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Individual managers, financial reporting and the managerial labor market

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INDIVIDUAL MANAGERS, FINANCIAL REPORTING AND THE MANAGERIAL LABOR MARKET

by

Zhejia Ling

An Abstract

Of a thesis submitted in partial fulfillment
of the requirements for the Doctor of
Philosophy degree in Business Administration
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Thesis Supervisors: Professor Douglas V. DeJong
Associate Professor Paul Hribar

ABSTRACT

This thesis comprises of three chapters. The first essay is titled 'Managers: Their Effects on Accruals and Firm Policies' and is joint work with Douglas V. DeJong. The second essay is titled 'Can the Capital Market Recognize a Manager's Financial Reporting Style?' and is sole-authored. The third essay is titled 'Executive Compensation in a Matching Model' and is joint work with Douglas V. DeJong, Elena Pastorino and B. Ravikumar.

Chapter one investigates whether top executives have significant individual-specific effects on accruals that cannot be explained by firm characteristics. Exploiting 37 years of individual executives and firm data, we find that individual executives play a significant role in determining firms' accruals. In addition, we examine whether executives' effects on accruals are related to their personal styles in investment, financing and operating decisions. Our results show that individual executives' effects on accruals are more correlated to their operating decisions than investment and financing decisions. We also compare effects exerted by CEOs to CFOs. We find CEOs are more likely to affect accruals through firm policy decisions and CFOs are more likely to affect accruals through accounting decisions. CFOs tend to report more "solid" earnings than CEOs, i.e., CFOs are more likely to push accruals to zero.

Chapter two examines whether investors can recognize idiosyncratic differences in managers' financial reporting behavior. Specifically, I investigate whether the capital market can recognize a manager's financial reporting aggressiveness and whether investors' recognition of a manager's style follows a Bayesian learning process. I use a manager's specific effect on discretionary accruals to measure her financial reporting aggressiveness. My results show that investors find earnings forecasts issued by aggressive managers to be less credible and thus respond less strongly. I also find investors follow a Bayesian learning process to identify a manager's individual style. As a manager's financial reporting history becomes longer, there is less uncertainty about the

manager's true style. Consequently, the discount on the market reaction to earnings forecast news due to the manager's aggressiveness becomes larger. In sum, these results suggest that a manager's prior financial reporting history allows her to develop a financial reporting reputation, which can be inferred by investors through rationally processing historical information.

Chapter three revisits the relative importance of returns to firm-specific tenure and to general labor market experience in the labor market for executives. We shed light on the importance of explicitly accounting for an executive's firm-to-firm and job-to-job mobility, within and across firms, over the course of the executive's career in order to measure the magnitude of each type of returns. Treating the allocation of firm value among executives and other stakeholders as a standard joint consumption problem, we prove that a measure of the implied value sharing rule, as embedded in the observed total compensation of an executive, can be recovered.

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CERTIFICATE OF APPROVAL

PH.D. THESIS

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To my family

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INTRODUCTION

How much do individual managers matter for firm behavior? A prevailing view in the financial press and among managers themselves is that top executives are crucial determinants of corporate practices and performance. Consistent with this view, the influence of managers on firm policies, performance and shareholder value has been of interest to researchers for decades, especially in the economics, finance and management literatures. In the accounting literature, however, researchers typically rely on firm-level, industry-level and market-level characteristics to explain accounting practices and little consideration has been given to the effects of individual managers until recently.

This study focuses on top executives at both the individual level and labor market level. The first two chapters examine idiosyncratic differences among individual managers in terms of accounting choices they make and how investors react. The third chapter turns to the executive labor market and examines how the labor market compensated managers for experience, skills and personal characteristics, and how these compensations affect firm value.

Chapter one investigates whether top executives have significant individual-specific effects on accruals that cannot be explained by firm characteristics. We find that individual executives play a significant role in determining firms' accruals. In addition, we examine whether executives' effects on accruals are related to their personal styles in investment, financing and operating decisions. Our results show that individual executives' effects on accruals are more correlated to their operating decisions than investment and financing decisions. We also compare effects exerted by CEOs to CFOs. We find CEOs are more likely to affect accruals through firm policy decisions and CFOs are more likely to affect accruals through accounting decisions. Interestingly, CFOs are more likely to push accruals to zero than CEOs.

Building on the first chapter and other recent studies which document the existence and significance of manager effects on financial accounting practices, chapter two extends this stream of research by examining whether investors can recognize these idiosyncratic differences in managers' financial reporting behavior. Specifically, I investigate whether the capital market can recognize a manager's financial reporting aggressiveness and whether investors' recognition of a manager's style follows a Bayesian learning process. I use a manager's specific effect on discretionary accruals to measure her financial reporting aggressiveness. My results show that investors find earnings forecasts issued by aggressive managers to be less credible and thus respond less strongly. I also find investors follow a Bayesian learning process to identify a manager's individual style. As a manager's financial reporting history becomes longer, there is less uncertainty about the manager's true style. Consequently, the discount on the market reaction to earnings forecast news due to the manager's aggressiveness becomes larger. In sum, these results suggest that a manager's prior financial reporting history allows her to develop a financial reporting reputation, which can be inferred by investors through rationally processing historical information.

Chapter three focuses on the aggregate labor market for top managers while still examining managers' idiosyncratic differences. We revisit the relative importance of returns to firm-specific tenure and to general labor market experience in the labor market for executives. We shed light on the importance of explicitly accounting for an executive's firm-to-firm and job-to-job mobility, within and across firms, over the course of the executive's career in order to measure the magnitude of each type of returns. Treating the allocation of firm value among executives and other stakeholders as a standard joint consumption problem, we prove that a measure of the implied value sharing rule, as embedded in the observed total compensation of an executive, can be recovered.

CHAPTER 1 MANAGERS: THEIR EFFECTS ON ACCRUALS AND FIRM POLICIES

1.1 Introduction

We focus on individual top managers and examine whether they exert significant effects on accruals that cannot be explained by firm characteristics. We investigate these manager effects by asking whether they are related to managers' personal styles in investment, financing and operating decisions.[1] We next compare the effects that individual CEOs and CFOs impose on accounting accruals. Finally, we investigate the career path of the CEO and CFO and the effect of career path on accruals.

A prevailing view in the financial press and among managers themselves is that a top manager is a crucial determinant of corporate practices including financial disclosure. In an influential survey paper, Graham, Harvey and Rajgopal (2005) show that managers' career concerns and external reputation are important drivers of financial reporting practices. Consistent with this view, a large body of academic research in other fields has devoted considerable effort in isolating the contribution of management to firm corporate decisions and performance.[2] Alternatively, in the standard agency model, managers have discretion within the firm but generally their behavior within the firm does not vary across individual managers. Idiosyncratic differences across managers are generally not considered. As an example in the accounting literature, the standard agency model is the

[1] Real decisions, firm policies and firm policy decisions are used interchangeably thereafter for investment, financing and operating decisions.

[2] For example, in the finance and economics literature, Rotemberg and Saloner (2000) and Van den Steen (2005) explicitly model the vision of the CEO as an important determinant of firm policy. Liu and Yermack (2007) show a negative relationship between a CEO's home purchases and firm performance, while Bennedsen, Perez-Gonzalez and Wolfenzon (2007) find that CEOs and family deaths are correlated with firm performance. In the organizational theory literature, Finkelstein and Hambrick (1996) and Chatterjee and Hambrick (2006) argue that managerial ego, biases, and experiences affect firm behavior because of the ambiguity and complexity that characterize the task of top managers.

basis for investigating managers' influence on accruals from the perspective of economic incentives, e.g., Healy (1985) focuses on bonus contracts, Warfield, Wild and Wild (1995) focus on ownership, Bergstresser and Philoppon (2006) focus on stock based compensation.

The search and matching model of the labor market addresses concerns expressed in the previous paragraph, Jovanovic (1979), Sargent (1987). Managers are heterogeneous and differ in preferences, risk aversion, and skill levels. The manager searches for a position subject to the distribution of compensation offered by different firms. In the meantime, a firm is also searching for a manager. A match occurs when the firm offers the manager a compensation contract greater than his reservation wage and the manager maximizes the firm's wealth. The contract addresses the idiosyncratic characteristics of the manager. After the search and match process, individual managers have the opportunity to significantly affect firm policies and practices. In equilibrium, individual managers are the key to implementing corporate policy.[3] Recent research in financial economics, Bertrand and Schoar (2003), provides empirical evidence that individual managers have a significant impact on the firm's investment, financing and operating decisions and firm's performance.

In accounting literature, a set of recent studies provide indirect evidence on a manager's idiosyncratic effect on accruals. For example, using the frequency of press releases as a reputation measure, Francis, Huang, Rajgopal and Zang (2008) document a negative relation between CEO press coverage and earnings quality proxied by accrual-based measures. Matsunaga and Yeung (2008) find that CEOs who are ex-CFOs utilize more income-decreasing accruals and provide more precise earnings guidance to analysts

[3] One explanation given for individual manager effects is that managers impose their idiosyncratic style after matching with the firm. Another is that firms search for a certain manager from the beginning. In equilibrium, we observe the manager with his\her own idiosyncratic style which is reflected in the contract between the manager and firm, Graham, Li and Qui (2009).

relative to CEOs without financial experience. Schrand and Zechman (2012) show that overconfident executives are more likely to exhibit optimistic bias which is reflected in the accruals during the alleged misstatement periods.

A second set of accounting researchers have identified manager specific effects on accounting characteristics and correlated these effects with personal characteristics, age, gender and education (including legal background), of the individual managers. Bamber, Jiang and Wang (2010) show that top executives exert unique and economically significant influence on their firms' voluntary disclosures, and the personal characteristics of these top executives are associated with their unique disclosure styles;[4] Dyreng, Hanlon, and Maydew (2010) show that individual executives play a significant role in determining the level of tax avoidance that firms undertake and find that executives' personal characteristics cannot explain the variation in tax avoidance across executives. In this literature, the paper closest to our work is Ge, Matsumoto and Zhang (2011) who examine the effects of CFOs across a menu of the firm's financial reporting choices including abnormal accruals. Ge et al. also examine whether CFOs' personal characteristics are correlated with their financial reporting style but find mixed results. Furthermore, they do not control for executives' effects on accruals through their real decisions and acknowledge shortcomings associated with controlling for CEOs' and other executives' effects.

In this study, we focus on accruals and consider the effects of CEOs, CFOs and other key executives on accruals. Different from Ge et al., we use managers' decisions on real operating and investment decisions to isolate manager effects on accruals through these decisions and their effects on accruals above and beyond these decisions. In addition, rather than focus on personal characteristics, we investigate whether the

[4] A related study is Yang (2011) who while focusing on managers and their guidance styles asks and confirms that the market discriminates across individual managers.

position itself and the career path of the executive explain the choices made. As a consequence, we provide additional insights into the effects that firm policies and other decisions including accounting choices have on accruals, Graham et al. (2005) and Francis, LaFond, Olsson and Schipper (2005).

Taking advantage of 37 years of data compiled for individual managers and firms, we isolate a given manager's influence on accounting accruals as the manager moves across different firms. Exploiting the design of Bertrand and Schoar (2003), we track individual top managers across different firms over time. We quantify how much of the observed variation in firms' accounting accruals can be attributed to a manager's fixed effect, controlling for observable and unobservable differences across firms.[5] Our results show that these managers exert a significant individual-specific influence over accruals and are empirically important determinants of accruals. Adding the manager fixed effects to the models of accruals that already account for observable and unobservable firm characteristics results in increases in the adjusted R^2 (e.g., from .105 to .138 for total accruals and from .049 to .064 for abnormal accruals). All the *F*-tests reject the null hypothesis of no significant joint effects of managers. The difference between a manager in the 25th versus 75th percentile shows a .049 and .037 differential effect for total and abnormal accruals. We provide evidence that the manager fixed effect captures the active influence of the manager and not for example the spurious correlation that would occur with the decision of the firm's board to undertake real decisions and accrual decisions independent of the manager.

[5] To isolate the manager effect from the firm effect (which incorporates the industry effect), we require managers to move across different firms over time. We obtain managers from the external market for managers. As documented by Murphy (1999) and emphasized by Brickley (2003), outside replacement of managers is a significant and growing portion of the managerial labor market, e.g., outside hires of S&P 500 CEOs doubled to about 20% from the 1970s to 1990s. It is important to emphasize that our interest in the external market for managers is the ability to identify the idiosyncratic style of the manager.

We next ask how a manager affects accruals. As Lafond (2008) and Graham et al. (2005) point out, there are two important channels through which managers could affect accounting accruals. Since managers are the key decision-makers presiding over a firm's investment, financing, and operating policies, Bertrand and Schoar (2003), one channel through which managers potentially affect accounting accruals is through their real decisions. For example, managers' investment decisions have implications for accruals, e.g., whether to invest and when to invest. A second channel through which managers affect accounting accruals is accounting choices. Managers' accrual estimates, choice of measurement methods, and discretion in recognizing economic transactions affect accruals.

We estimate manager fixed effects on real decisions including investment, financing and operating decisions and ask whether and how managers' real decisions are associated with manager effects on accounting accruals. We find that managers' operating decisions, R&D, SG&A and advertising, have the most impact on managers' accrual decisions. Addressing the second channel, we find that managers significantly affect accruals after controlling for their real decisions. This confirms that firm policy decisions are not the only channel through which managers affect accruals; managers affect accruals significantly through other channels which include accounting choices. In addition, the number of managers who have statistically significant effects on accruals decreases after we control for manager real decisions, which shows that some managers' effects on accruals are due to their real decisions.

With specific effects for different management positions, we are able to study separately the effects of CEOs and CFOs controlling for other executives. Previous studies which examine top manager effects on real decisions and accruals often focus on CEOs. Although CEOs are responsible for major firm policy and ultimately accounting choices, CFOs are important when it comes to financial reporting issues. There is a healthy debate in the literature about the independence of the CFO and the CEO's

influence over the CFO, e.g., Graham and Harvey (2001), Mian (2001), Fee and Hadlock (2004) and Geiger and North (2006), and the debate continues.[6] It is, therefore, important to compare the effects of CEOs and CFOs on accounting accruals. We find that CFOs have the same influence as CEOs on accruals. However, after controlling for real decisions, CFOs tend to have a larger influence on abnormal accruals than CEOs. The larger influence of CFOs suggests that real decisions are more important for CEOs than for CFOs and CFOs are more likely to affect accruals through other means, e.g., accounting choices. In addition, the magnitude of accruals, measured by the absolute value of total accruals, is smaller for CFOs than for CEOs, i.e., CFOs tend to push accruals to zero, suggesting that CFOs tend to report more "solid" earnings than CEOs. We further examine the career paths of CEOs and CFOs, whether their previous position was a CEO, CFO or other key executive. Contrary to the Matsunaga and Yeung (2008) result noted earlier, our results are not affected by the career path of the manager, either the CEO or the CFO.

1.2 Empirical methodology

To ask how much of the variance in firm's accounting accruals can be attributed to manager-specific effects, we estimate the models below, following Bertrand and Schoar (2003).

$$y_{it} = \alpha_t + \gamma_{it} + \beta X_{it} + \varepsilon_{it} \quad (1)$$

$$y_{it} = \alpha_t + \gamma_{it} + \beta X_{it} + \lambda_{CEO} + \lambda_{CFO} + \lambda_{Others} + \varepsilon_{it} \quad (2)$$

The dependent variable y_{it} stands for the accrual variable for each firm in each year. We use four accrual variables including total accruals, abnormal accruals, absolute

[6] Our focus is not fraud. However, analyzing a set of SEC enforcement actions involving material accounting manipulations, Feng, Ge, Luo and Shevlin (2010) argue that CFOs incur substantially more costs relative to CEOs for such manipulations, with the potential implication that it is CEO pressure driving CFO behavior. Focusing on general earnings management with a broader sample, the results of Jing, Petroni and Wang (2010) suggest that it is contract incentives not CEO pressure that motivates CFO behavior.

total accruals and absolute abnormal accruals, which will be discussed in more detail later. We control for year and firm fixed effects by including an indicator variable for each year (α_t) and an indicator variable for each firm (γ_i).[7] X_{it} represents a vector of time-varying firm level controls.

We select firm level controls that have been documented to be associated with accruals by existing literature. Specifically, we include leverage because prior literature suggests that firms have incentives to manage earnings to reduce the probability of violating a covenant, Bowen, Noreen and Lacey (1981), Watts and Zimmerman (1990), DeFond and Jiambalvo (1994) and Minton and Schrand (1999). We control for growth opportunities by using the book-to-market ratio because Skinner and Sloan (2002) find that growth firms that fail to meet earnings benchmarks suffer large negative price reactions on the earnings announcement date and other studies, e.g., Beaver, Kettler and Scholes (1970), show that growth firms have an incentive to smooth earnings through accruals because earnings volatility increases perceived firm risk. We use the natural logarithm of market value to control for firm size because Watts and Zimmerman (1990) argue that larger firms have incentives to exercise accounting discretion to reduce unwanted political visibility. We also include return on assets because tests related to accounting discretion that do not control for performance are often mis-specified, Kothari, Leone and Wasley (2002). For the two unsigned accruals measures, we control for the standard deviation of cash flows from operations and the standard deviation of sales to account for firm specific operating volatility because Hribar and Nichols (2007) find that the magnitude of unsigned accruals is a function of firm operating volatility.

[7] Firm fixed effects are included to account for stable firm characteristics which do not change over time. Therefore, the industry a firm belongs to has already been controlled by firm fixed effects and industry dummies are absorbed into firm fixed effects and thus cannot be included separately. However, for further assurance, we also add industry fixed effects in the models as a robustness test. No industry dummy variables are significant and we find similar results of significant manager fixed effects.

λ_{CEO}, λ_{CFO} and λ_{Others} are our main variables of interest, representing the incremental fixed effects of individual managers on accrual variables. λ_{CEO} are fixed effects for the group of managers who are CEOs in the last position we observe them, λ_{CFO} are fixed effects for the group of managers who are CFOs in the last position we observe them, and λ_{Others} are fixed effects for the group of managers who are neither CEOs nor CFOs in the last position we observe them. This allows us to separately study the effect of CEOs, CFOs, and other top executives on firm accounting accruals.[8] When estimating these equations, we account for serial correlation by allowing for clustering of the error term at the firm level.[9] Finally, ε_{it} is an error term.

We estimate equation (1) as the benchmark model, which includes only the firm fixed effect, year fixed effect, and time-varying firm level controls. This allows us to test the explanatory power of these year and firm-level characteristics. We add the fixed effects for managers (CEOs, CFOs and other top positions) in equation (2). The comparison of these two models allows us to examine the significance and the extent to which individual manager fixed effects play a role in explaining accounting accruals after

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[8] Here, we use the last position of the manager to identify his\her position in the sample period. However, a manager could hold a different position in prior firms. The coding of managers as CEOs and CFOs based on the last position does not affect our test for the existence of manager individual effects because each λ represents one person and has no relation with the person's position. However, it may affect comparison of different positions. Therefore, when we compare CEOs' and CFOs' effects on accruals in Section 6, we discuss and examine their career paths.

[9] Following Bertrand and Shoar (2003), we include firm fixed effects in the models and also cluster standard errors at the firm level. This is because the firm fixed effects no longer fully capture the within-cluster dependence if the firm effect decays over time. Therefore, it is necessary to have both firm fixed effects and clustered standard errors if we want to control for both permanent and temporary firm effects. Meanwhile, one can argue that clustered standard errors at the year level are necessary for the same reason. However, since the consistency of clustered standard error depends on having a sufficient number of clusters, the problem of clustered standard errors at the year level arises due to the limited number of years. Based on results in Peterson (2009), five hundred clusters are considered sufficient (also see Kezdi, 2004; and Hansen, 2007). Therefore, two-way clustered standard errors at both the firm and the year levels are not necessary. See Petersen (2009) for a more detailed discussion. Nevertheless, to assure accuracy of the standard errors, we still estimate the models with two-way clustered standard errors as a robustness test and our results do not change.

controlling for the year fixed effect, firm fixed effect and the relevant time-varying firm characteristics.

If a manager has a unique impact on a firm's accounting accruals, we will observe significant manager fixed effects explaining accruals after controlling for relevant firm-level characteristics. It is evident from equation (2) that the estimation of the manager fixed effects is not possible for managers who never leave a given firm during the sample period. If a firm has no managerial turnover during the sample period, the firm fixed effect cannot be separated from the manager fixed effect because these two effects are perfectly collinear. Therefore, separating manager fixed effects from firm fixed effects is only possible when the firm has at least one manager who switched firms. In our sample construction, we restrict our attention to the subset of firms for which at least one top manager can be observed in at least one other firm and this allows us to estimate the firm fixed effects and manager fixed effects separately. The estimate of the fixed effect for each individual manager enables us to examine not only the existence but also the magnitude of individual managers' effects on firms' accounting accruals.

1.3 Sample and data

1.3.1 Sample Construction

To estimate manager fixed effects, we construct a manager-firm matched panel data set that allows us to track the same top managers across different firms over time.

We start with the Forbes 800 files, from 1969 to 1991,[10] and Execucomp database, from 1992 to 2006. The Forbes data provide information on the CEOs of the 800 largest U. S. firms. Execucomp allows us to track the top five highest paid executives in the S&P 1500. We match each firm-year with accounting data reported in the Compustat database and acquisition data reported in the SDC database. Firms from the

[10] We thank Kevin J. Murphy and Forbes for generously providing us with their data.

financial service industry (SIC code 6000-6999) and utility industry (SIC code 4900-4949) are excluded from the analysis.

We then track managers in our data. We require that an individual manager has to switch firms once in our sample period. We also impose the requirement that the manager has to be in each firm for at least three years. This three-year requirement ensures that managers have enough time to "make their mark" on a given company. For each firm satisfying these requirements, we keep years where this firm has other managers as well. The resulting sample contains 954 firms and 811 individual managers who can be followed in at least two different firms. The average length of stay of a manager in a given firm is a little over 6 years and the average number of different firms for each manager is 2.06.[11]

To identity a manager's position in a firm-year, we use Execucomp's data items *ceoann* and *titleann*. We classify a manager as CEO if the data item *ceoann* is flagged. Note that *ceoann* is only flagged when the manager served as CEO for all or most of the indicated fiscal year. Therefore, a CEO may not be identified as CEO by *ceoann* in his/her last year. It is also not uncommon for a CEO to become "president", "chairman" etc. in his/her last year in a specific firm. For these managers, their effects on their firms during their tenure are mostly due to the influence they exerted when they served as CEOs. So we then manually check these CEOs' title for each firm-year. If the manager was a firm's CEO except for the very last year (i.e., not flagged by *ceoann*), we still classify him/her as a CEO in this last year.[12] We classify a manager as CFO if his/her title included the words "CFO," "Chief Finance Officer", "Chief Financial Officer," or "Chief

[11] Only 32 managers are observed in strictly more than two different firms and the maximum number of firms is 4.

[12] In this very last year, there is already a new CEO who starts to impact the company. We still include this year to estimate old CEO's effect will only biased against finding results that the old CEO exerts significant effect on the firm.

of Finance." All other managers are identified as "Others." Managers who are classified as "Others" include both financial-related top managers (e.g., treasurer, controller, VP-finance) and non-financial top managers (e.g., president, COO). Since these managers can also exert important influence on firms' policy and accounting practices, it is necessary to include them in our study to examine their effects or at least control for their effects. After we identify a manager's position for each firm year, we use the last position of the manager to identify his/her position in the sample period. Among these 811 managers, we have 357 CEOs, 159 CFOs, and 295 other key executives.[13]

1.3.2 Sample Description

Table 1 presents means, medians and standard deviations for firm characteristic variables and the firm policy variables of interest.[14] Details for the definition and construction of the variables reported in the table are available in the Appendix A. All variables are winsorized at 1% tail to mitigate the outlier problem. The first three columns report descriptive statistics for our manager-firm matched sample. For comparison, we also report the same statistics for all firms, excluding financials and utilities, in Forbes files and Execucomp data over the period 1969 to 2006 (the population from which we choose our sample) in the last three columns.

