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Analyst Reputation, Conflict of Interest, and Forecast Accuracy

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Abstract

We study the effect of analyst reputation on earnings forecast accuracy using the 1983-2002 U.S. data. We find that All-American analysts are significantly more accurate than non-All-Americans. We also find that analysts who work at top-tier investment banks (large underwriters) are more accurate than others but become inaccurate in boom IPO markets, which is consistent with conflict of interest. Finally, we find that All-Americans do not become inaccurate in boom IPO markets. Personal reputation, as measured by All-American status, apparently mitigates the conflict of interest that becomes particularly acute for analysts employed at top-tier investment banks during boom years.

JEL classification: G1, G2
Keywords: Analyst reputation; Earnings forecast; Bank reputation; Conflict of interest; Investment banking.

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1. Introduction

While conflict of interest in Wall Street research has been quite extensively noted in the academic literature, the relevance of this research has never been as clear as in the post-Internet-bubble era, after shocking cases of conflicts have been exposed, and policy makers have scrambled to draft new rules and regulations to enforce the integrity of analyst research.[2] Notably, some of the highly publicized cases of conflict of interest have involved former-“star” analysts,[3] leading some critics to view the prevailing incentive schemes facing analysts as seriously flawed.

With these recent events as the backdrop, we revisit the topic of conflict of interest from a new angle by asking what role, if any, personal reputation plays in the self-regulation of sell-side research. What are the incentives faced by these analysts? Perhaps somewhat contrary to popular view, there are two distinct facets in the analysts’ compensation structure that produce two opposing incentives. On the one hand, as the recent press has made well known, an analyst’s compensation increases with the amount of underwriting revenue that he helps generate for his employer.[4] For various reasons, this provides an incentive to inflate earnings forecasts for potential clients. This incentive is what lies at the heart of the alleged conflict of interest. On the other hand, a much less considered fact is that an analyst’s compensation also increases with his external

[2] Most notably, on January 15, 2003, 10 of the largest investment banks settled with the New York Attorney General Eliot Spitzer, and agree to pay a combined $1.4 billion in fines.

[3] Henry Blodget, the Internet analyst for Merrill Lynch, and Jack Grubman, the telecom analyst for Salomon Smith Barney/Citigroup, both paid multi-million-dollar fines in their settlements with the prosecutors in 2003, and were barred from the securities industry for life. Both were All-American analysts prior to their falls. In contrast, the technology analyst at Morgan Stanley, Mary Meeker, once-called the “Queen of the Net”, was never charged, and continued to produce research after the stock bubble burst. She was an All-American from 1994-2000, and has been re-elected to an AA (runner-up) title in 2003 and 2004.

[4] Although the collective settlement reached between the 10 banks and Spitzer also required firms to sever the links between research and investment banking, including analyst compensation for equity research, the pay practice existed during our sample period, and may well come back in the future.

reputation, a popular indicator of which is the All-American ranking published by the influential *Institutional Investor* magazine. Since the All-American (henceforth AA) ranking is at least partially based on an analyst's forecast accuracy,[5] this facet of the compensation scheme provides analysts with an incentive to be accurate and unbiased. As evidence for both of these incentives, Hong and Kubik (2003) document that analysts are rewarded for both optimism and accuracy.

Thus, there is a reasonable economic incentive for an analyst to be accurate, and for a star analyst to remain accurate.[6] While the conflict-of-interest dimension has been extensively studied in the academic literature, the positive pay-reputation relation and its effects on accuracy have received much less attention. In addition, how the two incentives interact and jointly affect an analyst's accuracy is little explored. By focusing on the interaction between personal reputation and conflict of interest, our paper bridges the two strands of literature on analyst research that have remained largely separate.

The literature on conflict of interest is quite extensive. Dugar and Nathan (1995), Lin and McNichols (1998), and Dechow, Hutton and Sloan (2000), among others, find that analysts who are affiliated with covered firms through underwriting relationships are more positively biased than non-affiliated analysts.[7] In an influential study, Michaely

[5] See Stickel (1992) and Michaely and Womack (1999).

[6] The argument will be even stronger if one considers potential long-term career gains associated with portable, personal reputations. Anecdotal evidence suggests that having an All-American status increases the likelihood of becoming research directors, buy-side fund managers, or, simply having more bargaining power *vis-à-vis* investment banks regarding compensation.

[7] Dugar et al. (1995) find that investment-banker analysts are optimistic, relative to non-investment-banker analysts, in their earnings forecasts and recommendations. Lin et al. (1998) find that lead and co-lead underwriter analysts' long-term outlook, namely growth forecasts and recommendations, are more favorable, but short-term outlook, i.e., earnings forecast, are not generally greater. Dechow et al. (2000) document that forecasts are overly optimistic around IPOs in general, and among affiliated analysts in particular. Ljungqvist, Marston, and Wilhelm (2005), in a paper that examines the effects of bank relationships and analyst behavior on underwriter choice, report that, prior to new equity or debt issues, analysts whose banks have had strong underwriting relationships with firms are more aggressive in recommending the stocks.

and Womack (1999) compare trade recommendations made by analysts working at IPO lead underwriters versus others, and find that underwriter analysts are more positively biased. While affiliation has been defined in various ways, the general conclusion from this strand of literature is that the more closely connected is an analyst with a covered firm, the more positively biased he tends to be, evidence pointing to conflict of interest.

