Two Essays in Finance: The Consequences of Mandated Compensation Disclosure, and the Idiosyncratic Volatility Puzzle

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ABSTRACT
(Academic)

This Dissertation consists of two essays. The first essay studies the causal impacts of compensation disclosure on executive compensation, turnover, and executives' job responsibilities. We find that, after the SEC mandates the disclosure of Chief Financial Officers (CFOs)' compensation in 2006, CFO pay increases significantly relative to CEO pay, particularly in firms most affected by the mandate. CFOs are 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 that the CFOs hide bad news and lower corporate reporting quality after the mandate, suggesting that CFOs engage in more short-term behavior to boost their performance and avoid termination.

The second essay of my dissertation focuses on the idiosyncratic volatility puzzle - the negative relation between estimated idiosyncratic volatility and the subsequent month returns documented by Ang et al (2006). We document a systematic pattern of temporary increases in the estimated idiosyncratic volatility for the quintile of stocks with the highest estimated idiosyncratic volatility in a given month. A large portion of this temporary increase in the estimated idiosyncratic volatility is reversed in the subsequent month. This temporary increase in the idiosyncratic volatility for the quintile of stocks with the highest estimated idiosyncratic volatility is associated

with relatively large positive returns (positive abnormal returns) in the estimation month and relatively low returns (negative abnormal returns) in the subsequent month. Our evidence shows that these temporary increases in the estimated idiosyncratic volatility and the related positive and negative abnormal returns in the estimation and subsequent months, respectively, create a negative relation between the estimated idiosyncratic volatility and subsequent month returns documented in the prior literature (Ang et al. 2006). We find no significant relation between idiosyncratic volatility and subsequent returns for eighty percent of the stocks that do not exhibit large changes in idiosyncratic volatility despite large differences in the levels of their idiosyncratic volatility. Finally, there is no relation between the estimated idiosyncratic volatility and subsequent returns after a lag of 3 months when the abnormal returns associated with temporary changes are no longer present. Overall, our results are consistent with the notion that there is no relation between the true underlying idiosyncratic volatility and expected returns, and that the previously documented negative relation between estimated idiosyncratic volatility and subsequent month's returns is being driven by temporary one-month increases in the estimated idiosyncratic volatility and the associated abnormal returns for a subset of stocks.

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ABSTRACT
(Public)

The disclosure of executive compensation is an important issue because it affects the investors' ability to monitor the firms' compensation practices. Properly designed compensation contracts, in turn, incentivize the executives to make decisions that serve the investors' interests. The SEC has made continuous regulatory efforts to monitor the executive compensation and has adopted several disclosure rules. However, the impacts of such enhanced compensation disclosure has not been well understood. My first essay studies the impacts of compensation disclosure on executive compensation, turnover, and executives' job responsibilities. We find that, after the SEC mandates the disclosure of Chief Financial Officers (CFOs)' compensation in 2006, CFO pay increases significantly relative to CEO pay, particularly in firms most affected by the mandate. CFOs are more likely to leave their firms following poor performance. There is also some evidence that the CFOs hide bad news and lower corporate reporting quality after the mandate, suggesting that CFOs engage in more short-term behavior to boost their performance and avoid termination.

Traditional asset pricing models in which investors hold well-diversified portfolios imply that there should be no relation between firm specific risk (the idiosyncratic volatility) and the expected returns. However, Ang et al (2006) document that stocks with high firm specific risks earn low subsequent returns. The significant negative relation between firm specific risk and subsequent returns has puzzled many researchers. The second essay of my dissertation provides a possible resolution to this puzzle.

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DEDICATION

To my father, who left this world too soon.

Dad, I miss you so much.

Table of Contents

List of Figures

List of Tables

ESSAY I

The Consequences of Mandated Compensation Disclosure

1.1 Introduction

Since the inception of the Securities and Exchange Commission (SEC) in 1934 which mandated the disclosure of executive compensation, there has been continuous regulatory efforts to monitor the executive compensation practices of major corporations (Frydman and Saks, 2010). 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). In 2006, the SEC enacted new compensation disclosure rules including the new reporting format detailing equity grants and the mandated disclosures of compensation peer groups and the Chief Financial Officer's (CFO) compensation. The Dodd-Frank Act that followed established the say-on-pay voting requirement which was also intended to facilitate investor monitoring of the managerial compensation processes. Despite the heightened attention given to compensation disclosures by regulators, the effects of compensation disclosure on executive pay practices have not been well understood. Our paper intends to study this question.

An empirical examination of the question is difficult due to endogeneity concerns. In particular, the quality of corporate governance can affect both compensation disclosure policies and executive pay practices. Regulatory changes in compensation disclosure requirements can provide quasi-natural experiments for such a study. For example, Faulkender and Yang (2013) examine the 2006 mandated reporting of compensation peer groups and find greater uses of high-pay benchmarks following the enhanced disclosure. However, one potential concern remains that some confounding events occur at the same time as the regulatory change that lead to the observed compensation practice changes. While this may not be a big concern in a specific context like peer benchmarking, it is particularly problematic when general compensation practices are in question. In this paper, we surmount this challenge by studying a unique empirical setting that enables a triple-differences test strategy.

We examine the mandated disclosure rule regarding CFO compensation in 2006. Before the 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, however, CFO compensation must be disclosed along with CEO compensation regardless of their pay rankings. Thus, the CFO experiences enhanced compensation disclosure. For identification of the effects of compensation disclosure on CFO pay, the first difference we exploit is the change in CFO pay before and after the disclosure mandate. The second difference is the differential impacts of the disclosure mandate on CFO pay across firms with different pre-mandate reporting status. In our sample of the S&P 1,500 firms, only 40% firms always disclosed their CFOs' pay in the seven years before the mandate; that is, 60% of the firms were affected by the mandate and had to comply with the new disclosure rule after 2006. We hypothesize that firms that never or seldom reported CFO pay 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. By examining the first two differences, augmented by industry and reporting group fixed effects, we control for general time trends in CFO pay.

We additionally explore the third difference: that between the CFO and the CEO. The new disclosure mandate is specific to the CFO, not other executives. Meanwhile, the reporting of the CEO's pay has always been required since 1934. We compare the differential post-mandate changes in CEO pay across reporting groups with those in CFO pay. The third difference controls for confounding factors that may correlate with the pre-2006 CFO pay reporting status but that influence general pay practices at the firm without differential implications on the CFO.

Theoretically, the compensation disclosure mandate facilitates 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). Improved investor monitoring can have opposing effects on the level of CFO 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).

We examine the S&P 1,500 firms (past and present) in a balanced panel of 1,003 firms between 1999 and 2013.[1] During the seven years before 2006, about 15% of these firms never or seldom reported CFO pay, 45% of the firms often reported CFO pay, and 40% of the firms always reported CFO pay. Comparisons across these reporting groups show that firms that never or seldom reported CFO pay prior to 2006 are larger and have better financial standing and lower risk than other firms. Across industries, financial firms are the least likely to report CFO pay before 2006. We thus include reporting group fixed effects and industry fixed effects in the regression analyses.

In the main analysis, we compare CFO total pay before and after the disclosure mandate across the pre-mandate reporting groups, and contrast CFO pay with CEO pay. We find results consistent with Hermalin and Weisbach (2012) and Faulkender and Yang (2013). CFO total pay increases after 2006. Consistent with our conjecture, the CFO pay increases are the most salient in

[1] We focus on the constant sample in the majority of our analyses to eliminate possible sample compositional effects. However, we confirm that our results are not driven by the use of a constant sample. As we show in Section 1.3.3, 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.

firms that never or seldom reported CFO pay previously, while they do not exist in firms that always reported CFO pay.[2] The average increase in CFO pay in 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 from the triple difference analysis support the interpretation that the compensation disclosure mandate leads to higher CFO pay.[3]

We also examine changes in the equity incentives of CFO pay around the disclosure mandate. 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. 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

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[2] 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 1.3.3.5 and find qualitatively similar results.

[3] 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.

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 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.[4] 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 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).[5] 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 1.2 introduces the sample and compares firm characteristics across pre-2006 CFO pay reporting groups. Section 1.3 examines

[4] 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.

[5] 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.

the effect of the mandatory compensation disclosure rule on CFO pay and turnovers. Section 1.4 investigates the effects of the disclosure mandate on firms' financial reporting practices. Section 1.5 concludes.

1.2 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 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.[6] 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.

[6] 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.

In Table 1.1, 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, 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.

1.3 CFO pay

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.

1.3.1 Univariate analysis

Table 1.2 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 1.1 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.

1.3.2 Regression analysis

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

$$y_{it} = \alpha + \beta_1 d2006rule + \beta_2 d2006rule * often + \beta_3 d2006rule * seldom + \beta_4 d2006rule * never + \gamma_1 * Controls_{it} + \gamma_2 d2006rule * Controls_{it} + \varepsilon_{it} \quad (1.1)$$

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, β_2, β_3, or β_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 β_2, β_3, and β_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 1.3, 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 1.3. 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.

