WHAT DRIVES SELL-SIDE ANALYST COMPENSATION AT HIGH-STATUS INVESTMENT BANKS?

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ABSTRACT

We use proprietary data from a major investment bank to investigate factors associated with analysts' annual compensation. We find that compensation is positively related to "All-Star" recognition, investment-banking contributions, the size of analysts' portfolios, and whether an analyst is identified as a top stock-picker by the *Wall Street Journal*. We find no evidence that compensation is related to earnings forecast accuracy. However, consistent with prior studies, we find that analyst turnover is related to forecast accuracy, suggesting that analysts' forecasting incentives are primarily termination-based. Additional analyses indicate that "All-Star" recognition proxies for buy-side clients' votes on analyst research quality, which are used to allocate commissions across banks and analysts. Taken as a whole, our evidence is consistent with compensation for the bank's analysts being designed to reward actions that increase its brokerage and investment-banking revenues. To assess the generality of our findings, we test the same relations using compensation data from a second high-status bank with similar results.

JEL Classification: G24, G29, J33, J44, L84, M41, M52

Keywords: Performance measurement, incentive compensation, financial analysts, *Institutional Investor* ratings, investment banking.

1. *Introduction*

Sell-side research plays an important role in modern capital markets. Within the United States, most top-tier investment banks spend in excess of one hundred million dollars annually on equity research.[1] Institutions and retail investors use equity research to help make investment decisions (e.g., Madan, Sobhani, and Bhatia (2003)) and corporations rely on analysts to market their securities and boost liquidity (e.g., Krigman, Shaw, and Womack (2001)).

However, because of limited access to data on analyst pay, prior studies' assumptions about analysts' incentives are largely based on plausible conjectures rather than systematic evidence. What little is known about analyst compensation is from the memoirs of former analysts (e.g., Reingold and Reingold (2006)) and a handful of stories on the reputed pay and characteristics of well-known outliers, such as Jack Grubman, Mary Meeker, and Henry Blodget (e.g., Gasparino (2005)). The primary contribution of this paper, which uses analyst compensation for a prominent integrated investment bank for the period 1988 to 2005,[2] is to formally examine analyst compensation and its drivers. By examining actual analyst compensation data, our study is able to test the incremental economic and statistical significance of investment banking, brokerage and other factors on analyst pay, deepening our understanding of their financial incentives.

Our findings also shed light on the relationship between analyst compensation and the two most studied measures of analyst performance, forecast accuracy and stock recommendation profitability. Mikhail, Walther, and Willis (1999) and Hong and Kubik (2003) find that analyst

[1] The Sanford C. Bernstein estimates cited by Francis, Chen, Willis, and Philbrick (2004) imply that annual research budgets at the top-8 investment banks averaged between $200 and $300 million during the 2000–2003 period.

[2] Our sample bank is rated as "high status" or "top tier" based on a variety of criteria including the Carter-Manaster ten-tier "tombstone" ranks provided in Carter, Dark, and Singh (1998), the size-based categorization provided in Hong, Kubik, and Solomon (2000), and *Institutional Investor's* annual buy-side polls.

forecast accuracy is associated with job turnover and career prospects, suggesting an association with compensation. But as Mikhail et al. (1999) and Bradshaw (2008) note, this relation is not based on actual evidence, and conflicts with practitioner comments that such factors are not considered when making bonus awards.

We document several important facts. First, there were large, systematic swings in the level and skewness of real compensation throughout the 1988-2005 period. Median compensation increased from $397,675 in 1994 to a peak of $1,148,435 in 2001, then declined to $647,500 in 2005. These ebbs and flows were highly correlated with swings in capital market activity, as captured by the Baker and Wurgler (2006) market activity index. The increase in compensation skewness was reflected in the ratio of analyst compensation at the 90th and 10th percentiles, which was 2.6 in 1990, increased to 6.1 in 2000 and declined to 4.0 by 2005. Variation in the level and skewness of compensation over time was almost exclusively driven by bonus awards, which grew from a low of 46% of total compensation in 1990 to 84% in 2002, and dropped to 70% by 2005. Collectively, the evidence indicates that during periods when market activity and, as a result, trading commissions and corporate finance fees are high, the bank's bonus pool expands, leading to large increases in analyst compensation. However, these large increases are not shared equally among the bank's analysts as pay differentials also expand during "hot" markets.

Second, most of the variation in analyst compensation can be explained by four factors: recognition by *Institutional Investor* (*II*) as an "All-Star" analyst, recognition by the *Wall Street Journal* (*WSJ*) as a star stock-picker, an analyst's investment-banking contributions, and the size of an analyst's portfolio. Pooled regressions, which estimate the market level of analyst pay based on observable characteristics, indicate that controlling for other hypothesized determinants, *II* All-Star analysts earn 61% higher compensation than their unrated peers; top stock-pickers, as recognized by the *WSJ*, earn about 23% more than their peers; analysts who cover stocks that generate underwriting fees for the bank earn 7% higher pay for each million dollars of fees earned; and the cross-sectional elasticity of compensation with respect to portfolio size is approximately 0.18.

To evaluate pay-for-performance sensitivities (i.e., incentives), we follow the guidance in Murphy (1985) and estimate analyst-fixed-effect regressions. These regressions, which rely on intra-analyst variation in performance and compensation, indicate that after controlling for other characteristics, gaining (losing) *II* status is associated with a 16% compensation premium (penalty); gaining/losing "star stock-picker" status in the *WSJ* is associated with an 11% change in pay; covering stocks that generate underwriting fees for the bank is accompanied by 6% higher pay for each million dollars of fees earned; and the intra-analyst elasticity of compensation with respect to portfolio scale is just under 0.07.

Third, forecast accuracy plays little direct role in determining compensation of the sample firm's analysts. Consistent with this finding, interviews with equity research professionals at eleven large banks (including the sample bank), as well as an examination of our sample bank's 2005 performance evaluation and development booklet, found that the banks do not formally track forecast accuracy for compensation purposes. However, we do find that inaccurate analysts at our sample bank are more likely to move to lower-status banks or exit I/B/E/S, as documented in the analyst turnover literature. "Fired" analyst-year observations had larger forecast errors than other analysts who covered the same stocks and other analysts within our sample firm. These findings suggest that forecasting incentives resemble a Mirrlees contract. Under a normal range of forecasting outcomes, there is no relation between forecasting performance and annual compensation within banks, but extremely adverse forecasting outcomes are associated with an increased probability of dismissal.[3]

Fourth, we find that much of the *WSJ* "star stock-picker" compensation premium reflects public recognition, not underlying stock-picking performance. Moreover, the association between underlying stock-picking performance and compensation occurs with a one-year lag, reflecting the timing of the *WSJ* report. We conclude that stock-picking performance affects analyst compensation, but the effect is delayed and only economically significant if it boosts an analyst's visibility. Discussions during our interviews support this inference. Although stock-picking performance is widely tracked by banks as part of their evaluation and development purposes, insiders indicated that it is generally not a major determinant of analyst compensation.

Finally, we find that the *II* compensation premium can be explained by underlying votes from institutional investors (i.e., "the buy-side") and does not appear to be attributable to the additional visibility that comes from being assigned star status in *II*'s October publication. Discussions with bank insiders indicate that the underlying votes approximate "broker votes" used to allocate commissions across banks and analysts. Consistent with this claim, using the information from the research director's 2005 analyst evaluation and development booklet, we find that *II* All-Star analysts have significantly higher broker votes than their peers.

Our findings are robust to a battery of tests, including alternative definitions of key variables, alternative measurement windows, controlling for sample-selection bias, first differencing (i.e., "changes"), and replication using data from a second high-status investment bank. In addition, interviews with research directors at other leading banks indicated a

[3] See Bolton and Dewatripont (2005) and Christensen and Feltham (2005) for a discussion of Mirrlees contracts and the "Mirrlees Problem."

remarkable consistency in the performance metrics used for determining analyst bonus awards, suggesting that our findings are likely to also hold for other top-tier banks.

Nevertheless, we caution the reader against generalizing our findings to non-representative settings. In particular, it seems highly unlikely that they will apply to lower-status banks or brokerage firms that employ few if any *II*-ranked analysts and do not generate substantial investment banking revenues. Also, we recognize that the importance of investment banking for analyst compensation is likely to diminish following the Global Settlement. However, we are unable to test this hypothesis given restrictions imposed by our sample firm and the limited number of post-settlement observations.

This paper is organized as follows. Section 2 describes the proprietary and public data used in this study. Section 3 provides summary information on analyst compensation. Section 4 discusses the hypothesized drivers of analyst compensation. Empirical results are presented in Sections 5 and 6. Section 7 concludes with a discussion of the results and suggestions for future research.

2. Data

The data used in this study come from a proprietary compensation file and five publicly available sources: I/B/E/S, CRSP, SDC Platinum, *Institutional Investor*, and the *Wall Street Journal*. From a leading Wall Street investment bank, we obtained a set of electronic data files for the years 1988-2005. Each annual file contains the name, hire date, and compensation for each of the firm's analysts. No other variables are contained in these electronic files.

In addition to the annual electronic compensation files, senior research staff at the bank provided us with marked-up photocopies of the research director's 2005 analyst evaluation and development booklet. These copies report analyst performance using a series of figures and tables. The booklet tracked five broad categories of metrics: analyst ratings, marketing, portfolio scale, research activity, and stock-picking performance. Analyst ratings were from the bank's institutional sales force and clients. Marketing contributions were measured by the number of one-on-one meetings that each analyst held with the bank's buy-side clients (mean = 140.30), the number of corporate marketing events held by each analyst (mean = 8.22) and the number of company visits made by each analyst (mean = 2.75). The scale of an analysts' portfolio was measured by the aggregate market capitalization of covered stocks. Research activity proxies included the number of forecast revisions, the number of initiations, and the number of notes posted. Finally, stock-picking performance was measured using the annualized return to buy and strong buy recommendations. Although we do not have data on these variables for years prior to 2005, the bank's research staff informed us that similar measures were used in prior years.

The annual electronic files from our sample company contained 609 analyst-year observations for the period 1988 to 2005 (an average of 33.8 analysts per year) that overlapped the I/B/E/S database. However, our primary tests utilize observations from 1994 onwards, when the *WSJ* began rating analysts' stock-picking performance.[4] This sample comprises 401 analyst year observations (an average of 33.4 analysts per year).

3. *Analyst Compensation: 1988-2005*

Descriptive data on analyst compensation (in 2005-equivalent dollars) for the 609 analyst-year observations from the 1988-2005 files are reported in Figures 1–3. The dramatic changes in analysts' real compensation shown in Figure 1 were almost entirely attributable to

[4] The first *WSJ* report was published in September 1993; however, this initial report was less developed, utilized different eligibility criteria, and contained only a subset of the industries covered in later years. Consequently, we treat 1994 as the first year of the *WSJ* survey (from 1994 onward, all *WSJ* reports were published in June/July). Our results are unchanged if we begin the sample in 1993.

bonus awards; median real bonuses grew from $177,475 in 1994 to $940,007 in 2001, and declined to $450,000 in 2005 (see Figure 2). In contrast, median *real* salaries showed small but steady declines throughout much of the eighteen-year period, from $244,979 in 1988 to $175,000 in 2005 (see Figure 2), as modest *nominal* salary growth was more than offset by inflation. Consequently, salaries declined from 54% of total compensation in 1990 to 16% in 2002, and grew to 30% in 2005.