A caveat here is that the body of existing evidence, while extensive, still cannot rule out the possibility that the positive bias is a result of either (behavioral) human nature or self selection. After all, studies have found that optimism also exists among non-affiliated and non-broker analysts.[8] Thus, it is possible that analysts and banks with more favorable views of a firm are more likely to cover or underwrite for it in the first place, which could explain why affiliation is positively related to bias. Since the coverage is an endogenous decision, this behavioral hypothesis is difficult to distinguish from the conflict-of-interest hypothesis.

Our paper contributes to the conflict-of-interest literature in two ways. First, we attempt to disentangle the two hypotheses discussed above by examining more exogenous forces of underwriting pressure. Based on the premise that underwriting pressure (the source of conflict of interest) is stronger during hot IPO years, we study the time variation of analysts' relative accuracy through the peak and troughs of the *overall* underwriting market. While self-selection bias alone predicts a constant affiliation-related differential in accuracy, a time-varying differential is more indicative of conflict

[8] See, for example, Francis and Philbrick (1993) for a study of ValueLine analysts. Bradley, Jordan, and Ritter (2003) also document that research coverage initiated immediately after the expiration of the IPO quiet period is almost always with a favorable rating, including research provided by non-lead-underwriter banks.

of interest. Second, we introduce the personal-reputation dimension and investigate how it interacts with potential conflict of interest.

Compared to the literature on conflict of interest, research on personal reputation is surprisingly sparse. Stickel (1992) shows that star analysts (those with the AA title) produce more accurate forecasts than others. He concludes that there is a positive relation between reputation and performance, and hence pay and performance. Our study not only updates Stickel's early work, but also adds an inter-temporal dimension to the reputation-accuracy relation. By looking at this relation through the peaks and troughs of the underwriting cycle, we provide a richer picture that allows us to study the interaction between personal reputation and underwriting pressure.

Our empirical results are summarized as follows. First, we document that the labor market for sell-side analysts is largely efficient in the sense that there is a positive pay-performance relation. Personal reputation is positively related to forecast accuracy, as we find that AA analysts are significantly more accurate than non-AAs. This result is stronger among analysts working at lower-status banks. Since AAs are significantly higher paid, this confirms the positive pay-performance relation in Stickel (1992), and suggests that the AA election conducted by *Institutional Investor*, whereby analysts with superior ability tend to be rewarded, is largely efficient. Further supporting this positive pay-performance relation, we find that analysts working at top-tier banks are generally more accurate than those employed at lower-status banks. This is particularly true for non-AAs. Since top-tier-bank analysts are also better paid, this further demonstrates the positive pay-performance relation and suggests that competition for jobs at top-tier banks serves as an additional screening device in the labor market for analysts.

The labor-market-efficiency result is important because it provides a baseline case for correctly-aligned incentives among analysts to produce accurate forecasts. The finding is also interesting in its own right because it challenges the more cynical view that has become popular in recent years.

Our second main finding is that while the labor market for analysts is, on the whole, efficient in normal times, conflict of interest does exist, and becomes acute in boom-market conditions. We find that forecast accuracy drops significantly among top-tier-bank analysts in boom years. Since the payoff from generating underwriting revenue is largest in top-tier investment banks (large underwriters) and during boom underwriting periods, this time-varying pattern strongly supports the conflict-of-interest hypothesis, and is difficult to reconcile with the behavioral or self-selection explanation which predicts a time-invariant differential in relative accuracy.

Building on the previous two findings, our third and most interesting finding is that personal reputation, as measured by the AA status, appears to have a mitigating effect on the conflict of interest. We reach this conclusion primarily from the evidence that in peak underwriting years, the general decline in accuracy among top-tier analysts is driven by existing non-AAs. In comparison, star analysts working at top-tier banks become significantly more accurate relative to their non-star colleagues in peak years. This suggests a disciplinary role of personal reputation because without personal reputation at stake, non-star analysts at top-tier banks have much to gain and little to lose by becoming more biased in hot markets. In contrast, star analysts *do* have personal reputations at stake, and this concern can effectively counter the temptation of becoming more biased in peak underwriting years.

The rest of the paper is organized as follows. Section 2 presents our hypotheses and testable implications. Section 3 describes the data and methodology. Sections 4 and 5 present the uni-variate and multivariate results, respectively. Section 7 concludes.

2. Analysts' Incentives and the Determinants of Forecast Accuracy

Since an analyst's job is to produce forecasts and issue recommendations, an important performance evaluation comes from external buy-side managers who use his research output. The *Institutional Investor* magazine plays an important role in this process. Every year, it conducts a large survey among buy-side managers,[9] asking them to evaluate the analysts along the following four dimensions: stock-picking, earnings forecasts, written reports, and overall service. The results of this survey lead to the annual election of the All-American analysts, which is featured in the October issue of the magazine every year. The issue not only publishes the winning analysts' names and photos, but also writes elaborate editorials on the reasons for the award. The editorials almost always contain comments from buy-side managers on the winning analysts' superior forecasting abilities (including stock-picking and market timing).

Having an AA title is not only prestigious, but also financially rewarding. AAs earn substantially higher salaries than non-AAs. In addition, anecdotal evidence suggests that AAs, especially veteran, consecutive winners, can exercise significant power within the investment bank, because their affiliation is coveted by rival banks. Importantly, because the AA title is awarded by buy-side managers who ultimately care about the profitability of the analysts' advice, an analyst will have an incentive to be as accurate as

[9] The survey coverage is quite comprehensive. In 2002, for example, the survey was sent to more than 900 institutions. Over 3,500 individuals from about 600 firms responded. The responses covered more than 90 percent of the 100 largest U.S. equity managers. (*Institutional Investor,* October 2002).

possible.[10] Moreover, since taking up a money-manager position on the buy-side is often considered an ultimate reward for successful sell-side analysts, this long-horizon goal further incentivizes the analysts to resist biasing their forecasts, because doing so will hurt their relations with investors *ex post*.

