# The Consequences of Mandated Compensation Disclosure *

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# The Consequences of Mandated Compensation Disclosure

Abstract

After the SEC mandates the disclosure of Chief Financial Officers' (CFO) compensation in 2006, CFO total pay increases significantly with strengthened equity incentives and CFOs become more likely to leave their firms following poor performance. The results are absent for the CEO or other executives, suggesting they are unique outcomes of enhanced CFO compensation disclosures. The evidence is consistent with more intense monitoring following the disclosure mandate. CFOs require additional compensation for the loss of private benefits due to greater monitoring and are subject to greater internal discipline. There is also some evidence of increased bad news hoarding, suggesting that CFOs engage in more short-term behavior to boost their performance and avoid termination.

Keywords: SEC compensation disclosure mandate; CFO pay; executive turnover; corporate financial reporting
JEL classifications: G38, J33, M52

## I. Introduction

> *"Increasing shareholder power to intervene, I argue, would improve corporate governance and enhance shareholder value by addressing important agency problems that have long afflicted publicly traded companies."*[1]

Since the inception of the Securities and Exchange Commission (SEC) in 1934 which initiated the disclosure of executive compensation, there has been continuous regulatory efforts promoting timely and detailed compensation disclosures.[2] By providing investors with a clearer and more complete picture of the compensation earned by corporate executives, regulations on compensation disclosure aim to facilitate shareholder monitoring of executive compensation practices in major corporations (Frydman and Saks, 2010). In 2006, the SEC enacted a new rule mandating the disclosure of the Chief Financial Officer's (CFO) compensation, among other rules.[3] Our paper studies the consequences of this new disclosure mandate that makes CFO pay public information for all investors.

Better compensation disclosures can facilitate investor monitoring as shareholders are permitted to work with the board to design executive compensation in ways consistent with shareholder value maximization (Brickley, Lease and Smith, 1994; Zeckhauser and Pound, 1990). For example, while U.S. corporations are required to hold shareholder votes on executive pay policies under the Dodd-Frank Act, it remains a concern that shareholders may lack necessary information to vote sensibly (e.g., Bebchuk, 2005; Larcker, McCall, Ormazabal, and Tayan, 2012). On the other hand, shareholders may not have strong incentives to monitor or their monitoring

[1] Bebchuk, Lucian A. 2005. The case for increasing shareholder power, *Harvard Law Review* 118, p.836.
[2] For example, the 1992 rules standardized the format of compensation disclosures and required the reporting of option grant values and performance measures used for managerial evaluations (Perry and Zenner, 2001).
[3] Other rules include the new reporting format detailing equity grants and the mandated disclosures of compensation peer groups.

may be ineffective against entrenched managers (Bebchuk, 2005). Thus, it is an empirical question whether better executive compensation disclosures can lead to more intense shareholder monitoring of top executives. In this paper, we shed light on this question by studying the impacts of enhanced CFO compensation disclosure on firms' pay and termination decisions related to these executives and their main job function, financial reporting practices.

We focus on the CFO compensation disclosure mandate instead of other pay disclosure rules because it provides a unique setting to identify the causal impact of enhanced compensation disclosures. Generally, the interpretation of any observed compensation changes following a new compensation disclosure-themed regulation can be obscured by confounding events that coincide with the regulation. For instance, the option expensing rule in FAS123R could lead to compensation changes around the same time of the CFO compensation disclosure mandate. However, such confounding events affect pay practices for *all* executives. By contrast, the CFO compensation disclosure mandate uniquely targets the CFO, not any other executives. In fact, the reporting of CEO pay has always been required since 1934. Thus, we compare the post-mandate changes in compensation between the CFO and the CEO in the same firm. The specification controls for omitted factors that influence general pay practices at the firm without differential implications on the CFO. As such, our results bear the unique interpretation of the impact of the CFO compensation disclosure mandate.

We also exploit cross-sectional variations in this setting to further corroborate our interpretation. Before the CFO pay disclosure mandate, firms were required to report only the compensation of the chief executive officer (CEO) and four other most highly paid executives which may or may not include the CFO. After the mandate, CFO compensation must be disclosed along with CEO compensation regardless of their pay rankings. Thus, we expect differential

impacts of the disclosure mandate across firms with different pre-mandate pay reporting status. We hypothesize that firms that never or seldom reported CFO pay prior to 2006 are affected the most by the mandate; firms that always reported CFO pay are not affected. Firms that sometimes reported CFO pay lie in between.

We begin our empirical analysis by examining CFO compensation changes. Improved investor monitoring can have opposing effects on CFO total pay. On the one hand, CFO pay may decline as it becomes more difficult for executives to set their own pay. On the other hand, more intense monitoring means fewer perks for the executives and, as a result, they may require higher pay as compensation (Hermalin and Weisbach, 2012). For instance, Faulkender and Yang (2013) find that a mandatory disclosure requirement on compensation peer groups exacerbates the use of high-pay benchmarks. In any circumstance, however, investors should push toward better shareholder-manager incentive alignment by granting more equity incentives to the CFO.

We examine the S&P 1,500 firms (past and present) between 1999 and 2013. We find results consistent with Hermalin and Weisbach (2012). CFO total pay increases after 2006, but not CEO pay. We explore cross-sectional variations of the result in a balanced panel of 1,003 firms.[4] We compare CFO total pay before and after the disclosure mandate across the pre-mandate reporting groups, and contrast CFO pay with CEO pay. Consistent with our conjecture, the CFO pay increases are the most salient in firms that never or seldom reported CFO pay previously, while they are close to zero in firms that always reported CFO pay.[5] The average increase in CFO pay in

[4] The constant sample eliminates possible sample compositional effects. However, our results are not driven by the use of a constant sample. As we show in Section 3.3, the results are robust when we allow firms to drop out of the sample after 2006, and when we do not place any restriction on firm entry and exit.

[5] To tackle the problem that CFO pay is sometimes unobserved before 2006 in firms in the "never", "seldom", and "often" reporting groups, we construct a proxy CFO pay measure by assigning the lowest reported top five executive's pay to the CFO when CFO pay is not reported. This method over-estimates CFO pay before 2006 because unreported CFO pay is actually below the lowest pay among the top five ranks. This strategy makes it more difficult for us to find evidence of CFO pay increases while easier to find evidence of CFO pay declines. We also check robustness of our results using actual CFO pay in Section 3.3.5 and find qualitatively similar results.

the "never" reporting firm group is 17 percentage point higher than that in the "always" reporting firm group. By comparison, there are no significant changes in the CEO's total pay after 2006 in any group of firms; our results are also robust to using the ratios of CFO pay relative to the pay of the CEO or other executives. Overall, the results support the interpretation that the compensation disclosure mandate leads to higher CFO total pay.[6] Regarding equity incentives, we find that CFO (but not CEO) equity incentives increase more at firms in the "seldom" reporting group than at those in the "always" reporting group.[7] This result suggests that, although CFOs ask for higher pay under enhanced monitoring, boards ensure that incentives are strengthened.

More intense monitoring following the compensation disclosure mandate can also affect CFO turnover. We expect a greater sensitivity of CFO turnover to firm performance, particularly accounting performance because the CFO has direct influences on it. We find that CFOs at firms in the "never" and "seldom" reporting groups are much more likely to depart their firms following poor accounting performance than CFOs at firms in the "always" group after the disclosure mandate. By comparison, we do not find the same result for CEO turnover.

In the final set of our analysis, we study the CFO's main job function, financial reporting. Enhanced monitoring may pressure the CFO to do a better job at improving financial reporting quality. On the other hand, both the strengthened equity incentives in pay and the greater threat of

[6] We conduct several additional tests to further address the concern that the results are driven by fundamental differences between firms in different pre-mandate reporting groups that do not necessarily reflect the extent to which the firms are affected by the disclosure mandate. First, we include in the main regression specifications not only firm characteristics (including corporate governance measures) affecting compensation but also their interactions with the post disclosure mandate time dummy. Second, we employ a propensity score matching procedure to identify counterparts of firms that never or seldom reported CFO pay before 2006 among the "always" and "often" reporting firms and repeat the regression analysis in the propensity score matched subsample. Third, we employ the Core, Holthausen, and Larcker (1999) compensation model to project CFO pay in the post-2006 period and define excess CFO pay as actual CFO pay minus projected CFO pay. We then test whether excess CFO pay is significantly greater among firms that seldom disclosed CFO pay. Our results are robust under all these strategies and reinforces our interpretation.

[7] We cannot use a proxy for missing equity incentives when a firm does not disclose CFO pay. Therefore, firms in the "never" group are excluded from the test.

turnover-for-poor performance due to more intense monitoring can make the CFO place a heavier emphasis on short-term performance goals. Therefore, he may engage in more short-term behavior in corporate reporting practices (Jiang, Petroni, and Wang, 2010; Edmans, 2011; Laux, 2012). We examine changes in earnings surprises and financial reporting quality after the compensation disclosure mandate. Jin and Myers (2006) and Bleck and Liu (2007) argue that corporate managers often hoard bad news which can later lead to large negative realizations of asset values. We find that, after the CFO compensation disclosure mandate, firms have more large negative earnings surprises and the change is more salient among firms in the "never" and "seldom" reporting groups. These results are consistent with more bad news hoarding after the mandate. On the other hand, we only find limited evidence of worse reporting quality after the mandate. Specifically, firms in the "seldom" reporting group have larger positive total and discretionary accruals, a higher propensity to just beat analyst earnings forecasts, and lower accruals quality. However, firms in the "never" reporting group do not experience large declines in reporting quality.

This paper contributes to our understanding of the effects of enhanced compensation disclosure. We show that, after compensation disclosure becomes mandatory, CFOs receive higher compensation and greater equity incentives. CFOs are also more likely to depart their firms if firm performance is poor. These results are consistent with improved monitoring under mandatory CFO pay disclosure. Our study complements recent research examining some other compensation disclosure regulations.[8] By exploiting a unique regulatory event that allows us to effectively control for omitted factors and confounding events, our results provide more convincing evidence for a causal link between compensation disclosure and executive pay practices. Our findings may

[8] Craighead, Magnan, and Thorne (2004) and Park, Nelson, and Huson (2001) examine an amendment to Regulation 638 in Canada that requires U.S.-like executive compensation disclosures. Vafeas and Afxentious (1998) study changes in the structure of compensation committees and the relation between CEO pay and firm performance following the SEC's new compensation disclosure rule in 1992.

shed light on the recent say-on-pay regulatory campaign which also permits shareholders to participate in the executive pay setting process.

This paper also adds to the recent literature on CFO pay and answers to the increasing regulatory emphasis on the CFO since the outburst of the public scandals of Enron and alike and the passage of the Sarbanes-Oxley Act (SOX).[9] Prior studies document a significant correlation between CFO equity incentives and earnings management (Jiang et al., 2010), stock crash risk (Kim, Li, and Zhang, 2011), and material accounting manipulation (Feng, Ge, Luo and Shevlin, 2011). Causal links are yet to be established. To the extent that the CFO's concerns about pay and turnover are the channel through which compensation disclosure affects CFO reporting behavior, our evidence supports a causal impact of CFO incentives on financial reporting. Our comparisons of firm characteristics and industry distribution of firms in various reporting groups also complement recent research relating CFO compensation to the functionality of the position (Hui and Matsunaga, 2015; Hoitash, Hoitash, and Johnstone, 2012).

The remainder of the paper proceeds as the follows. Section II introduces the sample and compares firm characteristics across pre-2006 CFO pay reporting groups. Section III examines the effect of the mandatory compensation disclosure rule on CFO pay and turnovers. Section IV investigates the effects of the disclosure mandate on firms' financial reporting practices. Section V concludes.

## II. Data and firm characteristics across reporting groups

We obtain the executive compensation information from the ExecuComp database, stock level information from CRSP and financial accounting data from Compustat. We get institutional

[9] As stated in the 2006 SEC rule, the regulators "believe that compensation of the principal financial officer is important to shareholders because, along with the principal executive officer, the principal financial officer provides the certifications required with the company's periodic reports and has important responsibility for the fair presentation of the company's financial statements and other financial information." Proposed and final rules of the SEC in release numbers 33-8732A, 34-54302A, and IC-27444A.

ownership data from Thomson Reuters Database and board of director information from the ISS database.

Our sample consists of a balanced panel of S&P1500 firms (past and present) in 1999-2013. That is, for inclusion in the sample, we require a firm to exist during the entire 1999 - 2013 period (i.e., 7 years before and 7 years after the disclosure mandate). This sampling strategy has two benefits. First, it ensures that no compositional effects are at play. Second, it allows us to define firm reporting groups based on the number of times CFO pay was disclosed in the 7 years prior to 2006.[10] The final sample consists of 1,003 firms. We classify these firms into four groups according to their CFO pay reporting frequency during the pre-disclosure-mandate period: "never" (reporting CFO pay no time), "seldom" (reporting 1-3 times), "often" (reporting 4-6 times), and "always" (reporting 7 times). 405 firms belong to the "always" group, 452 firms belongs to the "often" group, 103 firms belong to the "seldom" group, and 43 firms belong to the never group. This means about 15% of firms never or rarely report their CFO compensation before the disclosure mandate.

In Table 1, Panel A, we compare firm characteristics of firms before the disclosure mandate across different CFO pay reporting groups. Firms that never or seldom reported CFO pay prior to 2006 were larger in assets and sales, more profitable, and valued higher relative to their book values, had lower financial leverage, and made more capital expenditures but relatively less research and development investment. These firms also had smaller risk evident by lower volatilities in sales, cash flow, and stock return. Thus, firms that never or seldom reported CFO pay prior to 2006 (because CFO pay was below top 5) seem to have better financial standing,

[10] We acknowledge that the balanced panel excludes firms that join in or drop from the sample during the sample period. However, we do not think that survivorship bias is a concern in our context because there is no theoretical reason why the CFO pay level or its changes should relate to corporate survivorship. Despite this, we later test the robustness of our results in a non-balanced panel.

greater transparency, and lower risk than firms that always or often reported CFO pay. These results suggest that firms with worse financial standing, lower transparency, and higher risk attach greater importance to the CFO position.

We also examine the distribution of the CFO reporting status for different industries. Firms in the Finance and Retail Trade industries are the least likely to report CFO pay while firms in Transportation, Mining, and Construction are the most likely to report CFO pay before 2006. Thus, it seems that CFOs are relatively more important in industries where they are more likely to contribute greatly to their firms. In sum, the results on firm characteristics and industry distribution generally support the contracting theory that cross-sectional variations in CFO pay reflects compensation for job difficulty and productivity.

