# Do CFOs Have Styles of Their Own? An Empirical Investigation of the Effect of Individual CFOs on Financial Reporting Practices*

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**November 20, 2008**

* We thank Brad Blaylock, Mei Feng, Jane Kennedy, Kevin Koh, Sarah McVay, Shivaram Rajgopal, Lisa Sedor, Terry Shevlin, Ryan Wilson, and the workshop participants at the University of Oregon and the University of Washington for their helpful comments. We also thank Jared Jennings for his valuable research assistance.

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## Abstract

This paper examines the effect of Chief Financial Officers' (CFOs') individual philosophy or "style" on corporate financial reporting practices. We track 691 CFOs across different firms over time and investigate whether CFO-specific factors explain a firm's earnings related and disclosure related reporting choices. We find that, across a wide range of financial reporting strategies, individual CFOs styles do matter. CFO-specific factors explain a significant portion of the heterogeneity in financial reporting practices. Moreover, we trace the CFO style to observable CFO characteristics. Specifically, we examine whether CFOs' gender, age, and educational background affect their styles. We find that older CFOs are generally more conservative in deciding financial reporting strategies, while CFOs with undergraduate business school backgrounds appear to be more aggressive.

Key Words: Managerial Style, Management Turnover, Accounting Choice, Voluntary Disclosure

*"I believe in being disciplined but aggressive,"*
– Chris Liddell, CFO, Microsoft Corp. 2005-present [1]

## 1. Introduction

What impact, if any, do individual differences in Chief Financial Officers' (CFOs') preferences have on firms' financial reporting choices? The purpose of this paper is to examine this question. The CFO typically oversees the process of preparing financial reports and has a direct impact on accounting related decisions, from choosing accounting methods and making accounting adjustments, to forming voluntary disclosure strategies. However, little research has examined the effect of individual CFOs' preferences on financial reporting practices. Prior research generally focuses on the impact of various firm-level (e.g., Klein, 2002), industry-level and market-level characteristics (e.g., Leuz, Nanda, and Wysocki, 2003) on financial reporting outcomes. In contrast, we investigate whether CFOs have idiosyncratic preferences – which, for expositional purposes we will refer to as a CFO's "style" – that manifest themselves in the financial reporting choices of the firms for which he/she works.[2] Specifically, we test for a "managerial fixed effect" in the financial reporting choices of the firms for which a CFO works.

One might argue that there is little doubt that individuals vary in their utility functions and/or risk preferences and that these differences shape the choices they make. The neoclassical view of the firm, in contrast, assumes managers are homogenous or perfect substitutes – in other words, faced with the same economic circumstances, including economic incentives, different managers would make the same choices. Under this view, CFOs might be recognized as the

[1] Wakabayashi, Daisuke, "CFO Brings Philosophy of Change to Microsoft," Reuters News, 2/8/2008.
[2] The source of these differences in individual preferences is somewhat beyond the scope of this paper. We provide some exploratory evidence in Section 5 on the relation between observable CFO characteristics such as age, gender, and educational background and individual CFO differences. However, there are likely other unobservable CFO characteristics – both psychology-based (such as disposition) as well as economics-based (such as personal wealth) – that impact CFOs individual preferences.

manager responsible for making the firm's financial reporting choices but these choices would not be influenced by his/her individual "style". If one did accept that managers have different styles it is still possible that these differences would not impact a firm's financial reporting choices because economic circumstances dictate these choices and CFOs have limited ability to affect these decisions. It is perhaps for these reasons that the existing literature in accounting generally does not seem to consider the possibility that CFOs might have individual styles that influence their financial reporting choices. Yet firm-level factors are able to explain only a small fraction of the cross-sectional variation in many financial reporting variables.[3]

Moreover, the opening quote in the paper introduces the possibility that CFOs may have individual styles that impact their financial reporting choices.[4] Prior research in management also recognizes the importance of top executives' characteristics (e.g., experiences, values, and personalities) in understanding corporate-level decisions – a view known as the upper echelons theory (Hambrick and Mason, 1984; Hambrick, 2007). Consistent with the theory, Bertrand and Schoar (2003) find that individual management fixed effects matter for a wide range of corporate level decisions such as investment policies, financing policies, organizational strategies and performance. Other studies have examined manager-specific factors such as CEO house size (Liu and Yermack 2007) and CEO overconfidence (Malmendier and Tate 2005) on firm performance. The findings in these studies lend support to the important role of individual CEOs in corporate behavior and performance. Since CFOs are typically in charge of financial planning,

[3] Consider, for example, the existence of a high-quality auditor as a determinant of the level of discretionary accruals as examined in Becker et al. 1994. The reported $R^2$ from their pooled regression is 1%. Warfield et al. (1995) examine the relation between managerial ownership and discretionary accruals and report an $R^2$ of 12% in an OLS regression that includes numerous control variables including leverage, growth, variance in earnings and persistence. In general, it appears that there is significant unexplained variation in many financial reporting choices.

[4] It is interesting to note that under Chris Liddell's tenure at Microsoft, the company has changed its policy of not capitalizing any software development costs to one of capitalizing a portion of these costs. In addition, the company is considering taking on debt for the first time in the company's history. Both actions are consistent with Liddell having a more "aggressive" philosophy.

budgeting, internal control, and financial reporting processes (Gore, Matsunaga, and Yeung, 2008; Kaufman, 2003) one might expect CFOs' styles to have a significant impact on the outcomes of financial reporting. However, given the numerous constraints on financial reporting (e.g., requirements under GAAP, external auditors, the SEC, etc.), it is an empirical question whether CFOs styles manifest themselves in firms' financial reporting decisions.

To provide comprehensive evidence of CFOs' effect on financial reporting choices, we investigate a wide range of reporting choices. We nominally categorize the financial reporting practices we examine into two groups: 1) earnings related strategies and 2) disclosure related strategies. We recognize that these two groups are not clearly delineated but we use these rough categories to facilitate our discussion. For earnings related reporting strategies, we investigate three dimensions – aggressiveness, conservatism, and earnings smoothness. For disclosure related reporting strategies, we focus on the accuracy, amount and bias in managers' earnings guidance.[5] We expect CFOs to be able to exercise discretion over these various dimensions of financial reporting; therefore, their individual styles are likely reflected in these financial reporting choices.

One potential difficulty in identifying the effect of individual CFOs using a firm-year panel dataset arises from the persistence of firm-specific factors (observable and unobservable) and the correlations between CFO and firm effects. For example, certain firms may have a propensity to offer equity incentives to their upper management team, which likely induce CFOs to select more aggressive financial reporting strategies (Cheng and Warfield 2005). To avoid attributing a firm-effect, such as the use of equity incentives, to the CFO, we track CFOs across different firms over time and construct a sample of 691 CFOs who have occupied the CFO

[5] Because we use data prior to Regulation FD (Reg FD), managers potentially provide earnings guidance privately to the investment community, making it difficult for us to accurately measure these disclosures. As a result, we rely on properties of analyst forecasts to infer management disclosure choices under the assumption that analyst forecasts reflect the guidance given by management via disclosures (Feng 2008).

position in at least two companies. This sample allows us to disentangle CFO-specific effects from firm-specific and time-specific effects by including individual firm and time period specific indicator variables along with CFO indicator variables.[6] We start by estimating base model regressions of our financial reporting variables as a function of firm fixed effects (to control for both observable and unobservable firm-level characteristics), year fixed effects (to control for time-specific effects), and additional control variables as necessary. We then add CFO fixed effects to the base model and find that CFO fixed effects are statistically significant in explaining our financial reporting variables. More importantly, adding CFO fixed effects to this base model increases the adjusted R-squares by 5.8 percent on average, suggesting the economic significance of CFO fixed effects. We find that CFO-specific factors play a significant role in explaining firms' discretionary accruals, off-balance sheet activities, probability of accounting manipulations, financial reporting conservatism (using numerous measures), earnings smoothness, and disclosure accuracy and bias.

One concern with the above results is whether documented CFO fixed effects are actually only a demonstration of CEO style. CFOs are subordinates of CEOs and it is possible CEOs can exert their will and influence CFOs' decisions on financial reporting. Feng, Ge, Luo, and Shevlin (2008) provide evidence consistent with CFOs succumbing to CEOs' pressure to manipulate earnings. Hambrick (2007) argues that the effect of individual managers depends on the extent of managerial discretion. Because CEOs and CFOs may change firms together, it is possible the effect we are attributing to CFOs is really a CEO effect. We therefore examine whether CFO fixed effects continue to matter for financial reporting choices after controlling for

[6] Note that in order for a CFO fixed effect to be significant in the presence of firm-fixed effects, the CFO would need to consistently select accounting policies that are above (or below) the mean on some dimension (e.g., aggressiveness) in all the firms the CFO works for. Thus, the fact that CFOs tend to work for firms with similar characteristics (e.g., firms that provide equity incentives) would not, in itself, result in a significant CFO fixed effect unless the CFO tends to choose policies that are unusually high (or low) relative to what normally occurs at the firm.

CEO fixed effects. We find that CFO fixed effects remain significant in explaining most of our financial reporting variables, and adding CFO fixed effects to the base model that already controls for CEO fixed effects increases the adjusted R-squares by 4 percent on average.

To provide further evidence that our results are due to CFO-specific factors, we perform another set of tests. We first regress each CFO's reporting choice variables in his subsequent firm (Firm 2) on the reporting variables in his previous firm (Firm 1). If the CFO's individual "style" impacts his/her financial reporting choices, his/her choices in Firm 1 will be positively related to his/her choices in Firm 2. Our results support this prediction. We then run similar tests using "placebo" data – replacing the CFO's Firm 2 data with data from that firm three years prior to the date the CFO actually joined the firm. The correlations between the financial reporting variables at the two firms are generally insignificant based on "placebo" data, indicating that changes in accounting choices of the CFO's subsequent firms do not occur prior to the CFO's arrival. Together these results provide further support for the active influence of CFOs on financial reporting.

Finally, we trace the above-documented CFO style to CFO characteristics. We investigate whether the variations in CFO style in financial reporting can be explained by the CFOs' individual observable characteristics. We explore the impact of three observable CFO characteristics – gender, age, and educational background - on financial reporting policies after controlling for time-varying firm characteristics and firm- and time-fixed effects. We find that older CFOs are generally more conservative in accounting choices and provide less earnings guidance, while CFOs with undergraduate business school backgrounds appear to be more aggressive in their accounting choices. CFOs with undergraduate business school backgrounds

as well as CFOs with MBAs from top thirty programs are both more likely to engage in strategic earnings guidance strategies (e.g., are more likely to just meet/beat analysts' forecasts).

In summary, the above results indicate that CFO-specific factors play a significant role in determining corporate financial reporting strategies. Our findings have important implications for understanding financial reporting because they indicate that a considerable portion of variation in financial reporting practices could be attributable to CFO-level managerial characteristics. We expand the literature on various determinants of earnings and disclosure quality by pointing out CFO-level managerial characteristics as a new dimension of determinants worth considering for future work in the area. Moreover, our study supports auditing practices that emphasize the importance of evaluating managers' personality traits and ethics in predicting fraud risk (e.g., Cohen, Ding, Lesage, and Stolowy, 2008). Finally, the fact that CFOs' financial reporting choices are impacted by their individual style potentially complicates firms' ability to obtain optimal reporting choices via economic incentives alone, unless firms are aware of these style differences and select CFOs accordingly based on the firm's time-varying needs.[7]

A few recent studies have investigated the role of managerial characteristics on earnings quality. For example, Francis, Huang, Rajgopal and Zang (2007) find that reputed CEOs are associated with poorer earnings quality, while Koh (2007) documents an improvement in earning quality after CEOs win 'superstar' awards. Matsunaga and Yeung (2008) find systematic differences in financial reporting practices based on whether a CEO has previously held the position of CFO. Demerjian, Lev, and McVay (2007) develop a measure of managerial ability by using frontier analysis and find a positive association between managerial ability and earnings

[7] Note that we do not attempt to distinguish between the explanation that CFOs exert their style resulting in sub-optimal reporting choices (that is, their personal style results in choices that the firm's shareholders do not desire) versus the alternative explanation that CFOs with a certain style are selected by firms to meet the firms' time-varying needs. Under both explanations, CFOs style impacts firms' corporate financial reporting choices.

quality. Finally, in concurrent work, Dyreng, Hanlon, and Maydew (2008) use a methodology similar to ours to investigate managerial fixed-effects with respect to tax avoidance behavior. Our paper complements and extends this stream of research by focusing on the impact of CFO style on financial reporting practices. Our study contributes beyond these prior studies in two ways. First, CFO style could result from a wide range of individual characteristics (observable or unobservable); hence our analysis is not limited to any single aspect of managerial characteristics (e.g., reputation). Second, we focus on the fixed effect of CFOs rather than CEOs and find that CFO style also impacts corporate decisions, over and above the impact of CEOs.[8] These results suggest CFOs play an important role in shaping certain corporate decisions and are not necessarily just carrying out the desires of their CEOs.

The remainder of the paper is organized as follows. Section 2 discusses our classifications and measures of financial reporting practices. Section 3 describes our sample construction and research design. Sections 4 and 5 present empirical results. Section 6 concludes.

## 2. Financial reporting practices – classifications and measures

This section describes the financial reporting practices we examine in this paper. CFOs are likely to influence various dimensions of financial reporting, including accounting choices that directly affect earnings, as well as voluntary disclosure decisions that likely change investors' expectations about the firm. We organize our discussion of the financial reporting practices examined in this paper along these two lines: 1) earnings-related financial reporting strategies and 2) disclosure-related financial reporting strategies.

[8] Demerjian et al. (2007) and Dyreng et al. (2008) also include CFOs listed on Execucomp in their analysis. However, because Execucomp has limited coverage of CFOs, their samples of CFOs are much smaller – 170 in the case of Demerjian et al. (2007) and 62 in the case of Dyreng et al. (2008). In contrast, we utilize two additional databases – AuditAnalytics and Management Change Database – resulting in a much larger sample of 691 CFOs.