However, analysts are also compensated (mostly in the form of year-end bonuses) for helping their employer generate underwriting revenue. For various reasons (for instance to cultivate good relationships with issuing firms, and to put together an aggressive marketing campaign), a natural way to win underwriting business is to bias the earnings forecasts upward, i.e., to exhibit over-optimism for covered companies.

Thus, there are two conflicting incentives that affect the analysts' forecast accuracy. There is a positive incentive due to reputation, and a negative incentive, due to underwriting pressure. Our study attempts to shed light on how these two opposing forces interact in determining forecast accuracy. This is an important empirical question, however, to the best of our knowledge, it has been relatively overlooked in the literature.

Given the prominence of the AA designation, using the AA status to proxy for personal reputation is a natural choice. In devising proxies for underwriting pressure, we observe that underwriting-related compensation usually comes as year-end bonuses. It is therefore conceivable that the reward from, and hence the short-term temptation to, becoming overly optimistic, is highest during booms of the underwriting market.[11] In addition, the temptation to be biased is likely to be even stronger in top-tier investment banks (large underwriters) because there is more money at stake. These observations

[10] Stickel (1992) reports evidence that lower relative accuracy leads to removal from the AA team. The finding suggests that once elected, AAs are incentivized to remain accurate.

[11] It is important to note here that we use overall market-level underwriting volume instead of bank-level underwriting volume. This choice is made to alleviate the reverse-causality problem that analysts' over-bullishness can help increase bank-level underwriting activity.

suggest that both market-wide underwriting activity and bank status are potential proxies for underwriting pressure, and underwriting pressure is likely to be the strongest under the combined condition of hot markets and top-tier banks.

It should be noted that the effect of bank status alone is ambiguous. On the one hand, the lure of year-end bonuses is high at top-tier banks, and perhaps so is analysts' susceptibility to conflict of interest. On the other hand, jobs at top-tier banks are higher paid and more prestigious; competition in the labor market should imply that bank status is positively correlated with performance (accuracy). This leaves the overall effect of bank status alone as an empirical question. However, when interacted with underwriting pressure, bank status is an informative inference device for conflict of interest.

Our main research goal is to empirically answer the following four questions:

1. *Is the All-American status significant in determining analysts' forecast accuracy?*

2. *Is bank status (top-tier vs. lower-status) important in determining analysts' forecast accuracy? Does this effect depend on the personal reputation of the analysts?*

3. *Does accuracy of analysts who work at top-tier banks go down during IPO boom markets?*

4. *Does accuracy of analysts with All-American status go down during IPO boom markets?*

The first and second questions provide baselines for the effect of personal reputation and bank status alone. The third question addresses the effect of underwriting pressure, and sheds light on conflict of interest. The last question examines the interaction between personal reputation and underwriting pressure, and thus addresses the question of whether personal reputation has a mitigating effect on the conflict of interest problem.

3. Data and Methodology

3.1. Data and Descriptive Statistics

Our data are compiled from several sources. Details on analyst forecasts – company information, analyst and broker codes, EPS estimates and actuals – are obtained from the I/B/E/S Detailed History file. Since comprehensive data coverage by I/B/E/S started in 1983, our sample period is from 1983 to 2002. I/B/E/S provides coverage for multiple forecast horizons. Consistent with prior research, and to utilize the most comprehensive data coverage, in this study, we focus on fiscal-year-end forecasts only. For firm characteristics and stock prices, we use data from Compustat and CRSP.

Analysts' AA status is obtained from the October issue of the *Institutional Investor* for each year in the sample. For confidentiality, I/B/E/S uses numeric codes to identify brokers and analysts in the details file; the names are available upon request in a separate translation file. We matched the names of the AA analysts from the *Institutional Investor* listings with the names in the translation file. Out of the entire sample of 1,376 distinct AAs over the 20-year period, we were able to find a match for 1,240 analysts.

For bank status, we identify the ten underwriters with the highest Carter-Manaster ranks provided in Carter, Dark, and Singh (1998) as the "top-tier" group. These top-tier banks are: Alex Brown & Sons, Drexel Burham Lambert, First Boston Corporation, Goldman Sachs & Company, Hambrecht & Quist, Merrill Lynch, Morgan Stanley & Company, Paine Webber, Prudential-Bache, and Salmon Brothers. All ten banks have significant market shares in the underwriting market, and many are considered "bulge-bracket" firms.

For the market-level underwriting activity, we compile from SDC the overall IPO volume for a given year. We deliberately use market-level rather than bank-specific underwriting volume because there is a potentially severe endogeneity in bank-level activity. It could well be that a particular bank's surge in the underwriting ranks is due to the exceptional bullishness of its analysts. With bank-level underwriting volume, it is difficult to distinguish this possibility from the alternative that underwriting pressure *causes* the analysts to be inaccurate in boom years. Since market-level volume is less influenced by individual analysts' action, but there are more financial rewards (potential bonuses) at stake during overall market booms, using overall market volume to proxy underwriting pressure at least partially alleviates the endogeneity issue.

