Segment Disaggregation and Equity-Based Pay Contracts

Young Jun Cho
Singapore Management University

Hojun Seo
Purdue University

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Correspondence: Young Jun Cho, School of Accountancy, Singapore Management University, Singapore, Email: yjcho@smu.edu.sg

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ABSTRACT

We study the role of segment disaggregation in equity-based pay contracts in diversified firms. Disaggregated segment disclosures can improve the observability of managerial actions in internal capital markets and thus increase implicit incentives for managers to allocate resources as desired by shareholders, substituting for explicit incentives provided to CEOs. We use the adoption of SFAS 131 as an identification strategy and find that firms affected by this segment reporting mandate significantly decreased the provision of equity-based incentives in the post-adoption period, especially more for firms with higher operating volatilities. This effect is also more pronounced for firms with weaker board monitoring in the pre-adoption period but with stronger external monitoring in the post-adoption period. Overall, our results suggest that disaggregated segment disclosures reduce the use of equity-based pay contracts in diversified firms by enhancing the monitoring of managers.

Keywords: Segment Disclosure, Diversified Firms, SFAS 131, Equity-Based Pay, Flow Delta, Agency Costs

JEL Classification: D80, J30, L20, M12

Data availability statement: Data used in the study are available from public sources as described in the manuscript.

1. INTRODUCTION

We study the role of segment disaggregation in equity-based pay contracts of diversified firms. Since equity grants allow risk-sharing between shareholders and managers, equity-based pay contracts can help reduce agency conflicts arising from the unobservability of managerial actions (Jensen and Meckling, 1976; Bliss and Rosen, 2001; Datta et al., 2001; Minnick et al., 2011). While research in finance suggests that diversified firms suffer from agency costs associated with inefficient internal capital markets (e.g., Stulz, 1990; Berger and Ofek, 1995; Scharfstein, 1998; Scharfstein and Stein, 2000; Rajan et al., 2000), disaggregated segments can expose the actions of their managers, such as internal capital allocation, to outsiders, increasing interferences by external monitors and shareholders (Berger and Hann, 2003; Cho, 2015). We thus argue that segment disaggregation can change the way that boards of directors explicitly incentivize CEOs, as it can increase implicit incentives for managers to allocate resources as desired by shareholders (e.g., Tirole, 2010). Prior literature, however, has paid little attention to the role of segment disclosures in CEO compensation. In this paper, we fill this gap by examining whether segment disaggregation influences the provision of equity-based pay contracts in diversified firms.

The effect of segment disaggregation on the provision of equity in pay contracts is not clear a priori. On the one hand, segment disaggregation improves the observability and thus the external monitoring of managerial actions in internal capital markets, increasing implicit incentives for managers to behave in accordance with shareholders' interests. For example, to the extent that any suboptimal capital allocations are revealed more transparently, boards of directors would be under greater pressure from external monitors and shareholders, disappointed at price declines or displeased with poor performance in subsidized segments, to discipline and potentially remove

CEOs. With more disaggregated segment disclosures, corporate raiders can also better identify undervalued firms, particularly those with unprofitable segments or those with greater room to improve the efficiency in internal capital markets, rendering firms more vulnerable to hostile takeovers. Therefore, given that equity-based incentives are costly (e.g., Murphy, 1999; Core et al., 2003), to the extent that implicit incentives allow firms to achieve the same level of desirable managerial actions, firms would reduce the provision of explicit incentives, such as equity-based pay contracts when they disclose their segments in a more disaggregated manner.[1]

On the other hand, however, firms can award equity grants to managers for reasons other than incentive alignment, for example, to attract managers from CEO labor markets (e.g., Cadman et al., 2021; Edmans et al., 2021). Moreover, shareholders entrust their capital to managers and are willing to cede their decision rights over their assets due to their limited business expertise (Jensen, 1998). Accordingly, even if shareholders could observe all managerial actions (i.e., under perfect monitoring or observability where incentive provision is unnecessary), firms may continue to rely on stock price in setting pay contracts (Jensen, 1998; Prendergast, 2000). Disaggregated segments then may not necessarily decrease the provision of equity-based pay contracts.

We exploit the adoption of SFAS 131 as a source of plausibly exogenous variation in segment disaggregation. SFAS 131 requires firms to define segments as internally viewed by managers, leading to many firms disaggregating segments (e.g., Herrmann and Thomas, 2000; Street et al., 2000). While other regulations and reporting mandates are mainly aimed at improving how to measure or report a firm's ex-post financial performance, SFAS 131 is unique in that it mandates firms to reveal their internal organizational structure to outsiders through the eyes of

[1] Consistent with this argument, Tirole (2010) states that "the threat of dismissal or other interferences resulting from poor performance provides incentives for managers over and beyond those provided by explicit incentives. Explicit and implicit incentives are therefore substitutes: with stronger implicit incentives, fewer stocks and stock options are needed to curb managerial moral hazard" (p. 26).

management. The adoption of SFAS 131 is unlikely to have changed firms' internal information environments but revealed previously hidden information, making cross-segment capital transfers in internal capital markets more observable to external shareholders. Consistent with this notion, Cho (2015) finds that SFAS 131 helped increase the efficiency with which firms allocate internal resources. His result suggests that disaggregated segments can strengthen the external monitoring of managers, increasing implicit incentives for managers to act in shareholders' interests.

We use a CEO's flow delta as our main measure of equity incentives in CEO pay contracts. Based on annual pay, flow delta resembles portfolio delta, except that only stocks and options granted in a given year are included in its calculation (i.e., the change in the value of the CEO's equity-based compensation granted in a given year for a 1% increase in the firm's stock price). Hence, flow delta captures the incentive effect resulting from the annual grants of new equity making CEO wealth more sensitive to shareholder returns. While overall incentive effects are greater from cumulative holdings than annual flows (Jensen and Murphy, 1990), we use flow delta, as is the case with Hayes et al. (2012) and Gormley et al. (2013). Similar to those studies, our objective is to examine whether boards of directors adjust managers' incentives in annual pay in response to certain changes in corporate environments (such as an increase in external monitoring in our setting). Gormley et al. (2013) note that boards of directors quickly react and adjust new annual incentives while total incentives in cumulative holdings are slower to adjust.

To isolate the effect of segment disaggregation, we take a difference-in-differences approach with firm fixed effects. Following Cho (2015), we define our treatment group as a set of diversified firms that changed their segment definitions upon adopting SFAS 131. We define the control group as another set of diversified firms whose reported segments were already consistent with the reporting mandate and therefore did not change. The difference-in-differences research design allows us to estimate the causal effects if the mandatory change in segment reporting is plausibly exogenous to firms in the treatment group. However, firms are not randomly assigned to the treatment and control groups. Prior research shows that firms with higher agency and proprietary costs were more likely to aggregate segments before SFAS 131 (e.g., Botosan and Stanford, 2005; Berger and Hann, 2007; Bens et al., 2011). Hence, we use firm fixed effects throughout this study to control for unobservable time-invariant differences in firm characteristics correlated with firms' segment reporting choices and compensation policies (Cho, 2015).

Based on 4,752 firm-year observations over 10 years around the adoption of SFAS 131, we find a significant decrease in flow delta for CEOs in treatment firms relative to control firms after the change in the reporting mandate. Our result suggests that after accounting for an increasing trend in the use of stock options over our sample period (Murphy, 2013), CEOs in treatment firms receive, on average, $15,598 less compensation for a 1% increase in their firms' stock price after SFAS 131 adoption relative to control firms. This result is robust to using the propensity-score matched (PSM) sample and unlikely to be attributed to a differential timing in adopting equity pay between the treatment and control firms. Using portfolio delta, we also find that CEOs in treatment firms hold less amount of equity incentives from accumulated equity holdings during the post-period than before. Taken together, these results overall suggest that disaggregated segments can reduce the provision of equity incentives due to the enhanced monitoring of managerial actions in internal capital markets.

We next conduct cross-sectional analyses to illuminate the underlying channels. First, we focus on the role of operating volatilities. Our prediction is based on the premise that equity-based pay contracts are costly (e.g., Murphy, 1999; Core et al., 2003) and firms have incentives to minimize monitoring costs, which are to be ultimately born by shareholders (Jensen and Meckling, 1976). CEOs demand higher risk premiums for the use of equity-based pay contracts if firms operate in more volatile environments with less predictable future outcomes (Murphy, 1999). Implicit incentives would then better substitute for explicit incentives when the latter is more costly than the former. Therefore, we expect the reduction in flow delta to be more pronounced if the firm would have provided a greater amount of costly incentives had it not disaggregated its segments. Consistent with this expectation, we find that the decrease in flow delta after the adoption of SFAS 131 is greater for treatment firms with higher sales and stock return volatilities.

Second, if managers were already well monitored internally by boards of directors, the increase in external monitoring could have little impact on managerial behavior. Thus, we expect to find the reduction in flow delta to be more pronounced for treatment firms where board monitoring was weaker before the adoption of SFAS 131. Coles et al. (2014) suggest that co-opted directors appointed by the firm's CEO tend to exhibit allegiance to the CEO, even though they joined the board as independent directors, and that board capture tends to intensify with the director's tenure. Board monitoring is also not very effective if the CEO also holds the position of chairman on the firm's board. Consistent with this expectation, we find that the decrease in flow delta is greater for treatment firms with weaker internal monitoring during the pre-period, as proxied for by a greater proportion of co-opted directors, a greater tenure-weighted proportion of co-opted directors, and the CEO serving as the chairman of the board.

Third, in contrast to internal monitoring during the pre-period, external monitoring during the post-period would be instrumental in promoting implicit incentives stemming from the increased segment reporting transparency. Upon a revelation of inefficient internal capital markets, motivated monitors, such as institutional investors holding the firm in their portfolios with a significant weight (Fich et al., 2015), can express their concerns or preferred actions directly to managers. Financial analysts can also serve as corporate monitors as they can raise questions in conference calls or write reports about inefficient internal capital markets. Moreover, to the extent that inefficient operations are better revealed to corporate raiders with more granular segment reporting (Berger and Hann, 2002), takeover threats can also serve as a form of external monitoring. Consistent with these expectations, we find that the decrease in flow delta is greater for treatment firms owned by institutional investors more motivated to monitor, followed by more financial analysts, and more susceptible to hostile takeover threats in the post-period.

For further insights into our results, we explore whether there are changes in the relevance of segment information for incentive contracting after the adoption of SFAS 131. Bloomfield (2021) suggests that firms use cash-based bonus incentives as a credible signal of their commitment to strategic objectives to external stakeholders. Given that external investors can assess the efficiency in internal capital markets (ICM) by using the information provided in segment disclosures and incorporate their assessments into pricing (e.g., Berger and Hann, 2003; Cho, 2015), we posit that diversified firms would be able to signal their commitment to enhancing ICM efficiency, as one of their strategic objectives, to external investors by linking their CEOs' cash bonuses to ICM efficiency. We find that the CEO's cash-based incentive plans are more highly associated with ICM efficiency for treatment firms after the adoption of SFAS 131. This result is consistent with treatment firms proactively using CEO cash bonuses as a trustworthy way to demonstrate their commitment to improving ICM efficiency upon revealing the existence of previously undisclosed internal capital markets.

Finally, we perform the following robustness checks. First, despite the mandatory nature of accounting standards, firms still have discretion in compliance with SFAS 131. As a result, the control group may include firms that should have changed segment definitions under a neutral application of the standard. The segment reporting literature suggests that firms with higher agency and proprietary costs are more likely to obscure segment information and hence have a higher incentive not to comply with SFAS 131. Our results are robust to excluding firms less likely to comply with the mandate from the control group. Second, we find that our results are robust to (1) a shorter window excluding the post-SOX period, (2) firm-CEO fixed effects, and (3) a subsample of firm-years with the same CEOs remaining constant both before and after SFAS 131. Moreover, we find a significant increase in the proportion of cash compensation to total compensation but a significant decrease in flow vega for firms affected by SFAS 131, providing further evidence consistent with firms reducing the use of equity-based pay with segment disaggregation (results reported in the online appendix).[2]

While our study builds on the agency cost literature, segment disaggregation can reveal a firm's proprietary information and hence hurt its competitive position (e.g., Hayes and Lundholm, 1996), which likely affects incentive contracts. However, research finds a positive relationship between product market competition and equity-based incentives (Hubbard and Palia, 1995; Karuna, 2007, 2020), suggesting that our results are unlikely to be explained by changes in the competitive environment. Moreover, Bens et al. (2018) find an increase in the timeliness of loss recognition in accounting earnings after the adoption of SFAS 131. However, our results are unlikely to be due to the enhanced properties of earnings for contracting purposes, as we do not find an increase in the sensitivity with which a CEO's total compensation varies with earnings after the adoption of SFAS 131 (results untabulated).

[2] We also examine whether treatment firms exhibited increased myopic behavior, such as earnings management, during the post-period. However, we do not find such evidence, inconsistent with SFAS 131 adoption intensifying the focus on reported segment profits and hence creating short-term pressure on CEOs (results reported in the online appendix).