*2.1 Earnings-related reporting strategies*

Prior research suggests that mangers are able to exercise discretion over reported earnings along various dimensions (Dechow and Schrand 2004). For example, managers might exercise their discretion to report higher earnings (aggressive reporting), lower net asset values (conservative reporting), or to smooth out volatility in earnings (earnings smoothing). Prior studies have generally examined management incentives to engage in these behaviors as a result of firm-specific factors (e.g., debt covenants, labor union negotiations, capital market pressures, etc.). However, it is possible that the tendency to use accounting discretion in a particular way (e.g., to report aggressively) is also a result of a CFOs particular "style". We therefore examine these three potential earnings related reporting strategies: aggressiveness, conservatism, and earnings smoothing.[9]

*2.1.1 Reporting aggressiveness*

"Aggressiveness" refers to a CFOs' tendency to make accounting choices that increase reported earnings and we use several measures to proxy for this construct. First, CFOs can improve reported financial performance by managing the accrual component of earnings. We therefore investigate whether the propensity to report income-increasing discretionary accruals is CFO specific (i.e., whether individual CFOs have a fixed effect on discretionary accruals). We measure discretionary accruals (*DISC_ACC*) based on the cross-sectional modified Jones model (DeFond and Jiambalvo, 1994). Table 1 provides more precise definitions of this variable and all other variables discussed below.

[9] One could argue that low values of our aggressiveness measures represent conservatism (i.e., conservatism is the opposite of aggressiveness). However, the conservatism literature has generally relied on a separate set of measures and, therefore, we treat this dimension separately from aggressiveness (although we recognize that one could argue aggressiveness is the flip side of conservatism).

CFOs can also exercise their discretion in the choice of off-balance sheet activities to window-dress reported earnings. For example, CFOs can take more operating leases instead of purchasing equipment through capital leases or loans because the accounting for operating leases allows a company to report lower expenses during the early stage of the lease life. Therefore, we analyze whether the tendency to use more operating leases vs. on-balance sheet debt is CFO specific. In order to measure the extent of operating lease activities (off-balance sheet debt) relative to on-balance sheet debt, we calculate the present value (*PVOL*) of the next five years' minimum operating lease payments using a 10 percent discount rate (Ge 2007). We then divide *PVOL* by the sum of *PVOL* and on-balance sheet long-term debt and term this *OPLEASE*.

Another off-balance sheet activity that allows discretion relates to accounting for pension obligations and plan assets for defined benefit plans. CFOs have substantial flexibility in deciding the assumptions that affect reported pension expense. For example, CFOs can assume higher expected returns on the plan assets to reduce reported pension expense (Comprix and Mueller, 2006; Picconi, 2006). We therefore investigate whether the tendency to make higher assumptions of the expected rate of return for pension assets (*PENSION_RET*) is CFO specific.[10]

Finally, to mitigate potential measurement error, we use an overall summary measure of reporting aggressiveness. The F-Score, developed by Dechow, Ge, Larson, and Sloan (2008), is intended to measure the overall likelihood of accounting manipulations through both accruals and off-balance sheet activities. Specifically, *FSCORE* is calculated as a scaled logistic probability for each firm-year based on a model of the determinants of accounting manipulations (see Table 1 for further detail). We use this as a summary measure of aggressive reporting.

[10] Following Comprix and Muller (2006), we focus on the expected rate of return as the key pension assumption because it is generally viewed as the assumption most subject to managerial discretion. We obtain similar results using the pension discount rate as our pension variable.

### *2.1.2 Reporting conservatism*

An alternative accounting reporting strategy is conservatism. In the literature, conservatism is generally referred to as the accounting practice resulting in the systematic undervaluation of the entity's net assets (equity) relative to their economic value (Watts, 2003a). Managers might choose more conservative accounting for contracting reasons (Watts, 2003a; Watts, 2003b); however, it is also possible CFOs have a bias toward making conservative accounting choices. Because any single variable measures conservatism with error, relying on a single measure can lead to erroneous inferences (Givoly, Hayn, and Natarajan 2007). We therefore employ three commonly used conservatism measures for more reliable inferences.

Our first measure of reporting conservatism is the book to market ratio (*BTM*). The book to market ratio will be lower when reporting for the book value of equity is more conservative. However, because the measure also captures market expectations of future growth, we control for past returns in explaining the book to market ratio in subsequent analyses (Roychowdhury and Watts, 2006; Beaver and Ryan, 2000).

Since accruals tend to reverse, time variant conservatism measures have drawbacks. Our second measure of conservatism is accumulated non-operating accruals (*NON_OPACC*) (Givoly and Hayn, 2000). The intuition is that negative accruals in one year do not necessarily indicate conservatism due to mean reversion, but a consistent predominance of negative accruals over a long time period does imply conservatism.

Finally, we use the differential timeliness measure developed by Basu (1997) to measure conservatism (*BASU_CONS*). This measure is based on the notion that bad news of economic events will be incorporated in reported accounting earnings more quickly than good news when accounting is more conservative (Basu, 1997).

### *2.1.3 Earnings smoothing*

The final earnings-related reporting strategy we investigate is earnings smoothing. Earnings smoothing involves both downward and upward earnings management to hide the true variance of economic performance and can be accomplished by using either real transaction management or accrual management. It is well established in the literature that managers have strong incentives to show a smooth string of earnings rather than volatile earnings (Graham, Harvey, and Rajgopal, 2005). However, managers may vary in the extent to which they believe smoothing earnings is appropriate or beneficial, resulting in CFO-specific factors impacting the degree of earnings smoothing by a firm.

Our earnings smoothing measure is based on the approach used in Lang, Raedy, and Yetman (2003) and Leuz, Nanda, and Wysocki (2003). Specifically, we measure earnings smoothness as the variance of the residuals obtained from a regression of the absolute value of changes in quarterly earnings on a set of control variables, including growth, cash flows from operations, size and industry dummies (*EARN_SMOOTH*). For ease of interpretation, we multiply this measure by -1000 so that larger values represent smoother earnings and the magnitude of the coefficients in our regressions can be more easily presented in the tables.

## *2.2 Voluntary disclosure related reporting strategies*

Voluntary disclosures are another dimension of reporting strategies over which CFOs are likely to have discretion. Prior research supports various firm-specific reasons for managers to provide greater disclosure (e.g., size, industry membership, investor sophistication). However, it is also likely that managers have predispositions toward providing more or less disclosure and/or engaging in more or less strategic behavior with respect to their disclosure policies. We focus on

voluntary disclosures related to earnings guidance because of the relative importance of such guidance in recent years (Anilowski et al. 2007).

One difficulty with examining management disclosures is that prior to Reg FD these disclosures were frequently provided to analysts through private communications (Hutton 2005) and are therefore not observable.[11] Thus, we rely on analyst forecasts to infer managers' disclosure behavior. As shown in Feng (2008), analysts incorporate information in management forecasts to update their own forecasts and therefore, analyst forecasts closely follow management forecasts of earnings. We use analyst forecasts to infer three dimensions of managers' earnings guidance disclosures – the accuracy, amount, and bias in these disclosures.

*2.2.1 Accuracy of earnings guidance*

CFOs likely have differential ability to foresee changes in their business environment (Trueman 1986) and, therefore, we predict a CFO fixed effect with respect to the accuracy of disclosures. We expect analyst forecasts to be more accurate for firms providing more accurate earnings guidance and therefore use analyst forecast errors as our measure of the accuracy of a manager's earnings guidance. Analyst forecast error (*FORECAST_ERROR*) is measured as the absolute difference between the actual EPS and the mean consensus estimate prior to the earnings announcement.

*2.2.2 Amount of earnings guidance*

CFOs also likely differ in their propensity to provide voluntary disclosures such as earnings guidance. For example, in recent years numerous firms publicly announced the decision to stop providing earnings guidance, often citing as the reason for this decision a philosophical opposition to the practice (Chen et al. 2008). When managers provide more

[11] Thus, First Call's Company Issued Guidance (CIG) database would not include guidance given privately in the pre-Reg FD era. Moreover, First Call provides incomplete coverage of management forecasts and might introduce a bias to management forecast measures (e.g., see Chuk, Matsumoto, and Miller, 2008).

specific earnings guidance, the dispersion in analyst forecasts should decrease. Thus, to the extent CFOs differ in their propensity to provide more or less specific guidance, we expect a CFO fixed effect with respect to analyst forecast dispersion. We measure analyst forecast dispersion as the standard deviation of analysts' forecasts (*FORECAST_DISP*) prior to the earnings announcement.

*2.2.3 Issuance of biased management guidance*

Prior research suggests that managers have strong incentives to meet or beat analyst forecasts because negative earnings surprises generally lead to negative stock market reactions (e.g., Skinner and Sloan, 2001). One way that managers can avoid negative earnings surprises is by issuing biased earnings guidance. Matsumoto (2002) investigates different firm-specific incentives for managers to engage in this behavior (e.g., the presence of transient institutional investors). However, it is also possible that managers themselves have predispositions toward engaging in strategic disclosure behavior such as providing downward biased earnings guidance. Therefore, we predict a CFO fixed effect with respect to this behavior.

We use three measures of downward biased earnings guidance. First, we treat a firm-quarter as "walking down analyst forecasts" if the first analyst forecast for the quarter is greater than actual EPS and the last analyst forecast is less than or equal to actual EPS for that quarter. Following Bartov et al. (2002), we require the first forecast to be made at least three days after the release of the earnings announcement for the previous quarter, the last forecast to be made at least three days prior to the release of the earnings announcement for that quarter, and these two forecasts to be made at least 20 days apart (*WALKDOWN*).

We also identify firm quarters in which the firm reports earnings that meet or beat analysts' forecasts (*MBE*) and quarters in which the firm beats expectations by a small amount (less than or

equal to three cents) (*SMBE*) and consider these events as indicative of downward biased earnings guidance.[12] We recognize that meeting or beating analysts' expectations is possibly the result of earnings-related financial reporting choices (e.g., earnings management) as much as it is a result of voluntary-disclosure related choices and results should be interpreted accordingly.

**[Table 1]**

## 3. Sample construction and research design

### *3.1 Sample construction*

One potential problem in investigating whether individual CFOs have an influence on financial reporting practices is the fact that CFOs may move between firms that have similar economic characteristics and it may be these underlying factors that are captured by the CFO fixed effect. One could possibly address this concern by including control variables that capture potentially relevant firm characteristics but this approach is problematic because not all relevant firm characteristics are readily observable. Instead, we construct a CFO-firm matched panel data set – tracking the same CFOs across different firms over time as well as including data for the same firm under different CFOs. Thus, the dataset allows us to include both CFO and firm fixed effects, thereby enabling us to disentangle the impact of the CFO from the underlying economic factors that are specific to the firm.

To construct a comprehensive CFO sample we combine three databases: Execucomp (1992 to 2006), Management Change Database (2002-2004), and AuditAnalytics (2002-2006). We first use Execucomp to track the names of the top five highest paid executives in 1,500 publicly traded U.S. firms.[13] We next obtain additional CFO data from Management Change

[12] Our results are similar if we define "small" meet/beat to be 1 or 2 cents.

[13] We use the variable "titlean" in Excecucomp to identify the CFO of the firm. The following key words are chosen: "Chief Financial Officer," "CFO," "Vice President in Finance," "VP Finance," etc.

Database and AuditAnalytics. The Management Change Database is a new database that gathers executive changes from press releases. AuditAnalytics provides information on the top executive changes from firms' 8-K.[14] Both databases provide information on CFO name, the names of the first and second firms, and the dates of CFO change.[15] However, we also need to know the years the CFO worked for each company in order to conduct our analyses. Therefore, we identify the exact years that a CFO worked with each firm by searching for CFOs' biographies using Google or firms' SEC filings. CFOs whose tenure cannot be identified are deleted from our sample.

We then combine the above datasets and limit our sample to firms with CFOs that can be traced to at least one other firm – i.e., we require CFOs to have worked as a CFO for at least two companies. In order to separate the firm fixed effects from the CFO fixed effects, however, it is also necessary to have the firms in our sample appear under more than one CFO. Thus, for those firms appearing under only one CFO, we add data for the three years prior to the starting year of the CFO at the firm. This unidentified CFO is not part of the CFO fixed effect estimation. We provide an example of this process in Appendix A. Bennett Nussbaum worked as a CFO for Kinko from 1997 to 2000 and then at Burger King from 2001 to 2003. However, our sample did not initially include firm-years for these two firms under a different CFO. Therefore, we add three filler years for both companies in order to disentangle the "Kinko effect" and "Burger King effect" from the "Nussbaum effect".

Table 2 Panel A reports the sample selection procedure. The resulting sample contains 9,878 firm-years, 1,358 firms, and 691 individual CFOs that can be traced to at least two

[14] Most of the manager changes in these datasets are in 2005 and 2006 because firms were required to file 8-Ks for chief executive changes starting in 2004. There are very few observations from 2002-2004.

[15] Management Change Database provides only ticker symbol as the company identifier. We hand-collect cusips for each company based on company name and ticker symbol.

different firms (i.e., excluding the unidentified CFOs associated with the "filler" years). For this sample of firm-years, we use COMPUSTAT, CRSP, and IBES data to construct our annual and quarterly financial reporting variables.

Table 2 Panel B presents the frequency of CFO-firm pairs based on the number of years the CFO worked with a given firm. For about 80 percent of our CFO-firm pairs, the CFO stayed in a firm for at least two years. The average tenure of stay of a CFO is 3.3 years, indicating that CFOs are given a reasonable time to have an influence on a firm's financial reporting outcomes.

Table 2 Panel C tabulates the distribution of the sample firms based on the number of distinct CFOs they each have. Of the 1,358 firms identified in our sample, 182 have at least two distinct CFOs in the sample. As discussed previously, for the 1,176 firms that have only one CFO in the CFO-matched sample, we add three filler years when the firm is under a different CFO so that we can disentangle the CFO fixed effect from the firm fixed effect.[16]

Table 2 Panel D focuses on the distribution of CFOs according to how many times they have changed their jobs. All of the 691 CFOs in our sample have assumed the CFO position in at least two companies, and 151 of them have changed their jobs more than once.