1.3.3 Robustness checks

1.3.3.1 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 1.3, 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 1.3, Panel A (the base case). The results are highly consistent with those in the base case.

1.3.3.2 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 1.3, Panel B reports the regression result. The results are almost identical to those in the base case.

1.3.3.3 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 1.3, Panel B shows, the results are again in line with those in the base case.

1.3.3.4 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 1.3, 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.

1.3.3.5 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 1.3, 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 1.3, Panel A). This is not surprising given that the proxy CFO pay overestimates the pre-mandate CFO pay.

1.3.3.6 Propensity score matching

In our main regression specifications in Table 1.3, 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 1.3) and additional variables that differ across reporting groups (see Table 1.1). 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 1.3 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 1.4. 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 1.3. These results are consistent with those in Table 1.3. 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.

1.3.3.7 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 1.5. 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.

1.3.3.8 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 1.3 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 1.6. Panel A corresponds to the results using the first alternative sample and follow the regression specifications in Table 1.3. Consistent with those of Table 1.3, 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 1.3.

Overall, the evidence in the first alternative sample suggests that our main results reported in Table 1.3 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 1.3 using the constant sample.

1.3.4 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 1.3.3.5 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 1.7, 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 1.7. 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.

1.3.5 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:

$$\begin{aligned} Turnover_{it} = {} & \alpha + \beta_1\, d2006rule * R + \beta_2\, d2006rule * often * R + \beta_3\, d2006rule * seldom \\ & * R + \beta_4\, d2006rule * never * R + \emptyset_1\, R + \emptyset_2\, often * R + \emptyset_3\, seldom * R \\ & + \emptyset_4\, never * R + \theta_1 d2006rule + \theta_2 d2006rule * often + \theta_3 d2006rule \\ & * seldom + \theta_4 d2006rule * never + \gamma_1 * OtherControls_{it} + \varepsilon_{it} \end{aligned} \quad (1.2)$$

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, β_1–β_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, β_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, β_4 should be significantly negative.

We report the turnover probit regression results in Table 1.8. 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 β_3 and β_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.

1.4 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.

1.4.1 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 1.9, 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;[7] 2) the SUE is less than or equal to -2; 3) the earnings surprise is in the bottom quintile 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.[8]

1.4.2 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

[7] 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.
[8] 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.

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 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:

$$Total\ Accruals_{it} = \alpha + \beta_1\big((1+k)\Delta Sales_{it} - \Delta REC_{it}\big) + \beta_2 PPE_{it} + \beta_3 Total\ Accruals_{it-1} + \beta_4 SalesGrowth_{it+1} + \varepsilon_{it} \quad (1.3)$$

where k is the coefficient obtained by regressing changes in accounts receivable (ΔREC_{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 1.10.

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 1.11.

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 1.12. 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.

1.5 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 disclosure 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 1.1 The ratio of proxy CFO total pay to CEO total pay across groups overtime

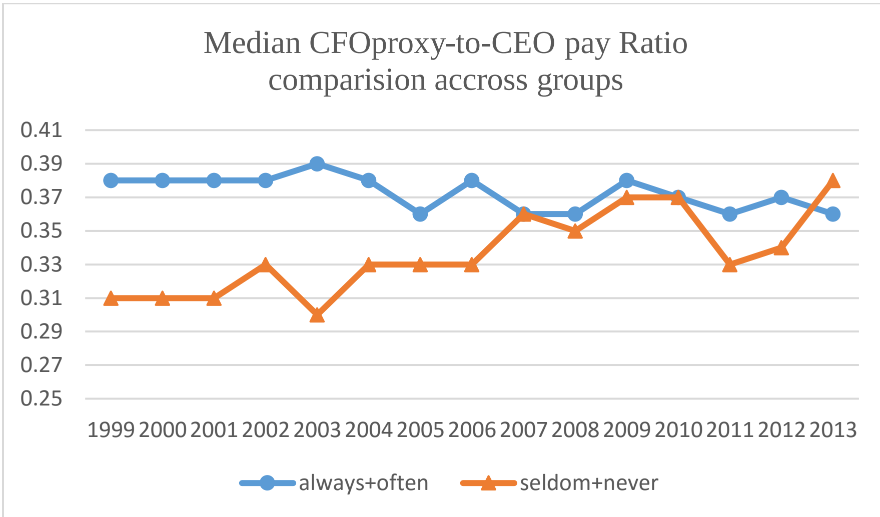

Figure 1.1 presents the ratio of proxy CFO total pay to CEO total pay across groups overtime. Proxy total pay is the lowest pay among the top five executives when CFO pay is unreported. We classify firms into four groups according to their CFO pay reporting frequency during the pre-disclosure-mandate period: "never" (firms never reporting CFO pay), "seldom" (reporting 1-3 times), "often" (reporting 4-6 times), and "always" (always reporting).

Table 1.1: 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 1.2 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***	4457***	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 1.3: 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) (1)	Ln(Proxy CFO pay) – Ln(CEO pay) *Year 2008 and 2009 excluded (2)	Ln(Proxy CFO pay) – Ln(CEO pay) *Financial and Utility Industry excluded (3)	Ln(Proxy CFO pay) – Ln(CEO pay) *Inflation adjusted (4)	Ln(CFO pay) – Ln(CEO pay) *Actual CFO Pay (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 1.4 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)
treat	-0.243***	-0.152**	-0.091	-0.056	-0.268***
	(0.000)	(0.040)	(0.152)	(0.140)	(0.000)
D2006rule*treat	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 1.5. 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 (1)	Excess CEO Pay (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 1.6: 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 1.7: 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 1.8: 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 Performance is measured by	All CFO turnover ROA (1)	Forced CFO turnover ROA (2)	All CFO turnover Stock return (3)	Forced CFO turnover Stock return (4)	All CEO turnover ROA (5)	Forced CEO turnover ROA (6)	All CEO turnover Stock return (7)	Forced CEO turnover Stock return (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*often* Performance	-0.153	-0.031	0.015	0.006	-0.068	0.075	0.023	0.012
	(0.381)	(0.844)	(0.704)	(0.875)	(0.660)	(0.449)	(0.494)	(0.651)
D2006rule*Seldom*	-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)
Often* 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)
Seldom* 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*often	-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*Seldom	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)
Often	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)

Seldom	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	11169	11169	11254	11254	11403	11403	11488	11488
Pseudo R-square	0.028	0.019	0.026	0.017	0.050	0.023	0.048	0.020

Table 1.9: 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 1.10: 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 1.11: 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 (1)	JustBeat (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 1.12: 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 A: 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.

ESSAY II

The Relation Between Idiosyncratic Volatility and Expected Returns: A Statistical Artifact of Temporary Changes in Idiosyncratic Volatility

2.1 Introduction

Rational asset pricing models in which investors hold well-diversified portfolios, and are not exposed to unsystematic risk, imply that there should be no relation between the idiosyncratic volatility (IVOL) and the expected returns. Merton (1987) suggests that idiosyncratic risk and expected return should be positively related as investors with incomplete information will hold under-diversified portfolios. However, researchers such as Ang, Hodrick, Xing and Zhang (hereafter AHXZ 2006) have documented a negative relation between estimated idiosyncratic volatility and subsequent realized returns. The direction of the relationship and what may explain this relationship are disputed. Fu (2009) using an EGARCH model documents a significant positive relationship between idiosyncratic volatility and returns. Cao et al. (2013) finds a positive (negative) relation between idiosyncratic risk and returns among relatively undervalued (overvalued) stocks. Bali et al. (2008) and Han et al. (2011) show that there is no relation between the two. The observed relation between the idiosyncratic (firm-specific) risk of a firm and the stock returns goes against the predictions of the traditional asset pricing models and remains an unresolved puzzle.

We document a systematic pattern of temporary increases in the estimated idiosyncratic volatility for the quintile of stocks with the highest estimated idiosyncratic volatility in a given month. A large portion of this temporary increase in the estimated idiosyncratic volatility is reversed in the subsequent month. This temporary increase in the idiosyncratic volatility for the quintile of stocks with the highest estimated idiosyncratic volatility is associated with positive abnormal returns in the estimation month and negative abnormal returns in the subsequent month. Our evidence shows that these temporary changes in the estimated idiosyncratic volatility and the related positive and negative abnormal returns in the estimation and subsequent months, respectively, create a negative relation between the estimated idiosyncratic volatility and subsequent month returns documented in the prior literature (Ang et al. 2006). After controlling for the (negative) relation with the past month's return, there is no significant relation between idiosyncratic volatility and subsequent month's returns as predicted by traditional asset pricing models. Moreover, we find no significant relation between idiosyncratic volatility and subsequent returns for eighty percent of the stocks that do not exhibit large changes in idiosyncratic volatility despite large differences in the levels of their idiosyncratic volatility. `Moreover, we find no significant relation between idiosyncratic volatility and subsequent returns for subsets of stocks that do not exhibit any significant changes in idiosyncratic volatility despite large differences in the levels of their idiosyncratic volatility. Finally, there is no relation between the estimated idiosyncratic volatility and subsequent returns after a lag of 2 months when the abnormal returns associated with temporary changes are no longer present. Overall, our results are consistent with the notion that there is no relation between the true underlying idiosyncratic volatility and expected returns, and that the previously documented negative relation between estimated idiosyncratic volatility and subsequent month's returns is being driven by temporary one-month increases in the estimated idiosyncratic volatility and the associated abnormal returns for a subset of stocks.