Our study makes the following contributions. First, we contribute to the segment reporting literature by documenting the benefits of segment disaggregation beyond capital market benefits. While studies suggest that segment disaggregation reduces the cost of financing by improving a firm's information environment (e.g., Franco et al., 2016; Akins, 2018), it also strengthens external monitoring, disciplining managers and raising the threat of dismissal unless managers were to improve efficiencies in internal capital allocations (Berger and Hann, 2003; Cho, 2015). We find that segment disaggregation leads to a reduced provision of equity incentives, consistent with enhanced segment disclosures increasing implicit incentives for managers to act as desired by shareholders.[3] We believe our findings are novel and timely as the Financial Accounting Standards Board (FASB) recently updated disclosure rules for operating segments under "a chief operating decision maker (CODM) approach." [4]

Note that we do not intend to imply that external shareholders can directly contract with CEOs. However, to the extent that the improvement in external monitoring under SFAS 131 induced managers to engage in actions more desired by shareholders, as demonstrated by Cho (2015), shareholders would have less need to provide strong but costly incentives to managers via equity-based compensation. Compensation committees would then adjust CEO pay accordingly, as is shown by Gibbons and Murphy (1992), where implicit incentives from career concerns and explicit incentives from the compensation contract jointly determine optimal compensation

[3] Our story is unlikely to be applicable to other shocks to financial reporting requiring more transparent disclosures in general without segment disaggregation. For example, Ozkan et al. (2012) find an increase in the sensitivity of cash compensation to accounting earnings for firms which adopted IFRS, consistent with IFRS adoption improving the contractual usefulness of accounting earnings (i.e., complementarity relationship). However, our result indicates a decrease in the provision of equity incentives as the increased observability of managerial actions increases managers' implicit incentives, reducing the use of explicit incentives (i.e., substitutive relationship).

[4] While still sticking with the chief operating decision maker's view in aggregating segments, the update focuses on how to define segment expense categories and report the amounts (https://www.fasb.org/page/getarticle?uid=fasb_Media_Advisory_11-27-23).

contracts. That is, the explicit incentives from the optimal compensation contract should decline as managers' implicit incentives from career concerns increase.

Second, we contribute to the CEO compensation literature. Understanding the extent to which the observability of managerial actions affects CEO pay contracts is important (Holmström, 1979, 1982), but to our knowledge, empirical evidence about this issue is scant in the literature. While the increase in the observability of internal capital allocations, as revealed through segment disaggregation, can decrease shareholders' demand for a provision of strong incentives to CEOs, firms could still rely on equity-based pay to retain managers by fairly measuring and recognizing managerial performance through stock price (e.g., Cadman et al., 2021; Edmans et al., 2021). Our findings suggest that the observability of managerial actions relates negatively to the provision of equity incentives, consistent with firms using equity-based pay contracts to address the unobservability of managerial actions (Holmström, 1979). However, it is important to note that CEO pay is typically tied to various performance measures, whereas we focus on stock returns. Hence, without further investigation, our findings should not be interpreted as evidence of SFAS 131 reducing the risk embedded in all other performance measures used in CEO pay contracts.

2. LITERATURE REVIEW AND HYPOTHESIS DEVELOPMENT

2.1. Research on SFAS 131

Effective for firms with fiscal years beginning after December 15, 1997, SFAS 131 requires firms to define segments as internally viewed by managers, resulting in firms providing more disaggregated segment information (e.g., Herrmann and Thomas, 2000; Street et al., 2000; Ettredge et al., 2006). Research suggests that SFAS 131 improved firms' information environments. For example, Berger and Hann (2003) find a reduction in analysts' forecast errors for firms affected by SFAS 131. Ettredge et al. (2005) show that SFAS 131 adoption improved the stock market's ability to predict a firm's future earnings. Moreover, Franco et al. (2016) report a decrease in the cost of debt, and Akins (2018) finds a reduction in credit rating disagreement for firms affected by SFAS 131. Jayaraman and Wu (2019) suggest that SFAS 131 increased (decreased) the amount of public (private) information embedded in stock price.

Disaggregated segments can also help improve the external monitoring of managers by rendering managers' actions in internal capital markets more observable to external shareholders. Berger and Hann (2003) find that single-to-multiple firms (i.e., those reported as single-segment firms before SFAS 131 but as multiple-segment firms afterward) suffered a value loss upon adoption, consistent with the new mandate better exposing the agency problems in internal capital markets. Moreover, given that inefficient resource allocations are revealed more transparently and thus penalized more heavily by the market (Berger and Hann, 2003), SFAS 131 made it more costly for managers to acquire, keep, or subsidize losing businesses, a typical agency problem identified in multiple-segment conglomerates (e.g., Stulz, 1990; Berger and Ofek, 1995; Scharfstein, 1998; Scharfstein and Stein, 2000; Rajan et al., 2000). Consistent with this idea, Cho (2015) finds that SFAS 131 increases the efficiency with which firms allocate internal resources across segments as desired by shareholders, suggesting that disaggregated segments can increase implicit incentives for managers to behave in accordance with shareholders' interests in internal capital markets.[5] Berger and Hann (2002) also find that firms affected by SFAS 131 faced a higher likelihood of hostile takeovers, suggesting that the reporting mandate facilitated the market for corporate control.

[5] The value discount reported by Berger and Hann (2003) and the improved efficiency by Cho (2015) do not contradict to each other. The value discount explains why firms have incentives to improve internal capital market efficiency after the adoption of SFAS 131.

2.2. Hypothesis Development

Optimal contracting theory suggests that firms use equity-based pay contracts to provide incentives to managers to act as desired by shareholders, as managerial actions are not fully observable (Holmström, 1979, 1982). However, equity-based pay contracts are costly, as managers bear more risk under such contracts and demand risk premiums.[6] Given that firms have incentives to minimize monitoring costs (Jensen and Meckling, 1976), to the extent that the same level of desirable actions is ensured by an alternative governance mechanism, they would rely less on costly pay contracts. Consistent with this notion, Gibbons and Murphy (1992) show a negative relationship between equity incentives and CEO career concerns, suggesting that the implicit incentives from career concerns substitute for the explicit incentives from pay contracts. That is, with a higher threat of dismissal or implicit incentives, fewer stocks and stock options are needed to address agency problems, as the threat of being dismissed or removed by the market for corporate control keeps CEOs on their toes (Tirole, 2010).

We argue that segment disaggregation enforced by the adoption of SFAS 131 improves the observability and thus external monitoring of managerial actions in internal capital markets, increasing implicit incentives for managers to act as desired by shareholders. Specifically, to the extent that any suboptimal internal capital allocations are revealed more transparently, boards of directors would be under greater pressure from external monitors to discipline and potentially remove CEOs.[7] Moreover, with more disaggregated segments, corporate raiders can better identify

[6] Murphy (1999) reports that executives demand premiums of 20% to 30% for restricted stock and 100% to 200% for stock options when accepting a salary reduction in return for stock-based instruments, which are riskier by nature. Moreover, equity incentives can drive myopic managers to pursue short-term profits at the expense of long-term value (e.g., Cheng and Warfield, 2005, Bergstresser and Philippon, 2006, Efendi et al., 2007). Risk-averse managers can also make suboptimal investments to reduce equity risk in response to increases in equity-based pay (Brick et al., 2012).

[7] Boards of directors dismiss CEOs under the implicit or explicit pressure of shareholders observing low stock prices or low profits (Tirole, 2010). External shareholders can pressure management with the threat of a proxy contest when they find areas in need of improvement. For example, displeased with inefficiencies previously hidden in internal

undervalued firms, particularly those with unprofitable segments or those with greater room to improve the efficiency in internal capital markets, rendering firms more vulnerable to hostile takeover threats or more likely to be targeted by activist hedge funds (Berger and Hann, 2002). Hence, segment disaggregation raises the threat of CEO dismissal unless managers were to improve efficiencies in internal capital allocations. Also, disaggregated segments could allow potential recruiters in CEO labor markets to better understand managerial actions in internal resource allocations (e.g., whether or not the manager they consider hiring exhibited any tendency to subsidize unprofitable pet projects for empire-building purposes). With more disaggregated segments, therefore, CEOs can be better monitored in labor markets, further elevating implicit incentives for managers with career concerns.

While improving external monitoring, SFAS 131 is unlikely to have improved the internal monitoring environment for boards of directors because the incremental information revealed under SFAS 131 was likely internally available beforehand. However, due to the enhanced observability of managerial actions in internal capital markets, to the extent that the improvement in external monitoring increased implicit incentives and induced managers to act as more desired by shareholders, firms would have less need to use costly incentives in motivating managers. They then would adjust CEO pay accordingly in shareholders' interests, where equity incentives decline as managers' implicit incentives from career concerns increase (Gibbons and Murphy, 1992; Tirole, 2010). Therefore, we expect the provision of equity incentives to be lower for firms affected by SFAS 131. This discussion leads to our hypothesis in alternative form as follows:

H1: Equity incentive provisions decreased for firms affected by SFAS 131 adoption.

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capital markets, they can attempt to vote out senior management or vote against the management proposals, including share issuance likely intended to finance value-decreasing expansions. The threat of a proxy fight can provide an implicit incentive for managers to accept shareholders' request so that they can bypass the cost and adverse publicity of any potential confrontations (Yermack, 2010).

However, firms can award equity grants to managers for reasons other than incentive alignment, for example, to attract managers from CEO labor markets (e.g., Cadman et al., 2021; Edmans et al., 2021). Moreover, shareholders entrust their capital to managers and are willing to relinquish their decision rights over their assets because they believe that managers have better skills or knowledge for identifying investment opportunities (Jensen, 1998). Accordingly, even if they could observe all managerial actions (i.e., under perfect monitoring or observability where incentive provision is unnecessary), shareholders may lack the ability to fully evaluate the implications of managerial actions. Firms would then continue to rely on stock price in setting pay contracts to evaluate and compensate managers (Jensen, 1998; Prendergast, 2000). With this tension, the effect of segment disaggregation on equity-based pay contracts is an empirical question.

3. SAMPLE AND RESEARCH DESIGN

3.1. Data and Sample

SFAS 131 was effective for firms with fiscal years beginning after December 15, 1997. To test the effect of SFAS 131, we focus on the five years before and after its adoption as the pre- and the post-periods, respectively, between 1993 and 2004. We exclude the adoption year from our analyses to keep the transition effect from confounding the results. To select our sample, we begin with U.S. firms with multiple business segments whose financial, stock price, and compensation data are available in Compustat, CRSP, and ExecuComp, respectively. We exclude financial firms and regulated utilities since they have different pay practices than other firms (Murphy, 1999).

The treatment group consists of firms that changed their segment definitions upon the adoption of SFAS 131, including single-to-multiple firms (i.e., those reported as single-segment firms before SFAS 131 but as multiple-segment firms afterward) and multiple-to-multiple firms (i.e., those reported as multiple-segment firms both before and after the adoption).[8] The control group consists of firms with multiple business segments that did not change segment definitions with SFAS 131. To determine whether a firm changed its segment definitions, we follow Cho (2015) and compare the segments reported in the last year pre-adoption and those restated in the adoption year. To ensure that the treatment group consists only of firms with pure reporting changes, we follow Berger and Hann's (2003) algorithm, excluding firms from the treatment sample if they were contaminated by events other than pure reporting changes, such as acquisitions, divestitures, or restructurings.[9] This procedure results in 3,205 and 1,547 treatment and control firm-years, respectively, as our final sample.

3.2. Research Design

We perform the difference-in-differences analysis with fixed effects by estimating the following regression model:

$$
\begin{aligned}
\textit{Flow Delta}_{it} = \;& \alpha_1\, \textit{Post}_t \times \textit{Treatment}_i + \alpha_2\, \textit{Age}_{it} + \alpha_3\, \textit{Age}{\geq}60_{it} + \alpha_4\, \textit{Tenure}_{it} \\
& + \alpha_5\, \textit{Size}_{it-1} + \alpha_6\, \textit{MTB}_{it-1} + \alpha_7\, \textit{Lev}_{it-1} + \alpha_8\, \textit{Vol}_{it-1} + \alpha_9\, \textit{Ret}_{it-1} \\
& + \alpha_{10}\, \textit{Ownership}_{it-1} + \alpha_{11}\, \textit{Chair}_{it-1} + \alpha_{12}\, \textit{Interlock}_{it-1} \\
& + \alpha_{13}\, \textit{Peer Ret}_{it-1} + \alpha_{14}\, \textit{Peer Grants}_{it-1} + \alpha_{15}\, \textit{HHI}_{it-1} \\
& + \alpha_{16}\, \textit{Speed of Profit Adj}_{it-1} + \alpha_{17}\, \textit{Earn Pers}_{it-1} + \delta_i + \gamma_j + \eta_t + \varepsilon_{it}
\end{aligned}
\tag{1}
$$

Flow Delta is a CEO's flow delta, our proxy for a firm's provision of equity-based incentives, calculated as the change in the value of the CEO's equity compensation granted in a given year (expressed in thousands of dollars) for a 1% increase in the firm's stock price (e.g., Hayes et al.,

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[8] A handful of multiple-to-single firms (i.e., those reported as multiple-segments firms before SFAS 131 but as single-segments firms afterward) are not included in the treatment sample. However, we include multiple-to-multiple firms that decreased or did not change the number of segments upon adopting SFAS 131 because the newly defined segments under SFAS 131 are better aligned with the spirit of segment disaggregation, being more consistent with the firm's internal organizational structures. Our inferences do not change when removing those firms from the sample.

[9] Berger and Hann (2003) compare the sums of segment revenues (and earnings) for the last year in the pre-period between the restated data and historical reports, considering firms as contaminated by events other than pure reporting changes if the difference between the two sums is greater than 1% of the restated sum.

2012; Gormley et al., 2013). *Post* × *Treatment* is a product of *Post* and *Treatment*. *Post* is an indicator variable that equals one for the period after the adoption of SFAS 131 and zero otherwise. *Treatment* is an indicator variable that equals one for a treatment firm and zero otherwise.[10] We expect to find a significantly negative coefficient on *Post* × *Treatment* under H1.