The large increases in compensation that occurred during the late 1990s were not shared equally among the firms' analysts. As shown in Figure 1, the variance and skewness of the income distribution increased substantially over the sample period, peaking in 2000-2002. In 1990, the ratio of analyst pay for the 90th and the 10th percentiles was 255%. By 2000, this ratio had more than doubled, to 610%. As bonuses declined from 2002 to 2005, the ratio dipped to 400%.

Figure 3 shows that the time-series variation in compensation is highly correlated with the Baker and Wurgler (2006) market activity index.[5] During periods in which activity and, as a result, trading commissions and corporate finance fees are high, the bank's bonus pool expands, leading to large increases in analyst compensation. Moreover, the strong relation between these variables between 1988 and 2005, combined with their simultaneous decline towards the end of our sample suggests that at least some of the post-2002 decrease in analyst compensation arises from a general decline in market activity, and thus cannot be solely attributed to the Global Settlement.

[5] The Baker and Wurgler (2006) index captures a variety of market activity signals including banking-related variables (such as IPO volume, first-day IPO returns, and the equity-share in new issues) as well as commission-related variables (such as average monthly turnover on NYSE-listed stocks).

4. *Drivers of Compensation*

In this section, we develop hypotheses about the drivers of sell-side analysts' compensation and describe the proxies used in our study. Our analysis draws on economic compensation theories and research findings that suggest that compensation is related to outcome- and action-based performance measures as well as job and human-capital characteristics.[6] We used the bank's 2005 analyst evaluation and development booklet, as well as field interviews at eleven investment banks and prior analyst research to identify variables that represent these factors.

In assembling these variables, which are defined in the Appendix, we ensured that their measurement intervals were consistent with the timing of compensation awards. Each variable's outcome is realized prior to the compensation award date, and is therefore a potential input to the compensation decision.

4.1. OUTCOME-BASED PERFORMANCE VARIABLES

Institutional Investor Ratings. Since 1972, *Institutional Investor* (*II*) has conducted an annual survey of buy-side institutions' ratings of sell-side analysts who "have been the most helpful to them and their institution in researching U.S. equities over the past twelve months" (*Institutional Investor* (1996 and 1997)). Based on these surveys, the October *II* magazine publishes a list of the top-three analysts and runners-up by industry. "All-Star" ratings are widely viewed as the most comprehensive public measure of analyst performance (e.g., Bradshaw (2008)). We construct an *II* All-Star indicator variable that takes the value one if an analyst at the sample firm is named by *II* as one of the top-three analysts or a runner-up in a given year, and zero otherwise.

[6] See Milgrom and Roberts (1992), Gibbons and Waldman (1999), Prendergast (1999 and 2002), Baker and Jorgensen (2003), and Raith (2008) for reviews of this literature.

Prior research suggests that All-Star analysts contribute to the performance of their investment banks by generating higher trading volume (Jackson (2005)) and attracting investment-banking clients (Dunbar (2000), Krigman et al. (2001), and Clarke, Khorana, Patel, and Rau (2007)). Among higher-status banks, *II*-ratings are likely to be highly correlated with client votes on the quality of analysts' research that are used to allocate transactions and hence commissions among banks. Data from the research director's 2005 performance-evaluation booklet indicates that among the ten analysts with the highest client votes, eight were *II*-rated; among the ten analysts with the lowest client votes, none were *II*-rated.[7]

Firm management and practitioners stated that banks prefer, when available, to tie analyst compensation to client votes (as opposed to the trading volumes and commissions of covered stocks). According to these insiders, client votes incorporate the impact of important externalities and better reflect the analyst's contribution to *total* (i.e., bank-wide) commission revenue. Votes are not as affected by the quality of the bank's traders. Further, an analyst's research on a stock can lead clients to continue holding the security and hence not affect trading. Yet clients that value such research typically reward the bank for the research by allocating it trading commissions on *other* stock transactions. Consistent with these arguments, O'Brien and Bhushan (1990, p. 59) observe "it is rare (and controversial) for research analysts' compensation to be explicitly based on commissions."

Investment-Banking Contribution. Our second outcome-based performance measure is the analyst's contribution to the firm's investment-banking operations. Analysts contribute to investment-banking deals by identifying potential issuers, providing valuable information on issuers to investors, and participating in road shows to sell issues to institutional clients. For each

[7] Fisher's exact test indicates that this association is statistically significant at the 1% level.

analyst-year, our primary banking variable is the annual equity underwriting fees earned by the bank from companies the analyst covered. Since the fees received by each bank are not publicly disclosed, we estimate the banking fees received from each deal using the following algorithm: the management fee, which typically accounts for 20% of the gross spread, is divided equally among the book-runners, and the underwriting fee and selling concessions are divided equally among all syndicate members.[8]

Stock Recommendation Performance. Prior research suggests that (changes in) stock recommendations have investment value for bank clients (Womack (1996), Irvine (2004), Green (2006), Jegadeesh, Kim, Krische, and Lee (2004), Juergens and Lindsey (2009)). Analysts with superior recommendation performance potentially create value by enhancing a bank's reputation in public research ratings in the *Wall Street Journal* that are based on recommendation performance, and by generating more commission revenues from clients who value their research. Many of the research directors we interviewed noted that they track analysts' recommendation performance. In addition, anecdotal evidence suggests that investment banks care about the *WSJ*'s ratings. For example, Merrill Lynch posted on its Web site the names of its nine analysts who made the 2005 *WSJ* rankings, and the head of the bank's Americas Equity Research commented on its strong ranking (Merrill Lynch (2005)).

Our primary measure of stock-picking ability is an indicator variable that takes the value one if the *WSJ* identified the analyst as one of the top-five stock pickers in his or her industry in

[8] This simple algorithm assumes an equal allocation across syndicate members. Although we are unaware of any research on SEO allocations, research on IPO allocations using proprietary data indicates that book-runners typically receive a larger share allocation (see Iannotta (2010) for a review of this literature). Consequently, we also estimated equity underwriting fees by dividing total fees by the number of book-runners. This approach implicitly assumes that a deal's book-runners were allocated 100% of the shares in the equity underwriting process. Our results are robust to this alternate fee estimation algorithm (see Section 6.4).

its annual ratings, and zero otherwise.[9] This variable is not susceptible to the data irregularities identified in Ljungqvist, Malloy, and Marston (2009). In addition, we examine the mean annualized raw return to Strong Buy and Buy recommendations, the approach used by the sample firm to measure recommendation performance. This measure is constructed by scaling the return to each "Buy" and "Strong Buy" recommendation by the number of days the stock is held relative to the number of days in the year. For example, if a buy recommendation generates a return of 7% for 60 days, the annualized return is 43% (7%*365/60).

Earnings Forecast Accuracy. Research on analysts' earnings forecasts finds more accurate forecasting to be associated with "favorable" job transitions (Mikhail et al. (1999) and Hong and Kubik (2003)) and top-tier investment banks to employ significantly more accurate forecasters (e.g., Clement (1999), Malloy (2005), and Cowen et al. (2006)). It thus appears that prestigious Wall Street research houses, such as our sample firm, demand forecast accuracy.

However, forecast accuracy was not formally tracked within the 2005 performance evaluation and development booklet we received from the sample bank. Consequently, we rely on prior literature to guide our choice of forecast accuracy index. We use analysts' most recent annual earnings forecasts[10] issued from 360 to 90 days prior to annual earnings announcements that fall within the compensation evaluation period.[11] Following Gu and Wu (2003), and Basu

[9] To be eligible, an analyst must cover five or more *qualified* stocks in the industry (i.e., stocks that trade above $2/share and have a market cap of more than $50 million), and at least two of the qualified stocks must be among the ten largest stocks in the industry (the theory is that no one can truly understand an industry without a thorough knowledge of at least some of its biggest firms). As noted by Emery and Li (2009), these eligibility conditions are generally non-binding for analysts at larger brokerage houses, such as the bank studied here and, conditional on eligibility, the ratings are entirely determined by stock-picking performance.

[10] Prior studies of analysts' incentives use annual (as opposed to quarterly) earnings forecasts (e.g., Hong and Kubik (2003), Ke and Yu (2006), Leone and Wu (2007), Ertimur, Mayew, and Stubben (2008), and Call, Chen, and Tong (2009)), and the most recent forecast issued (e.g. O'Brien (1990), Clement (1999), Jacob, Lys, and Neale (1999), Mikhail et al. (1999), Hong and Kubik (2003), Ke and Yu (2006)).

[11] Our results are robust to alternate windows, including 10-90 days before the announcement, 90-180 days before the announcement, 180-270 days before the announcement, and 270-360 days before the announcement. We also obtain similar results when we control for the length of the forecasting horizon.

and Markov (2004), we compute absolute (as opposed to squared) forecast errors for each analyst-firm-year. These unsigned errors are aggregated into a single relative performance score using the following formula: $100 - \frac{Rank_{ijt} - 1}{I_{jt} - 1} \times 100$, where I_{jt} is the number of analysts following firm *j* in year *t* and $Rank_{ijt}$ is analyst *i*'s accuracy rank, relative to all other analysts covering firm *j* in year *t* (see Hong and Kubik (2003) and Ke and Yu (2003)). Finally, we average the relative scores over all companies covered by an analyst within a performance-evaluation year.[12]

4.2. ACTION-BASED PERFORMANCE MEASURES

Earnings Forecast Update Frequency. This measure was included in the 2005 performance evaluation and development booklet we received from our sample bank, and was widely tracked by other banks we interviewed. It is also the most widely used action-based performance measure in the analyst literature. Jacob et al. (1999), Mikhail, Walther and Willis (2009), and Pandit, Willis, and Zhou (2009) all use it as a proxy for analyst effort. Theory predicts it should be strongly associated with incentive compensation (e.g., Holmström (1979)). Moreover, prior research suggests that this variable is a leading indicator of investment-banking revenues. Krigman et al. (2001), for example, find dissatisfaction with frequency of coverage to be a key determinant of firms' decisions to switch underwriters. Finally, frequent revisions may generate abnormal commission revenue (e.g., Juergens and Lindsey (2009)).

We compute earnings estimate update frequency as the number of annual forecasts issued by an analyst each year during the 360 to 90 days prior to a covered company's EPS announcement (broadly similar to the approach used in Hong, Kubik, and Solomon (2000)).

[12] Following Jacob et. al. (1999), Ke and Yu (2006), and others, we drop companies where $I_{jt} < 3$, since relative performance isn't meaningful in such situations.