## III. Results on CFO pay and turnovers

In this section, we study the change in CFO compensation following the SEC disclosure mandate. Because CFO pay is unobserved in some firms before 2006, we construct a proxy CFO total pay measure that assigns the lowest pay among the top five executives to the CFO when CFO pay is not reported. Because the unreported CFO pay is always below the lowest top five pay, this method overestimates CFO pay before 2006. It would thus make it more difficult for us to find evidence of CFO pay increases while easier to find evidence of CFO pay declines. Moreover, the "never" and "seldom" firm groups should be under the most influence of such a bias.

### *3.1. Univariate analysis*

Table 2 presents univariate comparisons of executive pay across the CFO pay reporting groups and over time. We report the average total pay of the CFO (using the proxy measure), the CEO, and the other three top executives who are the most highly paid beside the CEO and the CFO. We also report the average ratio of the proxy CFO total pay to CEO total pay and the average ratio of the proxy CFO total pay to the average of other executives' total pay. Panel A presents

statistics over the entire sample period of 1999-2013. Total pay of all executives increases across the four reporting groups: roughly speaking, firms in the "never" group pay their executives the most, while firms in the "always" group pay their executives the least. However, the extent to which total pay increases across groups is the least for the CFO. Consistent with this, the ratio of CFO pay to CEO pay or to other executives' pay declines almost monotonically across the reporting groups: the ratios are the lowest for firms in the "never" or "seldom" group and the highest for firms in the "always" group. For example, while the average ratio of CFO pay to CEO pay is 0.594 in the "always" group, it is only 0.496 in the "never" group. Thus, in firm groups that pay their top executives more (that are presumably larger firms), the size of CFO pay relative to other executives including the CEO tends to be smaller.

Panel B presents average executive pay before the disclosure mandate in December 2006 and Panel C presents the average executive pay after the disclosure mandate. The average CFO total pay of "always" reporting firms is $1,680 thousand (median=$1,028 thousand) before the mandate. After the mandate, the average CFO pay is $1,936 thousand and the median is $1,415 thousand, respectively. The growth in CFO pay is modest: about $250 thousand on average. For the "never" reporting group firms, proxy CFO pay averages $2,061 thousand with a median of $1,253 thousand before 2006, while the average CFO pay after 2006 is $2,734 thousand with a median of $1,867 thousand. Compared with the "always" group, the growth in CFO pay in the "never" group of firms is much larger: around $700 thousand on average. There is a similarly large increase in CFO pay in the "seldom" group of firms, while the increase in CFO pay is more modest for firms in the "often" reporting group. Given the fact that the proxied CFO pay is overestimated before 2006, it is more difficult for us to find a pay increase particularly for the "never" and

"seldom" groups of firms. Therefore, the actual magnitude of the CFO's pay increase should be even larger for firms that did not always report CFO pay before 2006.

By comparison, the pay growth for the CEO and other executives around 2006 is not very different across the reporting groups. As a result, the ratio of CFO pay to CEO pay or other executives' pay further confirms the pattern in the change of proxy CFO total pay across reporting groups: the size of CFO pay relative to the CEO's and other executives' pay increases dramatically for firms in the "never" reporting group, while it declines for firms in the "always" reporting group. Overall, the results in the univariate comparisons of executive compensation around the CFO compensation disclosure mandate and across the pre-mandate reporting status groups reflect changes in pay unique to the CFO and is most likely the outcome of the disclosure mandate. These results are consistent with the labor market influences hypothesis while inconsistent with enhanced monitoring by shareholders.

We also depict the time series of the ratio of proxy CFO total pay to CEO total pay throughout the sample period to better illustrate the time trend. Figure 1 compares the medians of the ratio overtime between firms in the "always" or "often" group and those in the "seldom" or "never" group. The figure suggests that, while the CFO-to-CEO pay ratio was quite different between the two sets of firms before 2006, it converges after 2006. In particular, the ratio increases significantly among firms in the "seldom" or "never" reporting group. The increase in the median ratio is about 5%. The time patterns in CFO pay relative to CEO pay support the notions that firms with different CFO pay reporting statuses respond to the disclosure mandate differently and that the effects are unique to the CFOs.

*3.2. Regression analysis*

We next examine the CFO pay in a regression setting. Our primary regression equation can be described as follows:

$$y_{it} = \alpha + \beta_1 d2006rule + \beta_2 d2006rule * often + \beta_3 d2006rule * seldom + \beta_4 d2006rule * never + \gamma_1 * Controls_{it} + \gamma_2 d2006rule * Controls_{it} + \varepsilon_{it} \quad (1)$$

where $y_{it}$ is the dependent variable to be examined; $d2006rule$ is a dummy variable that equals one for firm-years with fiscal year ends on or after December 15, 2006 (the disclosure mandate effective date) and zero otherwise; $never$ is a dummy variable that equals one if a firm belongs to the "never" group and zero otherwise. $seldom, often, and\ always$ dummies are defined similarly. We are interested in the estimated coefficients $\beta_2 \sim \beta_4$. Such a regression setting allows us to compare the effects of the disclosure mandate on the dependent variable across firm groups. To be more specific, $\beta_2$, $\beta_3$, or $\beta_4$ measures the difference in the effect of the disclosure mandate between the "often", "seldom", or "never" group and the "always" group.

We include in the regression model a host of control variables that may affect executive compensation, including various firm characteristics and corporate governance measures (Fernandes et al. (2013)). This is to address the concern that firm characteristics may change after 2006, leading to apparent differential responses across the CFO pay reporting groups. An additional concern is that firms with different characteristics can respond to the law change differently, which cannot be captured by the control variables alone. Thus, we additionally include in the regression model the control variables interacted with the d2006rule dummy. These control interactions will absorb any differential responses to the disclosure mandate pertaining to each firm characteristic so that $\beta_2$, $\beta_3$, and $\beta_4$ cleanly capture the differential responses by firms with

different CFO pay reporting statuses. Detailed variable definitions can be found in the Appendix. All regressions included industry fixed effects at the two-digit SIC level.

The regression results, presented in Table 3, are consistent with the univariate results. Relative to firms that always reported CFO pay before 2006, CFO pay increases more for firms that often reported CFO pay, but it increases even more among firms that seldom or never reported CFO pay. Column 1 suggests that the "seldom" and "never" groups of firms increase their CFO pay by 18.8% and 16.8% more than the "always" group of firms. When we examine CEO pay as a benchmark, we find no significant difference in changes in CEO pay after 2006 across groups (Column 2). The test using the difference in CFO pay and CEO pay further confirms that, relative to CEO pay, CFO pay increases after 2006 and that such an increase is the greatest in the "never" group (Column 3). Assuming that CEO pay reflects any changes in a firm's general executive compensation policies, these results suggest that the observed large increases in CFO compensation, particularly if the firm is not automatically compliant with the mandatory CFO compensation disclosure, are unique to the financial officers. The economic magnitude of the effect is in line with that from the univariate comparison: the "never" group of firms experience an increase in CFO pay (relative to CEO pay) that is 24.1% higher than the "always" group of firms. Take an average CFO pay of $2 million, the effect amounts to about half a million dollars. Furthermore, the results are reiterated when we examine the ratios of CFO pay to CEO pay (Column 4) and to other top three most highly paid executives' pay (Column 5). All the regression results remain quantitatively similar if we control for firm fixed effects instead of industry fixed effects.

There are some interesting results regarding the regression coefficients on the control variables. Larger and better performing firms pay their CFOs and CEOs both more, as one would

expect. Firms with greater institutional ownership and larger and more independent boards pay their executives more, consistent with prior literature (e.g., Fernandes et al. 2013). Not surprisingly, the CEO-chairman duality is associated with significantly greater pay for the CEO but not the CFO. For the most part, the interaction terms between the control variables and the d2006rule dummy do not have significant coefficients, with the exceptions of stock return volatility and institutional ownership. This suggests that the concern about changing sensitivity of CFO pay on firm characteristics is perhaps not too worrisome.

Lastly, we consider a falsification test examining the other three most highly paid executives' average pay. If the effects in executive compensation are specific to the CFO but not any other executive, we should not expect to see a significant increase in the other three top executives' pay. To be comparable with the CFO test, we use the ratio of the other three top executives' average pay to the CEO's pay as the dependent variable and run the same regression specification as in the other columns of Table 3. The result is presented in Column 6. As expected, there is no significant change in the other three executives' pay to CEO pay ratio after the mandate for any group of firms. This result further strengthens the interpretation that the increase in CFO pay is the result of the CFO compensation disclosure mandate.

### *3.3. Robustness checks*

#### *3.3.1. Excluding SOX years*

One concern is that the Sarbanes-Oxley Act (SOX) affects CFO pay in a similar direction. To address that concern, we shrink our sample period to 2002-2011. The reduced time period does not contain the pre-SOX period and, thus, is not contaminated by any SOX effect. Column 1 of Table 3, Panel B reports the regression result of the log difference between proxy CFO pay and

CEO pay, following the specification in Column 3 of Table 3, Panel A (the base case). The results are highly consistent with those in the base case.

*3.3.2. Excluding Great Recession years*

To ensure that our results are not driven or biased by the recent financial crisis, we exclude 2008 and 2009 from our sample and repeat the analysis. Column 2 of Table 3, Panel B reports the regression result. The results are almost identical to those in the base case.

*3.3.3. Excluding financial and utility industries*

We also check robustness by excluding financial and utility industries because pay practices may follow different dynamics in these industries. As Column 3 of Table 3, Panel B shows, the results are again in line with those in the base case.

*3.3.4. Inflation adjustment*

To check whether our results are sensitive to inflation adjustments, we adjust the CFO and CEO pay figures by inflation and re-estimate the regression model following the base case specification. As Column 4 of Table 3, Panel B shows, the results are largely consistent with those in the base case. The coefficient on d2006rule*never is economically large, although the *p*-value of the coefficient is slightly above the 0.10 significance cutoff.

*3.3.5. Actual CFO pay*

One could wonder whether our results are specific to the use of proxy CFO pay rather than actual CFO pay. To explore this possibility, we conduct an additional analysis using actual CFO pay before 2006. (Note that CFO pay after 2006 is always actual.) Firms that never disclosed CFO pay before 2006 can no longer be included in the analysis because no observation of actual CFO pay is available before 2006 for these firms. Firms that sometimes disclosed CFO pay before 2006, and particularly those that seldom disclosed it, would have relatively few observations compared

with the post-mandate period, which could result in low power in the statistical tests. To alleviate the problem, we interpolate the CFO total pay variable by filling missing observations using the average of the two most adjacent nonmissing observations. Such an interpolation strategy is based on an assumption that total pay (and later, pay structure) does not vary dramatically between two adjacent years, and that even if it does, the sum of the variations over time should not be biased in a particular direction.

Column 5 of Table 3, Panel B shows the results. The never group dummy and its interaction terms drop out of the model due to missing observations, and we focus on the interaction term between the seldom group dummy and the mandate time dummy. We find a significant coefficient on the term, suggesting that firms that rarely disclosed CFO pay experience a significant increase in CFO pay after the mandate. The coefficient of 0.192, which is significant at the 5% level, is larger in magnitude than that using proxy CFO pay (Column 3 of Table 3, Panel A). This is not surprising given that the proxy CFO pay overestimates the pre-mandate CFO pay.

*3.3.6. Propensity score matching*

In our main regression specifications in Table 3, we control for various firm characteristics and corporate governance measures and their interaction terms with the disclosure mandate time dummy. The strategy is used to parse out any changes in compensation due to its relations with these control variables and the possibility that such relations may vary around the disclosure mandate. We also consider an alternative approach to further address this concern, i.e., the propensity score matching approach. We first estimate the propensity for a firm to be in the "never" or "seldom" group (treated group) and not in the "always" or "often" group (control group) prior to 2006 based on firm characteristics that affect executive compensation (see Table 3) and additional variables that differ across reporting groups (see Table 1). For each firm in the treated

group, we then look for its closest match from the control group by the estimated propensity score, within a caliper of 0.25 times its standard deviation (Rosenbaum and Rubin (1985)). Finally, we re-estimate the regression models from Table 3 in the subsample containing all treated firms for which we find a corresponding control firm and their matched control firms.

The results are summarized in Table 4. Panel A compares the firm characteristics and proxy CFO total pay between the treated firms and their matched control firms in the pre-mandate period. There are no economically or statistically significant differences in any of these variables in either their means or medians, suggesting a good matching quality. Panel B reports the regression results of compensation measures following the specifications in Table 3. These results are consistent with those in Table 3. The results again support the idea that our results are not driven by different firm characteristics between firms with different pay reporting statuses before the disclosure mandate.

*3.3.7. Excess CFO pay*

To further address the concern of different characteristics between firms with different pay reporting statuses, we employ a methodology that follows the idea of Core, Holthausen and Larcker (1999). We first regress CFO total pay (actual) on its fundamental determinants in years 1999-2006 (December) and generate coefficients on the determinants. We additionally include in the regression dummies for CFO pay reporting groups and industry dummies to account for any time-invariant components in CFO pay within each category. Because actual CFO pay data are missing for firms that never reported CFO pay in the pre-2006 period, these firms are naturally absent from the analysis.

In the next step, we apply the regression coefficients to the data after December 2006 and calculate the predicted CFO total pay for each firm and year (except firms in the "never" reporting

group). Finally, we construct the excess CFO pay variable by subtracting the predicted CFO total pay from the actual CFO total pay. We then test whether excess CFO total pay differs across the various reporting groups. Particularly, we check whether, after the disclosure mandate, firms that seldom reported CFO pay before 2006 have larger excess CFO pay than firms that always reported CFO pay.

The results are summarized in Table 5. Excess CFO pay is $318,939 greater among firms that seldom reported CFO pay than firms that always reported CFO pay. Firms that often reported CFO pay also have greater CFO pay than firms that always reported CFO pay, though by a less magnitude. The same results do not hold when we check CEO excess pay (constructed in a similar way). These results confirm our results in the baseline analysis. Thus, we conclude that our results are not driven by different firm characteristics between firms in different pre-2006 pay reporting groups.