Table I reports the number of firms, analysts, and forecasts in each year of the sample. The number of firms covered grew steadily over time, from 2,423 in 1983 to the peak of 5,475 in 1997. Since 1997, there has been a decline, particularly in 2001 and 2002, and by 2002, the sample has fallen back to 3,373 firms. The number of analysts in the sample grew from 2,171 in 1983 to 4,758 in 2002. Most of this growth, however, is accounted for by the non-AAs. While the number of non-AAs more than doubled over the sample period from 1,939 in 1983 to 4,442 in 2002, the number of AAs was relatively stable, increasing from 232 in 1983 to 316 in 2002. The number of forecasts also grew steadily over the sample period, from 52,359 in 1983 to 125,215 in 2002. Together with the number of firms in the sample, these figures imply that, on average, each covered firm gets between 20 and 30 reports per year.

It is interesting to note that the non-AA to AA ratio in head count is always larger than the same ratio in the number of reports generated.[12] In fact, the former is roughly twice as large as the latter. For instance, in 1997, the non-AA to AA head-count ratio was 12.53, but the non-AA to AA total-reports ratio was only 5.93. This indicates that AAs tend to issue more forecasts per year than non-AAs.

Table II provides more statistics on the working patterns of AAs versus non-AAs. Statistics in this table show that AAs provide coverage to more firms, and for each firm covered, they also issue more frequent earnings forecasts. The differences are highly significant. These patterns are consistent with previous evidence in Stickel (1992).

Table III compares select characteristics of firms covered by AAs with those covered by non-AAs. This table reveals that AAs cover significantly larger (measured by market capitalization) and less risky (measured by return volatility) firms (the 1st and 4th vertical panels, respectively). AA-covered firms are also more likely to be listed on the NYSE (the 3rd vertical panel). All three comparisons indicate that more public information is available for firms covered by AAs. Since this could contribute to higher accuracy, it is important to control for these variables in multivariate analyses.

Table III also shows that firms covered by AAs have higher leverage than those covered by non-AAs (the 2nd vertical panel). While higher leverage could indicate higher risk, it could also indicate more stable cash flows and higher debt capacity. Thus, *a priori*, it is difficult to judge the effect of leverage on forecast accuracy, and the sign of the leverage variable in a multivariate regression remains an empirical question.

[12] Since the election to AA status is announced in October, we use the following rule in determining whether a particular forecast is made by an AA. For each AA analyst, only those forecasts made by that analyst from October of the election year to the end of September of the following year are determined as being made by an AA.

One final remark on Table III is that the fraction of covered firms that are listed on the NYSE generally drops over time, and this pattern is particularly strong for firms covered by non-AAs (the 3rd vertical panel). This indicates that over time, more information is produced for smaller firms.

3.2. Empirical Methods

We define the key variable of interest, analyst forecast accuracy, as follows:

$$Error_{i,j,t,n} = \frac{|\, EPS\ Forecast_{i,j,t,n}\ \text{-}\ Actual\ EPS\ Reported_{j,t}\,|}{Book\ Value\ Equity_{j,t-1}}, \qquad (1)$$

where "*i*" indicates an analyst, "*j*" indicates a firm, "*t*" is the forecast period, i.e., the fiscal year, and "*n*" orders the forecasts made by analyst *i* for firm *j* for fiscal year *t*.

We use absolute forecast error rather than signed error because although conflict of interest suggests that analysts are mainly tempted to bias their forecast upward, in order to be a good stock picker and offer valuable investment advice (which are among the AA election criteria), an analyst should want to be accurate, which means minimizing both positive and negative errors. Since we are interested in both incentives, we use the absolute errors to capture both considerations.[13]

We scale the absolute forecast error by the firm's book value of equity per share at the previous fiscal year end. Scaling is crucial because in regression analyses, un-scaled errors produce heteroskedasticity.[14] For example, some firms might have very large EPSs simply because they have a very small share base. Comparing un-scaled forecast errors across firms in this case would clearly be inappropriate.

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[13] Analyses using signed errors yield qualitatively similar results as the results reported in this paper.
[14] Other studies have similarly used scaling to justify homoskedasticity assumption. See, for example, Keane and Runkle (1998).

We use as the scaling factor the book value of equity rather than either the actual EPS reported or the market value of equity because of the undesirable time-series property of the latter two measures. Recent years have witnessed a dramatic rise in corporate earnings and stock prices, especially in the late 1990s, followed by an equally dramatic decline in the post-bubble years of the early 2000s. Given this time-series pattern, using either earnings or stock prices as the scaling factor would result in making scaled errors during boom years artificially small relative to errors in trough years. Yet the conflict of interest allegation is about manipulations of forecasts made during those boom years. Using book value of equity, which is more stable and much less correlated with movements of the financial markets, does not introduce this potential bias.

To empirically investigate the research questions listed in Section 2, in the next section we perform uni-variate tests on forecast accuracy along the personal reputation dimension and bank status dimension. Section 5 then addresses the conflict of interest problem and its interaction with personal reputation using multivariate regressions.

4. Uni-variate Analyses

We begin our analysis with a series of uni-variate tests. Table IV compares the forecast accuracy of AA analysts versus that of non-AA analysts. The first vertical panel presents the overall comparison; the latter two panels present results for top-tier-bank and lower-status-bank sub-samples, respectively. The forecast-error terms are reported as the actual scaled error times 100, thus they are in percentage terms.

Several observations can be made from this table. First, AAs are more accurate than non-AAs in the whole sample and the lower-status-bank sub-sample. In the whole sample, the t-statistics on the differences between AAs and non-AAs are highly significant for twelve out of the twenty years. The pooled test for the twenty years as a whole shows that, on average, AAs are more accurate than non-AAs by 0.40% (4.19%-4.59%), with a *t*-statistic of 5.74. This indicates that, with the overall sample average forecast error of 4.51%, being an AA improves an analyst's forecast accuracy by an economically significant 8.86% (0.40/4.51). This "AA effect" is even stronger among analysts working in lower-status banks: Not only does the pooled test show a larger *t*-statistic of 7.80, the margin by which the AAs in these banks are more accurate than the non-AAs, at 0.68% (3.94%-4.62%), is also significantly larger than in the overall sample.