The average firm in our sample has a higher level of total assets, sales and market value than the average firm in the Forbes and Execucomp data. Similar to Bertrand and

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[13] Design and data place constrains on the number of managers included in the sample. For example, the Dyreng et al. (2010) sample consists of 899 managers, but their sample included financial services and utilities firms; the Ge et al. (2010) sample is selected from three databases (Execucomp, Management Change Database and AuditAnalytics) and they only require each manager to stay in a firm for two years.

[14] Firm policy variables are from Bertrand and Schoar (2003) who show managers exert significant effects on these firm policies. We use these variables later in the paper when we investigate whether managers' individual effects on accruals are associated with the managers' effects on these corporate policies.

Schoar (2003), this tells us that the selection criteria lead us to choose firms larger than the population average. The reason being managers from larger firms are more likely to move to another firm within the Forbes 800 and S&P1500 firms, and managers from smaller firms are more likely to move to private firms or positions in large firms that are below the top five highest paid positions. Therefore, managers from smaller firms cannot be tracked in our data sources and are excluded from our sample. Our focus on larger firms may bias our results, but it is very likely to bias against finding important manager individual effects. In smaller firms, managers might have more influence because they have more personal involvement in the firm's daily activities. In fact, Finkelstein and Hambrick (1996) show that managerial discretion – and hence any manager-specific effect – declines with company size.

Besides being larger than the average firm in the population, the average firm in our sample engages in more acquisition activities and has slightly higher leverage and lower interest coverage, but is very similar to the average population firm with respect to all other characteristics.

1.3.3 Accounting Accruals

The major accrual measures we use are total accruals and abnormal accruals from the modified Jones model. We focus on these two measures when addressing manager effects on accruals in general and the implications of a manager's real decisions on accruals. When comparing the effects of CEOs and CFOs, we also examine the absolute value of these two accrual measures. Overall, we have four accrual measures in total.

We use the balance sheet approach to calculate total accruals, instead of the cash flow approach, because our sample period starts in 1969 and cash flow statement data are not available before 1988.[15] Specifically,

[15] In our robustness checks, we use the cash flow statement approach to calculate total accruals for the subsample from 1988 to 2006. Our results still hold and we discuss this in section 4.3.

$$TACC_{i,t} = \Delta CA_{i,t} - \Delta CL_{i,t} - \Delta CASH_{i,t} + \Delta STD_{i,t} - DEP_{i,t},$$

where:

$TACC_{i,t}$ = firm i's total accruals in year t;

$\Delta CA_{i,t}$ = change in firm i's current assets from year $t - 1$ to year t;

$\Delta CL_{i,t}$ = change in firm i's current liabilities from year $t - 1$ to year t;

$\Delta CASH_{i,t}$ = change in firm i's cash and cash equivalents from year $t - 1$ to year t;

$\Delta STD_{i,t}$ = change in firm i's debt included in current liabilities from year $t - 1$ to year t;

$DEP_{i,t}$ = firm i's depreciation and amortization expenses in year t;

all variables are scaled by lagged total assets.

We calculate abnormal accruals using the time series modified Jones model:[16]

$$TACC_{i,t} = \beta_0 + \beta_1(\Delta Sales_{i,t} - \Delta REC_{i,t}) + \beta_2 PPE_{i,t} + \varepsilon_{i,t},$$

where:

$\Delta Sales_{i,t}$ = change in firm i's sales from year $t - 1$ to year t;

$\Delta REC_{i,t}$ = change in firm i's accounts receivables from year $t - 1$ to year t;

$PPE_{i,t}$ = firm i's year t gross property, plant, and equipment;

all variables are scaled by lagged total assets.

We estimate this regression for each firm with at least 5 observations from Compustat. Using the estimated coefficients from the regression, we are able to obtain the predicted values of total accruals, which are considered "normal" accruals. Abnormal accruals are measured by subtracting the normal accruals from total accruals, i.e., the error terms in the above regression. We then match the estimated abnormal accruals with the firm-year observations in our firm-manager matched sample.

[16] The original Jones model is a firm specific time-series model that controls for the reversal of the firm's accruals. While there is a concern about the size of the variance when using the time-series model, we have sufficient data to run the time-series model.

Table 2 summarizes the distribution statistics of the four accrual measures and the correlation between them. As expected, the mean and the median of the signed abnormal accruals are close to zero by construction, while the means of the unsigned measures are positive.[17] In the correlation analysis, Pearson (Spearson) correlations are shown above (below) the diagonal. As expected and consistent with previous literature, the correlations between these accrual variables are high. For the signed accrual variables, the Pearson correlation between total accruals and abnormal accruals is 0.697. For the unsigned accrual variables, the Pearson correlation between total accruals and the abnormal accruals is 0.506.

1.4 Individual Manager Effects on Accruals

1.4.1 Incremental Explanatory Power of Manager Fixed Effects

Panel A of Table 3 reports the results of the estimation of equation (1) and (2) for the four accounting accrual measures.[18] For each variable, we report in the first row the fit of equation (1), which we use as the benchmark specification that includes only firm fixed effects, year fixed effects and time-varying firm controls. The next row reports the change in adjusted R^2 when we estimate equation (2), where we add the fixed effects for all three groups of managers (CEOs, CFOs and other top positions). This row also reports *F*-tests for the joint significance of the manager fixed effects, the number of managers with a significant individual effect and the total number of managers in the regression.

Overall, the findings in Panel A of Table 3 suggest that manager fixed effects matter statistically for firms' accounting accruals. Including managers' fixed effects

[17] The means of abnormal accruals are not precisely zero because we estimate abnormal accruals first and then match them with the firm-year observations in our firm-manager matched sample.

[18] The adjusted R^2 is calculated based on the within R^2 from the command xtreg, fe in Stata.

increases the adjusted R^2 of the estimated models and *F*-tests allow us to reject the null hypothesis that manager fixed effects are jointly not different from zero. We now discuss the results in more detail.

The first variable in Table 3 Panel A is total accruals. The first row of total accruals is the benchmark specification for equation (1), which includes controls for firm fixed effects, year fixed effects, leverage, B/M, logarithm of market value, ROA. The adjusted R^2 for this specification is 0.105. In the second row of total accruals, where we include all manager fixed effects, the adjusted R^2 of the model increases to 0.138. The *F*-tests are reported in cells of columns labeled CEOs, CFOs and Others to test the joint significance of the manager fixed effects and all of them are large enough ($p < 0.001$) to reject the null hypothesis of no significant joint effects of these managers. However, one possible reason for these significant F-tests is that they are influenced by only a few significant coefficients, which suggest that most managers do not have significant individual effect on total accruals. To check whether the significant F-statistics are driven by only a few significant coefficients, we count the number of managers whose specific fixed effect is significant, i.e., the t-statistic on their fixed effect coefficient is significant. Our sample has 711 managers with sufficient data to estimate the total accruals model. Under the null hypothesis that managers have no significant incremental effect, we would expect 71 executives to be significant in the regression at the 10% level. In our regression results, 451 managers in total have significant effects on total accruals, which is much greater than what we would expect under the null hypothesis. Specifically, 203 out of 321 CEOs, 93 out of 141 CFOs, and 155 out of 249 other managers have significant individual effects on total accruals. These numbers are reported under the F-statistic in each cell. This tells us that our significant F-tests are not driven by a few managers but by a majority of the managers in our sample. In sum, these results show that after controlling for the known economic determinants of total accruals as well as year and firm fixed

effects, individual manager effects play a significant incremental role in explaining total accruals.

The next variable is abnormal accruals estimated from the modified Jones model. The benchmark specification also includes controls for firm fixed effects, year fixed effects, leverage, B/M, logarithm of market value, ROA. The results indicate an increase in the adjusted R^2 when including the manager fixed effects. Specifically, the adjusted R^2 increases from 0.049 to 0.064 when we add manager fixed effects to explain abnormal accruals. All of the *F*-tests are significant ($p < 0.001$) and reject the null hypothesis of no significant joint effects of managers. In fact, in the estimation of equation (2) for abnormal accruals, 435 out of 711 manager fixed effects are individually statistically significant at the 10% level.[19]

The next two variables are absolute total accruals and absolute abnormal accruals. For each accrual measure, the benchmark specification includes controls for firm fixed effects, year fixed effects, leverage, B/M, logarithm of market value, ROA, standard deviation of cash flows from operations and the standard deviation of sales. For the absolute total accruals, the adjusted R^2 for the benchmark specification is 0.076; when we add manager fixed effects, the adjusted R^2 increases to 0.114 and 425 out of 711 manager

[19] Our results are quantitatively different from the abnormal accrual results of Ge et al. (2011). The obvious possible reason is the different way the software runs the regression. We use command xtreg, fe in Stata to run fixed effect regressions, where the adjusted R^2 is calculated based on the within R^2. So in our results, the firm fixed effects are not used to calculate the adjusted R^2. We do not know how Ge et al. (2011) ran the regressions and calculated the adjusted R^2. If they create dummy variables for all the firm fixed effects in the regression and use them to calculate the R^2, they would end up with a much higher reported adjusted R^2. Using this approach to run all the regressions, we got much higher adjusted R^2, which were much closer to the results of Ge et al. (2011). We stay with the xtreg, fe fixed effect regression, because we believe this better captures the incremental explanatory power of manager fixed effects given the known and observable related factors. A second possible reason is our use of the time series model to estimate abnormal accruals while Ge et al. (2011) used the cross-sectional model, which by design has a lower variance. In addition, there are other sample and design differences that make it very difficult to completely reconcile the differences between the samples, e.g., Ge et al.(2011) require managers to be in place for 2 years to be included in the sample while we require three years, etc.

fixed effects are statistically significant at the 10% level. For the absolute abnormal accruals, we observe an increase from 0.059 to 0.081 following the inclusion of the manager fixed effects and 431 out of 711 manager fixed effects are statistically significant. For these two measures, all of the *F*-tests are significant ($p < 0.001$) and we reject the null hypothesis of no significant joint effects of managers.

To summarize, Panel A of Table 3 shows that manager fixed effects play a statistically significant role in explaining accounting accruals. Adding manager effects to the model increases the adjusted R^2 and all manager fixed effects are jointly significant.

1.4.2 Distribution and Magnitude of Manager Effects

Given manager effects explain a significant fraction of the variation in accruals, we investigate the economic magnitude of these manager effects. We report the size distribution of these manager fixed effects in Panel B of Table 3, where we show mean, standard deviation, 25th percentile, median and 75th percentile.

Overall, Table 3 Panel B shows that the variation in the size of manager fixed effects is economically large. For the distribution of manager fixed effects on total accruals, the difference between a manager at the 25th percentile and the 75th percentile is 0.049. This suggests that replacing a manager at the 25th percentile with one at the 75th increases the average total accruals by 0.049. Given the average level of total accruals in our sample is -0.043 (see Table 2), this increase is economically significant. The difference between the 25th percentile and 75th percentile in the abnormal accruals is 0.037, compared with an average abnormal accruals level of -0.003 in our sample. The table also shows that a manager in the bottom quartile reduces the absolute value of total accruals by 0.018, while a manager in the top quartile increases it by 0.019. For the absolute value of the abnormal accruals, the difference between a manager at the 25th percentile and the 75th percentile is 0.027.

The mean and the median manager fixed effects for most of these accruals measures are very close to zero. This indicates that our sample construction and our focus on externally hired managers do not lead us to select relatively more conservative or more aggressive managers.

1.4.3 Robustness tests

1.4.3.1 Persistent Effect or a One-Time Random Event

One potential concern is that manager fixed effects identified above are due to a random event that occurred during a manager's tenure in one of the firms. For example, for a manager who happens to be part of a firm during a period when this firm has large total accruals, we may estimate a positive manager fixed effect for that manager even though the effect does not persist across firms. To address this concern, we examine whether a significant manager effect is driven by just one of the firms or whether these effects persist across the multiple firms the manager works for during his career. Specifically, we estimate firm-year residuals by regressing accrual variables on firm fixed effects, year fixed effects, and the time-varying firm level controls, and *hence the manager effects are left in the residuals*. For the managers with significant effects on accruals, we then regress the average residual in the second firm on the average residual in the first firm. In untabulated results, we find a significant positive association between residuals in a manager's first and second firm for all the accrual variables.[20] The results imply that managers with significant effects on accruals carry their individual effects from firm to firm, and their effects are persistent and not driven by a random event at only one of the firms.

1.4.3.2 Balance Sheet vs. Cash Flow Approach to Calculate Total Accruals

[20] The coefficient is 0.303 ($p<0.0001$) for total accruals, 0.364 ($p<0.0001$) for abnormal accruals, 0.307 ($p<0.0001$) for absolute total accruals and 0.245 ($p=0.0004$) for absolute abnormal accruals.

In the analysis above, we use the balance sheet approach to calculate accruals, instead of the cash flow approach, because our sample period starts in 1969 and cash flow statement data are not widely available before 1988. Our accrual measures could be misestimated because of corporate transactions such as mergers and acquisitions (Hribar and Collins 2002). To assess whether our results are sensitive to the approach we use to calculate total accruals, we replicate the analysis by using total accruals based on cash flow statement data. We define total accruals as earnings from continuing operations minus cash flows from continuing operations earnings. Because the necessary cash flow statement data for most firms are available starting in 1988, we limit this analysis to the period starting 1988. Using cash flow statement data to compute abnormal accruals does not change the results. Untabulated results show that manager fixed effects are still statistically and economically significant in explaining accounting accruals.

1.4.3.3. Alternative Accrual Models

To estimate abnormal accruals, we use the modified Jones model in the analysis above. The modified Jones model is a widely-used but not the only way to estimate abnormal accruals. Therefore, we want to check whether our results still hold when using an alternative approach to estimate abnormal accruals. Specifically, we use the forward-looking Jones model developed by Dechow, Richardson and Tuna (2003) and find that our results are robust to the forward-looking Jones model.

1.4.3.4 Fitness of the Accrual Model

Large abnormal accruals may arise from earnings management, but could also be due to lack of fit in the accrual estimation model (see Hribar and Nichols 2007). The regression residual from the estimation of the accrual model measures “true” abnormal accruals with error. When using signed abnormal accruals, pure measurement error reduces the power of the test but does not bias the test. However, measurement error in signed abnormal accruals increases unsigned abnormal accruals on average and will bias

the test. Therefore, one might worry that the significant manager fixed effects on absolute abnormal accruals are driven by a few managers who happen to stay in firms whose accrual models lack good fitness. In other words, if a manager stays in multiple firms where the accrual models do not fit the accrual generating process, we will then observe large absolute abnormal accruals for this manager and mistake them for the manager's preference for managing accruals.

We address this concern by examining the relationship between the R^2 of the accrual model estimation for each firm and the manager fixed effects on absolute abnormal accruals. If the significant manager fixed effects on absolute abnormal accruals are due to lack of fit in the accrual model, we should observe that the lower the R^2 of accrual model estimation, the higher manager fixed effects on absolute abnormal accruals. We find that the Pearson correlation between manager fixed effects on absolute abnormal accruals and the R^2 of accrual models is only 0.001, which suggests they are not correlated. In addition, when we partition the firm-year observations into quintiles based on the R^2 of the accrual model for each firm, we do not observe any relationship between the quintile and the manager fixed effects on accruals.

1.4.4 Additional Discussion on Managers' Role

1.4.4.1 Active Role or the Firms' Endogenous Choice

In general, an alternative interpretation for significant manager fixed effects is that a manager may not actively influence his\her first firm's policies but by coincidence may be involved in changes in the firm's corporate policies. Erroneously assuming the manager's personal influence over these changes, another firm which has decided to make similar changes will hire this specific manager. In this case, we would observe a significant manager effect which is not due to the manager's active influence. To provide evidence that our manager fixed effects are due to managers' active influence, we analyze the timing of the change in firms' accruals. In the story outlined above, one would expect

that some of the changes in accrual variables precede the arrival of the new manager, since the board or the firm already decided to undertake the changes. If the changes in accrual variables are actually due to the active role of managers, the changes in accrual variables will only occur after the manager is hired. We repeat the analysis in the previous paragraph but assume each manager arrives in his second firm three years before the date he\she actually joins the firm. Untabulated results show that the estimated coefficients are economically very small compared with those estimated in the previous paragraph and all but one of the estimated coefficients are insignificant.21 This tells us that the changes in a firm's accruals happen after and not prior to the new manager's arrival and confirms the manager's active influence on firm accruals.

In addition, one may argue that managers' fixed effects on accruals in our study are due to situations in which they "crop up". For example, some managers are more specialized in growing firms and therefore are more likely to be hired by firms which need to grow. In this case, the manager's style on accruals captured by fixed effect is probably not due to his/her personal style on accounting but his/her style on growing firms. To examine whether this is the case, we examine firms' characteristics including financial distress, merger and acquisition activities and sales growth to see whether there are correlations between these firm characteristics and manager fixed effects on accruals. Untabulated results show that managers with different styles on accruals distribute evenly across firms with different characteristics. For example, managers in firms with high sales growth do not have significantly different effects on accruals from managers in firms with low sales growth; managers in firms with high Altman Z-scores do not have

[21] The coefficient is 0.159 (p=0.0198) for total accruals, 0.0001 (p=0.9992) for abnormal accruals, -0.0518 (p=0.5348) for absolute total accruals and -0.0457 (p=0.6548) for absolute abnormal accruals. Note that although the total accruals coefficient is significant, the magnitude is much smaller, less significant than the one in the previous paragraph, footnote 15, and the difference between the coefficients is statistically significant.

significantly different effects on accruals from managers in firms with low Altman Z-scores. These results indicate that the significant manager fixed effects on accruals are not driven by firms' demand for specific managers.

1.4.4.2 Corporate Governance Quality and Audit Quality

Out results show that managers have idiosyncratic styles which reflect their personal preference and cognitive bias. While these are stable personal characteristics, the extent to which a manager can imprint his/her own style on accruals can be affected by the firm's corporate governance and audit quality.

Prior literature (for example, see Bowen, Rajgopal and Venkatachalam, 2008) has showed that poor governance quality is associated with greater accounting discretion. This suggests that for a certain individual manager, he/she may exert greater effect on accruals when staying in firms with weak governance but smaller effect on accruals when staying in firms with strong governance. Meanwhile, other studies on audit quality (For example, see Francis and Krishnan, 1999; Krishnan, 2003) suggest that, relative to small audit firms, large accounting firms provide higher quality audits and, as such, more aggressively constrain their clients' attempts to manage earnings in general. Therefore, one may argue that a manager's effect on accruals reflect his/her employers' corporate governance and audit quality instead of his/her own financial reporting style.

However, by construction, a manager's fixed effect captures his/her persistent effect that he/she carries from one firm to another. It is unlikely that different firms he/she has stayed in have the same corporate governance structure and audit quality. Therefore, in our study, a manager's fixed effect on accruals should not be driven by firms' corporate governance and audit quality, but captures the manager's persistent individual effect on accruals.

1.5 Manager Effects on Accruals and Real Decisions

Bertrand and Schoar (2003) find evidence that managers have different "styles" when conducting their businesses. For example, some managers prefer external growth while others emphasize internal growth. In addition, manager preferences for some firm policies are correlated with each other. For example, managers who follow expansion strategies through external acquisitions engage in less R&D expenditures. Since we have shown that managers have individual effects on accounting accruals, it is important to examine whether managers firm policy decisions are associated with their effects on accruals.

We update Bertrand and Schoar (2003) to estimate manager fixed effects on these real decisions by extending the period covered by seven years from 1969 to 1999 to 2006. The empirical method is the one discussed in section 2. Instead of estimating equation (1) and (2) for accrual variables, we estimate these two equations for the different firm policy variables with the appropriate controls. Specifically, for investment, we consider investment, investment to Q sensitivity, investment to cashflow sensitivity, number of announced and effective acquisitions. For financial policy, we consider leverage, interest coverage, cash holdings and dividends to earnings. For operating policy (or organizational policy per Bertrand and Schoar), we consider number of diversifying acquisitions, R&D, advertising and SG&A. All these variables are taken from Bertrand and Schoar. Appendix B reports the tabled results, Table B1, and briefly discusses the results showing the manager effects on these firm policies. Overall, the results are qualitatively the same as Bertrand and Schoar (2003),[22] which show that manager individual effects explain a significant fraction of the variation in firm policies.

[22] Our results are quantitatively different from the results of Bertrand and Schoar (2003). One possible reason is that we have a different sample period and the economic environment has changed, e.g., the dot com bubble. The other reason may be the different way the software runs the regression. We use command xtreg, fe in Stata to run fixed effect regressions, where the adjusted R^2 is calculated based on the within R^2. So in our results, the firm fixed effects are not

Table 4 reports the simple Pearson correlation between manager fixed effects on real decisions and accounting accruals. We see that manager fixed effects for accruals are correlated with certain manager firm policy decisions. However, an interpretation of these simple correlations is premature, because manager effects on different real decisions are correlated as well. For example, Bertrand and Schoar (2003) shows that there is strong positive correlation between manager fixed effects on capital expenditure and on R&D, which can be interpreted as managers who follow internal investments engage in more R&D. In untabulated results, we show similar correlations between manager fixed effects for these firm policy variables. Hence, to provide a better perspective on the individual decisions of the manager, in Table 5 we consider the multivariate relation between manager fixed effects on accrual measures and on firm policies.

Table 5 reports results for the multivariate regressions. The dependent variables are manager fixed effects on total and absolute accrual measures and the independent variables are manager fixed effects on firm policy variables. In untabulated results, manager fixed effects on "N of announced acquisitions", "N of effective acquisitions" and "N of diversifying acquisitions" are highly correlated. Thus, we only include "N of announced acquisitions" in the regression.[23] A few interesting patterns seem to emerge from this table.

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used to calculate the adjusted R^2. We do not know how Bertrand and Schoar (2003) ran the regressions and calculated the adjusted R^2. If they create dummy variables for all the firm fixed effects in the regression and use them to calculate the R^2, they would end up with a much higher reported adjusted R^2. Using this approach to run all the regressions, we got much higher adjusted R^2, which were much closer to the results of Bertrand and Schoar (2003). We stay with the xtreg, fe fixed effect regression, because we believe this better captures the incremental explanatory power of manager fixed effects given the known and observable related factors.

[23] The correlation between "N of announced acquisitions" and "N of effective acquisitions" is 0.9, the correlation between "N of announced acquisitions" and "N of diversifying acquisitions" is 0.59 and the correlation between "N of effective acquisitions" and "N of diversifying acquisitions" is 0.62. Although we only report results for the regression including the number of

On the operating side, we observe that R&D, advertising and SG&A expenditures are all negatively correlated with accruals. This result can be interpreted as managers who prefer to cut discretionary spending on R&D, advertising, and SG&A tend to have a higher level of accounting accruals. This is consistent with prior literature which recognizes that managers can take both real economic actions and accounting actions to meet earnings benchmarks. Alternatively, this result suggests that managers who invest in R&D, advertising, and SG&A are more likely to have lower accounting accruals. We argue that managers who focus on long term growth of their firms pay less attention to achieving an earnings target through income increasing accruals.

For investment, there is a negative correlation between capital expenditures and abnormal accruals, which can be interpreted as managers who prefer internal growth tend to have lower abnormal accruals. Number of acquisitions is also negatively correlated with accrual variables.[24] Managers who prefer external growth through acquisitions and diversification have a lower level of accounting accruals. Therefore, as long as managers follow expansion strategies, no matter through internal investment or external acquisitions, they tend to have lower accounting accruals. A plausible explanation is that when managers focus on long term growth of their firms, they pay less attention to achieving an earnings target through income increasing accruals. In contrast, managers who use the firm's market valuation as a benchmark for their investment decisions have a higher level of total accruals; note the positive correlation between investment-Q

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announced acquisitions, all the results are the same when we use number of effective acquisitions instead.