*3.3.8. Unbalanced panels*

Our main sample is a balanced sample consisting of firms that exist during the entire sample period of 1999-2013. One can be curious whether our results still hold in unbalanced panels that allow firms to enter or exit sometime during the sample period. We thus consider two alternative samples. In the first alternative sample, we require the firms to exist in the pre-mandate years during 1999 to December 2006, but allow the firms to disappear afterwards. In this alternative sample, we remain able to classify firms into the four groups based on the frequency in which each firm reported CFO pay among top five executives prior to 2006. We can thus employ the same regression specification as in the main analysis in Table 3 and focus on the interactions between the mandated disclosure time dummy, *D2006rule*, and the reporting group dummies, *often, seldom,* and *never.* Tests in the alternative sample address the concern that surviving firms

have different pay practices from non-surviving firms. In the second alternative sample, we do not impose any restriction and include all firms covered by the ExecuComp database. Because we cannot define the reporting groups, we will focus on just the post-disclosure mandate dummy, *D2006rule* and test whether CFO pay increases are a robust result in the general sample allowing entries and exits of firms.

The results are summarized in Table 6. Panel A corresponds to the results using the first alternative sample and follow the regression specifications in Table 3. Consistent with those of Table 3, the results show that firms in the "never" and "seldom" reporting groups increase CFO pay significantly more than firms in the "always" group. Firms in the "often" group increase CFO pay more than firms in the "always" group, but less than firms in the "never" and "seldom" groups. Again, the results are specific to the CFO, as the change in CEO pay is similar across firm groups. The relative CFO pay measures also produce similar results, confirming the ones in Table 3. Overall, the evidence in the first alternative sample suggests that our main results reported in Table 3 are not specific to the constant sample.

Panel B presents results using the second alternative sample, i.e., all ExecuComp firms without any restriction. Because we cannot divide firms by their pre-2006 reporting groups in this enlarged sample, we no longer have the dummies representing the reporting groups or their interactions with the disclosure mandate time dummy. Similarly, we do not need the interaction terms between the firm control variables and the time dummy. The coefficient on *D2006rule* estimates the average effect of the 2006 CFO compensation disclosure mandate on the dependent pay variable. We confirm that CFO pay increases significantly while CEO pay remains unchanged after 2006, and that the relative size of CFO pay to CEO pay or to other top three executives' pay

increases after 2006 as well. Again, the results in the most encompassing sample are consistent with our results in Table 3 using the constant sample.

*3.4. CFO equity incentives*

We additionally examine whether and how the equity incentives in CFO pay are affected by the disclosure mandate. We compare the changes in CFO equity incentives relative to those of the CEO and across the reporting groups. This investigation suffers a data limitation. While it is reasonable to use a proxy measure for CFO pay level when actual CFO pay is unobserved, the same cannot be argued about pay-performance sensitivity. Therefore, we have to use actual CFO pay data in this test, and then use the same interpolation strategy as in Section 3.3.5 to make the number of observations more comparable across groups before and after the mandate. The term $\beta_4 d2006rule * never$ naturally drops from the regression model.

The first measure of equity incentives we consider is portfolio delta, constructed following Core and Guay (2002). The delta measure reflects the dollar change in an executive's wealth for a 1% increase in the firm's stock price. In columns 1 to 3 of Table 7, we present results from regressions of CFO delta, CEO delta, and CFO delta minus CEO delta. Importantly, the "seldom" group of firms increase delta more after 2006 than the "always" group (the coefficient on the interaction term *D2006rule*seldom* is 37.953 and *p*-value = 0.020). The difference in the change of delta for "seldom" firms and "always" firms is statistically significant and economically nontrivial, considering that the median delta across all firms and years is $42 thousand. The "often" group also increase delta more than the "always" group though the effect is statistically insignificant (the coefficient on the interaction term *D2006rule*often* is 13.496 and *p*-value = 0.145).

We also use the equity incentive ratio described by Bergstresser and Philippon (2006) and Jiang et al (2010). The equity incentive ratio is delta normalized by delta plus salary and bonus. Bergstresser and Philippon (2006) point out that "(the measure) captures the share of a hypothetical executive's total compensation that would come from a one percentage point increase in the value of the equity of his or her company". Since the equity incentive measure is a ratio between 0 and 1, we follow the statistics literature to transform it into ln(incentive/(1-incentive)), which has nicer statistical properties. This is done for both the CFO's and the CEO's equity incentives but not the difference between the CFO and CEO incentives. The results are presented in columns 4 to 6 of Table 7. The results are in line with those using portfolio delta. CFO equity incentives in the "seldom" group of firms significantly increased more after disclosure mandate than those in the "always" and "often" group (the coefficient on the interaction term *D2006rule*seldom* is 0.547 and *p*-value = 0.014). This suggests that the CFO equity incentive in the "seldom" group is increased more than 40% than that of the "always" group after the disclosure mandate.

Therefore, the evidence suggests that the disclosure mandate leads to increases in both the level and the equity incentives of CFO compensation. The equity incentive result is consistent with more intense monitoring, thus reinforcing our interpretation of the increased total pay result.

*3.5 CFO turnover*

More intense monitoring following the compensation disclosure mandate can affect CFO turnover. We hypothesize that, after compensation disclosure becomes mandatory, CFOs are more likely to be terminated for poor firm performance.

We start the CFO turnover data collection from ExecuComp. For firm-years missing the CFO data in ExecuComp, we collect the information from 10-k filings. On average, 15% of CFOs

are turned over each year. This is slightly above the proportion of CEO turnovers (11%), implying that CFOs generally have shorter tenure than CEOs. The regression model is the follows:

$$Turnover_{it} = \alpha + \beta_1\, d2006rule * R + \beta_2\, d2006rule * often * R + \beta_3\, d2006rule * seldom * R + \beta_4\, d2006rule * never * R + \emptyset_1\, R + \emptyset_2\, often * R + \emptyset_3\, seldom * R + \emptyset_4\, never * R + \theta_1 d2006rule + \theta_2 d2006rule * often + \theta_3 d2006rule * seldom + \theta_4 d2006rule * never + \gamma_1 * OtherControls_{it} + \varepsilon_{it} \quad (2)$$

In this model, *R* is firm performance over the last year. We measure firm performance by return-to-assets (*ROA*) and stock return. The list of controls include all the double interactions among d2006rule, R, and the reporting group dummies, as well as each of these variables. It also includes common controls for executive turnovers. The coefficients on the triple interaction terms, $\beta_1$–$\beta_4$, capture the effects of the disclosure mandate on the sensitivity of CFO turnover to firm performance for firms in various reporting groups. For example, $\beta_4$ measures the effect of the disclosure mandate on the CFO turnover-to-performance sensitivity for firms in the "never" reporting group relative to that for firms in the "always" group. Our hypothesis implies that firms affected more by the mandate should experience a larger post-mandate increase in the CFO turnover-to-performance sensitivity. That is, $\beta_4$ should be significantly negative.

We report the turnover probit regression results in Table 8. In Columns 1-2, firm performance is measured by ROA. Column 1 examines all CFO turnovers. The estimated $\beta_2 - \beta_4$ are all negative and $\beta_3$ and $\beta_4$ are statistically and significantly different from zero. These results are consistent with our hypothesis. After the implementation of the disclosure mandate, firms who never or rarely reported CFO pay previously are more likely to see their CFOs depart them following poor accounting performance. In Column 2, we consider forced CFO turnovers, defined

as turnovers of CFOs at the age of 60 or younger. The results are very similar to those of all turnovers in Column 1. In Columns 3–4, firm performance is measured by stock return. For both all turnovers and forced turnovers, the estimated $\beta_2 - \beta_4$ are statistically indistinguishable from zero. Thus, after the disclosure mandate, the CFO turnover-to-stock return sensitivity does not change differentially between firms in different reporting groups. The different results between ROA and stock return are perhaps because CFOs have direct influences on accounting returns, but only indirect impacts on stock performance.

We also conduct a parallel analysis on CEO turnovers to address a potential concern that some confounding event drives greater turnover-to-performance sensitivity of all executives. Columns 5 – 8 present the results from the parallel tests on CEO turnovers. We find no differential change in the sensitivity of CEO turnover to firm performance across the various reporting groups after the disclosure mandate. Therefore, the results on changes in executive turnover-to-performance sensitivity after 2006 are not driven by a confounding event that affects turnovers of all executives, but are unique to the CFOs. These results on executive turnovers corroborate our hypothesis that the CFO compensation disclosure mandate leads to greater transparency and more intense monitoring on the CFO, which is manifested as greater CFO turnover-to-performance sensitivity.

## IV. Results on corporate financial reporting practices

In this section, we examine the impact of CFO compensation disclosure mandate on corporate financial reporting practices, the main job function. We first investigate the level of earnings relative to analyst forecast consensus (i.e., earnings surprises). We then examine financial reporting quality.

### *4.1. Negative earnings surprises*

Jin and Myers (2006) and Bleck and Liu (2007) argue that corporate managers often hoard bad news or hold on to bad projects which can later lead to large negative realizations of asset values. If higher pay makes the CFOs become more concerned about firm performance, they will engage in more bad news hoarding. This mechanism should lead to more severe outbursts of bad news. We examine annual earnings announcements and measure the amount of earnings surprises by the difference between the announced earnings per share (EPS) and the analysts' consensus (median) EPS forecast. We test the prediction that, following the CFO compensation disclosure mandating rule in 2006, firms should have more negative earnings surprises and that the change should be the most salient among firms that did not report CFO pay prior to the mandate.

The results from probit regressions, presented in Table 9, are consistent with our prediction. First, while there is no difference in the probability of negative earnings surprises before and after the 2006 rule for firms that always reported CFO pay, the probability increases for firms in the "seldom" and "never" groups of firms (Column 1). Particularly, the change in the probability of negative earnings surprises increases monotonically across the reporting groups: the marginal effect of the change in the probability of negative earnings surprises around the rule is 0.009 for firms in the "often" group (insignificant with *p*-value=0.646), 0.084 for firms in the "seldom" group (*p*-value=0.012), and 0.099 for firms in the "never" group (*p*-value=0.009). That is, firms in the "seldom" group are 8.4% more likely to have negative unexpected earnings and firms in the "never" group are 9.9% more likely to have negative unexpected earnings after 2006.

We also define several *Large Negative Surprise* dummies to capture large negative earnings surprises. It equals one if 1) the standardized unexpected earnings (SUE) is less than or equal to -1;[11] 2) the SUE is less than or equal to -2; 3) the earnings surprise is in the bottom quintile

[11] Standardized unexpected earnings (SUE) equals the difference between the annual fiscal EPS and the most recent consensus analyst forecast for that fiscal year standardized by the standard deviation of analyst forecast.

in the overall sample; or 4) the earnings surprise is in the bottom quintile in the corresponding year in the sample; and zero otherwise.

The results, presented in Columns 2-5, are also consistent with our prediction. Firms in the "never" group show the greatest increase in the likelihood to have a large negative earnings surprise, while the effect is weaker for the "seldom" group and disappears for the "often" group of firms. Therefore, the evidence suggests that firms are more likely to have large negative earnings surprises after the disclosure of their CFOs' compensation becomes mandatory, particularly if the firm was not automatically in compliance with the disclosure mandate previously. This is consistent with the notion that increased incentives prompt the CFOs to withhold bad news which in turn leads to negative earnings surprises.[12]

*4.2. Financial reporting quality*

We compare the changes in financial reporting quality across different reporting groups after the 2006 CFO compensation disclosure mandate. Jiang et al. (2010) show that greater CFO equity incentives are associated with more earnings management by the firm. Because mandated compensation disclosure leads to higher CFO equity incentives, we expect to see worsened financial reporting quality following the CFO compensation disclosure mandate. We measure financial reporting quality along three dimensions: accruals management, the likelihood of meeting or narrowly beating analyst forecasts, and accruals quality.

We use six accruals management measures: the absolute value of total accruals, positive total accruals, negative total accruals, the absolute value of discretionary accruals, positive discretionary accruals, and negative discretionary accruals. Total accruals are calculated as the difference between earnings before extraordinary items and cash flows from operations, scaled by

[12] Motivated by prior literature, we also checked whether stock crash risk increases significantly for firms most affected by the disclosure mandate, but did not find such evidence.

the previous year's total assets. The positive and negative accruals measures are used to further test whether firms manage their earnings more upward or downward.

Since not all accruals are manageable in terms of earnings management, we follow Jiang et al. (2010) to use Dechow, Richardson and Tuna (2003) procedure to measure the discretionary accruals. We first run the following regression to get the coefficients to estimate the non-discretionary accruals:

$$Total\ Accruals_{it} = \alpha + \beta_1\big((1+k)\Delta Sales_{it} - \Delta REC_{it}\big) + \beta_2 PPE_{it} + \beta_3 Total\ Accruals_{it-1} + \beta_4 SalesGrowth_{it+1} + \varepsilon_{it} \quad (3)$$

where $k$ is the coefficient obtained by regressing changes in accounts receivable ($\Delta REC_{it}$) on changes in sales ($\Delta Sales_{it}$) for each 2-digit SIC-year grouping. PPE stands for the gross amount of property, plant and equipment scaled by average total assets. Discretionary accruals are then estimated as the difference between the total accruals and the estimated nondiscretionary accruals (fitted value of the above regression).

The control variables are selected following Jiang et al. (2010). For example, Standard deviation of cash flows from operations (StdCashflow) and the standard deviation of revenues (StdRev) are included to control for firm-specific volatility. The regression Results are shown in Table 10.

The "always" group of firms do not change their accruals after the CFO compensation disclosure mandate, as evidenced by the insignificant estimated coefficients using all measures of accruals management ($p$-values range from 0.153 to 0.839). The "often" group of firms do not change their total accruals, but increase their discretionary accruals, particularly positive discretionary accruals. The "seldom" group of firms significantly increase their positive total accruals, absolute discretionary accruals, and positive discretionary accruals. Consistent with our expectations, the magnitude of the increase in the accruals is larger for firms in the "seldom" group,

which are more affected by the compensation disclosure mandate, than for firms in the "often" group. For example, in the regression of absolute discretionary accruals, the marginal effect of the interaction term D2006rule*seldom is 0.719 (*p*-value=0.043) and the marginal effect of the interaction term D2006rule*often is 0.492 (*p*-value=0.090). The results suggest that, after the 2006 mandatory disclosure rule and other things equal, the "seldom" group of firms increase their discretionary accruals by about 71.9 basis points more than the "always" group of firms. Across the board, it also seems that firms are managing their accruals upward more after the compensation disclosure mandate as the results mainly concentrate in positive accruals.