Coverage Initiations. Our second action-based performance measure is the number of coverage initiations an analyst made. Initiations were widely tracked by the banks we interviewed, appeared in our firm's 2005 performance evaluation and development booklet, and have been the subject of prior academic research. Ertimur, Muslu and Zhang (2007) and Bradshaw, Richardson, and Sloan (2006), for example, cite anecdotes that suggest that analysts may be compensated on the basis of initiation frequency. Initiation frequency is the number of stocks newly covered by each analyst in a given year, excluding those when an analyst is first hired (see McNichols and O'Brien (1997), Irvine (2003), and Ertimur et al. (2007)).

4.3. JOB CHARACTERISTICS

Portfolio Scale. Research directors we interviewed commented that it is particularly important to have strong analysts covering large, highly traded stocks that have a disproportionate impact on the business, a claim supported by the large-sample evidence in Hong and Kubik (2003) and a recent report by Sanford C. Bernstein (Hintz, Werner, and St. John (2006)), which argues that analysts who cover large portfolios are more visible, generate greater commissions, and are allocated a large share of the firm's research resources. Because banks bid aggressively for the services of analysts with the requisite skill to cover these stocks, we expect a positive association between portfolio scale and compensation.

Consistent with the sample bank, we measure portfolio scale as the aggregate market capitalization of covered stocks.[13] To ensure that we capture scale and not the performance of covered stocks, we measure the market capitalization of each stock covered during year *t*, at the *beginning* of the performance-evaluation period (i.e., December 1st, *t-1*). Finally, to facilitate

[13] We also use the natural logarithm of the lagged dollar value of trading volume for all covered stocks during the performance year. The correlation between the market capitalization and the dollar trading volume is 0.87. The inferences from using this alternative scale measure are identical to those reported for market capitalization.

interpretation of the pay-scale relation, we take the natural logarithm of our portfolio scale proxy; consequently, our regression parameters can be interpreted as the partial elasticity of pay with respect to portfolio size (e.g., Rosen 1992).

4.4. HUMAN CAPITAL CHARACTERISTICS

Experience. If analysts learn important tasks such as mentoring through experience, and these benefits are not fully captured in our outcome- and action-based measures of performance, we should find a positive association between analyst compensation and experience. Equivalently, experience can be viewed as a control for unobservable performance variables. In our tests, analyst experience is defined as the number of years an analyst has been employed as a senior analyst.[14]

To preserve the sample firm's anonymity we do not report descriptive statistics for its analysts on the explanatory variables described above. However, unreported tests show that the sample analysts are indistinguishable from their peers at other top-20 rated firms in terms of *II* rating, experience, forecast accuracy, strong buy/buy recommendation performance, portfolio scale, number of firms covered, and the annual number of forecast revisions and stock initiations issued.

5. Main Results

Following other compensation studies and the guidance in Rosen (1992), we use logarithmic regressions to estimate the implicit weights placed on various measures by the sample firm's compensation system. Compensation response coefficients are estimated using

[14] Experience, as noted by Clement, Koonce, and Lopez (2007), is a broad concept, and different types of experience can be associated with different types of human capital. Our definition of experience is similar to Clement's (1999), and captures analysts' experience within the profession (as opposed to experience covering specific stocks, events, or transactions).

total direct compensation for the period 1994 to 2005. We estimate three models: a pooled model, a "within-analyst" fixed-effects model, and a "between-analyst" cross-sectional model. Significance levels for the first two models are based on heteroskedasticity-robust standard errors clustered by analyst and year (Petersen (2009)). Because the between-analyst cross-sectional model includes only one observation per analyst, we report significance levels based on White's (1980) heteroskedasticity-robust standard errors. To control for the effects of general market movements documented in Figure 3, we include lagged values of the Baker and Wurgler (2006) index in our regression models. Our results are quantitatively and qualitatively similar when we replace this index with a set of year indicator variables.

5.1. POOLED MODEL

The first column of Table 1 presents results for our pooled total compensation model. The estimated coefficients indicate that four variables (in addition to the market activity index) have economically and statistically significant associations with analyst pay: All-Star status, portfolio scale, investment-banking contributions, and recognition by the *WSJ* as a star stock-picker. The coefficients for forecast accuracy, number of forecast revisions, number of stock initiations, and analyst experience are statistically indistinguishable from zero.

The estimated All-Star coefficient is 0.476, which implies that, on average, total compensation of All-Star analysts was 61% higher than that of non-star analysts, holding other factors constant.[15] The parameter estimate on our portfolio scale variable, the natural logarithm of the lagged market capitalization of covered stocks, is 0.178, implying that an analyst whose portfolio is at the third market capitalization quartile earned approximately 41% higher total compensation than a peer who covered a portfolio at the first market capitalization quartile,

[15] A one-unit change in the explanatory variable X is associated with a $100(e^{b} - 1)$% change in compensation, where b denotes the estimated coefficient on variable X.

holding other factors constant. The investment-banking coefficient of 0.070 indicates that, on average, an analyst that generates $1 million in banking-related revenue will earn 7% more compensation than a peer with no banking contributions, holding other factors constant. Finally, the 0.209 *WSJ* coefficient indicates that analysts who are formally recognized by the *WSJ* for their stock-picking performance earn approximately 23% more than their unranked peers, holding other factors constant.

5.2. FIXED-EFFECTS MODEL

As noted by Murphy (1985), in a pooled compensation regression, the explanatory variables may be correlated with unobservable factors (such as talent/ability) that are the real drivers of compensation. A common approach to dealing with this concern is to use a fixed-effects model. Wooldridge (2002), among others, has shown that a fixed-effect specification provides consistent parameter estimates if choices (e.g., assignment or selection) are a function of the unobservable fixed-effects. By including analyst fixed-effects in our compensation model, we control for time-invariant cross-sectional differences in analyst ability, allowing us to examine whether "within-analyst" variation in our explanatory variables (e.g., *II* ranking, investment-banking relations) is related to "within-analyst" variation in compensation. Moreover, as noted by Murphy (1985), fixed-effects models provide the most reliable estimates of pay-for-performance sensitivities (i.e., incentives from annual compensation awards).[16]

[16] As an alternative to fixed-effects, some compensation studies employ first-differences (i.e., "changes"). As noted by Wooldridge (2002) and Cameron and Trivedi (2005), when the number of time periods equals 2, both estimators are equivalent. When $T > 2$ and the model is well specified, both estimators are unbiased and consistent. Consequently, differences between the estimators will reflect differences in efficiency. Which estimator is more efficient depends on the structure of the time-variant disturbance term. If the time-variant disturbance term approximates a random walk, first-differences are more efficient. If the time-variant disturbance is serially uncorrelated, fixed-effects are more efficient. In most cases, the truth lies somewhere in between. Thus, as a robustness test, we repeated each of our analyses using first-differences. Our inferences were unchanged.

The results of the fixed-effects model are reported in the second column of Table 1. Because fixed-effects require at least two observations for each analyst, the sample size declines from 401 to 374 analyst-year observations. Similar to our pooled results, we find that four variables are highly associated with compensation: All-Star status, portfolio scale, investment-banking transactions by covered firms, and star stock picking. The All-Star coefficient of 0.145 implies gaining/losing *II* status confers a 16% compensation premium/penalty. In unreported tests, we are unable to reject the hypothesis that the percentage increase in compensation from gaining All-Star status equals the percentage decrease in compensation from losing All-Star status. This fixed-effect estimate is considerably smaller than what was obtained from our pooled model (61%). There are two potential explanations for this difference. First, if All-Star status and analyst ability are correlated, some of the All-Star effect from the pooled model will be subsumed in the analyst fixed-effect. A second explanation is that there is a compensation premium for star analysts who are ranked highly year after year. This premium is reflected in pooled estimates, but not in the fixed-effect model because it is unusual for analysts who have been ranked for a number of years to lose their ranking.

The portfolio scale elasticity estimate of 0.069 implies that an analyst who increases the scale of her portfolio by 10% will, on average, increase her compensation by less than 1%. Although statistically significant, this estimated coefficient is considerably smaller than the corresponding figure from the pooled regression, indicating that the pooled compensation-scale relation partially reflects the matching of more talented analysts to larger portfolios of stocks (i.e., pay for ability). This result is consistent with the job characteristics literature prediction that highly talented analysts will be sought to cover economically important industries/portfolios of stocks and their high pay will reflect a scarcity rent. According to the theory, compensation and portfolio scale are jointly determined by (unobservable) abilities.

Our fixed-effect estimates indicate that gaining/losing “star stock-picker” status in the *WSJ* is associated with an 11% change in pay. We are unable to reject the hypothesis that the percentage increase in compensation from gaining *WSJ* recognition equals the percentage decrease in compensation from losing *WSJ* recognition. It is worth noting that, unlike the *II* coefficient from the fixed-effects model, which is only 30% as large as the corresponding pooled coefficient, the *WSJ* coefficient from the fixed-effects model is 50% as large as the corresponding pooled estimate. This finding is consistent with the evidence in Emery and Li (2009), who report that *WSJ* ratings are considerably less persistent than *II* ratings since they are solely determined by stock-picking performance, which is highly volatile (the year-to-year probability of retaining *II* Status is around 0.7; the year-to-year probability of retaining *WSJ* status is around 0.2).

The fixed-effect estimate for investment banking is 0.058, similar to the pooled estimate. Estimates for the remaining variables – forecast accuracy, forecast revisions, stock initiations, and experience – are insignificantly different from zero.

5.3 “BETWEEN-ANALYST” CROSS-SECTIONAL MODEL

Our third model uses the average values of compensation and independent variables for each analyst while employed at the sample firm. By including each analyst in the sample only once, this approach controls for any dependence among analyst observations (Greene (2000)).[17] More importantly, however, this approach mitigates any timing mismatches (i.e., lead/lag issues) in our independent variables and indicates whether consistently strong performance is rewarded

[17] Given that we report significance levels based on 2-way clustered standard errors (Petersen (2009)), pooled and fixed effect models should not be affected by dependence among analyst observations.

with greater pay over an analyst's tenure with the firm. Finally, it provides a bridge between the pooled model and the "within-analyst" fixed-effect model, as it represents the variation in the pooled model that has been purged from the fixed-effect model (Murphy (1985), Greene (2000), Verbeek (2005)).

The findings reported in the third column of Table 1 again indicate that analyst compensation is related to the same four analyst characteristics. Not surprisingly, the estimates are typically much larger than those reported for the "within-analyst" fixed-effect model. For example, the *II* All-Star estimate of 0.736 implies that, on average, analysts rated as All-Stars consistently throughout their employment at the sample firm earned 109% higher compensation than analysts who were never rated.