[24] Since we use the balance sheet approach to calculate total accruals, total accruals are positively correlated with the number of acquisitions at the firm level. This negates our concern about the use of the balance sheet approach to calculate total accruals. If we find a positive correlation between number of acquisition fixed effects and total accruals fixed effects, the balance sheet approach could be the reason for this result. However, since we find a negative correlation here, the balance sheet approach should bias against our findings. In addition, we conduct robustness checks by using cash flow statement approach and our results still hold.

sensitivity and total accruals. Managers who are more sensitive to the firm's market valuation when they make investment decisions care more about how the market evaluates their firms. Therefore, these managers may use more accruals to meet market expectations.

As discussed, our manager firm policy decisions can be categorized into three groups: investment, financial, and operating. We have shown that manager effects on accounting accruals are correlated with certain firm policy decisions. Next, we examine which firm policy group is more correlated with accruals. In Table 6, we repeat the multivariate analysis in Table 5, but only examine one group of firm policy decisions at a time. All significant coefficients in Table 6 regressions are consistent with results in Table 5. We focus on the R^2 of each group's regression and use the Vuong test to examine which group of manager firm policy decisions has the most explanatory power for the manager effects on accruals. The Vuong test shows that manager operating decisions have the highest explanatory power. This result suggests that manager preferences for discretionary spending on R&D, advertising, and SG&A are the dominant firm policy decisions directly or indirectly affecting accounting accruals.

1.6 Comparison of CEOs and CFOs

When estimating manager specific effects on accruals in section 4, we created three different groups of managers: CEOs, CFOs and Others. Therefore, the fixed effects we estimate for each position is its effect on accruals after controlling for other position effects in the same firm-year. This allows us to separately study the effects of CEOs and CFOs on accounting accruals.

Until recently, the limited accounting literature which examines top manager effects on accruals focused on CEOs. One reason is that the CFO is the CEO's agent, Graham and Harvey (2001) and a CEO has the power to replace a CFO who does not follow the CEO's preferences, Mian (2001) and Fee and Hadlock (2004). So it may be

the case that CFOs make financial reporting decisions consistent with the wishes of their CEOs. However, Geiger and North (2006) show that newly appointed CFOs drive changes in discretionary accruals of their new firms, suggesting CFOs exercise independent influence on firms' financial reporting. Jing, Petroni and Wang (2010) suggest it is contract incentives not CEO pressure that motivates CFO behavior. Alternatively, Feng, Ge, Luo and Shevlin (2010) focusing on a select set of material accounting manipulations suggest CEO pressure as the motivation. As noted in the introduction, Ge et al. (2010) document the effects of CFOs on abnormal accruals without isolating and fully controlling for CEOs and other executives. Policy choices and their effects on accruals are also not controlled. Recent corporate fraud cases such as Enron, WorldCom, Qwest, and Adelphia also indicate that CFOs can significantly affect accounting quality. It is, therefore, important to compare the role played by CEOs and CFOs in affecting accounting accruals.

1.6.1 Current CEOs and CFOs

From previous sections, a manager can have either a positive or negative effect on accrual measures. A positive effect represents a manager preference for increasing a specific accrual measure, while a negative effect represents a manager preference for decreasing that accrual measure. To evaluate a manager's effect on accruals, we first consider the manager absolute effect and then the direction of the effect. Panel A of Table 7 compares the absolute value of fixed effects for CEOs and CFOs (manager fixed effects from equation 2 in Table 3 for each accrual variable), which represents manager influence regardless of direction. Since all t-tests are insignificant, the results tell us that the fixed effects of CEOs and CFOs on accounting accruals, regardless of direction, are the same.

We next investigate whether these two different positions affect accruals in the same direction. Panel B of Table 7 compares the signed fixed effects of these two

positions, which represent manager directional influence on accruals. The t-test for absolute total accruals is significant, which means CFOs tend to decrease the absolute value of total accruals, i.e., the magnitude is smaller for CFOs than CEOs. This tells us that CFOs tend to push accruals to zero more so than CEOs and suggests that CFOs report more "solid" earnings than CEOs.

1.6.2 CEO and CFO Effects Controlling for Firm Policy Decisions

As Lafond (2008) and Graham et al. (2005) point out, there are two important channels through which managers affect accounting accruals. One channel through which managers affect accounting accruals is through their decisions on firm policies and the correlation between manager effects on accruals and firm policies provide evidence for this channel. The other channel is through accounting choices which include managers' accrual estimates, choice of measurement methods, and discretion in recognizing economic transactions that affect accruals.

We expect CEOs to be the key decision-makers presiding over a firm's investment, financing, and operating policies and CFOs to be the key decision-makers over a firm's accounting choices, keeping in mind that the CFO serves at the discretion of the CEO. Thus, it is important to determine whether CEOs still have the same influence on accruals as CFOs have after controlling for their effects on firm policy decisions.

To determine manager effects on accruals through factors other than their firm policy decisions, we run the regressions for accrual measures as following:

$$y_{it} = \alpha_t + \gamma_{it} + \beta X_{it} + \lambda_{CEO\_firm} + \lambda_{CFO\_firm} + \lambda_{Others\_firm} + \varepsilon_{it} \quad (3)$$

$$y_{it} = \alpha_t + \gamma_{it} + \beta X_{it} + \lambda_{CEO\_firm} + \lambda_{CFO\_firm} + \lambda_{Others\_firm} + \lambda_{CEO} + \lambda_{CFO} + \lambda_{Others} + \varepsilon_{it} \quad (4)$$

These two equations are similar to equation (1) and (2) in section 2. The only difference is that we add more controls, λ_{CEO_firm}, λ_{CFO_firm} and λ_{Others_firm}. λ_{CEO_firm} represents CEO fixed effects on different firm policies, i.e., CEO fixed effects on

investment, investment-Q sensitivity, investment-CF sensitivity, N of acquisitions, etc.. Similarly, λ_{CFO_firm} represents CFO fixed effects on different firm policies, and λ_{Others_firm} represents other manager fixed effects on different firm policies.[25] These firm policy fixed effects are retrieved from regressions in Appendix B, Table B1. Since we have already showed that these manager fixed effects affect manager fixed effects on accruals, they could be considered as economic determinants of accruals. Therefore, λ_{CEO} λ_{CFO} and λ_{Others} in this regression are the manager fixed effects on accruals after we control for year fixed effect, firm fixed effect, the relevant time-varying firm characteristics, and more importantly, all manager fixed effects on firm policies. These fixed effects that we estimate should be managers' effects on accruals through channels other than their firm policy decisions.

Results, Appendix B, Table B2, show that managers still affect accruals significantly after controlling for their firm policy decisions. All the F-tests for manager effects are significant and the adjusted R^2 of regressions increases as we move from equation (3) to (4). In line with a priori intuition we find that the number of manager fixed effects which are statistically significant at 10% level decreases after we control for manager firm policy decisions. For example, for total accruals, as we discuss in section 4, where we do not control for manager firm policy decisions, 451 out of 711 manager fixed effects are significant at the 10% level in equation (2). After we control for manager firm policy decisions here, the number of significant manager fixed effects decreases to 318. The results are the same for the other three accrual measures. Again, this shows that manager effects on accruals are partially due to their effects through firm policies. After we control for their firm policies, some managers do not exert significant effects on

[25] Obviously, there could be several "other" managers who are not the CEO or CFO in a given firm-year. In that case, we average these other manager fixed effects and have only one "other" manager effect for each corporate policy variable in the regression.

accruals through the second channel of influence. In addition, we see the fixed effects estimated from equation (4) have larger standard deviations (as shown in Panel C in Table B2) than the ones shown in Panel C of Table 3. This suggests that when managers affect accruals through channels other than firm policy decisions, they may be more heterogeneous. Since these other channels include accounting choices, this implies that managers may have a wider degree of heterogeneity in accounting choices than in economic policy decisions.

Panel C of Table 7 compares the absolute value of fixed effects for CEOs and CFOs controlling for real decisions. The t-test for abnormal accruals and absolute abnormal accruals are significant. This result tells us that after controlling for real decisions, CFOs tend to have a larger influence on abnormal accruals than CEOs. The larger influence of CFOs after controlling for real decisions suggests that the implications of real decisions on accruals are more important for CEOs than for CFOs, and CFOs are more likely to affect accruals through other means such as accounting choices. Panel D of Table 7 confirms the results in Panel B. Controlling for real decisions, CFOs still tend to push accruals to zero more so than CEOs.

1.6.3 Career Path of CEO and CFO

In all the analysis above, we use the last position of the manager to identify his\her position throughout the entire sample period. However, a manager could hold a different position in a previous firm. For example, a manager could be CFO in the first firm and CEO in the second firm. Therefore, it is important to examine whether different career paths have any implication for a manager's effects on accruals.

Panel A of Table 8 summarizes a manager's transition from one firm to the next by title. We exclude 32 managers out of 811 who move to strictly more than two firms. For the 779 managers who move only once, 338 managers current position is CEO, 156 managers current position is CFO, and 285 managers current position is another key

executive. For 163 managers who started as CEO, 121 become CEO of another firm; 0 moves to a CFO position; and 42 move to another top position. This suggests that it is rare for a CEO to take a lower position at another firm. This is true of the CFO position as well. Among the 170 managers who started as CFOs, 24 become CEOs, 131 move to another CFO position, and 15 move to another top position. Finally, for other key managers, 193 move from other top positions to CEO, 25 to CFO, and 228 managers move between other non-CEO or other non-CFO positions. In sum, most managers, 480 out of 779 in our sample, retain the same position when moving from one firm to another.

Panel B compares fixed effects of managers who move from CEO to CEO and managers who move from CFO to CEO. All the t-tests are insignificant, suggesting that there is no significant difference between CEOs with and without CFO background in terms of their individual effects on accruals. In untabulated results, we compare managers who move from other top positions to CEO with CEO to CEO and CFO to CEO types of managers. None of the t-tests are significant, acknowledging the limited number of observations in the CFO to CEO category. These results tell us that a CEO's career path does not have a significant impact on his individual effect on accruals. Similar results hold in untabulated results for CFOs. Therefore, our comparison in Table 7 should not be affected by the CEO's or CFO's career path.

Panel C investigates the fixed effects of managers who retain the same position when they move. In this panel, we compare fixed effects of managers who move from CEO to CEO and managers who move from CFO to CFO. The comparison results are the same as those in Table 7, where we only use the last position of managers to compare CEOs and CFOs. The results again show that it is the CFOs who have more negative effects on the absolute value of total accruals. Therefore, the comparative results in Table 8 Panel C confirm our finding in Table 7, i.e., CFOs tend to push total accruals to zero. In untabulated results, we show that Table 8 results are robust when controlling for firm policy decisions.

Overall, the results in Table 8 suggest that managers generally keep the same position as they move from one firm to another. Therefore, using the last position of a manager as the position throughout the entire sample period is factually accurate. Also, for CEOs with and without CFO experience, there is no significant difference, Table 7 results, and CFOs tend to have less absolute total accruals than CEOs.

1.7 Conclusion

Existing accounting literature typically relies on critical events and firm-level characteristics to explain firm accruals, e.g., Klein (2002), Leuz, Nanda and Wysocki (2003), Hribar and Nichols (2007), and until recently rarely examined the role played by individual top managers. The contracting models of Jovanovic (1979) and Sargent (1987) in labor economics address this concern by allowing for idiosyncratic characteristics of the manager. Building on such contract models and the empirical works of Graham and his co-authors and Bertrand and Schoar (2003), we analyze the importance of the manager dimension in explaining variation in the different accrual measures.

We quantify how much of the observed variation in a firm's accounting accruals can be attributed to manager fixed effects after controlling for observable and unobservable differences across firms. Our results show that individual managers play both statistically and economically significant incremental roles in explaining accounting accruals. Our results show that firm policy decisions is one channel but not the only one through which managers affect accounting accruals. Firm policy effects on accruals are more important for CEOs than for CFOs. When managers affect accruals through channels other than firm policy decisions, they are much more heterogeneous. CFOs exert a larger influence on accruals through the second channel that includes accounting choices than CEOs. CFOs report more "solid" earnings than CEOs. This result is

independent of the CEO and CFO career path, both including and controlling for firm policy decisions.

Our study contributes to the accounting literature by adding a “people” dimension to explanations for accounting accruals. The significant influence managers exert on accounting accruals, coupled with the limited number of accounting studies on managers and accruals, e.g., Hribar and Yang (2010), suggest that considering the role played by individual managers has great potential to enhance our understanding of how managers and consequently firms make financial reporting choices. Since managers have significant individual effects on firm policies, performance and accruals, can these individual effects be recognized by investors? Do investors reward or punish a certain type of style? How does a manager’s compensation package affect or interact with his\her effect on accruals or preferences for specific accruals? Such questions we leave for future research.

Table 1-1 Summary Statistics for Firm Characteristics

	Manager-firm matched sample			Forbes and Execomp firms		
	Mean	Median	St. dev.	Mean	Median	St. dev.
Total Assets	5734.93	1653.92	13336.17	3617.30	960.63	9211.35
Total Sales	5479.37	1774.59	11592.51	3498.22	1134.83	7735.66
Market Value	6101.35	1477.03	16033.96	3958.05	893.12	11398.05
Investment	0.31	0.22	0.32	0.32	0.23	0.34
Average Tobin's Q	1.92	1.45	1.48	1.95	1.47	1.51
Cash Flow	0.52	0.34	1.24	0.53	0.35	1.47
N of announced acquisitions	1.08	1.00	1.58	0.95	0.00	1.44
N of effective acquisitions	0.79	0.00	1.34	0.69	0.00	1.22
Leverage	0.36	0.34	0.27	0.33	0.32	0.27
Interest coverage	32.97	7.91	130.65	47.29	8.20	210.05
Cash holdings	1.32	0.19	3.99	1.56	0.22	4.78
Dividend/earnings	0.10	0.08	0.12	0.09	0.07	0.12
N of diversifying acquisitions	0.42	0.00	0.91	0.35	0.00	0.80
R&D	0.06	0.03	0.08	0.06	0.03	0.09
Advertising	0.05	0.03	0.06	0.05	0.03	0.06
SG&A	0.24	0.20	0.17	0.23	0.19	0.18
Return on assets	0.06	0.06	0.11	0.06	0.06	0.12
Operating return on assets	0.11	0.11	0.11	0.11	0.11	0.13
Sample size	14930			32403		

Note:

a. "Manager-firm matched sample" includes: (1) firm-year observations for firms that have at least one manager observed in multiple firms with at least a three-year stay at each firm; (2) observations for these firms in the years in which they have other managers that we do not observe in multiple firms. "Forbes and Execomp firms" includes all firms in Forbes and Execomp data over period 1969 to 2006. Both samples exclude firms in the financial service industry and utility industry.
b. Definition and construction of the variables are reported in Appendix A.
c. All variables are winsorized at 1% tail.
d. Sample size is the maximum number of observations; not all variables are available for each year and firm.

Table 1-2 Summary Statistics for Different Accrual Variables

Panel A: Distribution Statistics

	Mean	Std. Dev.	25th pctl.	Median	75th pctl.
Total accruals	-0.043	0.071	-0.08	-0.045	-0.009
Abnormal accruals	-0.003	0.055	-0.028	-0.002	0.023
Abs. Total accruals	0.065	0.055	0.028	0.054	0.087
Abs. abnormal accruals	0.038	0.041	0.011	0.026	0.051

Panel B: Correlation Analysis

	Total accruals	Abnormal accruals	Abs. total accruals	Abs. abnormal accruals
Total accruals		0.697	-0.391	0.032
Abnormal accruals	0.659		-0.293	-0.053
Abs. total accruals	-0.688	-0.400		0.506
Abs. abnormal accruals	0.002	-0.071	0.278	

Note:

a. Sample is the manager-firm matched panel data set as described in subsection 3.1. Definition and construction of the variables are described in subsection 3.3 and also reported in Appendix A.
b. All variables are winsorized at 1% tail.
c. In Panel B, Pearson (Spearson) correlations are shown above (below) the diagonal.

Table 1-3 Manager Fixed Effects on Accounting Accruals

Panel A: Test of Manager Fixed Effects on Accounting Accruals					
	F-tests on fixed effects for				
	CEOs	*CFOs*	*Others*	*N*	*Adj. R^2*
Total accruals				11380	.105
Total accruals	21954.65*** 203/321	23464.47*** 93/141	520000*** 155/249	11380	.138
Abnormal accruals				11380	.049
Abnormal accruals	130000*** 190/321	4868.72*** 85/140	120000*** 160/250	11380	.064
Abs. total accruals				11380	.076
Abs. total accruals	110000*** 206/321	4447.73*** 81/140	130000*** 138/250	11380	.114
Abs. abnormal accruals				11380	.059
Abs. abnormal accruals	45458.45*** 190/321	4768.75*** 86/140	12971.79*** 155/250	11380	.081

Panel B: Size Distribution of Manager Fixed Effects					
	Mean	Std. dev.	25th pctl.	Median	75th pctl.
Total accruals	-0.001	0.049	-0.025	0.000	0.024
Abnormal accruals	-0.001	0.038	-0.018	0.002	0.019
Abs. total accruals	-0.001	0.042	-0.018	-0.000	0.019
Abs. abnormal accruals	-0.000	0.028	-0.013	-0.001	0.014

Note:

a. Sample is the manager-firm matched panel data set as described in subsection 3.1. Definition and construction of the variables are described in subsection 3.3 and also reported in Appendix A.

b. Reported in the table are the results from fixed effects panel regressions, where standard errors are clustered at the firm level. For each dependent variable (as reported in column 1), the fixed effects included are row 1: firm and year fixed effects; row 2: firm, year, and all manager fixed effects. Included in all regressions are leverage, B/M, logarithm of market value and ROA. Also included in the absolute accruals regressions are standard deviation of cash flows from operations and the standard deviation of sales.

c. Reported in Panel A are the *F*-tests for the joint significance of the CEO fixed effects (column 2), CFO fixed effects (column 3), and other executives fixed effects (column 4). For each F-test we report the value of F-statistic. ***, ** and * represent statistical significance are the 1%, 5% and 10% levels, respectively. Under each F-statistic, we report the number of managers who have individually significant effects and the total number of managers with sufficient data to be included in the regression estimation. For example, in the regression of total accrual where we include manager fixed effects, in the CEOs column (column 2), 201/315 means that we have 315 CEOs included in the regression and 201 CEOs have individually significant fixed effects on total accruals. Also reported in this panel are the adjusted R^2 which is calculated based on the within R^2 from the command xtreg, fe in Stata.

d. Reported in Panel B are the size distributions of fixed effects from the regressions. Column 1 reports the mean fixed effect for each accruals measure. Column 2 reports the standard deviation of the fixed

effects. Column 3, 4 and 5 report the fixed effects at the 25th percentile, 50th percentile, and 75th percentile of the distribution, respectively.

Table 1-4 Spearman Correlation between Manager Fixed Effects

	Total accruals	Abnormal accruals
Investment	0.04	**-0.10**
	(0.33)	**(0.01)**
Inv to Q sensitivity	-0.01	**-0.16**
	(0.76)	(**<.0001**)
Inv to CF sensitivity	0.00	0.06
	(0.96)	(0.13)
N of announced acquisitions	-0.01	-0.02
	(0.78)	(0.57)
N of effective acquisitions	-0.01	-0.05
	(0.81)	(0.15)
Leverage	0.01	-0.03
	(0.74)	(0.36)
Interest coverage	0.00	0.01
	(0.95)	(0.85)
Cash holding	0.02	-0.03
	(0.59)	(0.40)
Dividends/earnings	-0.06	-0.05
	(0.11)	(0.17)
N of diversifying acquisitions	0.03	-0.04
	(0.36)	(0.27)
R & D	**-0.07**	0.02
	(0.10)	(0.60)
Advertising	-0.02	**-0.15**
	(0.69)	**(0.00)**
SG & A	**-0.07**	-0.01
	(0.06)	(0.78)

Note:

a. Sample is the manager-firm matched panel data set as described in subsection 3.1. Definition and construction of the variables are reported in Appendix A.
b. Reported in the table are the spearman correlation between manager fixed effects on accrual variables and manager fixed effects on firm policy variables. P-values of the correlation are in parentheses. The fixed effects in this table are retrieved from the regressions reported in Table 3 and Table B1 in Appendix B (equation 2 for each variable).
c. Correlations significant at 10 percent level or better are indicated by bold numbers.

Table 1-5 Regression of Manager Fixed Effects on Accruals

	Total accruals		Abnormal accruals	
	Parameter Estimate	*Pr > |t|*	*Parameter Estimate*	*Pr > |t|*
Investment	0.002	0.905	**-0.029**	**0.013**
Inv to Q sensitivity	**0.017**	**0.001**	0.006	0.144
Inv to CF sensitivity	0.000	0.896	-0.002	0.452
N of acquisitions	**-0.009**	**0.003**	**-0.005**	**0.027**
Leverage	-0.007	0.737	-0.004	0.785
Interest coverage	0.000	0.170	0.000	0.520
Cash holdings	0.002	0.120	-0.001	0.397
Dividends/earnings	-0.032	0.496	0.005	0.890
R&D	**-0.130**	**0.023**	0.022	0.615
Advertising	0.027	0.765	**-0.159**	**0.028**
SG&A	**-0.100**	**0.041**	0.001	0.987
R^2	0.13		0.08	

Note:

a. Sample is the manager-firm matched panel data set as described in subsection 3.1. Definition and construction of the variables are reported in Appendix A.

b. Reported in the table are the results of regressions, where the manager fixed effects on different accrual variables are dependent variables and manager fixed effects on different firm policy variables are independent variables. The fixed effects in this table are retrieved from the regressions reported in Table 3 and Appendix B, Table B1 (equation 2 for each variable.)

c. "N of acquisitions" in this table is "N of announced acquisitions".

d. Estimated parameters significant at 10 percent level or better are indicated by bold numbers.

Table 1-6 Regression of Sub-group Manager Fixed Effects on Accruals

	Total accruals			Abnormal accruals		
	(1)	(2)	(3)	(1)	(2)	(3)
Investment	0.017			**-0.022**		
Inv to Q sensitivity	-0.001			-0.002		
Inv to CF sensitivity	0.000			0.001		
N of acquisitions	-0.002			-0.002		
Leverage		-0.001			0.000	
Interest coverage		0.000			-0.001	
Cash holdings		0.001			0.000	
Dividends/earnings		-0.046			0.001	
N. of Diversifying Acquisition			-0.005			-0.006
R&D			**-0.138**			-0.013
Advertising			0.007			**-0.164**
SG&A			**-0.096**			0.013
R^2	0.006	0.006	0.040	0.022	0.006	0.022
	H_0: Model(1) = Model(2) -0.42 (0.674)			H_0: Model(1) = Model(2) **-1.69 (0.092)**		
Vuong Test Z-statistic (p-value)	H_0: Model(1) = Model(3) **3.87 (0.001)**			H_0: Model(1) = Model(3) **3.93(0.001)**		
	H_0: Model(2) = Model(3) **4.34 (<0.001)**			H_0: Model(2) = Model(3) **4.91(<0.001)**		

Note:

a. Sample is the manager-firm matched panel data set as described in subsection 3.1. Definition and construction of the variables are reported in Appendix A.
b. Reported in the table are the results of regressions, where the manager fixed effects on different accrual variables are dependent variables and manager fixed effects on different firm policy variables are independent variables. The fixed effects in this table are retrieved from the regressions reported in Table 3 and Appendix B, Table B1 (equation 2 for each variable.)
c. "N of acquisitions" in this table is "N of announced acquisitions".
d. Estimated parameters significant at 10 percent level or better are indicated by bold numbers.