**[Table 2]**

*3.2 Descriptive Statistics*

Table 3 presents means and standard deviations for our variables of interest. We report summary statistics for the CFO-firm matched sample as well as the descriptive statistics for the COMPUSTAT universe between 1980 and 2006. It appears that firms in our CFO-firm matched sample are larger than the Compustat average in terms of total assets and market value. This is

[16] Note that in Panel B of Table 2, multiplying the number of years a CFO is in each firm by the number of CFO-firm pairs and summing the resulting amounts does not result in an amount equal to the 9,878 firm-year observations reported in Panel A. This is because of the 3,528 "filler" years for the 1,176 firms that appear only once in our sample (1,176 x 3 = 3,528).

not surprising for two reasons. First, we rely on Execucomp for the sample period of 1992-2006, and Execucomp only covers relatively large firms; second, we limit our sample to firms whose CFO moves to another firm. This procedure would lead us to larger firms because executives from larger firms are more likely to move between public firms. CFOs from smaller firms might move to a private firm or to a divisional CFO position in a large firm. The average firm in our sample also has a higher return on assets, higher market-to-book ratio, more operating leases, and somewhat higher F-scores and pension assumptions. They also tend to have lower forecast errors and smaller analyst forecast dispersion, which might be a result of their larger firm size.

**[Table 3]**

*3.3 Research design*

We utilize two basic research designs depending on whether our dependent variable of interest is measured at the CFO-firm-year (or quarter) level or only at the CFO-firm level. The financial reporting variables that can be measured at the CFO-firm-year (or quarter) level include *DISC_ACC*, *OPLEASE*, *PENSION_RET*, *FSCORE*, *BTM*, *NONOPACC*, *FORECAST_ ERROR*, and *FORECAST_DISP*. For each of these variables, we regress the variable of interest on a set of CFO indicator variables as well as a set of firm indicator variables, year indicator variables (as well as quarter indicators if applicable), and control variables if necessary:

$$FINANCIAL\ REPORTING_{it} = \alpha\ CONTROLS_{it} + TIME_t + FIRM_i + CFO_j + \varepsilon_{it} \quad (1)$$

For conciseness, we relegate the exact specification used in each of these regressions to Appendix B. In each case, we perform an F-test for the joint significance of the CFO indicator variables to test for a CFO fixed effect.

Several of our financial reporting variables require time-series data to calculate (e.g., *EARN_SMOOTH*) and are therefore computed at the CFO-firm pair level. As a result, we have

only two observations per CFO for the majority of our CFOs and estimating a CFO fixed effect in the traditional manner is not feasible. Instead, we choose an alternative approach to examine the commonality across the different firms in which a CFO works. We regress each financial reporting variable measured at the firm the CFO moves to (*FIRM 2*) on the same variable measured at the firm the CFO moves from (*FIRM 1*). For example, for our earnings smoothing variable, we run the following regression:

$$EARN\_SMOOTH^{FIRM\ 2}{}_{ij} = \alpha_0 + \alpha_1\ EARN\_SMOOTH^{FIRM\ 1}{}_{ij} + \varepsilon_{ij} \quad (2)$$

We expect $\alpha_1$ to be positive if a CFO has a style with respect to earnings smoothness; i.e., a CFO who prefers to smooth earnings (and is capable of it) will likely smooth earnings at his subsequent employer. Appendix B details the exact regressions run for each variable.

We use this approach to test *EARN_SMOOTH*, *WALKDOWN*, *MBE*, and *SMBE*.[17] We calculate the mean of *WALKDOWN*, *MBE*, and *SMBE* at *FIRM 1* and *FIRM 2* for each CFO. Because these variables are indicator variables, the mean represents the percentage of quarters the manager engaged in that behavior (i.e., "walked down" analyst forecasts or met/exceeded expectations).

The one drawback of our approach for these variables is that $\alpha_1$ might be positive due to the common factors between firm 1 and firm 2 (e.g., industry level factors). We use two approaches to alleviate this concern. First, we control for the industry fixed effect either when calculating the measure (*EARN_SMOOTH*) or directly in the second stage regression (see Appendix B). Second, we adopt placebo tests used in Bertrand and Schoar (2003), which are described in more detail in Section 4.3. Briefly, however, we replace each CFO's *FIRM 2* data

[17] *WALKDOWN*, *MBE*, and *SMBE* are all indicator variables measured at the firm-quarter level and could, theoretically, be tested in a similar manner to our first procedure (using panel data and CFO fixed effects) except using a logistic regression specification. However, we were unable to run this regression because the model would not converge with the dependent variable using a logistic regression.

with "placebo data" – data from the same firm but *three years prior to* the actual date the CFO began his tenure at *FIRM 2*. Estimating Equation (2) with this new data, $\alpha_1$ should not be significantly positive. Alternatively, if the CFO effect we observe is actually driven by the correlations between Firm 1 and Firm 2, $\alpha_1$ will be significantly positive using the placebo data.

Finally, the one exception to our two basic research designs is our test of Basu's (1997) measure of conservatism. This measure is based on the notion that conservative firms incorporate bad news into earnings in a more timely manner than good news and is based on the following regression:

$$NI_{it} = \alpha_0 + \alpha_1 DRET_{it}^{-} + \alpha_2 RET_{it} + \alpha_3 DRET_{it}^{-} * RET_{it} + FIRM_i + YEAR_t + \varepsilon_{it} \quad (3)$$

In the above equation, *NI* is Net Income per share scaled by beginning period price, *RET* is the contemporaneous 12-month returns less value-weighted market returns (month [3, 15]), and $DRET^{-}$ is an indicator variable set to 1 if RET < 0. Stock returns (RET) proxy for contemporaneous economic events. The coefficient $\alpha_2$ reflects the timeliness of economic gains being incorporated in earnings, while $(\alpha_2 + \alpha_3)$ indicates the timeliness of economic losses being incorporated in earnings. The *incremental* timeliness of economic losses versus economic gains, i.e., conservatism, is captured by $\alpha_3$. We build on this model to test for a CFO effect on the differential timeliness measure:

$$\begin{aligned} NI_{it} = {} & \alpha_0 + \alpha_1 DRET_{it}^{-} + \alpha_2 RET_{it} + \alpha_3 RET_{it}^{-} * RET_{it} + FIRM_i + YEAR_t \\ & + CFO_j + CFO_j * RET_{it}^{-} * RET_{it} + \varepsilon_{it} \end{aligned} \quad (4)$$

The coefficient on the three way interaction term $CFO * RET_t^{-} * RET$ captures whether CFOs affect the differential timeliness measure. We perform an F-test for the joint significance of $CFO_j * RET_{it}^{-} * RET_{it}$ to investigate the effect of CFOs on the differential timeliness of earnings.

## 4. Empirical results

### *4.1 Earnings-related reporting strategies*

Table 4 presents the results of estimating CFO fixed effects. For the financial reporting variables that we measure at the CFO-firm-year (or quarter) level, we report F-tests and adjusted R-squares. For the reporting variables that we measure at the CFO-firm level, we report the OLS regression results because estimating a CFO fixed effect in the traditional manner is not feasible.

Panel A of Table 4 reports the regression results for the aggressiveness and conservatism measures. For each measure, the first row reports the adjusted R-square from a base regression excluding the CFO indicator variables. The second row reports the F-statistics, the associated p-value from tests of the joint significance of the CFO fixed effects, and the adjusted R-square when the CFO indicator variables are added into the regression (Appendix B, equations 1-6). Across all the reporting aggressiveness and conservatism measures, the F-statistics suggest that CFO fixed effects are statistically significant at less than the one percent level. Thus for each measure, we are able to reject the null hypothesis that all the CFO fixed effects on reporting aggressiveness are zero. Moreover, the increases in adjusted R-square from including CFO fixed effects are economically significant in most cases.

For example, the first variable in Table 4 Panel A is discretionary accruals (*DISC_ACC*). The adjusted R-square in the base regression (regressing *DISC_ACC* on firm and year fixed effects only) is 33 percent. When we include CFO fixed effect, the adjusted R-square increases by 8 percent. The F-test also yields a significance level less than one percent, which allows us to reject the null hypothesis of no CFO fixed effect on discretionary accruals.[18]

[18] As an alternative, we also tested the CFO fixed effect on total accruals (defined as the difference between net income and cash flows from operations). We estimate a regression similar to that used for *DISC_ACC* but also include the change in sales (adjusted for accounts receivables), the level of *PPE*, and *ROA* as additional control

The next two variables measure the extent of aggressive reporting via off-balance sheet activities, such as increasing operating leases or changing pension accounting assumptions. The adjusted R-squares for the base regressions are high, ranging from 84 percent to 92 percent, likely due to the fact that firm-specific factors account for the use of operating leases and pension rate assumptions (and the base regression includes firm fixed effects). However, adding CFO fixed effects still provides an additional 3-4 percent increase in the adjusted R-squares. F-tests also reject the null hypothesis that there is no joint CFO effect in these off-balance sheet activities.

Our last variable, *FSCORE,* measures the overall likelihood of accounting manipulations. Results reported in Table 4 Panel A indicate that adjusted R-squares increase by 9 percent from the base regressions and the F-test rejects the null hypothesis of no joint CFO effect on accounting manipulations. Overall, our results provide strong evidence that there is a commonality in the aggressiveness with which earnings are reported across different firms that a CFO works for – suggesting that it is more than just the economic circumstances of the firm that determine financial reporting aggressiveness.

Regarding accounting conservatism, F-tests for all three measures reject the null hypothesis that jointly CFOs do not have effects on conservatism. The marginal increases in adjusted R-squares are 7 percent for the book-to-market ratio (*BTM*), 1 percent for accumulated non-operating accruals (*NON_OPACC*), and 14 percent for the Basu measure (*BASU_CONS*).

The final earnings-related reporting strategy we investigate is earnings smoothing. Using the methodology described in Section 3.2 (equation 2), we estimate whether the degree of earnings smoothing at one firm that a CFO works determines the level of earnings smoothing at the next firm he/she works. As reported in Panel B of Table 4, the coefficient on

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variables. We obtain similar results – the joint F-test is significant at $p < 0.001$ and the R-square increases from 40% to 49% with the inclusion of the CFO indicator variables.

*EARN_SMOOTH AT FIRM 1* is significantly positive at a p-value less than 1 percent, consistent with the hypothesis that individual CFOs vary in the degree to which they adopt an earnings smoothing strategy. The estimated coefficient is also economically significant. For example, a one percent increase in *EARN_SMOOTH* for a CFO at his first job is associated with 0.27 percent increase in *EARN_SMOOTH* at his second job.

Taken together, the above results suggest that the overall effects of individual CFOs on earnings-related reporting strategies are both economically and statistically significant. CFO fixed effects seem to matter for reporting aggressiveness, conservatism, and earnings smoothing.

**[Table 4]**

*4.2 Disclosure-related reporting strategies*

Table 5 reports the results of our disclosure-related financial reporting variables. Panel A reports the results for *FORECAST_ERROR* and *FORECAST_DISP*, both of which are measured at the CFO-firm-quarter level and are therefore tested using our panel data and including CFO indicator variables (equations 7 and 8 in Appendix B). Again, we first present base regression results, then the estimation results with CFO fixed effects. Adding CFO fixed effects increases adjusted R-square by 3 percent for *FORECAST_ERROR* and 4 percent for *FORECAST_DISP*. F-tests are significant at less than 1 percent level. These results suggest a CFO-specific factor associated with analyst forecast accuracy and forecast dispersion – consistent with our conjecture that managers differ in 1) their ability to provide accurate guidance on earnings and 2) their propensity to provide more detailed guidance (and thereby reduce analyst dispersion).

We next examine whether CFO fixed effects help explain bias in voluntary disclosures. Our three bias variables – *WALKDOWN*, *MBE*, and *SMBE* – are averaged across quarters for a given CFO-firm and therefore, we test these variables using our alternative method, regressing

the value of these variables at the CFO's current firm on the value of these variables at the CFO's prior firm. Table 5 Panel B reports the results for these three variables. The coefficient estimate on $WALKDOWN^{FIRM1}$ is significantly positive with a p-value of 0.01, suggesting that if CFOs walk down analyst expectations in their first jobs, they also tend to walk down analyst forecasts during their second jobs. The same is true for meeting/beating analyst forecast – CFOs who frequently meet/beat analyst forecasts (as well as meet/beat by a small amount) are more likely to do so in subsequent jobs.

Similar to our results for earnings-related financial reporting variables, we find evidence consistent with the conjecture that manager-specific factors impact firms' earnings guidance decisions. Thus, it is likely more than just firm-specific economic incentives that drive disclosure decisions – a manager's individual style likely also plays a role. In the next sub-section we attempt to address potential alternative explanations.

**[Table 5]**

*4.3 Robustness of results*

We employ four additional tests to examine the robustness of our results. First, even though CFOs have a direct influence on financial reporting, they are subordinates to CEOs, and CEOs might affect companies' financial reporting by putting pressure on CFOs (Feng et al., 2008). Whether CFOs have a style of their own depends on how much discretion they have over financial reporting. If they just do what they are told to do (zero discretion under CEOs), then CFO fixed effects would be explained away by CEO effects. We therefore conduct additional analyses controlling for CEO fixed effects. We first collect CEO names from Excucomp; when CEO names are not available, we hand-collect CEO names for all of our sample firm-years, including filler years, from proxy statements or other resources (using Google).

For our financial reporting variables that are measured at the CFO-firm-year (or quarter) level, we first estimate the following base equation as a benchmark:

$$FINANCIAL\ REPORTING_{it} = \alpha\ CONTROLS_{it} + TIME_t + FIRM_i + CEO_k + \varepsilon_{it} \quad (5)$$

where $CEO_k$ stands for the CEO fixed effects. We then add the CFO fixed effect:

$$FINANCIAL\ REPORTING_{it} = \alpha\ CONTROLS_{it} + TIME_t + FIRM_i + CEO_k + CFO_j + \varepsilon_{it} \quad (6)$$

We investigate whether CFOs impact financial report decisions after controlling for the impact of the CEO fixed effect by testing the joint significance of the CFO indicator variables in equation 6.[19] We conduct this sensitivity analysis only for those variables measured at the CFO-firm-year level; however, for those financial reporting measured at the CFO-firm level (e.g., earnings smoothness), our prior analysis excludes those observations with the same CEOs during the time period of the CFO's first job and second job. Thus, our prior results for these variables are not subject to the concern that the correlation is due to a CEO effect rather than a CFO effect.

The results of equations 5 and 6 are reported in Table 6. Across all the earnings-related and disclosure-related reporting strategies, CFO fixed effects continue to be jointly significant, except for *BASU_CONS*. The increases in adjusted R-squares using Equation (6) as benchmark have an average of 4 percent. The F-tests for the CEO fixed effects also reject the null hypothesis that the joint effects of CEOs are zero.