Based on our results we conjecture that there are two possible scenarios, which may explain why the level of idiosyncratic risk does not matter, but the changes in IVOL matter. First, an inefficient and/or incomplete market reaction to unexpected (negative) events could induce both higher temporary idiosyncratic risk and return predictability in observed returns and this in turn induces a relation between estimated idiosyncratic volatility and subsequent returns. Second, the changes in firm characteristics and idiosyncratic risk may result in trades among investors to adjust their portfolios. When this process is completed and new equilibrium is reached, the level of idiosyncratic risk does not matter.

This paper adds to the current understanding of the IVOL puzzle. We contribute by documenting the importance of separating IVOL levels and temporary IVOL changes and their separate effects on future return, respectively. Overall, we conclude that the IVOL puzzle is mostly driven by firms with negative past performance which also experience temporary increases in their idiosyncratic volatility and continue to have abnormal negative returns in the subsequent month. The level of idiosyncratic risk does not matter for firms that do not undergo changes in their idiosyncratic volatility.

The rest of this paper proceeds as follows: section 2.2 briefly reviews the related literature and presents our motivation; section 2.3 documents the data and the methodology; section 2.4 examines the relation between the IVOL level and returns; section 2.5 investigates the relation between IVOL changes and subsequent returns; and section 2.6 concludes the paper.

2.2 Related Literature and Motivation

Traditional asset pricing models in which investors hold well-diversified portfolios imply that there should be no relation between the idiosyncratic volatility (IVOL) and the expected returns. However, Ang, Hordrick, Xing and Zhang (hereafter AHXZ 2006) document that stocks with high idiosyncratic volatility earn low subsequent returns. The presence of significant relation between idiosyncratic risk and return both in US market and in international markets (AHXZ 2009) has puzzled many researchers.

Several papers have attempted to empirically resolve this puzzle and some theories have been proposed to explain this puzzle. Merton (1987) suggests that idiosyncratic risk and expected return should be positively related when investors with incomplete information hold under-diversified portfolios. Consistent with Merton (1987), Fu (2009) uses an EGARCH model and finds a significant positive relation between expected idiosyncratic risk and returns. However, Fink, Fink and He (2012) suggest that the Fu (2009) EGARCH model introduces a look-ahead bias. They find that there is no relation between expected returns and expected idiosyncratic volatility when only information up to time t-1 is used to estimate idiosyncratic volatility. Pontiff (2006) shows that idiosyncratic risk is the single most holding cost faced by arbitrageurs, and therefore, suggests that the negative relation between idiosyncratic volatility and returns may be a result of subsequent price correction of overpriced high IVOL stocks which may be too costly to arbitrage. Shleifer and Vishney (1997) suggest that arbitrage has limits, risk and costs. Consistent with the limits of arbitrage argument, Cao et al (2013) and Stambaugh et al (2013) show a positive relation between idiosyncratic risk and return among relative undervalued stocks, and a negative relation between IVOL and return among relative overvalued stocks. Stambaugh et al (2013) further suggest that the IVOL effect is related to investor sentiment. Yet the debate continues: Bali et al (2008) show that the relation between IVOL and return is not robust when using different weighting schemes and different data frequencies. Han and Lesmond (2011) suggest that the bid-ask bounce biases the estimation of idiosyncratic volatility, and show that the relation between IVOL and return diminishes when CRSP mid-quote based price is used in estimation during sample period 1984 to 2008. Despite these attempts, the overall significantly negative relation between the estimated idiosyncratic volatility and the subsequent month's return continues to be a puzzle. This study will provide a possible resolution for the puzzle by documenting a relatively large temporary increase in the estimated volatility and the associated abnormal return behavior for the sub-group of stocks with the highest estimated idiosyncratic volatility, and by documenting that there is no relation between idiosyncratic volatility and returns for stocks which do not have such temporary increases in idiosyncratic volatility.

We first replicate the AHXZ (2006) results while extending the sample period from July 1963 to December 2013. We find a systematic pattern of relatively large and temporary increases in the estimated idiosyncratic volatility in the estimation month for the quintile of stocks with the highest estimated idiosyncratic volatility and associated positive abnormal returns in the estimation month followed by negative abnormal returns for these stocks in the subsequent month. The large and temporary increases in the estimated idiosyncratic volatility for a sub-group of stocks and the associated abnormal return behavior for these stocks suggests the need to isolate the effect of the IVOL level from the temporary IVOL changes. Otherwise, we may draw a false or spurious conclusion about the relation between IVOL and return. A systematic relation between temporary changes in the estimated idiosyncratic volatility for a sub-group of stocks in a given month and the abnormal return behavior in the subsequent month can create an empirical relation between the estimated idiosyncratic volatility and subsequent month's return even when there is no relation between the true underlying idiosyncratic volatility and expected returns.

2.3 Data and Methodology

The data include all NYSE, AMEX, and NASDAQ stocks with share code 10 or 11 from CRSP for the period from July 1963 to December 2013. Following AHXZ (2006), we require a minimum of 17 trading days in a month.

Following AHXZ (2006) and the literature, we define idiosyncratic volatility relative to the Fama-French 3 factor model. For each stock i, we run the following regression using daily returns within the month:

$$r_t^i = \alpha^i + \beta_{MKT}^i MKT_t + \beta_{SMB}^i SMB_t + \beta_{HML}^i HML_t + \varepsilon_t^i \qquad (2.1)$$

and $\sqrt{Var(\varepsilon_t^i)}$ is defined as the idiosyncratic risk for stock i in that month.

2.4 Idiosyncratic Volatility and Returns

For every month in our sample period we form five quintile portfolios based on the estimated idiosyncratic volatility in that month, and calculate the average IVOL for each portfolio in the estimation month and in the preceding and subsequent months. Panel A of Table 2.1 presents the average IVOL levels for the five portfolios from month t-3 to month t+3. IVOL1 is the portfolio of firms with the lowest estimated IVOL in month t and IVOL5 is the portfolio with the highest estimated IVOL in month t. For the portfolio IVOL5, there is a sharp increase in the average IVOL from 0.0505 to 0.0611 from month t-1 to month t and then a drop back to 0.0510 in month t+1. Similarly, portfolio IVOL1 displays a temporary drop in the average IVOL in month t. These results are not surprising because forming portfolios on a variable not only groups them by the true levels of the variable but also on the temporary changes. Moreover, as we will show later, these temporary changes in IVOL are associated with abnormal returns in the current and subsequent months. Figure 2.1.a shows the patterns of the average levels of idiosyncratic volatility from month t-12 to month t+12 for the five portfolios. A similar pattern of temporary changes is observed.

Panel B of Table 2.1 presents the average returns of the five quintile portfolios from month t-3 to month t+3. Portfolio IVOL5 has relatively large negative average monthly returns (less than -1%) in the preceding months, a relatively large positive return of 1.95% in month t, and then a drop back to 0.09% in month t+1. Thus, the firms in IVOL5 which exhibit a large temporary one-month increase in IVOL in month t also exhibit a large increase in average returns in month t from negative returns in month t-1 to 1.95% in month t and then a large drop to 0.09% in month t+1. It is also noteworthy that IVOL5 has almost twice as high returns than the other portfolios in month t, while exhibiting lower returns in the preceding and subsequent months with large differences. It appears that portfolio IVOL5 includes firms which have large temporary increases in IVOL and abnormal positive returns in month t, and abnormal negative returns in month t+1. Figures 2.1.b and 2.1.c show the patterns of these returns for these portfolios from month t-12 to month t+12. The sharp and temporary changes in the estimated idiosyncratic volatility for the extreme portfolios and the changes in their observed returns reveal the need to isolate the effect of IVOL level from the temporary IVOL changes. Before examining the relation between the level of IVOL and subsequent returns, it is critical to control for the effect of temporary changes in IVOL on subsequent returns.

In order to isolate the effects of IVOL level from the temporary IVOL changes, we create a subsample of stable IVOL firms for every month t in our sample period. We do this by first calculating the average IVOL for each stock over the preceding 12 months (t-12 to t-1) and then calculating the change in IVOL in month t from the past 12-month average. Finally, we select 80% of the stocks for our portfolio of stable IVOL firms for each month by excluding 20% of the firms with the highest absolute value of the change in IVOL in month t. We further subdivide the 20% of the firms with the largest absolute value of the changes into the IVOL increase group (IVOLINC) and the IVOL decrease group (IVOLDEC) based on the sign of the change. We also form five revised quintile portfolios (IVOLS1 to IVOLS5) from the 80% subset of stable IVOL firms every month.

