the role of board co-option (Coles, Daniel, and Naveen 2014). Directors often demonstrate loyalty to the CEO who appointed them, potentially reducing their motivation to effectively carry out oversight responsibilities—a phenomenon known as board co-option. Boards become more co-opted as new directors are hired during a CEO's tenure. If non-reclassifying firms hired more new directors, then co-option could explain the underperformance. As with the adjustment cost hypothesis, this alternative explanation is undercut by the results in figure 1 and Table 11 that show hiring is statistically indistinguishable across the groups in our test in all years.

Moreover, if increased co-option among non-reclassifying firms were driving their underperformance, it would likely also result in higher CEO compensation (see Bebchuk and Fried 2003). To test this, we use CEO compensation within our empirical framework, predicting a negative DDD coefficient under the co-option hypothesis. Instead, the results in Table 12 show point estimates that are statistically insignificant, with four tests yielding negative estimates.

Table 12. CEO compensation.
This table reports OLS estimates of a DDD) model given by equation (1). The sample comprises firm–year observations from 1999 to 2006. See Table 1 for more details on the sample. All variables are defined in Table 2. FE denotes fixed effects. Unreported interaction terms of P_t, R_i, and NC_i are absorbed by the fixed effects. *t*-Statistics, clustered by firm, are reported in parentheses. The symbols ***, **, and * indicate statistical significance at the 1 percent, 5 percent, and 10 percent levels, respectively.

Independent variable:	log(TDC1$_t$)		log(TDC2$_t$)		log(Salary$_t$+Bonus$_t$)	
	(1)	(2)	(3)	(4)	(5)	(6)
$P_t \times R_i \times NC_i$	−0.063	−0.055	0.180	0.176	0.048	0.063
	(−0.52)	(−0.45)	(1.27)	(1.26)	(0.51)	(0.65)
$P_t \times R_i$	0.028	0.041	−0.049	−0.054	−0.033	−0.031
	(0.32)	(0.46)	(−0.51)	(−0.58)	(−0.48)	(−0.45)
$P_t \times NC_i$	0.031	−0.013	−0.048	−0.088	−0.009	−0.020
	(0.46)	(−0.19)	(−0.53)	(−1.03)	(−0.17)	(−0.37)
Controls	No	Yes	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Ind. FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,141	4,060	4,183	4,100	4,189	4,106
Adj. R^2	0.61	0.62	0.53	0.54	0.68	0.68
Avg. Y	7.79	7.79	7.56	7.57	6.79	6.79

This finding suggests that board co-option is unlikely to be the driver of their decreased performance.

6. Conclusion

Corporate governance reforms have long focused on increasing monitoring to prevent excess private rent accruing to executives, reduce corporate misconduct, and avoid scandals that can erode trust in capital markets. These efforts focus on tail risks, but regulatory compliance imposes costs on most firms.

To understand the potential costs of such reforms, this article focuses on the 2002 NYSE and NASDAQ board independence mandates and develops a novel approach to examine their impact on firm performance. By exploiting the differences in the manner in which non-compliant firms meet the mandated push toward board independence, we find evidence that the mandate reduced performance for firms that were unable to reclassify pre-existing inside directors as independent. Thus, real outcomes were better for firms whose post-mandate boards were closer to their pre-mandate boards. This finding is consistent with the view that the composition of directors reflects tradeoffs between various skill sets and knowledge necessary to guide the firm.

Our findings underscore the need for deeper exploration into the specific qualities that make some directors highly effective. Gaining a clearer understanding of how directors balance monitoring responsibilities with advisory roles might provide valuable insights for regulators, investors, and firms. Such research could guide the design of governance structures that not only enhance oversight but also capitalize on strategic advice essential for long-term performance.

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Supplementary material

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Conflicts of interest: None declared.

Data and code availability

The code for this article is available on GitHub at https://github.com/donbowen/RoF_RevisitingBoardIndependenceMandates. This repository includes data collected by the authors. Some datasets (e.g., CRSP, Compustat, Execucomp, ISS) cannot be shared publicly due to licensing restrictions, but we provide pseudo-data so that users can run our code. We will share the underlying data upon request from researchers with licensing access to the necessary data providers.

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Appendix A

Table A1. Definition of additional firm–year level variables.
Table 2 defines the key outcome variables and controls in the article. This table defines additional variables at the firm–year level used in the analysis of the drivers of operating performance in Table 7, using variable abbreviations from the CRSP-Compustat Merged (CCM) database. All variables are winsorized at the 1 percent tails.

COGS/Sale	cogs/sale
SGA/Sale	xsga/sale
R&D/Sale	xrda/sale, missing set to zero
Prof/L	$\text{oibdp}_t/((\text{emp}_t+\text{emp}_{t-1})/2)$
Sale/L	$\text{sale}_t/((\text{emp}_t+\text{emp}_{t-1})/2)$
Product Innov.	"Life1" from Hoberg and Maksimovic (2022)
Process Innov.	"Life2" from Hoberg and Maksimovic (2022)
Maturity	"Life3" from Hoberg and Maksimovic (2022)
Product Discont.	"Life4" from Hoberg and Maksimovic (2022)
SalesGrth	$\text{sale}_t/\text{sale}_{t-1}-1$
Total Asset Turnover	sale/at
Fixed Asset Turnover	sale/ppent
Inventory Turnover	cogs/invt
AP Turnover	cogs/ap
AR Turnover	sale/rect
CCC	Cash Conversion Cycle, defined as days sales outstanding (DSO) plus days of inventory on hand (DIOH) minus days payable outstanding (DPO) $= DSO + DIOH - DPO = \frac{rect}{(sale/365)} + \frac{invt}{(cogs/365)} - \frac{ap}{(cogs/365)}$

Table A2. Definition of director–firm–year level variables.
This table defines variables at the director–firm–year level. Raw data are from the ISS Directors Legacy database and covers 1999–2006. The steps used to process the raw data are described in Supplementary Appendix OA.2. Names in italics denote variable names in the ISS Directors Legacy database.

Independent	Equals one if the director is independent in a given year and zero otherwise. Directors are defined as independent according to the procedure in Supplementary Appendix OA.2
Fraction of firm shares held	100**num_of_shares/votecref*. ISS stopped collecting *votecref* after 2003, so we use *CSHO* from Compustat when *votecref* is missing.
Former employee	Equals one if the director was previously an employee of the firm and zero otherwise (*former_employee_yn* = 1).
Audit committee	Equals one if the director is on the audit committee and zero otherwise. (*audit_membership* + *audit_chair* > 0)
Compensation committee	Equals one if the director is on the compensation committee and zero otherwise. (*comp_membership* + *comp_chair* > 0)
Governance committee	Equals one if the director is on the governance committee and zero otherwise. (*gov_comm_mem* = 1)
Nominating committee	Equals one if the director is on the nominating committee and zero otherwise. (*nom_membership* + *nomchair* > 0)
On any of A/C/G/N committees	Equals one if the director is at least one of the committees above and zero otherwise.
Director tenure	Years the director has served on the board of a given firm (*year* – *dirsince*)
Num. of outside board positions	*outside_public_boards*
Low attendance	Equals one if the director attends less than 75% of board meetings in a given year and zero otherwise (*attend_less75_pct*).

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