Gibbons and Murphy (1992) show that firms provide stronger explicit incentives as CEOs approach retirement as they have lower career concerns. As such, we include *Age*, *Age⩾60*, and *Tenure* in the regression equation. While *Age* captures the CEO's age, we further include *Age≥60*, an indicator variable that equals one if the CEO's age is higher than or equal to 60 and zero otherwise, to allow for a non-linear effect of the CEO's proximity to retirement (e.g., Parrino, 1997; Peters and Wagner, 2014; Lin et al., 2022). We expect to find a positive coefficient on *Age≥60*. Gibbons and Murphy (1992) predict that once career concerns are taken into account, longer-tenured CEOs will receive greater explicit incentives. However, if CEOs with longer tenure have already accumulated a large amount of equity incentives, the demand for additional equity incentives would be lower (Core and Guay, 2010). Thus, the prediction of the coefficient on *Tenure* is not clear ex-ante.

Following prior studies (e.g., Smith and Watts, 1992; Core et al., 1999; Core et al., 2008; Cadman et al., 2021), we also include a battery of firm characteristics to control for factors that may influence a firm's reporting decision with SFAS 131 and the provision of equity incentives, such as firm size (*Size*), the market-to-book ratio (*MTB*), leverage (*Lev*), stock volatility (*Vol*), stock returns (*Ret*), CEO share ownership (*Ownership*), whether or not the CEO is the firm's board chair (*Chair*), and whether or not the CEO is involved in an interlocking relationship (*Interlock*).[11] In addition, we control for several measures of industry characteristics and competitiveness: peers'

[10] *Post* and *Treatment* are removed from the regression due to collinearity with firm and year fixed effects.
[11] A CEO is involved in an interlocking relationship if the CEO serves on the firm's compensation committee.

average stock returns (*Peer Ret*), peers' average equity grants (*Peer Grants*), the concentration ratio (*HHI*), the speed of profit adjustment (*Speed of Profit Adj*), and the earnings persistence (*Earn Pers*) of the industry in which the firm operates.[12] Firms benchmark industry peers' compensation policies in setting CEO pay (Cadman et al., 2021). Also, industry competitiveness is likely correlated with a firm's propensity for segment aggregation (Harris, 1998) and thus its decision to change segment definitions upon adopting SFAS 131 (Berger and Hann, 2007). Δ_i, γ_j, and η_t represent firm, industry, and year fixed effects, respectively.[13] The appendix provides details on the variable construction.

The segment reporting literature identifies agency and proprietary costs as the primary motives behind a firm's decision to aggregate segments before the adoption of SFAS 131 (e.g., Botosan and Stanford, 2005; Berger and Hann, 2007; Bens et al., 2011), suggesting that the treatment group may consist of more firms with higher agency or proprietary costs. However, this would rather cause a bias against our findings as firms with higher agency costs would demand greater use of equity incentives. Prior studies also find that firms with higher competition provide greater equity incentives to CEOs (e.g., Hubbard and Palia, 1995; Karuna, 2007, 2020). Nonetheless, in Equation (1), we include firm fixed effects to control for unobservable time-invariant firm-specific factors. To the extent that the agency and proprietary cost factors are unlikely to change year by year, any differences in those factors between the two groups are unlikely to explain our results.

4. EMPIRICAL RESULTS

[12] For the concentration ratio, we use Hoberg and Phillips' (2016) 10-K text-based network industry classification HHI, which can be downloaded at https://hobergphillips.tuck.dartmouth.edu/industryconcen.htm.

[13] We control for industry fixed effects in addition to firm fixed effects because firms can change their primary industries over our sample period.

4.1. Descriptive Statistics

Table 1 reports the descriptive statistics. During the pre-period, the mean of *Flow Delta* is 31.024 (26.488) for treatment firms (control firms), suggesting that a 1% increase in stock price, on average, leads to an increase in the value of CEO pay by $31,024 ($26,488). During this period, *Flow Delta* is significantly higher for treatment firms than for control firms, suggesting that treatment firms provided a greater amount of equity incentives to their CEOs. This result is consistent with treatment firms with more opaque disclosures before the adoption of SFAS 131 suffering from higher agency conflicts.[14] Although both groups experience an increase in *Flow Delta* from the pre- to the post-period (due to the increasing use of stock options over our sample period), the difference in *Flow Delta* between the two groups narrows and becomes insignificant during the post-period.[15]

In Column (7) of Table 1, the result of a univariate difference-in-differences test exhibits a weakly significant decrease in *Flow Delta* for treatment firms, relative to control firms, after the adoption of SFAS 131. We also note that *Vol* increased significantly more for treatment firms than control firms. To the extent that firms with higher stock volatility are less likely to use equity-based pay contracts, this result raises a possibility that the decrease in *Flow Delta* is driven by the increase in *Vol*. In an untabulated analysis, however, using a matched sample based on *Vol* and industry, we continue to find a consistent result, suggesting that the increase in *Vol* is unlikely to

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[14] This result is also consistent with Cho's (2015) finding that treatment firms allocated internal capital less efficiently than control firms before the adoption of SFAS 131 due to higher agency costs as implied in their more opaque segment disclosure policies.

[15] Murphy (2013) reports that the median CEO pay in S&P 500 firms increased by more than three times between 1992 and 2001, primarily driven by an explosion in the use of stock options, which is attributable to shareholder pressure for equity-based pay, SEC holding-period rules, SEC option disclosure rules, Clinton's $1 million deductibility cap, accounting rules for options, and NYSE listing requirements.

drive our result.[16] We also observe a significant decrease in *Speed of Profit Adj* for treatment firms, relative to control firms, consistent with increased competition. However, our results are unlikely to be explained by higher competition because an increase, not a decrease, in *Flow Delta* is expected as firms face higher competition (Hubbard and Palia, 1995; Karuna, 2007, 2020).[17]

Table 2 reports the Pearson correlation coefficients. We find that *Flow Delta* is positively correlated with *Size*, *MTB*, and *Ret*, suggesting that firms with higher market capitalization, higher growth opportunities, and higher stock performance are likely to provide more equity incentives. *Flow Delta* is also positively correlated with *Peer Grants*, consistent with firms tending to benchmark equity grants to their peer firms (Cadman et al., 2021). In contrast, *Flow Delta* is negatively correlated with *Age*, *Age⩾60*, and *Tenure*, inconsistent with the prediction implied in Gibbons and Murphy (1992). It is also negatively correlated with *Vol*, consistent with equity incentives being more costly for firms with higher volatilities.

4.2. Main Analysis of Flow Delta

Table 3 presents the results of the regression estimating Equation (1). In Column (1) with no control variables, the coefficient on *Post* × *Treatment* is -14.826, significantly negative at the 5% level. Given the increasing trend of *Flow Delta* as shown in Table 1, primarily due to the increasing popularity of stock options during our sample period (Murphy, 2013), the negative coefficient suggests that the increase in *Flow Delta* for treatment firms is not as large as that for control firms. In other words, after controlling for the increasing trend in the use of stock options, a firm's use of equity incentives decreased for treatment firms after the adoption of SFAS 131. In

[16] Unlike *Vol*, *Ret* is not significantly different between the two groups, mitigating the possibility that the decrease in *Flow Delta* is driven by the value discount experienced by single-to-multiple firms right after the SFAS 131 adoption (Berger and Hann, 2003).
[17] Furthermore, when we split the sample into two groups based on the level of competition facing the firm in the product market (Li et al., 2012), we do not find a significant difference in the treatment effect between the two groups, suggesting that our results are unlikely explained by competition (results untabulated).

Column (2) with control variables, the coefficient on *Post* × *Treatment* is -15.598, significantly negative at the 1% level. This coefficient suggests that the CEOs in treatment firms receive, on average, $15,598 less compensation for a 1% increase in stock price than those in control firms after the change in the reporting mandate.[18] Given that the sample mean of *Flow Delta* during the pre-period is 29.489 (averaged across both the treatment and control firms), the drop in equity incentives after the adoption of SFAS 131 is economically non-trivial.

While Gibbons and Murphy (1992) suggest that CEOs with fewer years remaining before retirement receive greater explicit incentives, the coefficient on *Age≥60* is insignificant. The coefficient on *Tenure* is significantly negative, suggesting that longer-tenured CEOs receive less explicit incentives, possibly due to such CEOs having already accumulated a large amount of equity incentives (Core and Guay, 2010). The coefficient on *Lev* is also significantly negative, consistent with higher leverage contributing to monitoring and substituting for incentive contracts. The coefficients on *Size* and *MTB* are significantly positive, indicating that firms with higher market capitalization and growth options are likely to provide more equity incentives to their CEOs. The coefficient on *Peer Grants* is also significantly positive, consistent with firms tending to benchmark equity grants to their peers (Cadman et al., 2021).

Overall, the results reported in Table 3 are consistent with H1, suggesting that segment disaggregation improves external monitoring, reducing the provision of equity-based incentives, consistent with implicit incentives substituting for explicit incentives.[19]

4.3. Parallel Trend Assumption

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[18] The coefficients on *Post* × *Treatment* are similar between the two columns, suggesting that we are unlikely to suffer from bad control problems (Angrist and Pischke, 2009).

[19] Enhanced disclosure under SFAS 131 can reveal bad news for treatment firms, such as lower segment profitability or inefficient internal capital markets (Berger and Hann, 2003, 2007), raising a possibility that our finding is driven by treatment firms performing more poorly in the post-period. However, when using a sample where the treatment and control firms are matched based on the level of stock returns, we find a consistent result (results untabulated).

Figure 1 plots the average *Flow Delta* for the treatment and control firms separately in each year during our sample period. Panels A and B show the results when we use the simple and the conditional average of *Flow Delta* (i.e., the average residual from the regression of *Flow Delta* on a set of firm and industry characteristics included in Equation (1)), respectively. In both Panels A and B, we do not find a differential trend in *Flow Delta* between the two groups before the adoption of SFAS 131, which is aligned with the parallel trend assumption. Additionally, to reinforce our inference of the parallel trend, we create a set of indicator variables, *Pre-1*, *Pre-2*, *Pre-3*, and *Pre-4*, equal to one for the observation that is one, two, three, and four year(s) before the adoption of SFAS 131, respectively, and zero otherwise. We then re-estimate Equation (1) after adding the interactions of these indicators with *Treatment*. As reported in Table 4, in both Columns (1) and (2), we find that these interaction variables are all insignificant, suggesting that relative to the first year in our sample period, treatment firms did not experience a significantly differential change in *Flow Delta* compared to control firms in any of the years leading up to SFAS 131 adoption.

4.4. Propensity-Score Matching

In an additional effort to control for the observable differences between the treatment and control groups, we perform a propensity-score matching (PSM) analysis. We first estimate a probit model and calculate the probability that a firm changes its segment definitions upon adopting SFAS 131, conditional on a group of control variables in Equation (1), i.e., covariates correlated with a firm's segment reporting decision and *Flow Delta*. We then carry out one-to-one nearest-neighbor matching by finding, for each firm in the treatment group, a control firm that is as close to the conditional probability (estimated from the probit model) as possible across all permutations of pairs without replacement. Table 5 reports the estimation results using the PSM sample. In Panel A, we report the descriptive statistics after PSM matching and find that none of the mean values of covariates differs significantly between the treatment and control groups during the pre-period.[20] In Panel B of Table 5, we estimate Equation (1) using this sample and find that the coefficient on *Post* × *Treatment* is significantly negative in both Columns (1) and (2), suggesting that our results are robust to using the PSM sample. In particular, given that *Flow Delta* during the pre-period is no different between the two groups in the PSM sample, this result mitigates the possibility that our finding is attributable to treatment firms accelerating and slowing down the use of equity pay in the pre- and post-period, respectively.[21]

4.5. Analysis of Portfolio Delta

Flow delta creates incentive effects as it makes CEO wealth more sensitive to stock returns. Then CEO's accumulated equity incentives, as proxied for by portfolio delta, would also be lower for treatment firms after the adoption of SFAS 131, compared to the change experienced by control firms. To validate this claim, we re-estimate Equation (1) after replacing *Flow Delta* with *Portfolio Delta*, measured as the change in the value of a CEO's cumulative equity portfolio (expressed in thousands of dollars) for a 1% increase in the firm's stock price (e.g., Core and Guay, 1999, 2002; Coles et al., 2006). In Table 6, we find that the coefficient on *Post* × *Treatment* is significantly negative for *Portfolio Delta* in both the full and the PSM sample in Columns (1) and (2), respectively. The coefficient of -195.417 in Column (1) suggests that CEOs in treatment firms have, on average, $195,417 less incentives in their equity portfolios for a 1% increase in stock

[20] The number of firm-years differs slightly between the two groups after matching because we estimate the propensity score for each firm (not firm-year).

[21] Additionally, we construct an alternative matched sample, where the treatment and control firms are matched based on *Flow Delta* measured during the pre-period. If our finding is driven solely by treatment firms achieving a higher level of equity incentives earlier in the pre-period and slowing down later in the post-period (while control firms caught up on equity pay mainly in the post-period), we should not find a consistent result when using this matched sample. However, we continue to find a consistent result, further mitigating the possibility that our finding is due to equity pay having grown more rapidly early in the sample period for treatment firms than control firms.

price than those in control firms after the adoption of SFAS 131.[22] Interestingly, the coefficient on *Age≥60* is significantly positive in Column (1), consistent with the prediction provided by Gibbons and Murphy (1992). Overall, the results in Table 6 suggest that our inference remains robust to using *Portfolio Delta* as an alternative measure of incentive strength.

5. ADDITIONAL ANALYSES

5.1. Cross-Sectional Test: Operating Volatilities

A firm's reduction in flow delta would be greater if the firm would have provided a greater amount of costly incentives had it not disaggregated its segments. For equity-based pay contracts whose value is contingent on risky outcomes, CEOs will demand higher risk premiums if they find it more difficult to predict future outcomes due to higher uncertainties. Hence, the use of equity incentives should be more costly for firms operating in a more uncertain business environment (e.g., Murphy, 1999; Core et al., 2003). We thus conduct a set of cross-sectional analyses, where we split the sample into two groups based on operating volatilities measured for each firm during the pre-period. For this analysis, we add an indicator variable, *High Volatility*, and its interaction with *Post* and *Post* × *Treatment* to Equation (1).[23] Table 7 reports the results of this analysis.