**[Table 6]**

[19] Note that we will not be able to estimate some individual CEO fixed effects when there is only one firm-CEO match because the CEO impact will not be distinguishable from the firm effect. However, because we are not particularly interested in whether the CEO fixed effects are significant but rather whether the CFO fixed effects are significant after controlling for the impact of CEOs, this issue is not problematic. We always have multiple firm-CFO pairs thus we are able to estimate all CFO fixed effects, which is the focus of the paper. We also cannot estimate CEO fixed effects when there is only one CEO-CFO match in the sample. There are 26 CFOs with the same CEOs during the time period of the CFO's first job and second job. Our results remain extremely similar after removing these 26 CFOs from the sample.

Second, to address possible concern that only a small number of CFOs drive the significant results in our prior F-tests, we count the frequency of the significant CFO fixed effects for each financial reporting variable that we examine using F-tests. Figure 1 presents the results. For each variable, the first column presents the actual percentage of significant CFO fixed effects, and the second column reports the percentage of significant CFO fixed effects expected under the null hypothesis of no CFO fixed effect at the 5 percent significance level. The number of significant CFO fixed effects is reported inside each column. Across all the variables, the percentage of significant CFO fixed effects is far greater than expected under the null of no CFO fixed effects. For example, for *FSCORE*, the number of significant CFOs (11% * 553 = 59) more than doubles what is expected under the null hypothesis (5% * 553 = 28), where 553 is the total number of CFOs with sufficient data to estimate fixed effects for *FSCORE*.

Third, to further examine the robustness of our F-test results, we compare the frequency of significant CFO fixed effects against a distribution of significant CFO fixed effects based on a set of randomization tests. Specifically, we randomly assign the 691 CFOs to our sample firms and conduct the F-test as in Table 6. We repeat this process 1,000 times and compute the percentage of significant (at the 5 percent level) CFO fixed effect in each iteration. Figure 2 illustrates the distribution of these percentages for three of our variables (*FSCORE*, *BTM*, and *FORECAST_ ERROR*).[20] This distribution based on random matches between CFOs and firms provides a benchmark against which we can compare the actual percentage of significant CFO fixed effects. As shown in Figure 2A, on average, 5.5 percent of CFO fixed effects on *FSCORE* are significant at the 5 percent level among the 1,000 randomized tests, whereas the actual percentage of significant CFO fixed effects is 10.7 percent in our sample, exceeding 1,000 out of

[20] We chose the three variables to represent the aggressive reporting, conservatism, and disclosure choices. We are unable to conduct a similar analysis for earnings smoothing because this analysis is not based on F-tests of CFO fixed effects.

1,000 iterations. Similar results hold for *BTM* and *FORECAST_ERROR*. The actual percentage of significant CFO fixed effects is greater than 1,000 out of 1,000 iterations for *BTM* and 901 out of 1,000 iterations for *FORECAST_ERROR*. These tests provide further support for the overall significance of CFO fixed effects on financial reporting variables.

**[Figure 1 and 2]**

Finally, to provide further support that our results are due to the active influence of CFOs on firms' policies, we conduct "placebo tests" similar to that used in Bertrand and Schoar (2003). First, for those variables measured at the CFO-firm-year (or quarter) level, we regress the variable on year indicator variables (and quarter if applicable), industry indicator variables, and the applicable control variables discussed in Appendix B. We then compute the average residual for each CFO at his previous (*FIRM 1*) and subsequent (*FIRM 2*) employers. We then regress the CFO's average residual in *FIRM 2* on his average residual in the *FIRM 1*. We expect the coefficient estimate on the CFO's residual in his first job to be positive if CFO-specific factors influence firm's financial reporting decisions. Note, for those variables that are measured at the CFO-firm level (*EARN_SMOOTH*, *WALKDOWN*, *MBE*, and *SMBE*), we use the same specification discussed earlier (equations 9-12 in Appendix B).

The results of these regressions are then used as a benchmark and compared against results using "placebo" data. Specifically, we create a dataset using data from each CFO's *FIRM 2*, only using data from three years prior to the date the CFO actually joined the firm. Appendix A depicts this process for our example CFO, Bennett Nussbaum.[21] If the benchmark results are the result of CFO-specific factors, then $\alpha_1$ should not be significantly positive using the placebo

[21] Specifically, we create our "placebo data" by assuming CFO Bennett Nussbaum joined Burger King in 1998 rather than 2001. We run our regressions replacing the CFO's *FIRM 2* data with the placebo data. In our example, we would run Burger King's data (Nussbaum's *FIRM2*) from 1998 to 2000 on Kinko's data (Nussbaum's *FIRM1*) from 1997 to 2000.

data. Alternatively, if the significant coefficient on $\alpha_1$ in the benchmark case is due to commonalities between firm 1 and firm 2 (unrelated to the CFO) or economic changes that occur at firm 2 prior to the arrival of the new CFO, $\alpha_1$ will remain significantly positive.

Table 7 reports the results of the regressions for the real data in the first column and the placebo data in the second column. Using the real data, the association between a CFO's average residuals in *FIRM 1* are positively associated with his/her residuals in *FIRM 2* for all financial reporting variables except *BTM* and *FORECAST_ERROR*. In comparison, the second column reports the same regressions for the placebo data. We find that most of the estimated coefficients that are significant in Column (1) are insignificant in Column (2), except for *OPLEASE*, *MBE*, and *SMBE*. Even though the estimated coefficients on *OPLEASE*, *MBE*, and *SMBE* are significant, note that the coefficients estimates for these variables in Column (2) are less significant than the coefficient estimates in Column (1) in terms of both magnitude and statistical significance. These results reduce the likelihood that our results are due to economic changes at the firm that led them to change CFOs, to the extent such economic changes occur in the years leading up to the change in CFOs. Overall, our robustness tests provide further support for our conclusion that CFO-specific factors play a role in determining firms' financial reporting choices.

**[Table 7]**

## 5. CFO Characteristics

### *5.1 Predictions about CFO characteristics*

The previous analyses provide evidence suggesting that CFO-specific factors have an effect on corporate financial reporting policies. However, it is unclear what factors are associated with these individual CFO effects. What are the characteristics of individual CFOs that lead to different styles of financial reporting choices? In this section, we explore several

CFO-specific characteristics that possibly underlie the CFO-fixed effects noted in the previous analyses. We explore three observable characteristics – gender, age, and educational background.

It is well established in the existing research in sociology and psychology that women tend to be more risk averse than men (Eckel and Grossman, 2008; Fellner and Maciejovsky, 2007). Moreover, psychology literature has suggested that men are more overconfident than women, especially in finance related matters (Prince, 1993; Lundeberg, Fox, and Puncochar, 1994). Consistent with the above theory, Barber and Odean (2001) find that men trade more frequently than women and earn a lower return. Therefore, we expect that women tend to estimate higher costs to engaging earnings management than men and thus would engage in less earnings management activity (e.g., earnings smoothing). Female CFOs are also likely to be less aggressive and more conservative in financial reporting choices.

We also examine the possible role of CFO age. On one hand, research in psychology has suggested that risk aversion usually increases with age (Palsson, 1996). We therefore expect older CFOs to be more conservative in their accounting choices overall than younger CFOs. On the other hand, older CFOs are usually more experienced and probably have better skills in managing earnings and choosing disclosure policies. Hence, older CFOs could be more aggressive and strategic in their choices of financial reporting practices.[22]

Finally, with respect to educational background, we examine whether a CFO has a BBA, CPA, and/or MBA. If the CFO has an MBA, we also determine whether the degree is from an MBA program of a top thirty U.S. business school. We expect CFOs with CPA qualifications to have better knowledge of professional ethics. They are likely to perceive higher litigation risk

[22] Here we focus on the cross-sectional variation in CFO age. In our main analysis in previous sections, we implicitly assume that CFO style is persistent over time. However, this is less of a concern especially because the average length of CFO tenure is about 3 years. We still acknowledge that CFO style might change over time, which would introduce bias against our findings.

associated with earnings management than CFOs without CPA qualifications, which might result in more conservative financial reporting by CFOs with CPA qualifications. However, CPA qualifications also suggest the ability to manage earnings; i.e., it is easier for people with accounting knowledge to come up with accounting schemes to boost earnings.[23] Finally, we expect BBA or MBA education to affect CFOs' financial reporting choices. This effect could arise from business school training, alumni networking or other human capital accumulation associated with business school education.

*5.2 Empirical results on CFO characteristics*

To analyze the role of CFOs' characteristics in influencing firm's financial reporting decisions, we estimate regressions similar to equation (1), but replacing the CFO indicator variables, with a set of variables representing the CFO characteristics described previously:

$$FINANCIAL\ REPORTING_{it} = \alpha\ CONTROLS_{it} + YEAR_t + FIRM_i + \beta_1\ WOMEN_j + \beta_2 AGE_j + \beta_3 BBA_j + \beta_4 CPA_j + \beta_5 MBA_j + \beta_6 MBA\_TOP30_j + \varepsilon_{it} \quad (7)$$

*WOMEN* is an indicator variable, taking the value of one if the CFO is a woman. *AGE* is the age of the CFO. *CPA* is an indicator variable, equal to one if the CFO has CPA qualification and zero otherwise. *BBA* (*MBA*) is also an indicator variable, equal to one if the CFO has BBA (MBA) educational background. *MBA_TOP30* is equal to one if the CFO graduates from a top thirty U.S. business school and zero otherwise. Note, we are able to conduct this type of analysis for both the variables that are measured at the CFO-firm-year level as well as the CFO-firm level because we are no longer including CFO fixed effects (thus, the fact that we have only two

[23] Consistent with this view, Feng et al. (2008) find that CFOs of firms with accounting manipulations are more likely to be CPAs than those of control firms.

observations per CFO for most CFOs is no longer a constraint as there are numerous CFOs in each category, with the exception of *WOMEN* in some cases).[24]

We hand-collect data on CFO characteristics from SEC filings. When the information cannot be found in SEC filings, we use Google to search for individual information. Table 8 Panel A presents the descriptive statistics. Overall, only 6 percent of 691 CFOs in our sample are women, while the average age of CFOs is 46. CFO age ranges from 25 to 65. With respect to education, 75 percent of CFOs have an undergraduate degree in business administration or economics. 46 percent of CFOs are CPAs. Among the 55 percent of CFOs with MBA degrees, about half of them received MBA degrees from a top thirty business school.

Turning to the correlations reported in Panel B, we see that *CPA* is positively correlated with *BBA* (r = 0.25). This finding is not surprising due to the prerequisites required to sit for the CPA exam. Interestingly, *CPA* is negatively correlated with *MBA* (r = -0.25), indicating that an MBA is not necessary for CPAs to become CFOs. Finally, *AGE* is negatively correlated with both *WOMEN* and *CPA*, suggesting that women CFOs and CPA CFOs tend to be younger.

**[Table 8]**

Table 9 reports the multivariate regression results on estimating various specifications of Equation (7). We first provide some descriptive evidence by estimating Equation (7) including only one indicator variable, representing one characteristic, at a time. The results are reported in Panel A. Because many CFO characteristics are correlated with each other, we report the results of multi-characteristics regressions (Equation 7) in Table 9 Panel B. We take out the *WOMEN* indicator variable due to lack of power (34 female CFOs) and exclude the *MBA* indicator,

[24] However, for the CFO-firm level variables, *YEAR* and *FIRM* indicator variables are not included in estimating Equation (7) because each observation represents several years of data and we generally do not have multiple observations per firm. Note that filler years are not included when estimating Equation (7) because we only collect data on individual characteristics for the CFOs we track across time.

focusing instead on MBA education at a top thirty school (*MBA_TOP30*). For each reporting variable, the first (second) row presents the coefficient estimates (p-values). The coefficients that are significant at the 10% level using two-tailed t tests are highlighted in bold. We provide Panel A primarily for descriptive purposes and focus our discussion on Panel B.

Starting with Column (1) in Panel B, we find that, similar to the results in Panel A, older CFOs are associated with lower accumulated non-operating accruals and seem to incorporate bad earnings news in a more timely manner than good news, suggesting that they are more conservative. They also report less smooth earnings streams. Overall, these results suggest older CFOs are more conservative in their financial reporting choices.

Turning to Column (2), we see that CFOs with BBA backgrounds appear to be associated with a higher likelihood of accounting manipulations (*FSCORE*) – increasing this likelihood by 15 basis points. These CFOs are also less conservative in terms of differential timeliness in incorporating bad news in reported earnings. They are also associated with smoother quarterly earnings and appear to engage in more strategic earnings guidance – leading to lower forecast errors and dispersion and a greater likelihood of meeting/beating analysts' quarterly earnings forecasts. Being a BBA CFO increases the probability of meeting/beating analyst quarterly earnings forecasts by 5 percent. Overall, the results are consistent with BBA CFOs being more aggressive and more strategic in their financial reporting choices than non-BBA CFOs.

Column (3) reports the results for *CPA*. We notice that CFOs with CPA licenses tend to engage in more off-balance sheet activities. CPA CFOs use more operating leases (4 percent of total debt) than non-CPA CFOs. They also use more aggressive pension assumptions on rate of return of pension assets. On average, their assumption on rate of return for pension assets is 0.27% higher than that of other CFOs. CPA CFOs also tend to report smoother earnings and provide

more earnings guidance, resulting in lower analyst forecast dispersion. Overall, it appears that CFOs with CPAs engage in more aggressive financial reporting (using off-balance sheet earnings management) and are also strategic in reporting choices.

The last column presents the *MBA_TOP30* indicator variable. We do not find consistent earnings-related reporting strategies for CFOs with MBAs from top thirty schools. However, we find that CFOs with top thirty school MBA degrees appear to have lower analysts' forecast dispersion, and they are more likely to meet/beat analysts' quarterly earnings forecast. More interestingly, the marginal impact of a CFO having a top thirty school MBA on the likelihood of meeting or beating forecasts is the highest compared to other CFO characteristics (8 percent for *MBE* and 7 percent for *SMBE*).

Overall, the results presented in Table 9 suggest that the CFO effect on financial reporting outcomes could be partially attributed to some observable managerial characteristics. We document that older CFOs appear to be more conservative and less strategic, whereas CFOs with BBAs tend to be more aggressive and strategic in their financial reporting. Moreover, CFOs with MBAs from top thirty programs appear to be more strategic in meeting or beating analyst forecasts than those without such MBAs.