6. *Additional Analyses*

6.1. FORECAST ACCURACY

Finding no economically or statistically significant association between forecast accuracy and compensation is somewhat surprising given prior evidence that forecast accuracy is related to analysts' career prospects (see Mikhail et al. (1999) and Hong and Kubik (2003)). However, it is consistent with the inferences from our interviews at eleven leading banks and the fact that the research director's 2005 analyst evaluation and development booklet did not track analysts' forecasting performance. In this section, we provide additional analysis of analysts' forecasting incentives and examine whether the lack of association documented in Table 1 is an artifact of our research design.

6.1.1. Limited Variation in Forecast Accuracy for Sample Firm Analysts. If the sample firm selects and retains analysts with similar forecasting performance then there could be little variation in forecast accuracy across their analysts. This would reduce the power of our tests. To examine this possibility, we compare the variation in analyst forecast accuracy within the sample bank to the variation within I/B/E/S as a whole. Consistent with Ke and Yu (2006, Table 1 Panel C), the interquartile range of the relative forecast accuracy score within the I/B/E/S population is approximately 17. We find a similar interquartile range (approximately 16) among analysts employed by our sample bank, indicating that the insignificant forecasting results documented in Table 1 cannot be attributed to a lack of variation in our forecast accuracy measure. Although the average analyst at our bank is slightly more accurate than the average I/B/E/S analyst, there is considerable variation in forecasting performance within the bank.

6.1.2. Noisy Forecast Accuracy Measure, Correlated Regressors, and Small Sample Size. To investigate whether our findings are sensitive to the forecast accuracy metric employed, we construct four other forecast accuracy metrics that are popular in the analyst literature: (i) undeflated absolute forecast errors, (ii) price-deflated absolute forecast errors, (iii) the proportional mean absolute forecast error (PMAFE) metric reported in Clement (1999), Jacob, et al. (1999), and Clement et al. (2007), and (iv) the standard-deviation-deflated measure (PSAFE) reported in Groysberg, Healy, and Chapman (2008).[18] Each of these metrics is estimated using both annual and quarterly forecast data from I/B/E/S. Definitions are reported in Panel B of the Appendix. In addition to these self-constructed metrics, we use an indicator variable that takes the value one if the *WSJ* identified the analyst as one of the top-five forecasters in his or her industry in its annual ratings, and zero otherwise.

To ensure that the relation between compensation and forecast accuracy is not subsumed by other regressors, we exclude all other variables from the model except the lagged Baker and

[18] In unreported analyses we examine forecast metrics for the most recent two and three years (as opposed to the most recent year); year-to-year changes in forecast accuracy, as opposed to the level of forecast accuracy; the first (as opposed to last) forecast within the forecast window; squared, as opposed to absolute forecast errors; and analysts' median, as opposed to mean, forecast accuracy.

Wurgler index that controls for exogenous variation in the bank's bonus pool. For tests involving the I/B/E/S-based forecast accuracy metrics, dropping these variables also allows us to utilize the larger 1988-2005 sample, increasing the power of our tests. However, since the *WSJ* forecasting data are only available from 1994 to 2001 (the *WSJ* stopped tracking forecast accuracy after 2001), tests using the *WSJ* variable are confined to only eight years of data.

Pay-for-accuracy coefficients are estimated for each of our alternative forecast accuracy measures as well as for the average relative forecast accuracy score used above. Standardization of dependent and independent variables is a commonly used technique for comparing parameter estimates for a variable that is scaled in different ways (e.g., average undeflated forecast errors, which are reported in cents, and our primary forecast accuracy index, which is reported on a 0-100 relative scale). We therefore scale the compensation and explanatory variables to have zero mean and unit standard deviation. The estimated coefficients then indicate how a one standard deviation change in forecast accuracy affects compensation (in standard deviations). The estimates can also be interpreted as the partial correlation between compensation and accuracy.

Panel A of Table 2 reports standardized coefficients for the self-constructed accuracy metrics. The results indicate that the weak pay-for-accuracy relation documented in Table 1 is not an artifact of the forecast metric used, the inclusion of other (potentially correlated) variables, or the smaller 1994-2005 sample. Even with 18 years of data from one of the largest sell-side research departments on Wall Street, none of the forecast accuracy estimates is statistically or economically significant. Moreover, the primary forecast-accuracy variable (the average relative forecast accuracy score computed using annual forecasts) exhibits the strongest economic and statistical association with compensation among the ten I/B/E/S-based measures. [19] On average, a

[19] Interestingly, Ke and Yu (2006) report that this variable best explains analyst "firings."

one standard deviation increase in this accuracy measure increases compensation 0.031 standard deviations (p-value = 0.283), which is neither economically nor statistically significant at conventional levels.

The standardized *WSJ* forecast accuracy estimated coefficient is reported in Panel B of Table 2. Since it is a binary variable and only available for eight of the eighteen years examined in Panel A, the magnitude and significance of the *WSJ* parameter estimate cannot be directly compared to those of the I/B/E/S-based measures reported in Panel A. However, the results are quite consistent - appearing in the *WSJ*'s star forecaster table does not have a meaningful impact on analyst compensation. Gaining/losing star status is associated with a 0.046 standard deviation change in compensation (p-value = 0.681).

6.1.3. Economic vs. Statistical Significance. The above analyses indicate that many factors that weaken statistical power (i.e., lack of variation, a noisy regressor, colinearity, and small sample size) do not appear to drive our forecast-accuracy findings. Instead, the weak statistical significance appears to reflect weak economic significance. To further investigate this issue and put an upper-bound on the pay-for-accuracy relation, we use the 1988-2005 sample and regress the natural log of analyst compensation on our primary forecast accuracy measure, the Baker and Wurgler control variable, and a set of analyst fixed-effects. The results (untabulated) indicate that the upper bound of the 95% confidence interval for the forecast accuracy coefficient is 0.0033. Thus, moving from the bottom to top quartile of the relative forecast accuracy distribution is associated with *at most* a 5.4% increase in pay. This further indicates that the lack of statistical significance is driven by weak economic significance, not low statistical power.

6.1.4. Forecasting and Analyst Turnover. Although we are unable to detect an association between annual compensation and forecast accuracy, it is important to note that annual compensation captures only a portion of analysts' incentives. If subsequent employers use past- and present-period performance realizations to make posterior assessments of analysts' abilities, then even in the absence of a formal bonus-based contract an analyst may face strong forecasting incentives (e.g., Holmström (1999)). When faced with an outside offer from a rival, analysts at high-status banks typically receive matching offers from their current employers (e.g., Gasparino (2005)). As a result, Ke and Yu (2006, p. 970) argue that analysts' career incentives are more influenced by threat of dismissal than the prospect of receiving an offer from a higher-ranked firm. To assess the performance-dismissal relation at our sample firm, we examine the characteristics of "fired" analysts (i.e., analysts who move from our high-status bank to a lower-status I/B/E/S employer or exit I/B/E/S completely). Univariate tests indicate that "fired" analysts had larger absolute forecast errors than other analysts who covered the same stocks, and larger relative forecast errors than other analysts within the sample firm. These findings suggest that forecasting is related to turnover, but not compensation. It also increases our confidence that our compensation-forecast findings are not attributable to an outlier sample firm (see Section 6.6 for additional discussion of generalizability and external validity).

6.2. STOCK-PICKING

Our primary tests, reported in Table 1, indicate that top stock pickers, as rated by the *WSJ*, are paid significantly more than their non-rated peers and that intra-analyst variation in *WSJ* status has a meaningful effect on analyst compensation. Since the *WSJ's* ratings are based solely on stock-picking performance, our tests indicate that stock-picking performance has implications for analyst compensation. This finding is noteworthy given that both Mikhail et al. (1999) and Hong and Kubik (2003) do not find a significant association between stock-picking performance and analyst turnover.

In this section of the paper we provide a more detailed investigation of analysts' stock picking incentives. In addition to the *WSJ* variable from our primary tests, we examine the metric used by the sample bank for internal purposes (i.e., analyst evaluation and development). As discussed in Section 4.1, this measure is constructed by scaling the recommended holding period return of each "buy" and "strong buy" recommendation by the number of recommended holding days relative to the number of days in the year. For robustness, we also examine two measures based on the calendar-time, long-window abnormal-return methodology in Barber, Lehavy, and Trueman (2007) (hereafter, BLT). To implement their approach we create a portfolio of Buy/Strong Buy recommendations and estimate daily returns to this portfolio using the daily rebalancing technique described in BLT. We then estimate each analyst's abnormal stock picking performance for a given year as the intercept, α_{it}, from the following daily time-series regressions:

(BLT$_1$) $$r_{it}^{d} - r_{t}^{fd} = \alpha_{it} + \beta_{it}(r_{t}^{md} - r_{t}^{fd}) + \varepsilon_{it} \; r_{it}^{d} - r_{t}^{fd} = \alpha_{it} + \beta_{it}(r_{t}^{md} - r_{t}^{fd}) + \varepsilon_{it}^{d}$$

(BLT$_2$) $$r_{it}^{d} - r_{t}^{fd} = \alpha_{it} + \beta_{it}(r_{t}^{md} - r_{t}^{fd}) + s_{it}SMB_{t}^{d} + h_{it}HML_{t}^{d} + w_{it}WML_{t}^{d} + \varepsilon_{it}$$

where r_{it}^{d} is the portfolio return on day *d* for analyst *i* in year *t*; r_{t}^{fd} is the CRSP daily risk-free return on day *d* in year *t*; r_{t}^{md} is the daily return on the CRSP value-weighted market index; SMB_{t}^{d} is the return on day *d* in year *t* of a value-weighted portfolio of small stocks minus the return on of a value-weighted portfolio of big stocks; HML_{t}^{d} is the return on day *d* of year *t* of a value-weighted portfolio of high book-to-market stocks minus the return on a value-weighted portfolio of low book-to-market stocks; WML_{t}^{d} is the return on day *d* of year *t* on a value-weighted portfolio of stocks with high recent returns minus the return on a value-weighted portfolio of stocks with low recent returns.[20] To ensure compatibility with the bank's stock-picking metric, we annualize the analyst-year alphas by multiplying them by 365.

During the 1994-1999 (2000-2005) period, when the *WSJ's* analyses were performed by Zack's (Thomson/First Call), stock-picking performance was measured over the period December 1 – November 30 (January 1 – December 31). However, the results of the analyses did not appear in print until June or July of the following year. Given the bank's performance evaluation period runs from December 1 to November 30, this implies that compensation at the bank was related to one-year lagged (rather than current year) stock picking performance. Unreported univariate findings confirm that analysts who received *WSJ* awards had strong stock picking performance one year prior to the award but not in the award year itself.[21]

Table 3 reports pay-for-performance sensitivities (i.e., fixed-effect regression parameters) for contemporaneous WSJ award status and one-year lagged stock-picking performance using the bank's average annualized return metric and the two BLT measures. Since stock-picking performance may be associated with other variables, such as II status, we exclude all other variables from the model except lagged market activity index.