Table 1-7 Comparison of CEO and CFO Fixed Effects on Accruals

Panel A: Unsigned CEO and CFO Fixed Effects on Accruals				t-tests	
	CEO	*CFO*	*Difference Between Means*	*t-value*	*Pr > |t|*
Total accruals	0.033	0.037	-0.004	-1.23	0.219
Abnormal accruals	0.025	0.027	-0.003	-0.62	0.506
Abs. total accruals	0.028	0.031	-0.003	-1.05	0.293
Abs. abnormal accruals	0.002	0.020	-0.002	-0.89	0.376
Panel B: Signed CEO and CFO Fixed Effects on Accruals					
	CEO	*CFO*	*Difference Between Means*	*t-value*	*Pr > |t|*
Total accruals	-0.003	-0.000	-0.003	-0.62	0.532
Abnormal accruals	-0.002	0.001	-0.003	-0.84	0.401
Abs. total accruals	0.002	-0.007	0.009	**1.90**	**0.059**
Abs. abnormal accruals	0.001	-0.003	0.004	1.46	0.145
Panel C: Unsigned CEO and CFO Fixed Effects on Accruals Controlling for Firm Policy Decisions					
	CEO	*CFO*	*Difference Between Means*	*t-value*	*Pr > |t|*
Total accruals	0.044	0.050	-0.006	-1.3	0.1932
Abnormal accruals	0.034	0.040	-0.007	**-1.96**	**0.0511**
Abs. total accruals	0.038	0.031	-0.003	-0.26	0.7919
Abs. abnormal accruals	0.022	0.027	-0.005	**-1.68**	**0.0953**
Panel D: Signed CEO and CFO Fixed Effects on Accruals Controlling for Firm Policy Decisions					
	CEO	*CFO*	*Difference Between Means*	*t-value*	*Pr > |t|*
Total accruals	-0.008	0.001	-0.009	-1.33	0.184
Abnormal accruals	0.003	-0.001	-0.002	-0.38	0.704
Abs. total accruals	0.002	-0.008	0.010	**1.87**	**0.062**
Abs. abnormal accruals	0.001	-0.005	0.006	1.54	0.124

Note:

a. Sample is the manager-firm matched panel data set as described in subsection 3.1. Definition and construction of the variables are reported in Appendix and B.

b. Reported in this table is the comparison of CEO fixed effects on accrual variables and CFO fixed effects on accrual variables. Panel A compares absolute value of fixed effects of the two positions before controlling for manager firm policy decisions; Panel B compares signed fixed effects of the two positions before controlling for manager firm policy decisions; Panel C compares absolute value of fixed effects of the two positions after controlling for manager firm policy decisions; Panel D compares signed fixed effects of the two positions after controlling for manager firm policy decisions. Column 1 is the mean of CEO fixed effects. Column 2 is the mean of CFO fixed effects. Column 3 is the difference between column 1 and 2. Column 4 and 5 are the results of t-tests to compare column 1 and 2.

c. t-tests are the usual pooled t-test because the "Equality of Variances" test results show that the assumption of equal variances is reasonable.

d. t-statistics significant at the 10 percent level or better are indicated by bold numbers.

Table 1-8 Fixed Effects of Managers with Different Career Paths

Panel A: Manager Transition Between Positions				
to:	CEO	CFO	Other	Total
from:				
CEO	121	0	42	163
CFO	24	131	15	170
Other	193	25	228	446
Total	338	156	285	779

Panel B: CEOs with and without Financial Experience				*t-tests*	
	CEO_CEO	*CFO_CEO*	*Difference Between Means*	*t-value*	*Pr > |t|*
Total accruals	-0.001	-0.003	0.002	0.22	0.824
Abnormal accruals	-0.003	-0.002	-0.001	-0.05	0.961
Abs. total accruals	0.004	-0.004	0.007	0.69	0.489
Abs. abnormal accruals	0.001	-0.008	0.009	1.16	0.248

Panel C: CEOs and CFOs with the Same Position in the Previous Firm				*t-tests*	
	CEO_CEO	*CFO_CFO*	*Difference Between Means*	*t-value*	*Pr > |t|*
Total accruals	-0.001	-0.000	0.001	-0.03	0.975
Abnormal accruals	-0.003	0.000	-0.003	-0.49	0.628
Abs. total accruals	0.003	-0.007	0.010	**1.63**	**0.090**
Abs. abnormal accruals	0.001	-0.003	0.004	0.99	0.322

Note:

a. Sample is the manager-firm matched panel data set as described in subsection 3.1. The original sample totals 811 managers; we exclude 32 managers observed in strictly more than two firms in this table. Therefore, the total number of managers in this table is 779.

b. Panel A summarizes position changes as a manager move from one firm to another. The column describes the managers' title in the last firm in which the managers were observed (current title), while the row describes the managers' title with their previous firm. "Other" refers to any job title other than CEO or CFO.

c. Reported in Panel B and C is the comparison of fixed effects of managers with different career paths. Panel B compares CEOs who were CEO in the prior firm and CEOs who were CFO in the prior firms. Panel C compares CEOs who were CEO in the prior firm and CFOs who were CFO in the prior firms.

d. t-tests are the usual pooled t-test because the "Equality of Variances" test results show that the assumption of equal variances is reasonable.

e. t-statistics significant at the 10 percent level or better are indicated by bold numbers.

CHAPTER 2 CAN THE CAPITAL MARKET RECOGNIZE A MANAGER'S FINANCIAL REPORTING STYLE?

2.1 Introduction

The financial press is replete with discussion about firms' executives. Much of this attention comes from the fact these individuals are considered as the crucial determinants of corporate practice and performance. Consistent with this anecdotal evidence, the issue of executives' effects on firms has attracted researchers' attention for decades and the significance of executives in shaping corporate practices has been documented. In addition, prior literature (for example, Johnson, Magee, Nagarajan, and Newman 1985; Bonnier and Bruner 1989; Denis and Denis 1995; Hayes and Schaefer 1999), which shows the capital market reacts to managerial change, suggests that investors do understand the importance of managers and that some portion of firm value is due to managerial expertise.

It is unclear, however, whether the capital market can recognize managers' idiosyncratic differences in financial reporting. Corporate financial disclosure is critical to the capital market because it is a key source of information about the economic activities of a firm. Despite accounting rules, regulation, and other informal monitoring structures, financial reporting involves substantial managerial discretion. DeJong and Ling (2010) and Ge, Matsumoto, and Zhang (2011) show that managers play a significant role in explaining firms' financial reporting choices and that they carry their styles from one firm to the next. Therefore, understanding a manager's financial reporting style can benefit investors because it can help in predicting and understanding the manager's future financial disclosures. Given the growing attention received by CEOs and CFOs, especially following financial fraud cases such as Enron and WorldCom, one would expect the capital market to examine a manager's financial reporting history and identify her financial reporting style. In addition, evidence in the influential survey paper by

Graham, Harvey, and Rajgopal (2005) implies CFOs act as if the market can recognize their personal effects in financial reporting.[26] If investors do recognize a manager's financial reporting style, it justifies these managers' concern and behavior. However, recognizing a manager's financial reporting style requires investors to not only understand a firm's economic activities and accrual generating process, but also track a manager's reporting history to isolate her discretion. Given the complexity of these tasks, the capital market may not be able to identify a manager's individual financial reporting style.

Building on previous studies on manager individual style on financial reporting, I extend the literature by investigating whether the capital market can recognize managers' idiosyncratic differences and adjust for their individual styles on financial reporting. I focus on managers' financial reporting aggressiveness, which is defined as upward earnings management through income-increasing discretionary accruals. I predict a manager's aggressiveness affects investors' perceptions of her credibility and examine the capital market's reaction to management earnings forecasts. Management forecasts provide a setting to investigate my research question because firms' financial performance for the forecasting period has not been realized. Investors rely on their perception of the believability of the manager to evaluate the forecast, as opposed a more objective disclosure, such as an earnings announcement. During this process, investors' beliefs about a manager's financial reporting aggressiveness affect the manager's credibility and hence investors' reaction to the manager's earnings forecasts. I choose not to examine investors' reaction to earnings announcements because earnings and

[26] In Graham, Harvey, and Rajgopal (2005), CFOs express that career concerns, especially those related to external reputation, are important drivers of their financial reporting decisions. These managers also indicate that they choose real action over accounting actions to meet earnings benchmarks because they need to assure investors that there is no accounting-based earnings management. This concern implies that managers behave as if investors do understand their discretionary accounting choices.

accrual components have been realized in the financial reports. For the realized earnings, investors can analyze the accrual firm performance and its accrual and cash flow components when reacting to the earnings announcement. The manager's aggressiveness in the past should not affect investors' reaction. Therefore, I focus on the market's reaction to management forecast to test whether investors can recognize a manager's financial reporting aggressiveness.

While most existing literature focuses on financial reporting at the firm level, I adopt Bertrand and Schoar's (2003) methodology to isolate manager-specific effects on discretionary accruals. Consistent with prior literature, I find manager individual effects play a significant role in explaining discretionary accruals. I then utilize these estimated fixed effects to measure manager-specific financial reporting aggressiveness and examine whether investors can distinguish between managers' idiosyncratic differences on financial reporting aggressiveness. I do not expect investors follow the exact same approach to estimate a manager's fixed effect on discretionary accruals but use the manager fixed effect as a proxy for investor beliefs to distinguish between aggressive managers and conservative managers. Intuitively, investors can examine different managers' financial reporting history at different firms and form their beliefs about a certain manager's aggressiveness. When both an aggressive manager and a conservative manager issue financial disclosures, I expect investors to react differently. To avoid look-ahead bias in the market reaction test, I update the fixed effect estimates by adding the appropriate new financial information. An interesting result from this estimation process is that the updated manager fixed effect estimates are relatively persistent while the firm fixed effects are not. This suggests that the manager fixed effect captures a manager's personal preference and cognitive bias, which are relatively stable personal characteristics. The firm fixed effect, however, reflects a firm's characteristics such as operating cycle and business environment, which can fluctuate over time.

My results show that the capital market discounts news contained in earnings forecasts by aggressive managers. This suggests market participants recognize manager-specific effects on financial reporting aggressiveness. In addition, I examine whether investors update their belief of a manager's style following a Bayesian learning process. Consistent with a Bayesian learning model, my results show that the discount investors imposed on earnings forecast news because of manager aggressiveness is larger when the number of financial reports comprising a manager's financial reporting record increases. The evidence that the aggregate-market behavior is consistent with Bayesian learning shows that investors are able to process historical information and update their inference about a manager's individual style in a rational way. This provides support for why managers care about their reputation and how they build such a reputation

My study extends recent accounting research on managers' financial reporting styles. While these studies document the existence and significance of individual manager styles, they rarely examine whether the capital market recognizes and responds to a manager's style on financial reporting. My paper extends these studies to show that investors are able to identify a manager's aggressiveness on financial reporting. This finding is consistent with managers' expressed concerns for developing a personal reputation as showed in Graham, Harvey, and Rajgopal (2005). In addition, since I show manager style captured by manager fixed effect has valuation implication, I provide evidence that manager fixed effects estimated by using Bertrand and Schoar (2003) methodology are indicative of an individual style effect. My additional analysis shows that an approximate randomization test provides the same results as my main test and the randomized manager fixed effects do not have pricing implication.

My paper also contributes to the literature on the market's reaction to earnings forecasts. This stream of literature finds that earnings guidance is associated with security returns, trading volume, and analyst earnings forecasts (e.g., Patell, 1976; Penman, 1980; Ajinkya and Gift, 1984; Waymire, 1985; Jennings, 1987; Hutton et al., 2003; Atiase et

al., 2005). However, most of these studies examine whether firms' and earnings forecasts' characteristics affect the market's reaction but rarely investigate managers' characteristics. I add to this literature by showing that managers' individual styles matter.

Furthermore, my study is related to existing literature which shows that managers' personal beliefs affect multiple dimensions of corporate decisions. For example, Ben-David, Graham and Harvey (2007) document that CFO overconfidence induces distortions in multiple corporate decisions including investment, external financing, and payout decisions. Gong, Li and Xie (2009) point out that accrual accounting and earnings forecasts contain similar biases because both involve a high degree of managerial subjectivity. Along this line, the manager-specific effect on accruals reflects the manager's personal belief and cognitive bias, which should also be associated with a manager's earnings forecast and the manager's credibility. My paper adds to this line of literature by showing that a manager's financial reporting aggressiveness has implications for her credibility and therefore affects the market's reaction to her earnings forecasts.

2.2 Prior Literature and Hypothesis Development

2.2.1 Prior Literature

The influence of managers on firm policies, performance and shareholder value has been of interest to researchers for decades, especially in the economics, finance and management literature. In particular, a study in financial economics, Bertrand and Schoar (2003), empirically examines the effect of managerial idiosyncratic differences on corporate policies. Bertrand and Schoar use a research design that tracks managers over time to isolate the manager's individual effect from the firm effect. This design allows them to examine not only the existence, but also the magnitude of individual managers' effects on firms. They show that manager effects are significant in determining the firm's investment, financial, and organizational practices and performance, which suggests that managers exhibit unique styles that they carry from one firm to the next.

Given the significant effects managers exert on their firms' corporate policy, a large literature documents the market reaction to managerial change. For example, Bonnier and Bruner (1989) show significantly positive stock price reactions to managerial turnover news; Denis and Denis (1995) examine the relation between managerial turnover and subsequent changes in firm operating and stock price performance; Johnson, Magee, Nagarajan, and Newman (1985) and Hayes and Schaefer (1999) document a negative market reaction to unexpected CEO deaths. These studies suggest that the market does realize the important role played by the managers and some portion of firm value is due to managerial expertise. In addition, Bertrand and Schoar (2003), after documenting manager fixed effects on corporate behavior and performance, show a correlation between managers' compensation and their certain fixed effects. More recently, Graham, Li and Qiu (2009) use manager fixed effects to proxy for innate managerial ability and show that these fixed effects partially explain executive compensation. These results imply that boards can recognize managers' personal style and are willing to pay a premium for managers with preferred styles.

In the accounting research area, several recent studies adopt Bertrand and Schoar's (2003) methodology to examine manager effects on accounting practices. Bamber, Jiang, and Wang (2010) show that top executives exert unique and economically significant influence on their firms' voluntary disclosure choices. Dyreng, Hanlon, and Maydew (2010) also find that managers have significant effects on firm tax avoidance behavior. DeJong and Ling (2010) and Ge, Matsumoto, and Zhang (2011) show that manager fixed effects are important determinants of certain accounting choices. However, prior studies has not examined whether investors can recognize managers' individual financial reporting styles. In the influential survey paper by Graham, Harvey, and Rajgopal (2005), CFOs express that career concerns, especially those related to external reputation, are important drivers of their financial reporting decisions. These managers also indicate that they choose real action over accounting actions to meet

earnings benchmarks because they need to assure investors that there is no accounting-based earnings management. This concern implies that managers behave as if investors do understand their discretionary accounting choices.

A more recent study, Yang (2011), examines whether the capital market can identify managers' styles on voluntary disclosure. She finds that investors take into account manager-specific forecasting records, but only when information uncertainty is high. Since financial reporting is mandatory disclosure, which is more important and prevailing than voluntary disclosure, investors may exert more effort to analyze financial reports and therefore have a better understanding of manager-specific financial reporting style. From this perspective, Yang's results suggest investors may recognize managers' financial reporting style as well. On the other hand, because management earnings forecasts reveal forward-looking information which can be verified later by confirmatory financial reports, it is relatively easy for investors to observe a manager's forecasting history and identify whether a certain manager tends to have accurate forecasts. Recognizing a manager's financial reporting style, especially financial reporting aggressiveness, requires investors to understand a firm's accrual generating process and isolate the manager's discretion. Therefore, Yang's (2011) result cannot be generalized to managers' financial reporting styles and it does not necessarily indicate investors can recognize a manager's financial reporting aggressiveness.

2.2.2 Hypothesis Development

A substantial body of accounting research focuses on financial accounting aggressiveness, which is often measured by discretionary accruals. One important finding of these studies is that investors appear to consider earnings with large accruals to be indicative of poor quality resulting in a contemporaneous reduction in stock prices.[27]

[27] The capital market can adjust, at least partially, for abnormal accruals, although they do not necessarily fully incorporate the implication into stock price. For example, Defond and Park

These studies demonstrate that investors extract valuation information from discretionary accruals. In addition, both anecdotal evidence and previous studies show that investors consider firms with aggressive accounting as less credible. For example, Bratton (2003) notes that "The stock market awaken in 2002 to discover that it no longer had numbers it could trust" because of the accounting and corporate scandals. Li, Pincus and Rego (2008) show that investors reacts more positively to firms with aggressive accounting when SOX legislative events occurred because SOX would improve the credibility of these firms' financial reporting. Therefore, when manager-specific effects play a significant role in determining firms' discretionary accruals, which represent managers' aggressiveness, these individual manager effects have implications for management's credibility.

To test whether investors recognize and adjust for a manager's aggressiveness, I choose to examine the market reaction to the manager's earnings forecasts. In such a setting, the firm's financial performance has not been fully realized. Investors' perception of the believability of the manager, which is not an objective condition of a disclosure, affects their reaction. This idea is consistent with Jennings' (1987) notion that investors reactions to a management disclosure depends on both the unexpected component ("surprise") and the credibility ("believability") of the disclosure. As discussed in Mercer (2004), managers' credibility is an important factor in disclosure credibility. I expect managers who use more income-increasing accruals are considered less credible. Therefore, if investors exert effort to recognize managers' financial reporting style, I

(2001) show that abnormal accruals suppress the magnitude of market reactions to earnings surprises, suggesting that investors do not find abnormal accruals as reliable as normal accruals. Balsam, Bartov, and Marquardt (2002) document a negative association between unexpected discretionary accruals and stock returns over a 17-day window around the 10Q filing date. Baber, Chen, and Kang (2006) provide evidence that investors discount earnings when contemporaneous supplemental disclosure suggests earnings have been managed via discretionary accruals.

expect the market reaction to earnings forecasts by aggressive managers to be discounted, ceteris paribus. This leads to my first hypothesis:

Hypothesis 1: The market discounts the news in a management earnings forecast when the manager is relatively aggressive.

The first hypothesis provides evidence on the relation between manager financial reporting aggressiveness and market reaction to earnings forecasts. When investors incorporate a certain manager's style into their reaction to her earnings forecasts, they can only use up-to-date information to estimate the manager's individual style. As the manager issues more financial reports over time, investors keep updating their inferences about the manager. Figure 1 depicts the events during this process. Therefore, the market reaction to earnings forecasts depends on the perceived financial reporting style of the manager, which is determined by both the level of aggressiveness and financial reporting history. Park and Stice (2000) and Chen, Francis, and Jiang (2005) provide similar arguments and evidence in the context of analyst forecasting behavior. Park and Stice show that investor beliefs about the usefulness of a forecast are a function of both the accuracy and length of the prior forecasting record. Chen et al. empirically establish that a static model of investor learning about analyst forecasting performance is less descriptive of investor behavior than a Bayesian model. Hutton and Stocken (2009) adopt Chen et al.'s Bayesian model to apply to investors' learning of management forecast accuracy. They find that the stock price response to management forecast news is increasing in prior forecast accuracy and also in the length of a firm's forecasting record. Based on these studies, I expect the capital market's learning of managers' financial reporting style also follows a Bayesian process. Specifically, as a manager's financial reporting history becomes longer, there is less uncertainty about the manager's true style. Hence, the discount on the reaction to earnings forecast news based on the manager's aggressiveness will be larger. This leads to my second hypothesis as following:

Hypothesis 2: As the length of a manager's financial reporting history becomes longer, the discount on the market reaction to earnings forecast news due to the manager's aggressiveness becomes larger.

2.3 Research Design

2.3.1 Measuring Manager Financial Reporting Aggressiveness

Following prior literature, I use discretionary accruals to measure financial reporting aggressiveness.[28] Ideally, if the accrual process is modeled properly, non-discretionary accruals represent adjustments that reflect a firm's fundamental performance and innate financial characteristics, while discretionary accruals capture distortions induced by earnings management. However, all existing accrual models introduce misclassification errors. I further attribute discretionary accruals to manager-specific effects while controlling for firm fixed effect on discretionary accruals and time-varying firm characteristics. The estimated manager-specific effect on discretionary accruals captures the manager's personal discretion on accruals, i.e., the manager financial reporting aggressiveness. This individual effect is the manager's persistent financial reporting style that she carries from one firm to the next.

Following Bertrand and Schoar (2003), I estimate model (3) as follows to obtain manager effects on discretionary accruals. I also estimate model (1) and (2) to demonstrate the explanatory power of firm and manager fixed effects:

$$DAcc_{i,t} = \sum \alpha\, Year_t + \sum \alpha\, Firm_i + \sum \alpha\, Control_{i,t} + \varepsilon_{i,t} \quad (1)$$

$$DAcc_{i,t} = \sum \alpha\, Year_t + \sum \alpha\, Manager_j + \sum \alpha\, Control_{i,t} + \varepsilon_{i,t} \quad (2)$$

$$DAcc_{i,t} = \sum \alpha\, Year_t + \sum \alpha\, Firm_i + \sum \alpha\, Manager_j + \sum \alpha\, Control_{i,t} + \varepsilon_{i,t} \quad (3)$$

[28] I also examine total accruals as the measure of financial reporting aggressiveness and find the same results.

The dependent variable $DAcc_{i,t}$ stands for discretionary accruals estimated from the accrual model for each firm in each year. Specifically, I use a time series modified Jones model, which will be discussed later. The indicator variable for each firm ($Firm_i$) represents the firm fixed effect and the indicator variable for each manager ($Manager_j$) represents the manager fixed effect. I also control for year fixed effects by including an indicator variable for each year ($Year_t$).[29] I include time-varying firm level control variables that have been documented by prior literature to be associated with firm-level earnings management by the existing literature. Specifically, I control for leverage, growth opportunity (book-to-market ratio), size (the natural logarithm of market value), and performance (return on assets). I account for serial correlation by allowing for clustering of the error term at the firm level.[30] Finally, ε_{it} is an error term.

By construction, a manager's fixed effect is a constant number when estimating the model for a certain time period. However, as the manager issues more financial reports, investors can update their belief about her as depicted in Figure 1. To avoid the look-ahead bias in the market reaction test, I estimate these fixed effects by using information to date and then update the estimation by adding more financial information when the firm issues more financial reports. Therefore, the manager fixed effect estimate that I use to measure a manager's aggressiveness changes from year to year. This reflects investors' perception varies over time because new information becomes available.

[29] Industry dummies cannot be included because they are absorbed into firm fixed effects.

[30] I include firm fixed effects and also cluster standard errors at the firm level because the firm fixed effects cannot fully capture the within-cluster dependence if the firm effect decays over time. Therefore, it is necessary to have both firm fixed effects and cluster standard errors if I want to control for both permanent and temporary firm effects. See Petersen (2009) for a more detailed discussion.

2.3.2 Market Response to Earnings Forecast News

Hypothesis 1 examines whether the capital market responds to earnings forecast news differently for managers with different financial reporting aggressiveness. I test this hypothesis by utilizing the estimated firm and manager fixed effects in the following model:

$$CAR_{-1,+1} = \gamma_0 + \gamma_1 News_{i,t} + \gamma_2 M\_Effect_{j,t} + \gamma_3 News_{i,t} \times M\_Effect_{j,t} + \sum \gamma Control\ Variable_{i,t} + \sum \gamma News_{i,t} \times Control\ Varible_{i,t} + \varepsilon_{i,t} \quad (4)$$

$CAR_{-1,+1}$ is the three-day event window return centered on the day of the earnings forecast. I report results using size-adjusted returns, where the size-decile return is calculated based on CRSP NYSE/AMEX /Nasdaq Capitalization Deciles.

Forecast news, denoted $News_{i,t}$, equals the earnings per share forecast less the consensus analyst estimate prevailing on the day of the earnings forecast scaled by the stock price three days before the announcement date of the earnings forecast.

