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## What Drives Sell-Side Analyst Compensation at High-Status Investment Banks?

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

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

## ABSTRACT

We use proprietary data from a major investment bank to investigate factors associated with analysts' annual compensation. We find compensation to be 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. But consistent with prior studies, we find analyst turnover to be related to forecast accuracy, suggesting that analyst forecasting incentives are primarily termination-based. Additional analyses indicate that "All-Star" recognition proxies for buy-side client votes on analyst research quality used to allocate commissions across banks and analysts. Taken as a whole, our evidence is consistent with analyst compensation being designed to reward actions that increase 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 and obtain 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.<sup>1</sup> Institutions and retail investors use equity research to help make investment decisions (e.g., Madan, Sobhani, and Bhatia [2003]) and corporations rely on sell-side equity analysts to market their securities and boost liquidity (e.g., Krigman, Shaw, and Womack [2001]).

But because of limited access to data on analyst pay, prior studies' assumptions about analyst 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 like Jack Grubman, Mary Meeker, and Henry Blodget (e.g., Gasparino [2005]). This paper uses analyst compensation data for the period 1988 to 2005 obtained from a prominent integrated investment bank to formally examine analyst compensation and its drivers.<sup>2</sup> Examining actual analyst compensation data enables us to test the incremental economic and statistical significance of investment banking, brokerage, and other factors for analyst pay, and thereby deepen our understanding of analysts' 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 analyst

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<sup>1</sup> 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.

<sup>2</sup> 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 to be associated with job turnover and career prospects, suggesting an association with compensation. But Mikhail et al. [1999] and Bradshaw [2008] note that this relation is not based on actual evidence and conflicts with practitioner assertions that such factors are not considered when making bonus awards.

We document several important facts, the first being 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 90<sup>th</sup> and 10<sup>th</sup> percentiles, which was 2.6 in 1990, increased to 6.1 in 2000, and had declined to 4.0 by 2005. Variation in the level and skewness of compensation over time was driven almost exclusively by bonus awards, which grew from a low of 46% of total compensation in 1990 to 84% in 2002, and had dropped to 70% by 2005. Collectively, the evidence indicates that during periods of high market activity and correspondingly high trading commissions and corporate finance fees, a bank's bonus pool expands, leading to large increases in analyst compensation. But 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, (i) recognition by *Institutional Investor (II)* as an "All-Star" analyst, (ii) recognition by the *Wall Street Journal (WSJ)* as a star stock-picker, (iii) an analyst's investment-banking contributions, and (iv) the size of an analyst's portfolio. Pooled regressions that 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 their banks 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 that rely on intra-analyst variation in performance and compensation. These indicate that, 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, for the sample firm’s analysts, forecast accuracy plays an insignificant role in determining compensation. Interviews with equity research professionals at eleven large banks (including the sample bank) as well as examination of the sample bank’s 2005 performance evaluation and development booklet support this inference. It is unusual for large banks to formally track forecast accuracy for compensation purposes. But we do find that inaccurate analysts at the 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 the sample firm. Taken as a whole, 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 increased probability of dismissal.<sup>3</sup>

Fourth, we find that much of the *WSJ* “star stock-picker” compensation premium appears to reflect 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 commonly tracked as part of banks’ analyst evaluation and development processes, insiders indicated that it generally is not a major determinant of analyst compensation.

Finally, we find that the *II* compensation premium can be explained by the underlying votes of institutional investors (i.e., the “buy-side”), which closely approximate the “broker votes” used to allocate commissions across banks and analysts. Controlling for institutional investors’ votes, the relation between compensation and “All-Star” rating in *II* becomes economically and statistically insignificant. In other words, the association between compensation and *II*-status does not appear to be attributable to the added visibility associated with being assigned star status in *II*’s October issue.

Our findings are robust to a battery of tests including alternative definitions of key variables, alternative measurement windows, sample-selection controls, first differencing (i.e., “changes”), and replication using data from a second high-status investment bank. Interviews with research directors at other leading banks indicated remarkable consistency in the

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<sup>3</sup> See Bolton and Dewatripont [2005] and Christensen and Feltham [2005] for a discussion of Mirrlees contracts and the “Mirrlees Problem.”

performance metrics used to determine analyst bonus awards, suggesting that our findings are likely to hold for other top-tier banks as well.

We nevertheless caution readers 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. We further recognize that the importance of investment banking to analyst compensation is likely to diminish following the Global Settlement. But we are unable to test this hypothesis given restrictions imposed by our sample firm and the limited number of post-settlement observations.

The paper is organized as follows. Section 2 describes the proprietary and public data used in the 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 results and suggestions for future research.

## 2. *Data*

The data used in this study were obtained from a proprietary compensation file and five publicly available sources: I/B/E/S, CRSP, SDC Platinum, *Institutional Investor*, and the *Wall Street Journal*. The proprietary compensation file is based on a set of spreadsheets obtained from a leading Wall Street investment bank for the years 1988-2005. The spreadsheets report the name, hire date, and compensation of each of the bank's analysts. No other variables are contained in these spreadsheets.

The bank's senior research staff also provided marked-up photocopies of the research director's 2005 analyst evaluation and development booklet. This booklet reports analyst



performance in a series of figures and tables that track five broad categories of metrics: analyst ratings, marketing, portfolio scale, research activity, and stock-picking performance. Analyst ratings were based on surveys of the bank's institutional sales force and clients. Marketing contributions were measured by the number of one-on-one meetings each analyst held with the bank's buy-side clients (mean = 140.30), and number of corporate marketing events held (mean = 8.22) and company visits made (mean = 2.75) by each analyst. The scale of an analyst's portfolio was measured as the aggregate market capitalization of covered stocks. Research activity proxies included number of forecast revisions, number of initiations, and number of notes posted. 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 sample company's annual electronic files 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. Our primary tests utilize observations from 1994, when the *WSJ* began rating analysts' stock-picking performance, onward.<sup>4</sup> This sample includes 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 attributable almost entirely to bonus awards; median real bonuses grew from \$177,475 in 1994 to \$940,007 in 2001 and

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<sup>4</sup> Because the first *WSJ* report, published in September 1993, was less developed, utilized different eligibility criteria, and contained only a subset of the industries covered in later years, 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.