Table 2.2 presents the summary statistics for the IVOL in month t (Panel A), the past 12-month average IVOL (Panel B), and the IVOL change (IVOL – past 12-month average IVOL; Panel C) for the original five quintile portfolios which include all firms. The results show the summary statistics for the distribution of IVOL in month t for the five portfolios, the distribution of the past 12-month average IVOL, and the distribution of the changes in IVOL in month t. The mean IVOL in month t ranges from a low of 0.0087 for portfolio IVOL1 to 0.0611 for portfolio IVOL5, as compared to the past 12-month average IVOL of 0.0141for portfolio IVOL1 and 0.0463 for portfolio IVOL5. This suggests that monthly estimates of IVOL may include a large temporary change component. The mean IVOL change is -0.0054 for portfolio IVOL1 and 0.0149 for portfolio IVOL5.

Table 2.3 presents the summary statistics for the IVOL in month t (Panel A), the past 12-month average IVOL (Panel B), and the IVOL change (IVOL – past 12-month average IVOL; Panel C) for the revised five quintile portfolios (IVOLS1 – IVOLS5) which include the 80% stable IVOL firms, and for the two portfolios with IVOL increases (IVOLINC) and the IVOL decreases (IVOLDEC). The mean IVOL in month t ranges from a low of 0.0085 for portfolio IVOLS1 to 0.0433 for portfolio IVOLS5, as compared to the past 12-month average IVOL of 0.0122 for portfolio IVOLS1 and 0.0408 for portfolio IVOLS5. As expected, the monthly estimates of IVOL are much closer to their 12-month averages for the revised quintiles of stable firms. The mean IVOL change is -0.0037 for portfolio IVOLS1 and 0.0026 for portfolio IVOLS5, as compared to -0.0054 and 0.0149 for IVOL1 and IVOL5, respectively, demonstrating that we have reduced the magnitude of the change. As expected, portfolio IVOLDEC exhibits a mean change of -0.0235, and portfolio IVOLINC exhibits a mean change of 0.0330. The distribution of the IVOL levels in Panel A of the table show that the subset of the stable firms which include 80% of the firms continues to exhibit a large variation in their IVOL levels. The 75th percentile (Q3) for portfolio IVOLS5 is 0.0541, which is 7.84 times larger than the 25% percentile (Q1) of 0.0069 for portfolio IVOLS1. The mean and the median IVOL for portfolio IVOLS5 are about 5 times larger than the corresponding values for portfolio IVOLS1.

Panel A of Table 2.4 presents the average IVOL levels for the seven revised portfolios from month t-3 to month t+3. As expected, portfolios IVOLS1 and IVOLS5 exhibit smaller temporary changes in IVOL in month t, as compared to IVOL1 and IVOL5, respectively, in Table 2.1, and IVOLDEC and IVOLINC picking up the bigger changes. Panel B of Table 2.4 exhibits a similar attenuation of the changes in the returns for IVOLS1 and IVOLS5, with bigger changes picked up by IVOLDEC and IVOLINC portfolios. To further reduce the magnitude of the changes in IVOL in month t for our subset of stable firms, we repeated all of our analysis by successively excluding 30% or 40% of the largest absolute of the IVOL changes, instead of excluding only 20%. The results are consistent with the conclusions of the paper of no relation between the IVOL level and subsequent returns for the subset of stable firms. However, while the magnitude of the IVOL changes becomes smaller, the range of the IVOL level for the subset of stable firms also become smaller. We chose to present the results excluding only 20% of the largest absolute IVOL changes to retain most of the variation in the IVOL levels, while excluding only the most extreme temporary changes in IVOL.

Figure 2.2.a presents the IVOL levels for the seven portfolios. The relatively flat lines for IVOLS1 through IVOLS5 suggest that these firms do not experience large changes in IVOL in month t. Moreover, they exhibit stable levels of IVOL not only in the period prior to formation (by design), but also in the subsequent 12 month period while maintaining the relatively large differences in the IVOL levels.

Figure 2.2.b presents the plots of the average value-weighted monthly returns for the seven revised portfolios. The plots suggest that firms with high stable IVOL levels are firms with volatile and often negative past performance, while firms with low to medium stable IVOL levels earn relatively stable and similar past and future returns of about 1% per month. Moreover, when the returns of the stable high IVOL portfolios stabilize in month +5, they stabilize at about the same levels as those of the low to medium stable IVOL levels. These results suggest that the return differences across portfolios with different IVOL levels are being driven by short term and temporary changes in IVOL and the related return volatility, and not because of any relationship between the IVOL levels and expected return. As stated earlier, excluding 30% or 40% of the firms (instead of 20%) with the largest absolute value of the changes in IVOL from the subset of the stable IVOL firms makes the IVOL levels and the returns more stable for the stable IVOL portfolios, and the results of this paper continue to be consistent with the conclusions of the paper that there is no relation between idiosyncratic volatility and subsequent returns.

2.5 Cross-Sectional Regression results

We further examine the cross-sectional relation between the IVOL level and subsequent return at the firm level using Fama-Macbeth regressions. Specifically, each month from July 1963 to December 2013 we run a firm-level cross-sectional regression as the following:

$$R_{i,t+1} = \alpha_{0t} + \gamma_{1,t} IVOL_{i,t} + \gamma_{2,t} beta_{i,t} + \gamma_{3,t} size_{i,t} + \gamma_{4,t} BM_{i,t} + \gamma_{5,t} R_{i,t} + \varepsilon_{it} \tag{2.2}$$

The Dependent Variable (Ri,t+1) is the realized stock return of firm i in month t+1. Beta is estimated by CAPM model using previous 36 monthly returns. Following Bali et al (2011) and existing literature, firm size is measured by the natural logarithm of the market value of equity (a stock's price times shares outstanding in millions of dollars) at the month of t for each stock;

following Fama and French (1992) and Bali et al (2011), we compute a firm's book-to-market ratio using the market value of its equity at the end of December of the previous year and the book value of common equity plus balance-sheet deferred taxes for the firm's latest fiscal year ending in the prior calendar year. [9]

2.5.1 Regression Results of All firms and Subsample of Stable firms

Table 2.5 presents the time-series averages of the slopes of cross-sectional regressions of all firms using the standard Fama and MacBeth(1973) methodology. In column (1) we show the results of the simple regression of the subsequent month return on IVOL. The average slope co-efficient of -0.118 is significant. In column (2), wherein we control for beta, firm size and the book-to-market ratio, the average slope coefficient on IVOL of regression on all firms is negative, -0.140, and significant at 1% level. These significant negative coefficients on IVOL mirror the negative relation between idiosyncratic risk and future returns documented in prior research. However, when we include the return in month t as a control variable in columns (3) and (4), the average slope coefficient on IVOL drops in magnitude by about 50% and becomes insignificant. This suggests that the observed negative relation between the estimated idiosyncratic volatility for a given month and the subsequent month's return is a manifestation of the abnormal return behavior for a subset of the firms. The significant negative average slope coefficient on the return for month t, and the implied return reversal from month t to t+1 is consistent with the pattern of returns for the highest IVOL quintile of firms as documented in Panel B of Table 2.1 and Figures 2.1.b and 2.1.c.

Panel A of Table 2.6 presents the regression results for the subsample of stable firms which exclude the 20% of the firms each month with the largest absolute value of IVOL changes in the

[9] Flowing literature, the book-to-market ratio and size are winsorized at the 1% and 99% level to avoid issues of extreme observations

estimation month. The magnitude of the average estimated slope coefficient on IVOL for the simple regression drops from -0.118 to -0.042 and becomes insignificant. Moreover, it remains insignificant when other control variables are included. These results provide additional evidence that the previously documented negative relation between the estimated monthly idiosyncratic volatility and subsequent month's return is being a driven by a subsample of firms with large temporary changes in idiosyncratic volatility and the associated abnormal return behavior in the estimation and subsequent months. Panel B of Table 2.6 presents the regression results for a smaller (70%) subsample of stable IVOL stocks by excluding the 30% of the stocks with the highest absolute value of the change in volatility in the estimation month over its 12-month average. The results are consistent with the results in Panel A of the Table.

Panel A (Panel B) of Table 2.7 presents the regression results for the firms in the IVOLINC (IVOLDEC) portfolio, which have a positive (negative) change in IVOL from the 20% subset of the firms which were excluded from the subsample of stable firms because of large absolute value of IVOL changes. For the IVOLINC group of firms, the average slope coefficient on IVOL is negative and significant in the simple regression, and when beta, size and book-to-market are included as control variables, and becomes insignificant only when the return in month t is included. For the IVOLDEC group of firms, there is no significant relation between the estimated IVOL and the subsequent month return. These results suggest, along with the results for the stable subsample, suggest that the observed negative relation between the estimated idiosyncratic volatility in a given month and the subsequent month's return is being primarily driven by the subsample of firms which experience a large temporary increase in the estimated idiosyncratic volatility and relatively large positive returns in the estimation month which are reversed in the subsequent month. Excluding such firms or controlling for return reversals takes away the significance of the relation between idiosyncratic volatility and returns.