Several points are worth noting. First, after we drop all other variables from the model, the WSJ coefficient rises from 0.107 (in Table 1), to 0.148, implying that gaining/losing WSJ status is associated with a 16% change in compensation. Second, among the three alternate stock picking measures, lagged return performance using the bank's metric is most strongly associated with analyst compensation. However, while this effect is statistically significant (p-value < 0.001), it is not economically large, especially when compared to the effect of WSJ recognition.

[20] We thank Ken French for providing daily factor returns.

[21] Consistent with prior research (Emery and Li, 2009), for the sample firm, award-winning analysts' buy recommendations outperformed those issued by their peers by 30% per annum using the bank's annualized return metric and the annualized market-adjusted BLT alpha, and by approximately 11% using the annualized four-factor-adjusted BLT alpha. In contrast, during the award year itself, their superior returns were 4% using the bank's annualized return metric and 0% using on the two BLT measures.

The estimate of 0.066 implies that the average WSJ rated analyst who generated a lagged buy recommendation return of 30% earned only 2% (0.066 × 0.30) additional compensation from this performance, versus 16% higher pay for being rated by the WSJ. Third, there is no association between analyst compensation and the four-factor-adjusted BLT alpha, our most sophisticated measure of stock-picking performance. Finally, unreported tests show that compensation is unrelated to contemporaneous stock picking performance using any of the three return metrics.

6.3. INSTITUTIONAL INVESTOR RATINGS

In 1995, *Institutional Investor* began selling comprehensive information to investment banks on the number of votes received by all analysts who received one or more client votes within a given industry. This information, which we obtained for the years 1996-2002, partitions analysts who did not appear within the October edition of *Institutional Investor* magazine (i.e., analysts who did not receive at least a runner-up rank) into (i) analysts who received at least five votes, but not enough votes to appear in the magazine, (ii) analysts who received between one and four votes (termed “honorable mentions”), and (iii) analysts who received no votes.

The fixed-effect estimates (untabulated) are 0.487 for All-Star analysts named in *II Magazine* and 0.377 for analysts who received at least five votes but were not rated All-Stars, both highly significant. The estimate for analysts with between one and four votes (0.031) is statistically insignificant. These estimates imply that moving from no votes to All-Star status was associated with a 63% increase in pay and moving from no votes to at least 5 votes (but not enough votes to become an All-Star) was associated with a 46% increase in pay, whereas moving from no votes to four or fewer votes did not lead to a statistically significant increase in pay. It thus appears that the compensation allocation process is designed not only to reward the top-rated analysts, but also those analysts with moderate ratings from institutional clients. It is also consistent with *II*-ratings proxying for client votes on analyst research quality that are used to allocate commissions across banks. Merely appearing in *II*'s magazine (i.e., "Stardom") isn't all that matters – the number of votes also counts. In fact, controlling for the number of votes, we are unable to reject the hypothesis that compensation is unrelated to whether an analyst appears as an "All-Star" in *II*'s magazine.

6.4. INVESTMENT BANKING CONTRIBUTIONS

The analyst investment-banking variable used in our tests is the estimated equity underwriting fees earned by the bank from companies the analyst covers. This incorporates both book-runner- and syndicate-based deals and was chosen based on discussions with research staff at several major banks.

To evaluate whether our findings are sensitive to this proxy, we also examined a number of other specifications. These included (i) the number of firms covered by an analyst in a given year that hired the bank to be a book-runner on an equity transaction, (ii) equity book-runner fees to the bank in a given year from firms covered by an analyst, (iii) the number of firms covered by an analyst in a given year that hired the bank to be a book-runner or a syndicate participant on an equity transaction, (iv) estimated fees to the bank in a given year for book-runner/syndicate participation in equity and debt transactions, and for M&A advising from firms covered by an analyst, and (v) equity book-runner and syndicate fees to the bank in a given year from new and past client firms covered by an analyst.

The results (unreported) provide strong and consistent support for our earlier inferences. First, there is a strong positive association between compensation and equity underwriting fees. Larger deals, however measured, are clearly associated with higher pay. When we re-estimate

Model 2 from Table 1 using the alternate banking measures, our fixed-effects estimates indicate that an average equity underwriting transaction is associated with a 7.5% pay premium. The rewards for book-runner transactions are larger, around 9-10% per transaction, reflecting their larger fees. The reward per dollar of fees, however, is the same across book-runner- and non-book-runner-based deals; each million in fees is associated with a 6-7% pay premium. Second, the compensation effects of equity transaction fees are similar for new and existing clients (0.057 and 0.061, respectively). Thus, little is lost by combining these transactions, as was done in our primary tests and in the remainder of the paper. Third, we are unable to reject the null hypothesis that sell-side equity analyst compensation is unrelated to debt underwriting and M&A fees from covered stocks, further validating our emphasis on equity underwriting transactions. Finally, changing our investment banking proxy does not have a material impact on our model's other parameters, further supporting the validity of our primary model.

6.5. HECKMAN SAMPLE SELECTION MODEL

Prior research has shown that analysts at high-status banks differ from analysts at lower-status banks along several observable dimensions (e.g., Clement (1999)). Consequently, one could argue that any analyst employed by the sample bank who does not match this profile must have some desirable, offsetting, unobservable characteristics; otherwise our sample bank would not retain the analyst's services. If these unobservable factors are also related to analyst compensation, conditioning the compensation regression on analysts employed by only our high-status bank could induce an association between these unobservable compensation determinants (i.e., the residual in our compensation regression equation) and the explanatory variables.

Our primary analyses employed several econometric approaches to mitigate this concern. First, if these unobservable factors are relatively stable over time, they will be absorbed in our fixed-effect model, leaving our "within-analyst" inferences unaffected. As noted in Section 5.2, this is one of the primary benefits of panel data. Second, we include directly in our compensation equation a wide variety of variables that have been shown in prior research to be associated with "high-status" analyst employment. By decreasing the variance of our residual term, we reduce the potential for sample-selection bias since sample selection bias is confined to selection on "unobservables" (e.g., Vella (1998)).

Finally, to formally deal with time-variant unobservable factors in our pooled specification, we employ what Heckman and Robb (1985) refer to as a control function estimator. In this approach, a function of one or more variables is entered into the regression in an attempt to eliminate the correlation between the explanatory variables and the error term. Our control function is a variant of Heckman's (1979) λ and approximates E[$\sum \rightarrow$X, High-Status Employment] based on the outputs from a first-stage probit equation estimated on the population of I/B/E/S analysts. Intuitively, this term captures the possibility that (i) any analyst employed by our bank who has poor observable characteristics could have some other unobservable skills that led the bank to retain the analyst and (ii) these unobservable characteristics are related to compensation. By including this sample-selection control in the compensation equation, we purge any potentially induced associations (i.e., any potential sample-selection bias).

As noted by Wooldridge (2003, p. 588), in order for this method to work well, all variables from our compensation regression (i.e., the X from the conditional expectation operator shown above) must also appear in first-stage probit model. In addition to this condition, when estimating this first-stage probit equation, we follow the guidance in the applied econometrics literature (e.g., Vella (1998)) and include an additional regressor that is not included in our compensation regression: relative optimism.[22] Cowen et al. (2006) show that analysts at high-status banks have lower relative optimism than analysts at lower-status banks. But for a variety of reasons (many legal), banks' bonus plans do not explicitly reward optimism, supporting our exclusion of this variable from the compensation model. Further, there is no reason to expect an implicit link between optimism and compensation conditional on the other variables included in our compensation regression (e.g., investment-banking contributions).

The estimates (untabulated) suggest that our inferences are not driven by sample-selection bias. In the second-stage pooled regression model, the sample selection control is both economically and statistically indistinguishable from 0, implying that selectivity does not affect the validity of the model (see Davidson and MacKinnon (2004, 489)).[23] In addition, the other parameter estimates are virtually identical to those reported in Table 1.

6.6. GENERALIZABILITY

Due to data limitations, our sample is made up entirely of analysts from one firm. This restriction reduces the likelihood that our results are due to a spurious correlation caused by unobserved heterogeneity – a claim supported by the battery of robustness tests reported above. But it also raises a question about whether the findings can be generalized to other top-tier firms. Expressed somewhat differently, although we have taken steps to ensure and document the *internal* validity of our study, we have not provided any evidence on its *external* validity.

Our interviews with research directors indicated a remarkable consistency in the performance metrics used for determining analyst bonus awards. According to the research

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[22] Strictly speaking, an additional regressor is not required for identification because the control function is non-linear and the rest of the regression equation is linear. In practice, however, estimation benefits from an exclusion restriction (e.g., Verbeek (2005)).

[23] The highest variance-inflation-factor is less than 3, suggesting that colinearity-induced fragility and colinearity more broadly are not serious concerns.

directors we interviewed, two mechanisms ensure that compensation practices remain similar across top-tier banks. The first is considerable inter-firm job-hopping by analysts and research directors, which should facilitate the transfer of performance evaluation and remuneration practices across firms (Frederickson, Peffer, and Pratt (1999)). Second, compensation benchmarking is widespread on Wall Street.[24] Moreover, consistent with claims that analysts face similar remuneration practices and incentives across top-tier employers, prior research finds no evidence of changes in behavior when analysts move from one full-service investment bank to another (Clarke, Khorana, Patel, and Rau (2007)).

Nevertheless, to provide additional evidence on the robustness and generalizability of our findings, we re-estimate our regression equations using data from a different top-20 investment bank that provided us with annual total compensation data for 240 analyst-year observations in years 1988 to 1993. Over this period, the mean (median) real compensation (in 2005 dollars) for analysts at the second firm was $530,862 ($505,848), which is quite similar to the mean (median) real compensation for analysts at our primary firm of $545,177.40 ($525,386.24) over the same period.

Compensation regressions for the primary and secondary firms (firms 1 and 2, respectively), as well as statistical tests of differences, are reported in Table 4 (for brevity, we report only the fixed-effects results). Our results are generally quite similar across the two banks, increasing our confidence that the sample firm findings are not purely idiosyncratic.

7. Conclusion

[24] For much of our sample period, one firm, McLagan Partners, provided most of the benchmarking data and consulting services for financial services and securities firms.

Prior research has shown that analysts at leading investment banks have the ability to drive security prices (e.g., Stickel (1995) and Womack (1996)), trading volume (e.g., Irvine (2000, 2004) and Juergens and Lindsey (2009)), and corporate financing activity (e.g., Krigman et al. (2001)). Although much has been written about the explicit incentives of these important information intermediaries, prior hypotheses have not been subjected to direct empirical testing. As a result, financial economists have been unable to make *ceteris paribus* statements regarding the determinants of analysts' compensation. This study, which uses nearly two decades of compensation data from a large investment bank, is a first step towards closing this gap in the literature.

Prior studies have argued that analysts face strong, bonus-based forecasting incentives. This assumption is often motivated by associations between forecast accuracy and *Institutional Investor* "All-Star" status, which anecdotes link to analyst compensation (e.g., Stickel (1992)). Our paper challenges this view. Although All-Star status is strongly associated with analyst pay, the variation in All-Star status that drives analyst pay is orthogonal to forecast accuracy measured using a wide variety of forecast periods and estimation methods.