**[Table 9]**

## 6 Conclusion

This paper documents that financial reporting practices vary systematically across individual CFOs. We apply the empirical framework developed by Bertrand and Schoar (2003) to analyze the effect of individual CFOs on financial reporting strategies. We track 691 CFOs across different firms over time and investigate whether individual CFO style impacts a firm's financial reporting practices after controlling for firm, time, and CEO fixed effects.

We find that, across a wide range of earnings-related and disclosure-related reporting strategies, individual CFO style explains a significant portion of the heterogeneity in financial reporting practices. Further analysis indicates that the results are not driven by a small number of CFOs nor by changes at the firm that occur prior to the CFOs arrival. Finally, we analyze the determinants of CFO style by examining the effect of observable CFO characteristics on financial reporting practices. Specifically, we examine whether CFO gender, age, and educational background affect their styles. We find that older CFOs are generally more conservative in deciding financial reporting strategies, while CFOs with undergraduate business school backgrounds appear to be more aggressive.

These results add to our understanding of the determinants of firms' financial reporting choices. While prior studies focus primarily on particular economic circumstances and incentives facing a firm, our results suggest that individual factors – a CFOs individual philosophy or "style" – also influences a firm's financial reporting choices in an economically meaningful way. These results suggest that CFO turnover can have significant implications for firms' financial reporting strategies following such management changes. Moreover, the fact that CFO's individual style impacts a firm's financial reporting policies suggests that a firm's optimal financial reporting strategy could be subverted by an individual CFO's style unless the CFO's style is aligned with the firm's preferences.

Given our findings of the importance of individual factors on firms' financial reporting choices, future studies can further explore the underlying reasons for CFOs particular philosophy or style. While we examine certain observable characteristic – gender, age and educational background – it is possible that other factors such as social networks or religious beliefs also impact a CFO's individual style.

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**Appendix A: Example of Sample Construction**

**Example: Sample used in F-tests**

| 1994 | 1995 | 1996 | 1997 | 1998 | 1999 | 2000 | 2001 | 2002 | 2003 |
|---|---|---|---|---|---|---|---|---|---|
| | | | CFO: Bennett Nussbaum | | | | | | |
| **Filler Year** | **Filler Year** | **Filler Year** | **Kinko** | **Kinko** | **Kinko** | **Kinko** | | | |
| | | | | **Filler Year** | **Filler Year** | **Filler Year** | **Burger King** | **Burger King** | **Burger King** |

**Procedure of sample construction:**

Step 1: Combine data from ExecuComp, Management Change Database, and Audit Analytics.

Step 2: Track CFOs across time and construct a sample of 691 CFOs who have occupied the CFO position in at least two companies. In the above example, CFO Bennett Nussbaum worked for Kinko from 1997 to 2000 and Burger King from 2001 to 2003.

Step 3: We add three filler years when the firm is under a different CFO so that we can disentangle the CFO fixed effect from the firm fixed effect

**Example: Sample used in the analysis of real data versus placebo data (Table 7)**

| 1996 | 1997 | 1998 | 1999 | 2000 | 2001 | 2002 | 2003 |
|---|---|---|---|---|---|---|---|
| | CFO: Bennett Nussbaum | | | | | | |
| **Real data** | **Kinko** | **Kinko** | **Kinko** | **Kinko** | | | |
| **Real data** | | | | | **Burger King** | **Burger King** | **Burger King** |
| **Placebo data** | | **Burger King** | **Burger King** | **Burger King** | | | |

We create a "placebo" dataset by using real data for each CFO's *FIRM1* and only using data from *three years prior to the date* the CFO actually joined the firm for each CFO's *FIRM2*.

**Appendix B: Regression Specifications and Variable Definitions**

***CFO-firm-year (or quarter) level variables***

The model specifications used to test our financial reporting variables that are measured at the CFO-firm-year (or quarter) level are presented below. In each case, *FIRM, YEAR,* QTR, and *CFO* represent firm, year, quarter and CFO indicator variables. All dependent variables and control variables are defined in Table 1, except as listed below. Our main tests are based on an F-test for the joint significance of the CFO indicator variables.

Aggressiveness measures:

$$DISC\_ACC_{it} = \alpha_0 + FIRM + YEAR + CFO + \varepsilon_{it} \quad (1)$$

$$OPLEASE_{it} = \alpha_0 + \alpha_1 ROA_{it} + \alpha_2 SIZE_{it} + \alpha_3 BTM_{it} + \alpha_4 LEVERAGE_{it} + FIRM + YEAR + CFO + \varepsilon_{it} \quad (2)$$

$$PENSION\_RET_{it} = \alpha_0 + \alpha_1 SIZE_{it} + \alpha_2 BTM_{it} + \alpha_3 LEVERAGE_{it} + \alpha_4 ROA_{it} + FIRM + YEAR + CFO + \varepsilon_{it} \quad (3)$$

$$FSCORE_{it} = \alpha_0 + \alpha_1 SIZE_{it} + \alpha_2 BTM_{it} + \alpha_3 LEVERAGE_{it} + FIRM + YEAR + CFO + \varepsilon_{it} \quad (4)$$

Conservatism measures:

$$BTM_{t,i} = \sum_{j=0}^{6} \beta_j R_{t-j,i} + FIRM + YEAR + CFO + \varepsilon_{t,i} \quad (5)$$

$R_t$ is the percentage market returns on common equity over the fiscal year adjusted for stock distribution (month [3, 15]).

$$NON\_OPACC_{i,t} = \alpha_0 + FIRM + YEAR + CFO + \varepsilon_{t,i} \quad (6)$$

Disclosure accuracy and amount:

$$FORECAST\_ERROR_{it} = \alpha_0 + \alpha_1 GROWTH_{it} + \alpha_2 CF_{it} + \alpha_3 SIZE_{it} + FIRM + YEAR + QTR + CFO + \epsilon_{it} \quad (7)$$

$$FORECAST\_DISP_{it} = \alpha_0 + \alpha_1 GROWTH_{it} + \alpha_2 CF_{it} + \alpha_3 SIZE_{it} + FIRM + YEAR + QTR + CFO + \epsilon_{it} \quad (8)$$

***CFO-firm level variables***

The model specifications used to test our financial reporting variables that are measured at the CFO-firm level are presented below. For each CFO, we measure the variable at the firm level for both the firm the CFO moves to (FIRM2) and the firm the CFO moved from (FIRM1). All dependent variables and control variables are defined in Table 1, except as listed below. Our main tests are based on a test of whether $\alpha_1$, the coefficient on the FIRM 1 variable, is greater than zero.

Earnings smoothness (Quarterly data)

$$EARN\_SMOOTH^{FIRM\,2} = \alpha_0 + \alpha_1 EARN\_SMOOTH^{FIRM\,1} + \varepsilon_i \quad (9)$$

Disclosure bias (Quarterly data):

$$WALKDOWN^{FIRM\,2} = \alpha_0 + \alpha_1 WALKDOWN^{FIRM\,1} + \alpha_2 MEAN\_GROWTH^{FIRM2} + \alpha_3 MEAN\_CF^{FIRM\,2} + \alpha_4 MEAN\_SIZE^{FIRM\,2} + INDUSTRY + \varepsilon_i \quad (10)$$

$WALKDOWN^{FIRM1(FIRM2)}$ is the mean of *WALKDOWN* at FIRM 1 (*FIRM 2*) – in other words, the percentage of quarters the CFO walks down analyst forecasts at each respective firm. $MEAN\_GROWTH^{FIRM2}$, $MEAN\_CF^{FIRM2}$, and $MEAN\_SIZE^{FIRM2}$ are the means of *GROWTH*, *CF*, and *SIZE* during the CFOs tenure at *FIRM 2*. *INDUSTRY* is an indicator variable indicating the industry to which *FIRM 2* belongs.

$$MBE^{FIRM\,2} = \alpha_0 + \alpha_1 MBE^{FIRM\,1} + \alpha_2 MEAN\_GROWTH^{FIRM\,2} + \alpha_3 MEAN\_CF + \alpha_4 MEAN\_SIZE + INDUSTRY + \varepsilon_i \quad (11)$$

As with *WALKDOWN*, $MBE^{FIRM1(FIRM2)}$ is the mean of *MBE* at *FIRM 1* (*FIRM 2*). Remaining variables are as defined previously.

$$SMBE^{FIRM\,2} = \alpha_0 + \alpha_1 SMBE^{FIRM\,1} + \alpha_2 MEAN\_GROWTH^{FIRM\,2} + \alpha_3 MEAN\_CF^{FIRM\,2} + \alpha_4 MEAN\_SIZE^{FIRM\,2} + INDUSTRY + \varepsilon_i \quad (12)$$

As with our previous variables, $SMBE^{FIRM1(FIRM2)}$ is the mean of *SMBE* at *FIRM 1* (*FIRM 2*) and all remaining variables are as previously defined.

## Table 1
## Variable definitions

| *Variable* | *Definition* |
|---|---|
| **Earnings-related measures** | |
| ***(1)Aggressiveness*** | |
| *DISC_ACC* | Residuals from the following pooled regression:<br>$\frac{TA_{i,t}}{ASSET_{i,t-1}} = \alpha_0 + \alpha_1 \frac{1}{ASSET_{i,t-1}} + \alpha_2 \frac{\Delta SALES_{i,t} - \Delta AR_{i,t}}{ASSET_{i,t-1}} + \alpha_3 \frac{PPE_{i,t}}{ASSET_{i,t-1}} + \varepsilon_{i,t}$<br>Where for firm i year t, $TA_{i,t}$ is total accruals, which equal Net Income minus Cash Flow from Operations (data18-data308); $ASSET_{i,t-1}$ is lagged Total Assets (data6); $\Delta SALES_{i,t}$ is the change in Sales (data12); $\Delta AR_{i,t}$ is the change in Accounts Receivables (data2); and $PPE_{i,t}$ is Net Property, Plant, and Equipment (data8). |
| *OPLEASE* | Operating lease deflated by the sum of long term debt and operating lease, where operating lease is defined as the present value of the next five years' minimum rent commitment under operating leases, discounted at 10%:<br>((data96/1.1+data164/1.1^2+data165/1.1^3+data166/1.1^4+data167/1.1^5)<br>/(data34+data9+(data96/1.1+data164/1.1^2+data165/1.1^3+data166/1.1^4<br>+data167/1.1^5)) |
| *PENSION_RET* | The expected rate of return for pension assets (data336) |
| *FSCORE* | The predicted value from plugging time variant firm characteristics into the following model, which uses estimated coefficients from Dechow et al. (2008):<br>$F\_SCORE_t = -7.184 + 0.702 \times RSST \text{ accruals}_t + 3.035 \times Change \text{ in receivables}_t$<br>$+2.678 * Change \text{ in inventory}_t + 0.105 \times Change \text{ in cash sales}_t$<br>$-1.124 \times Change \text{ in earnings}_t + 0.839 \times Actual \text{ issuance}_t$<br>$-0.199 \times Abnormal \text{ change in employees}_t$<br>$+0.615 \times Exis\,tance \text{ of operating leases}_t$<br>In the above model, *RSST accruals* are the change in non-cash net operating assets and *Actual issuance* is an indicator variable which is one if the firm has issued new debt or equity during the time period. An F_SCORE of 1 indicates that the firm has the same probability of manipulation as the unconditional expectation. An F_SCORE less (more) than one indicate a lower (higher) probability of manipulation than the unconditional expectation. |
| ***(2) Conservatism*** | |
| *BTM* | Book to market ratio, defined as book value of equity over market value of equity (data60/( data25*data199)) |
| *NON_OPACC* | Accumulated non-operating accruals, defined as the accumulated non-operating accruals deflated by lagged total assets for a firm from 1985 to the event year. Non-operating accruals is defined as total accruals before depreciation minus operating accruals: (data18+data14-data308)-(Δdata2+Δdata3+Δdata160-Δdata70-Δdata71) |
| *BASU_CONS* | The coefficient on the interaction term of an indicator variable for bad news and stock return where the dependent variable is net income. This coefficient measures conservatism: the extent to which bad news is reflected in net income faster than good news |

| | |
|---|---|
| ***3) Earnings-smoothness*** | |
| *EARN_SMOOTH* | The variance of the residuals for each CFO-firm from the following pooled regression (Lang et al. 2003):<br>$\left\|\Delta NI_{i,t}\right\| = \alpha_1 GROWTH_{i,t} + \alpha_2 CF_{i,t} + \alpha_3 SIZE_{i,t} + INDUSTRY + \varepsilon_{i,t}$<br>Where for firm i quarter t, $\Delta NI_{i,t}$ is the change in net income deflated by total assets ([data69-lag(data69)]/data44). $GROWTH_{i,t}$ is the percentage change in sales (data2/lag(data2)-1); $CF_{i,t}$ is the cash flows from operations divided by total assets (data108/data44); and $SIZE_{i,t}$ is calculated as the natural log of sales (data2); *INDUSTRY* is indicator variable indicating the industry to which the firm belongs. We delete CFO-firm pairs with fewer than 6 observations. For easier interpretation, we define *EARN_SMOOTH* as this variance*(-1000) |