$M\_Effect_{j,t}$ represents manager *j*'s most recent estimated fixed effect on discretionary accruals. I interact manager fixed effects ($M\_Effect_{j,t}$) with forecast news ($News_{i,t}$) to examine whether investors respond differently to a per unit of forecast news for managers with different individual effects on accruals. Prior research uses this specification to examine the market reaction to forecasts (e.g., Jennings 1987; Rogers and Stocken 2005; Hutton and Stocken 2009; Yang 2010). Based on my prediction, I expect a negative coefficient on this interaction term, i.e., $\gamma_3 < 0$. A negative γ_3 indicates that investors discount the news contained in forecasts issued by aggressive managers, i.e. investors find these managers to be less credible.

Yang (2011) finds that investors do not take into account manager-specific forecasting behavior once the firm's forecasting record is considered unless the information uncertainty is high. Her result suggests that in a general setting, investors focus more on firms' forecast records rather than managers' forecast records. Therefore, I include firm fixed effects on abnormal accruals in model (4) as one of the control

variables. If investors only focus on firms, or are not able to separate firms' financial reporting characteristics from managers', I expect $\gamma_3$to be insignificant when the firm fixed effect on abnormal accruals is added as a control variable.

I also include several other control variables identified in previous literature as affecting earnings response coefficients or managers' forecasting behavior. Prior literature shows the form and time horizon of the forecast may influence how the stock market reacts to earnings forecasts. Although the evidence on this point is mixed, I control for forecast specificity and horizon of the forecast nevertheless.[31] Pownall et al. (1993) also find that interim forecasts are significantly more informative relative to annual forecasts, so I control for whether a forecast is an interim or annual forecast. In addition, I control for growth opportunities (Bamber and Cheon 1998), size (Baginski and Hassell 1997; Bamber and Cheon 1998), performance, and special items (Bradshaw and Sloan 2002). All control variables are defined in Appendix C. Finally, I include year fixed effects in the model.

2.3.3 Bayesian Learning Process

Hypothesis 2 examines the path investors take to form their belief about a manager's financial reporting style, which I model as a Bayesian learning process following Chen et al (2005).

In reality, investors do not observe the manager's true financial reporting aggressiveness p, but have a common prior that it is normally distributed with mean p_0. As the manager issues more financial reports, more information about her aggressiveness is revealed, so investors can update their prior belief. The signal about the manager's

[31] For example, Baginski, Conrad and Hassell (1993) report that point forecasts are more informative relative to other types of forecasts, but Pownall, Wasley and Waymire (1993) and Atiase et al. (2005a) find no difference in stock-price reaction conditional on the form of the forecast.

financial reporting aggressiveness is denoted as x_n. After *N* observation of x_n, investors' best estimate of the manager's aggressiveness, $\hat{p}(N)$, is

$$\hat{p}(N) = p_0\big(1 - W(N)\big) + AGG(N) \cdot W(N) \quad (5)$$

where:

$AGG(N) \equiv (1/N)\sum_{n=1}^{N} x_n$ = the aggressiveness of the manager based on *N* prior financial reporting signals;

$W(N)$ = the weight investors place on $AGG(N)$;

$1 - w(N)$ = the weight investors place on their prior perceived aggressiveness for the manager.

The variable $AGG(N)$ captures the manager's perceived aggressiveness based on her past financial reporting history. Investors' learning from the past financial reports is captured by $W(N)$. Hypothesis 2 predicts that investors form $\hat{p}$ over time in a manner consistent with Bayesian inference. Therefore, $W(N)$ is an increasing and concave function of *N*. That is, when a new manager makes her first financial report, investors assign p_0 as her prior perceived style. Each time the firm announces earnings, investors acquire signals about the manager's style and shift weight off their prior beliefs and onto the perceived aggressiveness of the manager, $AGG(N)$.

To test Hypothesis 2, I examine whether investors' reaction to earnings forecast news is correlated with both a manager's perceived aggressiveness and the number of her prior financial reports. Specifically, I estimate the following model:

$$CAR_{-1,+1} = \gamma_0 + \gamma_1 News_{i,t} + \gamma_2 News_{i,t} \times \hat{p}(N)_{j,t} + \sum \gamma\, Control\ Variable_{i,t} + \sum \gamma\, News_{i,t} \times Control\ Varible_{i,t} + \varepsilon_{i,t} \quad (6)$$

Substituting (5) into (6) and re-arranging terms yielding the following equation:

$$CAR_{-1,+1} = \gamma_0 + \gamma_1\, News_{i,t} + \gamma_2 News_{i,t} \times p_0 \times \big(1 - W(N)\big) + \gamma_3 News_{i,t} \times AGG(N) \times W(N) + \sum \gamma\, Control\ Variable_{i,t} + \sum \gamma\, News_{i,t} \times Control\ Varible_{i,t} + \varepsilon_{i,t} \quad (7)$$

I measure $AGG(N)$ by the fixed effect a manager exerts on the discretionary accruals in *N* financial reports available for this manager, which is her average effect on discretionary accruals in the past and consistent with how $AGG(N)$ is constructed. Specifically, $AGG(N)$ is measured by the most recent estimated fixed effect a manager exerts on discretionary accruals, i.e. $M\_Effect_{j,t}$. Because $W(N)$ is an increasing and concave function of *N*, I use the square root of the number of times a manager can be observed in ExecuComp database to proxy for the weight investors assigned to $AGG(N)$.[32] I use aggressiveness of an average manager as investor's prior belief about a manager's aggressiveness. That is, the mean fixed effect the managers exert on discretionary accruals, which equals zero. Intuitively, when a new manager issues her first financial report, investors consider her as a random draw from the entire population of all managers, controlling for firm characteristics. Therefore, this manager should not be relatively more conservative or more aggressive compared to all other managers. Equation (6) then becomes the following:

$$CAR_{-1,+1} = \gamma_0 + \gamma_1 News_{i,t} + \gamma_3 News_{i,t} \times M\_Effect_{j,t} \times W(N) + \sum \gamma\, Control\ Variable_{i,t} + \sum \gamma\, News_{i,t} \times Control\ Varible_{i,t} + \varepsilon_{i,t} \quad (8)$$

Based on Hypothesis 1, I expect γ_3 to be negative. This suggests that investors' discount on forecast news is larger when the manager has issued more financial reports and is perceived as more aggressive. The following example can illustrate the difference between a Bayesian model versus a static model.[33] Assume investors hold the same prior

[32] ExecuComp only collects data on managers who work for S&P1500 firms. Obviously, a manager's financial reports can also be observed by investors if she works for a non-S&P1500 public firm. However, when managers stay in S&P1500 firms, they have more visibility than in other firms. Hence, investors are more likely to use financial reports of S&P1500 firms to form belief about their managers. In addition, when investors do use other financial reports, which are not captured in my measure of financial reporting history, my measure will bias against finding results.

[33] This example is modified from an example in Chen et al (2005).

perceived aggressiveness for manager A, B and C, which equals 0. At time *t*, A has accumulated a financial reporting history of length *N*=500 with aggressiveness $M\_Effect_{j,t}$= 0.04, B has a financial reporting history of length *N*=10 and aggressiveness $M\_Effect_{j,t}$= 0.04, and C has a financial reporting history of length *N*=100 and aggressiveness $M\_Effect_{j,t}$= 0.01. A static model predicts the same discount on the market reaction to the most aggressive managers' forecast (A and B, each with aggressiveness of 0.04 at time t), and a smaller discount on the market reaction to the least aggressive manager C's forecast. In contrast, a Bayesian model predicts the following ordering of the discount on the market reactions at time t: A>C>B. Investors' discount on the forecast news is the smallest for manager B's forecasts because, while B is as aggressive as A, its short financial reporting history means that investors will not weight her financial reporting record heavily in forming their posterior beliefs; hence investors' posterior beliefs will be close to their prior belief for B. Investors discount A's forecast news the most because A's large *N* means that investors will weight A's financial reporting record heavily in forming their posterior; hence, investors' posterior beliefs are close to A's financial reporting record of 0.04. The discount on the forecast news of C lies between A and B, because investors posterior belief for C's aggressiveness will be close to 0.01 due to her relatively long financial reporting history. Therefore, investors' posterior belief of C's aggressiveness is larger than A's posterior (close to 0) but smaller than B's posterior (close to 0.04).

Note that $News_{i,t} \times M\_Effect_{j,t}$ is not a separate independent variable in Eq. (7) or Eq. (8). This highlights the distinction between a Bayesian model and a static model. The Bayesian model contains information about both the aggressiveness and the length of the financial reporting history observed at time *t*, and a static model conditions only on the manager's aggressiveness.

2.4. Sample and Data

2.4.1 Sample Construction and Fixed Effects Estimation

2.4.1.1 Sample Construction

Since I adopt the methodology of Bertrand and Schoar (2003) to isolate manager-specific effects on discretionary accruals, I follow their method to construct my sample. I start with all managers in the Standard and Poor ExecuComp database from 1992 to 2008. I require an individual manager to be observed in at least two firms and stay in each firm for at least three years. This three-year requirement ensures that managers have enough time to "make their mark" on a given company. I retain managers who once held a CEO or CFO title in their career history.[34] I focus on CEOs and CFOs because these managers are more likely to have a strong and direct effect on financial reporting. For each firm satisfying these requirements, I keep all observations, i.e., including years where this firm has managers that we do not observe in multiple firms.

As discussed before, to avoid the look-ahead bias in the market reaction test, I estimate these fixed effects by using information to date and then update the estimation by adding more financial information when firms issue more financial reports. For example, to estimate a manager's effect perceived in year 2005, I use financial reports of all firms and managers up to year 2004 to obtain discretionary accruals and then estimate the manager's fixed effect. For a manager's effect perceived in year 2006, I add the financial reports for year 2005 to get new estimates of discretionary accruals and update the manager's fixed effect estimate. So a manager's individual effect estimated in year 2005 differs from the one estimated in year 2006.

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[34] I use annual CEO flag variable *ANNCEO* from ExecuComp database to identify CEOs. I use both variables of *ANNCFO* and *TITLEANN* to identify CFOs. I classify a manager as CFO if he/she is flagged by *ANNCFO*. I also classify a manager as CFO if his/her title included the words "CFO," "Chief Finance Officer", "Chief Financial Officer," or "Chief of Finance."

Therefore, I have several time periods to select managers and estimate their fixed effects. Table 1 provides information on manager selection for periods ended in different years.[35] For the entire period 1992 to 2008, I obtain 1182 firms and 913 individual managers who can be observed in at least two different firms. Among these 913 managers, 620 managers currently hold or once held the CEO position and 365 managers currently hold or once held the CFO position.[36] On average, a manager stays at a firm for 5.37 years and stays at 2.08 firms during the entire sample period. Only 64 managers are observed in strictly more than two different firms, and the maximum number of firms a manager stays in is 5.

2.4.1.2 Manager Fixed Effects Estimation

After I obtain my sample for fixed effects estimation, I calculate discretionary accruals for each firm-year by using a time series modified Jones model as below.[37] The model includes change in operating cash flow because Dechow (1994) shows total accruals are negatively related to changes in operating cash flow.

$$TACC_{i,t} = \beta_0 + \beta_1(\Delta Sales_{i,t} - \Delta REC_{i,t}) + \beta_2 PPE_{i,t} + \beta_3 \Delta CFO_{i,t} + \varepsilon_{i,t}, \quad (3)$$

where:

$TACC_{i,t}$ = firm i's year t total accruals calculated as earnings from continuing operations minus cash flows from continuing operations earnings;

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[35] My ending years for different periods are 2001, 2002, ……, 2008. I choose these ending years because in my market reaction test, my earnings forecasts data are from period 2002-2009. Therefore, my sample periods to estimate manager fixed effects include 1992-2001, 1992-2002, …… , 1992-2008.

[36] The sum of CEOs and CFOs reported in the table does not equal to the total number of managers because some managers held both CEO and CFO positions during the sample period.

[37] Since I attribute part of the accrual variation in a certain firm to the change of managers, I believe the time-series model does a better job in capturing different managers' effect on a given firm's accruals over time. As a robustness test, I also estimate abnormal accruals by using cross-sectional model and find the same results.

$\Delta Sales_{i,t}$ = change in firm *i*'s sales from year *t* – 1 to year *t*;

$\Delta REC_{i,t}$ = change in firm *i*'s accounts receivables from year *t* – 1 to year *t*;

$PPE_{i,t}$ = firm *i*'s year *t* gross property, plant, and equipment;

all variables are scaled by lagged total assets.

For each firm in my sample, I obtain all observations from Compustat starting from 1988 because the necessary cash flow statement data for most firms are available starting in 1988. I then estimate the modified Jones model for each firm with at least 5 observations. Discretionary accruals are measured by subtracting the predicted accruals from total accruals, i.e., the error terms in the regression. I then merge discretionary accruals with observations in my sample so I can estimate manager fixed effects in the next step.

Since I use different sample periods to estimate manager fixed effects, I report the estimation results of the sample period ended in year 2008 as an example in Panel A and B of Table 2. Other years' results are very similar but untabulated. [38] In Panel A of Table 2, I report the regression results of equation (1) to (3). In the first row, the adjusted R^2 for equation (1) is 9.27%. In the second row, the adjusted R^2 for equation (2) is 13.05%. The last row reports regression result for model (3) and the adjusted R^2 increases to 17.30%.[39] The *F*-tests are reported in cells of columns labeled Firms and Managers to test the joint significance of the firm and manager fixed effects; all are large enough to reject the null hypothesis of no significant joint effects of these firm and managers. Panel B reports the

[38] The estimation results of other years are available from author upon request.

[39] Note that the adjusted R^2 here is higher than in DeJong and Ling (2010). The reason is that DeJong and Ling use command *xtreg, fe* in Stata to run fixed effect regressions, where the *adjusted* R^2 is calculated based on the within R^2. In their results, the firm fixed effects are not used to calculate the adjusted R^2. However, when *xtreg, fe* in Stata is used, Stata does not report firm fixed effect estimates in the regression results. In this study, since I need firm fixed effects as control variables in market reaction tests, I construct dummy variables for firms and include them in the regression to obtain the estimates. Therefore all the firm fixed effects are used to calculate the R^2.

distribution of the manager and firm fixed effect coefficients estimated from model (3). The mean and the median of manager fixed effects are very close to zero, which is consistent with how fixed effects are constructed and estimated. The results show that the variation in the size of manager and firm fixed effects is economically large. Replacing a manager at the 25th percentile with one at the 75th percentile increases the average discretionary accruals by 0.032. The difference between a firm at the 25th percentile and 75th percentile is 0.022. Overall, the estimation results show that both firm-specific and manager-specific effects matter statistically and economically for accruals, which is consistent with prior literature.

After the estimation of all sample periods, for a given manager, I obtain the manager's fixed effect coefficient for each year and this proxies for the manager's aggressiveness perceived by investors in the following year. Therefore, a manager's effect changes from year to year, which is consistent with the claim that investors are updating a manager's perceived style as more information is available. For the period from 2001 to 2008, I obtain individual effects for 821 managers from 997 firms. By construction, in this panel dataset, a manager's effect in one year is highly correlated with the one in the next year because their estimation periods overlap. In Table 3 Panel A, I show the association between a manager's effects estimated in the past five years and the one estimated in the current year. The dependent variable is a manager's fixed effect estimated in year *t*. Each column represents a model with the manager's fixed effect estimated in a prior year as the independent variable. All regressions include year fixed effects and the standard errors are clustered at the manager level. I find the coefficient on any past year's manager effect is significantly positive, and as the time period between two different years becomes longer, the positive association decreases and the explanatory power of the model becomes smaller.

As a comparison, I also examine the association between firm fixed effects estimated in the past years and in the current year and the results are reported in Panel B

of Table 5. The results are similar to Panel A. Interestingly, the positive correlations between firm fixed effects estimated in past years and in the current year are much smaller. The explanatory power of all the models is much lower as well. Further comparison tests show that these differences are statistically significant.[40] These results suggest when adding new information, the updated firm fixed effect is less stable than manager fixed effects. This is probably because a manager's effect is due to her personal preference and cognitive bias, which are relatively stable personal characteristics. The firm fixed effect, however, reflects a firm's characteristics such as operating cycle and business environment, which can fluctuate over time. Therefore, by updating their belief about managers, investors are likely to obtain relatively persistent inferences about managers.

Lastly, I require these managers to be observed in the ExecuComp database during the period 2002 to 2009.[41] This requirement leads to a dataset which contains 640 managers in 551 firms from 2002 to 2009. I then merge these firms and managers with earnings forecast data.

2.4.2 Earnings Forecast Data

I obtain earnings forecast data from the I/B/E/S Guidance File. I use the Historical Detail Guidance File, which started in December 2002.[42] Although I/B/E/S Guidance

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[40] I also examine Pearson correlation between fixed effects estimated in past years and the current year. The untabulated results are very similar. The Pearson correlations between firm fixed effects across years are also much lower than the ones between manager fixed effects across years. The Fisher's Z tests indicate these differences are statistically significant as well. These results are available upon request.

[41] I choose this period because I will obtain earnings forecasts data for period from 2002 to 2009. Section 4.2 discusses this choice of time period in more details.

[42] The I/B/E/S Guidance File contains management forecasts of earnings and other items such as cash flows, revenues, and capital expenditures. The file attempts to capture guidance for all firms covered in the I/B/E/S database. Some content dates back to 1994, but the number of observations and perhaps the depth of coverage increase over time.

provides other files covering earlier years, the usage of the Historical Detail Guidance File eliminates effects from privately issued guidance before Regulation Fair Disclosure (Regulation FD), and the coverage appears to be more complete than in earlier years. In addition, starting the market reaction test from December 2002 gives me at least ten years to estimate manager fixed effects. This is also the time when the capital market started to pay more attention to the aggressiveness of financial reporting and accounting discretion exerted by executives due to the accounting scandals.

The initial sample from the Historical Detail Guidance File includes 69,340 earnings forecasts by 3,685 firms. I then exclude observations when a firm makes multiple forecasts on the same day (20,105 forecasts) to insure the independence of observations and to allow interpretation of my market reaction test. I also exclude forecasts issued within a three-day window centered on earnings release dates (27,617 forecasts). These exclusions lead to 21,618 forecasts for 3,158 firms. I then merge these earnings forecasts data with managers who can be observed during the time period from 2002 to 2009 and also have fixed effects estimates. When merging earnings forecasts with a manager's perceived aggressiveness in year *t*, I require earnings forecasts to be issued after year *t-1*'s earnings announcement date and before year *t*'s earnings announcement date. I delete observations with missing CRSP and Compustat data. My final sample consists of 1,832 forecasts by 295 managers from 246 firms with 2,050 total observations.

2.5 Empirical Results

2.5.1 Descriptive statistics

Table 4 Panel A reports descriptive statistics for the final sample. The average three-day market response to earnings forecast news is -0.003 and the median is -0.026. Interestingly, the average (median) manager effect of the final sample is -0.004 (-0.014). Since the average (median) manager effect estimated from the initial manager effect

estimation sample is very close to zero by construction, this result implies that managers who issue earnings forecasts may have lower discretionary accruals than managers who do not issue earnings forecasts. The average (median) firm fixed effect is 0.019 (-0.023). For the firm characteristics variables, the average book-to-mark is 0.463, size is 8.657, and ROA is 0.056. These firm characteristics are similar to prior studies using Bertrand and Schoar's (2003) methodology. For the earnings forecasts observations in my sample, 83.6% forecasts are range forecasts, while 58.5% of them are annual forecasts. On average, an earnings forecast is issued 133 days before the fiscal period end, and the average news is -0.003.

Table 4 Panel B reports the Pearson correlations for these variables. In general, the correlations are low and the significant ones are consistent with prior studies. The notable exceptions are the positive correlation between *ROA* and *Special* and the positive correlation between *Horizon* and *Annual*, both of which are expected.

2.5.2 Test of Hypothesis 1

Table 5 provides regression results for the test examining whether the market can recognize and adjust for a manager's individual effect on discretionary accruals. The model is estimated using ordinary least squares (OLS) and I cluster the error term at the firm level. Explanatory power (R^2) of the model is 11.54% and the adjusted R^2 is 10.38%.

Consistent with prior studies, the market response is positively associated with forecast news. The coefficient on *News* is positive and significant. For the manager individual effects, I find the coefficient on *News* × *M_Effect* is significantly negative at the 1% level. This result suggests that investors can recognize managers' financial reporting aggressiveness and take into account managers' individual effects when reacting to earnings forecasts. Specifically, the capital market's response is weaker for earnings forecasts issued by more aggressive managers. This is consistent with my prediction that more aggressive managers are considered by investors as less credible.

Therefore, investors tend to discount the new information contained in earnings forecasts issued by aggressive managers. In addition, I have already controlled for the firm fixed effect on accruals to assure investors are responding to a manager-specific effect instead of a firm-specific effect. My result suggests investors can separate manager effects from firm effects on discretionary accruals.

With regard to other control variables, the significant coefficients are consistent with prior literature. The positive coefficients on *News * ROA* is consistent with investors reacts more strongly to earnings forecasts issued by firms with better performance. The negative coefficient on *News *Specificity* is consistent with point forecasts being more informative relative to range forecasts. The coefficient on *News*Special* is negative and significant, which suggests that investors view special items as more transitory.

2.5.3 Test of Hypothesis 2

Table 6 provides regression results for the test examining whether the capital market's recognition of a manager's style follows a Bayesian learning process. The focus is the coefficient for the term *News*M_Effect*W (N)*. This coefficient provides evidence on whether investors' reaction to earnings forecast news is correlated with both the average effect a manager exerts on discretionary accruals and the length of the manager's financial reporting history.

Hypothesis 2 predicts investors place more weight on a manager's perceived aggressiveness and less weight on their prior beliefs as the number of the manager's financial reports increases. With Hypothesis 1 results which show that investors respond less strongly to earnings forecast issued by more aggressive managers, I expect the coefficient on *News*M_Effect*W (N)* to be negative. The negative coefficient suggests that as a manager's financial reporting history becomes longer, the discount investors impose on earnings forecast news because of the manager's aggressiveness becomes larger. Consistent with my prediction, I find a significant and negative coefficient on

*News*M_Effect*W (N)*. This result is consistent with investors acting as Bayesian decision-makers. They follow a Bayesian process when learning about a manager's financial reporting style.

2.6 Additional Discussion and Analyses

2.6.1 Managers' Active Role or Firms' Choice of Managers

As discussed in Bertrand and Schoar (2003), there are two distinct interpretations as to how individual managers affect corporate choices. The first is a manager actively imposes her own idiosyncratic style on a firm. The second one is that managers do not impose their idiosyncratic style on the firm they lead, but are purposefully chosen by firms because of their specific attributes. While the robustness tests in Bertrand and Shoar (2003) show that managers do play an active role in shaping corporate policies, a recent working paper, Fee, Hadlock and Pierce (2011), argues that these managerial style effects detected using Bertrand and Schoar's (2003) method are due to the endogenous choice made by firms to simultaneously make operational, governance, and managerial changes. Since this study is built on the fact that in equilibrium individual managers do matter in the determination of firm policies, I do not distinguish between these different interpretations since either one is consistent with the fact that individual managers are central in bringing about the changes in corporate policies.

2.6.2 Style of Financial Reporting or Real Decisions

While I use manager fixed effects on accruals to proxy for managers' financial reporting aggressiveness, one may argue that a manager's fixed effect on accruals is due to her style on real decisions. For example, some managers are more specialized in growing firms and therefore are more likely to be hired by firms which need to grow. In this case, the manager's style on accruals captured by fixed effect is probably not due to her personal style on financial reporting but her style on growing firms. For these

managers, if their choice of earnings forecasts is driven by firm growth, then the correlation between their fixed effects on accruals and the market reaction to their earnings forecasts is due to their endogenous choice of growing firm and issuing earnings forecasts, but not due to investors' recognition of their financial reporting style.