In Column (1), *High Volatility* equals one for firms whose sales volatility during the pre-period is above the sample median and zero otherwise. We measure sales volatility as the standard deviation of annual sales revenues over the preceding ten years on a rolling basis each year. We then compute the average sales volatility for each firm across each year in the pre-period. In

[22] Given that *Portfolio Delta* measured after the adoption of SFAS 131 is contaminated by stocks and options awarded before SFAS 131 adoption, in an untabulated analysis, we restrict the sample to the last three years' observations in both the pre- and post-periods (i.e., years -3, -2 and -1 for the pre-period and +3, +4 and +5 period for the post-period). We find that the coefficient on *Post* × *Treatment* is -299.03, significantly negative at the 1% level (results untabulated).
[23] *Treat, Post, High Volatility* and *High Volatility* × *Treatment* are subsumed by firm and year fixed effects, so they are dropped from the regression. A similar approach is employed in the subsequent cross-sectional tests related to internal and external monitoring.

Column (2), *High Volatility* equals one for firms whose stock return volatility measured similarly during the pre-period is above the sample median and zero otherwise. In Columns (1) and (2), the variable of interest is *High Volatility* × *Post* × *Treatment*, whose coefficient captures the change in *Flow Delta* of treatment firms with high operating volatilities after the adoption of SFAS 131 incremental to that of treatment firms with low operating volatilities. We find that the coefficient on this variable is significantly negative at the 5% level in Columns (1) and (2), suggesting that the decrease in *Flow Delta* is more pronounced for treatment firms with higher operating uncertainties compared to those with lower operating uncertainties.

5.2. Cross-Sectional Test: Internal Monitoring by Boards

If managers were well monitored by boards of directors during the pre-period, the increase in external monitoring could have little impact on managerial behavior. Then the reduction in flow delta would be greater for firms with weaker board monitoring during the pre-period. We thus conduct a set of cross-sectional analyses, where we split the sample into two groups based on the weakness of board monitoring measured for each firm during the pre-period. For this analysis, we add an indicator variable, *Low Internal Monitoring*, and its interaction with *Post* and *Post* × *Treatment* to Equation (1). Table 8 reports the results of this analysis.

In Column (1), *Low Internal Monitoring* equals one if Coles et al.'s (2014) measure of *Co-option*, i.e., the number of co-opted directors divided by the total number of directors, averaged during the pre-period, is above the sample median and zero otherwise. Co-opted directors refer to the board members appointed after the CEO assumed office. Coles et al. (2014) find that co-opted directors tend to exhibit allegiance to the CEO and thus perform weak monitoring even though they joined the board as independent directors. In Column (2), *Low Internal Monitoring* equals one if *Tenure-Weighted Co-option*, i.e., the tenure-weighted fraction of co-opted directors, averaged during the pre-period, is above the sample median and zero otherwise, as board capture tends to intensify with directors' tenure. In Column (3), *Low Internal Monitoring* equals one if the indicator of CEO duality, averaged during the pre-period, is above the sample median and zero otherwise. Board monitoring is likely weaker when the CEO serves a dual role, i.e., when the CEO holds the position of chairman on the firm's board.

The variable of interest is *Low Internal Monitoring* × *Post* × *Treatment*. We find that the coefficient on this variable is significantly negative at the 5% level in all columns, suggesting that the decrease in *Flow Delta* after the adoption of SFAS 131 is more pronounced for treatment firms that suffered more from weaker board monitoring before the adoption of SFAS 131.

5.3. Cross-Sectional Test: External Monitoring Mechanisms

We next examine the role of external monitoring mechanisms. In contrast to internal monitoring during the pre-period, external monitoring during the post-period would be instrumental in promoting implicit incentives stemming from the increased segment reporting transparency. Then the reduction in flow delta would be greater for firms with stronger external monitoring mechanisms measured during the post-period. We thus conduct a set of cross-sectional analyses, where we split the sample into two groups based on the strength of external monitoring measured for each firm during the post-period. For this analysis, we add an indicator variable, *High External Monitoring*, and its interaction with *Post* and *Post* × *Treatment* to Equation (1). Table 9 reports the results of this analysis.

In Column (1), *High External Monitoring* equals one if the number of motivated monitors, averaged during the post-period, is above the sample median and zero otherwise. Institutional investors can directly express their concerns or preferred actions to managers, or they can have private discussions with corporate boards in the absence of managers (e.g., Carleton et al., 1998;

McCahery et al., 2016). Fich et al. (2015) find that an institution is more motivated to monitor a firm when the institution has the firm in its portfolio with a weight exceeding 10% of the total value of the portfolio. Hence, upon the detection of unprofitable segments or inefficient capital allocation, they are more likely motivated to initiate a proxy fight.[24]

In Column (2), *High External Monitoring* equals one if the number of analysts following, averaged during the post-period, is above the sample median and zero otherwise. Research suggests that financial analysts serve as corporate monitors (e.g., Yu, 2008; Irani and Oesch, 2013; Chen et al., 2015). They can provide direct monitoring through their interactions with managers or perform indirect monitoring by disseminating their reports to the market.[25] Given that financial analysts facilitate information flows from the firm to external shareholders, to the extent that SFAS 131 benefits financial analysts with disaggregated segment information (Berger and Hann, 2003), the new reporting mandate can also strengthen the role of financial analysts as corporate monitors.

In Column (3), *High External Monitoring* is defined based on Cain et al.'s (2017) H-Index, a firm-level measure of susceptibility to hostile takeovers. *High External Monitoring* equals one if the average H-Index in the post-period is above the sample median and zero otherwise. Berger and Hann (2002) find that firms affected by SFAS 131 faced a higher likelihood of takeover, suggesting that SFAS 131 adoption facilitated the market for corporate control. Unless firms take corrective actions, inefficiencies in internal capital markets are more transparently revealed to

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[24] The pre-period in our sample ends prior to the adoption of Regulation Fair Disclosure, before which selective disclosures were more prevalent (e.g., Bailey et al., 2003; Gintschel and Markov, 2004; Bhojraj et al., 2012). When identifying motivated institutions, we exclude blockholders. Like insiders, blockholders were likely to be informed about SFAS 131-type information even before the adoption of the standard and hence were likely not as surprised as non-blockholders by the segment information revealed. In untabulated tests, we find that our results are robust to including blockholders when identifying motivated monitors.

[25] For example, financial analysts can raise questions in conference calls, including why the firm transferred internal capital from a segment with higher opportunities to another with lower opportunities. Analyst reports can also help investors become more knowledgeable and better able to detect managerial misbehavior when evaluating internal capital allocations.

corporate raiders under SFAS 131, raising a possibility that CEOs lose their jobs. Hence, the increase in implicit incentives would be greater for firms more susceptible to hostile takeovers.

The variable of interest is *High External Monitoring* × *Post* × *Treatment*. We find that the coefficient on this variable is significantly negative at the 5%, 1%, and 10% levels, in Columns (1), (2), and (3), respectively, suggesting that the decrease in *Flow Delta* is more pronounced for treatment firms owned by institutions more motivated to monitor, followed by more financial analysts, and more susceptible to hostile takeovers. Overall, these results reinforce our inference and alleviate the concern that the decrease in *Flow Delta* could be driven by treatment firms with a poor governance system lowering the incentive power to please CEOs in the post-period.

5.4. Segment Information for Incentive Contracting

While our findings so far indicate that enhanced segment disclosures bring about a change in the way CEOs are incentivized between explicit vs. implicit incentives, we further explore if there are also changes in the relevance of segment information for incentive contracting after the adoption of SFAS 131. Bloomfield (2021) suggests that firms use cash-based bonus incentives as a credible signal of their commitment to strategic objectives to external stakeholders. Given that external investors can assess the efficiency in internal capital markets (ICM) by using the information provided in segment disclosures and incorporate their assessments into pricing (e.g., Berger and Hann, 2003; Cho, 2015), we posit that diversified firms would be able to signal their commitment to enhancing ICM efficiency, as one of their strategic objectives, to external investors by linking their CEO's cash bonus to ICM efficiency. If so, treatment firms would be better suited to do so following the adoption of SFAS 131 as segment information better reflects internal organizational structures and thus ICM efficiency.

To measure ICM efficiency, we follow prior studies and calculate excess capital expenditures at the segment level, defined as the difference between the segment's capital expenditures and its cash flows (Billett and Mauer, 2003; Berger and Hann, 2003, 2007), which is further adjusted by the firm's excess capital expenditures to control for investments financed by the firm's retained cash or external financing.[26] We use the adjusted excess capital expenditures as a proxy for the segment's investments subsidized by the firm's other segments. We consider the subsidized investment an inefficient capital allocation to the segment if the profitability of the segment receiving the subsidy is not above the asset-weighted average profitability of the firm's other segments. Alternatively, we also regard the subsidy as inefficient if the segment's *q* is not above the asset-weighted average *q* of the firm's other segments.[27] We then measure *ICM_Eff_1* as the proportion of segments with inefficient capital subsidies within a firm (based on either segment profitability or segment *q*), multiplied by (-1).

Additionally, we measure *ICM_Eff_2* as the average of subsidy efficiency across all segments within a firm, where subsidy efficiency at the segment level is calculated as the segment's adjusted excess capital expenditures scaled by segment assets, multiplied by (+1) if the segment's profitability (or the segment's *q*) is above the asset-weighted average profitability (or the asset-weighted average *q*) of the firm's other segments and by (-1) otherwise. To alleviate the potential influence of measurement errors, we use a decile ranking of *ICM_Eff_2* for the regression analysis.

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[26] Segment cash flows are proxied for by the sum of segment profits and segment depreciation. In cases where segment cash flows exceed segment capital expenditures, the segment's excess capital expenditures are considered zero. The segment's adjusted excess capital expenditures are calculated by subtracting the firm's excess capital expenditures from the segment's excess capital expenditures. If the firm's excess capital expenditures exceed the segment's excess capital expenditures, the segment's adjusted capital expenditures are considered zero.
[27] We follow prior studies and define segment *q* as the median *q* of single-segment firms operating in the same industry (e.g., Berger and Ofek, 1995; Cho, 2015). A segment's industry is determined based on the narrowest SIC code that includes at least five single-segment firms with available *q*. A firm's *q* is calculated as the sum of the market value of common stock, the book value of preferred stock and the book value of debt), divided by the book value of total assets.

To test the relevance of ICM efficiency, we estimate the following equation:

$$\begin{aligned} Cash\ Bonus_{it} = \ & \alpha_1 Post_t \times Treatment_i + \alpha_2 ICM\ Efficiency_{it} \\ & + \alpha_3 ICM\ Efficiency_{it} \times Post_t + \alpha_4 ICM\ Efficiency_{it} \times Treatment_i \\ & + \alpha_5 ICM\ Efficiency_{it} \times Post_t \times Treatment_i + \alpha_6 ROA_{it} + Controls \\ & + \delta_i + \gamma_j + \eta_t + \varepsilon_{it} \end{aligned} \quad (2)$$

The dependent variable is *Cash Bonus*, which is the natural logarithm of the CEO's annual cash-based bonus incentives. *ICM Efficiency* is either *ICM_Eff_1* or *ICM_Eff_2*. In addition to *Post* × *Treatment*, we include the interactions of *ICM Efficiency* with *Post*, *Treatment*, and *Post* × *Treatment*. Note that we use the same set of control variables and fixed effects in Equation (1), except that we additionally control for the firm's *ROA*.[28]

Table 10 reports the results of the analysis estimating Equation (2). In Panel A, we report the results when *ICM Efficiency* is measured based on segment profitability. In Column (1), when using *ICM_Eff_1*, we find that the coefficient on *ICM Efficiency* is significantly positive at the 5% level, consistent with control firms signaling to external investors their commitment to enhancing ICM efficiency by linking CEO cash bonuses to segment information during the pre-period. The coefficient on *ICM Efficiency* × *Treatment* is significantly negative at the 10% level, suggesting that treatment firms with more opaque segment disclosures during the pre-period were less motivated to signal their commitment to ICM efficiency. However, the coefficient on *ICM Efficiency* × *Post* × *Treatment* is significantly positive at the 5% level, suggesting that cash bonuses are more strongly tied to segment information for treatment firms after the adoption of SFAS 131. We find similar results in Column (2) when using *ICM_Eff_2*. In Panel B, when *ICM Efficiency* is based on segment *q*, we continue to find a significantly positive coefficient on *ICM Efficiency* × *Post* × *Treatment* in both Columns (1) and (2). Taken together, the results in Table 10 suggest that

[28] We control for the firm's *ROA* to ensure that *ICM Efficiency* included in the regression can capture incremental information on managerial actions over and above firm-level earnings. Due to the measurement of *ICM Efficiency*, this analysis is restricted to firms that provided segment information during both the pre- and post-periods.

the adoption of SFAS 131 increases the relevance of segment information for incentive contracting as it better reveals internal capital markets and their efficiency.

5.5. Control Firms Noncompliant with Segment Reporting Mandate

Despite the mandatory nature of accounting standards, firms can exercise discretion and restructure internal reporting not to comply with SFAS 131. The literature suggests that firms with greater agency or proprietary costs are more likely to hide segment information and not comply with the reporting mandate. A firm's discretion in compliance with SFAS 131 is less of an issue for treatment firms. Given that firms with greater agency or proprietary costs have a stronger incentive to provide equity incentives if the treatment group includes firms that failed to fully comply with SFAS 131, we would find an increase, not a decrease, in *Flow Delta*. However, if a firm avoided complying with SFAS 131 and remained a control firm due to agency or proprietary cost concerns, including such a firm in the control group causes a bias in favor of our findings.