To our surprise, firms in the "never" group do not experience large increases in accruals. One potential explanation for this is that firms in this group are highly visible and under constant monitoring for misconduct. As a result, these firms do not significantly increase their earnings management activities. The coefficients on the control variables are largely as expected. For example, the standard deviations of cash flows and of sales growth are associated with more accruals, both positive and negative, which is consistent with more earnings management in a more volatile environment. Overall, the evidence in this table suggests that the mandated CFO compensation disclosures are associated with more accruals management.

Degeorge, Patel, and Zeckhauser (1999) suggest that a small earnings surprise over analysts' earnings forecast suggests a tendency of earnings management of a firm. We measure the small earning surprise over analysts' forecast using two measures. Following Liu and Xuan (2016), we compare a firm's actual annual EPS with its latest consensus (median) analyst forecast before the end of the fiscal year. Our first measure, the dummy variable "Meet", equals one if the actual EPS is exactly the same as forecast or just above the forecast by one cent and zero otherwise. Our second measure, the dummy variable "JustBeat", is equal to one if the EPS is exactly one cent

above consensus forecast and zero otherwise. The probit regression results about small surprise over analyst forecast are presented in Table 11.

The results show that the likelihood of earnings meeting or just narrowly beating analyst forecast significantly decreases in the post SOX years and after the CFO compensation disclosure mandate, consistent with Cohen, Dey, and Lys (2008). However, there is no significant difference across the reporting groups in the change of the probability to meet analyst forecasts. Consistent with the discretionary accruals results, the "JustBeat" regression shows that the "seldom" reporting group of firms are more likely to narrowly beat financial analyst forecast after the 2006 disclosure mandate, compared with the "always" reporting group of firms. The result suggests a higher tendency of earnings management in these firms after the 2006 new disclosure rule.

We consider a third dimension of financial reporting quality following the accruals quality measures in Billett and Yu (2015). Billett and Yu (2015) find that opaque firms (i.e., with lower accruals quality) experience positive abnormal returns twice the magnitude of transparent firms after controlling for earnings management, governance and firm characteristics. The accruals quality measures, Opacity and Opac3, are based on the variability of unpredicted accruals. Specifically, Opacity is calculated as the standard deviation of firm's residuals from year t-4 to year t by running the following regression equation for each industry-year separately:

$$TCA_{i,t} = \alpha + \beta_1 CFO_{i,t-1} + \beta_2 CFO_{i,t} + \beta_3 CFO_{i,t+1} + \beta_4 \Delta Sales_{i,t} + \beta_5 PPE_{i,t} + \varepsilon_{i,t}$$

Where $TCA_{i,t}$ is total current accruals for firm *i* in year *t* and is defined as follows.

$$TCA_{i,t} = \Delta CA_{i,t} - \Delta CL_{i,t} - \Delta Cash_{i,t} + \Delta STDEBT_{i,t}$$

CA is the current asset (ACT). CL is the current liabilities (LCT). Cash is the cash and short-term investment (CHE) and STDEBT is the debt in current liabilities (DLC). And Δ indicates the change from year *t*-1 to *t*. CFO is firm *i'* s cash flow from operations in year *t* and is defined

as the firm's net income before extraordinary items (IB) minus total current accruals (TCA) defined above and add depreciation and amortization (DP). Opac3 is calculated similarly as Opacity except it is based on the 3-year (t-2 to t) standard deviation of regression residuals instead of 5 years to minimize loss of observations.

We report the accruals quality regression results in Table 12. Using both opacity measures, we find that opacity significantly increases after the CFO compensation disclosure mandate among those firms that rarely reported CFO pay before 2006, compared with the "always" group of firms. The estimated coefficients are 0.007 (P=0.035) in the opacity regression, and 0.005 (P=0.079) in the Opac3 regression, respectively. Similarly as in previous tests of financial reporting quality, we do not find significant increases of opacity for firms that never reported CFO pay before 2006.

In sum, the evidence presented in this subsection suggests that the mandated CFO compensation disclosure is associated with more earnings management and deteriorated financial reporting quality, particularly for firms that rarely disclosed CFO pay and were under influence of the new compensation disclosure rule.

**V. Conclusion**

In this paper, we examine the effects of compensation disclosure on executive pay practices by utilizing a triple-differences empirical strategy surrounding the SEC's disclosure mandate of CFO compensation in December 2006. Theoretically, better compensation disclosures can reduce executive pay as enhanced investor monitoring limits discretionary pay. However, the executive may also require additional compensation for the lost private benefits due to more intense monitoring. We find that firms on average increase CFO pay after 2006, consistent with the latter hypothesis. The result is the strongest among firms most affected by the mandate (i.e., those that were not automatically in compliance with the mandatory disclosure rule when it was

implemented). By contrast, firms in which the CFOs were always among the top five most highly paid executives, which were automatically in compliance, hardly change CFO pay. In a conservative estimate, the increase in CFO pay after 2006 is about half a million dollars greater for firms that never disclosed CFO pay previously than firms that always disclosed CFO pay. Meanwhile, average pay of the CEOs hardly changes after 2006 in any firm group. This triple-differences test strategy, along with a host of robustness checks, suggest that the increase in CFO pay is the outcome of the disclosure mandate and not of another concurrent confounding event.

Corroborating enhanced investor monitoring after the disclosure mandate, we also find that equity incentives in CFO pay becomes stronger and that CFO turnovers become more sensitive to poor firm performance. When we look at the CFO's main job responsibility, financial reporting, we find some evidence that the CFOs hide bad news after the mandate, suggesting that they engage in short-term behaviors to boost firm performance. But there is only limited evidence that the CFOs lower corporate reporting quality.

Overall, our results suggest that enhanced compensation disclosure from mandatory disclosure rules leads to more intense monitoring. The result of higher CFO total pay does not support the proposition that mandated compensation disclosure can overcome managerial discretion and avoid further rises in U.S. top executives' pay. The mandated disclosure, however, leads to better investor monitoring which, in turn, results in more high-powered compensation and turnover policies. Our results may have implications for the say-on-pay voting requirement established by the Dodd-Frank Act which was also intended to facilitate investor monitoring of the managerial compensation processes.

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**Figure 1**

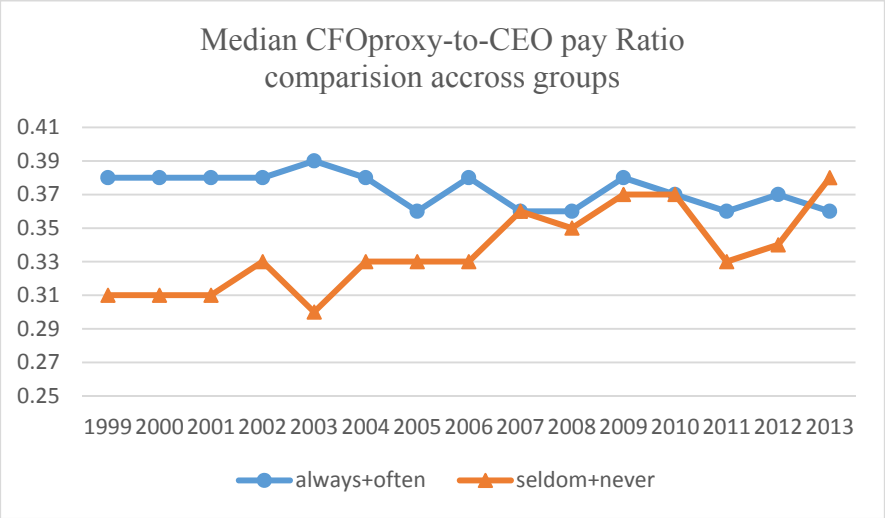


**Figure 1: The ratio of proxy CFO total pay to CEO total pay across groups overtime**
Proxy total pay is the lowest pay among the top five executives when CFO pay is unreported. We classify firms into four groups according to their CFO pay reporting frequency during the pre-disclosure-mandate period: "never" (firms never reporting CFO pay), "seldom" (reporting 1-3 times), "often" (reporting 4-6 times), and "always" (always reporting).

**Table 1: Firm Characteristics before disclosure mandate (1999 - December 15, 2006)**

This table compares firm characteristics across firms with different pre-2006 CFO pay reporting status. Detailed definition of variables can be found in Appendix. Variables are winsorized at [1%, 99%]. ***, **, and * stand for the 1%, 5%, and 10% significance level for the test of the difference in mean (median) between the indicated group and the "always" group.

*Panel A. Firm characteristics*

| | Mean | | | | Median | | | |
|---|---|---|---|---|---|---|---|---|
| | always | often | seldom | never | always | often | seldom | never |
| Assets | 7,151 | 13,908*** | 20,293*** | 32,602*** | 1,686 | 1,900** | 4,569*** | 3,382*** |
| Sales | 4,128 | 5,638** | 9,876*** | 10,907*** | 1,397 | 1,514* | 3,924*** | 2,783*** |
| MarkettoBook | 2.034 | 2.154*** | 2.234*** | 2.348*** | 1.464 | 1.516** | 1.658*** | 1.828*** |
| Book_leverage | 0.221 | 0.230* | 0.237** | 0.193*** | 0.212 | 0.221 | 0.226* | 0.155*** |
| Market_leverage | 0.160 | 0.160 | 0.162 | 0.131*** | 0.129 | 0.129 | 0.125 | 0.090*** |
| ROA | 0.130 | 0.125*** | 0.147*** | 0.153*** | 0.128 | 0.121*** | 0.138** | 0.154*** |
| R&D | 0.023 | 0.028*** | 0.023 | 0.018 | 0.000 | 0.000*** | 0.000 | 0.000 |
| CAPEX | 0.048 | 0.047 | 0.051*** | 0.059** | 0.036 | 0.035 | 0.041*** | 0.043** |
| StdSaleGrowth | 0.236 | 0.231 | 0.179*** | 0.175*** | 0.149 | 0.144 | 0.104*** | 0.127** |
| StdCashFlow | 0.049 | 0.046*** | 0.041*** | 0.041*** | 0.038 | 0.035*** | 0.031*** | 0.034** |
| StdRev | 0.147 | 0.138*** | 0.119*** | 0.126*** | 0.105 | 0.100* | 0.086*** | 0.088** |
| Number of firms | 405 | 452 | 103 | 43 | 405 | 452 | 103 | 43 |

*Panel B. Industry distributions*

| | | Number of firms | | | | | % in all | |
|---|---|---|---|---|---|---|---|---|
| Industry | SICs | All groups | always | often | seldom | never | always+often | never+seldom |
| Agriculture, Forestry, And Fishing | 01-09 | 1 | 0 | 1 | 0 | 0 | 100% | 0% |
| Mining | 10-14 | 44 | 12 | 27 | 2 | 3 | 89% | 11% |
| Construction | 15-17 | 17 | 11 | 4 | 1 | 1 | 88% | 12% |
| Manufacturing | 20-39 | 447 | 180 | 210 | 43 | 14 | 87% | 13% |
| Transportation, Communications, Electric, Gas, and Sanitary Services | 40-49 | 120 | 59 | 49 | 8 | 4 | 90% | 10% |
| Wholesale Trade | 50-51 | 30 | 15 | 10 | 4 | 1 | 83% | 17% |
| Retail Trade | 52-59 | 87 | 38 | 29 | 16 | 4 | 77% | 23% |
| Finance, Insurance and Real Estate | 60-67 | 115 | 33 | 56 | 17 | 9 | 77% | 23% |
| Services | 70-89 | 138 | 55 | 65 | 12 | 6 | 87% | 13% |
| Public Administration | 91-99 | 4 | 2 | 1 | 0 | 1 | 75% | 25% |
| Total | | 1,003 | 40% | 45% | 10% | 5% | 85% | 15% |

**Table 2 Executive Compensation around Disclosure Mandate**

The sample consists of 1,003 firms spanning a balanced panel of S&P1500 firms in 1999-2013. Variables are winsorized at the 1% and 99% value. Variables definitions are in the Appendix. ***, **, and * stand for the 1%, 5%, and 10% significance level for the test of the difference in mean (median) between the indicated group and the "always" group.

| **Panel A: All years** | Mean | | | | Median | | | |
|---|---|---|---|---|---|---|---|---|
| | always | often | seldom | never | always | often | seldom | never |
| Proxy CFO Pay | 1813 | 1987*** | 2311** | 2419*** | 1237 | 1307*** | 1705*** | 1521*** |
| CEO Pay | 5305 | 6004*** | 7778*** | 7108*** | 3413 | 3839*** | 5221*** | 4701*** |
| Other Three Executive Pay | 2027 | 2463*** | 3124*** | 3831*** | 1346 | 1536*** | 2357*** | 2370*** |
| Proxy CFO Pay/CEO Pay | 0.594 | 0.598*** | 0.448*** | 0.496*** | 0.382 | 0.362*** | 0.333*** | 0.361*** |
| Proxy CFO Pay/Other Three Executive Pay | 1.083 | 0.958*** | 0.843*** | 0.706*** | 0.995 | 0.897*** | 0.771*** | 0.666*** |

| **Panel B: Pre-mandate years** | Mean | | | | Median | | | |
|---|---|---|---|---|---|---|---|---|
| | Always | often | seldom | never | always | often | seldom | never |
| Proxy CFO Pay | 1680 | 1753 | 1957*** | 2061*** | 1028 | 1044 | 1337*** | 1253** |
| CEO Pay | 4916 | 5694*** | 7894*** | 7034*** | 2673 | 2977*** | 445*** | 3948*** |
| Other Three Executive Pay | 1888 | 2386*** | 3182*** | 3777*** | 1093 | 1318*** | 2169*** | 2181*** |
| Proxy CFO Pay/CEO Pay | 0.755 | 0.641*** | 0.416*** | 0.400*** | 0.394 | 0.364*** | 0.310*** | 0.324*** |
| Proxy CFO Pay/Other Three Executive Pay | 1.107 | 0.898*** | 0.707*** | 0.612*** | 0.989 | 0.825*** | 0.673*** | 0.637*** |

| **Panel C: Post-mandate years** | Mean | | | | Median | | | |
|---|---|---|---|---|---|---|---|---|
| | Always | often | seldom | never | always | often | seldom | never |
| Proxy CFO Pay | 1936 | 2197*** | 2626*** | 2734*** | 1415 | 1580*** | 2096*** | 1867*** |
| CEO Pay | 5661 | 6280*** | 7675*** | 7176*** | 4110 | 4629*** | 5866*** | 5602*** |
| Other Three Executive Pay | 2150 | 2533*** | 3070*** | 3880*** | 1550 | 1706*** | 2461*** | 2650*** |
| Proxy CFO Pay/CEO Pay | 0.448 | 0.560 | 0.482 | 0.580 | 0.373 | 0.360*** | 0.347** | 0.374 |
| Proxy CFO Pay/Other Three Executive Pay | 1.062 | 1.012*** | 0.964*** | 0.791*** | 1.003 | 0.963*** | 0.880*** | 0.747*** |

**Table 3: Regressions of Pay levels**

In Panel A, the dependent variable in columns 1 and 2 is the natural log of proxy CFO total pay and CEO total pay. The dependent variable in column 3 is the difference between ln proxy CFO total pay and CEO total pay. The dependent variable in column 4, 5 and 6 is the ratio of proxy CFO total pay to CEO total pay, proxy CFO total pay to other three executive pay and the other three executive pay to CEO total pay. D_logsale indicates the interaction between the d2006rule dummy and logsale. Other interaction variables are defined similarly. In Panel B, the dependent variable is the difference between ln proxy CFO total pay and CEO total pay. Column 1 shows results during 2002 - 2010 period to remove the SOX effect if any. Column 2 excludes year 2008 and 2009 data. Column 3 excludes financial and utility firms. Column 4 reflects the inflation adjusted amount. Column 5 shows results using CFO actual pay. All regressions in Panel B include the interaction of d2006rule and the firm level control variables. These coefficients are not reported for ease of presentation. All regressions include industry fixed effect. All continuous variables are winsorized at the 1% and 99% value. *P*-values based on firm-level clustered standard errors are in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. Variables definitions are in the Appendix.