To examine whether this is the case, I investigate firms' characteristics (financial distress, merger and acquisition activities and sales growth) and earnings forecast characteristics (horizon, specificity and whether it is annual earnings forecast) to see whether there are correlations between these characteristics and manager fixed effects on accruals. I classify managers into conservative and aggressive managers based on their fixed effects on accruals and examine whether these two types of managers stay in firms with significantly different characteristics or issue significantly different types of earnings forecasts. Untabulated results show that managers with different styles on accruals distribute evenly across firms with different characteristics and do not issue different types of earnings forecast.

2.6.3 Approximate Randomization Test

Another concern raised by Fee et al. (2011) is that the significance of the F-test when testing for individual effects is overstated. Fee et al. randomly assign managers to firms and still find significant managerial style effects. They conclude that the significance of the F-test for testing manager fixed effects alone cannot be used to conclude that managers exert significant individual effects on firms. Although I do not interpret the significance of the F-test, the results raise the possibility that similar results could be obtained from a random assignment of managers to firms. Therefore, to investigate whether manager fixed effects in my study capture meaningful individual manager styles, I conduct an approximate randomization test.

I randomly assign the estimated manager fixed effects to the managers in my sample and then estimate equation (4) to obtain the coefficient for *News*M_Effect*. I

repeat this procedure 1000 times. Consistent with the null hypothesis that there is no association between the market reaction to earnings forecasts and a manager's financial reporting style, I find that the mean coefficient for *News*M_Effect* is not significantly different from zero. This suggests that my manager fixed effect estimates do capture managers' personal style, as there is no pricing of managerial style when manager fixed effects are randomly assigned to managers. I also compare the coefficient of *News*M_Effect* from my original estimation of equation (4) to the distribution of the coefficient obtained from the randomization procedure. The p-value of my original estimation based on the distribution generated by the randomization procedure is 0.012, which is consistent with my original result.[43]

2.6.4 Improvement of Manager Fixed Effect Estimation

Estimates of manager fixed effects are calculated using the information available at a point of time and are updated as firms issue more financial reports. My result shows that investors follow a Bayesian learning process to identify a manager's individual style. As a manager's financial reporting history becomes longer, there is less uncertainty about the manager's true style. However, I use ExecuComp to identify a manager's employers. The ExecuComp database starts in 1991 and collects information for S&P1500 firms. Therefore, an alternative explanation for my result is that investors have more information about a manager than the database provides, and my estimation of fixed effects becomes better and more accurate in terms of proxying for investors' belief when I obtain more information from the database.

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[43] The significance level of the coefficient from the original estimation is determined as: (NGE + 1) / (NS +1), where NGE is the number of randomized coefficients smaller than or equal to the original coefficient and NS is the number of random shuffles of the dependent observations (i.e., 1,000).

To examine whether my result for Hypothesis 2 is due to this alternative explanation, I partition managers into terciles based on their fixed effects estimated in each year. I label these partitions: aggressive, neutral and conservative. I retain the sub-sample of managers who never change type through the entire sample period. These managers have a relatively stable managerial fixed effect during the sample period. Focusing on these managers suggests that my estimation of their personal style does not change much as the length of the financial reporting history in the database increases. I use this sub-sample of managers to estimate equation (8) to test Hypothesis 2 and still find the same result, i.e., the coefficient for *News*M_Effect*W (N)* is significantly negative. This confirms my conclusion that investors follow a Bayesian learning process to identify a manager's individual style.

2.7 Conclusion

This study examines whether investors recognize differences in managers' financial reporting behavior. Specifically, I investigate whether the capital market can recognize a manager's financial reporting aggressiveness and whether investors' recognition of a manager's style follows a Bayesian learning process. Following Bertrand and Schoar (2003), I estimate manager-specific effects on discretionary accruals and use these manager effects to measure manager financial reporting aggressiveness. I examine whether investors take into account individual manager effects when reacting to the news in earnings forecasts. My results show that investors find earnings forecasts issued by aggressive managers to be less credible and thus respond less strongly to these managers' earnings forecasts.

In addition, I show that investors follow a Bayesian process when learning a manager's individual style. Specifically, as a manager's financial reporting history becomes longer, there is less uncertainty about the manager's true style. Consequently,

the discount on the market reaction to earnings forecast news due to the manager's aggressiveness becomes larger.

I contribute to accounting research on managers' financial reporting styles. While prior studies document the existence and significance of individual manager styles, they rarely examine whether the capital market recognizes and responds to a manager's style on financial reporting. My paper extends these studies to show that investors are willing and able to identify a manager's aggressiveness on financial reporting. I also provide evidence that manager financial reporting style, which reflects their personal preference and cognitive bias, has implications for the credibility of financial disclosure.

In conclusion, this study establishes that a manager's financial reporting aggressiveness affects the credibility of her earnings forecasts. This result suggests that managers benefit from developing financial reporting reputation since the capital market can recognize their personal styles. The evidence that aggregate-market behavior is consistent with Bayesian learning shows that investors are able to process historical information and infer about a manager's individual style in a rational way. This implies firms can also benefit from hiring managers with favorable reputation to help impound firms' forward-looking information into stock price.

Table 2-1 Manager Selection for Periods Ended across Different Years

Ending Year	Firms	Managers	CEOs	CFOs
2001	549	358	255	118
2002	625	426	300	148
2003	743	513	358	184
2004	844	596	406	226
2005	922	670	458	258
2006	996	744	503	292
2007	1053	798	547	308
2008	1182	913	620	365

Note: This table reports manager selection for sample periods ended across different years. For example, for year 2001, the sample period is from year 1992 to 2001; for year 2008, the sample period is from year 1992 to 2008. For each sample period, I require a manager to be observed in at least two firms and stay in each firm for at least three years. I retain managers who once held a CEO or CFO position in their career history. CEOs are managers who once held a CEO position and CFOs are managers who once held a CFO position.

Table 2-2 Manager Fixed Effects on Discretionary Accruals

Panel A: Test of Manager Fixed Effects on Accruals

Model	*F*-tests on fixed effects for			
	Firm	Manager	N	*Adj.* R^2
(1) Year, Firm, Controls	2657.71 (*p*<0.001)		19148	9.27%
(2) Year, Manager, Controls		7.9e+05 (*p*<0.001)	19148	13.05%
(3) Year, Firm, Manager, Controls	2.3e+05 (*p*<0.001)	9789.74 (*p*<0.001)	19148	17.30%

Panel B: Size Distribution of Manager and Firm Fixed Effects

Fixed Effects	N	Mean	Std	Min	P25	Median	P75	Max
Manager	816	0.000	0.088	-0.789	-0.019	-0.002	0.013	1.725
Firm	982	-0.061	0.049	-0.587	-0.073	-0.062	-0.051	0.562

Note: Panel A and Panel B report the results from the fixed effects panel regression for period ended in 2008, i.e., period 1992 to 2008. Sample is the manager-firm matched panel data set as described in subsection 4.1. Panel A reports the regression results for a variation of model (1) and *F*-tests for the joint significance of firm and manager fixed effects. For each F-test, I report the value of the *F*-statistic and the *p*-value. Panel B reports the size distribution of manager and firm fixed effects estimated from model (1), where manager, firm and year fixed effects and control variables are all included.

Table 2-3 Correlation between Fixed Effects Estimated in Prior Years and the Current Year

Panel A: Manager Fixed Effects

	Prediction	Parameter Estimate				
M_Effect_{t-1}	+	1.001***				
M_Effect_{t-2}	+		0.997***			
M_Effect_{t-3}	+			0.884***		
M_Effect_{t-4}	+				0.853***	
M_Effect_{t-5}	+					0.812***
Adj. R^2		92.38	85.09	58.09	52.17	40.8
Obs.		3797	3057	2370	1746	1196
Cluster		740	687	624	550	474

Panel B: Firm Fixed Effects

	Prediction	Parameter Estimate				
F_Effect_{t-1}	+	0.822***				
F_Effect_{t-2}	+		0.736***			
F_Effect_{t-3}	+			0.670***		
F_Effect_{t-4}	+				0.557**	
F_Effect_{t-5}	+					0.339**
Adj. R^2		70.83	50.78	40.03	27.26	21.57
Obs.		4753	3859	3018	2235	1526
Cluster		895	842	783	710	608

Panel C: Comparison of Parameter Estimate in Panel A and B

t-statistic	-18.65	-16.77	-7.5	-10.54	-13.02
p-value	<0.000	<0.000	<0.000	<0.000	<0.000

Note: This table reports regression analysis of the relation between fixed effects estimated in prior years and the current year. Sample is the manager-firm matched panel data set used to estimate manager fixed effects, i.e., it is the initial sample without requiring managers to be observed in 2002 to 2009 and have earnings forecasts data. Sample includes 997 firms and 821 managers. Panel A shows the results of regressions with manager fixed effects estimated in year *t* as the dependent variable. Each column represents a regression with manager fixed effects estimated in a prior year as the independent variable. All regressions include year fixed effects and the standard errors are clustered at the manager level. Panel B shows the results of regressions with firm fixed effects estimated in year *t* as the dependent variable. Each column represents a regression with firm fixed effects estimated in a prior year as the independent variable.

All regressions include year fixed effects and the standard errors are clustered at the firm level. Significance at the 10%, 5%, and 1% level are denoted *, **, and ***.

Table 2-4 Summary Statistics for Earnings Forecasts Sample

Panel A: Descriptive Statistics

Variable	Mean	Std Dev	P25	Median	P75
CAR	-0.003	0.062	-0.273	-0.026	0.002
M_Effect	-0.004	0.028	-0.132	-0.014	-0.003
F_Effect	0.019	0.067	-0.118	-0.023	0.000
BM	0.463	0.269	0.037	0.270	0.417
Size	8.657	1.494	4.322	7.523	8.657
ROA	0.056	0.083	-0.421	0.028	0.056
Special	-0.001	0.012	-0.112	-0.001	0.000
News	-0.003	0.062	-0.273	-0.026	0.002
Specificity	0.836	0.371	0	1	1
Horizon	0.366	0.318	0.016	0.121	0.249
Annual	0.585	0.493	0	0	1

Table 2-4 Continued

Panel B: Correlation Analysis

	CAR	M_Effect	F_Effect	BM	Size	ROA	Special	News	Specificity	Horizon	Annual
CAR	1.000	0.005	-0.009	-0.038	**0.067**	0.007	-0.042	**0.251**	-0.007	**0.095**	**0.058**
M_Effect		1.000	**-0.193**	**0.047**	-0.008	**0.105**	**0.133**	-0.010	-0.027	-0.010	-0.020
F_Effect			1.000	-0.033	0.023	0.009	0.023	0.004	**-0.056**	0.004	-0.023
BM				1.000	**-0.320**	**-0.421**	**-0.136**	**-0.049**	-0.007	0.039	-0.021
Size					1.000	**0.240**	**0.127**	**0.082**	-0.039	**0.059**	**0.010**
ROA						1.000	**0.660**	**0.083**	0.038	-0.031	-0.010
Special							1.000	-0.002	0.015	0.004	-0.010
News								1.000	**-0.047**	0.040	**0.056**
Specificity									1.000	**0.173**	**0.097**
Horizon										1.000	**0.608**
Annual											1.000

Note: This table reports summary statistics for the earnings forecasts sample. Sample is the final manager-firm matched sample as constructed in subsection 4.1 and then matched with earnings forecasts data. The sample period is from year 2002 to 2009. Sample includes 1832 forecasts and 295 managers from 246 firms. Variable definitions are reported in the appendix C. Panel A reports descriptive statistics for all manager-firm-year observations. Panel B presents Pearson correlations. Correlations above 0.5 are highlighted and correlations significant at the 5% level are marked in bold.

Table 2-5 Regression Analysis of the Market Response to Earnings Forecast News

Parameter	Prediction	Estimate
News	+	2.894***
M_Effect	?	-0.019
News*M_Effect	-	-20.696***
F_Effect	?	-0.016
BM	?	-0.007
Size	?	0.002**
ROA	?	0.019
Special	?	-0.102**
Specificity	?	-0.002
Horizon	?	0.018***
Annual	?	-0.003
News*F_Effect	?	1.161
News*BM	-	-0.651*
News*Size	+	0.133
News*ROA	+	4.920***
News*Special	-	-13.703***
News*Specificity	-	-2.506***
News* Horizon	-	-0.491
News*Annual	-	0.091
Year Fixed Effect		Included
N		2009
Cluster		240
R^2 (*Adj.* R^2)		11.54%(10.38%)

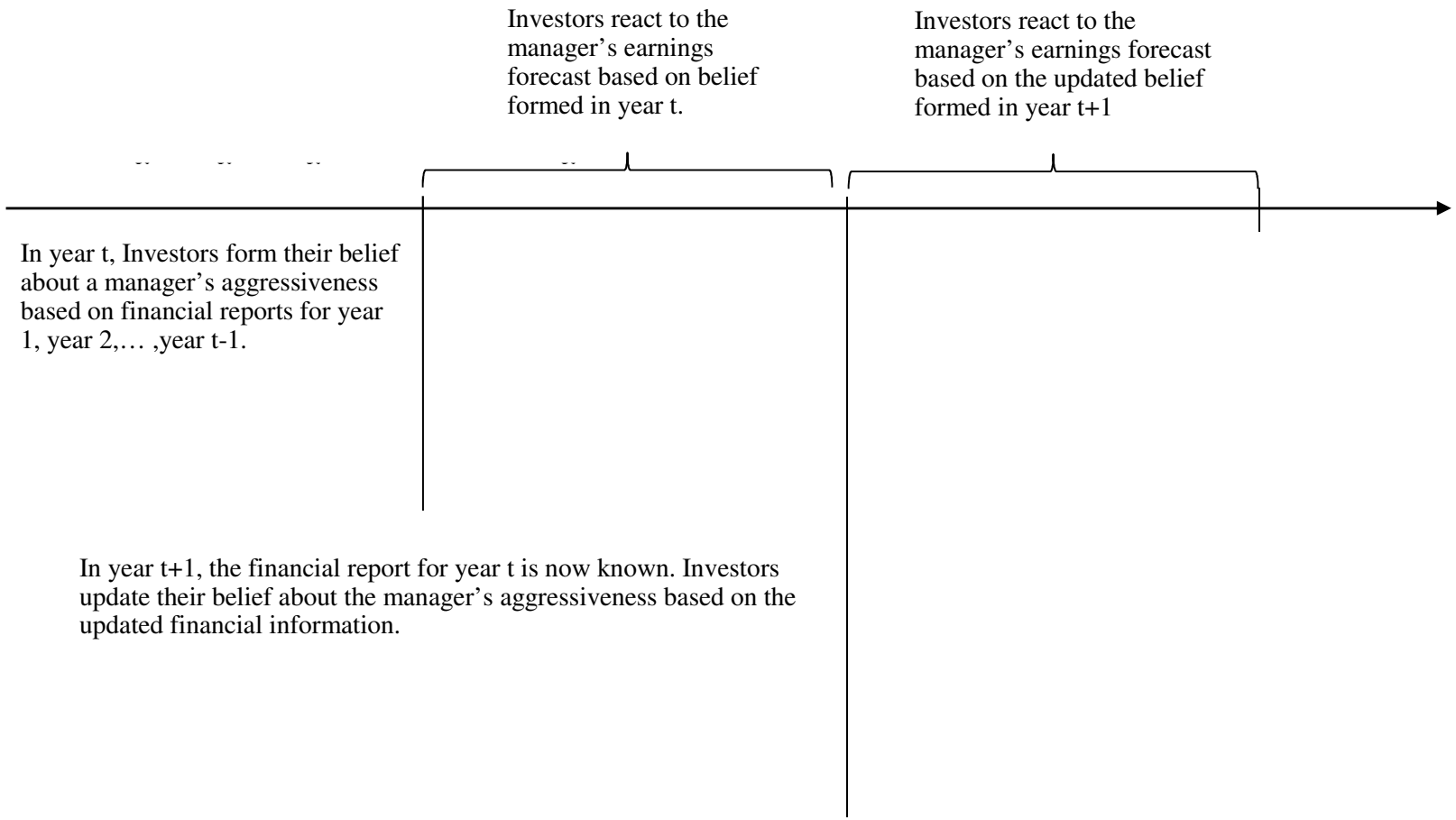
Note: This table reports the regression analysis for the market reaction test. Sample is the final manager-firm matched sample, where managers can be observed and issue earnings forecasts in 2002 to 2009. Sample includes 1832 earnings forecasts and 295 managers from 246 firms. The dependent variable is three-day cumulative adjusted returns centered on the forecast issuance date (*CAR*). All the independent variables' definitions are reported in the appendix C. Significance at the 10%, 5%, and 1% level are denoted *, **, and ***.

Table 2-6 Regression Analysis of Investors' Bayesian Learning Process

Parameter	Prediction	Estimate
News	+	1.842***
News*M_Effect*W(N)	-	-3.890**
F_Effect	?	0.003
BM	?	5.020
Size	?	-0.007***
ROA	?	0.003
Special	?	0.021
Specificity	?	-0.070
Horizon	?	-0.003***
Annual	?	0.020
News*F_Effect	?	-0.001
News*BM	-	-0.468
News*Size	+	0.259*
News*ROA	+	3.674**
News*Special	-	-10.665**
News*Specificity	-	-2.516***
News* Horizon	-	-0.743
News*Annual	-	0.049
Year Fixed Effect		Included
N		1755
Cluster		221
R^2 (*Adj.* R^2)		11.35% (10.07%)

Note: This table reports the regression analysis for the market reaction test. Sample is the final manager-firm matched sample, where managers can be observed and issue earnings forecasts in 2002 to 2009. Sample includes1832 earnings forecasts and 295 managers from 246 firms. The dependent variable is three-day cumulative adjusted returns centered on forecast issuance date (*CAR*). All the independent variables' definitions are reported in the appendix C. *W(N)* represents the square root of the number of times a manager can be observed in the ExecuComp database and is a proxy for the weight investors assigned to a manager's perceived aggressiveness. Significance at the 10%, 5%, and 1% level are denoted *, **, and ***.

Figure 2-1 Timeline of Investors' Recognition and Reaction to Manager Style



CHAPTER 3 EXECUTIVE COMPENSATION IN A MATCHING MODEL

3.1 Introduction and Motivation

Starting from the seminal work of Altonji and Shakotko (1987) and Topel (1991), a long-debated issue in the labor economics literature pertains to the relative importance of experience in the labor market and seniority in a firm in explaining wage growth over the life cycle and the resulting earnings differences across individuals. Traditionally, earnings differences across individuals have been attributed to differences in their education and, more recently to differences in their family background and skill endowment (see Cunha and Heckman, 2007). However, in addition to the role of schooling and other individual and family characteristics, growing empirical evidence has pointed out the importance of labor market participation for wage growth, in particular its timing over the life cycle, and the centrality of job mobility to explaining returns to labor market experience. For instance, Rubinstein and Weiss (2007) have documented that wage growth, which happens mainly early in the life cycle, is associated with increasing labor force participation and high job mobility. They estimate that wage growth during the first decade in the labor market is approximately 50% for high school graduates and approximately 80% for individuals with a college degree or more.

Despite its quantitative significance, no clear consensus exists as to the extent to which life-cycle wage growth can be attributed to general experience in the labor market or to specific experience in a given industry or occupation or within a firm. Further, in light of emerging evidence on differences in such returns among individuals of different skill and in different occupations (see Dustmann and Meghir (2005) and Kambourov and Manovski (2009)), a natural question is the extent to which differences in the returns to general experience and firm seniority arise due to differences in magnitude within a given occupation.

A common difficulty to addressing this question is data availability, on one hand, and endogeneity of sample information, on the other. Indeed, a challenge to empirically documenting the importance of firm seniority for wage growth in any given occupation is the lack of detailed information regarding firm and job characteristics in commonly used panel datasets, like the Panel Study of Income Dynamics (PSID) (for an illustration of the issues in the context of the PSID, see Altonji and Williams (2005) and Buchinsky, Fougère, Kramarz, and Tchernis (2010)). Moreover, as decisions about labor market participation and employment in a given occupation, industry, or firm are made by individuals in their best interests, the non-randomness of information on employment histories poses well-known challenges to the measurement of returns to experience and seniority.

Prior literature and the common difficulty in obtaining relevant data provide opportunity for us to examine an unexplored area: executive labor market. In this study, we consider the market for executives and, using an original dataset that combines detailed characteristics on the employment histories of executives and the firms employing them, we document the compensation patterns of a large sample of U.S. executives. We then turn to empirically investigate their sources. Specifically, we first examine the empirical determinants of job mobility and of the dynamics of compensation of executives in order to assess the magnitude of their returns to firm tenure and the importance of turnover for compensation growth. Second, we contrast results obtained by employing standard tools of applied analysis with those derived from applying more advanced techniques that take into account the endogeneity of mobility decisions on the part of executives, the cumulative effect of mobility on compensation, and the selection of executives to top positions within a firm based on unobserved and possibly time-varying characteristics. Third, treating the allocation of firm value among executives and other stakeholders as a standard joint consumption problem (see Bourguignon, Browning, and Chiappori (2009) and Browning, Chiappori, and Lewbel (2010)), we prove that a

measure of the implied value sharing rule, as embedded in the observed total compensation of an executive, can be recovered. We then explore the extent to which variation in this sharing rule over time for the same firm and across firms is responsible for the observed time profile of executive compensation and for differences in executive pay across firms of different market capitalizations.

Our preliminary results show that common estimates of the importance of individual and firm characteristics for executive pay that do not take explicitly into account mobility or individual and firm heterogeneity are severely biased. We suggest a number of estimators that attenuate this bias and evaluate their performance.

3.2 Model of Returns to Tenure and Experience

3.2.1 Topel (1991)

We start by reviewing Topel (1991)'s seminal work on measuring returns to firm tenure and labor market experience, as the paper provides an influential implementation of a standard model of wage determination. We treat his work as the benchmark against which we compare more recent contributions to the literature on returns to tenure and experience as well as our work.

3.2.1.1 Setup

Consider the following prototype model of wage determination

$$y_{ijt} = X_{ijt}\beta_1 + T_{ijt}\beta_2 + \epsilon_{ijt} \quad (1)$$

where y_{ijt} denotes the (log) wage for individual i on job j at time t, X_{ijt} is total labor market experience, and T_{ijt} is current job tenure (seniority). The parameters $\beta_1$and β_2 represent average returns to an additional year of either experience or tenure, respectively. The most popular interpretation of (1) is that β_1 represents the return on general human capital (training and the like) that accumulates with experience, while β_2 represents the return on accumulated job-specific capital that would be lost if a job were to end. Biases in estimating these returns are generated by the covariance between the

regressors and the unobservables, ϵ. Topel's main concern is with covariance that is the outcome of optimizing behavior, as workers seek to locate and maintain a productive (high-wage) employment relationship. Thus, one can decompose the unobservables as

$$\epsilon_{ijt} = \phi_{ijt} + \mu_i + \upsilon_{ijt} \tag{2}$$

where ϕ_{ijt} represents the stochastic component of wages that may be specific to a worker-firm pair, and μ_i is a person-specific effect that accounts for unobserved differences in earning capacity across individuals (e.g., 'ability'). The terms υ_{ijt} account for marketwide random shocks as well as measurement error that is known to plague survey data. Topel assumes that the components of (2): (a) are mutually orthogonal; (b) μ_i and υ_{ijt} are orthogonal to the regressors in (1) (for now).