The compensation consequences of All-Star status cannot be solely attributed to All-Star analysts having greater investment banking deal flow. Controlling for investment banking contributions and a host of other variables, we find that *II*-ranked analysts earn 61% more than non-*II*-ranked analysts. Fixed-effects regressions that control for unobserved analyst heterogeneity show that gaining/losing *II* status confers an immediate compensation premium/penalty equal to approximately 16% of annual compensation. Additional tests show that analyst compensation is related to *II* votes even for analysts who are not mentioned in *II* magazine. *II* votes are highly related to buy-side client votes used to allocate commissions across banks. Together these relationships suggest that the *II* compensation effect represents analyst rewards from generating institutional trading commission revenues.

Not surprisingly, investment-banking contributions are an important determinant of analyst remuneration. However, only equity underwriting activities are associated with analyst pay. For our sample firm, we find no evidence that sell-side equity analyst compensation is related to debt or M&A underwriting. We find that larger equity underwriting deals are associated with larger rewards, but there is no evidence that analysts are paid more for deals involving new clients (as opposed to existing "relationship" clients).

Our findings also indicate that analysts are rewarded for profitable stock recommendations, but the effect is delayed and only economically significant if it generates visibility in the *Wall Street Journal*'s annual stock-picking report. Much of the *WSJ* effect appears to reflect WSJ recognition, rather than underlying performance. Also, tests indicate that the rewards to superior recommendation performance are received in the period in which the *WSJ's* awards are announced, not the preceding year, when then the superior performance took place.

Finally, analysts who cover large portfolios earn significantly more than analysts who cover smaller portfolios. This factor appears to arise from more talented analysts being matched to economically important industries/stocks and receiving higher pay for their ability.

Given that annual compensation captures only a portion of an analyst's total incentives, we examine the characteristics of analysts who were likely "fired" from our sample bank (i.e., analysts who moved to lower-status banks/brokerages or exited I/B/E/S). Consistent with prior literature, we find that "fired" analysts have large relative forecast errors in their final full year of employment. This finding, taken in conjunction with the lack of association between compensation and forecast accuracy, suggests that analysts' forecasting incentives resemble a Mirrlees contract. Under a normal range of forecast outcomes, there is no relation between forecast performance and compensation within banks, although extremely negative forecasting outcomes are associated with increased probability of dismissal. Given the recent growth in the number of forecast-accuracy-based analyst turnover studies (e.g., Ke and Yu (2006), Ertimur et al. (2008), Call et al. (2009), and Pandit et al. (2009)), this finding is important, as it suggests that researchers are looking in the right place, but should be careful when generalizing their inferences to discussions of analyst bonuses, as some have done.

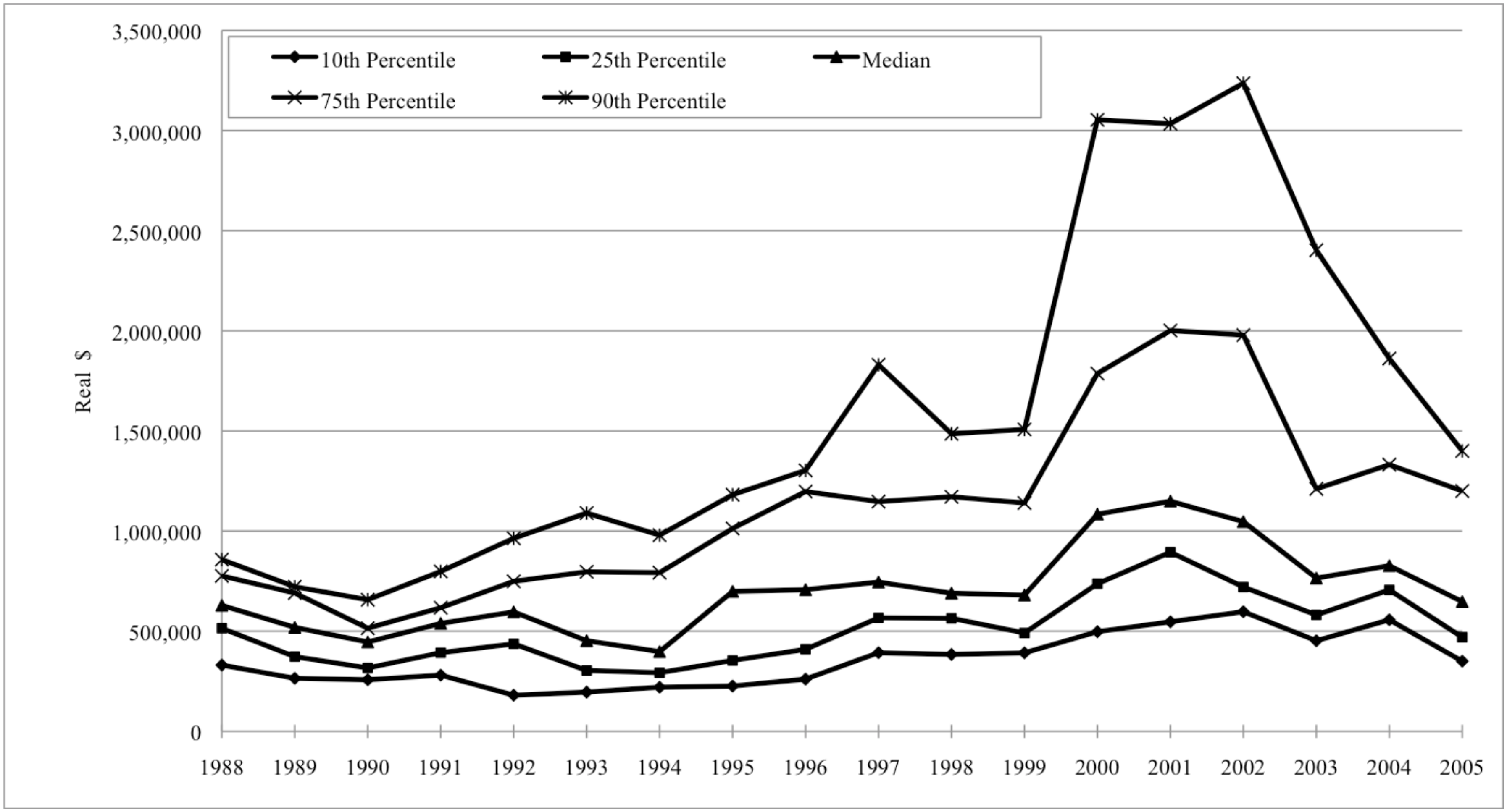
Our findings suggest several questions for future research. First, how do the compensation practices at banks vary depending on the sources of funding for research? For example, how are analysts compensated at brokerage firms and lower-status banks where analysts are typically not rated by *II* and where investment-banking opportunities are less prevalent? Second, our findings suggest that more research is needed to understand how clients rate analysts. How are client ratings affected by the quality of analysts' industry and firm analyses, their ability to provide access to corporate managers, and their responsiveness to client questions? Can analysts and banks influence client voting through lobbying? Do large institutions use *II* votes to reward analysts for the highly profitable selective prerelease of information documented by Irvine, Lipson, and Puckett (2007) and Juergens and Lindsey (2009)? Third, what is the relation between client votes, brokerage-level trading volume in covered stocks, and commission revenues? Fourth, given that analyst compensation is strongly associated with the market activity index, how is market activity related to analyst behavior?[25] Finally, given we are restricted from examining how the compensation relation changed

[25] See Bagnoli, Clement, Crawley, and Watts (2009), Hribar and McInnis (2009), Ke and Yu (2009), Mikhail et al. (2009), and Seybert and Wang (2009) for early research in this area.

following the Global Settlement, how effective has the new regulation been in limiting banks from tying compensation to banking deals?

FIGURE 1

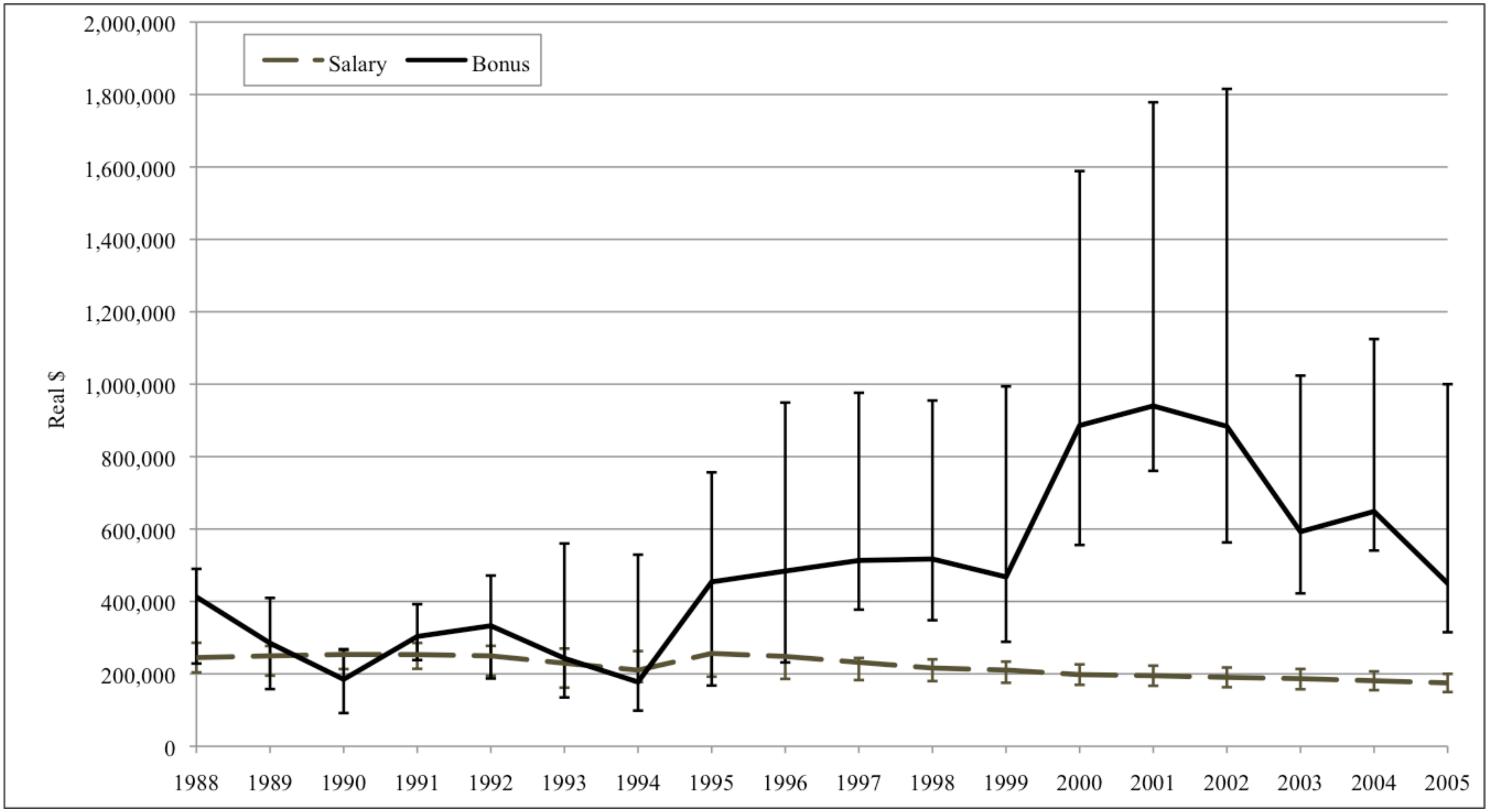
Sell-side Analysts' Total Compensation (in 2005 dollars)



This figure plots total real compensation for all I/B/E/S-listed, U.S. sell-side analysts from a major financial institution for the years 1988-2005. This figure is based on a total of 609 analyst-year observations. Total compensation equals salary plus bonus. Compensation data were inflation-adjusted using CPI data from the Federal Reserve Economic Database (FRED).