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. 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 across the firm's 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 90<sup>th</sup> and 10<sup>th</sup> 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 the time-series variation in compensation to be highly correlated with the Baker and Wurgler [2006] market activity index.<sup>5</sup> During periods in which 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. Moreover, the strong relation between these variables between 1988 and 2005, and their simultaneous decline towards the end of our sample period, 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.

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

#### 4. *Drivers of Compensation*

In this section, we draw on results from the human capital acquisition, job assignment, and principal agent (i.e., incentive contracting) literatures to categorize the determinants of analyst compensation.<sup>6</sup> Traditional models of human capital acquisition and job assignment (e.g., Mayer [1960], Becker [1964], and Rosen [1982]) predict that compensation will be increasing in experience and the value of assets under an employee's control. These predictions follow from three assumptions, (i) productive talent is a scarce resource, (ii) productivity is increasing in experience and innate ability, and (iii) the marginal impact of workers' talent is increasing in the value of assets under their control, implying that only the most talented employees will be assigned to large, complicated, portfolios of tasks.

Traditional treatments of the principal-agent problem predict that "action-based" (that is, high signal-to-noise ratio) performance measures will be used extensively, as they allow stronger incentives without requiring a high risk premium for the employee (e.g., Banker and Datar [1989]). As noted by Baker [2002], however, action-based measures are "narrow" or "incomplete," potentially providing distorted incentives (i.e., if overemphasized, they incentivize the wrong behavior). "Outcome-based" performance measures, on the other hand, typically provide greater goal-congruence but require a higher risk premium for the employee. Moreover, in complex production environments in which each action's marginal product is state-contingent, tying their rewards to outcome-based measures provides employees with stronger incentives to utilize non-contractible (i.e., tacit), state-specific knowledge (Prendergast [2002], Baker and Jorgensen [2003], and Raith [2008]).

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<sup>6</sup> See Milgrom and Roberts [1992], Gibbons and Waldman [1999], and Prendergast [1999] for reviews of these literatures.

We use these insights to frame our investigation of the determinants of sell-side analyst compensation. Our choice of compensation determinants also is guided by the sample bank's 2005 analyst evaluation and development booklet as well as field interviews at eleven investment banks and prior analyst research.

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

#### 4.1. OUTCOME-BASED PERFORMANCE VARIABLES

*Institutional Investor Ratings.* Since 1972, *Institutional Investor (II)* has conducted annual surveys 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, 1997]). Based on these surveys, a list of the top-three analysts and runners-up by industry is published in the magazine's October issue. "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.

Prior research suggests that All-Star analysts contribute to the performance of their investment banks by generating higher trading volumes (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 (none of the ten analysts with the lowest client votes were *II*-rated).<sup>7</sup>

Firm management and practitioners stated that banks prefer to tie analyst compensation, when available, 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 an analyst's contribution to *total* (i.e., bank-wide) commission revenue. First, votes are not as affected by the quality of a 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 that "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 bank's investment-banking operations. Analysts contribute to investment-banking deals by identifying potential issuers, providing investors with valuable information about issuers, and participating in road shows to sell issues to institutional clients. For each analyst-year, our primary banking variable is the annual equity underwriting fees earned by the bank from the companies an 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,

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<sup>7</sup> Fisher's exact test indicates this association to be statistically significant at the 1% level.

is divided equally among the book-runners, and the underwriting fee and selling concessions are divided equally among all syndicate members.<sup>8</sup>

*Stock Recommendation Performance.* Prior research suggests that (changes in) stock recommendations have investment value for bank clients (Womack [1996], Irvine [2004], Jegadeesh, Kim, Krische, and Lee [2004], Green [2006], Juergens and Lindsey [2009]). Analysts with superior recommendation performance potentially create value by enhancing a bank's reputation in the *WSJ's* research ratings, which are based solely on recommendation performance, and by generating commission revenues from clients who value their research. Many of the research directors we interviewed indicated that they track analysts' recommendation performance, and 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 its Americas Equity Research commented on the bank's strong ranking (Merrill Lynch [2005]).

Our primary measure of stock-picking ability is an indicator variable that takes the value one if the *WSJ's* annual ratings identified the analyst as one of the top-five stock pickers in his or her industry, and zero otherwise.<sup>9</sup> In Section 6.2 we also examine the mean annualized raw return to buy and strong-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

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<sup>8</sup> 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).

<sup>9</sup> 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.

strong-buy recommendation by the number of days a 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% ( $7\% \times 365/60$ ).

*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, Groysberg, and Healy [2006]). It thus appears that prestigious Wall Street research houses like our sample firm demand forecast accuracy.

As discussed in Section 6.1, forecast accuracy was not formally tracked in the 2005 performance evaluation and development booklet received from the sample bank. Consequently, we rely on prior literature to guide our choice of a forecast accuracy index. We use analysts' most recent annual earnings forecasts issued from 360 to 90 days prior to annual earnings announcements that fall within the compensation evaluation period.<sup>10,11</sup> Following Gu and Wu [2003] and Basu 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:  $100 - \frac{Rank_{ijt} - 1}{I_{jt}} \times 100$ , 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 [2006]). Lastly, we

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<sup>10</sup> Prior studies of analyst 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], and Ke and Yu [2006]).

<sup>11</sup> 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 obtain similar results when we control for length of the forecasting horizon.

average the relative scores over all companies covered by an analyst within a performance-evaluation year.<sup>12</sup>

#### 4.2. ACTION-BASED PERFORMANCE MEASURES

*Earnings Forecast Update Frequency.* This measure was included in the 2005 performance evaluation and development booklet received from our sample bank and widely tracked by other banks we interviewed. It also is 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] use it as a proxy for analyst effort, which theory predicts 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 et al. [2000]).