Notably, however, the "AA effect" is much weaker in the top-tier-bank sub-sample: AAs are more accurate than non-AAs for only seven out of the twenty years in this sub-sample; the pooled *t*-statistic is an insignificant 0.55; and the average margin by which the AAs are more accurate is a minuscule 0.06% (4.37%-4.43%). Thus, it appears that while AAs are generally more accurate then non-AAs, AAs in lower-status banks are superior to their non-AA colleagues by a larger margin. This is reasonable because analysts in top-tier banks are generally better paid. To the extent that even the non-AAs are quite skilled, being an AA in top-tier banks is less of a distinction.

It must be emphasized that we do not interpret the higher accuracy among AAs as indicating that reputation causes analysts to be accurate. The results are only interpreted as a positive correlation between personal reputation and accuracy. Since election to AA status reflects past accuracy,[15] and since the comparisons are based on forecasts issued

[15] As a simple analysis on the probability of being elected as an AA, we estimate a probit equation of the form $AA_{i,t} = c + c_1 Error_{i,t-1} + c_2 Frequency_{i,t-1} + c_3 Coverage_{i,t-1} + \varepsilon_{i,t}$, where $Error_{i,t-1}$, $Frequency_{i,t-1}$ and $Coverage_{i,t-1}$ are analyst *i*'s average forecast error, reporting frequency, and number of firms covered in the past year, respectively. We find that the coefficient on *Error* is -0.57 with a t-statistic of 5.61,

after the AA status is granted, our results do indicate that analysts do not lose accuracy after becoming AAs. The positive correlation between AA status and forecast accuracy also suggests that skill or ability is an important selection criterion for AAs.

Table V compares forecast accuracy along the bank-status dimension. The first vertical panel presents the whole sample, and the latter two present the AA and non-AA sub-samples, respectively. Again, percentage error terms are reported. Results in this table are quite interesting. While in the whole sample and the non-AA sub-sample, analysts working in top-tier banks are found to be significantly more accurate than those working in lower-status banks (with pooled t-statistics of 2.34 and 2.58, respectively), the effect is the opposite in the AA sub-sample (with a pooled t-statistic of 3.69).

The fact that bank status is positively related to forecast accuracy in the whole sample and the non-AA sub-sample is consistent with labor market efficiency. Since jobs in top-tier banks are more competitive and higher paid, on average, analysts working in these banks should be more skilled and select workers; higher accuracy among them is consistent with a positive pay-performance relation.

The fact that, conditional on AA status, bank status has a weaker effect is also reasonable, since being an AA is itself an important distinction, and all AAs are highly paid. Our findings on AA status and bank status together suggest that the AA election and the competition for jobs at top-tier banks are both reasonably efficient screening devices in the labor market for analysts, and that both lead to a positive pay-ability relation.

Finally, the fact that AAs in top-tier banks are *less* accurate than those in lower-status banks is consistent with the notion that there might be a stronger culture to reward

suggesting that past forecast error significantly reduces the chance of being elected as an AA.

optimism in top-tier banks, which is indicative of conflict of interest.[16] However, we cannot draw conclusions from uni-variate results, and will revisit the bank-status effect in multivariate analyses.

In summary, uni-variate results in this section show an unambiguous AA effect, suggesting that personal reputation is positively correlated with accuracy. The effect of bank status, however, is ambiguous. While in the whole sample, bank status is positively related to accuracy, the effect does not exist among AAs. In the next section, we turn to multivariate analyses that allow us not only to draw more robust inferences but also to examine the interaction between personal reputation and conflict of interest.

5. Multivariate Analyses

5.1. The Effect of Personal Repuation

In principle, to investigate the effect of personal reputation, we would like to estimate a regression of forecast accuracy on the AA indicator and a list of control variables. Empirically, however, the complexity of our data poses a challenge for this simple framework. Not only are there more than 10,000 analysts covering over 4,000 firms during a 20-year period, each analyst covers multiple (and a time-varying number of) firms, and each firm is covered by multiple (and a time-varying number of) analysts in any given year. As a result, the residual in a simple pooled regression is almost surely not *i.i.d.*, and the statistical assumptions for OLS are most likely violated.

[16] An alternative explanation is that the AA election is based on slightly different criteria for analysts working in top-tier banks versus those in lower-status banks. Since the AA election depends on institutional investors' votes, it is plausible that while an analyst from a top-tier bank may be elected for his or her other values to investors – superior access to the management of the covered firms, for instance, – for an analyst working at a small bank, accuracy is the only game in town.

To address these econometric issues, we adopt the cross-sectional regression approach developped by Fama and MacBeth (1973). For each of the 20 fiscal years in our sample, we first estimate an equation of the form:

$$Error_{i,j,n} = \alpha + AA_i \beta_1 + \gamma' X_{i,j,n} + \varepsilon_{i,j,n}, \qquad (2)$$

and then test the significance of the coefficients using the empirical distribution of the 20 estimates. In equation (2), the dependent variable $Error_{i,j,n}$ is the scaled error of analyst i's nth forecast on firm j. The key variable of interest – AA_i – is analyst i's AA status at the time the forecast is issued. $X_{i,j,n}$ is a list of firm characteristics that could affect forecast accuracy. In the empirical specification, $X_{i,j,n}$ includes (log of) the distance in days between the forecast date and the earnings release date, market capitaliztion of the firm, leverage, and stock return volatility. To capture potentially missing firm-level variables, firm fixed effects are also included in the estimation.[17]

The advantage of the Fama-MacBeth regression is twofold. Econometrically, estimating the regression annually not only reduces the data complexity and improves the estimation, but also addresses the potential serial correlation problem in the data. More importantly, the annual estimations produce a time series of the AA coefficients, which is iself informative: It allows us to examine how the relative accuracy of the AAs changes through the peaks and troughs of the underwriting market. If the AA status purely reflects skill, in the absence of conflict of interest and a role for reputation, the relative accuracy of AA should be constant over time. However a time-varying relative accuracy implies presence of both conflict of interest and the role of reputation.