2.5.2 IVOL Level, IVOL Changes and Return Reversal

We next investigate the relationship between the idiosyncratic volatility and subsequent month returns by including both the changes in IVOL and the past average IVOL. Specifically, we run Fama-Macbeth regressions for all firms with the returns for subsequent months as the dependent variables, as follows

$$R_{i,t+1} = \alpha_{0t} + \gamma_{1,t}\overline{IVOL_{t-1},\ IVOL_{t-\mathrm{j}}} + \gamma_{2,t}IVOL_t/\overline{IVOL_{t-1},\ IVOL_{t-\mathrm{j}}} + \gamma_{3,t}beta_{i,t} +$$

$$\gamma_{4,t}size_{i,t} + \gamma_{5,t}BM_{i,t} + \gamma_{6,t}R_{i,t} + \varepsilon_{it} \quad (2.3)$$

$Where\ j = 3, 6\ and\ 12$, respectively

Panel A of Table 2.8 reports the results when we include both the past 12-month average IVOL ($\overline{IVOL_{t-1},\ IVOL_{t-12}}$) and the standardized IVOL changes (IVOL level in month t compared with its past 12 month average, $IVOL_t/\overline{IVOL_{t-1},\ IVOL_{t-12}}$) in the cross-sectional analysis. The results of column (1) to column (4) show that the average slope coefficients on IVOL level become insignificant in all specifications. On the other hand, the relationship between temporary changes in IVOL and subsequent month returns stays significant and negative, even after the return reversal effect is accounted for. When we include both the past 6-month average IVOL and the IVOL changes, results suggest similar pattern (Panel B of Table 2.8). When both the past 3-month IVOL and the IVOL changes are included, the average slope coefficients of -0.122 and -0.109 in column (10) and (12) of Panel C of Table 2.8 are significant at 5% level. This result is not surprising since some of the temporary changes start before month t as suggested by both figure 2.1.a and figure 2.2.a. Thus, the average coefficients on IVOL level pick up some of the effects of temporary changes in IVOL.

The results of Table 2.8 suggest that the negative relationship between estimated IVOL and subsequent month return is being driven by the changes in IVOL and not by the IVOL levels. Moreover, the negative relationship between changes in IVOL and subsequent month returns is independent of the return reversal effect.

We suggest two possible explanations for why there may be no relation between IVOL level and subsequent returns when there is no change in IVOL, but a significant negative relation between IVOL level and returns when firm experience IVOL increases. First, an inefficient and/or incomplete market reaction to unexpected (negative) events could induce both higher temporary idiosyncratic risk and return predictability in observed returns and this in turn induces a relation between estimated idiosyncratic volatility and subsequent returns. Second, the changes in firm characteristics and idiosyncratic risk may result in trades among investors to adjust their portfolios. When this process is completed and new equilibrium is reached, the level of idiosyncratic risk does not matter.

2.5.3 Persistence of the Effect of IVOL on Future Returns

Lastly, we examine the persistence of the effect of IVOL on future returns. Specifically, we run Fama-Macbeth regressions for all firms with the returns for subsequent months as the dependent variables, as follows

$$R_{i,t+n} = \alpha_{0t} + \gamma_{1,t} IVOL_{i,t} + \gamma_{2,t} beta_{i,t} + \gamma_{3,t} size_{i,t} + \gamma_{4,t} BM_{i,t} + \varepsilon_{it} \quad (2.4)$$

where n = 1, 2, 3, 4 and 6.

Other variables are as specified as in equation (2.2). Results are reported in Table 2.9.

Results show that the average slope coefficients on IVOL continues to be significant for the return in month t+2 but with a smaller absolute magnitude. The relation continues to weaken, and for months t+3, t+4 and t+6, there is no significant relation between the estimated idiosyncratic volatility and returns. In unreported results, we find no relation between the idiosyncratic volatility and returns for month 7 and beyond also. These results mirror the graph in Figures 2.1.b, 2.1.c and 2.2.b, and are noteworthy because despite the large differences in IVOL in month t, the returns start to converge and are not different from each other among different IVOL portfolios after 3 to 6 months, although their IVOL levels still stay different. The IVOL level of high IVOL portfolios remains high, and the IVOL level of low IVOL portfolio remains low, but the returns converge.

2.6 Conclusions

We document a systematic pattern of temporary increases in the estimated idiosyncratic volatility for the quintile of stocks with the highest estimated idiosyncratic volatility in a given month. A large portion of this temporary increase in the estimated idiosyncratic volatility is reversed in the subsequent month. This temporary increase in the idiosyncratic volatility for the quintile of stocks with the highest estimated idiosyncratic volatility is associated with relatively large positive returns (positive abnormal returns) in the estimation month and relatively low returns (negative abnormal returns) in the subsequent month. Our evidence shows that these temporary changes (specifically temporary increases) in the estimated idiosyncratic volatility and the related positive and negative abnormal returns in the estimation and subsequent months, respectively, create a negative relation between the estimated idiosyncratic volatility and subsequent month returns documented in the prior literature (Ang et al. 2006). After controlling for the (negative) relation with the past month's return, there is no significant relation between idiosyncratic volatility and subsequent month's returns as predicted by traditional asset pricing models. Moreover, we find no significant relation between idiosyncratic volatility and subsequent returns for eighty percent of the stocks that do not exhibit large changes in idiosyncratic volatility despite large differences in the levels of their idiosyncratic volatility. Finally, the negative relation between idiosyncratic volatility and subsequent returns starts to weaken as lags between the estimated volatility and returns are introduced to control for the problem created by the relation between the temporary changes and associated returns in the estimation and subsequent months. By month t+3, the negative relation is no longer present, despite large continued differences in their estimated volatilities. Overall, our results are consistent with the notion that there is no relation between the true underlying idiosyncratic volatility and expected returns, and that the previously documented negative relation between estimated idiosyncratic volatility and subsequent month's returns is being driven by temporary changes in the estimated idiosyncratic volatility and the associated abnormal returns for a subset of stocks.

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Figure 2.1.a: IVOL level of AHXZ portfolios

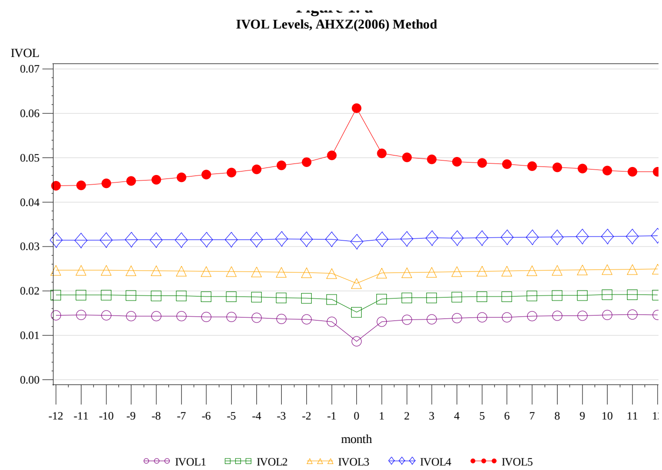

Figure 2.1.b: VW Return of AHXZ Portfolios

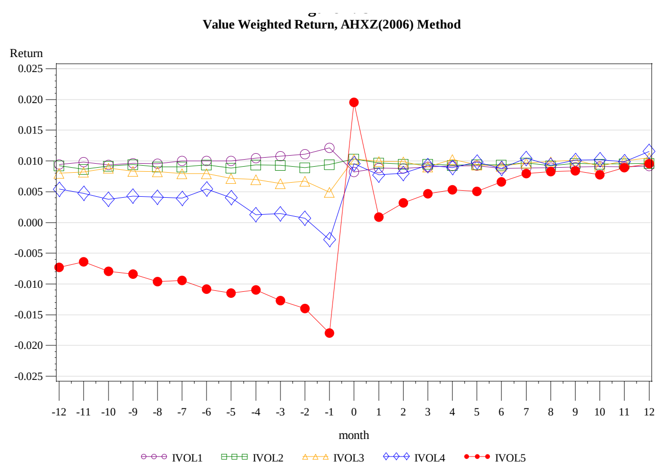

Figure 2.1.c: EW Return of AHXZ Portfolios

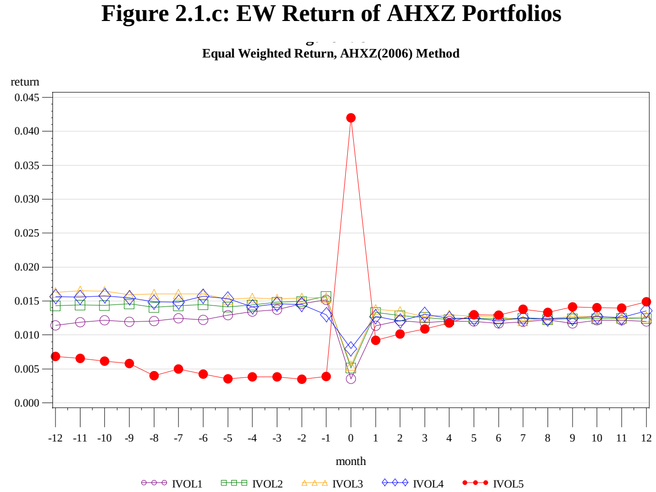

Figure 2.2.1 shows the IVOL level (a), value weighted average monthly return (b), equal weighted average monthly return (c) each month for 12 months before and after the portfolio formation. AHXZ (2006) method is used to estimate idiosyncratic volatility and to rank portfolios, i.e., we estimated idiosyncratic risk relative to ff-3 model using daily returns within that month. Then stocks are ranked according to their idiosyncratic risk level each month from July 1963 to December 2013. Portfolio formation month is the month *0* in the graph.