*Coverage Initiations.* Our second action-based performance measure is the number of coverage initiations made by an analyst. Initiations were widely tracked by the banks we interviewed, appeared in the sample 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.

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<sup>12</sup> Following Jacob et al. [1999], Ke and Yu [2006], and others, we drop companies for which  $I_{jt} < 3$  because relative performance isn't meaningful in such situations.



#### 4.3. JOB CHARACTERISTICS

*Portfolio Scale.* Research directors we interviewed indicated that it is particularly important to have strong analysts cover large, highly traded stocks that have a disproportionate impact on the business. Their claim is supported by large-sample evidence in Hong and Kubik [2003] and a recent Sanford C. Bernstein report (Hintz, Werner, and St. John [2006]) that 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 portfolios, 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.<sup>13</sup> 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 1<sup>st</sup>,  $t-1$ ). Finally, to facilitate 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 like 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,

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<sup>13</sup> We obtain a similar, albeit weaker, association when we substitute number of stocks covered for market capitalization. When we include both market capitalization and number of stocks covered, only the market capitalization of covered stocks is economically or statistically significant in our model.

analyst experience is defined as the number of years an analyst has been employed as a senior analyst.<sup>14</sup>

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 the sample analysts to be 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 annual number of forecast revisions and stock initiations issued.

## 5. *Main Results*

Following other compensation studies and guided by Rosen [1992], we use logarithmic regressions to estimate the implicit weights the sample firm's compensation system places on various measures. Compensation response coefficients are estimated using total direct compensation for the period 1994 to 2005. We estimate three models, (i) a pooled model, (ii) a "within-analyst" fixed-effects model, and (iii) 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 in our regression models lagged values of the Baker and Wurgler [2006] index. Our results are quantitatively and qualitatively similar when we substitute for this index a set of year indicator variables.

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<sup>14</sup> 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] in that it captures analysts' experience within the profession (as opposed to experience covering specific stocks, events, or transactions).

## 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 of 0.476 implies that, on average, total compensation of All-Star analysts was 61% higher than that of non-star analysts, holding other factors constant.<sup>15</sup> 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, holding other factors constant. The investment-banking coefficient of 0.070 indicates that, on average, an analyst who 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 whose stock-picking performance is formally recognized by the *WSJ* 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

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<sup>15</sup> A one-unit change in the explanatory variable  $X$  is associated with a  $100 \cdot (e^b - 1)\%$  change in compensation, where  $b$  denotes the estimated coefficient on variable  $X$ .

drivers of compensation. A common approach to dealing with this concern is to use a fixed-effects model. Wooldridge [2002], among others, has shown a fixed-effect specification to provide consistent parameter estimates if choices (i.e., assignment or selection) are a function of the unobservable fixed-effects. Including analyst fixed-effects in our compensation model controls for time-invariant cross-sectional differences in analyst ability, which enables us to examine whether “within-analyst” variation in our explanatory variables (e.g., *II* ranking, investment-banking contributions) is related to “within-analyst” variation in compensation.<sup>16</sup>

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, sample size declines from 401 to 374 analyst-year observations. Similar to our pooled results, we find four variables to be 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 that gaining (losing) *II* status is accompanied by 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 that 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 a compensation premium accrues to star analysts who are ranked highly year after year. This premium is

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<sup>16</sup> 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 it 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.

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 the compensation of analysts who increase the scale of their portfolio by 10% will increase, on average, 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 or 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. Note that whereas the *II* coefficient from the fixed-effects model 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 evidence reported by Emery and Li [2009] that *WSJ* stock-picking ratings are considerably less persistent than *II* ratings (the year-to-year probability of retaining *II* status is approximately 0.7, the year-to-year probability of retaining *WSJ* status 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 during each analyst’s employment with the sample firm. By including each analyst in the sample only once, this approach controls for any dependence among analyst observations (Greene [2000]).<sup>17</sup> More important, it mitigates any timing mismatches (i.e., lead/lag issues) in our independent variables, and indicates whether consistently strong performance is rewarded with greater pay over an analyst’s tenure. Finally, it provides a bridge between the pooled model and “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 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]). But 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’

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

forecasting performance. When asked about this omission, the bank’s research director remarked: “I have never tracked it and nowhere that I have been did before I arrived. I don’t think it is any kind of acid test for whether an analyst has keen insight. If the clients pay attention to and pay for the services of an analyst, then that is a ‘good’ analyst, whether or not they get the earnings, or for that matter, stock prices, right.”

The fact that forecast accuracy was excluded from the bank’s analyst evaluation and development documents presents strong evidence that forecast accuracy is not a *direct* determinant of analyst compensation. It is possible, though, that forecast accuracy is implicitly rewarded through other mechanisms, such as *II* ratings. Consequently, in this section we provide additional analyses 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. 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].<sup>18</sup> 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 I/B/E/S-based metrics, we use an indicator variable that takes the value one if the *WSJ*’s annual ratings identified the analyst as one of the top-five forecasters in

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<sup>18</sup> 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.

his or her industry, 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 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 enables us to utilize the larger 1988-2005 sample, thereby increasing the power of our tests.

Pay-for-accuracy coefficients are estimated for each forecast accuracy measure as well as for the average relative forecast accuracy score used above. To compare the parameter estimates for these variables given their scale differences (e.g., average undeflated forecast errors are reported in cents and the forecast accuracy index is a 0-100 relative scale), we standardize 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).

Panel A of Table 2 reports standardized coefficients for the I/B/E/S-based 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.

The standardized *WSJ* forecast accuracy estimated coefficient is reported in Panel B of Table 2. Because it is a binary variable and available only 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



consistent with those reported earlier; appearing in the *WSJ*'s star forecaster table does not have a meaningful impact on analyst compensation.

*6.1.2. Limited Variation in Forecast Accuracy for Sample Firm Analysts.* To test whether the sample firm selects and retains analysts with similar forecasting performance, reducing the power of our tests, 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 our insignificant forecasting results are not due to lack of variation in our forecast accuracy measure.