[17] To allow for heteroskedastic residuals in the cross-section, we use the Huber/White/sandwich variance estimator for the coefficient estimates.

Table VI reports the Fama-MacBeth regression results on equation (2). The first vertical panel shows the results for the whole sample, and the latter two show the results for the top-tier and the lower-status-bank sub-samples, respectively. Consistent with uni-variate results and the reputation effect documented in Stickel (1992), AAs are more accurate than non-AAs: The pooled coefficient on the AA variable is negative and statistically significant. Moreover, the coefficient of 0.0020 indicates that its economic significance remains even after controlling for various firm and forecast characteristics. Specifically, it indicates that, with the overall sample average forecast error of 0.0451, being an AA improves an analyst's accuracy by 4.43% (0.0020/0.0451).[18]

Also consistent with uni-variate results, the reputation effect is strong in lower-status banks but weak in top-tier banks. In the top-tier sub-sample, although the AA coefficient is still negative, it is insignificant. In terms of magnitude, the AA coefficient of –0.0022 in the lower-status bank sub-sample is twice as large as the –0.0010 in the top-tier-bank sample. Thus, while AAs in lower-status banks are significantly more accurate than their non-star colleagues, AAs in top-tier banks are not.

There are two possible explanations for this. First, it may be that AAs at top-tier banks face more pressure to bias their estimates than AAs at lower-status banks, because the rewards for biasing estimates is bigger in top-tier banks. This supports the conflict of interest view. Alternatively, it could also be that since jobs at top-tier banks are better paid and more competitive, on average, the non-AAs at top-tier banks are more skilled

[18] Since AAs cover larger and less risky firms than non-AAs (as reported in Table III), we expect the differential in accuracy between AAs and non-AAs to drop once we control for these firm characteristics. Indeed, in the uni-variate analysis reported in Table IV, we report the raw error differential of about 0.0040 between the two groups, or 8.86% of the average error. Multivariate analysis here reveals that about 50% of that differential is due to a different composition of the firms they cover and that the remaining 50% is due to the superior accuracy of AAs.

than those at lower-status banks. As a result, being an AA is less of a distinction in top-tier banks. This suggests a degree of efficiency in the analysts' labor market. The analysis on AA status alone is insufficient to rule out either explanation. Thus, we will revisit these two alternatives when we also examine bank status in detail.

All control variables in equation (2) have expected signs. The positive coefficient on ln(distance) indicates that the earlier are the estimates made, the less accurate they are. The negative coefficient on firm size suggests that forecast errors are smaller for larger firms, consistent with the notion that information is more readily available for larger firms. Higher volatility in stock prices indicates higher risk, and, as expected, it is associated with larger forecast errors. Finally, higher leverage is associated with larger errors, consistent with the notion that leverage reflects cash-flow risk.

In sum, multivariate results in this subsection reinforce the uni-variate finding that personal reputation is positively related to forecast accuracy.

5.2. The Effect of Bank Status

To examine the effect of bank status on forecast accuracy, we again employ the Fama-MacBeth regression method and estimate 20 annual regressions of the form:

$$Error_{i,j,n} = \alpha + TopTier_i \beta_1 + \gamma' X_{i,j,n} + \varepsilon_{i,j,n}, \quad (3)$$

and then aggregate over the years. In (3), $TopTier_{i,t}$ equals 1 if analyst i works at a top-tier bank at the time of the forecast, and 0 otherwise. The vector $X_{i,j,n}$ is the same set of control variables as before.

Table VII reports estimation results for Equation (3). The first vertical panel shows the results for the overall sample, and the latter two show the results for the AA and non-AA sub-samples, respectively. Consistent with uni-variate results in Table V, this table shows that while the coefficient on the top-tier dummy is negative and significant in the whole sample and the non-AA sub-sample, it is insignificant for the AA sub-sample. Thus, although top-tier-bank analysts are generally more accurate than lower-tier bank analysts, this result comes from non-AA analysts. Conditional on being an AA, however, bank status does not affect forecast accuracy.

The bank-status dummy's varying degree of importance among AAs and non-AAs is interesting. Since jobs at top-tier banks are better paid and more competitive, the overall positive relation between bank status and analyst accuracy suggests a positive pay-performance relation and indicates efficiency in the analysts' labor market. This effect is strong among non-AAs. In contrast, AAs at lower-status banks are also well-paid for their individual status, so the top-tier-bank affiliation is less important among the AAs. Notably, multivariate results here no longer support the previous uni-variate result (Table V) that AAs in top-tier banks are less accurate than those in lower-status banks. This suggests that the uni-variate result is likely to be driven by differences in firm-characteristics.

It is interesting to note that our results on AA status and bank status both point to a positive pay-ability relation, and suggest that both personal reputation and bank status are reasonably effective screening devices in the analysts' labor market. Contrary to the notion that the AA election is a pure popularity contest, our results provide support for the election process. This positive aspect in Wall Street research has been largely overlooked, especially when conflict of interest has recently been the paramount issue.