*Panel A Regressions of Pay levels (1999-2013)*

| | Ln(Proxy CFO Pay) | Ln(CEO pay) | Ln(Proxy CFO pay) – Ln(CEO pay) | Proxy CFO pay/CEO pay | Proxy CFO pay/other three | Other three /CEO pay |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| D2006rule | 0.430*** | 0.734*** | -0.304* | -0.208 | -0.038 | -0.899 |
| | (0.003) | (0.000) | (0.054) | (0.783) | (0.778) | (0.386) |
| D2006rule_often | 0.115*** | 0.021 | 0.093*** | 0.158 | 0.154*** | 0.147 |
| | (0.000) | (0.587) | (0.003) | (0.152) | (0.000) | (0.275) |
| D2006rule_seldom | 0.188*** | 0.044 | 0.144*** | 0.203 | 0.282*** | 0.059 |
| | (0.000) | (0.476) | (0.006) | (0.134) | (0.000) | (0.757) |
| D2006rule_never | 0.168* | -0.073 | 0.241** | 0.278* | 0.201*** | -0.011 |
| | (0.055) | (0.459) | (0.012) | (0.064) | (0.001) | (0.962) |
| often | -0.113*** | -0.077* | -0.036 | -0.018 | -0.197*** | 0.098 |
| | (0.000) | (0.059) | (0.248) | (0.893) | (0.000) | (0.522) |
| seldom | -0.234*** | -0.064 | -0.170*** | -0.261* | -0.339*** | -0.100 |
| | (0.000) | (0.327) | (0.001) | (0.068) | (0.000) | (0.547) |
| never | -0.295*** | -0.081 | -0.213*** | -0.225** | -0.416*** | 0.266 |
| | (0.000) | (0.515) | (0.009) | (0.016) | (0.000) | (0.311) |
| logsale | 0.384*** | 0.424*** | -0.039*** | 0.094 | -0.012 | 0.151* |
| | (0.000) | (0.000) | (0.002) | (0.116) | (0.209) | (0.051) |
| book_leverage | -0.027 | 0.078 | -0.106 | 0.705 | 0.126 | 0.491 |
| | (0.787) | (0.553) | (0.411) | (0.504) | (0.119) | (0.649) |
| MarkettoBook | 0.118*** | 0.117*** | 0.001 | 0.163 | -0.022** | 0.231 |
| | (0.000) | (0.000) | (0.937) | (0.179) | (0.014) | (0.116) |
| stdReturn | 7.712*** | 6.140*** | 1.572 | 11.487 | -0.007 | 12.657* |
| | (0.000) | (0.000) | (0.232) | (0.103) | (0.994) | (0.069) |
| preyearreturn | 0.161*** | 0.178*** | -0.018 | 0.030 | 0.040* | 0.045 |
| | (0.000) | (0.000) | (0.440) | (0.808) | (0.097) | (0.768) |
| inst_own_pct | 0.526*** | 0.846*** | -0.320*** | -0.834* | 0.053 | -1.120** |
| | (0.000) | (0.000) | (0.000) | (0.054) | (0.434) | (0.045) |
| boardsize | 0.022*** | 0.028*** | -0.006 | -0.063* | -0.008* | -0.088** |
| | (0.000) | (0.001) | (0.382) | (0.054) | (0.089) | (0.022) |

| | | | | | | |
|---|---|---|---|---|---|---|
| idpt_pct | 0.004*** | 0.007*** | -0.003*** | 0.000 | 0.002** | -0.002 |
| | (0.000) | (0.000) | (0.002) | (0.954) | (0.042) | (0.640) |
| ceochair | -0.012 | 0.094** | -0.105*** | 0.016 | -0.052* | 0.056 |
| | (0.718) | (0.028) | (0.001) | (0.824) | (0.094) | (0.501) |
| D_logsale | -0.015 | -0.040** | 0.025* | -0.002 | -0.010 | 0.050 |
| | (0.238) | (0.030) | (0.086) | (0.965) | (0.366) | (0.504) |
| D_book_leverage | 0.109 | 0.190 | -0.081 | -0.735 | -0.045 | -0.758 |
| | (0.278) | (0.147) | (0.528) | (0.436) | (0.626) | (0.432) |
| D_MarkettoBook | -0.034* | -0.041 | 0.008 | -0.020 | 0.016 | 0.012 |
| | (0.094) | (0.100) | (0.725) | (0.891) | (0.269) | (0.951) |
| D_stdReturn | -8.196*** | -11.072*** | 2.875* | -0.274 | -0.163 | 8.245 |
| | (0.000) | (0.000) | (0.073) | (0.976) | (0.876) | (0.506) |
| D_preyearreturn | 0.032 | 0.069** | -0.037 | -0.041 | -0.003 | -0.133 |
| | (0.230) | (0.034) | (0.197) | (0.688) | (0.935) | (0.280) |
| D_inst_own_pct | -0.189** | -0.241** | 0.052 | 0.410 | 0.008 | 0.369 |
| | (0.024) | (0.019) | (0.543) | (0.254) | (0.922) | (0.455) |
| D_boardsize | 0.005 | 0.002 | 0.003 | 0.041 | 0.000 | 0.061* |
| | (0.496) | (0.819) | (0.673) | (0.103) | (0.958) | (0.067) |
| D_idpt_pct | 0.001 | 0.001 | -0.001 | -0.004 | 0.000 | -0.005 |
| | (0.614) | (0.317) | (0.409) | (0.298) | (0.944) | (0.386) |
| D_ceochair | -0.002 | -0.075 | 0.073** | 0.022 | 0.030 | 0.008 |
| | (0.950) | (0.106) | (0.043) | (0.758) | (0.401) | (0.929) |
| N | 12203 | 12203 | 12203 | 12203 | 11691 | 11686 |
| Adj R-square | 0.562 | 0.511 | 0.077 | 0.036 | 0.069 | 0.040 |

*Panel B Robustness Checks*

| | Ln(Proxy CFO pay) – Ln(CEO pay) *Sample period 2002-2010 (NO SOX EFFECT) | Ln(Proxy CFO pay) – Ln(CEO pay) *Year 2008 and 2009 excluded | Ln(Proxy CFO pay) – Ln(CEO pay) *Financial and Utility Industry excluded | Ln(Proxy CFO pay) – Ln(CEO pay) *Inflation adjusted | Ln(CFO pay) – Ln(CEO pay) *Actual CFO Pay |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| D2006rule | -0.197 | -0.301* | -0.287 | -0.395 | -0.311 |
| | (0.213) | (0.058) | (0.105) | (0.159) | (0.118) |
| D2006rule_often | 0.088*** | 0.079** | 0.093** | 0.126*** | 0.044 |
| | (0.007) | (0.015) | (0.011) | (0.006) | (0.218) |
| D2006rule_seldom | 0.100* | 0.144*** | 0.155*** | 0.152** | 0.192** |
| | (0.083) | (0.007) | (0.005) | (0.021) | (0.050) |
| D2006rule_never | 0.237** | 0.243*** | 0.310*** | 0.504 | |
| | (0.034) | (0.009) | (0.003) | (0.123) | |
| often | -0.020 | -0.034 | -0.052 | -0.009 | 0.022 |
| | (0.540) | (0.271) | (0.139) | (0.843) | (0.534) |
| seldom | -0.133** | -0.169*** | -0.205*** | -0.188*** | -0.231** |
| | (0.014) | (0.001) | (0.000) | (0.001) | (0.025) |
| never | -0.211** | -0.214*** | -0.238** | -0.200** | |
| | (0.016) | (0.008) | (0.011) | (0.034) | |
| logsale | -0.044*** | -0.038*** | -0.049*** | -0.026 | -0.053*** |
| | (0.001) | (0.002) | (0.000) | (0.251) | (0.002) |
| book_leverage | -0.160 | -0.107 | -0.096 | -0.196 | -0.116 |
| | (0.144) | (0.407) | (0.530) | (0.258) | (0.453) |
| MarkettoBook | 0.001 | -0.000 | -0.004 | -0.012 | -0.013 |
| | (0.947) | (0.997) | (0.823) | (0.605) | (0.526) |
| stdReturn | 1.504 | 1.669 | 1.369 | 2.256 | 2.634 |
| | (0.236) | (0.204) | (0.332) | (0.202) | (0.106) |
| preyearreturn | -0.017 | -0.013 | -0.005 | 0.039 | -0.021 |
| | (0.611) | (0.553) | (0.846) | (0.463) | (0.435) |
| inst_own_pct | -0.284*** | -0.321*** | -0.360*** | -0.416*** | -0.257** |
| | (0.001) | (0.000) | (0.000) | (0.000) | (0.022) |
| boardsize | 0.001 | -0.005 | -0.002 | -0.010 | -0.008 |
| | (0.911) | (0.437) | (0.768) | (0.367) | (0.353) |
| idpt_pct | -0.003** | -0.003*** | -0.002** | -0.003** | -0.001 |
| | (0.010) | (0.002) | (0.022) | (0.016) | (0.279) |
| ceochair | -0.116*** | -0.104*** | -0.079** | -0.079** | -0.092** |
| | (0.002) | (0.001) | (0.020) | (0.026) | (0.016) |
| N | 6707 | 10549 | 9836 | 12203 | 11668 |
| Adj - Rsq | 0.086 | 0.079 | 0.082 | 0.050 | 0.070 |

**Table 4 Propensity Score Matching Results**

The table presents the propensity score matching results. "Treated" firms are firms in the "seldom" or "never" reporting group. "Matched" firms are firms with the nearest propensity score in the "always" or "often" group within the same industry. Propensity score is estimated using data in the before disclosure mandate period. When identifying the match, a caliper of 0.25 standard deviation of the estimated propensity score is applied and replacement is not allowed. Panel A presents the comparison of firm characteristics used in the matching between the treated firms (with matches) and their respective matched control firms. ***, **, and * stand for the 1%, 5%, and 10% significance level for the test of the difference in mean (median) between the treated group and the control group. Panel B presents the results from regressions of pay variables. Treat is a dummy variable that equals to 1 if the firm is a "treated" firm and 0 if the firm is a matched control firm. All regressions include industry fixed effect. All continuous variables are winsorized at the 1% and 99% value. *P*-values based on firm-level clustered standard errors are in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. Variables definitions are in the Appendix.

*Panel A Firm Characteristics for the treated and control firms before the disclosure mandate*

| | Mean | | | Median | | |
|---|---|---|---|---|---|---|
| | Treated (No. firms =91) (1) | Matched (No. firms =91) (2) | *P*-Value for testing the difference | Treated (No. firms =91) (3) | Matched (No. firms =91) (4) | *P*-Value for testing the difference |
| Proxy CFO Total Pay | 1483 | 1938 | 0.145 | 1179 | 1290 | 0.415 |
| logsale | 7.777 | 7.615 | 0.388 | 7.872 | 7.567 | 0.208 |
| book_leverage | 0.211 | 0.234 | 0.310 | 0.206 | 0.212 | 0.824 |
| MarkettoBook | 0.146 | 0.162 | 0.323 | 0.115 | 0.145 | 0.415 |
| Std return | 0.027 | 0.027 | 0.524 | 0.024 | 0.023 | 0.711 |
| preyearreturn | 0.158 | 0.154 | 0.835 | 0.129 | 0.132 | 0.824 |
| inst_own_pct | 0.687 | 0.682 | 0.682 | 0.716 | 0.707 | 0.505 |
| boardsize | 9.990 | 10.153 | 0.771 | 10 | 10 | 0.533 |
| Idpt_pct | 0.652 | 0.641 | 0.511 | 0.665 | 0.635 | 0.335 |
| ceochair | 0.786 | 0.781 | 0.716 | 1.000 | 0.875 | 0.604 |
| logat | 8.079 | 8.016 | 0.722 | 7.989 | 8.105 | 0.604 |
| ROA | 0.140 | 0.136 | 0.560 | 0.137 | 0.137 | 0.941 |
| RandD | 0.028 | 0.024 | 0.650 | 0.000 | 0.000 | 0.826 |
| CAPEX | 0.049 | 0.051 | 0.533 | 0.044 | 0.042 | 0.335 |