Figure 2.2.a: IVOL Level of Revised Seven Portfolios

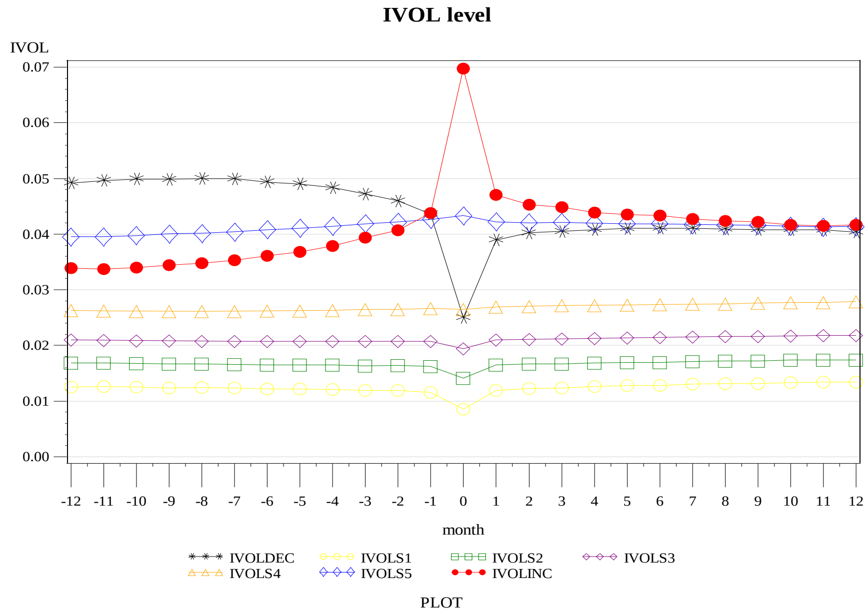

Figure 2.2.b: VW Return of Revised Seven Portfolios

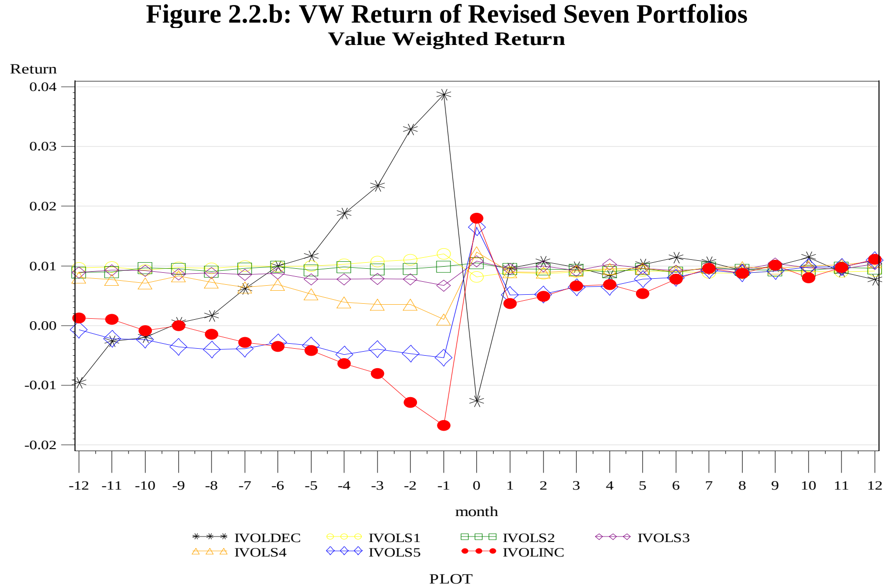

Figure 2.2.a presents the IVOL levels for the seven revised portfolios. **Figure 2.2.b** presents the plots of the average value-weighted monthly returns for the seven revised portfolios. *IVOLS1-IVOLS5:* We first calculate the average IVOL for each stock over the preceding 12 months (t-12 to t-1) and then calculate the change in IVOL in month t from the past 12-month average. We select 80% of the stocks for our portfolio of stable IVOL firms for each month by excluding 20% of the firms with the highest absolute value of the change in IVOL in month t. We form five revised quintile portfolios (IVOLS1 to IVOLS5) from the 80% subset of stable IVOL firms every month. *IVOLINC and IVOLDEC:* We further subdivide the 20% of the firms with the largest absolute value of the changes into the IVOL increase group *(IVOLINC*) and the IVOL decrease group (*IVOLDEC*) based on the sign of the change.

Table 2.1 Idiosyncratic Volatility and Returns of All Firms

For every month in our sample period we form five quintile portfolios based on the estimated idiosyncratic volatility in that month, and calculate the average IVOL for each portfolio in the estimation month and in the preceding and subsequent months. Panel A of Table 2.1 presents the average IVOL levels for the five portfolios from month t-3 to month t+3. IVOL1 is the portfolio of firms with the lowest estimated IVOL in month t and IVOL5 is the portfolio with the highest estimated IVOL in month t. Panel B of Table 2.1 presents the average returns of the five quintile portfolios from month t-3 to month t+3.

Panel A							
IVOL	t-3	t-2	t-1	***t***	t+1	t+2	t+3
IOVL1	0.0137	0.0136	0.0131	0.0087	0.0130	0.0135	0.0136
IOVL2	0.0184	0.0184	0.0181	0.0152	0.0182	0.0184	0.0185
IOVL3	0.0242	0.0241	0.0239	0.0217	0.0240	0.0242	0.0243
IOVL4	0.0317	0.0317	0.0316	0.0311	0.0316	0.0318	0.0320
IOVL5	0.0483	0.0490	0.0505	0.0611	0.0510	0.0501	0.0496

Panel B							
VW Returns	t-3	t-2	t-1	***t***	t+1	t+2	t+3
IOVL1	0.0108	0.0111	0.0121	0.0082	0.0089	0.0088	0.0090
IOVL2	0.0093	0.0089	0.0094	0.0103	0.0097	0.0095	0.0095
IOVL3	0.0064	0.0068	0.0049	0.0103	0.0099	0.0099	0.0090
IOVL4	0.0014	0.0007	-0.0028	0.0096	0.0077	0.0080	0.0093
IOVL5	-0.0127	-0.0140	-0.0180	0.0195	0.0009	0.0032	0.0047

Table 2.2 IVOL, Average IVOL and the IVOL change of All Firms

For every month in our sample period we form five quintile portfolios based on the estimated idiosyncratic volatility in that month. Table 2.2 presents the summary statistics for the IVOL in month t (Panel A), the past 12-month average IVOL (Panel B), and the IVOL change (IVOL – past 12-month average IVOL; Panel C) for the original five quintile portfolios which include all firms.

Panel A IVOL	Mean	Std	P10	Q1	Median	Q3	P90
IOVL1	0.0087	0.0025	0.0059	0.0068	0.0083	0.0101	0.0113
IOVL2	0.0152	0.0039	0.0112	0.0124	0.0143	0.0174	0.0201
IOVL3	0.0217	0.0059	0.0157	0.0172	0.0201	0.0256	0.0297
IOVL4	0.0311	0.0090	0.0221	0.0240	0.0282	0.0375	0.0437
IOVL5	0.0611	0.0209	0.0406	0.0444	0.0537	0.0769	0.0900

Panel B Past 12 month Average IVOL	Mean	Std	P10	Q1	Median	Q3	P90
IOVL1	0.0141	0.0027	0.0113	0.0122	0.0137	0.0153	0.0176
IOVL2	0.0188	0.0042	0.0146	0.0156	0.0175	0.0216	0.0239
IOVL3	0.0244	0.0062	0.0185	0.0197	0.0224	0.0291	0.0332
IOVL4	0.0316	0.0086	0.0231	0.0246	0.0283	0.0393	0.0447
IOVL5	0.0463	0.0142	0.0325	0.0344	0.0397	0.0610	0.0686

Panel C IVOL Change	Mean	Std	P10	Q1	Median	Q3	P90
IOVL1	-0.0054	0.0023	-0.0084	-0.0071	-0.0050	-0.0039	-0.0027
IOVL2	-0.0035	0.0027	-0.0063	-0.0047	-0.0036	-0.0021	-0.0009
IOVL3	-0.0028	0.0036	-0.0065	-0.0044	-0.0030	-0.0011	0.0012
IOVL4	-0.0005	0.0050	-0.0053	-0.0028	-0.0009	0.0016	0.0047
IOVL5	0.0149	0.0114	0.0048	0.0084	0.0124	0.0182	0.0280

Table 2.3 IVOL, Average IVOL and the IVOL change of the Revised Seven Portfolios

IVOLS1-IVOLS5: We first calculate the average IVOL for each stock over the preceding 12 months (t-12 to t-1) and then calculate the change in IVOL in month t from the past 12-month average. We select 80% of the stocks for our portfolio of stable IVOL firms for each month by excluding 20% of the firms with the highest absolute value of the change in IVOL in month t. We form five revised quintile portfolios (IVOLS1 to IVOLS5) from the 80% subset of stable IVOL firms every month. *IVOLINC and IVOLDEC:* We further subdivide the 20% of the firms with the largest absolute value of the changes into the IVOL increase group *(IVOLINC*) and the IVOL decrease group (*IVOLDEC*) based on the sign of the change. Table 2.3 presents the summary statistics for the IVOL in month t (Panel A), the past 12-month average IVOL (Panel B), and the IVOL change (IVOL – past 12-month average IVOL; Panel C) for the revised five quintile portfolios (*IVOLS1 – IVOLS5*) and for the two portfolios with IVOL increases *(IVOLINC*) and the IVOL decreases (*IVOLDEC*).