While these static results are interesting in their own right, they do not allow us to say much about either conflict of interest or the role of personal reputation. Next, we examine the dynamic interactions between personal reputation, bank status, and underwriting-pressure in order to shed light on these central questions.

5.3. Underwriting Pressure and Conflict of Interest

To investigate the conflict of interest hypothesis, we conjecture that if there is conflict of interest, its effects should be highest in top-tier banks *and* during boom years. This is because both in top-tier banks and during boom IPO markets, there is more potential underwriting-related bonus at stake, and thus the temptation to bias forecast is high. To empirically examine this hypothesis, we estimate the following regression:

$$Error_{i,j,t,n} = \alpha + TopTier_{i,t}\beta_1 + TopTier_{i,t} * \ln(IPOVolume)_t \beta_2 + \gamma' X_{i,j,t,n} + \varepsilon_{i,j,t,n} \quad (4)$$

Note that equation (4) is a pooled regression using all years of data since we need to capture the time-varying effect of market conditions.[19] In (4), $TopTier_{it}$ is the dummy indicating whether analyst i works for a top-tier bank at the time of the forecast, $X_{i,j,t,n}$ is the same list of controls, and ln*(IPOVolume)* is the log of the IPO volume of year t. This variable is interacted with the top-tier dummy to examine the conflict of interest hypothesis. If conflict of interest arises from underwriting pressure, a positive sign on the interaction term is expected, since analysts will become inaccurate in top-tier banks during boom years.

Regression results for equation (4) are reported in Table VIII. As before, the first vertical panel pertains to the whole sample, and the latter two pertain to the AA and non-

[19] To allow for heteroskedasticity, the Huber/White/sandwich estimator of variance is used for the coefficient estimates.

AA sub-samples, respectively. For the overall sample, the coefficient on the interaction term between bank status and IPO volume is insignificant. This, however, masks interesting patterns in sub-samples: The interaction term is negative and significant for the AA sub-sample, and positive and significant for the non-AA sub-sample.

The non-AA sub-sample result is consistent with the existence of conflict of interest. The positive sign on the interaction term indicates that the forecast accuracy of top-tier-bank non-AAs drops in peak IPO years relative to that of lower-status-bank non-AAs. In light of the static result in Section 5.2 that top-tier non-AAs are significantly more accurate than lower-status non-AAs, we find this time-varying result here to be difficult to reconcile with a behavioral- or self-selection-type explanation: Even if analysts tend to cover firms for which they hold favorable views, there is little (behavioral) reason to believe that all non-AA analysts working at top-tier investment banks come to hold more favorable views about their covered firms during peak years.[20] Instead, consistent with the notion that underwriting pressure is at its strongest at top-tier investment banks (large underwriters) *and* during boom years of the new-issues market, this result strongly supports the view that conflict of interest exists.

In contrast to the above, the coefficient on the interaction term is negative and significant for the AA sub-sample. This indicates that, unlike non-AAs, AA analysts working at top-tier banks do not become inaccurate during boom years; on the contrary, they become relatively *more* accurate. This result is consistent with the view that the AA

[20] Readers may wonder whether over-optimism of analysts drive some banks into the top-tier-bank category. This was our concern, too, so we deliberately employed a time-invariant definition of top-tier banks by using the Carter-Manaster tombstone measure. Our selection of top-tier banks is thus based on the long track record of past transactions rather than how a given bank did in the past 12 months at a given point in time.

status makes these star analysts more immune to the conflict of interest problem that seems to adversely affect the accuracy of non-AAs during boom years.

5.4. Does Personal Reputation Have a Mitigating Role?

In the previous sections, we separately documented a positive effect of personal reputation and a negative effect of underwriting pressure on forecast accuracy. In this section, we come to the central question of how these two forces interact. Does reputation have a mitigating role in the conflict of interest problem?

To address this question, we first estimate the following regression equation:

$$Error_{i,j,t,n} = \alpha + AA_{i,t}\beta_1 + (AA_{i,t} * \ln(IPOVolume)_t)\beta_2 + \gamma' X_{i,j,t,n} + \varepsilon_{i,j,t,n} \quad , \quad (5)$$

where $AA_{i,t}$ is the reputation dummy, $X_{i,j,t,n}$ is the same vector of controls as before, and $AA_{i,t} * \ln(IPOVolume)_t$ is the interaction term between reputation and underwriting volume (which proxies for underwriting pressure). Since the previous sub-section shows that, consistent with conflict of interest, top-tier-bank analysts tend to become inaccurate during peak IPO years, if personal reputation mitigates the effect of conflict of interest, a negative sign on the interaction term is expected. In other words, if personal reputation alleviates the conflict of interest problem, AAs should be relatively more accurate when the other analysts are succumbing to conflict of interest.

Table IX presents the estimation results of Equation (5). The three vertical panels exhibit results for the whole sample, the top-tier-bank sub-sample, and the lower-status-bank sub-sample, respectively. The first observation is that the sign on the key interaction term between AA status and IPO volume is *negative* and significant for the whole sample. Furthermore, sub-sample panels show that this overall result is driven entirely by top-tier-bank analysts. This means that AA-analysts in top-tier banks become significantly *more* accurate, relative to their non-AA peers during peak years. This is consistent with personal reputation playing a mitigating role in the conflict of interest problem that becomes particularly acute in top-tier banks in boom years.