*Panel B Regression of Pay levels between treated and control firms*

| | Ln(Proxy CFO Pay) | Ln(CEO pay) | Ln(Proxy CFO pay) – Ln(CEO pay) | Proxy CFO pay/CEO pay | Proxy CFO pay/other three |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| D2006rule | 0.062 | -0.055 | 0.117** | 0.043 | 0.058 |
| | (0.216) | (0.379) | (0.023) | (0.173) | (0.146) |
| treated | -0.243*** | -0.152** | -0.091 | -0.056 | -0.268*** |
| | (0.000) | (0.040) | (0.152) | (0.140) | (0.000) |
| D2006rule*treated | 0.170*** | 0.011 | 0.159** | 0.106** | 0.200*** |
| | (0.009) | (0.893) | (0.023) | (0.038) | (0.001) |
| logsale | 0.376*** | 0.415*** | -0.039 | 0.013 | -0.056*** |
| | (0.000) | (0.000) | (0.218) | (0.554) | (0.000) |
| book_leverage | 0.245 | 0.452** | -0.207 | -0.089 | 0.062 |
| | (0.119) | (0.026) | (0.197) | (0.396) | (0.539) |
| MarkettoBook | 0.114*** | 0.120*** | -0.006 | 0.011 | -0.028* |
| | (0.000) | (0.005) | (0.805) | (0.377) | (0.065) |
| stdReturn | 3.875** | 1.292 | 2.583* | 1.817* | 1.193 |
| | (0.018) | (0.473) | (0.071) | (0.068) | (0.350) |
| preyearreturn | 0.193*** | 0.215*** | -0.023 | 0.018 | 0.044* |
| | (0.000) | (0.000) | (0.453) | (0.520) | (0.080) |
| inst_own_pct | 0.556*** | 1.060*** | -0.504*** | -0.340*** | -0.085 |
| | (0.000) | (0.000) | (0.002) | (0.005) | (0.289) |
| boardsize | 0.034*** | 0.020 | 0.014 | -0.007 | 0.017** |
| | (0.004) | (0.219) | (0.296) | (0.373) | (0.049) |
| idpt_pct | 0.004*** | 0.008*** | -0.004** | -0.003*** | 0.003*** |
| | (0.006) | (0.000) | (0.012) | (0.000) | (0.000) |
| ceochair | -0.114*** | 0.025 | -0.139*** | -0.038 | -0.050 |
| | (0.008) | (0.628) | (0.006) | (0.161) | (0.274) |
| N | 2364 | 2364 | 2364 | 2364 | 2316 |
| Adj - Rsq | 0.610 | 0.568 | 0.159 | 0.115 | 0.137 |

**Table 5. Regression of Pay levels –Excess CFO Pay after mandate (Dec 2006 -2013)**

Excess CFO pay is defined as the difference between actual CFO pay and the predicted CFO pay. Excess CEO pay is defined similarly. Predicted CFO pay is estimated by applying the estimated confidents of regressing actual CFO pay on the determinants of CFO pay during the 1999 to Dec 2006 (the pre-mandate period). Since we use actual CFO pay, the "never" reporting group drops out of the sample. The table presents the excess pay results after mandate period (Dec 2006-2013). All regressions include industry fixed effect. All continuous variables are winsorized at the 1% and 99% value. *P*-values based on firm-level clustered standard errors are in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. Other variables definitions are in the Appendix.

| | Excess CFO Pay<br>(1) | Excess CEO Pay<br>(2) |
|---|---|---|
| Intercept | 2334.022 | 1414.119 |
| | (0.218) | (0.600) |
| Often | 139.270* | 253.329 |
| | (0.083) | (0.303) |
| Seldom | 318.939** | 225.474 |
| | (0.024) | (0.544) |
| logsale | 96.700** | 60.978 |
| | (0.025) | (0.649) |
| book_leverage | -261.919 | 693.439 |
| | (0.376) | (0.377) |
| MarkettoBook | -201.434*** | -607.870*** |
| | (0.000) | (0.000) |
| stdReturn | -24081.41*** | -87401.18*** |
| | (0.000) | (0.000) |
| preyearreturn | 85.919* | 591.754*** |
| | (0.091) | (0.001) |
| inst_own_pct | -135.192 | 8.670 |
| | (0.527) | (0.989) |
| boardsize | 8.177 | 39.004 |
| | (0.697) | (0.539) |
| idpt_pct | 1.087 | 8.146 |
| | (0.681) | (0.332) |
| ceochair | 10.669 | -204.579 |
| | (0.853) | (0.234) |
| N | 6432 | 6427 |
| Adj- Rsq | 0.120 | 0.130 |

**Table 6: Regressions of Pay levels, non-constant samples**

The dependent variable in columns 1 and 2 is the natural log of proxy CFO total pay and CEO total pay. The dependent variable in column 3 is the difference between ln proxy CFO total pay and CEO total pay. The dependent variable in column 4 and 5 is the ratio of proxy CFO total pay to CEO total pay and proxy CFO total pay to other three executive pay. All regressions include industry fixed effect. Regressions presented in Panel A include the interaction of d2006rule and the firm level control variables. These coefficients are not reported for ease of presentation. All continuous variables are winsorized at the 1% and 99% value. *P*-values based on firm-level clustered standard errors are in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. Variables definitions are in the Appendix.

*Panel A. Alternative sample 1: Require firms to exist during years 1999-2006*

| | Ln(Proxy CFO pay) | Ln(CEO pay) | Ln(Proxy CFO pay) – Ln(CEO pay) | Proxy CFO pay/CEO pay | Proxy CFO pay/other three |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| D2006rule | 0.413*** | 0.610*** | -0.197 | -0.039 | -0.074 |
| | (0.001) | (0.000) | (0.162) | (0.951) | (0.559) |
| D2006rule_often | 0.109*** | -0.009 | 0.118*** | 0.163* | 0.133*** |
| | (0.000) | (0.801) | (0.000) | (0.088) | (0.000) |
| D2006rule_seldom | 0.191*** | -0.002 | 0.193*** | 0.230** | 0.270*** |
| | (0.000) | (0.977) | (0.000) | (0.042) | (0.000) |
| D2006rule_never | 0.162** | -0.055 | 0.217** | 0.273* | 0.196*** |
| | (0.048) | (0.576) | (0.022) | (0.058) | (0.001) |
| often | -0.121*** | -0.071** | -0.050* | 0.003 | -0.176*** |
| | (0.000) | (0.047) | (0.065) | (0.981) | (0.000) |
| seldom | -0.221*** | -0.023 | -0.198*** | -0.245** | -0.331*** |
| | (0.000) | (0.703) | (0.000) | (0.022) | (0.000) |
| never | -0.293*** | -0.129 | -0.163** | -0.187** | -0.408*** |
| | (0.000) | (0.263) | (0.028) | (0.021) | (0.000) |
| logsale | 0.385*** | 0.432*** | -0.047*** | 0.077 | -0.014* |
| | (0.000) | (0.000) | (0.000) | (0.135) | (0.055) |
| book_leverage | -0.019 | 0.099 | -0.119 | 0.541 | 0.063 |
| | (0.816) | (0.357) | (0.249) | (0.509) | (0.344) |
| MarkettoBook | 0.117*** | 0.109*** | 0.007 | 0.158 | -0.026*** |
| | (0.000) | (0.000) | (0.620) | (0.116) | (0.001) |
| stdReturn | 7.712*** | 6.130*** | 1.582 | 10.215** | -0.107 |
| | (0.000) | (0.000) | (0.146) | (0.074) | (0.890) |
| preyearreturn | 0.155*** | 0.193*** | -0.038** | 0.001 | 0.041** |
| | (0.000) | (0.000) | (0.049) | (0.990) | (0.040) |
| inst_own_pct | 0.557*** | 0.709*** | -0.152** | -0.540 | 0.050 |
| | (0.000) | (0.000) | (0.048) | (0.198) | (0.362) |
| boardsize | 0.021*** | 0.020*** | 0.001 | -0.054** | -0.005 |
| | (0.000) | (0.006) | (0.865) | (0.049) | (0.284) |
| idpt_pct | 0.003*** | 0.006*** | -0.003*** | -0.002 | 0.001* |
| | (0.000) | (0.000) | (0.002) | (0.559) | (0.053) |
| ceochair | 0.003 | 0.087** | -0.084*** | 0.059 | -0.059** |
| | (0.914) | (0.017) | (0.003) | (0.385) | (0.026) |
| N | 14926 | 14926 | 14926 | 14926 | 14290 |
| Adj - Rsq | 0.557 | 0.508 | 0.073 | 0.031 | 0.065 |

*Panel B. Alternative sample 2: No restriction*

| | Ln(Proxy CFO pay) | Ln(CEO pay) | Ln(Proxy CFO pay) – Ln(CEO pay) | Proxy CFO pay/CEO pay | Proxy CFO pay/ other three executive |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| D2006rule | 0.103*** | 0.022 | 0.081*** | 0.128*** | 0.033** |
| | (0.000) | (0.249) | (0.000) | (0.002) | (0.023) |
| logsale | 0.381*** | 0.421*** | -0.039*** | 0.103* | -0.021*** |
| | (0.000) | (0.000) | (0.000) | (0.055) | (0.000) |
| book_leverage | 0.031 | 0.190*** | -0.158** | 0.107 | 0.048 |
| | (0.582) | (0.008) | (0.010) | (0.800) | (0.276) |
| MarkettoBook | 0.102*** | 0.099*** | 0.003 | 0.145** | -0.025*** |
| | (0.000) | (0.000) | (0.785) | (0.027) | (0.000) |
| stdReturn | 3.233*** | 1.076 | 2.157*** | 7.960*** | -0.482 |
| | (0.000) | (0.149) | (0.000) | (0.008) | (0.246) |
| preyearreturn | 0.163*** | 0.207*** | -0.044*** | -0.047 | 0.033*** |
| | (0.000) | (0.000) | (0.001) | (0.481) | (0.002) |
| inst_own_pct | 0.441*** | 0.653*** | -0.212*** | -0.455* | 0.076** |
| | (0.000) | (0.000) | (0.000) | (0.072) | (0.046) |
| boardsize | 0.015*** | 0.017*** | -0.002 | -0.042** | -0.011*** |
| | (0.001) | (0.003) | (0.596) | (0.028) | (0.001) |
| idpt_pct | 0.003*** | 0.006*** | -0.003*** | -0.004 | 0.002*** |
| | (0.000) | (0.000) | (0.000) | (0.169) | (0.000) |
| ceochair | 0.019 | 0.077*** | -0.058*** | 0.044 | -0.032** |
| | (0.226) | (0.000) | (0.000) | (0.230) | (0.016) |
| N | 19775 | 19775 | 19775 | 19775 | 18692 |
| Adj R-square | 0.533 | 0.486 | 0.058 | 0.021 | 0.034 |

**Table 7: Regressions of Equity Incentives**

This table includes the subset of firm-years with observable CFO pay. The dependent variable in columns 1 and 2 is the portfolio delta of CFOs and CEOs, respectively. The dependent variable in column 3 is the difference between CFO delta and CEO delta. The dependent variables in columns 4 and 5 are the CFO and CEO equity incentives transformed as ln(equity incentive/(1-equity incentive)) so that the dependent variable is linear. The dependent variable in column 6 is the difference between CFO equity incentive and CEO equity incentive. D_logsale indicates the interaction between the d2006rule dummy and logsale. Other interaction variables are defined similarly. All regressions include industry fixed effect. All continuous variables are winsorized at the 1% and 99% value. *P*-values based on firm-level clustered standard errors are in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. Variables definitions are in the Appendix.

| | CFO delta | CEO delta | CFO delta-CEO delta | CFO equity incentive | CEO equity incentive | CFO –CEO equity incentive |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| D2006rule | 85.498** | 1165.432* | -1079.934* | -0.045 | -0.529 | 0.073 |
| | (0.036) | (0.050) | (0.065) | (0.904) | (0.224) | (0.212) |
| D2006rule_often | 13.496 | 21.385 | -7.890 | 0.179*** | 0.002 | 0.003 |
| | (0.145) | (0.861) | (0.948) | (0.009) | (0.985) | (0.812) |
| D2006rule_seldom | 37.953** | -458.212 | 496.165* | 0.547** | -0.266* | 0.039** |
| | (0.020) | (0.118) | (0.094) | (0.014) | (0.072) | (0.046) |
| Often | -19.526** | -128.925 | 109.399 | -0.305*** | 0.004 | -0.013 |
| | (0.048) | (0.349) | (0.414) | (0.000) | (0.962) | (0.250) |
| Seldom | -30.784 | 200.416 | -231.200 | -0.688*** | 0.261* | -0.041* |
| | (0.116) | (0.516) | (0.451) | (0.002) | (0.094) | (0.061) |
| Logsale | 48.223*** | 494.073*** | -445.850*** | 0.187*** | 0.227*** | -0.011** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.010) |
| Book_leverage | -19.779 | -1086.049** | 1066.270** | -0.195 | -0.952*** | 0.158*** |
| | (0.499) | (0.040) | (0.041) | (0.402) | (0.002) | (0.000) |
| MarkettoBook | 36.363*** | 284.990*** | -248.626*** | 0.228*** | 0.226*** | -0.012*** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.001) |
| StdReturn | 362.988 | 5712.531 | -5349.543 | 2.819 | 5.659* | -0.252 |
| | (0.163) | (0.254) | (0.280) | (0.266) | (0.063) | (0.522) |
| Preyearreturn | 8.908 | 277.548** | -268.640** | 0.007 | -0.044 | 0.008 |
| | (0.222) | (0.012) | (0.011) | (0.868) | (0.383) | (0.163) |
| Inst_own_pct | 22.165 | -997.347*** | 1019.512*** | 0.988*** | -0.353 | 0.109*** |
| | (0.373) | (0.007) | (0.004) | (0.000) | (0.104) | (0.000) |
| Boardsize | 3.560 | -15.850 | 19.411 | 0.008 | -0.082*** | 0.011*** |
| | (0.119) | (0.647) | (0.566) | (0.578) | (0.000) | (0.000) |
| Idpt_pct | -0.210 | -18.919*** | 18.709*** | -0.002 | -0.013*** | 0.002*** |
| | (0.404) | (0.001) | (0.001) | (0.360) | (0.000) | (0.000) |
| CEOChair | 2.658 | 422.668*** | -420.010*** | -0.044 | 0.336*** | -0.057*** |
| | (0.762) | (0.000) | (0.000) | (0.609) | (0.000) | (0.000) |
| D_Logsale | -6.437 | -241.881*** | 235.444*** | -0.001 | -0.034 | 0.002 |
| | (0.151) | (0.000) | (0.000) | (0.980) | (0.367) | (0.685) |
| D_Book_leverage | -4.599 | 1025.818* | -1030.418** | 0.182 | 0.996*** | -0.149*** |
| | (0.855) | (0.053) | (0.050) | (0.466) | (0.002) | (0.000) |
| D_MarkettoBook | 2.590 | 44.461 | -41.871 | 0.173*** | 0.210*** | -0.017** |
| | (0.698) | (0.591) | (0.606) | (0.000) | (0.000) | (0.014) |
| D_StdReturn | -915.487*** | -9082.503* | 8167.016 | -9.382*** | -11.448*** | 0.260 |
| | (0.001) | (0.085) | (0.116) | (0.001) | (0.001) | (0.541) |
| D_Preyearreturn | 7.461 | -126.287 | 133.748 | 0.148*** | 0.138** | -0.005 |
| | (0.359) | (0.273) | (0.227) | (0.004) | (0.027) | (0.490) |
| D_Inst_own_pct | -9.631 | 298.487 | -308.118 | -0.689*** | -0.104 | -0.006 |
| | (0.657) | (0.328) | (0.303) | (0.001) | (0.650) | (0.827) |
| D_Boardsize | -3.529 | -7.307 | 3.778 | -0.019 | 0.013 | -0.003 |
| | (0.161) | (0.834) | (0.912) | (0.293) | (0.544) | (0.305) |
| D_Idpt_pct | -0.317 | 7.372 | -7.689 | -0.002 | -0.001 | -0.000 |
| | (0.302) | (0.207) | (0.184) | (0.392) | (0.718) | (0.610) |
| D_CEOChair | 11.720 | -102.560 | 114.280 | 0.218** | 0.067 | 0.019 |
| | (0.197) | (0.358) | (0.293) | (0.018) | (0.468) | (0.108) |
| N | 11340 | 11340 | 11340 | 11340 | 11340 | 11340 |
| ADJ R-square | 0.268 | 0.200 | 0.185 | 0.192 | 0.256 | 0.169 |