To address this concern, we take an approach similar to that used by Cho (2015). First, given that firms with weaker boards are more likely to suffer from higher agency costs, we classify a control firm as one with a higher likelihood of noncompliance if it belongs to the top quartile in the distribution of *Co-option* in our sample of control firms during the pre-period. Similarly, using Li et al.'s (2012) measure of competition, we also classify a control firm as one with a higher likelihood of noncompliance if it belongs to the top quartile in the distribution of competition during the pre-period. Columns (1) and (2) of Table 11 report the results estimating Equation (1) after removing control firms with a higher likelihood of noncompliance based on agency and proprietary costs, respectively. We find that the coefficient on *Post* × *Treatment* is significantly negative in both Columns (1) and (2). Also, in Column (3) of Table 11, when we remove control firms less likely to comply with the reporting mandate based on either agency or proprietary costs, we continue to find a similar result. Overall, the results in Table 11 suggest that our inferences are robust to excluding control firms less likely to comply with SFAS 131.

6. CONCLUSION

Using SFAS 131 adoption as an identification strategy, we examine the role of segment disaggregation in equity-based pay contracts of diversified firms. Disaggregated segments increase the observability and thus the monitoring of managerial actions in internal capital markets, which can decrease the provision of equity incentives to motivate CEOs. Consistent with this idea, we find a significant decrease in flow delta for CEOs in treatment firms relative to control firms after the adoption of SFAS 131. We perform cross-sectional tests and find that the reduction in flow delta in the post-period is more pronounced for firms with more volatile operating environments during the pre-period, for firms with weaker internal monitoring by boards during the pre-period, and for firms with stronger external monitoring mechanisms during the post-period.

We contribute to the segment reporting literature by documenting the effect of segment disaggregation beyond capital market benefits. Consistent with SFAS 131 adoption improving monitoring, our evidence suggests that segment disaggregation can substitute for the use of equity incentives to motivate managers. We also contribute to the compensation literature. Understanding the extent to which the observability of managerial actions affects CEO pay contracts is important but, to our knowledge, empirical evidence about this issue is scant in the literature. While the role of the observability of managerial actions in equity-based pay contracts is not clear a priori, we find that the observability relates negatively to the provision of equity-based incentives, consistent with firms using equity-based pay contracts to address the unobservability of managerial actions.

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APPENDIX
Variable Definitions

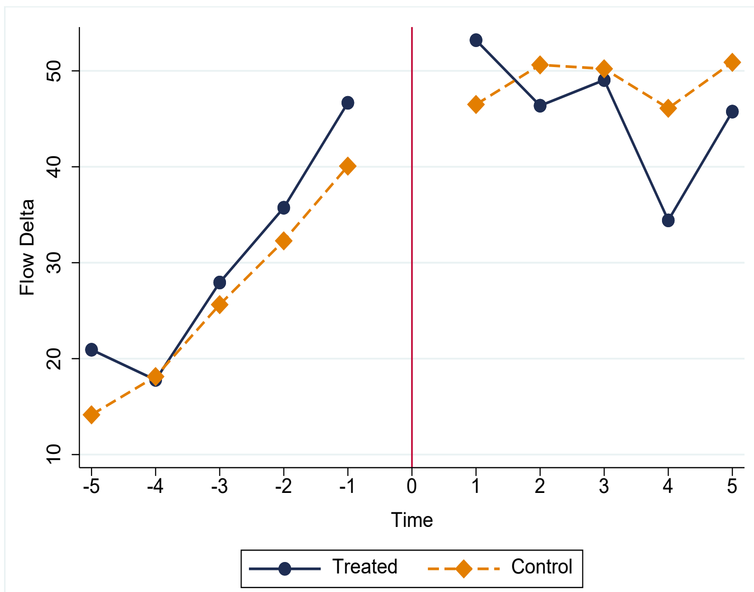
Variables in the Main Analysis	
Flow Delta	the change in the value of CEO compensation granted in a given year (expressed in thousands of dollars) for a 1% increase in the firm's stock price.
Post	an indicator variable that equals one for the post-period and zero otherwise.
Treatment	an indicator variable that equals one if the firm changed segment definitions upon adopting SFAS 131 and zero otherwise.
Age	the natural logarithm of one plus CEO age.
Age≥60	an indicator variable that equals one if the CEO's age is higher than or equal to 60 and zero otherwise.
Tenure	the natural logarithm of one plus CEO tenure (i.e., the number of years since the firm's CEO took office).
Size	the natural logarithm of one plus sales.
MTB	the market-to-book ratio of total assets, measured as the market value of assets (total assets plus the market value of common equity minus the book value of common equity) divided by total assets.
Lev	the total debt (short-term debt plus long-term debt) divided by total assets.
Vol	the standard deviation of abnormal daily returns for a given year. Abnormal daily returns are calculated as the residuals of firm-year specific regressions of daily stock returns on daily market returns in excess of the risk-free rate.
Ret	the buy-and-hold annual stock returns.
Own	CEO ownership (i.e., the number of shares owned by the firm's own CEO divided by the number of total shares outstanding). This variable takes a value of zero if it is missing.
Chair	an indicator that equals one if the firm's CEO is the board chair and zero otherwise.
Interlock	an indicator variable that equals one if the firm's CEO is involved in an interlocking relationship and zero otherwise. A CEO is involved in an interlocking relationship if the CEO serves on the compensation committee of the firm. This variable takes a value of zero if it is missing.
Peer Ret	Industry peer firms' average buy-and-hold annual stock returns (based on the three-digit SIC industries).
Peer Grants	Industry peer firms' average equity grants (based on the three-digit SIC industries) are measured as the grant date fair value of annual restricted stock and option grants.
HHI	Hoberg and Phillips' (2016) 10-K text-based network industry classification HHI.
Speed of Profit Adj	the speed of profit adjustment in the industries in which the firm operates (based on the three-digit SIC industries). Following Harris (1998) and Botosan and Stanford (2005), the speed of profit adjustment is calculated by estimating the following equation in each industry over the prior 20-year period: $X_{ijt} = \beta_{0j} + \beta_{1j}(D_n X_{ijt-1}) + \beta_{2j}(D_p X_{ijt-1}) + \varepsilon_{ijt}$, where X_{ijt} is the difference between firm *i*'s profit and the median profit for its industry *j* in year *t*. D_n equals one if X_{ijt-1} is not positive and 0 otherwise. D_p equals one if X_{ijt-1} is positive and 0 otherwise. The firm's profit is defined as the earnings before interest and taxes divided by the beginning-of-period total assets. The slope coefficient, β_{2j}, captures the speed of profit adjustment in industry *j*.
Earn Pers	the persistence of abnormal earnings in the industries in which the firm operates (based on the three-digit SIC industries). The persistence of abnormal earnings is

	calculated by estimating the following equation in each industry over the prior 20-year period: $X_{ijt} = \beta_{0j} + \beta_{1j} X_{ijt-1} + \varepsilon_{ijt}$, where X_{ijt} is the difference between firm *i*'s profit and the median profit for its industry *j* in year *t*. The firm's profit is defined as the earnings before interest and taxes divided by the beginning-of-period total assets. The slope coefficient, β_{1j}, captures the persistence of abnormal earnings in industry *j*.
Variables in Other Analyses	
Portfolio Delta	the change in the value of a CEO's cumulative equity portfolio (expressed in thousands of dollars) for a 1% increase in the firm's stock price.
Cash Bonus	the natural logarithm of one plus the CEO's cash bonus.
ICM Efficiency (*ICM_Eff_1*)	(-1) × the proportion of segments with inefficient capital subsidies within a firm. A capital subsidy is considered an inefficient capital allocation if the profitability of the segment (or the *q* of the segment) receiving the subsidy is not above the asset-weighted average profitability (or the asset-weighted average *q*) of the firm's other segments. For each segment, capital subsidies are proxied for by adjusted excess capital expenditures, which are defined as Max [segment excess capital expenditures – firm excess capital expenditures, 0], where excess capital expenditures are defined as Max [capital expenditures – cash flows, 0]. Segment cash flows are proxied for the sum of segment profits and segment depreciation.
ICM Efficiency (*ICM_Eff_2*)	the average of subsidy efficiency across all segments within a firm, where subsidy efficiency at the segment level is calculated as the segment's adjusted excess capital expenditures scaled by segment assets, multiplied by (+1) if the segment's profitability (or the segment's q) is above the asset-weighted average profitability (or the asset-weighted average *q*) of the firm's other segments and by (-1) otherwise. For each segment, adjusted excess capital expenditures are defined as Max [segment excess capital expenditures – firm excess capital expenditures, 0], where excess capital expenditures are defined as Max [capital expenditures – cash flows, 0]. Segment cash flows are proxied for the sum of segment profits and segment depreciation.
ROA	the net income divided by the average total assets.

FIGURE 1 Parallel Trend of Flow Delta

This figure plots the average *Flow Delta* for the treatment and control firms separately in each year during our sample period for the full sample. Panels A and B show the trends of the simple and the conditional average of *Flow Delta*, respectively. The conditional average is calculated as the average residual from the regression of *Flow Delta* on a set of firm and industry characteristics included in Equation (1).

Panel A: Simple Average of Flow Delta



Panel B: Conditional Average of Flow Delta

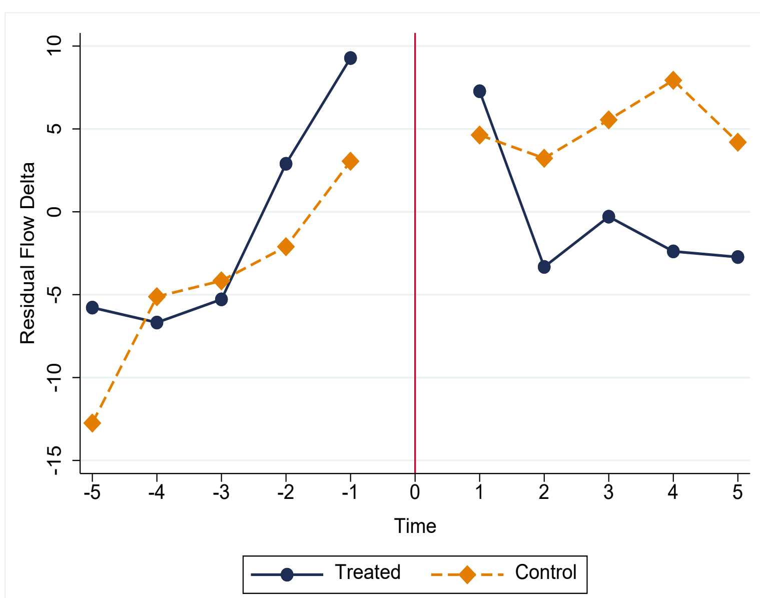


TABLE 1 Descriptive Statistics

This table presents the descriptive statistics for our sample firms. All variables are defined in the Appendix. To avoid the undue influence of outliers, all continuous variables are winsorized at the 1st and 99th percentiles.

	Pre-Period			Post-Period				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
	Treatment Group (N=1,492)	Control Group (N=763)	Diff.	Treatment Group (N=1,713)	Control Group (N=784)	Diff.	Diff. in Diff.	
	Mean	Mean	*p*-value	Mean	Mean	*p*-value	Mean	*p*-value
Flow Delta	31.024	26.488	0.082	45.877	48.855	0.390	-7.513	0.093
Age	4.039	4.050	0.066	4.038	4.045	0.227	0.004	0.623
Age≥60	0.267	0.320	0.008	0.281	0.284	0.875	0.050	0.074
Tenure	1.935	1.945	0.792	1.853	1.832	0.568	0.030	0.556
Size	7.029	7.175	0.034	7.186	7.386	0.002	-0.054	0.564
MTB	1.934	1.728	<.0001	1.922	1.758	0.001	-0.041	0.573
Lev	0.218	0.249	<.0001	0.248	0.267	0.006	0.012	0.230
Vol	0.022	0.019	<.0001	0.031	0.025	<.0001	0.002	0.001
Ret	0.212	0.208	0.858	0.095	0.093	0.915	-0.001	0.971
Ownership	0.024	0.025	0.943	0.024	0.026	0.491	-0.002	0.668
Chair	0.567	0.562	0.829	0.254	0.274	0.284	-0.025	0.386
Interlock	0.101	0.102	0.900	0.057	0.075	0.100	-0.016	0.332
Peer Ret	0.185	0.195	0.388	0.100	0.088	0.427	0.022	0.287
Peer Grants	1.440	1.247	0.043	3.411	3.045	0.033	0.173	0.426
HHI	0.325	0.333	0.528	0.316	0.333	0.145	-0.009	0.622
Speed of Profit Adj	0.543	0.557	0.364	0.464	0.563	<.0001	-0.085	0.000
Earn Pers	0.468	0.489	0.056	0.396	0.425	0.022	-0.008	0.647

TABLE 2 Correlation Coefficients

This table presents the Pearson correlation coefficients of the main variables used in our regressions, where variable (1) refers to *Flow Delta*. All variables are defined in the Appendix. Correlation coefficients with a p-value lower than 5% are bolded. To avoid the undue influence of outliers, all continuous variables are winsorized at the 1st and 99th percentiles.