FIGURE 2

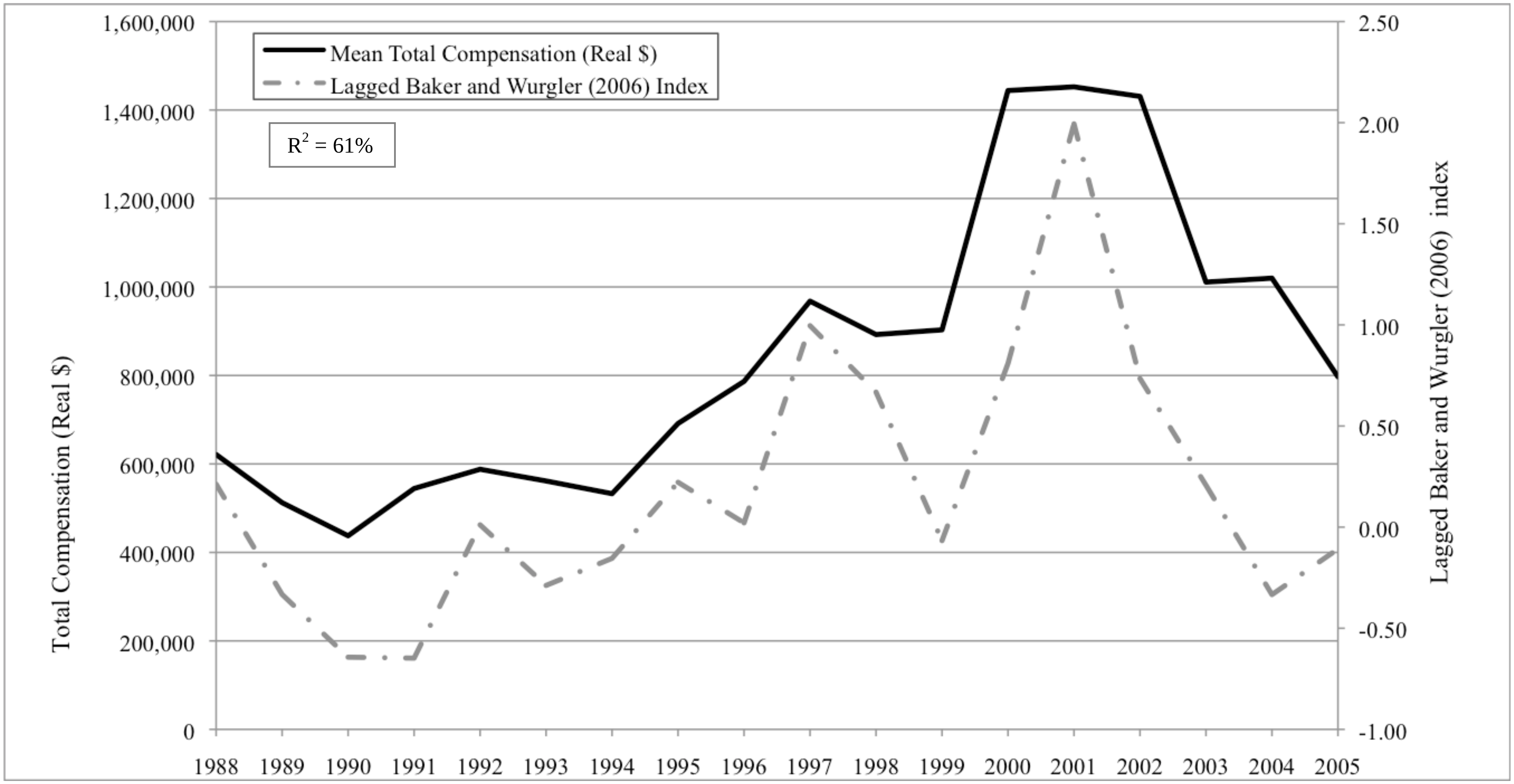
Sell-side Analysts' Salary and Bonus Compensation (in 2005 dollars)



This figure plots real salary and bonus compensation for all I/B/E/S-listed, U.S. sell-side analysts from a major financial institution for the years 1988-2005.This figure is based on a total of 609 analyst-year observations. Compensation data were inflation-adjusted using CPI data from the Federal Reserve Economic Database (FRED). Median salary (bonus) is denoted by the dashed (solid) line. Vertical bars denote the inter-quartile range (i.e., first and third quartiles) of the salary and bonus distributions.

FIGURE 3

Sell-side Analysts' Compensation and Capital Market Activity



This figure plots the relation between mean sell-side analyst compensation and capital market activity. The sample consists of all I/B/E/S-listed, U.S. sell-side analysts from a major financial institution for the years 1988-2005. Total compensation equals salary plus bonus. Compensation data were inflation-adjusted using CPI data from the Federal Reserve Economic Database (FRED). Capital market activity is measured using the Baker and Wurgler (2006) index and captures a variety of capital market activity signals including banking related variables (such as IPO volume, first day IPO returns, and the equity share in new issues), as well as commission-related variables (such as average monthly turnover on NYSE-listed stocks

TABLE 1
The Determinants of Analyst Compensation

		Coefficient estimates (*p*-values)		
		Model 1:	Model 2:	Model 3:
			"Within Analyst"	"Between Analyst"
	Pred.	Pooled	Fixed-Effects	Cross-Sectional
Institutional Investor "All-Star"	+	0.476***	0.145***	0.736***
		(<.001)	(0.003)	(<.001)
Log (lagged market capitalization of portfolio)	+	0.178***	0.069***	0.185***
		(<.001)	(0.012)	(<.001)
Investment banking contribution ($ mill)	+	0.070***	0.058***	0.102***
		(<.001)	(<.001)	(<.001)
WSJ star stock-picker	+	0.209***	0.104**	0.518*
		(0.009)	(0.042)	(0.059)
Average relative forecast accuracy score	+	0.001	0.000	-0.001
		(0.654)	(0.852)	(0.806)
Number of forecast revisions	+	0.000	0.001	-0.002
		(0.717)	(0.288)	(0.469)
Number of initiations	+	-0.005	0.006	0.013
		(0.610)	(0.417)	(0.575)
Analyst experience	+	0.000	0.019**	0.007
		(0.996)	(0.038)	(0.481)
Lagged Baker and Wurgler (2006) index	+	0.369***	0.310***	0.395**
		(<.001)	(<.001)	(0.028)
Number of observations		401	374	116
Adjusted R-square		0.45	0.83	0.53

*, **, and *** indicate significance at the 10%, 5% and 1% levels, respectively (based on a two-tailed t-test).

This table reports compensation response coefficients for analysts employed by a high-status investment bank for the years 1994-2005. Significance levels (reported in parentheses) are based on heteroskedasticity-robust standard errors, clustered by analyst and year, and test the null hypothesis that the respective coefficient is zero (Petersen (2009)). Model 3 is based on the mean value of each variable across all years in which the analyst was employed by the sample firm. Because this “between-analyst” cross-sectional model includes only one observation per analyst, we report significance levels based on White’s (1980) heteroskedasticity-robust standard errors. The dependent variable is the natural log of total compensation. All variables are defined in Panel A of the Appendix.

TABLE 2

Comparing Alternate Measures of Earnings Forecast Performance: Standardized Pay-for-Accuracy Coefficients

Panel A: Full Sample 1988-2005	Pred.	Annual Metrics: Standardized Coefficient Estimate	Annual Metrics: *p*-value	Quarterly Metrics: Standardized Coefficient Estimate	Quarterly Metrics: *p*-value
<u>*Absolute Accuracy:*</u>					
-(Average undeflated absolute forecast error)	+	0.006	0.812	-0.002	0.932
-(Average price-deflated forecast error)	+	0.005	0.854	0.021	0.359
<u>*Relative Accuracy:*</u>					
Average relative forecast accuracy score	+	0.031	0.283	0.000	0.985
-(Average PMAFE)	+	0.022	0.457	-0.004	0.870
-(Average PSAFE)	+	0.029	0.307	-0.008	0.753

Panel B: *WSJ* EPS Sample 1994–2001	Pred.	Standardized Coefficient Estimate	*p*-value
WSJ star EPS forecaster	+	0.046	0.681

*, **, and *** indicate significance at the 10%, 5% and 1% levels, respectively (based on a two-tailed t-test).

Panel A reports standardized pay-for-accuracy coefficients for a variety of forecast accuracy indices commonly used within the sell-side analyst literature. Each standardized pay-for-accuracy coefficient, β, is obtained by estimating the following fixed-effect regression on the 1988-2005 sample (N = 567):

$$Std.\ ln(Compensation_{it}) = \alpha_i + \beta\,(Std.\ Accuracy\ Index_{it}) + \delta\,(Std.\ Baker\,Wurgler_{t\text{-}1}) + \varepsilon_{it}$$

Std. ln(Compensation) is the natural logarithm of analyst compensation, rescaled to have a mean of zero and a standard deviation of one. *Std. Baker Wurgler* is the Baker and Wurgler (2006) market activity index, rescaled to have a mean of zero and a standard deviation of one. *Std. Accuracy Index* is the chosen forecast accuracy index averaged over all stocks within analyst *i*'s portfolio in year *t*, rescaled to have a mean of zero and a standard deviation of one. To be included in the annual metrics, an annual EPS forecast must be made between 90 and 360 days before the earnings announcement and the earnings announcement must occur within the compensation evaluation period. Each firm can contribute at most one forecast to the annual accuracy metric. To be included in the quarterly metrics, a one-quarter ahead EPS forecast must be made between 5 and 30 days before the quarterly earnings announcement and the earnings announcement must occur within the compensation evaluation period. Each firm can contribute, at most, four forecasts (one for each quarter), to the quarterly accuracy metric. Additional details on the forecast accuracy indices are provided in the variable definition table, which is reported in Panel B of the Appendix.