Notice that fixed 'job effects' ($\phi_{ijt} = \phi_{ij}$) are a special case of (2) in which the specific value of a job does not evolve over time. This component captures the notion of a 'good match' in the sense of wages that are higher than what a worker could obtain elsewhere. It will generate bias in estimating (1) if ϕ is correlated with experience or job tenure. Correspondingly, let the auxiliary regression of ϕ on the observables be

$$\phi_{ijt} = X_{ijt}b_1 + T_{ijt}b_2 + \mu_{ijt} \tag{3}$$

In light of (3), since

$$y_{ijt} = X_{ijt}\beta_1 + T_{ijt}\beta_2 + \epsilon_{ijt} = X_{ijt}\beta_1 + T_{ijt}\beta_2 + \phi_{ijt} + \mu_i + \upsilon_{ijt}$$

we can express y_{ijt} as

$$\begin{aligned} y_{ijt} &= X_{ijt}\beta_1 + T_{ijt}\beta_2 + X_{ijt}b_1 + T_{ijt}b_2 + u_{ijt} + \mu_i + \upsilon_{ijt} \\ &= X_{ijt}(\beta_1 + b_1) + T_{ijt}(\beta_2 + b_2) + \mu_{ijt} + \mu_i + \upsilon_{ijt} \end{aligned} \tag{4}$$

Least squares applied to (1) will yield biased estimates of $\beta_1$and β_2 since $E\widehat{\beta_1} = \beta_1 + b_1$and $E\widehat{\beta_2} = \beta_2 + b_2$ (however, $\beta_1 + b_1$ and $\beta_2 + b_2$ are consistently estimated if μ_i and ν_{ijt} are orthogonal to X_{ijt} and T_{ijt}). Topel (1991) suggests the following two-step procedure to correct for the bias in $\widehat{\beta_2}$, the main parameter of interest.

First Step. Within-job wage growth can be analyzed from the first differences of (1) for persons who do not change jobs, which eliminates fixed job and individual effects. Specifically,

$$y_{ijt} - y_{ijt-1} = X_{ijt}(\beta_1 + b_1) + T_{ijt}(\beta_2 + b_2) + \mu_{ijt} + \mu_i + \upsilon_{ijt} - X_{ijt-1}(\beta_1 + b_1) - T_{ijt-1}(\beta_2 + b_2) - \mu_{ijt-1} - \mu_i - \upsilon_{ijt-1} \quad (5)$$

$$= \beta_1 + \beta_2 + b_1 + b_2 + \mu_{ijt} + \upsilon_{ijt} - \mu_{ijt-1} - \upsilon_{ijt-1} \quad (6)$$

since $\Delta X = X_{ijt} - X_{ijt-1} = 1$ and $\Delta T = T_{ijt} - T_{ijt-1} = 1$. From the model in (1) in first differences

$$y_{ijt} - y_{ijt-1} = \beta_1 + \beta_2 + \epsilon_{ijt} - \epsilon_{ijt-1} \quad (7)$$

it follows that if job effects are fixed, that is, $\phi_{ijt} = \phi_{ijt-1}$, then (6) specializes to

$$y_{ijt} - y_{ijt-1} = \beta_1 + \beta_2 + \mu_{ijt} + \upsilon_{ijt} - \mu_{ijt-1} - \upsilon_{ijt-1} .$$

Remark 1. *The first step of Topel's two-stage estimation would not be warranted in general if job effects were time-varying*.

If $\epsilon_{ijt} - \epsilon_{ijt-1}$ has mean zero ($\phi_{ijt} = \phi_{ij}$ but Topel's favored interpretation is that ϕ_{ijt} follows a random walk with mean-zero innovations), then least squares applied to (7) will yield a consistent estimate of average within-job wage growth (note, however, that mobility decisions may also generate selection in (7) because only acceptable values of $\epsilon_{ijt} - \epsilon_{ijt-1}$ are observed).

Remark 2. *The endogeneity of mobility decisions may affect the estimation of $\beta_1 + \beta_2$, that is, estimates may not be consistent*.

Second Step. Given (7), an estimate of β_1 can be obtained from initial wages on new jobs,

$$y_{ijt} = X_{0jt}\beta_1 + \phi_{ij} + \mu_i + \upsilon_{ijt} \quad (8)$$

where X_0 is initial experience on the job. Observe that the error term in (8) is nonrandom because only acceptable new job offers are observed. For example, ϕ and X_0 are positively correlated if expected match quality rises with time in the market. One approach to this problem is to explicitly model the mobility decisions that underlie this

selection bias, in which case standard sample selection corrections (e.g., Heckman 1976) might be applied. With this strategy, identification relies crucially on distributional assumptions (wage offers must be normally distributed), as well as on other (strong) restrictions (Topel 1986).

According to Topel, a more robust alternative is simply to acknowledge the selection bias implicit in (8) and to treat $\widehat{\beta_1 + \beta_2} - \widehat{\beta_1}$ as an estimate of the return to seniority. In particular, since $X \equiv X_0 + T$, letting $B = \beta_1 + \beta_2$ implies that (1) can be rewritten as

$$y_{ijt} = X_{ijt}\beta_1 + T_{ijt}\beta_2 + \epsilon_{ijt} = (X_{0jt} + T_{ijt})\beta_1 + T_{ijt}\beta_2 + \epsilon_{ijt}$$
$$= X_{0jt}\beta_1 + T_{ijt}(\beta_1 + \beta_2) + \epsilon_{ijt} = X_{0jt}\beta_1 + T_{ijt}B + \epsilon_{ijt}$$

or

$$y = X_0\beta_1 + TB + \epsilon \tag{9}$$

By using the first-step estimate $\widehat{\beta_1 + \beta_2}$ of $\beta_1 + \beta_2$ from (4), we then obtain

$$y - T\hat{B} = X_0\beta_1 + TB + \epsilon - T\hat{B} = X_0\beta_1 + e \tag{10}$$

where $e = T\left(B - \hat{B}\right) + \epsilon$. Topel notices that (10) is preferable to (8) because it makes use of data from all periods of all jobs.

3.2.1.2 Implementation

The first step is implemented as follows. Note first that if the evolution of wages within jobs follows a random walk, then the residuals of the wage growth model are serially independent and least squares applied to (7) is an efficient estimator. As in Topel and Ward (1991), Topel's examination of the time-series properties of within-job wage changes yields two important conclusions:

(1) Topel finds no evidence of positive serial correlation in within-job wage innovations, $\epsilon_{ijt} - \epsilon_{ijt-1}$. This is a strong finding since one might expect that some types of jobs offer steeper wage profiles than others. This lack of serial correlation implies that heterogeneity in permanent rates of wage growth among jobs is empirically unimportant;

(2) Topel finds that the within-job evolution of the wage has a strong permanent component that closely approximates a random walk, so the residuals satisfy

$$\phi_{ijt} = \phi_{ijt-1} - \eta_{ijt} \tag{11}$$

where η_{ijt} is serially independent with mean zero (see the details in Topel (1990)). Then, values of η_{ijt} reflect 'permanent' changes in a worker's expected lifetime wealth. For example, these may reflect uncertain returns on investments in human capital or simply new information about a worker's productivity (*note that this latter interpretation is supportive of a learning model to explain the increase of wages with seniority*). If these changes are firm-specific rents, they will affect future job-changing decisions. In contrast, if they mainly represent changes in general human capital, then future job mobility will be unaffected by them (*note that this interpretation of Topel may not be warranted in a model of Betrand competition with firms of heterogeneous productivity*).These possibilities have different implications for interpreting the estimated returns to seniority .

Remark 3. *Under (11), the first step in Topel's two-stage estimation is warranted if job effects are time-varying but follow a random walk.*

In implementing the second-step, consistent estimates of $\beta_1 + \beta_2$ and the parameters of higher-order terms in experience and tenure (plausibly included for reasons of fit) are obtained from the within-job growth model, that is, the first-step model (1). Denote these terms by $\chi\hat{\Gamma}$. Recall (9), $y = X_0\beta_1 + TB + \epsilon$, now re-interpreted as $y = X_0\beta_1 + \chi\Gamma + \epsilon$. Let F denote the vector of other factors (education, etc.) that affect wages, so $y = X_0\beta_1 + \chi\Gamma + \mathrm{F}\gamma + \epsilon$. Subtracting $\chi\hat{\Gamma}$ from both sides of the wage equation yields the second-step model,

$$y - \chi\hat{\Gamma} = X_0\beta_1 + \chi\Gamma + \mathrm{F}\gamma + \epsilon - \chi\hat{\Gamma} = X_0\beta_1 + \mathrm{F}\gamma + e \tag{12}$$

where now $e = \epsilon + \chi(\Gamma - \hat{\Gamma})$. Topel's estimated value of β_1 from implementing (12) is about 7 percent (7.13%). This estimate is substantially smaller than the value of $\beta_1 + \beta_2$ estimated from within-job growth, which is 12.58%. The remainder is the main effect of job tenure on wages,

$$\widehat{\beta_2} = \widehat{\beta_1 + \beta_2} - \widehat{\beta_1} = 12.58\% - 7.13\% = 5.45\%$$

That is, Topel estimates that in the first year of the typical new job, the real wage rises by over 5 percent ($\widehat{\beta_2} = 0.0545$) because of the accumulation of job-specific experience alone. Cumulative returns to various lengths of job tenure are based on the main effect of $\widehat{\beta_2} = 0.0545$, together with the concavity of the wage profile implied by the effects of higher-order terms (χ). The returns to seniority are large: *Topel estimates that 10 years of job seniority increase the wage of the typical worker by 28 percent* ($e^{2459} - 1$) *relative to alternatives.* Compared to the estimates of the wage profile generated by ordinary least squares (OLS) applied to (1), these effects are larger, though not dramatically so. Since Topel argues that the two-step procedure generates a lower bound on the true returns, his conclusion is that the OLS estimates may actually be close to the truth.

A final point relates to the estimation of the bias in β_1 and β_2. Though the two-step procedure cannot identify the bias terms b_1 and b_2 separately, their sum is clearly identified since $\beta_1 + \beta_2$ is consistently estimated. In fact, $b_1 + b_2$ is the component of wage growth that is caused by systematic job changing (compare $y = X_0\beta_1 + TB + \epsilon$ when $T = 0$ and when $T > 0$). Since $E\phi = X_0 b_1 + T(b_1 + b_2)$, the notion that 'good jobs survive' is equivalent to $b_1 + b_2 > 0$. Observe that the sum $b_1 + b_2$ can be estimated directly by reinserting the term $T(b_1 + b_2)$ on the right side of equation (12) and applying least squares. Conceptually, from

$$y = X\beta_1 + T\beta_2 + \epsilon = X_0\beta_1 + TB + \epsilon$$

recalling that $B = \beta_1 + \beta_2$ and, by construction, $X = X_0 + T$ we obtain

$$\begin{aligned} y &= X_0\widehat{\beta_1} + T(\widehat{\beta_1 + \beta_2}) + \epsilon = X_0\widehat{\beta_1} + T(\widehat{\beta_1 + \beta_2}) + \phi + \mu + \upsilon \\ &= X_0\widehat{\beta_1} + T(\widehat{\beta_1 + \beta_2}) + Xb_1 + Tb_2 + u + \mu + \upsilon \\ &= X_0\widehat{\beta_1} + T(\widehat{\beta_1 + \beta_2}) + X_0 b_1 + T(b_1 + b_2) + u + \mu + \upsilon \\ &= X_0(\widehat{\beta_1} + b_1) + T(\widehat{\beta_1 + \beta_2} + b_1 + b_2) + u + \mu + \upsilon \end{aligned}$$

As noted, if the evolution of wages within jobs follows a random walk, then the residuals of the wage growth model are serially independent and least squares applied to (7) delivers an efficient estimator of $\beta_1 + \beta_2$. Therefore,

$$\widehat{b_1 + b_2} = \widehat{\beta_1 + \beta_2 + b_1} + b_2 - \widehat{\beta_1 + \beta_2}$$

(and $\widehat{b_1} = \widehat{\beta_1 + b_1} - \widehat{\beta_1}$). In practice, a similar argument applies and, *under the assumption of a linear relationship between tenure and job specific unobservables*,

$$\begin{aligned} y = X_0\widehat{\beta_1} + F\hat{\gamma} + \chi\hat{\Gamma} + \epsilon &= X_0\widehat{\beta_1} + F\hat{\gamma} + \chi\hat{\Gamma} + Xb_1 + Tb_2 + u + \mu + v \\ &= X_0\widehat{\beta_1} + F\hat{\gamma} + \chi\hat{\Gamma} + X_0b_1 + T(b_1 + b_2) + u + \mu + v \\ &= X_0\left(\widehat{\beta_1} + b_1\right) + T(b_1 + b_2) + F\hat{\gamma} + \chi\hat{\Gamma} + u + \mu + v \end{aligned} \qquad (13)$$

The resulting estimate is a wage growth bias of about 0.2% per year. Finally, it can be shown (see Topel (1991)) that the bias in the two-step estimators of β_1 and β_2 is $b_1 + \gamma_{X_0T}(b_1 + b_2)$, where $\gamma_{X_0T} = (X_0'X_0)^{-1}X_0'T$ is the least-squares coefficient from a regression of tenure on initial experience, X_0. Topel reports that a regression of current tenure on initial experience yields $\gamma_{X_0T} = -0.25$, so $\gamma_{X_0T}(b_1 + b_2) = -0.25 \times 0.002 = -0.005$, which is one-twentieth of one percentage point per year. Note that this implies that the bias in the two-step estimator of β_2, the return to job tenure, is virtually independent of any covariance of job tenure with the unobservables, that is, of the unsigned value of $b_2$since $b_1 \geq 0$, the downward bias in the estimated return to seniority is solely due to improvement in match quality with total labor market experience.

3.2.1.3 Discussion

Topel estimates job-specific wage premiums that would be earned by a typical worker as he accumulates seniority. According to Topel's opinion, the most popular interpretation of these returns is that workers anticipate rising compensation over the life of a job, as in contract models such as Becker (1964), Salop and Salop (1976), or Lazear (1981). A second interpretation is also possible, however, since jobs that yield high wage growth may be more likely to survive. In this case returns to seniority are realized period

by period, though they may not be anticipated at the start of a job. This would generate *selection bias in wage growth*.

Yet an alternative rationale for the positive relationship between job tenure and wages is that workers' unobserved productivities are negatively related to mobility, which would generate *ability bias in the returns to job tenure*. For example, more able (high-wage) persons may change jobs less often, so tenure and wages will be positively correlated in survey data even if $\beta_2 = 0$. Evidence suggestive of this possibility is that education, an observed element of human capital, is negatively related to job changing. Alternatively, if turnover is costly to employers, then the net productivity of stable workers will be greater, and employers will pay more to obtain them. In either case, unobserved characteristics that raise wages (μ_i) are positively correlated with observed tenure, which raises the estimated returns to job seniority. Topel corrects for potential correlation between 'ability', μ_i, and initial experience, X_0, through an instrumental variable (IV) scheme that relies on the existence of a variable that is uncorrelated with the fixed effect but correlated with X_0 in order to 'net out' the correlation between μ_i and X_0. Topel argues that a plausible candidate instrument is total experience. In particular, Topel assumes that the distribution of μ_i is unrelated to experience (successive cohorts of workers are equally able and equally mobile) so that $E(X'\mu) = 0$. Under this condition, X may be used as an instrumental variable for X_0 in estimating the second-step model.

Remark 4. *Topel's IV correction for the potential correlation between the individual fixed effect and initial experience requires earning capacity to be uncorrelated with experience.*

If recorded experience varies with unobserved individual earning capacity, Topel's scheme is no longer valid. A natural question is to what extent this restriction is plausible. Two facts seem likely to undermine Topel's IV scheme: (1) correlation between unobserved ability and total labor market experience, and (2) the presence of cohort effects.

3.2.1.4 Comparison with Related Papers

Here we briefly compare the approach of Topel (1991) with the one by Altonji and Shakotko (1987), henceforth AS, who provide a much lower estimate for the returns to firm tenure, and other papers that followed. Specifically, Topel reports an estimate (see Tables 1 and 2 of Topel (1991)) of the cumulative returns to experience at 10 years of experience of 0.354. The estimates of AS at this level of experience range between 0.372 and 0.442. Our discussion is largely based on Buchinsky, Fougère, Kramarz, and Tchernis (2010).

We start by remarking that, under the assumption that experience at the entry level is exogenous and, hence, uncorrelated with the error terms, Topel (1991) obtains an unbiased estimate for $\beta_1 + \beta_2$ and an upward biased estimate for β_1 (due to the selection bias induced by not modelling mobility decisions on the part of workers). Hence, Topel argues that his estimate of β_2, $\widehat{\beta_2} = 0.0545$, provides a lower bound for the returns to seniority. Topel (1991) also examined two additional sources of potential biases in the estimates of $\beta_1 + \beta_2$ but finds that accounting for these potential biases had a very small effect on the estimate for β_2.) Of course, if experience is not exogenous and is positively (negatively) correlated with ϕ_{ijt} because most mobile workers voluntarily (involuntarily) change jobs for better (worse) matches, then the estimate of β_1, say $\widehat{\beta_1}$, will be upward (downward) biased.

In contrast, AS use an instrumental variables approach in which it is assumed (in Topel's notation) that $\phi_{ijt} = \phi_{ij}$, that is, the individual job-specific term is time-invariant. Under this assumption, deviation of seniority from its average in a specific job is a valid instrument for seniority. Since this method is a variant of Topel's two-step approach, it is not surprising that AS obtain an estimate for $\beta_1 + \beta_2$ that is similar to that obtained by Topel. Yet, AS's procedure appears to induce an upward bias in the IV estimate for β_1, and hence a downward bias in the estimate for β_2. The problem is potentially magnified by two other factors: (a) measurement error problem in the tenure

data used by AS; and (b) differences in the treatment of time trends in the regression. Namely, Topel uses a specific index for the aggregate changes in real wages by using data from the current population survey, while AS used a simple time trend. As a result, the growth in the quality of jobs, due to better matches over time, would cause an additional downward bias in the estimate of β_2.[44]

Altonji and Williams (2005) specify a model that is closer in spirit to Topel's model but their approach differs in some meaningful way. AW crucially rely on the assumption that the match effect ϕ and time are independent, that is, $Cov(t, \phi) = 0$, conditional on experience (or experience and tenure). This assumption may be questionable, especially in cases where workers have had more time to find jobs with higher match value ϕ. Additionally, t may also be correlated with (the person-specific effect in Topel (1991)) because of changes in the sample composition. The estimates of AW of the cumulative returns to experience at 10 years of experience range between 0.310 and 0.374.

Overall, one important conclusion from both Topel (1991) and Altonji and Williams (2005) is that individual heterogeneity is an important factor of the wage growth process. It appears that some of the reduction in the upward bias in the estimate for β_1 in Topel (1991) is due to a reduction in the bias that stems from individual heterogeneity. Topel reports an estimate (see Tables 1 and 2 of Topel, 1991) of the cumulative returns to experience at 10 years of experience of 0.354. The estimates of AS at this level of experience range between 0.372 and 0.442, while those of AW range between 0.310 and 0.374.

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[44] Abraham and Farber (1987) use a somewhat different set of assumptions. In particular, they use completed tenure to proxy for the unobserved dimensions of the individual's, or job's, quality. A problem with their approach is that many of the workers in their data extract have censored spells of employment. Also, they use a quadratic polynomial in experience when estimating the log wage equation, whereas AS and Topel use a quartic specification.

In another recent paper, Dustmann and Meghir (2005) (DM, hereafter) allow for three different sources of returns due to the accumulation of human capital, namely experience, sector-specific seniority, and firm-specific seniority. In order to estimate the returns to experience, they use data on displaced workers in their new jobs, assuming that such workers could not predict closure of an establishment more than a year in advance. Furthermore, under the assumption that displaced workers have preferences for work similar to those that induced their sectorial choices, controlling for the endogeneity of experience also controls for the endogeneity of sector tenure. In a subsequent step, DM estimate two reduced-form equations, one for experience and another one for participation. The residuals from these two regressions are used as regressors in the wage regression of displaced workers. This allows DM to account for possible sample selection biases induced by restricting attention to only the individuals staying with their current employer. Using data from Germany and the United States, DM find that the returns to tenure for both skilled and unskilled workers are large. The estimated returns to sector-specific tenure are much smaller but (statistically) significant.[45]

Finally, Farber (1999) notes the importance of modeling some specific features of the mobility process. First, he shows that in the first few months of a job there is an increase in the probability of job separation, which decreases steadily thereafter. Farber provides strong evidence that contradicts the simple model of pure unobserved heterogeneity, suggesting that one must distinguish heterogeneity from duration dependence. He also finds strong evidence that: (a) firms tend to lay off less senior workers who have lower specific firm capital; and (b) job losses result in substantial permanent earnings losses. On this latter point, see also Gibbons and Katz (1991).

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[45] See also in Farber (1999) a discussion of the empirical findings in the literature on displaced workers. Further references are Addison and Portugal (1989) and Jacobson, LaLonde, and Sullivan (1993).

3.2.2 Buchinsky, Fougère, Kramarz, and Tchernis (2010)

We now focus on a paper that in spirit and specification is closest to our approach. In the literature, much of the focus on the returns to seniority has concentrated on the possible endogeneity of job changes and its effect on the estimated returns to tenure. Buchinsky, Fougère, Kramarz, and Tchernis (2010) (BFKT, henceforth) contributes to the debate by also considering the possible endogeneity of labor market experience, and its potential effects on the estimated returns to tenure and experience. To address this issue, they develop a model in which individuals make two key decisions, namely employment (or participation) and inter-firm mobility. In turn, these decisions influence the observed outcome of interest, namely wages.

Within this model, they revisit the issue regarding the magnitude of the returns to seniority in the United States. They use data from the PSID (a slightly different sample extract that the one used by Altonji and Shakotko (1987) and Topel (1991)) and estimate their model for three separate education groups: high school drop-outs, high school graduates, and college graduates. They adopt a Bayesian approach and employ Markov Chain Monte Carlo methods for estimating the joint posterior distribution for the model's parameters. (Note that one difference with respect to DM is that BFKT do not model sectoral choices, hence they abstract from sector-specific returns. See Neal (1995) and Parent (1999, 2000) for the importance of sector- and firm-specific human capital.)

The authors find that the returns to seniority are higher than those previously estimated in the literature, including those reported by Topel (1991). Specifically, their results indicate that, while the estimated returns to experience are somewhat higher than those previously found in the literature, they are of similar magnitude. In contrast, the estimates of the returns to seniority are much higher than those previously obtained, including those obtained by Topel (1991). Consequently, their estimates of total within-job wage growth are significantly higher than Topel’s estimates, and those reported by

Abraham and Farber (1987). This results holds true for all three education groups analyzed.

Their study also sheds light on several important factors leading to the apparent differences between their estimates and those obtained in previous studies. First, their study highlights the importance of explicitly modeling the employment and mobility decisions, which, in turn, define experience and seniority. Second, they establish the need to account for unobserved heterogeneity in the participation and mobility decisions, as well as in the wage function. Third, they demonstrate the need to explicitly control for job-specific components in the wage function, through the introduction of a function that serves as a summary statistic for what they term an individual's specific career path.

This function captures the overall effect of the worker's specific career path on the worker's market wage. In particular, they find that the magnitude of the estimated returns changes markedly when they account for this factor, but qualitative results remain similar. This strongly indicates that the timing of a job change during the course of an individual's career is important for an individual's wage trajectory.

3.2.2.1 Setup

BFKT's model builds on a specification of the wage function common in the literature. They specify the observed log wage equation for individual i in job j at time t as

$$w_{ijt} = w_{ijt}^{*} I(y_{it} = 1) = (x'_{wijt}\delta_0 + \varepsilon_{ijt}) I(y_{it} = 1) \tag{14}$$

where, by definition, $w_{ijt}^{*} = x'_{wijt}\delta_0 + \varepsilon_{ijt}$. In (14), x'_{wijt} is a vector of observed characteristics, including education, labor market experience and firm tenure, of an individual in the current job, $I(\cdot)$ is an indicator function that equals one if, and only if, $y_{it} = 1$, that is, if, and only if, the i-th individual participates in the labor market at time t. So, the wage offer, w_{ijt}^{*} , is observed only if the individual chooses to work. BFKT decompose the error term ε_{ijt} into three components,

$$\varepsilon_{ijt} = J_{ijt}^{W} + \alpha_{ijt} + \xi_{ijt}$$

where α_{ijt} is a person-specific correlated random effect, analogous to μ_i in Topel (1991), and ξ_{ijt} is a contemporaneous idiosyncratic error term. The term J_{ijt}^{W} is analogous to the term ϕ_{ijt} in Topel (1991) with the important difference that in BFKT it explicitly provides a summary statistic for the individual's work history and career. Namely, J_{ijt}^{W} captures the timing and magnitude of all discontinuous jumps in the individual's wages that resulted from all job changes experienced by the individual until date t.