**Disclosure-related measures**

| | |
|---|---|
| ***1) Accuracy*** | |
| *FORECAST_ERROR* | Absolute value of analyst forecast errors, defined as the absolute difference between the actual EPS for firm *j* at quarter *t* (IBES detail History Actuals, Unadjusted) and the mean consensus estimate for firm *j* at time *t* (IBES Detailed History file), deflated by lagged price, then *1000 |
| *FORECAST_DISP* | Analyst forecast dispersion, defined as the standard deviation of analyst EPS forecast for firm *j* at quarter *t* (IBES Detailed History file), deflated by lagged price, then *1000 |

| | |
|---|---|
| ***2) Bias*** | |
| *WALKDOWN* | An indicator variable which is one if managers walk down expectations in order to meet or beat analyst forecasts and zero otherwise ($EPS \leq F_{first}$ and $F_{last} \leq EPS$), where $F_{first}$ is the first forecast for the quarter made at least 3 days after the release of the earnings announcement for the previous quarter; $F_{last}$ is the last forecast for the quarter made at least three days prior to the release of the earnings announcement for the quarter. We also require the first and last forecasts to be at least 20 days apart |
| *MBE* | An indicator variable that is set to one if the firm meets/beats expectations (Meanest ≤ EPS) for the quarter, where Meanest is the last consensus forecast for the quarter |
| *SMBE* | An indicator variable that is set to one if the firm beats expectations by three cent per share or less (EPS - Meanest ≤ $0.03) for the quarter, where Meanest is the last consensus forecast for the quarter |

| | |
|---|---|
| **Control variables** | |
| *SIZE* | Log transformation of sales, defined as log(data2) for quarterly data and as log(dat12) for annual data |
| *LEVERAGE* | Leverage ratio, defined as long-term debt plus debt in current liabilities over long-term debt plus debt in current liabilities plus the book value of common equity ( (data9+data34)/ (data9+data34+data60)) for annual data |
| *CHGEARN* | Change in quarterly earnings, defined as change in quarterly net income deflated by total assets ((data69-ldata69)/data44) |
| *ROA* | Return on assets ratio, defined as EBITDA over lagged total assets (data18/lag(data6)) for annual data |
| *GROWTH* | Sales growth, defined as percentage change in total sales (data2/lag(data2)-1) for quarterly data |
| *CF* | Quarterly cash flow, defined as cash flow from operating activities deflated by total assets (data108/data44) |

| **CFO characteristics** | |
|---|---|
| *WOMEN* | An indicator variable for female CFOs |
| *AGE* | CFOs' age |
| *BBA* | An indicator variable for CFOs' education background. It is one if the CFO has a bachelor's degree in business administration or economics and zero otherwise |
| *CPA* | An indicator variable which is one when the CFO is a Certified Public Accountant and zero otherwise |
| *MBA* | An indicator variable for CFOs' education background. It is one if the CFO has a master's degree in business administration and zero otherwise |
| *MBA_TOP30* | An indicator variable for CFOs graduating from the MBA program of a top 30 U.S. business school ranked by BusinessWeek in 2008. http://www.businessweek.com/bschools/rankings/ |

**Table 2**
**Sample selection and sample description**

**Panel A: Sample Selection**

| | Number of firm-years | Number of distinct firms |
|---|---|---|
| Firm-years from Auditanalytics, Management Change data, and Execucomp data | 25,930 | 6,261 |
| Less: Firms without CFOs that worked with at least two firm | (15,882) | (4,876) |
| Total firm-years with CFOs that worked with at least two firms | 10,048 | 1,385 |
| Less: firm-years not on Compustat | (170) | (27) |
| Final CFO-firm matched sample | **9,878** | **1,358** |

**Panel B: Frequency of CFOs based on number of years in each firm**

| No. of years in each firm | N of CFO-firm pairs | Percentage (%) |
|---|---|---|
| 1 | 299 | 19.25 |
| 2 | 347 | 22.34 |
| 3 | 356 | 22.92 |
| 4 | 231 | 14.87 |
| 5 | 121 | 7.79 |
| 6 | 67 | 4.31 |
| 7 | 48 | 3.09 |
| 8 | 31 | 2.00 |
| 9 | 14 | 0.90 |
| 10 | 13 | 0.84 |
| 11 and above | 26 | 1.66 |
| Total | 1,553 | 100 |

**Table 2 (continued)**
**Sample selection and sample description**

**Panel C: Frequency of Firms based on the number of different CFOs**

| No. of different CFOs | Freq of firms | Percentage (%) | No. of CFO-firm pairs |
|---|---|---|---|
| 1 | 1176 | 86.6 | 1176 |
| 2 | 169 | 12.44 | 338 |
| 3 | 13 | 0.96 | 39 |
| Total | **1,358** | 100 | 1553 |

**Panel D: Frequency of CFOs based on the number of changes**

| No. of changes | Freq of CFOs | Percentage (%) | No. of CFO-firm pairs |
|---|---|---|---|
| 1 | 540 | 78.15 | 1080 |
| 2 | 132 | 19.1 | 396 |
| 3 | 18 | 2.6 | 72 |
| 4 | 1 | 0.14 | 5 |
| Total | **691** | 100 | 1,553 |

Panel A presents our sample selection process. Panel B presents the frequency of the CFOs for the CFO-firm matched sample, based on how many years they worked for each firm. Panel C presents the frequency of the firms for the CFO-firm matched sample, based on how many different CFOs have worked with each firm. For the 1176 firms which only have one CFO in the CFO-firm matched sample, we add 3 filler years to disentangle the CFO effect from the firm effect. Panel D presents the frequency of CFOs for the CFO-firm matched sample, based on how many times they change their jobs.

**Table 3**
**Descriptive statistics**

| | CFO-firm matched sample | | Compustat | |
|---|---|---|---|---|
| ***Annual data*** | Mean | St. Dev. | Mean | St. Dev. |
| *TOTAL ASSETS* | 2,184 | 4,158 | 1,868 | 6,609 |
| *MARKET VALUE* | 2,105 | 3,897 | 997 | 3,396 |
| *TOTAL SALES* | 287 | 578 | 106 | 507 |
| *RETURN ON ASSETS* | -0.02 | 0.18 | -0.05 | 0.24 |
| *LEVERAGE* | 0.31 | 0.30 | 0.30 | 0.40 |
| *DISC_ACC* | -0.01 | 0.10 | -0.00 | 0.12 |
| *OPLEASE* | 0.36 | 0.38 | 0.25 | 0.33 |
| *PENSION_RET* | 8.25 | 2.13 | 7.17 | 3.24 |
| *FSCORE* | 1.05 | 0.47 | 0.95 | 0.54 |
| *BTM* | 0.52 | 0.37 | 0.58 | 1.10 |
| *NONOPACC* | -0.12 | 0.54 | -0.12 | 0.94 |

| | CFO-firm matched sample | | IBES | |
|---|---|---|---|---|
| ***Quarterly data*** | Mean | St. Dev. | Mean | St. Dev. |
| *FORECAST_ERROR* | 3.76 | 5.78 | 4.44 | 7.11 |
| *FORECAST_DISP* | 1.96 | 2.61 | 2.05 | 2.68 |
| *WALKDOWN* | 0.10 | 0.30 | 0.09 | 0.29 |
| *MBE* | 0.57 | 0.49 | 0.31 | 0.46 |
| *SMBE* | 0.35 | 0.48 | 0.17 | 0.38 |

"CFO-firm matched sample" refers to the set of firm-year observations for firms that have at least one CFO observed in multiple firms. This sample includes observations for these firms in the years in which they have other CFOs that we do not observe in multiple firms. "Compustat" is a comparison sample of all listed firms on Compustat over the period 1980 to 2006. "IBES" is a comparison sample of all listed firms on IBES over the period 1980 to 2006. For annual data, the maximum number of observations is 9,878 and 543,543 for "CFO-firm matched sample" and "Compustat", respectively. For the quarterly data, the maximum number of observations is 158,912 and 1,967,442 for "CFO-firm matched sample" and "Compustat", respectively. Not all variables are available for each year and firm. All variables are described in Table 1. Each of the continuous variables are winsorized at 5% and 95% to mitigate outliers.

**Table 4**
**CFO Effects on Earnings-related Financial Reporting Practices**

**Panel A: Earning-related: Aggressiveness and Conservatism**

| | F-test on fixed effects for CFOs | N | Adj. $R^2$ (%) |
|---|---|---|---|
| ***Aggressiveness*** | | | |
| *DISC_ACC* | | 7,608 | 33 |
| *DISC_ACC* | 1.34 (<.001, 598) | 7,608 | 45 |
| *OPLEASE* | | 7,660 | 84 |
| *OPLEASE* | 2.59 (<.001, 664) | 7,660 | 88 |
| *PENSION_RET* | | 2,051 | 92 |
| *PENSION_RET* | 3.20 (<.001, 235) | 2,051 | 95 |
| *FSCORE* | | 6,853 | 36 |
| *FSCORE* | 1.44(<.001, 619) | 6,853 | 45 |
| ***Conservatism*** | | | |
| *BTM* | | 4,654 | 74 |
| *BTM* | 2.18 (<.001, 478) | 4,654 | 80 |
| *NONOPACC* | | 7,280 | 96 |
| *NONOPACC* | 3.66(<.001, 656) | 7,280 | 97 |
| *BASU_CONS* | | 6,904 | 55 |
| *BASU_CONS* | 1.32(<.001, 611) | 6,904 | 69 |

**Panel B: Earning-related: Earnings Smoothness**

| Dependent Variable: $EARN\_SMOOTH^{FIRM\ 2}$ | Predicted Sign | Coefficient Estimate | P-value | Adj. $R^2$ (%) |
|---|---|---|---|---|
| $EARN\_SMOOTH^{FIRM\ 1}$ | + | .27 | <.001 | 3.8 |

This table reports the test results for CFO fixed effects on earnings-related financial reporting practices. Sample is the CFO-firm matched panel data set as described in Table1. Reported in Panel A are the results from fixed effects panel regressions. For each dependent variable (as reported in column 1), the fixed effects included are row 1: firm and year fixed effects; row2: firm, year, and CFO fixed effects. Reported are the F-test for the joint significance of the CFO fixed effects (column 2). For each F-test we report the value of the F-statistic and, in parentheses, the p-value and number of constraints. Also reported are the number of observations (column 3) and adjusted $R^2$s (column 4) for each regression. Reported in Panel B are the results of a second stage regression. There are a total of 845 observations used in Panel B. The first stage is a panel regression using the firm-quarter level data, in which the absolute value of change in net income (scaled by total assets) is regressed on the control variables and industry dummies. We then use the variance of the residuals from this regression * (-1000) to measure earnings smoothness at the CFO-firm level. The second stage is a panel regression at the CFO-firm level. *EARN_SMOOTH* for the CFO's second firm is regressed on *EARN_SMOOTH* for his first firm. All variables are described in Table 1.

**Table 5**
**CFO Effects on Disclosure-related Financial Reporting Practices**

**Panel A: Accuracy of Voluntary Disclosures**

| | F-test on fixed effects for CFOs | N | Adj. $R^2$ (%) |
|---|---|---|---|
| *FORECAST_ERROR* | | 15,610 | 40 |
| *FORECAST_ERROR* | 3.08 (<.001, 476) | 15,610 | 45 |
| *FORECAST_DISP* | | 15,766 | 49 |
| *FORECAST_DISP* | 3.77 (<.001, 483) | 15,766 | 54 |

**Panel B: Bias in Voluntary Disclosures**

| Dependent Variable: *WALKDOWN (MBE, SMBE)* $^{FIRM\ 2}$ | Predicted Sign | Coefficient Estimates | P-value | Adj. $R^2$ (%) |
|---|---|---|---|---|
| *WALKDOWN* $^{FIRM\ 1}$ | + | .19 | .01 | 30.1 |
| *MBE* $^{FIRM\ 1}$ | + | .15 | <.001 | 13.2 |
| *SMBE* $^{FIRM\ 1}$ | + | .21 | <.001 | 9.1 |

This table reports the test results for CFO fixed effects on disclosure-related financial reporting practices. Sample is the CFO-firm matched panel data set as described in Table1. Reported in Panel A are the results from fixed effects panel regressions. For each dependent variable (as reported in column 1), the fixed effects included are row 1: firm, year, and quarter fixed effects; row2: firm, year, quarter, and CFO fixed effects. Reported are the F-test for the joint significance of the CFO fixed effects (column 2). For each F-test we report the value of the F-statistic and, in parentheses, the p-value and number of constraints. Also reported are the number of observations (column 3) and adjusted $R^2$s (column 4) for each regression. Reported in Panel B are the results of OLS regressions. The maximum number of observations used in Panel B is 403. For *WALKDOWN* (*MBE*, *SMBE*) $^{FIRM\ 1}$, reported are the coefficient estimates of regressing *WALKDOWN* (*MBE*, *SMBE*) $^{FIRM\ 2}$ on *WALKDOWN* (*MBE*, *SMBE*) $^{FIRM\ 1}$ and control variables, where *WALKDOWN* (*MBE*, *SMBE*) $^{FIRM\ 1}$and *WALKDOWN* (*MBE*, *SMBE*) $^{FIRM\ 2}$ are measured at the CFO-firm spell: they are the percentage of time a CFO walk down (meet/beat, just meet/beat) analysts' forecast in his first and second job, respectively.

**Table 6**
**CFO Effects on Financial Reporting Practices: Controlling for CEO Fixed Effects**

| Panel A: Earning-related: Aggressiveness and Conservatism | | | | |
|---|---|---|---|---|
| | F-test on fixed effects for | | | |
| | CEOs | CFOs | N | Adj. $R^2$ (%) |
| ***Aggressiveness*** | | | | |
| *DISC_ACC* | 1.47 (<.001, 825) | | 7,608 | 45 |
| *DISC_ACC* | 1.34 (<.001, 760) | 1.14 (0.001, 584) | 7,608 | 52 |
| *OPLEASE* | 2.31 (<.001, 806) | | 7,600 | 88 |
| *OPLEASE* | 2.35 (<.001, 738) | 2.63 (<.001, 596) | 7,600 | 91 |
| *PENSION_RET* | 4.71 (<.001, 205) | | 2,051 | 95 |
| *PENSION_RET* | 3.48 (<.001, 163) | 2.13 (<.001, 193) | 2,051 | 96 |
| *FSCORE* | 1.41(<.001, 732) | | 6,853 | 47 |
| *FSCORE* | 1.31(<.001, 665) | 1.32(<.001, 552) | 6,853 | 55 |
| ***Conservatism*** | | | | |
| *BTM* | 2.11 (<.001, 442) | | 4,654 | 79 |
| *BTM* | 1.75 (<.001, 365) | 1.84 (<.001, 401) | 4,654 | 84 |
| *NONOPACC* | 3.23(<.001, 809) | | 7,280 | 97 |
| *NONOPACC* | 2.36(<.001, 726) | 2.60(<.001, 573) | 7,280 | 98 |
| *BASU_CONS* | 1.29(<.001, 1311) | | 6,904 | 75 |
| *BASU_CONS* | 1.40(<.001, 1311) | 0.99(<.56, 341) | 6,904 | 82 |

| Panel B: Disclosure-related: Accuracy | | | | |
|---|---|---|---|---|
| | F-test on fixed effects for | | | |
| | CEOs | CFOs | N | Adj. $R^2$ (%) |
| *FORECAST_ERROR* | 3.21 (<.001, 457) | | 15,610 | 45 |
| *FORECAST_ERROR* | 2.75 (<.001, 377) | 2.63 (<.001, 396) | 15,610 | 49 |
| *FORECAST_DISP* | 3.62 (<.001, 466) | | 15,766 | 54 |
| *FORECAST_DISP* | 3.24(<.001, 381) | 3.43 (<.001, 398) | 15,766 | 58 |

This table reports the test results for CFO fixed effects on financial reporting practices after controlling for CEO fixed effects. Reported in the table are the results from fixed effects panel regressions. For each dependent variable (as reported in column 1), the fixed effects included are row 1: firm, year, and CEO fixed effects; row2: firm, year, CEO, and CFO fixed effects. Reported are the F-test for the joint significance of the CFO fixed effects (column 2). For each F-test we report the value of the F-statistic and, in parentheses, the p-value and number of constraints. Also reported are the number of observations (column 4) and adjusted $R^2$s (column 5) for each regression. All variables are described in Table 1.