Panel A IVOL	Mean	Std	P10	Q1	Median	Q3	P90
IVOLS1	0.0085	0.0022	0.0062	0.0069	0.0083	0.0096	0.0108
IVOLS2	0.0141	0.0034	0.0106	0.0117	0.0134	0.0159	0.0183
IVOLS3	0.0194	0.0048	0.0144	0.0157	0.0183	0.0225	0.0259
IVOLS4	0.0265	0.0068	0.0196	0.0213	0.0246	0.0315	0.0360
IVOLS5	0.0433	0.0117	0.0316	0.0337	0.0388	0.0541	0.0614
IVOLINC	0.0697	0.0243	0.0435	0.0494	0.0613	0.0908	0.1066
IVOLDEC	0.0250	0.0089	0.0157	0.0182	0.0216	0.0327	0.0398

Panel B Past 12 month Average IVOL	Mean	Std	P10	Q1	Median	Q3	P90
IVOLS1	0.0122	0.0025	0.0096	0.0104	0.0118	0.0133	0.0155
IVOLS2	0.0166	0.0035	0.0131	0.0140	0.0155	0.0189	0.0207
IVOLS3	0.0208	0.0048	0.0161	0.0171	0.0193	0.0243	0.0275
IVOLS4	0.0263	0.0065	0.0197	0.0214	0.0241	0.0315	0.0356
IVOLS5	0.0408	0.0108	0.0302	0.0317	0.0361	0.0515	0.0579
IVOLINC	0.0367	0.0122	0.0242	0.0267	0.0316	0.0485	0.0556
IVOLDEC	0.0485	0.0161	0.0329	0.0352	0.0422	0.0626	0.0725

Panel C IVOL Change	Mean	Std	P10	Q1	Median	Q3	P90
IVOLS1	-0.0037	0.0016	-0.0055	-0.0042	-0.0035	-0.0029	-0.0023
IVOLS2	-0.0024	0.0020	-0.0045	-0.0033	-0.0024	-0.0015	-0.0003
IVOLS3	-0.0014	0.0024	-0.0037	-0.0024	-0.0015	-0.0003	0.0009
IVOLS4	0.0002	0.0028	-0.0022	-0.0009	-0.0001	0.0011	0.0027
IVOLS5	0.0026	0.0029	0.0005	0.0012	0.0020	0.0031	0.0049
IVOLINC	0.0330	0.0131	0.0173	0.0238	0.0306	0.0411	0.0512
IVOLDEC	-0.0235	0.0084	-0.0352	-0.0286	-0.0222	-0.0174	-0.0133

Table 2.4 Idiosyncratic Volatility and Returns of the Revised Seven Portfolios

IVOLS1-IVOLS5: We first calculate the average IVOL for each stock over the preceding 12 months (t-12 to t-1) and then calculate the change in IVOL in month t from the past 12-month average. We select 80% of the stocks for our portfolio of stable IVOL firms for each month by excluding 20% of the firms with the highest absolute value of the change in IVOL in month t. We form five revised quintile portfolios (*IVOLS1 to IVOLS5*) from the 80% subset of stable IVOL firms every month. *IVOLINC and IVOLDEC:* We further subdivide the 20% of the firms with the largest absolute value of the changes into the IVOL increase group *(IVOLINC)* and the IVOL decrease group (*IVOLDEC*) based on the sign of the change. Panel A of Table 2.4 presents the average IVOL levels for the seven revised portfolios from month t-3 to month t+3. Panel B of Table 2.4 presents the average returns for the seven revised portfolios from month t-3 to month t+3.

Panel A							
IVOL Level	t-3	t-2	t-1	***t***	t+1	t+2	t+3
IVOLS1	0.0119	0.0119	0.0116	0.0085	0.0119	0.0123	0.0124
IVOLS2	0.0164	0.0164	0.0163	0.0141	0.0165	0.0167	0.0167
IVOLS3	0.0207	0.0207	0.0207	0.0194	0.0210	0.0211	0.0212
IVOLS4	0.0265	0.0265	0.0266	0.0265	0.0270	0.0270	0.0272
IVOLS5	0.0419	0.0422	0.0426	0.0433	0.0422	0.0420	0.0421
IVOLINC	0.0394	0.0407	0.0438	0.0697	0.0471	0.0453	0.0448
IVOLDEC	0.0472	0.0460	0.0436	0.0250	0.0389	0.0402	0.0405

Panel B							
VW Returns	t-3	t-2	t-1	***t***	t+1	t+2	t+3
IVOLS1	0.0107	0.0110	0.0120	0.0081	0.0089	0.0087	0.0090
IVOLS2	0.0094	0.0095	0.0099	0.0105	0.0095	0.0094	0.0094
IVOLS3	0.0079	0.0078	0.0067	0.0108	0.0095	0.0100	0.0092
IVOLS4	0.0035	0.0036	0.0010	0.0123	0.0090	0.0089	0.0092
IVOLS5	-0.0039	-0.0047	-0.0054	0.0165	0.0051	0.0053	0.0065
IVOLINC	-0.0081	-0.0129	-0.0168	0.0180	0.0037	0.0049	0.0067
IVOLDEC	0.0234	0.0328	0.0387	-0.0127	0.0096	0.0107	0.0097

Table 2.5 Fama-MacBeth Regression Analysis of All Firms

The table presents the time-series averages of the slopes in cross-sectional regressions using the standard Fama and MacBeth(1973) methodology. The Dependent Variable $R_{i,t+1}$ is the realized stock return of firm *i* in month t+1. Beta is estimated by CAPM model using previous 36 monthly return. Size and book-to-market ratio are defined as Fu(2009). Standard errors are Newey-West method corrected. P-value are in parenthesis. *, ** and *** indicates 10%, 5% and 1% significance level respectively.

$$R_{i,t+1} = \alpha_{0t} + \gamma_{1,t} IVOL_{i,t} + \gamma_{2,t} beta_{i,t} + \gamma_{3,t} size_{i,t} + \gamma_{4,t} BM_{i,t} + \gamma_{5,t} R_{i,t} + \varepsilon_{it}$$

	All firms			
	(1)	(2)	(3)	(4)
$Ivol_t$	-0.118**	-0.140***	-0.066	-0.056
	(0.011)	(0.000)	(0.203)	(0.102)
beta		0.001		0.001
		(0.210)		(0.295)
Size		-0.002***		-0.001***
		(0.000)		(0.005)
BtoM		0.002***		0.003***
		(0.003)		(0.000)
Rt			-0.053***	-0.062***
			(0.000)	(0.000)
Average Adjusted Rsq	0.017	0.042	0.025	0.050

Table 2.6 Fama-MacBeth Regression Analysis of Stable IVOL Firms

We first calculate the average IVOL for each stock over the preceding 12 months (t-12 to t-1) and then calculate the change in IVOL in month t from the past 12-month average. We select 80% of the stocks for our portfolio of stable IVOL firms for each month by excluding 20% of the firms with the highest absolute value of the change in IVOL in month t.