*6.1.3. 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 base posterior assessments of analysts' abilities on past- and present-period performance realizations, then even in the absence of a formal bonus-based contract an analyst may face strong forecasting incentives (e.g., Holmström [1999]). Analysts at high-status banks faced with an outside offer from a rival 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 by 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). The results (untabulated) 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 to compensation. It also increases our confidence that our compensation-forecast findings are not attributable to an outlier sample firm (see Section 6.5 for further 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, 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 market- and four-factor-adjusted analyst-year alphas estimated using the calendar-time, long-window abnormal-return methodology in Barber, Lehavy, and Trueman [2007] (hereafter, BLT). These variables are discussed in greater detail in Panel C of the Appendix.

Since the *WSJ* ratings are released with a lag, there is a separation between the period in which performance occurs and the period in which the award is announced.<sup>19</sup> This implies that the effect of *contemporaneous WSJ* award status should be compared to the effect of *lagged* stock-picking performance, as captured by the bank's measure and the two BLT measures. Consistent with this argument, and the results in Emery and Li [2009], we find that analysts who received *WSJ* awards had strong stock-picking performance one year prior to, but not in, the award year.<sup>20</sup>

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 sample bank's average annualized return metric and the two BLT measures. Because stock-picking performance may be associated with other variables, such as *II* status, we exclude from the model all other variables except the lagged market activity index.

Several points are worth noting. First, when all other variables are dropped from the model, the *WSJ* coefficient rises from 0.107 (in Table 1) to 0.148, implying that gaining or losing *WSJ* status is associated with a 16% change in compensation. Second, among the three alternate stock picking measures, the bank's metric is most strongly associated with analyst compensation. But although statistically significant ( $p$ -value < 0.001), this effect is not economically large, especially when compared to the effect of *WSJ* recognition. The estimate of 0.066 implies that the average *WSJ*-rated analyst who generated a lagged buy recommendation return of 30%

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<sup>19</sup> As noted earlier, the bank's performance evaluation period runs from December 1 to November 30. During the 1994-1999 (2000-2005) period, when the *WSJ*'s report was prepared by Zack's (Thomson/First Call), stock-picking performance was measured over the period December 1-November 30 (January 1-December 31), but the results did not appear in print until June or July of the following year.

<sup>20</sup> For our sample firm, award-winning analysts' recommendations outperformed their peers' recommendations by 30% (11%) over the December 1-November 30 period *preceding* the award's announcement based on the bank's annualized return metric and the market- (four-factor-) adjusted annualized alpha. Over the December 1-November 30 period surrounding the award's June/July announcement, the performance differential dropped to 4% (0%) based on the bank's metric (the two BLT metrics).

earned only 2% ( $0.066 \times 0.30$ ) additional compensation from this performance, versus 16% higher pay for appearing in the *WSJ* report. 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 investment banks comprehensive information on the number of votes received by all analysts who received one or more buy-side votes within a given industry. This information, which we obtained for the years 1996-2002, partitions analysts who did not appear in the October issue of *Institutional Investor* (i.e., analysts who did not receive at least a runner-up rank) into (i) analysts who received at least five, 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* 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 economically and 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, but moving from no votes to four or fewer votes did not lead to a significant increase in pay. It thus appears that the compensation allocation process is designed to reward not only top-rated analysts, but

also analysts with moderate ratings.<sup>21</sup> It is also consistent with *II*-ratings proxying for the institutional client votes that are used to allocate commissions across banks and analysts.<sup>22</sup>

Merely appearing in *II*'s magazine (i.e., having “Stardom”) is not all that matters; the number of votes also counts. In fact, when we re-estimate the fixed-effects model reported in Table 1 with an additional variable, the number of votes received by each analyst in the *II* poll, the *II* All-Star indicator variable becomes economically and statistically indistinguishable from zero. Thus, 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 the companies covered by an analyst. 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 examined a number of other specifications including (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 for firms covered by an

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<sup>21</sup> Similarly, among analysts that appear in *II*'s October issue, we find a significant step in compensation between first- and second-ranked analysts and between second- and third-ranked analysts, but not between third-ranked and runner up analysts.

<sup>22</sup> As discussed in Section 4.1, interviews and the research director's 2005 performance-evaluation booklet indicate that client votes and *II* votes exhibit significantly positive associations.

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, 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, approximately 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. GENERALIZABILITY

Due to data limitations, our sample is composed 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 our 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 of its *external* validity.

Nevertheless, our interviews with research directors indicated remarkable consistency in the performance metrics used to determine analyst bonus awards. According to the research 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.<sup>23</sup> Moreover, consistent with claims that analysts encounter 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 et al. [2007]).

To provide additional evidence of the robustness and generalizability of our findings, we re-estimate our regression equations using data from a different top-20 investment bank from which we obtained annual total compensation data for 240 analyst-year observations over the years 1988 to 1993. During this period, mean (median) real compensation (in 2005 dollars) for analysts at this second firm was \$530,862 (\$505,848), quite similar to the mean (median) real compensation for analysts at our primary firm, which was \$545,177 (\$525,386) 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

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<sup>23</sup> One firm, McLagan Partners, provided most of the benchmarking data and consulting services for financial services and securities firms for much of our sample period.

report only the fixed-effects results). The similarity of results across the two banks increases our confidence that the sample firm findings are not purely idiosyncratic.

## 7. *Conclusion*

Prior research has shown analysts at leading investment banks to 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, because prior hypotheses have not been subjected to direct empirical testing, 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 are linked anecdotally 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 attributed solely 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 analyst compensation to be related to *II* votes even for analysts not mentioned in *II* magazine. *II* votes are strongly related to buy-side client votes, which are used to allocate commissions across banks. Together these relationships suggest that the *II* compensation effect likely represents rewards for generating institutional trading commission revenues.

Not surprisingly, investment-banking contributions are an important determinant of analyst remuneration. But 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 (as opposed to existing “relationship”) clients.

Our findings also indicate that analysts are rewarded for profitable stock recommendations, but the effect is delayed and economically significant only if it generates visibility in the *WSJ*'s annual stock-picking report. Recognition rather than underlying stock-picking performance seems to account for much of the *WSJ* effect. Also, tests indicate that the compensation rewards for 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 occurred.