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
(2)	*Age*	**-0.03**															
(3)	*Age≥60*	**-0.04**	**0.69**														
(4)	*Tenure*	**-0.05**	**0.37**	**0.34**													
(5)	*Size*	**0.37**	**0.14**	**0.05**	**-0.09**												
(6)	*MTB*	**0.20**	**-0.10**	**-0.05**	**0.04**	**-0.14**											
(7)	*Lev*	0.01	**0.03**	0.00	**-0.05**	**0.23**	**-0.27**										
(8)	*Vol*	**-0.07**	**-0.19**	**-0.08**	0.00	**-0.43**	**0.11**	**-0.09**									
(9)	*Ret*	**0.06**	**-0.06**	-0.01	**0.05**	**-0.09**	**0.35**	**-0.10**	-0.01								
(10)	*Ownership*	**-0.10**	**0.16**	**0.13**	**0.30**	**-0.17**	**0.04**	**-0.10**	**0.09**	**0.04**							
(11)	*Chair*	**0.05**	**0.15**	**0.12**	**0.11**	**0.22**	-0.01	0.03	**-0.27**	-0.01	**0.06**						
(12)	*Interlock*	**-0.03**	**0.09**	**0.08**	**0.14**	**-0.10**	**0.04**	**-0.05**	**0.03**	0.02	**0.13**	**0.06**					
(13)	*Peer Ret*	**0.05**	**-0.04**	-0.02	**0.03**	**-0.06**	**0.19**	**-0.09**	0.02	**0.40**	0.01	**0.04**	0.02				
(14)	*Peer Grants*	**0.29**	**-0.07**	**-0.05**	-0.01	**0.09**	**0.14**	-0.02	**0.16**	**0.05**	**-0.04**	**-0.04**	-0.02	**0.12**			
(15)	*HHI*	**-0.03**	**0.08**	**0.04**	**-0.05**	0.02	0.00	-0.01	**-0.21**	-0.02	**-0.03**	-0.01	0.01	**-0.04**	**-0.07**		
(16)	*Speed of Profit Adj*	**-0.06**	**0.10**	**0.06**	0.02	**0.04**	**-0.04**	**0.07**	**-0.15**	**-0.05**	**0.04**	**0.06**	0.01	**-0.10**	**-0.05**	**0.11**	
(17)	*Earn Pers*	**-0.07**	**0.05**	0.01	0.02	**0.04**	**-0.08**	**0.07**	**-0.14**	0.01	0.01	**0.07**	**0.03**	-0.02	**-0.19**	**0.08**	**0.15**

TABLE 3 SFAS 131 Adoption and Flow Delta

This table presents the results of the regression of *Flow Delta* estimating Equation (1). All variables are defined in the Appendix. To avoid the undue influence of outliers, all continuous variables are winsorized at the 1st and 99th percentiles. *T*-statistics are based on standard errors clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-sided), respectively.

	(1)			(2)		
	Coef.		*t*-Stat.	Coef.		*t*-Stat.
Post × Treatment	**-14.826**	**	**-2.555**	**-15.598**	***	**-2.945**
Age				-21.362		-1.265
Age≥60				2.286		0.643
Tenure				-5.577	**	-2.349
Size				5.658	*	1.741
MTB				4.988	**	2.377
Lev				-25.676	**	-2.212
Vol				-80.981		-0.499
Ret				2.853		1.203
Ownership				27.792		1.184
Chair				-1.277		-0.370
Interlock				1.742		0.498
Peer Ret				-1.810		-0.465
Peer Grants				4.270	***	6.201
HHI				12.939	**	2.475
Speed of Profit Adj				-5.123		-0.975
Earn Pers				-10.615		-1.479
Firm Fixed Effects	Yes			Yes		
Industry Fixed Effects	Yes			Yes		
Year Fixed Effects	Yes			Yes		
Number of Observations	4,752			4,752		
Adjusted R^2	0.384			0.421		

TABLE 4 Parallel Trend Assumption Test

This table presents the results of testing the parallel trend assumption. *Pre-1*, *Pre-2*, *Pre-3*, and *Pre-4* are indicator variables equal to one for the observation that is one, two, three, and four year(s) before the adoption of SFAS 131, respectively, and zero otherwise. *Pre-1* × *Treatment*, *Pre-2* × *Treatment*, *Pre-3* × *Treatment*, and *Pre-4* × *Treatment* are the products of *Pre-1*, *Pre-2*, *Pre-3*, and *Pre-4* and *Treatment*, respectively. All other variables are defined in the Appendix. To avoid the undue influence of outliers, all continuous variables are winsorized at the 1st and 99th percentiles. *T*-statistics are based on standard errors clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-sided), respectively.

	(1)			(2)		
	Coef.		*t*-Stat.	Coef.		*t*-Stat.
Post* × *Treatment	**-15.499**	**	**-2.003**	**-17.994**	***	**-2.601**
Pre -1* × *Treatment	**4.813**		**0.632**	**3.139**		**0.437**
Pre -2* × *Treatment	**-4.435**		**-0.733**	**-5.183**		**-0.909**
Pre -3* × *Treatment	**-2.023**		**-0.365**	**-7.151**		**-1.507**
Pre -4* × *Treatment	**-2.607**		**-0.705**	**-4.281**		**-1.249**
Age				-20.985		-1.232
Age≥60				2.067		0.578
Tenure				-5.407	**	-2.283
Size				5.347	*	1.661
MTB				5.093	**	2.437
Lev				-25.195	**	-2.194
Vol				-127.441		-0.773
Ret				2.752		1.169
Ownership				28.824		1.224
Chair				-2.764		-0.850
Interlock				1.728		0.498
Peer Ret				-0.675		-0.163
Peer Grants				4.242	***	6.153
HHI				12.863	**	2.459
Speed of Profit Adj				-4.734		-0.903
Earn Pers				-11.020		-1.533
Firm Fixed Effects	Yes			Yes		
Industry Fixed Effects	Yes			Yes		
Year Fixed Effects	Yes			Yes		
Number of Observations	4,752			4,752		
Adjusted R^2	0.385			0.422		

TABLE 5 Propensity-Score Matching Analysis

This table presents the results from the propensity-score matching (PSM) analysis. Panel A reports the after-matching mean values of *Flow Delta* and covariates included in the probit model in calculating the propensity score during the pre-period. Panel B reports the results of the regression of *Flow Delta* using the PSM-matched sample. All variables are defined in the Appendix. To avoid the undue influence of outliers, all continuous variables are winsorized at the 1st and 99th percentiles. *T*-statistics are based on standard errors clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-sided), respectively.

Panel A: Pre-Period After-Matching Descriptive Statistics

	(1)	(2)	(3)
	Treatment Group (N=739)	Control Group (N=763)	Difference
	Mean	Mean	*p*-value
Flow Delta	27.388	26.488	0.751
Age	4.059	4.050	0.198
Age≥60	0.305	0.320	0.522
Tenure	1.918	1.945	0.538
Size	7.105	7.175	0.345
MTB	1.693	1.728	0.422
Lev	0.237	0.249	0.128
Vol	0.019	0.019	0.910
Ret	0.204	0.208	0.839
Ownership	0.024	0.025	0.855
Chair	0.563	0.562	0.979
Interlock	0.096	0.102	0.690
Peer Ret	0.190	0.195	0.684
Peer Grants	1.267	1.247	0.779
HHI	0.336	0.333	0.878
Speed of Profit Adj	0.583	0.557	0.148
Earn Pers	0.482	0.489	0.550

TABLE 5 Propensity-Score Matching Analysis, Continued

Panel B: Regression Analysis Using the PSM Sample

	(1)			(2)		
	Coef.		*t*-Stat.	Coef.		*t*-Stat.
Post* × *Treatment	**-17.082**	**	**-2.542**	**-14.124**	**	**-2.419**
Age				-14.672		-0.669
Age≥60				4.054		0.923
Tenure				-5.958	*	-1.884
Size				6.917		1.473
MTB				13.132	***	3.890
Lev				9.150		0.654
Vol				-238.514		-1.156
Ret				3.538		0.941
Ownership				28.728		1.030
Chair				-1.761		-0.429
Interlock				-0.462		-0.124
Peer Ret				-1.813		-0.410
Peer Grants				5.190	***	4.194
HHI				6.676		1.159
Speed of Profit Adj				-10.027		-1.300
Earn Pers				-9.427		-0.970
Firm Fixed Effects	Yes			Yes		
Industry Fixed Effects	Yes			Yes		
Year Fixed Effects	Yes			Yes		
Number of Observations	2,881			2,881		
Adjusted R^2	0.421			0.469		

TABLE 6 SFAS 131 Adoption and Portfolio Delta

This table presents the results of the regression of *Portfolio Delta*. We replace *Flow Delta* with *Portfolio Delta* in estimating Equation (1). Columns (1) and (2) report the results based on the full and PSM sample, respectively. All variables are defined in the Appendix. To avoid the undue influence of outliers, all continuous variables are winsorized at the 1st and 99th percentiles. *T*-statistics are based on standard errors clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-sided), respectively.

	(1) *Full Sample*			(2) *PSM Sample*		
	Coef.		*t*-Stat.	Coef.		*t*-Stat.
Post* × *Treatment	**-195.417**	**	**-2.427**	**-236.388**	***	**-2.990**
Age	278.515		1.126	222.366		0.635
Age≥60	87.209	*	1.878	77.919		1.433
Tenure	131.447	***	5.456	116.816	***	4.195
Size	159.027	***	3.421	175.374	***	2.738
MTB	127.597	***	3.855	98.440	***	3.231
Lev	-6.746		-0.049	-44.094		-0.311
Vol	-4,705.554	**	-2.341	-4,624.461		-1.548
Ret	36.065		1.453	28.273		0.836
Ownership	3,340.463	***	3.022	4,186.716	**	2.414
Chair	-140.135	***	-3.418	-106.870	**	-2.244
Interlock	-29.874		-0.476	-108.464		-1.307
Peer Ret	-27.561		-0.822	-3.980		-0.089
Peer Grants	21.248	***	3.568	23.653	**	2.446
HHI	77.546		1.307	58.065		0.743
Speed of Profit Adj	-120.052		-1.555	-41.323		-0.480
Earn Pers	53.253		0.543	231.012	*	1.910
Firm Fixed Effects	Yes			Yes		
Industry Fixed Effects	Yes			Yes		
Year Fixed Effects	Yes			Yes		
Number of Observations	4,752			2,881		
Adjusted R^2	0.782			0.684		

TABLE 7 Role of Operating Volatilities

This table presents the results of the cross-sectional analysis of *Flow Delta* based on operating volatilities. In Column (1), *High Volatility* equals one for firms whose sales volatility during the pre-period is above the sample median and zero otherwise. In Column (2), *High Volatility* equals one for firms whose stock return volatility during the pre-period is above the sample median and zero otherwise. All other variables are defined in the Appendix. To avoid the undue influence of outliers, all continuous variables are winsorized at the 1st and 99th percentiles. *T*-statistics are based on standard errors clustered by firm. ** and *** indicate statistical significance at the 5%, and 1% levels (two-sided), respectively.

	(1) *Volatility Based on Sales Revenue*			(2) *Volatility Based on Stock Return*		
	Coef.		*t*-Stat.	Coef.		*t*-Stat.
Post × Treatment	-4.501		-1.106	-3.490		-0.535
High Volatility × Post	28.152	***	3.245	13.579		1.499
High Volatility × Post × Treatment	**-21.444**	**	**-2.023**	**-23.942**	**	**-2.255**
Control Variables	Included			Included		
Firm Fixed Effects	Yes			Yes		
Industry Fixed Effects	Yes			Yes		
Year Fixed Effects	Yes			Yes		
Number of Observations	4,359			4,359		
Adjusted R^2	0.434			0.433		

TABLE 8 Role of Internal Monitoring by Boards

This table presents the results of the cross-sectional analysis of *Flow Delta* based on internal monitoring by boards. In Column (1), *Low Internal Monitoring* equals one for firms if the fraction of co-opted directors, averaged during the pre-period, is above the sample median and zero otherwise. In Column (2), *Low Internal Monitoring* equals one for firms if the tenure-weighted fraction of co-opted directors, averaged during the pre-period, is above the sample median and zero otherwise. In Column (3), *Low Internal Monitoring* equals one for firms if the indicator of CEO duality, averaged during the pre-period, is above the sample median and zero otherwise. All other variables are defined in the Appendix. To avoid the undue influence of outliers, all continuous variables are winsorized at the 1st and 99th percentiles. *T*-statistics are based on standard errors clustered by firm. ** and *** indicate statistical significance at the 5%, and 1% levels (two-sided), respectively.

	(1) *Internal Monitoring Based on Co-Option*			(2) *Internal Monitoring Based on Tenure-Weighted Co-Option*			(3) *Internal Monitoring Based on CEO Duality*		
	Coef.		*t*-Stat.	Coef.		*t*-Stat.	Coef.		*t*-Stat.
Post × Treatment	-2.394		-0.305	0.189		0.022	-7.329		-1.483
Low Internal Monitoring × Post	7.615		0.640	7.814		0.673	33.482	***	2.759
Low Internal Monitoring × Post × Treatment	**-29.831**	**	**-2.177**	**-33.608**	**	**-2.415**	**-28.747**	**	**-2.061**
Control Variables	Included			Included			Included		
Firm Fixed Effects	Yes			Yes			Yes		
Industry Fixed Effects	Yes			Yes			Yes		
Year Fixed Effects	Yes			Yes			Yes		
Number of Observations	2,930			2,930			4,359		
Adjusted R^2	0.446			0.447			0.434		

TABLE 9 Role of External Monitoring Mechanisms

This table presents the results of the cross-sectional analysis of *Flow Delta* based on external monitoring mechanisms. In Column (1), *High External Monitoring* equals one for firms if the number of motivated monitors, averaged during the post-period, is above the sample median and zero otherwise. An institution is defined as a motivated monitor if the institution has the firm in its portfolio with a weight exceeding 10% of the total value of the portfolio. In Column (2), *High External Monitoring* equals one for firms if the number of analysts following, averaged during the post-period, is above the sample median and zero otherwise. In Column (3), *High External Monitoring* equals one for firms if Cain et al.'s (2017) H-index, averaged during the post-period, is above the sample median and zero otherwise. All other variables are defined in the Appendix. To avoid the undue influence of outliers, all continuous variables are winsorized at the 1st and 99th percentiles. *T*-statistics are based on standard errors clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-sided), respectively.