**Table 7**
**CFO Effects on Financial Reporting Practices: Placebo data**

| | Real data<br>Coefficient estimate<br>(p-value)<br>[R-square] | Placebo data<br>Coefficient estimate<br>(p-value)<br>[R-square] |
|---|---|---|
| | 0.12** | 0.05 |
| *DISC_ACC* | (0.02) | (0.20) |
| | [0.01] | [-0.001] |
| | 0.09** | 0.06* |
| *OPLEASE* | (0.02) | (0.06) |
| | [0.01] | [0.00] |
| | 0.28*** | -0.10 |
| *PENSION_RET* | (0.00) | (0.38) |
| | [0.09] | [-0.00] |
| | 0.08** | 0.003 |
| *FSCORE* | (0.02) | (0.43) |
| | [0.01] | [-0.00] |
| | 0.00 | -0.01 |
| *BTM* | (0.47) | (0.44) |
| | [0.00] | [-0.01] |
| | 0.06* | 0.04 |
| *NONOPACC* | (0.10) | (0.14) |
| | [0.00] | [0.00] |
| | 0.27*** | 0.07 |
| *EARN_SMOOTH* | (0.00) | (0.20) |
| | [0.04] | [0.00] |
| | 0.001 | -0.28 |
| *FORECAST_ERROR* | (0.49) | (0.36) |
| | [-0.00] | [-0.01] |
| | 0.04** | 0.05 |
| *FORECAST_DISP* | (0.04) | (0.18) |
| | [0.01] | [-0.01] |
| | 0.19*** | 0.03 |
| *WALKDOWN* | (0.01) | (0.28) |
| | [0.30] | [0.27] |
| | 0.15*** | 0.10*** |
| *MBE* | (0.00) | (0.01) |
| | [0.13] | [0.08] |
| | 0.21*** | 0.07* |
| *SMBE* | (0.00) | (0.09) |
| | [0.09] | [0.03] |

***, **, * Significantly different from zero at the 1%, 5%, and 10% levels, respectively (one tailed t-tests). Sample is the CFO-firm matched panel data set. Each entry in this table corresponds to a different regression. We use two types of research designs. First, for each measure except *EARN_SMOOTH, WALKDOWN, MBE, and SMBE,* we regress in the first column a CFO's average residual in his second firm on his average residual in his prior firm. In column 2 we regress for each of the accounting variable an average residual based on three years prior to the CFO joining the second firm on his true average residual in his first firm. The first number in each cell is the estimated coefficient on the first job residual, the second number is the P-value (in parentheses) and the third number is the adjusted $R^2$s (in square brackets) for each regression. Second, for *EARN_SMOOTH, WALKDOWN, MBE, and SMBE,* we regress in the first column the mean of each financial reporting variable in his second firm on that from his first job. In column 2 we regress the mean of each financial reporting variable in his second firm three years prior to the CFO joining that firm on his true mean of each variable in his first firm. The first number in each cell is the estimated coefficient on the mean of each accounting variable in the first job, the second number is the P-value (in parentheses) and the third number is the adjusted $R^2$s (in square brackets) for each regression. The number of observations varies based on the variable used. The maximum number of observation is 655. Details on the definition and construction of the variables are in Table 1.

**Table 8**
**Panel A: Summary Statistics: CFO Characteristics**

| Variable | N | Mean | Median | Min | Max | Standard Deviation |
|---|---|---|---|---|---|---|
| *WOMEN* | 691 | 0.06 | 0 | 0 | 1 | 0.23 |
| *AGE* | 677 | 46 | 46 | 25 | 65 | 6.68 |
| *BBA* | 691 | 0.75 | 1 | 0 | 1 | 0.43 |
| *CPA* | 691 | 0.46 | 0 | 0 | 1 | 0.50 |
| *MBA* | 691 | 0.55 | 1 | 0 | 1 | 0.50 |
| *MBA_TOP30* | 691 | 0.27 | 0 | 0 | 1 | 0.44 |

**Panel B: Correlation between CFO Characteristics**

| | *WOMEN* | *AGE* | *BBA* | *CPA* | *MBA* | *MBA_TOP30* |
|---|---|---|---|---|---|---|
| *WOMEN* | 1 | | | | | |
| *AGE* | **-0.07** ***(0.06)*** | 1 | | | | |
| *BBA* | -0.01 *(0.81)* | 0.02 *(0.61)* | 1 | | | |
| *CPA* | 0.00 *(0.97)* | **-0.10** ***(0.01)*** | **0.25** ***(<.0001)*** | 1 | | |
| *MBA* | -0.02 *(0.58)* | **0.09** ***(0.02)*** | **0.07** ***(0.05)*** | **-0.25** ***(<.0001)*** | 1 | |
| *MBA_TOP30* | 0.05 *(0.19)* | 0.04 *(0.29)* | -0.04 *(0.30)* | **-0.19** ***(<.0001)*** | **0.54** ***(<.0001)*** | 1 |

Panel A provides summary statistics of the CFO characteristics. Sample is the set of firm-year observations for which we can obtain information on the age and educational background of the CFO. Column 2 reports number of CFOs with available information. Columns 3 through 7 report the mean, median, min, max and standard deviation of each variable, respectively. Panel B shows the Pearson correlation coefficients between CFO characteristics. Two-tailed P-values are reported in the second row. Coefficients that are significant at the 10 percent level are highlighted in bold. All variables are described in Table1 Panel B.

**Table 9**
**Panel A: CFOs' Gender, Education and Age Effects on Financial Reporting Practices**

| | **Intuition** | **N** | ***WOMAN*** Coefficient P-value (1) | ***AGE*** Coefficient P-value (2) | ***BBA*** Coefficient P-value (3) | ***CPA*** Coefficient P-value (4) | ***MBA*** Coefficient P-value (5) | ***MBA_Top30*** Coefficient P-value (6) |
|---|---|---|---|---|---|---|---|---|
| *DISC_ACC* | + | | 0.03 | -0.67 | -0.01 | 0.00 | -0.01 | -0.00 |
| | Aggressive | 4,239 | *(0.11)* | *(0.31)* | *(0.31)* | *(0.60)* | *(0.55)* | *(0.34)* |
| *OPLEASE* | + | | **0.09** | 0.45 | **0.04** | **0.04** | 0.02 | -0.01 |
| | Aggressive | 4,313 | ***(0.01)*** | *(0.70)* | ***(0.03)*** | ***(0.02)*** | *(0.30)* | *(0.41)* |
| *PENSION_RET* | + | | -0.05 | **24.43** | -0.17 | 0.07 | -0.01 | **0.10** |
| | Aggressive | 1,376 | *(0.89)* | ***(0.01)*** | *(0.23)* | *(0.59)* | *(0.92)* | ***(0.06)*** |
| *FSCORE* | + | | -0.05 | -2.30 | **0.15** | 0.06 | -0.01 | -0.01 |
| | Aggressive | 3,880 | *(0.53)* | *(0.46)* | ***(0.00)*** | *(0.12)* | *(0.84)* | *(0.54)* |
| *BTM* | + | | 0.02 | 0.00 | -0.01 | -0.01 | **-0.05** | -0.02 |
| | Aggressive | 2,632 | *(0.71)* | *(0.19)* | *(0.60)* | *(0.59)* | ***(0.09)*** | *(0.19)* |
| *NONOPACC* | + | | -0.01 | **-1.28** | **-0.02** | -0.01 | -0.00 | 0.01 |
| | Aggressive | 4,354 | *(0.62)* | ***(0.08)*** | ***(0.09)*** | *(0.36)* | *(0.88)* | *(0.14)* |
| *BASU_CONS* | + | | -0.08 | **0.004** | **-0.05** | 0.02 | **0.05** | **0.08** |
| | Conservative | 3,803 | *(0.13)* | ***(0.04)*** | ***(0.09)*** | *(0.53)* | ***(0.03)*** | ***(0.00)*** |
| *EARN_SMOOTH* | + | | **-0.27** | **-10.33** | 0.10 | 0.11 | **-0.11** | -0.07 |
| | Strategic | 1032 | ***(0.01)*** | ***(0.01)*** | *(0.11)* | *(0.03)* | ***(0.05)*** | *(0.28)* |
| *FCST_ERR* | + | | 0.08 | 4.54 | **-0.55** | -0.14 | -0.34 | -0.01 |
| | Less Strategic | 9,620 | *(0.90)* | *(0.84)* | ***(0.10)*** | *(0.65)* | *(0.27)* | *(0.95)* |
| *FCST_DISP* | + | | 0.04 | 9.34 | **-0.45** | **-0.36** | 0.02 | 0.07 |
| | Less Strategic | 9,710 | *(0.89)* | *(0.32)* | ***(0.00)*** | ***(0.00)*** | *(0.89)* | *(0.29)* |
| *WALKDOWN* | + | | 0.05 | 0.90 | 0.01 | 0.01 | 0.00 | -0.01 |
| | Strategic | 435 | *(0.11)* | *(0.37)* | *(0.49)* | *(0.56)* | *(0.86)* | *(0.58)* |
| *MBE* | + | | -0.07 | -0.39 | **0.05** | -0.00 | **0.08** | **0.08** |
| | Strategic | 785 | *(0.14)* | *(0.81)* | ***(0.02)*** | *(0.96)* | ***(0.00)*** | ***(0.00)*** |
| *SMBE* | + | | 0.00 | -1.11 | **0.05** | 0.02 | **0.06** | **0.06** |
| | Strategic | 785 | *(0.96)* | *(0.47)* | ***(0.02)*** | *(0.35)* | ***(0.00)*** | ***(0.01)*** |

This table reports the results of regressing each financial reporting variable on one CFO characteristics at a time. ***Each cell corresponds to a different regression.*** Sample is the set of firm years observations for which we could obtain information on the gender, education background, and age of the CFO. Reported are the estimated coefficients on *WOMEN, BBA, MBA, CPA, MBA_TOP30* dummies and *AGE*, respectively. Also included in each regression are year fixed effects, firm fixed effects, and other control variables. P-values (two-tailed) are in the second row of each cell. All coefficient estimates that are statistically significant at the 10% level are highlighted. The coefficient on *AGE* is the actual coefficient estimate *1000. Details on the definition and construction of the variables are available in Table 1 Panel B.

**Table 9 Continued**
**Panel B: CFOs' Gender, Education and Age Effects on Financial Reporting Practices**

| | **Intuition** | **N** | ***AGE*** Coefficient P-value (1) | ***BBA*** Coefficient P-value (2) | ***CPA*** Coefficient P-value (3) | ***MBA_Top30*** Coefficient P-value (4) |
|---|---|---|---|---|---|---|
| *DISC_ACC* | + | | -0.71 | -0.01 | -0.01 | **-0.02** |
| | Aggressive | 4,219 | *(0.29)* | *(0.62)* | *(0.26)* | ***(0.07)*** |
| *OPLEASE* | + | | 0.79 | 0.03 | **0.04** | 0.03 |
| | Aggressive | 4,295 | *(0.51)* | *(0.18)* | ***(0.02)*** | *(0.14)* |
| *PENSION_RET* | + | | **24.95** | **-0.29** | **0.27** | 0.22 |
| | Aggressive | 1,373 | ***(0.01)*** | ***(0.06)*** | ***(0.07)*** | *(0.15)* |
| *FSCORE* | + | | -1.69 | **0.15** | 0.02 | -0.05 |
| | Aggressive | 3,874 | *(0.59)* | ***(0.00)*** | *(0.71)* | *(0.26)* |
| *BTM* | + | | 0.003 | -0.00 | 0.002 | -0.00 |
| | Aggressive | 2,622 | *(0.21)* | *(0.89)* | *(0.93)* | *(0.96)* |
| *NONOPACC* | + | | **-1.54** | -0.01 | -0.001 | **0.04** |
| | Aggressive | 4,334 | ***(0.04)*** | *(0.36)* | *(0.90)* | ***(0.00)*** |
| *BASU_CONS* | + | | **0.003** | **-0.05** | 0.04 | 0.04 |
| | Conservative | 3,789 | ***(0.04)*** | ***(0.09)*** | *(0.14)* | *(0.13)* |
| *EARN_SMOOTH* | + | | **-9.18** | **0.14** | **0.12** | -0.04 |
| | Strategic | 1032 | ***(0.04)*** | ***(0.04)*** | ***(0.04)*** | *(0.51)* |
| *FCST_ERR* | + | | 2.98 | **-0.59** | 0.02 | -0.17 |
| | Less Strategic | 9,535 | *(0.90)* | ***(0.10)*** | *(0.96)* | *(0.62)* |
| *FCST_DISP* | + | | 3.50 | **-0.36** | **-0.29** | **-0.24** |
| | Less Strategic | 9,623 | *(0.71)* | ***(0.02)*** | ***(0.04)*** | ***(0.10)*** |
| *WALKDOWN* | + | | 1.05 | 0.01 | 0.01 | -0.01 |
| | Strategic | 433 | *(0.31)* | *(0.55)* | *(0.65)* | *(0.55)* |
| *MBE* | + | | -0.90 | **0.05** | -0.00 | **0.08** |
| | Strategic | 784 | *(0.58)* | ***(0.04)*** | *(0.99)* | ***(0.00)*** |
| *SMBE* | + | | -1.41 | **0.04** | 0.02 | **0.07** |
| | Strategic | 784 | *(0.37)* | ***(0.10)*** | *(0.37)* | ***(0.00)*** |

This table reports the results of regressing each financial reporting variable on CFO characteristics. ***Each row corresponds to a different regression.*** Sample is the set of firm years observations for which we could obtain information on the gender, education background, and age of the CFOs. Reported are the estimated coefficients on *WOMEN, BBA, MBA_TOP30, CPA* dummies and *AGE*. Also included in each regression are year fixed effects, firm fixed effects, and other control variables. P-values (two-tailed) are in the second row of each cell. All coefficient estimates that are statistically significant at the 10% level are highlighted. The coefficient on *AGE* is the actual coefficient estimate *1000. Details on the definition and construction of the variables reported in the table are available in Table 1 Panel B.