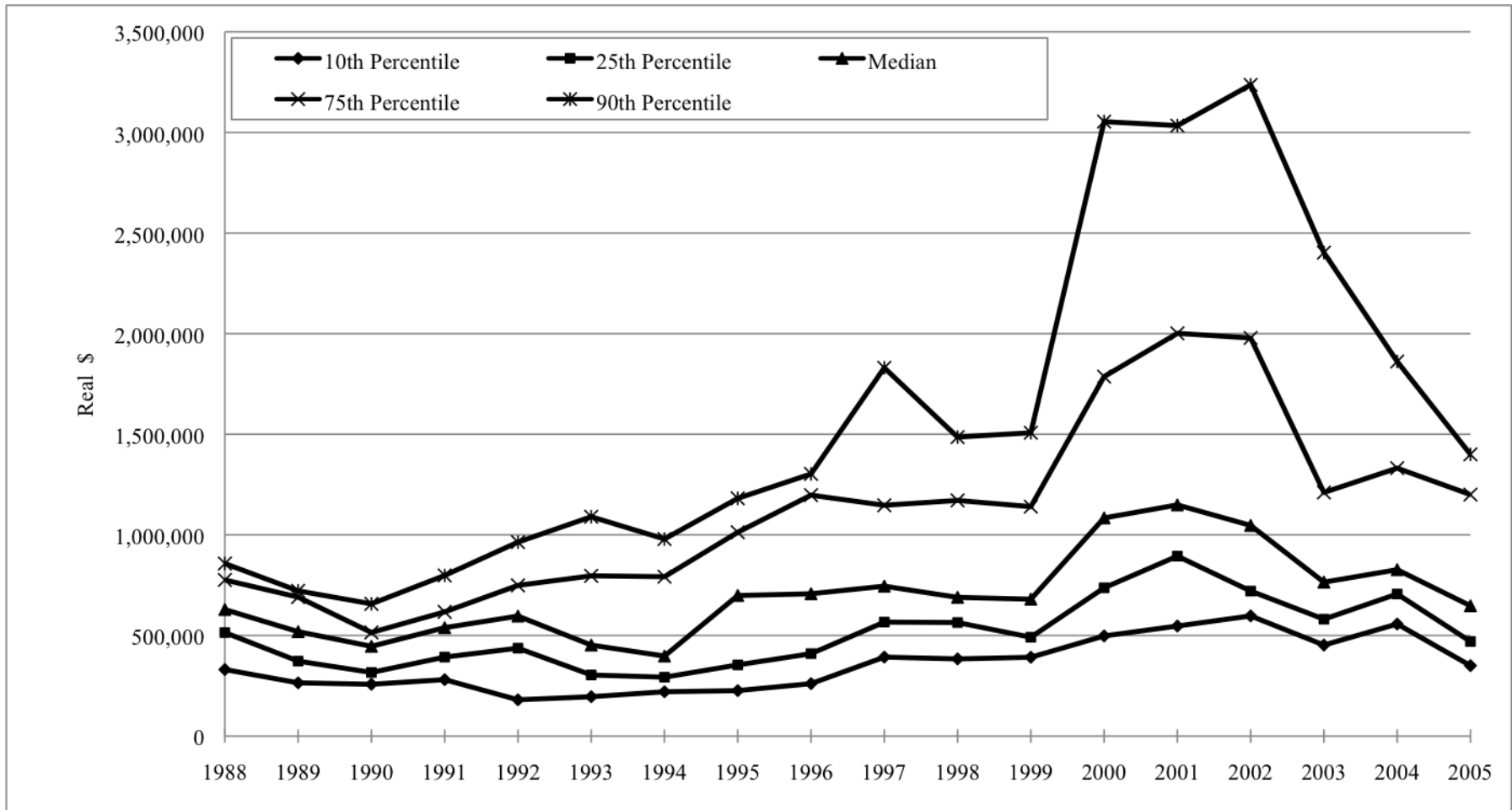
Finally, analysts who cover large portfolios earn significantly more than analysts who cover smaller portfolios. This factor appears to arise primarily from more talented analysts being matched to economically important industries or stocks and receiving higher pay for their ability.

Given that annual compensation captures only a portion of total analyst incentives, we examine the characteristics of analysts who likely were “fired” from our sample bank (i.e.,

moved to lower-status banks or 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, but extremely negative forecasting outcomes are associated with increased probability of dismissal. This finding is important in light of 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]), 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.