**Table 8: CFO Turnover**

CFO turnover is a dummy variable that equals one if there is CFO turnover during that firm-year. CEO turnover is a dummy variable that equals one if there is CEO turnover during that firm-year. Age is the age of the CFO (CEO) in previous year. All regressions include industry fixed effects. All continuous variables are winsorized at [1%,99%]. Numbers reported are the average marginal effects from Probit regression. *P*-values are in parenthesis and based on firm-level clustered standard errors. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. Other variables are defined as in the Appendix.

| Dependent variable | All CFO turnover | Forced CFO turnover | All CFO turnover | Forced CFO turnover | All CEO turnover | Forced CEO turnover | All CEO turnover | Forced CEO turnover |
|---|---|---|---|---|---|---|---|---|
| Performance is measured by | ROA | ROA | Stock return | Stock return | ROA | ROA | Stock return | Stock return |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Intercept | -0.738*** | -0.497*** | -0.753*** | -0.512*** | -0.859*** | -0.095*** | -0.876*** | -0.099*** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.006) | (0.000) | (0.004) |
| D2006rule | 0.100 | 0.201*** | 0.140 | 0.228*** | -0.003 | 0.001 | 0.035 | 0.011 |
| | (0.239) | (0.010) | (0.091)* | (0.003) | (0.968) | (0.975) | (0.688) | (0.811) |
| D2006rule*Performance | 0.002 | -0.022 | -0.035 | -0.019 | -0.104 | -0.115 | -0.006 | -0.009 |
| | (0.991) | (0.866) | (0.199) | (0.440) | (0.421) | (0.158) | (0.809) | (0.632) |
| D2006rule*Sometime456* | -0.153 | -0.031 | 0.015 | 0.006 | -0.068 | 0.075 | 0.023 | 0.012 |
| Performance | (0.381) | (0.844) | (0.704) | (0.875) | (0.660) | (0.449) | (0.494) | (0.651) |
| D2006rule*Sometime123* | -0.658*** | -0.505** | 0.007 | -0.000 | 0.161 | 0.243 | 0.001 | 0.014 |
| Performance | (0.010) | (0.027) | (0.922) | (0.997) | (0.424) | (0.194) | (0.987) | (0.680) |
| D2006rule*Never* | -0.772*** | -0.606** | 0.093 | 0.122 | -0.345 | -0.332 | -0.038 | 0.034 |
| Performance | (0.008) | (0.018) | (0.370) | (0.279) | (0.315) | (0.194) | (0.572) | (0.597) |
| Sometime456* Performance | 0.050 | 0.022 | 0.000 | 0.002 | 0.077 | 0.003 | -0.018 | -0.013 |
| | (0.697) | (0.855) | (0.997) | (0.922) | (0.498) | (0.966) | (0.455) | (0.470) |
| Sometime123* Performance | -0.012 | -0.025 | -0.023 | -0.013 | -0.230 | -0.325** | -0.028 | -0.033 |
| | (0.946) | (0.875) | (0.518) | (0.690) | (0.172) | (0.016) | (0.415) | (0.231) |
| Never* Performance | 0.643** | 0.463* | -0.045 | -0.078 | 0.076 | 0.097 | 0.052 | -0.031 |
| | (0.013) | (0.062) | (0.545) | (0.337) | (0.785) | (0.654) | (0.302) | (0.578) |
| D2006rule*Sometime456 | -0.054** | -0.052** | -0.074*** | -0.056*** | 0.007 | -0.007 | -0.003 | 0.001 |
| | (0.037) | (0.031) | (0.000) | (0.000) | (0.766) | (0.643) | (0.782) | (0.897) |
| D2006rule*Sometime123 | 0.011 | 0.007 | -0.077*** | -0.060*** | -0.024 | -0.024 | 0.001 | 0.002 |
| | (0.781) | (0.856) | (0.001) | (0.004) | (0.476) | (0.324) | (0.967) | (0.866) |
| D2006rule*Never | 0.038 | 0.034 | -0.089** | -0.070** | 0.072 | 0.072 | 0.032 | 0.018 |
| | (0.496) | (0.452) | (0.012) | (0.033) | (0.154) | (0.122) | (0.279) | (0.284) |
| sometime456 | 0.080*** | 0.069*** | 0.086*** | 0.072*** | 0.016 | 0.014 | 0.027*** | 0.015** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.364) | (0.193) | (0.004) | (0.013) |

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| sometime123 | 0.098*** | 0.084*** | 0.098*** | 0.081*** | 0.052* | 0.043** | 0.019 | 0.005 |
| | (0.001) | (0.001) | (0.000) | (0.000) | (0.058) | (0.013) | (0.208) | (0.664) |
| never | -0.042 | -0.029 | 0.061** | 0.047* | -0.013 | -0.030 | -0.015 | -0.013 |
| | (0.376) | (0.516) | (0.032) | (0.070) | (0.772) | (0.470) | (0.553) | (0.381) |
| ROA | -0.111 | -0.113 | | | -0.113 | -0.076 | | |
| | (0.329) | (0.280) | | | (0.264) | (0.219) | | |
| age | 0.006*** | 0.001 | 0.006*** | 0.001 | 0.008*** | -0.002*** | 0.008*** | -0.002*** |
| | (0.000) | (0.455) | (0.000) | (0.395) | (0.000) | (0.000) | (0.000) | (0.000) |
| logsale | 0.004 | 0.005 | 0.004 | 0.005 | 0.004 | 0.002 | 0.004 | 0.001 |
| | (0.355) | (0.229) | (0.324) | (0.218) | (0.282) | (0.458) | (0.299) | (0.689) |
| book_leverage | 0.047 | 0.027 | 0.043 | 0.024 | 0.028 | 0.002 | 0.026 | 0.004 |
| | (0.141) | (0.368) | (0.180) | (0.421) | (0.314) | (0.912) | (0.340) | (0.840) |
| MarkettoBook | 0.006 | 0.005 | 0.003 | 0.002 | 0.004 | 0.002 | 0.001 | -0.001 |
| | (0.118) | (0.138) | (0.442) | (0.571) | (0.184) | (0.356) | (0.718) | (0.789) |
| stdReturn | 1.086*** | 0.884** | 1.337*** | 1.148*** | 1.213*** | 0.349 | 1.459*** | 0.561** |
| | (0.010) | (0.025) | (0.001) | (0.002) | (0.002) | (0.190) | (0.000) | (0.036) |
| preyearreturn | -0.034*** | -0.032*** | -0.016 | -0.019 | -0.039*** | -0.026*** | -0.029* | -0.014 |
| | (0.000) | (0.000) | (0.440) | (0.310) | (0.000) | (0.000) | (0.086) | (0.221) |
| inst_own_pct | 0.062** | 0.075*** | 0.058** | 0.069*** | 0.031 | 0.017 | 0.030 | 0.015 |
| | (0.019) | (0.002) | (0.031) | (0.005) | (0.212) | (0.314) | (0.224) | (0.369) |
| boardsize | 0.002 | 0.002 | 0.002 | 0.002 | 0.004* | 0.001 | 0.004** | 0.001 |
| | (0.348) | (0.289) | (0.409) | (0.337) | (0.055) | (0.466) | (0.038) | (0.439) |
| idpt_pct | 0.000 | 0.001** | 0.000 | 0.001** | 0.000 | 0.000 | 0.000 | 0.000 |
| | (0.151) | (0.028) | (0.158) | (0.025) | (0.182) | (0.536) | (0.149) | (0.463) |
| ceochair | -0.008 | -0.012 | -0.010 | -0.014 | -0.009 | -0.009 | -0.011 | -0.009 |
| | (0.543) | (0.345) | (0.447) | (0.272) | (0.468) | (0.261) | (0.394) | (0.246) |
| N | 0.028 | 11169 | 11254 | 11254 | 11403 | 11403 | 11488 | 11488 |
| Pseudo R-square | 11169 | 0.019 | 0.026 | 0.017 | 0.050 | 0.023 | 0.048 | 0.020 |

**Table 9: Negative Earnings Surprises**

The dependent variable in columns 1 is a dummy variable that equals one if the actual EPS is lower than the most recent consensus forecast for that fiscal year and zero otherwise. The dependent variable in columns 2 is a dummy variable that equals one if SUE is less than or equal to -1 and zero otherwise. The dependent variable in columns 3 is a dummy variable that equals one if SUE is less than or equal to -2 and zero otherwise. The dependent variable in columns 4 is a dummy variable that equals one if the difference between earnings and forecast of a firm is in the bottom quintile in the overall sample. The dependent variable in columns 5 is a dummy variable that equals one if the difference between earnings and forecast is in the bottom quintile in the year in the sample. The number reported are average marginal effects for the probit model. All regressions include industry fixed effect. All continuous variables are winsorized at the 1% and 99% value. *P*-values based on firm-level clustered standard errors are in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. Other variables definitions are in the Appendix.

| Dependent variable: | D(Earnings < Forecast) | DSUE1 | DSUE2 | Large Neg Surp_All | Large Neg Surp_Year |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| d2006rule | 0.023 | -0.003 | 0.009 | 0.038*** | -0.021 |
| | (0.153) | (0.797) | (0.398) | (0.005) | (0.111) |
| d2006rule_often | 0.009 | 0.014 | -0.003 | 0.010 | 0.015 |
| | (0.646) | (0.391) | (0.800) | (0.554) | (0.356) |
| d2006rule_seldom | 0.084** | 0.042 | 0.009 | 0.056** | 0.052* |
| | (0.012) | (0.156) | (0.680) | (0.045) | (0.061) |
| d2006rule_never | 0.099*** | 0.092** | 0.064* | 0.098** | 0.106*** |
| | (0.009) | (0.027) | (0.089) | (0.022) | (0.008) |
| Dsox | -0.023 | -0.010 | -0.011 | -0.016 | -0.012 |
| | (0.100) | (0.437) | (0.259) | (0.171) | (0.281) |
| Often | -0.014 | -0.017 | 0.000 | -0.002 | -0.010 |
| | (0.352) | (0.214) | (0.964) | (0.873) | (0.474) |
| Seldom | -0.068*** | -0.044* | -0.006 | -0.046* | -0.041* |
| | (0.006) | (0.076) | (0.757) | (0.066) | (0.080) |
| Never | -0.077** | -0.062* | -0.030 | -0.087** | -0.105*** |
| | (0.024) | (0.068) | (0.313) | (0.015) | (0.002) |
| Size | -0.001 | -0.012* | 0.001 | 0.022*** | 0.027*** |
| | (0.927) | (0.066) | (0.808) | (0.002) | (0.000) |
| Salesgrowth | -0.067*** | -0.088*** | -0.088*** | -0.073*** | -0.077*** |
| | (0.007) | (0.000) | (0.000) | (0.008) | (0.005) |
| Shares | -0.006 | 0.001 | -0.012** | -0.025*** | -0.036*** |
| | (0.519) | (0.873) | (0.040) | (0.003) | (0.000) |
| NOA | 0.011 | 0.015** | 0.016*** | 0.017** | 0.018*** |
| | (0.138) | (0.018) | (0.003) | (0.014) | (0.007) |
| Litigation | -0.056** | -0.018 | -0.026* | -0.041* | -0.019 |
| | (0.013) | (0.370) | (0.074) | (0.056) | (0.342) |
| Implicit claims | -0.056*** | -0.021 | 0.010 | -0.043** | -0.034* |
| | (0.006) | (0.267) | (0.479) | (0.024) | (0.066) |
| Analyst Following | -0.006*** | -0.005*** | -0.004*** | -0.007*** | -0.006*** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| Forcast Dispersion | -0.008 | -0.090** | -0.058* | -0.003 | -0.025 |
| | (0.873) | (0.031) | (0.072) | (0.955) | (0.596) |
| Book_Leverage | 0.060 | 0.016 | 0.012 | 0.058 | 0.048 |
| | (0.126) | (0.618) | (0.643) | (0.101) | (0.196) |
| Inst_Own_Pct | 0.003 | 0.033 | 0.023 | 0.005 | -0.003 |
| | (0.919) | (0.258) | (0.302) | (0.865) | (0.913) |
| Boardsize | -0.002 | 0.000 | -0.000 | 0.001 | 0.001 |
| | (0.435) | (0.852) | (0.833) | (0.559) | (0.709) |
| Idpt_Pct | -0.001 | -0.000 | 0.000 | -0.000 | 0.000 |
| | (0.136) | (0.929) | (0.793) | (0.998) | (0.466) |
| CEOChair | -0.009 | -0.011 | -0.007 | -0.010 | -0.007 |
| | (0.406) | (0.208) | (0.348) | (0.248) | (0.422) |
| N | 10,741 | 9,909 | 9,909 | 10,741 | 10,741 |
| Pseudo-Rsq | 0.039 | 0.032 | 0.031 | 0.058 | 0.051 |