Panel B reports the standardized pay-for-accuracy coefficient for an additional variable. *WSJ star EPS forecaster*, a dummy variable that equals one if the analyst was named as one of the "Best on the Street" earnings forecasters by the *Wall Street Journal* in year *t*, zero otherwise.

Significance levels are based on heteroskedasticity-robust standard errors, clustered by analyst and year, and test the null hypothesis that the respective coefficient is zero (Petersen (2009)).

TABLE 3
Comparing Alternate Measures of Stock-Picking Performance: Pay-for-Performance Coefficients

		Fixed-Effect Coefficient Estimates (*p*-value)				
	Pred.	Model 1	Model 2	Model 3	Model 4	Model 5
WSJ star stock-picker	+	0.148 ***				0.134 **
		(0.008)				(0.015)
Average annualized return	+		0.066 ***			0.050 **
			(<.001)			(0.049)
Market-adjusted BLT (2007) portfolio alpha	+			0.053 ***		0.031
				(0.001)		(0.161)
Four-factor-adjusted BLT (2007) portfolio alpha	+				-0.008	-0.059 *
					(0.803)	(0.060)
Controlling for Baker and Wurgler (2006) index		Yes	Yes	Yes	Yes	Yes
Controlling for other determinants		No	No	No	No	No
Number of observations		339	339	339	339	339
Adjusted R-square		0.78	0.79	0.79	0.78	0.79

*, **, and *** indicate significance at the 10%, 5% and 1% levels, respectively (based on a two-tailed t-test).

This table reports pay-for-stock-picking-performance sensitivities for analysts employed by a high-status investment bank for the years 1995-2005. This period was chosen based on the availability of I/B/E/S recommendation data, which became available in 1994, and the need to ensure consistency with the *WSJ* ratings, which are based on lagged stock-picking performance. Significance levels (reported in parentheses) are based on heteroskedasticity-robust standard errors, clustered by analyst and year, and test the null hypothesis that the respective coefficient is zero (Petersen (2009)). The dependent variable is the natural log of total compensation expressed in real (2005-equivalent) terms. The stock-picking performance indices are defined in Panel C of the Appendix.

TABLE 4

Comparison with Compensation Practices at Another Top-Tier Bank

Fixed-Effects Coefficient Estimates (*p*-values)				
	Pred.	Firm 1	Firm 2	Difference Firm 2 - Firm 1
Institutional Investor "All Star"	+	0.237***	0.248***	0.011
		(0.002)	(<.001)	(0.905)
Log(lagged market capitalization of portfolio)	+	0.076**	0.049*	-0.027
		(0.033)	(0.060)	(0.540)
Investment banking contribution ($ mill)	+	0.034**	0.033***	-0.001
		(0.015)	(<.001)	(0.953)
Average relative forecast accuracy score	+	0.001	-0.001	-0.002
		(0.246)	(0.308)	(0.129)
Number of forecast revisions	+	0.001	-0.001	-0.002
		(0.630)	(0.344)	(0.327)
Number of initiations	+	0.000	0.001	0.001
		(0.973)	(0.910)	(0.967)
Analyst experience	+	-0.004	0.063***	0.067***
		(0.746)	(<.001)	(0.002)
Lagged Baker and Wurgler (2006) index	+	0.338***	0.249**	-0.089
		(<.001)	(0.035)	(0.492)
Number of observations		173	240	
Adjusted R-square		0.79	0.85	

*, **, and *** indicate significance at the 10%, 5% and 1% levels, respectively (based on a two-tailed t-test).

This table compares the compensation practices of our primary sample firm (Firm 1) to those of another top-tier investment bank (Firm 2) over the years 1988- 1993. This period was chosen based on the availability of data for Firm 2. Significance levels (reported in parentheses) are based on heteroskedasticity-robust standard errors clustered by analyst and year, and test the null hypothesis that the respective coefficient is zero (Petersen (2009)). The dependent variable is the natural log of total compensation. All variables are defined in Panel A of the Appendix.

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APPENDIX

Variable Definitions

For all variables, the subscript *i* refers to analyst *i*; the subscript *j* refers to stock *j*; and the subscript *t* refers to year *t*, defined as the period from December 1, *t-1* to November 30, *t*.

Panel A: Primary Variables

Variable	Definition
Ln(Compensation)$_{it}$	Analyst *i's* total compensation (salary + bonus) for year *t*, expressed in real (2005-equivalent) dollars (sources: proprietary compensation file, Federal Reserve Economic Database).
Institutional Investor "All-Star"$_{it}$	An indicator variable equal to one if analyst *i* was named as an "All-American" in the October year *t* issue of *Institutional Investor* magazine, zero otherwise (source: *Institutional Investor*).
Log(lagged market capitalization of portfolio)$_{it}$	For each of the *j* stocks in analyst *i*'s portfolio during year *t*, we measure the market capitalization on December 1, *t-1* (i.e., at the beginning of the performance evaluation period). Then we sum these amounts across the J_{it} securities in analyst *i*'s portfolio to estimate the lagged aggregate market capitalization of analyst *i*'s year *t* portfolio. Finally, we take the natural logarithm (sources: CRSP, I/B/E/S, Federal Reserve Economic Database).
Investment banking contribution ($ mill)$_{it}$	The estimated equity underwriting fees received by the sample bank from all firms covered by analyst *i* in year *t*, expressed in real (2005-equivalent) dollars. For deals in which the bank was a book-runner, fees are estimated as $\frac{\text{Management Fee}}{\text{\# of Book-runners}} + \frac{\text{Underwriting Fee + Selling Concession}}{\text{\# of Syndicate Members}}$. For deals in which the bank was not a book runner, fees are estimated as $\frac{\text{Underwriting Fee + Selling Concession}}{\text{\# of Syndicate Members}}$ (sources: SDC, I/B/E/S, Federal Reserve Economic Database).
WSJ star stock-picker$_{it}$	An indicator variable equal to one if analyst *i* was named as one the "Best on the Street" stock-pickers by the *Wall Street Journal* in year *t*, zero otherwise (source: the *Wall Street Journal*).

APPENDIX – *Continued*

Variable	Definition
Average relative forecast accuracy score$_{it}$	The average accuracy of analyst *i's* earnings forecasts in year *t*. For each of the J_{it} firms covered by analyst *i* in year *t*, we compute relative accuracy using the following formula: $100-\frac{Rank_{ijt}-1}{I_{jt}-1}\times 100$, where I_{jt} is the number of analysts following firm *j* in year *t* and $Rank_{ijt}$ is analyst *i*'s rank, relative to all other analysts covering firm *j* in year *t*, based on absolute forecast errors. See Figure A.1 for the timing and computation of absolute forecast errors (source: I/B/E/S).
Number of forecast revisions$_{it}$	The number of annual EPS forecasts issued between 90 and 360 days before the earnings announcement date (source: I/B/E/S).
Number of initiations$_{it}$	The number of new firms covered by analyst *i* in year *t*. Following McNichols and O'Brien (1997), we exclude initiations issued within the first six months of an analyst's appearance in I/B/E/S (source: I/B/E/S).
Analyst experience$_{it}$	The number of years that the analyst has appeared in I/B/E/S (source: I/B/E/S).
Baker and Wurgler (2006) index$_{t-1}$	The lagged value of the Baker and Wurgler (2006) activity index, which captures a variety of capital market activity signals including banking related variables (such as IPO volume, first day IPO returns, and the equity share in new issues) as well as commission-related variables (such as average monthly turnover on NYSE-listed stocks) (source: Baker and Wurgler).

Panel B: Additional Forecast Accuracy Variables (Table 2)

For all variables, $Actual_{jt}$ is the actual EPS announced by firm *j* during year evaluation year *t;* for the annual (quarterly) measures, $Forecast_{ijt}$ is the last EPS forecast issued by analyst *i* for company *j* between 90 and 360 days (5 and 30) before the announcement of $Actual_{jt}$; J_{it} is the number of stocks covered by analyst *i* during year *t;* I_{jt} is the number of analysts following stock *j* in year *t*; and $\overline{|Actual - Forecast|}_{jt} = \frac{1}{I_{jt}}\sum_{i=1}^{I_{jt}}|Actual_{jt} - Forecast_{ijt}|$.

Variable	Definition
Average undeflated absolute forecast error$_{it}$	$\frac{1}{J_{it}}\sum_{j=1}^{J_{it}}\left|Actual_{jt} - Forecast_{ijt}\right|$ (source: I/B/E/S).
Average price-deflated forecast error$_{it}$	$\frac{1}{J_{it}}\sum_{j=1}^{J_{it}}\frac{\left|Actual_{jt} - Forecast_{ijt}\right|}{P_{jt-1}}$, where P_{jt-1} is firm *j*'s stock price measured at the beginning of the evaluation period (source: I/B/E/S, CRSP).
Average DAFE$_{it}$	*Average DAFE$_{it}$* = $\frac{1}{J_{it}}\sum_{j=1}^{J_{it}}\left(\left|Actual_{jt} - Forecast_{ijt}\right| - \overline{\left|Actual - Forecast\right|}_{jt}\right)$ (source: I/B/E/S).
Average PMAFE$_{it}$	$\frac{1}{J_{it}}\sum_{j=1}^{J_{it}}\left(\frac{\left|Actual_{jt} - Forecast_{ijt}\right| - \overline{\left|Actual - Forecast\right|}_{jt}}{\overline{\left|Actual - Forecast\right|}_{jt}}\right)$ (source: I/B/E/S)
Average PSAFE$_{it}$	$\frac{1}{J_{it}}\sum_{j=1}^{J_{it}}\left(\frac{\left|Actual_{jt} - Forecast_{ijt}\right| - \overline{\left|Actual - Forecast\right|}_{jt}}{Std.Dev\left(\left|Actual - Forecast\right|\right)_{jt}}\right)$ (source: I/B/E/S)

APPENDIX – *Continued*

Panel C: Additional Stock-Picking Performance Variables (Table 3)

Variable	Definition
Average annualized return $_{i,t-1}$	The annualized return to buy and strong-buy recommendations, computed using the method employed by the sample bank (source: I/B/E/S, CRSP).
Market-adjusted BLT (2007) portfolio alpha$_{i,t-1}$	The long-window calendar-time "market-adjusted" alpha, obtained using the methodology in Barber, Lehavy, and Trueman (2007). To facilitate comparability with the *Average annualized return* metric employed by the sample bank, alphas are multiplied by 365 (source: I/B/E/S, CRSP, Ken French's website).
Four-factor-adjusted BLT (2007) portfolio alpha $_{i,t-1}$	The long-window calendar-time "four-factor-adjusted" alpha, obtained using the methodology in Barber, Lehavy, and Trueman (2007). To facilitate comparability with the *Average annualized return* metric employed by the sample bank, alphas are multiplied by 365 (source: I/B/E/S, CRSP, Ken French's website).