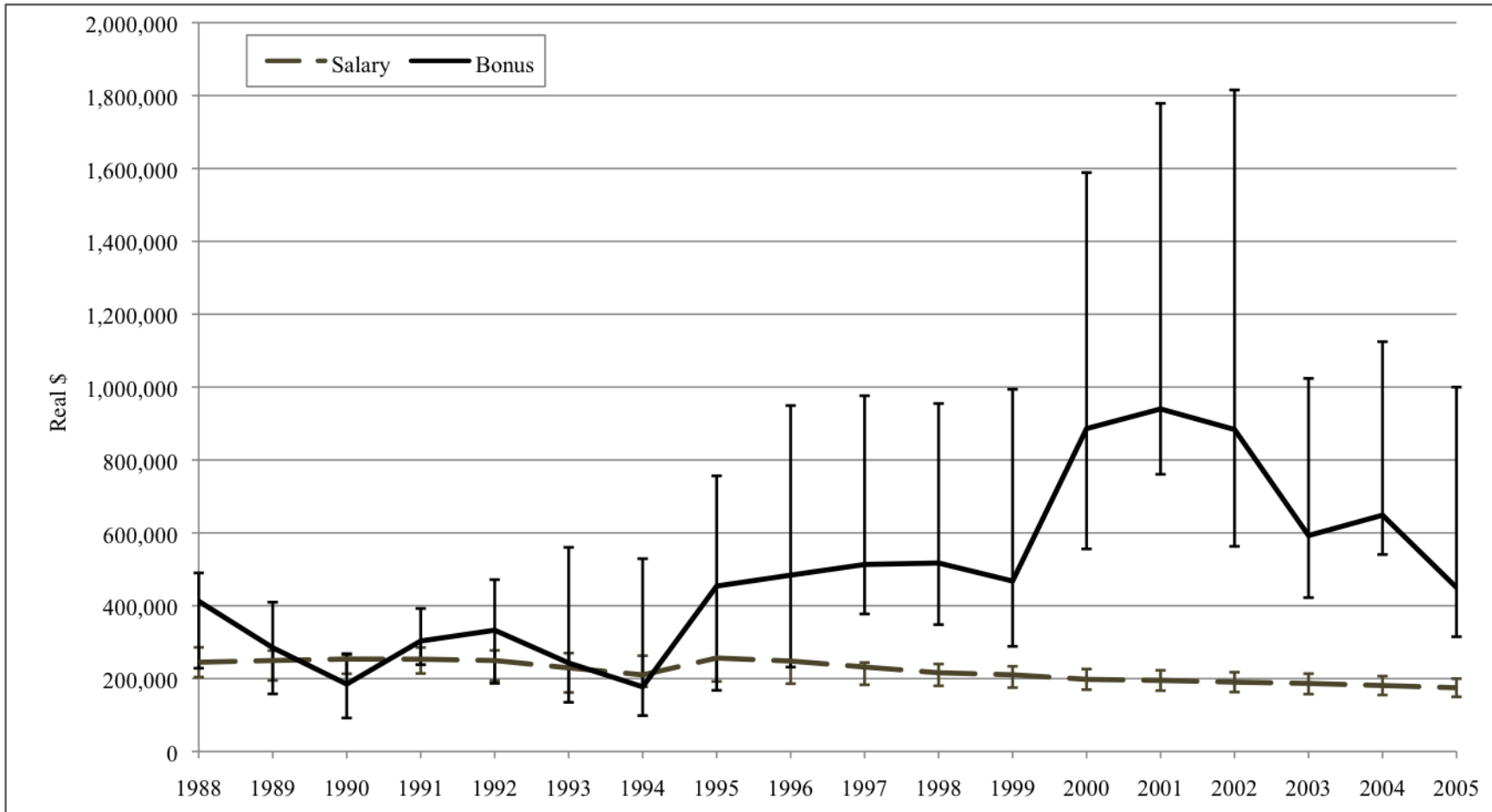
Our findings suggest several avenues for future research. First, more research on client-ratings seems warranted. For example, how are client ratings affected by the quality of analysts’ industry and firm analyses, ability to provide access to corporate managers, and responsiveness to client questions? Similarly, what is the relation between client votes, brokerage-level trading volume in covered stocks, and commission revenues? Second, given that the *WSJ* star premium appears to, and the *II* star premium not to, reflect visibility in the media, future research on incentives for analysts to generate public recognition and visibility also seems warranted. Finally, future research could examine how 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 at which investment-banking opportunities are less prevalent? Similarly, how effective was the Global Settlement in limiting the degree to which compensation is tied to banking deals?

**FIGURE 1**  
*Sell-side Analysts' Total Compensation (in 2005 dollars)*



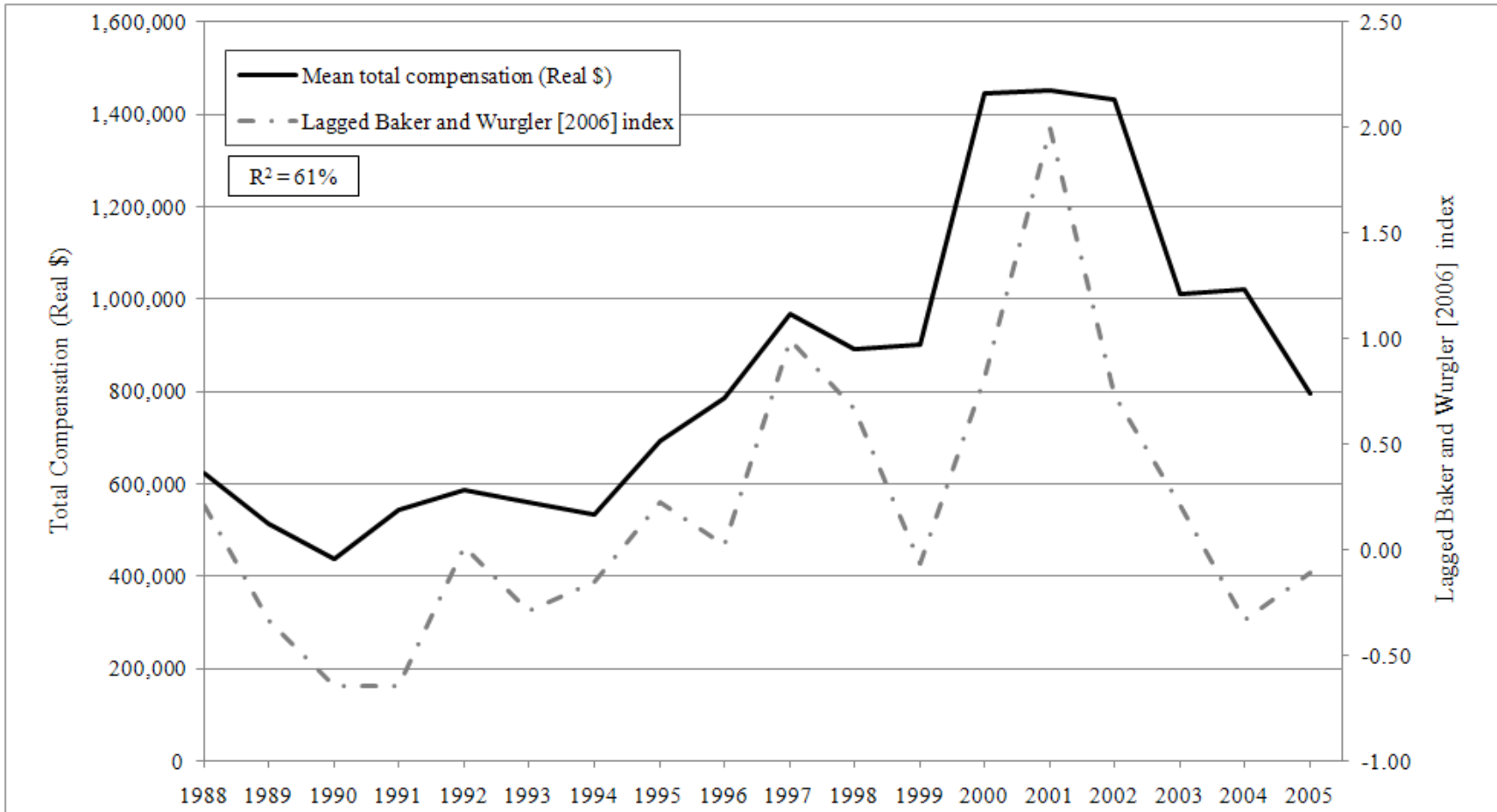
This figure, based on a total of 609 analyst-year observations, plots total real compensation for all I/B/E/S-listed, U.S. sell-side analysts at a major financial institution during the years 1988-2005. Total compensation equals salary plus bonus. Compensation data were inflation-adjusted using CPI data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