**Figure 1: Frequency of Significant Fixed Effects at the 5% Level**

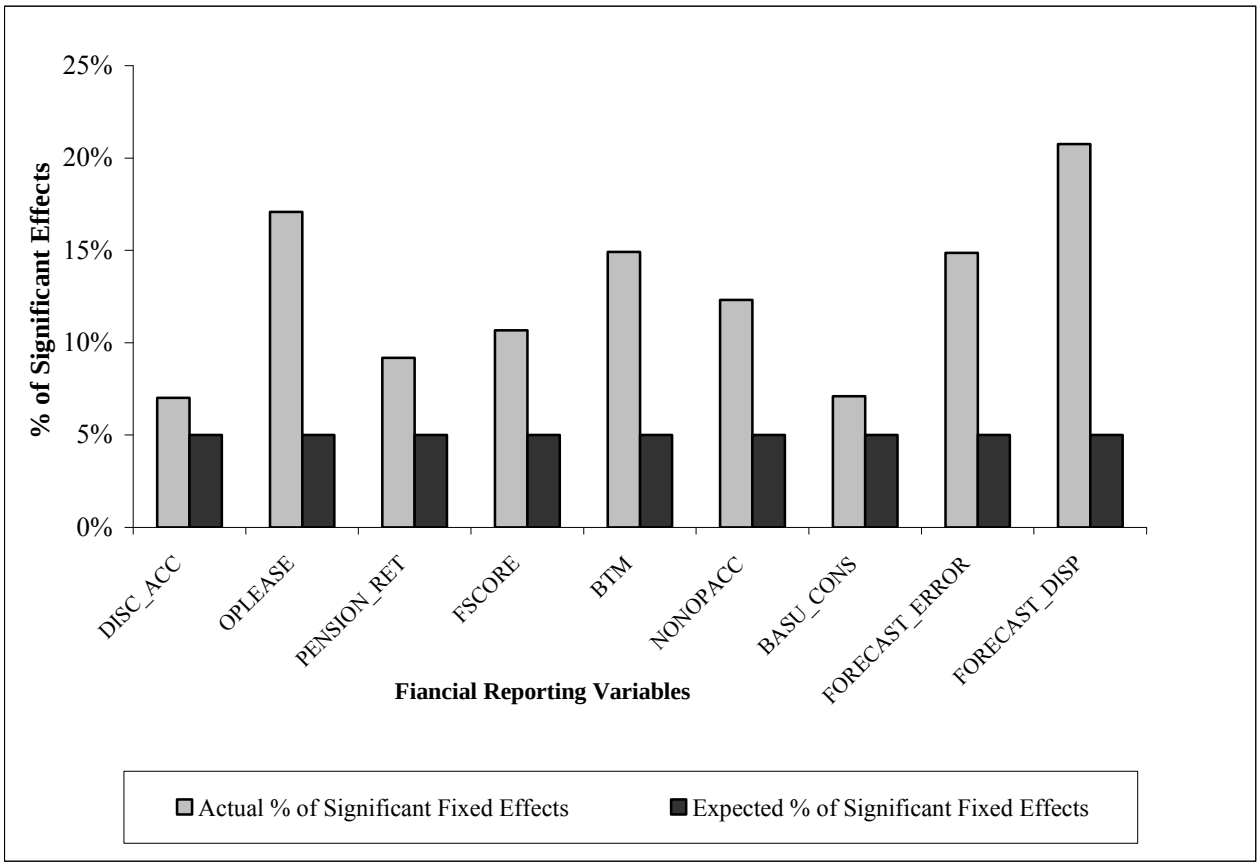
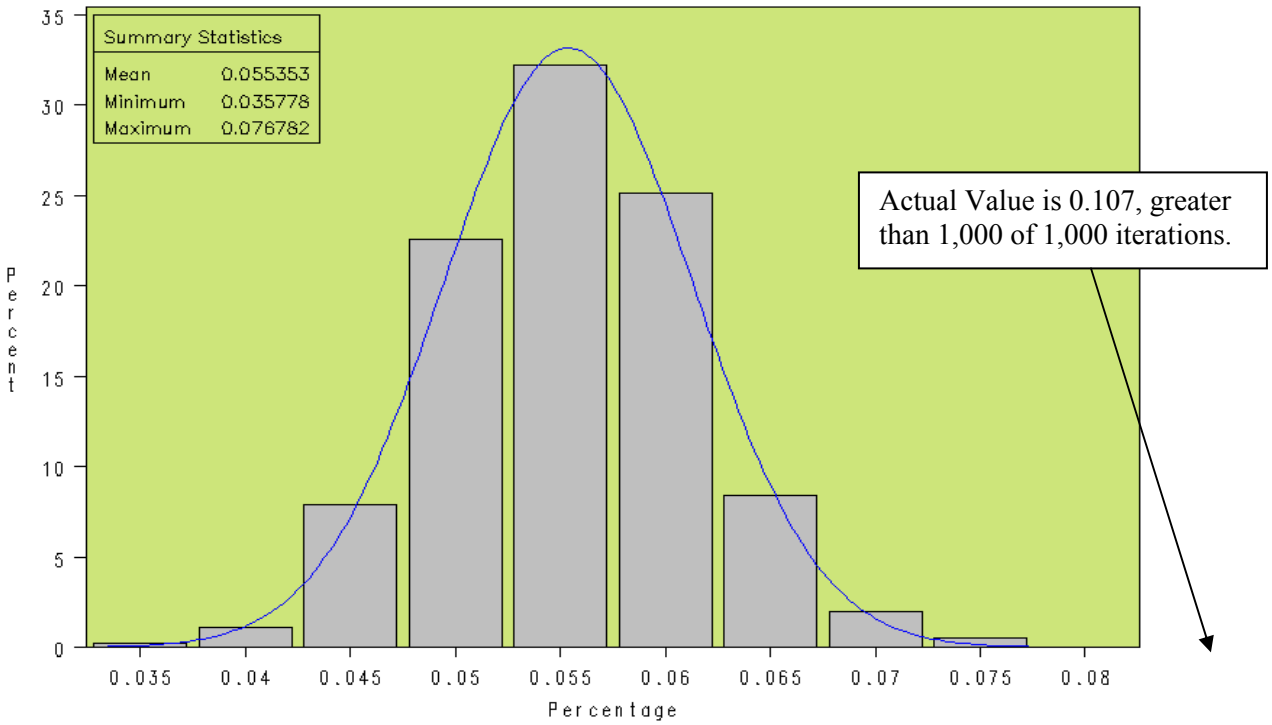



Figure 1 presents the frequency of Significant Fixed Effects. The grey column presents the actual percentage of significant CFO fixed effects for each variable. The black column presents the percentage of significant CFO fixed effects one would expect under the null hypothesis that there is no CFO fixed effect. The number of significant CFO fixed effects is reported inside each column.

**Figure 2: Simulation of Statistical Significance for F-tests in Table 6**
**Panel A: Distribution of Percentage of Significant Fixed Effects for FSCORE**




**Panel B: Distribution of Percentage of Significant Fixed Effects for BTM**

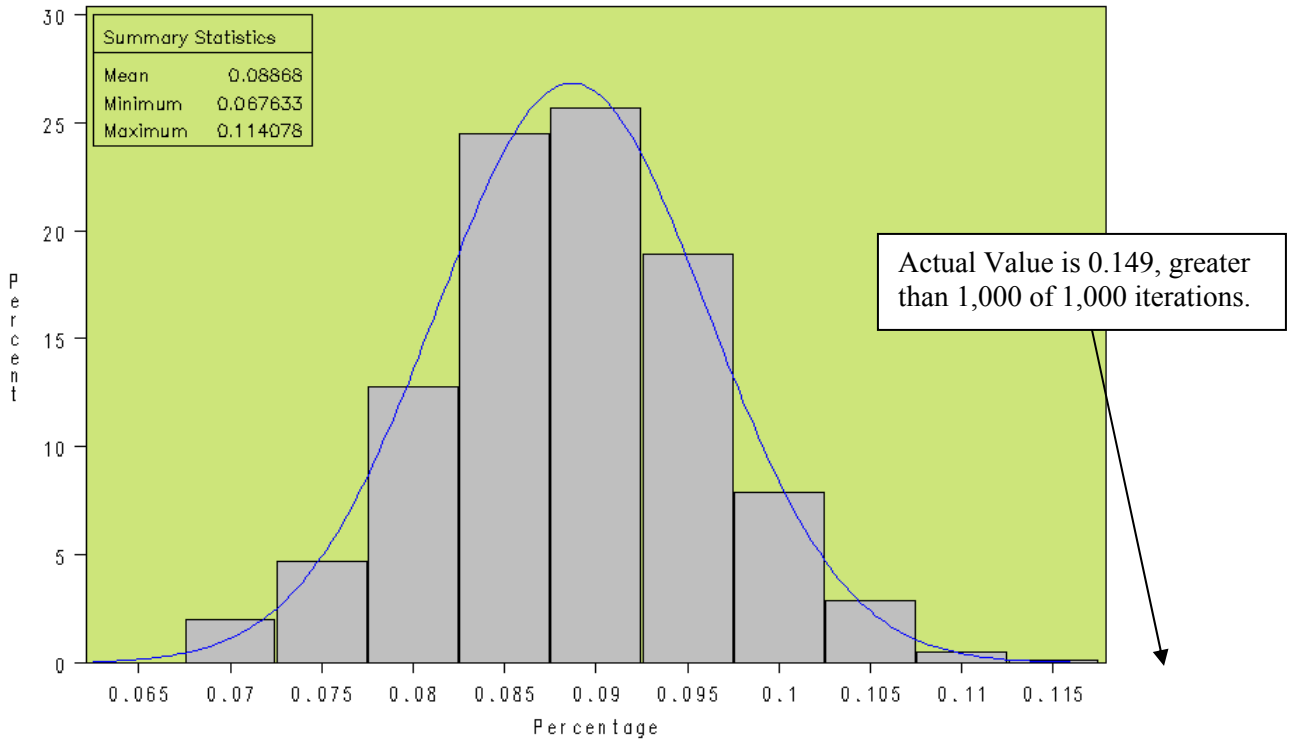


**Figure 2 (Continued)**

**Panel C: Distribution of Percentage of Significant Fixed Effects for *FORECAST_ERROR***

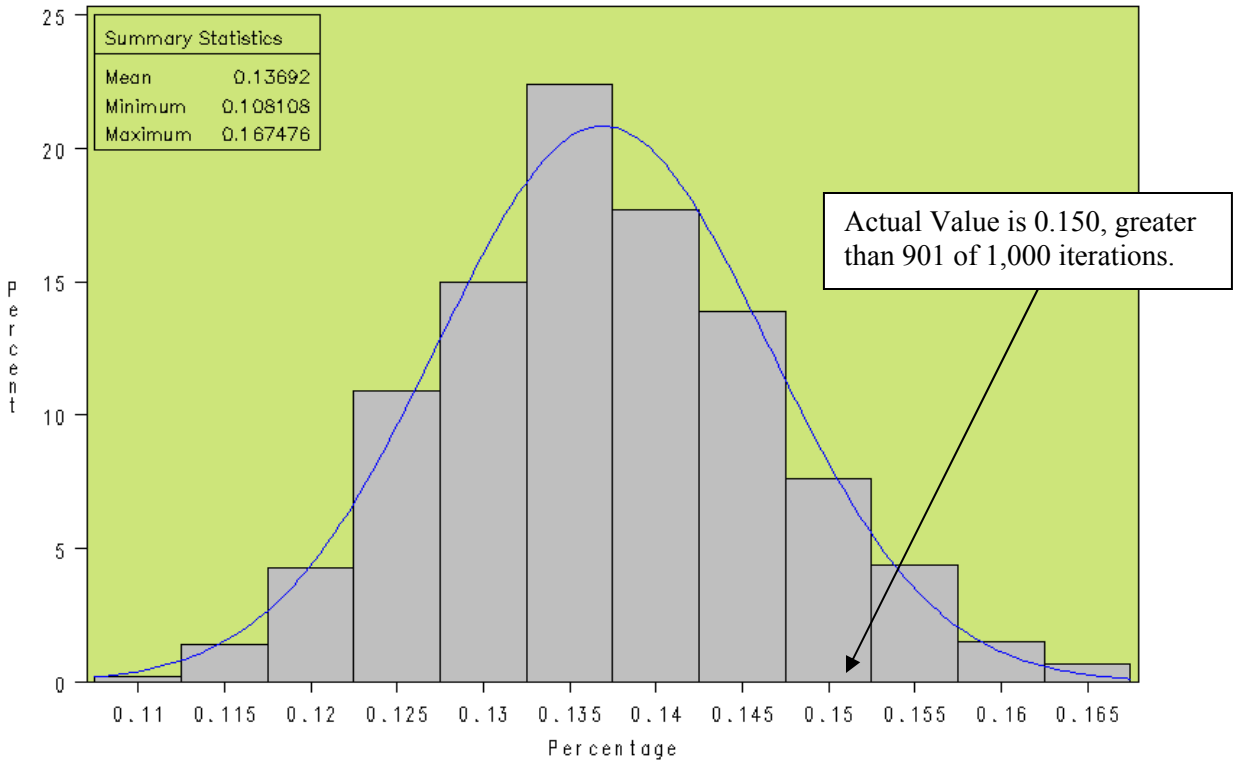



Figure 2 presents the distribution of percentage of significant CFO fixed effects for 1000 randomization tests, where we 1) randomly assign CFOs to our sample firms and conduct the F-test as in Table 6; 2) compute the percentage of significant (at the 5% level) CFO fixed effect for each iteration; and 3) repeat this random shuffling 1000 times.