	(1) *External Monitoring Based on Motivated Monitors*			(2) *External Monitoring Based on Financial Analysts*			(3) *External Monitoring Based on H-Index*		
	Coef.		*t*-Stat.	Coef.		*t*-Stat.	Coef.		*t*-Stat.
Post × Treatment	-5.659	*	-1.661	-1.350		-0.467	-7.236		-1.083
High External Monitoring × Post	36.466	***	3.959	41.277	***	5.036	17.867	*	1.911
High External Monitoring × Post × Treatment	**-22.638**	**	**-2.073**	**-26.795**	***	**-2.739**	**-18.322**	*	**-1.654**
Control Variables	Included			Included			Included		
Firm Fixed Effects	Yes			Yes			Yes		
Industry Fixed Effects	Yes			Yes			Yes		
Year Fixed Effects	Yes			Yes			Yes		
Number of Observations	4,571			4,571			4,377		
Adjusted R^2	0.427			0.428			0.427		

TABLE 10 ICM Efficiency and Incentive Contracting

This table presents the results of the regression of *Cash Bonus* estimating Equation (2). Panel A reports the results when *ICM Efficiency* is measured based on segment profitability (i.e., the profit-to-asset ratio). Panel B reports the results when *ICM Efficiency* is measured based on segment *q*. (i.e., the median *q* of single-segment firms operating in the same industry). In both Panel A and Panel B, Columns (1) and (2) report the results using *ICM_Eff_1* and *ICM_Eff_2* as a measure of *ICM Efficiency*, respectively. All variables are defined in the Appendix. To avoid the undue influence of outliers, all continuous variables are winsorized at the 1st and 99th percentiles. *T*-statistics are based on standard errors clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-sided), respectively.

Panel A: ICM Efficiency Based on Segment Profitability

	(1) *ICM Efficiency Measured with ICM_Eff_1*			(2) *ICM Efficiency Measured with ICM_Eff_2*		
	Coef.		*t*-Stat.	Coef.		*t*-Stat.
Post × Treatment	0.202		0.668	-1.522	**	-2.420
ICM Efficiency	1.135	**	2.046	0.822	**	2.220
ICM Efficiency × Post	-1.274		-1.501	-0.715		-1.071
ICM Efficiency × Treatment	-1.647	*	-1.962	-1.167	**	-2.000
ICM Efficiency × Post × Treatment	**3.201**	**	**2.427**	**2.584**	**	**2.577**
ROA	8.797	***	6.780	8.778	***	6.735
Age	-1.242		-0.990	-1.349		-1.067
Age≥60	0.157		0.995	0.152		0.948
Tenure	0.002		0.018	0.010		0.082
Size	-0.499	*	-1.784	-0.498	*	-1.773
MTB	-0.405	***	-2.875	-0.415	***	-2.897
Lev	-0.184		-0.233	-0.167		-0.211
Vol	5.149		0.557	5.717		0.613
Ret	0.519	***	3.486	0.510	***	3.421
Ownership	2.166		1.485	2.206		1.528
Chair	0.173		1.357	0.170		1.319
Interlock	-0.320		-1.195	-0.321		-1.185
Peer Ret	-0.089		-0.553	-0.081		-0.508
Peer Grants	0.003		0.297	0.004		0.441
HHI	0.139		0.590	0.131		0.552
Speed of Profit Adj	-0.295		-0.840	-0.285		-0.810
Earn Pers	0.022		0.053	-0.008		-0.021
Firm Fixed Effects	Yes			Yes		
Industry Fixed Effects	Yes			Yes		
Year Fixed Effects	Yes			Yes		
Number of Observations	2,019			2,019		
Adjusted R^2	0.459			0.460		

TABLE 10 ICM Efficiency and Incentive Contracting, Continued

Panel B: ICM Efficiency Based on Segment *q*

	(1) *ICM Efficiency Measured with ICM_Eff_1*			(2) *ICM Efficiency Measured with ICM_Eff_2*		
	Coef.		*t*-Stat.	Coef.		*t*-Stat.
Post × Treatment	0.167		0.567	-1.618	***	-2.629
ICM Efficiency	1.801	*	1.856	0.560		1.270
ICM Efficiency × Post	-1.886		-1.431	-0.898		-1.367
ICM Efficiency × Treatment	-3.107	**	-2.357	-1.310	*	-1.850
ICM Efficiency × Post × Treatment	**3.659**	**	**1.990**	**2.787**	***	**2.838**
ROA	8.227	***	5.512	8.327	***	5.605
Age	-1.239		-0.815	-1.240		-0.808
Age≥60	0.203		1.133	0.177		1.013
Tenure	-0.030		-0.189	-0.024		-0.148
Size	-0.636	**	-2.121	-0.653	**	-2.145
MTB	-0.381	**	-2.221	-0.391	**	-2.239
Lev	0.652		0.718	0.552		0.600
Vol	-7.660		-0.693	-7.787		-0.711
Ret	0.616	***	3.623	0.578	***	3.379
Ownership	1.966		1.111	2.319		1.301
Chair	0.266	*	1.744	0.237		1.558
Interlock	-0.447		-1.349	-0.452		-1.326
Peer Ret	-0.089		-0.454	-0.036		-0.182
Peer Grants	-0.003		-0.467	-0.003		-0.385
HHI	0.155		0.621	0.155		0.611
Speed of Profit Adj	-0.452		-1.122	-0.480		-1.191
Earn Pers	-0.082		-0.159	-0.157		-0.310
Firm Fixed Effects	Yes			Yes		
Industry Fixed Effects	Yes			Yes		
Year Fixed Effects	Yes			Yes		
Number of Observations	1,562			1,562		
Adjusted R^2	0.487			0.487		

TABLE 11 Excluding Control Firms Less Likely to Comply with SFAS 131

This table presents the results of the regression of *Flow Delta* estimating Equation (1) after excluding control firms that are less likely to comply with SFAS 131. All other variables are defined in the Appendix. To avoid the undue influence of outliers, all continuous variables are winsorized at the 1st and 99th percentiles. *T*-statistics are based on standard errors clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-sided), respectively.

	(1)			(2)			(3)		
	Excluding Control Firms w/ Weak Board Monitoring			*Excluding Control Firms w/ High Competition*			*Excluding Control Firms w/ Weak Board Monitoring or High Competition*		
	Coef.		*t*-Stat.	Coef.		*t*-Stat.	Coef.		*t*-Stat.
Post × Treatment	**-22.628**	***	**-3.455**	**-20.146**	***	**-2.712**	**-21.873**	**	**-2.399**
Age	4.430		0.767	-37.778	*	-1.816	5.835		0.935
Age≥60	8.706	**	2.348	2.260		0.554	9.988	**	2.107
Tenure	-8.125		-0.425	-6.244	**	-2.074	-4.335		-0.194
Size	180.365		0.599	5.292		1.417	9.163		0.027
MTB	1.600		0.400	6.125	**	2.247	2.727		0.583
Lev	2.930		0.454	-20.241		-1.343	-0.072		-0.009
Vol	4.887	***	5.266	-157.120		-0.840	5.082	***	5.181
Ret	-9.264	**	-2.546	4.097		1.323	-8.202	*	-1.894
Ownership	-13.692		-0.540	35.243		1.177	-40.004		-1.338
Chair	1.804		0.419	1.403		0.329	4.783		0.947
Interlock	-20.970		-0.585	2.556		0.538	-10.400		-0.255
Peer Ret	0.581		0.126	-1.763		-0.356	0.861		0.157
Peer Equity	2.795		0.504	4.441	***	5.904	2.061		0.274
HHI	14.969	*	1.951	17.244	***	2.965	20.125	**	2.386
Speed of Profit Adj	-11.836		-1.590	-1.700		-0.248	-5.998		-0.685
Earn Pers	-16.115	*	-1.703	-13.321		-1.472	-17.137		-1.528
Firm Fixed Effects	Yes			Yes			Yes		
Industry Fixed Effects	Yes			Yes			Yes		
Year Fixed Effects	Yes			Yes			Yes		
Number of Observations	2,760			3,377			2,065		
Adjusted R^2	0.440			0.426			0.454		

Online Appendix

Segment Disaggregation and Equity-Based Pay Contracts

Young Jun Cho and Hojun Seo

Segment Disaggregation and Equity-Based Pay Contracts

This appendix discusses the results of other robustness and additional tests. First, our sample period includes the passage of SOX in 2002, which may have affected treatment and control firms differently during the post-period. Given that SOX requires firms to strengthen board independence, if SOX improved corporate monitoring for one group to a greater extent than for another, our results could be confounded by SOX. However, we do not find evidence that there is a significant difference in the change in board independence before and after SOX between the treatment and control groups (results untabulated). Nevertheless, in Column (1) of Table A1, we report the results when we use a shorter window, i.e., three years before and after the adoption of SFAS 131, excluding the post-SOX period. Consistent with our main finding, the coefficient on *Post* × *Treatment* is significantly negative. We also find a consistent result when we only remove the firm-year observations in the post-SOX period (results untabulated). Taken together, these results suggest that our finding is unlikely to be attributable to SOX. Additionally, we also rule out the possibility that our results are attributable to the collapse of the dot-com bubble in 2000; i.e., we exclude firms in high-tech industries (three-digit SIC codes of 357 or 737) and find similar results.

Second, firms usually tailor the compensation package to attract the most suitable managers. Thus, it is expected that different CEOs receive different packages. To the extent that compensation contracts are affected by manager-specific factors, CEO turnover may also complicate our inferences. To ensure that CEO turnover does not drive our results, we replace firm-fixed effects with firm-CEO fixed effects and report the results in Column (2) of Table A1. In a similar spirit, we also restrict our sample to firm-year observations with the same CEOs remaining constant in both the pre- and post-periods and report the results in Column (3) of Table A1. In both Columns (2) and (3), the coefficient on *Post* × *Treatment* is significantly negative, suggesting that our results are robust to controlling for CEO turnover. Relatedly, if CEOs in treatment firms experienced higher turnover in the post-period and were replaced by younger CEOs, the observed reduction in equity incentives could be attributed to the differing composition of CEOs in terms of their proximity to retirement between the treatment and control firms (Gibbons and Murphy, 1992). However, as reported in column (7) of Table 1 in the paper, the difference in differences in *Age ⩾ 60* is 0.050, significantly positive at the 10% level, suggesting that CEOs in treatment firms, not control firms, were closer to retirement after the adoption of SFAS 131.

We next re-estimate Equation (1) after replacing *Flow Delta* with *Cash Comp*, defined as the ratio of cash compensation to total compensation for each CEO, and report the result in Column (1) of Table A2. To the extent that firms reduce the use of equity-based pay contracts, we expect to see an increase in the cash component of CEO pay. Consistent with this expectation, we find that the coefficient on *Post* × *Treatment* is significantly positive. We also re-estimate Equation (1) after replacing *Flow Delta* with *Flow Vega*, defined following Gormley et al. (2013) as the change in the value of CEO option grants in a given year (expressed in thousands of dollars) for a 1% increase in the firm's stock volatility, and report the result in Column (2) of Table A2. We find that the coefficient on *Post* × *Treatment* is significantly negative, suggesting that segment disaggregation also decreases shareholders' demand for the convexity of equity pay. If segment disaggregation increases the use of cash, rather than equity, in CEO pay, managers would be more willing to take on risky projects, as their wealth is more likely to be shielded from stock volatility.

An alternative explanation of our findings is that increased focus on reported segment profits may create short-term pressure on CEOs, leading them to forgo projects that would yield long-term profitability. Previous studies indicate that CEOs with higher equity incentives are more susceptible to short-term price declines and thus experience greater short-term pressures, engaging in myopic behavior such as earnings management (e.g., Cheng and Warfield, 2005; Bergstresser and Philippon, 2006; Efendi et al., 2007). Consequently, facing potential managerial myopia induced by SFAS 131 adoption, one possible course of action for the board would be to reduce equity incentives in CEO compensation. To address this possibility, we examine whether treatment firms exhibited more myopic behavior compared to control firms following the implementation of SFAS 131. To the extent that firms are under greater short-term pressure, they are more likely to engage in myopic earnings management, such as accruals management and real activities earnings management, and exhibit a stronger tendency to meet or beat analyst earnings expectations, a.k.a. MBE. However, we fail to find any evidence suggesting that treatment firms engaged in earnings management or MBE to a greater extent than did control firms after the adoption of SFAS 131, inconsistent with SFAS 131 adoption increasing managerial myopia. The results of this analysis are presented in Panels A, B, and C of Table A3.

Additionally, if the adoption of SFAS 131 increased short-term performance pressure, CEOs would experience greater pressure from myopic investors after SFAS 131 adoption when their firms are more owned by myopic investors. Then the decrease in flow delta after the adoption of SFAS 131, as documented in our study, would be more pronounced for firms with a greater presence of myopic investors. To test this idea, we split our sample into two groups based on Bushee's (1998) transient institutional investors' ownership measured during the post-SFAS 131 period. However, we do not find evidence that the decrease in flow delta after the adoption of SFAS 131 is significantly more pronounced for firms with higher transient institutional investors' ownership, mitigating the possibility that our findings are attributable to short-term pressure exerted by myopic investors. The results of this analysis are presented in Table A4.