**FIGURE 2**  
*Sell-side Analysts' Salary and Bonus Compensation (in 2005 dollars)*



This figure, based on a total of 609 analyst-year observations, plots real salary and bonus compensation for all I/B/E/S-listed, U.S. sell-side analysts at a major financial institution during the years 1988-2005. 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 at a major financial institution during 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, which captures a variety of capital market activity signals including banking-related variables (such as IPO volume, first day IPO returns, and equity share in new issues) and commission-related variables (such as average monthly turnover on NYSE-listed stocks).

**TABLE 1**  
*The Determinants of Analyst Compensation*

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

	Annual Metrics			Quarterly Metrics	
	Pred.	Standardized Coefficient Estimate	<i>p</i> -value	Standardized Coefficient Estimate	<i>p</i> -value
<b>Panel A: Full Sample 1988-2005</b>					
<i>Absolute Accuracy:</i>					
-(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
<i>Relative Accuracy:</i>					
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
<b>Panel B: WSJ EPS Sample 1994-2001</b>					
	Pred.	Standardized Coefficient			
		Estimate	<i>p</i> -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 in the sell-side analyst literature. Each standardized pay-for-accuracy coefficient,  $\beta$ , is obtained by estimating the following fixed-effect regression on the 1988-2005 sample (N = 567):

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

*Std. ln(Compensation)* is the natural logarithm of analyst compensation rescaled to have a mean of zero and standard deviation of one. *Std. Baker Wurgler* is the Baker and Wurgler [2006] market activity index rescaled to have a mean of zero and 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 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 in Panel B of the Appendix.

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

	Pred.	Fixed-Effect Coefficient Estimates ( <i>p</i> -value)				
		Model 1	Model 2	Model 3	Model 4	Model 5
WSJ star stock-picker	+	0.148 *** (0.008)				0.134 ** (0.015)
Average annualized return	+		0.066 *** ( $<.001$ )			0.050 ** (0.049)
Market-adjusted BLT [2007] portfolio alpha	+			0.053 *** (0.001)		0.031 (0.161)
Four-factor-adjusted BLT [2007] portfolio alpha	+				-0.008 (0.803)	-0.059 * (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 during the years 1995-2005. This period was chosen because I/B/E/S recommendation data became available in 1994, and 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 ( <i>p</i> -values)				
	Pred.	Firm 1	Firm 2	Difference Firm 2 - Firm 1
<i>Institutional Investor "All Star"</i>	+	0.237 <sup>***</sup> (0.002)	0.248 <sup>***</sup> (<.001)	0.011 (0.905)
Log(lagged market capitalization of portfolio)	+	0.076 <sup>**</sup> (0.033)	0.049 <sup>*</sup> (0.060)	-0.027 (0.540)
Investment banking contribution (\$ mill)	+	0.034 <sup>**</sup> (0.015)	0.033 <sup>***</sup> (<.001)	-0.001 (0.953)
Average relative forecast accuracy score	+	0.001 (0.246)	-0.001 (0.308)	-0.002 (0.129)
Number of forecast revisions	+	0.001 (0.630)	-0.001 (0.344)	-0.002 (0.327)
Number of initiations	+	0.000 (0.973)	0.001 (0.910)	0.001 (0.967)
Analyst experience	+	-0.004 (0.746)	0.063 <sup>***</sup> (<.001)	0.067 <sup>***</sup> (0.002)
Lagged Baker and Wurgler [2006] index	+	0.338 <sup>***</sup> (<.001)	0.249 <sup>**</sup> (0.035)	-0.089 (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) with those of another top-tier investment bank (Firm 2) for 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  $j$  to stock  $j$ , and the subscript  $t$  to year  $t$ , defined as the period from December 1,  $t-1$ , to November 30,  $t$ .

**Panel A: Primary Variables**

Variable	Definition
$\ln(\text{Compensation})_{it}$	Analyst $i$ 's total compensation (salary + bonus) for year $t$ , expressed in real (2005-equivalent) dollars (sources: proprietary compensation file, Federal Reserve Economic Database).
<i>Institutional Investor "All-Star"</i> $_{it}$	An indicator variable equal to one if analyst $i$ was named an "All-American" in the October year $t$ issue of <i>Institutional Investor</i> magazine, zero otherwise (source: <i>Institutional Investor</i> ).
$\text{Log}(\text{lagged market capitalization of portfolio})_{it}$	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). We then 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 and take the natural logarithm (sources: CRSP, I/B/E/S, Federal Reserve Economic Database).
<i>Investment banking contribution (\$ mill)</i> $_{it}$	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 $\frac{\text{Management Fee}}{\# \text{ of Book-runners}} + \frac{\text{Underwriting Fee} + \text{Selling Concession}}{\# \text{ of Syndicate Members}}$ . For deals in which the bank was not a book runner, fees are estimated as $\frac{\text{Underwriting Fee} + \text{Selling Concession}}{\# \text{ of Syndicate Members}}$ (sources: SDC, I/B/E/S, Federal Reserve Economic Database).
<i>WSJ star stock-picker</i> $_{it}$	An indicator variable equal to one if analyst $i$ was named one of the "Best on the Street" stock-pickers by the <i>Wall Street Journal</i> in year $t$ , and zero otherwise (source: the <i>Wall Street Journal</i> ).