The table presents the time-series averages of the slopes in cross-sectional regressions using the standard Fama and MacBeth(1973) methodology. The Dependent Variable $R_{i,t+1}$ is the realized stock return of firm i in month t+1. Beta is estimated by CAPM model using previous 36 monthly return. Size and book-to-market ratio are defined as Fu(2009). tandard errors are Newey-West method corrected. P-value are in parenthesis. *, ** and *** indicates 10%, 5% and 1% significance level respectively.

$$R_{i,t+1} = \alpha_{0t} + \gamma_{1,t}IVOL_{i,t} + \gamma_{2,t}beta_{i,t} + \gamma_{3,t}size_{i,t} + \gamma_{4,t}BM_{i,t} + \gamma_{5,t}R_{i,t} + \varepsilon_{it}$$

Panel A	Stable IVOL FIRMS (80% Firms)			
	(1)	(2)	(3)	(4)
$Ivol_t$	-0.042	-0.089	-0.038	-0.050
	(0.591)	(0.103)	(0.639)	(0.379)
beta		0.001		0.001
		(0.156)		(0.186)
Size		-0.001***		-0.001***
		(0.000)		(0.005)
BtoM		0.002***		0.002***
		(0.007)		(0.001)
Rt			-0.053***	-0.061***
			(0.000)	(0.000)
Average Adjusted Rsq	0.022	0.048	0.030	0.055

Panel B	Stable IVOL FIRMS (70% Firms)			
	(1)	(2)	(3)	(4)
$Ivol_t$	-0.034	-0.085	-0.036	-0.052
	(0.677)	(0.142)	(0.674)	(0.391)
beta		0.001		0.002
		(0.143)		(0.153)
Size		-0.001***		-0.001***
		(0.000)		(0.009)
BtoM		0.002***		0.002***
		(0.008)		(0.002)
Rt			-0.053***	-0.062***
			(0.000)	(0.000)
Average Adjusted Rsq	0.024	0.050	0.032	0.057

Table 2.7 Fama-MacBeth Regression Analysis of Firms that Experience Temporary Changes

Firms belong to the *IVOLINC* portfolio if they are among the 20% of the firms with the highest absolute value of the change in IVOL in month t from the past $12 -$ month average ($|IVOL_t - \overline{IVOL_{t-1},\ IVOL_{t-12}}|$) and if the change is positive. Firms belong to the *IVOLDEC* portfolio if they are among the 20% of the firms with the highest absolute value of the change in IVOL in month t from the past $12 -$ month average ($|IVOL_t - \overline{IVOL_{t-1},\ IVOL_{t-12}}|$) and if the change is Negative.

The table presents the time-series averages of the slopes in cross-sectional regressions using the standard Fama and MacBeth(1973) methodology. The Dependent Variable Ri,t+1 is the realized stock return of firm i in month t+1. Beta is estimated by CAPM model using previous 36 monthly return. Size and book-to-market ratio are defined as Fu(2009). Standard errors are Newey-West method corrected. P-value are in parenthesis. *, ** and *** indicates 10%, 5% and 1% significance level respectively.

$$R_{i,t+1} = \alpha_{0t} + \gamma_{1,t} IVOL_{i,t} + \gamma_{2,t} beta_{i,t} + \gamma_{3,t} size_{i,t} + \gamma_{4,t} BM_{i,t} + \gamma_{5,t} R_{i,t} + \varepsilon_{it}$$

	Panel A IVOLINC				***Panel B IVOLDEC***			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Ivol_t$	-0.108***	-0.136***	-0.061	-0.056	-0.029	-0.012	-0.076	0.076
	(0.008)	(0.002)	(0.147)	(0.222)	(0.737)	(0.905)	(0.407)	(0.592)
beta		-0.001		-0.000		0.003**		0.002
		(0.487)		(0.889)		(0.025)		(0.112)
Size		-0.006***		-0.004***		-0.004***		-0.002
		(0.000)		(0.000)		(0.000)		(0.223)
BtoM		0.002*		0.004***		0.003**		-0.000
		(0.057)		(0.001)		(0.021)		(0.911)
Rt			-0.047***	-0.053***			-0.074***	-0.057***
			(0.000)	(0.000)			(0.000)	(0.002)
Average Adj Rsq	0.015	0.034	0.027	0.046	0.017	0.030	0.024	0.039

Table 2.8 IVOL Puzzle, IVOL changes and Return Reversal

This table presents the time-series averages of the slopes in cross-sectional regressions using the standard Fama and MacBeth(1973) methodology. The Dependent variable $R_{i,t+1}$ is the realized stock return of firm *i* at month t+1. $\overline{IVOL_{t-1},\ IVOL_{t-12}}$ is the average IVOL level of firm *i* in the past 12 month (*t-1* to *t-12*). Beta is estimated by CAPM model using previous 36 monthly return. Size and book-to-market ratio are defined as Fu(2009). Newey-West P-value are in parenthesis. *, ** and *** indicates 10%, 5% and 1% significance level respectively.

$$R_{i,t+1} = \alpha_{0t} + \gamma_{1,t}\overline{IVOL_{t-1},\ IVOL_{t-j}} + \gamma_{2,t}IVOL_t/\overline{IVOL_{t-1},\ IVOL_{t-j}} + \gamma_{3,t}beta_{i,t} + \gamma_{4,t}size_{i,t} + \gamma_{5,t}BM_{i,t} + \gamma_{6,t}R_{i,t} + \varepsilon_{it}$$
$Where\ j = 3, 6\ and\ 12$, respectively

Panel A	All Firms. Dependent Var: Ret_{t+1}			
	(1)	(2)	(3)	(4)
$\overline{IVOL_{t-1},\ IVOL_{t-12}}$	-0.040	-0.069	-0.054	-0.045
	(0.633)	(0.310)	(0.531)	(0.522)
$IVOL_t/\overline{IVOL_{t-1},\ IVOL_{t-12}}$	-0.004***	-0.004***	-0.002***	-0.002***
	(0.000)	(0.000)	(0.000)	(0.001)
beta		0.001		0.001
		(0.385)		(0.327)
Size		-0.002***		-0.001***
		(0.000)		(0.001)
BtoM		0.002***		0.003***
		(0.001)		(0.000)
Rt			-0.055***	-0.062***
			(0.000)	(0.000)
Average Adjusted Rsq	0.026	0.046	0.034	0.053

Panel B	(5)	(6)	(7)	(8)
$\overline{IVOL_{t-1},\ IVOL_{t-6}}$	-0.068	-0.099	-0.085	-0.081
	(0.378)	(0.102)	(0.290)	(0.192)
$IVOL_t/\overline{IVOL_{t-1},\ IVOL_{t-6}}$	-0.003***	-0.003***	-0.002***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.006)
beta		0.001		0.001
		(0.308)		(0.260)
Size		-0.002***		-0.001***
		(0.000)		(0.000)
BtoM		0.002***		0.003***
		(0.001)		(0.000)
Rt			-0.054***	-0.062***
			(0.000)	(0.000)
Average Adjusted Rsq	0.025	0.046	0.033	0.053

Panel C	(9)	(10)	(11)	(12)
$\overline{IVOL_{t-1},\ IVOL_{t-3}}$	-0.100	-0.122**	-0.113	-0.109**
	(0.148)	(0.018)	(0.113)	(0.040)
$IVOL_t/\overline{IVOL_{t-1},\ IVOL_{t-3}}$	-0.002***	-0.002***	-0.001**	-0.001**
	(0.000)	(0.000)	(0.038)	(0.026)
beta		0.001		0.001
		(0.298)		(0.237)
Size		-0.002***		-0.001***
		(0.000)		(0.000)
BtoM		0.002***		0.003***
		(0.002)		(0.000)
Rt			-0.054***	-0.063***
			(0.000)	(0.000)
Average Adjusted Rsq	0.023	0.045	0.031	0.052

Table 2.9 Persistence of the Effect of IVOL on Future Returns

The table presents the time-series averages of the slopes in cross-sectional regressions using the standard Fama and MacBeth(1973) methodology. The Dependent Variables $R_{i,t+1,}$ $R_{i,t+2,}$ $R_{i,t+3,}$ $R_{i,t+4,}$ and $R_{i,t+6}$ are the realized stock return of firm *i* at month t+1 to t+4, and t+6, respectively. Beta is estimated by CAPM model using previous 36 monthly return. Size and book-to-market ratio are defined as Fu(2009). Newey-West P-value are in parenthesis. *, ** and *** indicates 10%, 5% and 1% significance level respectively.

$$R_{i,t+n} = \alpha_{0t} + \gamma_{1,t} IVOL_{i,t} + \gamma_{2,t} beta_{i,t} + \gamma_{3,t} size_{i,t} + \gamma_{4,t} BM_{i,t} + \varepsilon_{it}$$

$Where\ n = 1, 2, 3, 4, and\ 6$, respectively

All Firms	Dependent Variable				
	Ret_{t+1}	Ret_{t+2}	Ret_{t+3}	Ret_{t+4}	Ret_{t+6}
	(1)	(2)	(3)	(4)	(5)
$Ivol_t$	-0.140***	-0.083**	-0.029	-0.021	-0.003
	(0.000)	(0.011)	(0.376)	(0.535)	(0.942)
beta	0.001	0.001	0.001	0.001	0.001
	(0.210)	(0.238)	(0.465)	(0.346)	(0.516)
Size	-0.002***	-0.001***	-0.001***	-0.001***	-0.001**
	(0.000)	(0.001)	(0.007)	(0.009)	(0.021)
BtoM	0.002***	0.002***	0.003***	0.003***	0.003***
	(0.003)	(0.000)	(0.000)	(0.000)	(0.000)
Average Adjusted Rsq	0.042	0.041	0.040	0.040	0.039