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TABLE A1 Using a Shorter Window, Firm-CEO Fixed Effects, and Constant CEOs

This table presents the results of the regression of *Flow Delta*. Columns (1), (2), and (3) report the results using a shorter window around SFAS 131 adoption (i.e., three years before and after the adoption of SFAS 131), using firm-CEO fixed effects (instead of firm fixed effects), and using firms with constant CEOs in both the pre- and post-periods, respectively. All variables are defined in the Appendix of the manuscript. To avoid the undue influence of outliers, all continuous variables are winsorized at the 1st and 99th percentiles. *T*-statistics are based on standard errors clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-sided), respectively.

	(1) *Using a Shorter Window*			(2) *Using Firm-CEO Fixed Effects*			(3) *Using Firms with Constant CEO*		
	Coef.		*t*-Stat.	Coef.		*t*-Stat.	Coef.		*t*-Stat.
Post × Treatment	**-14.073**	**	**-2.41**	**-16.544**	**	**-2.49**	**-16.06**	**	**-2.40**
Age	-9.916		-0.44	63.208		0.51	-26.035		-0.19
Age≥60	3.466		0.67	5.027		1.11	2.546		0.44
Tenure	-5.875	*	-1.88	-1.534		-0.30	5.687		0.79
Size	3.069		0.58	3.409		0.75	5.219		0.90
MTB	1.607		0.61	2.059		0.78	2.336		0.70
Lev	-10.496		-0.74	-30.379	**	-2.40	-31.873	*	-1.94
Vol	-285.542		-1.14	-62.92		-0.34	-281.142		-1.12
Ret	4.438		1.26	4.308		1.56	7.243	*	1.88
Ownership	64.224	*	1.73	-5.56		-0.24	-36.095		-1.17
Chair	0.272		0.06	-0.433		-0.10	3.63		0.70
Interlock	-0.58		-0.10	0.241		0.07	3.941		0.83
Peer Ret	-4.533		-0.76	-1.259		-0.29	3.245		0.50
Peer Grants	4.014	***	5.14	4.269	***	5.88	5.214	***	4.93
HHI	11.241		1.20	10.814	**	2.02	8.124		1.08
Speed of Profit Adj	-7.335		-0.85	-3.249		-0.49	-10.457		-1.22
Earn Pers	-14.047		-1.17	-13.607		-1.52	-11.041		-0.86
Firm FE	Yes			Firm-CEO FE			Yes		
Industry FE	Yes			Yes			Yes		
Year FE	Yes			Yes			Yes		
No. of Obs.	2,960			4,600			2,543		
Adjusted R^2	0.414			0.439			0.406		

TABLE A2 Analyses of Cash Compensation and Flow Vega

This table presents the results we re-estimate Equation (1) after replacing *Flow Delta* with *Cash Comp* and *Flow Vega* in Columns (1) and (2), respectively. *Cash Comp* is defined as the ratio of cash compensation to total compensation for the firm's CEO. *Flow Vega* is measured as the change in the value of CEO option grants in a given year (expressed in thousands of dollars) for a 1% increase in the firm's stock volatility. All other variables are defined in the Appendix of the manuscript. To avoid the undue influence of outliers, all continuous variables are winsorized at the 1st and 99th percentiles. *T*-statistics are based on standard errors clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-sided), respectively.

	(4) *Using Cash Comp as Dep. Var.*			(5) *Using Flow Vega as Dep. Var.*		
	Coef.		*t*-Stat.	Coef.		*t*-Stat.
Post × Treatment	**0.041**	**	**2.33**	**-11.279**	***	**-2.72**
Age	0.117		1.61	-14.738		-1.17
Age≥60	-0.007		-0.56	-0.991		-0.35
Tenure	0.036	***	4.30	-3.035	*	-1.88
Size	-0.038	***	-2.65	3.631		1.51
MTB	-0.019	***	-2.89	5.778	***	3.23
Lev	0.133	***	2.63	-13.077		-1.47
Vol	0.141		0.19	-165.553	*	-1.68
Ret	0.016		1.47	0.998		0.55
Ownership	0.069		0.47	8.455		0.54
Chair	-0.001		-0.06	-2.933		-1.16
Interlock	-0.013		-0.64	2.994		1.12
Peer Ret	0.001		0.11	2.127		0.86
Peer Grants	-0.007	***	-5.12	2.777	***	5.81
HHI	-0.016		-0.81	11.399	***	2.80
Speed of Profit Adj	0.019		0.78	-1.552		-0.37
Earn Pers	0.063	*	1.91	-7.709		-1.34
Firm FE	Yes			Yes		
Industry FE	Yes			Yes		
Year FE	Yes			Yes		
No. of Obs.	4,752			4,723		
Adjusted R^2	0.416			0.475		

TABLE A3 SFAS 131 and Managerial Myopia

This table presents the results of the analyses testing the relationship between the adoption of SFAS 131 and managerial myopia. In Panel A, the dependent variable is discretionary accruals estimated following the modified Jones model (Jones 1991; Dechow, Sloan, and Sweeney 1995), the Larcker and Richardson (2004) model, and the McNichols (2002) model in columns (1), (2), and (3), respectively. In Panel B, the dependent variable is real activities earnings management (RM) measured using the cash flows from operations (CFO) model, the production costs model, and the discretionary expenses model in columns (1), (2), and (3), respectively (Roychowdhury 2006; Cheng, Lee, and Shevlin 2016). In Panel C, the dependent variable is *MBE* and *JUSTMBE* in columns (1) and (2), respectively. *MBE* is the frequency of quarterly earnings announcements meeting or beating analyst earnings expectations in a given year, and *JUSTMBE* is the frequency of quarterly earnings announcements just meeting or beating analyst earnings expectations in a given year. The quarterly earnings announcements are regarded as just meeting or beating analyst earnings expectations if the difference between actual earnings and analyst earnings expectations is less than or equal to two cents (e.g., Filzen and Peterson 2015). Analyst earnings expectation is defined as the median EPS forecast measured three days prior to the quarterly earnings announcement. All other variables are defined in the Appendix of the manuscript. To avoid the undue influence of outliers, all continuous variables are winsorized at the 1st and 99th percentiles. *T*-statistics are based on standard errors clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-sided), respectively.

Panel A: Accruals Earnings Management

	(1) *Disc. Accruals Based on the Modified Jones Model*			(2) *Disc. Accruals Based on Larcker and Richardson*			(2) *Disc. Accruals Based on the McNichols Model*		
	Coef.		*t*-Stat	Coef.		*t*-Stat	Coef.		*t*-Stat
Post × Treatment	**-0.006**		**-0.512**	**-0.003**		**-0.309**	**0.003**		**0.423**
Age	0.051		1.331	0.029		0.838	0.017		0.521
Age≥60	-0.009		-1.302	-0.009		-1.579	-0.005		-0.860
Tenure	0.003		0.804	0.004		1.116	0.003		0.995
Size	-0.046	***	-5.137	-0.044	***	-6.020	-0.041	***	-5.735
MTB	0.010	***	3.218	0.002		0.489	-0.000		-0.004
Lev	-0.013		-0.481	0.024		0.889	0.050	**	2.000
Vol	-0.318		-0.920	0.059		0.166	0.271		0.906
Ret	0.019	***	4.112	0.013	***	2.677	0.012	***	2.876
Ownership	-0.031		-0.452	-0.038		-0.542	-0.018		-0.296
Chair	-0.001		-0.210	0.006		1.096	0.009		1.626
Interlock	-0.018		-1.531	-0.008		-0.774	-0.004		-0.409
Peer Ret	0.003		0.528	0.005		0.933	0.007		1.499
Peer Grants	0.001		1.008	0.001		1.342	0.000		0.573
HHI	0.014		1.416	0.011		1.171	0.008		0.954
Speed of Profit Adj	0.040	***	2.949	0.035	***	3.009	0.035	***	3.471
Earn Pers	0.001		0.062	-0.002		-0.153	-0.004		-0.322
Firm Fixed Effects	Yes			Yes			Yes		
Industry Fixed Effects	Yes			Yes			Yes		
Year Fixed Effects	Yes			Yes			Yes		
Number of Observations	4,689			4,689			4,606		
Adjusted R^2	0.616			0.239			0.355		

TABLE A3 SFAS 131 and Managerial Myopia, Continued

Panel B: Real Activities Earnings Management (RM)

	(1)			(2)			(3)		
	RM Based on CFO			*RM Based on Production Costs*			*RM Based on Discretionary Expenses*		
	Coef.		*t*-Stat	Coef.		*t*-Stat	Coef.		*t*-Stat
Post × Treatment	**0.000**		**0.079**	**0.012**		**1.044**	**0.017**		**1.283**
Age	0.024		0.838	0.036		0.844	0.019		0.425
Age≥60	-0.007	*	-1.651	-0.015	**	-2.123	0.000		0.039
Tenure	-0.000		-0.114	0.011	**	2.131	0.006		1.145
Size	0.021	***	3.832	0.019	*	1.887	-0.012		-1.003
MTB	-0.015	***	-4.539	-0.026	***	-4.603	-0.001		-0.139
Lev	0.023		1.113	0.040		1.170	0.118	***	3.097
Vol	0.426	*	1.811	0.227		0.631	-1.071	**	-2.173
Ret	-0.010	***	-2.597	0.001		0.142	0.034	***	5.401
Ownership	-0.001		-0.011	-0.012		-0.166	0.022		0.284
Chair	0.002		0.555	0.005		0.827	0.000		0.034
Interlock	-0.001		-0.201	0.003		0.250	0.013		1.015
Peer Ret	-0.007		-1.527	-0.001		-0.212	0.019	**	2.540
Peer Grants	0.000		0.281	0.000		0.321	0.001		1.300
HHI	0.004		0.570	0.012		1.378	0.013		1.063
Speed of Profit Adj	0.013		1.621	-0.004		-0.348	0.002		0.139
Earn Pers	-0.007		-0.596	-0.003		-0.163	-0.020		-0.964
Firm Fixed Effects	Yes			Yes			Yes		
Industry Fixed Effects	Yes			Yes			Yes		
Year Fixed Effects	Yes			Yes			Yes		
Number of Observations	4,746			4,749			4,390		
Adjusted R^2	0.589			0.859			0.818		

TABLE A3 SFAS 131 and Managerial Myopia, Continued

Panel C: Meeting or Beating Analyst Expectations

	(1) *MBE*			(2) *JUSTMBE*		
	Coef.		*t*-Stat	Coef.		*t*-Stat
Post* × *Treatment	**-0.063**		**-0.589**	**-0.039**		**-0.607**
Age	-0.005		-0.012	-0.548	**	-1.972
Age≥60	0.020		0.267	-0.005		-0.093
Tenure	-0.014		-0.272	0.047		1.529
Size	-0.347	***	-4.671	-0.048		-0.989
MTB	-0.153	***	-4.405	0.045	*	1.922
Lev	0.514	*	1.750	-0.113		-0.608
Vol	0.535		0.136	0.988		0.365
Ret	0.325	***	6.071	0.070	*	1.723
Ownership	0.213		0.317	0.395		0.722
Chair	0.006		0.081	-0.019		-0.389
Interlock	-0.140		-1.325	-0.002		-0.032
Peer Ret	0.156	**	2.317	-0.126	***	-2.699
Peer Grants	-0.001		-0.110	0.005		1.089
HHI	0.006		0.051	-0.149	*	-1.912
Speed of Profit Adj	-0.114		-0.779	0.062		0.599
Earn Pers	0.341		1.607	0.113		0.937
Firm Fixed Effects	Yes			Yes		
Industry Fixed Effects	Yes			Yes		
Year Fixed Effects	Yes			Yes		
Number of Observations	4,367			4,367		
Adjusted R^2	0.162			0.111		

TABLE A4 Cross-Sectional Tests Based on Transient Institutional Ownership

This table presents the results of the cross-sectional analyses, where we split the sample into two groups based on transient institutional investors' ownership. In column (1), *High Transient* is an indicator variable that equals one for firms whose transient institutional investors' ownership is above the sample median and zero otherwise. In column (2), *High Transient HHI* is an indicator variable that equals one for firms where the concentration (HHI) of transient institutional investors' ownership is above the sample median, and zero otherwise. All other variables are defined in the Appendix of the manuscript. To avoid the undue influence of outliers, all continuous variables are winsorized at the 1st and 99th percentiles. *T*-statistics are based on standard errors clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-sided), respectively.

	(1)			(2)		
	Coef.		*t*-Stat.	Coef.		*t*-Stat.
Post × Treatment	-13.580	*	-1.730	-16.727	**	-2.051
High Transient × Post	8.547		1.021			
High Transient × Post × Treatment	**-3.803**		**-0.382**			
High Transient HHI × Post				-3.631		-0.496
High Transient HHI × Post × Treatment				**2.346**		**0.257**
Age	-19.451		-1.138	-19.985		-1.160
Age≥60	1.903		0.530	1.683		0.471
Tenure	-5.630	**	-2.310	-5.485	**	-2.259
Size	5.238		1.581	6.122	*	1.851
MTB	4.950	**	2.319	4.967	**	2.336
Lev	-27.095	**	-2.266	-25.719	**	-2.135
Vol	-93.960		-0.561	-89.357		-0.535
Ret	2.895		1.194	2.925		1.205
Ownership	29.456		1.211	30.772		1.254
Chair	-1.461		-0.407	-1.415		-0.397
Interlock	2.455		0.672	2.361		0.649
Peer Ret	-2.084		-0.521	-1.955		-0.489
Peer Grants	4.292	***	6.179	4.298	***	6.163
HHI	12.818	**	2.339	12.895	**	2.368
Speed of Profit Adj	-6.225		-1.200	-6.313		-1.220
Earn Pers	-10.569		-1.438	-9.781		-1.346
Firm Fixed Effects	Yes			Yes		
Industry Fixed Effects	Yes			Yes		
Year Fixed Effects	Yes			Yes		
Number of Observations	4,500			4,500		
Adjusted R^2	0.422			0.422		