APPENDIX – *Continued*

Variable	Definition
<i>Average relative forecast accuracy score<sub>it</sub></i>	<p>The average accuracy of analyst <i>i</i>'s earnings forecasts in year <i>t</i>. For each of the <math>J_{it}</math> firms covered by analyst <i>i</i> in year <i>t</i> we compute relative accuracy using the following formula:</p> $100 - \frac{Rank_{ijt} - 1}{I_{jt}} \times 100$ <p>where <math>I_{jt}</math> is the number of analysts following firm <i>j</i> in year <i>t</i> and <math>Rank_{ijt}</math> is analyst <i>i</i>'s rank relative to all other analysts covering firm <i>j</i> in year <i>t</i> based on absolute forecast errors. See Figure A.1 for the timing and computation of absolute forecast errors (source: I/B/E/S).</p>
<i>Number of forecast revisions<sub>it</sub></i>	<p>The number of annual EPS forecasts issued between 90 and 360 days before the earnings announcement date (source: I/B/E/S).</p>
<i>Number of initiations<sub>it</sub></i>	<p>The number of new firms covered by analyst <i>i</i> in year <i>t</i>. 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).</p>
<i>Analyst experience<sub>it</sub></i>	<p>The number of years that an analyst has appeared in I/B/E/S (source: I/B/E/S).</p>
<i>Baker and Wurgler [2006] index<sub>t-1</sub></i>	<p>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 equity share in new issues) and commission-related variables (such as average monthly turnover on NYSE-listed stocks) (source: Baker and Wurgler).</p>



APPENDIX – *Continued*

**Panel B: Additional Forecast Accuracy Variables (Table 2)**

For all variables,  $Actual_{jt}$  is the actual EPS announced by firm  $j$  during 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
<i>Average undeflated absolute forecast error<sub>it</sub></i>	$\frac{1}{J_{it}} \sum_{j=1}^{J_{it}}  Actual_{jt} - Forecast_{ijt} $ (source: I/B/E/S).
<i>Average price-deflated forecast error<sub>it</sub></i>	$\frac{1}{J_{it}} \sum_{j=1}^{J_{it}} \frac{ Actual_{jt} - Forecast_{ijt} }{P_{jt-1}}$ , where $P_{jt-1}$ is firm $j$ 's stock price measured at the beginning of the evaluation period (source: I/B/E/S, CRSP).
<i>Average DAFE<sub>it</sub></i>	$\frac{1}{J_{it}} \sum_{j=1}^{J_{it}} \left(  Actual_{jt} - Forecast_{ijt}  - \overline{ Actual - Forecast }_{jt} \right)$ (source: I/B/E/S).
<i>Average PMAFE<sub>it</sub></i>	$\frac{1}{J_{it}} \sum_{j=1}^{J_{it}} \left( \frac{ Actual_{jt} - Forecast_{ijt}  - \overline{ Actual - Forecast }_{jt}}{\overline{ Actual - Forecast }_{jt}} \right)$ (source: I/B/E/S)
<i>Average PSAFE<sub>it</sub></i>	$\frac{1}{J_{it}} \sum_{j=1}^{J_{it}} \left( \frac{ Actual_{jt} - Forecast_{ijt}  - \overline{ Actual - Forecast }_{jt}}{Std.Dev( Actual - Forecast )_{jt}} \right)$ (source: I/B/E/S)

APPENDIX – *Continued*

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

Variable	Definition
<i>Average annualized return</i> $r_{i,t-1}$	The annualized return to buy and strong-buy recommendations computed using the method employed by the sample bank (source: I/B/E/S, CRSP).
<p><i>BLT<sub>1</sub>: Market-adjusted BLT [2007] portfolio alpha</i> <math>\alpha_{it-1}</math></p> <p style="text-align: center;"><i>and</i></p> <p><i>BLT<sub>2</sub>: Four-factor-adjusted BLT [2007] portfolio alpha</i> <math>\alpha_{it-1}</math></p>	<p>The long-window calendar-time “market-adjusted” and “four-factor-adjusted” alpha obtained using the methodology in Barber, Lehavy, and Trueman [2007].</p> <p>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, <math>\alpha_{it}</math>, from the following daily time-series regressions:</p> <p>(BLT<sub>1</sub>) <math>r_{it}^d - r_t^{fd} = \alpha_{it} + \beta_{it}(r_t^{md} - r_t^{fd}) + \varepsilon_{it}^d</math></p> <p>(BLT<sub>2</sub>) <math>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}^d</math>,</p> <p>where <math>r_{it}^d</math> is the portfolio return on day <math>d</math> for analyst <math>i</math> in year <math>t</math>; <math>r_t^{fd}</math> is the CRSP daily risk-free return on day <math>d</math> in year <math>t</math>; <math>r_t^{md}</math> is the daily return on the CRSP value-weighted market index; <math>SMB_t^d</math> is the return on day <math>d</math> in year <math>t</math> of a value-weighted portfolio of small stocks minus the return on a value-weighted portfolio of big stocks; <math>HML_t^d</math> is the return on day <math>d</math> of year <math>t</math> 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; <math>WML_t^d</math> is the return on day <math>d</math> of year <math>t</math> 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.</p> <p>To facilitate comparability with the <i>Average annualized return</i> metric employed by the sample bank, alphas are multiplied by 365 (source: I/B/E/S, CRSP, Ken French’s website).</p>