**Table 10: Accruals Management**

Positive_total_accrual equal total accrual if total accrual is positive, and zero otherwise. Negative_total_accrual equal total accrual if total accrual is negative, and zero otherwise. Positive_discretionary equals discretionary accrual if discretionary accrual is positive, and zero otherwise. Negative_discretionary equals discretionary accrual if discretionary accrual is negative, and zero otherwise. Dependent variables are multiplied by 100 for ease of presentation. Variables definitions are as defined in Appendix. Regressions include industry fixed effect. All continuous variables are winsorized at the 1% and 99% value. *P*-values based on firm-level clustered standard errors are in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

| | Abs_ total_accrual (1) | Positive_ total_accrual (2) | Negative_ total_accrual (3) | Abs_ discretionary (4) | Positive_ discretionary (5) | Negative_ discretionary (6 |
|---|---|---|---|---|---|---|
| D2006rule | 0.225 | -0.046 | -0.292 | 0.047 | 0.096 | 0.092 |
| | (0.252) | (0.454) | (0.153) | (0.839) | (0.588) | (0.511) |
| D2006rule_often | -0.078 | 0.096 | 0.163 | 0.492* | 0.580*** | 0.078 |
| | (0.772) | (0.187) | (0.557) | (0.090) | (0.007) | (0.651) |
| D2006rule_seldom | 0.290 | 0.225** | -0.020 | 0.719** | 0.520* | -0.220 |
| | (0.450) | (0.020) | (0.959) | (0.043) | (0.071) | (0.454) |
| D2006rule_never | -0.141 | -0.027 | 0.118 | -0.473 | -0.473 | -0.047 |
| | (0.762) | (0.862) | (0.812) | (0.404) | (0.243) | (0.869) |
| Dsox | -0.543*** | -0.200*** | 0.360* | -0.692*** | -0.997*** | -0.286** |
| | (0.003) | (0.001) | (0.052) | (0.000) | (0.000) | (0.046) |
| Often | 0.360 | -0.086 | -0.422 | -0.290 | -0.366** | -0.108 |
| | (0.154) | (0.197) | (0.109) | (0.195) | (0.031) | (0.451) |
| Seldom | -0.214 | -0.139* | 0.038 | -0.670** | -0.336 | 0.286 |
| | (0.499) | (0.056) | (0.909) | (0.016) | (0.167) | (0.101) |
| Never | 0.527 | 0.105 | -0.447 | 0.072 | 0.209 | 0.142 |
| | (0.377) | (0.474) | (0.471) | (0.868) | (0.573) | (0.497) |
| Stdcashflow | 30.335*** | 7.885*** | -20.730*** | 30.066*** | 11.681*** | -16.449*** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| Stdrev | 1.386 | 0.246 | -1.076 | 3.533*** | 0.436 | -2.664*** |
| | (0.138) | (0.395) | (0.248) | (0.001) | (0.533) | (0.000) |
| Oldfirm | -1.181*** | 0.016 | 1.196*** | -0.128 | 0.276** | 0.354*** |
| | (0.000) | (0.731) | (0.000) | (0.436) | (0.033) | (0.002) |
| Stdsalegrowth | 3.764*** | 0.197 | -3.454*** | 2.555*** | 0.370 | -2.076*** |
| | (0.000) | (0.253) | (0.000) | (0.000) | (0.292) | (0.000) |
| Size | -0.130 | 0.002 | 0.138 | -0.024 | -0.083* | -0.042 |
| | (0.120) | (0.897) | (0.111) | (0.701) | (0.087) | (0.352) |
| Book_Leverage | 0.261 | 0.016 | -0.188 | -0.745 | -0.291 | 0.418 |
| | (0.673) | (0.925) | (0.768) | (0.160) | (0.496) | (0.245) |
| Inst_Own_Pct | 0.280 | 0.091 | -0.209 | 0.146 | -0.373 | -0.573* |
| | (0.567) | (0.501) | (0.685) | (0.740) | (0.269) | (0.056) |
| Boardsize | -0.042 | -0.029*** | 0.008 | -0.098*** | -0.056* | 0.036 |
| | (0.317) | (0.006) | (0.854) | (0.009) | (0.052) | (0.163) |
| Idpt_Pct | 0.004 | -0.000 | -0.003 | 0.005 | 0.004 | 0.000 |
| | (0.494) | (0.895) | (0.604) | (0.291) | (0.327) | (0.943) |
| CEOChair | -0.477*** | -0.085* | 0.376** | -0.129 | 0.145 | 0.262** |
| | (0.003) | (0.058) | (0.024) | (0.419) | (0.236) | (0.011) |
| N | 10,614 | 10,614 | 10,614 | 10,319 | 10,319 | 10,319 |
| Adj R-square | 0.167 | 0.087 | 0.141 | 0.186 | 0.178 | 0.063 |

**Table 11: Meeting or Just Beating Analyst Forecasts**

The number reported are average marginal effects for the probit model. All regressions include industry fixed effect. All continuous variables are winsorized at the 1% and 99% value. *P*-values based on firm-level clustered standard errors are in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. Variables definitions are as defined in Appendix.

| Dependent variable: | Meet | JustBeat |
|---|---|---|
| | (1) | (2) |
| D2006rule | -0.042*** | -0.029*** |
| | (0.001) | (0.001) |
| D2006rule_often | -0.004 | 0.004 |
| | (0.815) | (0.692) |
| D2006rule_seldom | -0.001 | 0.025* |
| | (0.980) | (0.073) |
| D2006rule_never | 0.015 | -0.005 |
| | (0.627) | (0.833) |
| Dsox | -0.032*** | -0.026*** |
| | (0.001) | (0.000) |
| Often | 0.014 | 0.003 |
| | (0.269) | (0.756) |
| Seldom | 0.012 | 0.008 |
| | (0.524) | (0.482) |
| Never | 0.055** | 0.041*** |
| | (0.036) | (0.009) |
| Size | -0.032*** | 0.007* |
| | (0.000) | (0.091) |
| Salesgrowth | 0.025* | 0.006 |
| | (0.078) | (0.407) |
| Shares | 0.036*** | -0.015*** |
| | (0.000) | (0.002) |
| NOA | 0.005 | -0.001 |
| | (0.433) | (0.776) |
| Litigation | -0.025 | 0.013 |
| | (0.162) | (0.215) |
| Implicit Claims | 0.034** | 0.027** |
| | (0.036) | (0.018) |
| Analyst Following | 0.004*** | 0.002*** |
| | (0.000) | (0.000) |
| Forecast Dispersion | -0.063** | -0.043*** |
| | (0.027) | (0.003) |
| Book_Leverage | 0.002 | -0.016 |
| | (0.953) | (0.414) |
| Inst_Own_Pct | -0.007 | -0.010 |
| | (0.795) | (0.568) |
| Boardsize | -0.002 | -0.003** |
| | (0.454) | (0.036) |
| Idpt_Pct | -0.000 | -0.000 |
| | (0.120) | (0.537) |
| CEOChair | 0.000 | 0.000 |
| | (0.970) | (0.970) |
| N | 10,739 | 10,739 |
| Pseudo R-Square | 0.047 | 0.030 |

**Table 12: Accruals quality (Opacity)**

Variables definitions are as defined in Appendix. Regressions include industry fixed effect. All continuous variables are winsorized at the 1% and 99% value. *P*-values based on firm-level clustered standard errors are in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

| Dependent variable: | Opacity | Opac3 |
|---|---|---|
| | (1) | (2) |
| D2006rule | -0.004*** | -0.002 |
| | (0.004) | (0.133) |
| D2006rule_often | -0.002 | -0.002 |
| | (0.210) | (0.263) |
| D2006rule_seldom | 0.007** | 0.005* |
| | (0.035) | (0.079) |
| D2006rule_never | 0.001 | 0.001 |
| | (0.812) | (0.681) |
| Dsox | 0.002* | -0.001 |
| | (0.091) | (0.363) |
| Often | 0.003 | 0.003 |
| | (0.110) | (0.142) |
| Seldom | -0.004 | -0.003 |
| | (0.127) | (0.268) |
| Never | -0.005* | -0.006** |
| | (0.099) | (0.043) |
| Logsale | -0.001** | -0.001** |
| | (0.026) | (0.019) |
| Book_Leverage | -0.001 | -0.002 |
| | (0.781) | (0.615) |
| Markettobook | 0.000 | -0.000 |
| | (0.792) | (0.611) |
| Stdreturn | 0.543*** | 0.544*** |
| | (0.000) | (0.000) |
| Preyearreturn | 0.002** | 0.001 |
| | (0.017) | (0.530) |
| Inst_Own_Pct | -0.003 | -0.001 |
| | (0.336) | (0.803) |
| Boardsize | -0.001** | -0.000 |
| | (0.011) | (0.131) |
| Idpt_Pct | -0.000 | -0.000 |
| | (0.106) | (0.183) |
| CEOChair | 0.000 | 0.000 |
| | (0.913) | (0.740) |
| N | 9,647 | 9,791 |
| Adj R-square | 0.210 | 0.147 |

## Appendix: Definition of Variables

| Variable Name | Definition |
|---|---|
| **Firm Characteristic Variables** | |
| Book leverage | =(DLTT + DLC)/AT |
| CAPEX | Net capital expenditure to assets=(capital expenditure – sale of PPE)/ASSETS =(CAPX-SPPE)/AT |
| Cash | Cash and short-term investment (CHE) over book value of total assets (AT). |
| Market leverage | = (DLTT + DLC)/(AT – CEQ + PRCC_F* CSHO) |
| Market to book | = (AT – CEQ +PRCC_F* CSHO)/ AT |
| R&D | = Research and development expenditure to assets= max(0, XRD)/AT |
| ROA | = OIBDP /AT |
| StdReturn | Stock return volatility, Calculated as the standard deviation of daily stock return over the fiscal year |
| StdCashFlow | The standard deviation of cash flows from operations(OANCF) deflated by total assets over the current and previous four years; |
| StdSalesGrowth | StdSalesGrowth is the standard deviation of sales growth over the current and previous four years; |
| StdRev | StdRev is the standard deviation of sales divided by total assets over the current and previous four years |
| **Compensation, Governance and Related Variables** | |
| D2006rule | D2006rule is a dummy variable that equals one for firm-years with fiscal year ends on or after December 15, 2006 (the disclosure mandate effective date) and zero otherwise; |
| Always, Seldom, Often, and Never | We classify firms into four groups according to their CFO pay reporting frequency during the pre-disclosure-mandate period: “never” (firms never reporting CFO pay), “seldom” (reporting 1-3 times), “often” (reporting 4-6 times), and “always” (always reporting) ; never is a dummy variable that equals one if a firm belongs to the “never” group and zero otherwise. Seldom, Often, and Always dummies are defined similarly. |
| D2006rule_seldom | The interaction of D2006rule dummy and seldom dummy; D2006rule_often and D2006rule_never are defined similarly; |

| | |
|---|---|
| Total Pay | TDC1 in ExecuComp database; |
| Proxy CFO pay | TDC1 for the CFO. It equals the lowest pay among the top five executives when CFO pay is unreported. |
| Other three executive pay | The mean of the top three mostly paid executives other than CFO and CEO in a firm. |
| Equity/total | Equity/total is the executives' equity pay scaled by total pay. Equity is defined as the sum of option grant value and stock grant value. Option valued is option_awards_blk_value or option_awards_fv for its appropriate period in ExecuComp. Stock grant value is Rstkgrnt or stock_awards_fv for its appropriate period in Execucomp. |
| Delta | CFO/CEO's dollar change in wealth for a 1% increase in the firm's stock price following Core and Guay (2002). |
| Equity Incentive | Equity Incentive Ratio per Jiang et al (2010). It equals Delta/(Delta+CashPay). Cash pay is the sum of salary and bonus. |
| Inst_own_pct | inst_own_pct is the percentage of shares owned by institutions from Thomson Reuters Database; |
| Boardsize | Boadsize is the number of board directors from ISS database. |
| Idpt_pct | Idpt_pct is the percentage of independent board members from ISS database. |
| CEOChair | CEO Chair is a dummy variable that equals one if CEO is also the chairman and zero otherwise. |
| DSOX | DSOX is a dummy variable that equals one if a firm's fiscal year is on or after 2002, and 0 otherwise. |

**Outcome Related Variables**

| | |
|---|---|
| Abs_total_accrual | Abs_total_accrual is the absolute value of total accruals. Total accruals are the difference between earnings before extraordinary items and cash flows from operations, scaled by the previous year's total assets. The definition follows Jiang et al. (2010). |
| Abs_discretionary | Abs_discretionary is the absolute value of discretionary accruals. Discretionary accruals are the difference between total accruals and estimated nondiscretionary accruals. The estimated nondiscretionary accruals is the fitted value of the regression of total accruals on the annual changes in sales and accounts receivable, gross property, plant and equipment, lagged total accruals, and sales growth. The definition follows Jiang et al. (2010). |
| Meet | The dummy variable "meet" equals one if the actual EPS is exactly the same as forecast or just one cent above the consensus (median) forecast and zero otherwise. |

| | |
|---|---|
| Justbeat | The dummy variable is equal to one if the EPS is exactly one cent above consensus forecast and zero otherwise. |
| Opacity | For each Fama-French 49 industry with at least 20 firms in a given year, we run five separate regressions for each of year *t-4* to year *t*. In each regression, total current accruals of a firm is regressed on 1) lagged, contemporaneous, and leading cash flows from operations; 2) change in sales; and 3) property, plant, and equipment. Total current accruals equals change in current assets minus change in current liabilities minus change in cash and short-term investments plus change in current debt. For each firm-year, opacity is the standard deviation computed across the residuals of total current accruals from the five industry-year regressions. The definition follows Billett and Yu (2015). |
| Opac3 | Opac3 is measured similarly as Opacity except it is based on the 3-year (t-2 to t) standard deviation of regression residuals instead of 5 to minimize loss of observations. The definition follows Billett and Yu (2015). |
| Litigation | Litigation equals one if the firm is in the following industries: pharmaceutical/biotechnology(SICcodes2833–2826,8731–8734), computer(3570–3577,7370–7374), electronics (3600–3674), or retail(5200–5961), and zero otherwise. |
| ImplicitClaims | ImplicitClaims equals one minus the ratio of gross PPE to total assets(1-PPEGT/AT) measured at the end of year t. |
| ForcastDispersion | ForecastDispersion is the standard deviation of analyst forecast dividend by the mean of analyst forecast. |
| AnalystFollowing | Analystfollowing is the number of analyst included in I/B/E/S during that statistical period. |
| NOA | NOA is the net operating assets scaled by sales at the end of last year. |
| Turnover | Dummy variable that equals one if there is a CFO(CEO) turnover during that firm-year. |
| Age | The age of CFO in the previous year. |
| Oldfirm | Dummy variable equals one if a firm is listed on Compustat for More than 20 years, and zero otherwise. |
| Size | The natural logarithm of logged total assets. |
| Shares | The natural logarithm of common shares outstanding measured at the end of year t. |