In principle, this function can be viewed as a full set of dummy variables capturing all observed jumps in the data. However, BFKT argue that in their empirical application this would require estimation of a prohibitively large number of parameters. Thus, they approximate J_{ijt}^{W} by a piece-wise linear function of experience and seniority at the time of a job change, which is given by

$$J_{ijt}^{W} = (\phi_0^s + \phi_0^e e_{i0})d_{i1} + \sum_{l=1}^{M_{it}} \left[\sum_{k=1}^{4} (\phi_{k0} + \phi_k^s s_{it_l-1} + \phi_k^e e_{it_l-1}) d_{kit_l} \right]$$

where $d_{1it_l} = 1$ if the l-th job of the i-th individual lasted less than a year and equals 0 otherwise, $d_{2it_l} = 1$ if the l-th job of the i-th individual lasted between 2 and 5 years and equals 0 otherwise, $d_{4it_l} = 1$ if the l-th job lasted between 6 and 10 years and equals 0 otherwise, $d_{4it_l} = 1$ if the l-th job lasted more than 10 years and equals 0 otherwise. Finally, M_{it} denotes the number of job changes experienced by the i-th individual at time t (not including the individual's first sample year). If an individual changed jobs in the first sample year, then $d_{i1} = 1$, otherwise $d_{i1} = 0$. The quantities s_{it_l-1} and e_{it_l-1} denote the individual's seniority and experience in yeart_l, respectively, when individual i leaves job l. Note that while the ϕ's are fixed parameters, the size of the jumps (within each of the four brackets of seniority) may differ depending on the level of seniority and labor market experience at the time of a job change. Overall the function J_{ijt}^{W} contains thirteen identifiable parameters, corresponding to the four brackets of seniority and the first sample year.

Observe that the J_{ijt}^{W} function generalizes the term ϕ in Topel (1991) and captures the initial conditions specific to the individual at the start of a new job. Equivalently, this function provides a measure of the opportunity wage of the worker if the worker were to move to a new job at that point in the worker's career. Note also that inclusion of actual rather than potential labor market experience as a determinant of initial earnings at a new job allows BFKT to distinguish between displaced workers, who experienced a period of non-employment after displacement, and workers who moved directly from one job to another. This difference is all the more critical since BFKT do not distinguish between participation and employment. Instead, the inclusion of the seniority level at past jobs allows BFKT to control for the quality of past job matches. Whether the frequency of changing jobs and the individual's labor market attachment matters is an empirical question that BFKT can explicitly address based on J_{ijt}^{W}.

Lastly, note that J_{ijt}^{W} is individual-job specific. In general, there are several ways to define a job. BFKT define a job as a particular employment spell in an individual's career. Hence, it is possible that different individuals will have the same values for J_{ijt}^{W} even though they may not be employed at the same firm. This definition of a job is consistent with their modelling approach. However, our data allows us to rely on a finer, much more precise definition of a job based on administrative records as corresponding to an executive's title within the hierarchy of titles of a firm.

3.3 Contribution

Our discussion presents testable implications: (1) Is there any evidence that the pattern of labor market experience differs among executives with higher education and/or more successful careers? (2) Do we detect any important cohort effect? One reason for this could be due to the recent changes in the legislation surrounding executive pay.

Observe that if the answer to either question is positive, then Topel's IV scheme would be inapplicable. The purpose of our research is to follow this line of argument to

first replicate and then improve on Topel's measures of the relative magnitude of returns to tenure and to labor market experience. Observe also that Topel assumes that the level of experience at a new job is exogenous, which may be a problematic assumption. Indeed, one of the reasons between the difference in estimates between BFKT and Topel (1991) is due to the fact that BFKT explicitly the participation decision as endogenous.

We contribute to the literature by doing so in four steps: (1) by making use of new detailed matched firm-executive data containing information on firm characteristics (absent from Topel's analysis) and job characteristics (poor in Topel's analysis, especially with respect to an executive's title and position within the hierarchy of jobs of a firm); (2) by allowing for a more flexible functional form specification; (3) by explicitly accounting for the endogeneity of the mobility and participation decisions in our empirical specification; and (4) by relying on a semi-parametric estimation approach.

We build on BFKT by accounting for the endogeneity of the participation and mobility decisions, by allowing for unobserved heterogeneity among executives, and by explicitly controlling for the effect of past mobility on current wages. We augment their work in two ways. First, from a modeling point of view, we account for the endogeneity of job mobility decisions both *within firms and between firms*, we allow for a flexible *nonparametric* specification of unobserved heterogeneity, and we control for the effect of past mobility on current wages via *alternative specifications* that are consistent with different structural interpretations.

Second, from an empirical point of view, by using a *matched employer-employee* database with rich information on firm's productivity and financial characteristics, we are able to assess the separate contribution of individual and firm characteristics to returns to tenure and labor market experience. The availability of firm data also allows us to incorporate firm characteristics in controlling for the impact of the quality of past job

matches on executives' mobility decisions, rather than merely relying on the inclusion of the seniority level at past jobs as BFKT do.[46]

Further, based on information on an executive's mobility between jobs within a firm, we can isolate the effect of mobility across jobs *within a firm* on the returns to firm tenure and contrast its importance to the importance of mobility across jobs *between firms* for total wage growth

Our key source of identification of returns to tenure and experience is the time-series dimension of our data. Specifically, our data contain observations on many individuals who changed jobs and firms over the sample period, and did so at different points in their life-cycle (given our focus on executives, necessarily our data contain information on older workers than in more representative samples like the PSID. Nonetheless, we show below that descriptive statistics from our data are comparable to those from the PSID if one restricts attention to individuals with college degree; see, for instance, Table 1 in BFKT).[47] Naturally, the time series dimension also allows us to control and pin down individual-specific effects.

[46] In this sense, we view our work as building on the analysis of Abowd, Kramarz, and Roux (2006), who, based on French matched employer-employee data, document the importance of mobility for wages in that they find that entry wages depend upon seniority in the previous job, as well as the number of previous jobs held by the individual.

[47] As in BFKT, since experience and seniority are fully endogenized, we need not impose any further restrictions on the data extract (e.g. restricting attention to only exogenously displaced workers) as is done in Dustmann and Meghir (2005) or Topel (1991).

SUMMARY

Existing accounting literature typically relies on critical events and firm-level characteristics to explain firms' accounting practices but rarely examined the role played by top managers until recently. This prompted me to investigate manager specific effects on accounting practices and compensations. Specifically, in this study, I examine whether managers exert a significant individual-specific influence over firm financial disclosure and how the idiosyncratic differences across managers affect their compensations.

The first two chapters of this study examine idiosyncratic differences among individual managers in terms of accounting choices they make and how investors react. The results show that individual managers play a significant role in explaining accounting accruals and the capital market recognizes and responds to a manager's style on financial reporting. The third chapter focuses on the executive labor market at the aggregate or market level and examines how the labor market compensates manager for their personal characteristics such as experience and skills, and how their personal characteristics and compensation affect firm value.

This study contributes to the accounting literature by adding a "people" dimension to explanations for financial reporting decisions and extends prior studies by showing that investors are willing and able to identify a manager's style. In addition, by revisiting the relative importance of returns to firm-specific tenure and to general labor market experience in the labor market that focuses on executives, this study also contributes to the literature which involves the long-debated issue pertains to the relative importance of experience in the labor market and seniority in a firm in explaining wage growth over the life cycle and the resulting earnings differences across individuals.

APPENDIX A VARIABLE DEFITION FOR CHAPTER 1

The specific variables used in Chapter 1's analysis are defined as follows:

- *Total Assets* is the firm's total assets (Compustat item 6) at the fiscal year-end.
- *Total Sales* is the firm's total sales (Compustat item 12) for the fiscal year.
- *Market Value* is defined as the product of common shares outstanding (Compustat item 199) and common stock's close price (Compustat item 25) at fiscal year-end.
- *Investment* is capital expenditures (Compustat item 128) over net property, plant, and equipment at the beginning of the fiscal year (Compustat item 8).
- *Average Tobin's Q* is defined as the market value of assets divided by the book value of assets (Compustat item 6), where the market value of assets equals the book value of assets plus the market value of common equity less the sum of the book value of common equity (Compustat item 60) and balance sheet deferred taxes (Compustat item 74).
- *Cash flow* is defined as the sum of earnings before extraordinary items (Compustat item 18) and depreciation (Compustat item 14) over net property, plant, and equipment at the beginning of the fiscal year (Compustat item 8).
- *Leverage* is defined as long-term debt (Compustat item 9) plus debt in current liabilities (Compustat item 34) over long-term debt plus debt in current liabilities plus the book value of common equity (Compustat item 60).
- *Cash holdings* is defined as cash and short-term investments (Compustat item 1) over net property, plant, and equipment at the beginning of the fiscal year (Compustat item 8).

- *Interest coverage* is earnings before depreciation, interest, and tax (Compustat item 13) over interest expenses (Compustat item 15).
- *Dividends/ earnings* is the ratio of the sum of common dividends (Compustat item 21) and preferred dividends (Compustat item 19) over earnings before depreciation, interest, and tax (Compustat item 13).
- *R&D* is the ratio of R&D expenditures (Compustat item 46) over lagged total assets (Compustat item 6).
- *Advertising* is the ratio of advertising expenditures (Compustat item 45) over lagged total assets (Compustat item 6).
- *SG&A* is the ratio of selling, general, and administrative expenses (Compustat item 189) over sales (Compustat item 12).
- *N of announced acquisitions* is the total number of announced acquisitions in the fiscal year.
- *N of effective acquisitions* is the total number of effective acquisitions in the fiscal year.
- *N of diversifying acquisitions* is the number of acquisitions during the fiscal year in two-digit industries different from those the acquirer currently operates in.
- *Return on assets* is the ratio of earnings before extraordinary items (Compustat item 18) over lagged total assets (Compustat item 6).
- *Operating return on assets* is the ratio of operating cash flow (Compustat item 308) over lagged total assets (Compustat item 6).
- *Total accruals* is calculated by using the balance sheet approach.
- *Abnormal accruals* is the abnormal accruals by estimating time series modified Jones model.

- *Abs. total accruals* is the absolute value of total accruals.
- *Abs. abnormal accruals* is the absolute value of *abnormal accruals.*

APPENDIX B REAL DECISION ANALYSIS AND CONTROLLING FOR REAL DECISIONS FOR CHAPTER 1

Analyzing manager firm policy decisions, Panel A of Table B1 reports the results for investment policy. The first variable is capital expenditures. The benchmark specification includes controls for firm fixed effects, year fixed effects, cash flow, lagged Tobin's *Q*, and the lagged logarithm of total assets. The adjusted R^2 for this specification is 0.234. The adjusted R^2 increases to 0.310 when we include the set of manager fixed effects. The next two variables are investment to Tobin's *Q* and investment to cash flow sensitivities, respectively. The fixed effects of interest here relate not to the level of a given variable, but rather to the sensitivity of that variable to Tobin's *Q* and cash flow. In practice, for investment to be *Q* sensitive, we start by regressing investment on year fixed effects, cash flow, lagged Tobin' *Q*, the lagged logarithm of total assets, firm fixed effects, and firm fixed effects interacted with lagged Tobin's *Q*. We then add to this benchmark specification manager fixed effects as well as manager fixed effects interacted with lagged Tobin's *Q*. The estimated coefficients of interest are those on the interaction terms. We proceed in a similar way to estimate manager fixed effects for investment to cash flow sensitivity. The results indicate increases in adjusted R^2 when including the interaction terms of manager fixed effects with cash flow and lagged Tobin's *Q*. The last two variables in Panel A are number of announced acquisitions and number of effective acquisitions. For both of these two variables we observe increases in adjusted R^2 following the inclusion of the manager fixed effects. Also, all the *F*-tests in this panel are significant and the number of managers whose specific effects is significant is much higher than what we would expect under the null hypothesis, leading us to reject the null hypothesis of no joint manager effect in all cases.

Panel B of Table B1 focuses on financial policy. Included in all regressions are firm fixed effects, year fixed effects, the lagged logarithm of total assets, and the rate of

return on assets. The adjusted R^2 of the leverage regression increases from 0.116 to 0.226 when we include the manager fixed effects. The adjusted R^2 of the interest coverage regression increases from 0.037 to 0.081. The adjusted R^2 of the cash holdings regression goes up by 0.152, from 0.121 to 0.273, when we compare the benchmark specification with the specification that includes all manager fixed effects. Finally, managers appear to be important determinants of dividend policy, with an overall increase in adjusted R^2 of about 0.092.

Panel C of Table B1 reports our results for the organizational (operating) policy variables. Again, we find that top executives have large effects on the realization of these variables. The fit of the diversification regression improves from 0.185 to 0.241. The adjusted R^2s of the R&D and advertising regressions increase by 0.123 and 0.130, respectively. Cost-cutting policy, as proxied by the ratio of SG&A to total sales, is affected by the identity of the managers as well.

Panel D of Table B1 reports the size distribution of the manager fixed effects for each of the regressions on firm policy variables. The results show that variation in the size of the manager fixed effects is economically large. Replacing a manager in the bottom quartile by a manager in the top quartile increases the firm policy variable significantly.

Table B-1 Manager Fixed Effects on Firm Policy Variables

	F-tests on fixed effects for				
	CEOs	*CFOs*	*Others*	*N*	*Adj. R^2*
Panel A: Investment Policy					
Investment				13332	0.234
Investment	4.1e+05 (<0.001, 293) 198/340	59774.77 (<0.001, 147) 96/154	4.2e+07 (<0.001, 240) 153/274	13332	0.310
Inv to Q sensitivity				13332	0.372
Inv to Q sensitivity	3.4e+07 (<0.001, 298) 223/347	4.7e+05 (<0.001, 147) 85/155	9.7e+08 (<0.001, 247) 172/275	13332	0.453
Inv to CF sensitivity				13332	0.416
Inv to CF sensitivity	1.1e+06 (<0.001, 294) 215/339	2.0e+07 (<0.001, 149) 105/151	2.9e+07 (<0.001, 242) 182/263	13332	0.522
N of announced acquisitions				13055	0.072
N of announced acquisitions	2.2e+05 (<0.001, 313) 182/349	64425.79 (<0.001, 157) 77/158	7.2e+057 (<0.001, 262) 162/282	13055	0.147
N of effective acquisitions				13048	0.048
N of effective acquisitions	3.2e+05 (<0.001, 313) 187/349	91284.24 (<0.001, 157) 78/158	4.1e+05 (<0.001, 262) 156/282	13048	0.133
Panel B: Financial Policy					
Leverage				14688	0.116
Leverage	2.3e+06 (<0.001, 323) 190/349	2.6e+05 (<0.001, 157) 82/157	25882.20 (<0.001, 262) 154/281	14688	0.226
Interest coverage				13704	0.037
Interest coverage	5.5e+06 (<0.001, 302) 105/341	1.6e+05 (<0.001, 147) 57/154	55372.56 (<0.001, 247) 98/273	13704	0.081
Cash holding				14741	0.121
Cash holding	2.3e+05 (<0.001, 324) 173/350	4700.42 (<0.001, 157) 67/157	3.0e+06 (<0.001, 262) 114/282	14741	0.273
Dividends/earnings				14718	0.029
Dividends/earnings	8.7e+05 (<0.001, 323) 188/350	22483.37 (<0.001, 157) 72/157	96123.97 (<0.001, 262) 143/282	14718	0.121

Table B-1 Continued

Panel C: Organizational Policy

	F-tests on fixed effects for				
	CEOs	*CFOs*	*Others*	*N*	*Adj. R^2*
N of diversifying acquisitions				12988	0.185
N of diversifying acquisitions	14329.70 (<0.001, 314) 187/349	35103.30 (<0.001, 157) 83/157	1.4e+07 (<0.001, 262) 149/282	12988	0.241
R & D				9733	0.091
R & D	9.8e+058 (<0.001, 218) 183/286	33393.55 (<0.001, 114) 83/157	3395.30 (<0.001, 165) 149/282	9733	0.214
Advertising				5645	0.107
Advertising	5.9e+05 (<0.001, 120) 101/181	3.2e+05 (<0.001, 64) 49/80	2.2e+05 (<0.001, 89) 86/140	5645	0.237
SG & A				13462	0.227
SG & A	3.9e+05 (<0.001, 292) 199/332	66275.03 (<0.001, 150) 80/153	8.7e+06 (<0.001, 241) 138/262	13462	0.367

Panel D: Size Distribution of Manager Fixed Effects

	Mean	Std. dev.	25th pctl.	Median	75th pctl.
Investment	0.002	0.220	-0.074	-0.004	0.081
Inv to Q sensitivity	0.036	1.308	-0.156	-0.007	0.151
Inv to CF sensitivity	-0.128	1.702	-0.291	-0.006	0.252
N of announced acquisitions	0.060	1.073	-0.496	-0.016	0.556
N of effective acquisitions	0.052	0.944	-0.421	0.001	0.438
Leverage	0.000	0.162	-0.089	-0.004	0.083
Interest coverage	0.018	71.243	-10.335	0.737	10.088
Cash holding	-0.096	2.628	-0.416	0.010	0.357
Dividends/earnings	-0.001	0.071	-0.028	-0.002	0.023
N of diversifying acquisitions	0.026	0.519	-0.244	-0.026	0.235
R & D	-0.004	0.048	-0.016	-0.003	0.012
Advertising	0.002	0.029	-0.009	0.001	0.011
SG & A	-0.002	0.054	-0.024	-0.003	0.020

Table B-1 Continued

Note:

a. Sample is the manager-firm matched panel data set as described in subsection 3.1. Definition and construction of the variables are reported in Appendix A.

b. Reported in the table are the results from fixed effects panel regressions, where standard errors are clustered at the firm level. For each dependent variable (as reported in column 1), the fixed effects included are row 1: firm and year fixed effects; row 2: firm, year, and all manager fixed effects. In Panel A, included in the "Investment to Q" and "Investment to cash flow" regressions are interactions of these fixed effects with lagged Tobin's Q and cash flow, respectively. Also the "Investment," "Investment to Q," and "Investment to cash flow" regressions include lagged logarithm of total assets, lagged Tobin's Q, and cash flow. The "Number of Acquisitions" and "Number of effective Acquisitions" regressions include lagged logarithm of total assets and return on assets. In Panel B, each regression contains return on assets, cash flow, and the lagged logarithm of total assets. In Panel C, each regression contains the logarithm of total assets, return on assets, and cash flow. The "N of diversifying acquisitions" regressions also include a dummy variable for whether the firm undertook any acquisition in that year.

c. Panel A through C report the *F*-tests for the joint significance of the CEO fixed effects (column 2), CFO fixed effects (column 3), and other executives fixed effects (column 4). For each F-test we report the value of F-statistic, the p-value, and the number of constraints. Under each F-statistic, we report the number of managers who have individually significant effects and the total number of managers with sufficient data to be included in the regression estimation. The adjusted R^2 is calculated based on the within R^2 from the command xtreg, fe in Stata.

d. Reported in Panel D are the size distributions of fixed effects from the regressions. Column 1 reports the mean fixed effect for each policy variable. Column 2 reports the standard deviation of the fixed effects. Column 3, 4 and 5 report the fixed effects at the 25^{th} percentile, 50^{th} percentile, and 75^{th} percentile of the distribution, respectively.

Table B-2 Manager Fixed Effects on Accounting Accruals Controlling for Manager Firm Policy Decisions

	Panel A: Signed Accounting Accruals				
	F-tests on fixed effects for				
	CEOs	*CFOs*	*Others*	*N*	*Adj. R^2*
Total accruals				11380	.117
Total accruals	90570.22*** 124/320	83799.82*** 79/141	7.1e+05*** 115/250	11380	.145
Abnormal accruals				11380	.052
Abnormal accruals	1.7e+05*** 97/319	1.1e+05*** 41/142	1.2e+05*** 81/250	11380	.065
	Panel B: Unsigned Accounting Accruals				
	F-tests on fixed effects for				
	CEOs	*CFOs*	*Others*	*N*	*Adj. R^2*
Abs. total accruals				11380	.084
Abs. total accruals	2.7e+06*** 125/319	3.7e+05*** 42/142	43335.62*** 101/250	11380	.115
Abs. abnormal accruals				11380	.061
Abs. abnormal accruals	1.2e+05*** 97/320	2.8e+05*** 43/141	63879.61*** 85/250	11380	.082

Panel C: Size Distribution of Manager Fixed Effects					
	Mean	Std. dev.	25th pctl.	Median	75th pctl.
Total accruals	0.010	0.148	-0.045	0.002	0.049
Abnormal accruals	0.004	0.084	-0.031	0.002	0.033
Abs. total accruals	-0.005	0.064	-0.032	-0.006	0.027
Abs. abnormal accruals	-0.001	0.060	-0.021	0.000	0.019

Note:

a. Sample is the manager-firm matched panel data set as described in subsection 3.1. Definition and construction of the variables are described in subsection 3.3 and also reported in Appendix A and Appendix B.

b. Reported in the table are the results from fixed effects panel regressions, where standard errors are clustered at the firm level. For each dependent variable (as reported in column 1), the fixed effects included are row 1: firm and year fixed effects; row 2: firm, year, and all manager fixed effects. Included in all regressions are leverage, B/M, logarithm of market value, ROA, and manager fixed effects on firm policies estimated in Table IIA. Also included in the unsigned accruals regressions are standard deviation of cash flows from operations and the standard deviation of sales.

c. Reported in Panel A and B are the F-tests for the joint significance of the CEO fixed effects (column 2), CFO fixed effects (column 3), and other executives fixed effects (column 4). For each F-test we report the value of F-statistic. ***, ** and * represent statistical significance are the 1%, 5% and 10% levels, respectively. Under each F-statistic, we report the number of managers who have individually significant effects and the total number of managers with sufficient data to be included in the regression estimation. The adjusted R^2 is calculated based on the within R^2 from the command xtreg, fe in Stata.

d. Reported in Panel C are the size distributions of fixed effects from the regressions. Column 1 reports the mean fixed effect for each accruals measure. Column 2 reports the standard deviation of the fixed effects. Column 3, 4 and 5 report the fixed effects at the 25th percentile, 50th percentile, and 75th percentile of the distribution, respectively.

APPENDIX C VARIABLE DEFINITION FOR CHAPTER 2

The specific variables used in Chapter 2's analysis are defined as follows:

- *DAcc* is annual discretionary accruals calculated using a time-series modified Jones Model. For each fiscal year, the modified Jones Model is estimated by using up-to-date information, i.e., financial information till that year.
- *CAR* is three-day cumulative adjusted returns centered on forecast issuance date.
- *M_Effect* is the estimated manager fixed effect on accruals.
- *F_Effect* is the estimated firm fixed effect on accruals.
- *BM* is book-to-market ratio at the beginning of the year.
- *Size* is natural logarithm of market value of equity for the firm at the beginning of the year.
- *ROA* is the ratio of earnings before extraordinary items over total assets in the last year.
- *Special* is annual special items scaled by total assets in the last year.
- *News* is the management earnings per share forecast less the consensus analyst estimate prevailing on the day of the management forecast scaled by the stock price on three days before announcement date of the management forecast.
- *Specificity* is indicator variable equal to one when the management earnings forecast is a range estimate and zero otherwise
- *Horizon* is the number of days between the forecast release date and the earnings announcement date for the earnings being forecasted, divided by 365.
- *Annual* is indicator variable equal one when annual earnings are being forecasted and zero otherwise.

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