This result is consistent with our finding in Table VIII that the accuracy of non-AA, top-tier-bank analysts goes down during peak years (relative to that of non-AA, lower-status-bank analysts). In Table IX, when we compare top-tier analysts alone, AAs become significantly more accurate relative to their non-AA peers during boom years. If we view the inaccuracy during boom years as an indication of conflict of interest, this says that the conflict of interest is prevalent among non-AAs, but not AA analysts.

Thus, results in tables VIII and IX together show that 1) conflict of interest exists, since non-AA, top-tier-bank analysts become inaccurate during boom years, the conditions under which conflict of interest is likely to be most acute, and 2) personal reputation, as indicated by the AA status, apparently mitigates this conflict of interest problem because AAs' accuracy is not compromised during boom years in top-tier banks.

To further illustrate this finding, we analyze the time series of the coefficients on the AA indicator in the 20 annual regressions that we presented in Section 5.1. Figure 1 plots the time series of AA coefficients from the top-tier-bank regressions against the corresponding IPO volumes for the 20 years in our sample. A striking pattern emerges: Here, we see that when the underwriting volumes are high, the coefficient on AA drops significantly (become more negative). In fact, the two lines move almost in exactly opposite directions. This graphically illustrates the conclusion that in top-tier banks, AAs become significantly more accurate relative to non-AAs during boom IPO years.

What can we learn from these observations? Our findings suggest that while conflict of interest exists, it seems to have the greatest impact on *non-star* analysts working in top-tier banks. As observed before, the attraction of the year-end bonus, and thus the temptation to become biased, are presumably strongest in top-tier banks during peak underwriting periods. As conflict of interest would predict, we find that, indeed, analysts working at top-tier banks become significantly less accurate during boom years. Importantly, however, this drop in accuracy is driven by non-AAs; in comparison, AAs become significantly more accurate than non-AAs in top-tier banks during boom years, the condition under which conflict of interest is expected to be most severe. Since both bank status and market conditions are controlled for and the only variation is the AA designation, we find it compelling to argue that the difference is driven by personal reputation. Without personal reputation at stake, non-AA analysts working at top-tier banks have little to lose and much to gain from biasing their estimates in hot markets. In contrast, AAs working at the same top-tier banks have more to lose, and this concern, on average, makes them remain more accurate in the peak years. The conclusion we draw from this is that personal reputation does have a mitigating role for the conflict of interest problem that becomes particularly acute in top-tier banks during hot markets.

Our finding is perhaps contrary to the popular press depiction of “fallen-star” analysts in recent years, embroiled in scandals and their reputations tarnished. Our evidence suggests that those “fallen” analysts who were also All-Americans, were not representative of the AA group at large, but rather were a few exceptional cases of analysts working at top-tier banks at the very peak of the market boom. On average, it is those non-star analysts working at top-tier investment banks whose forecast accuracy is most dramatically compromised during peak years. Without a personal reputation at stake, these analysts are most susceptible to the temptations of conflict of interest.

5.5 Does Analyst Composition Change over Time?

Our main results in the previous subsections suggest that, consistent with conflict of interest, top-tier analysts become less accurate during IPO boom years. Consistent with the mitigating role of personal reputation, this inaccuracy is primarily driven by non-AAs; AAs, in contrast, do not become inaccurate during IPO boom years.

Can these results be explained by an alternative hypothesis? It is possible that the lowered accuracy of top-tier non-AAs during peak IPO years is driven by (i) a high number and (ii) the diluted quality/ability of new analysts being hired by top-tier banks during these peak years. Since the underwriting volumes fluctuate significantly over time, these banks may meet the fluctuating demands for equity analysts by hiring a disproportionately large number of new analysts (either newly-minted MBAs or seasoned analysts from lower-status banks) during the peak years. If banks fill their personnel needs by lowering the average quality/ability of new hires (who are, by definition, unlikely to be AAs), this hiring pattern alone can result in lowered accuracy of top-tier non-AAs during peak years. As a result, our finding can be driven by the changing composition of the top-tier non-AA pool, rather than conflict of interest among *existing* top-tier non-AAs.

To investigate the composition effect as an alternative hypothesis, we first examine in more detail the actual composition of the top-tier, non-AA pool. Specifically, for each year of our sample, we divide the pool of top-tier, non-AA analysts into three subsamples: existing, transfers, and new hires. An existing top-tier non-AA is such an analyst who also belongs to the top-tier, non-AA pool the previous year. The remaining top-tier non-AAs are, by definition, non-existing, and this group is then further divided into transfers and new hires. A "transfer" is a top-tier, non-AA analyst this year, who does not belong to the top-tier, non-AA pool the previous year, but is in the total analyst pool the previous year. A "new hire" is a new top-tier non-AA who is not in the database the previous year at all.

Figure 2 provides a graph of the composition over time. It is immediately clear that the proportion of existing analysts in the top-tier, non-AA pool is about 80%, or, equivalently, the turnover among this group of analysts is about 20% each year. This ratio is stable and does not appear to trend up or down over the sample period. Importantly, there is no systematic increase in the hiring of new analysts by top-tier banks during peak IPO years, as indicated by the dotted vertical lines in the figure.[21] The stability in the actual composition means that new and inexperienced analysts are unlikely to be the main drivers of the falling accuracy among top-tier non-AAs even in peak years.

Next, we compare the accuracy of these three groups of top-tier, non-AA analysts over time. The results are reported in Table XI. The first panel compares the accuracy of non-existing (transfers and new hires) analysts and existing analysts; the second panel compares the accuracy of transfers and new hires. In addition to the yearly results, we

[21] These four years are identified as peak IPO years using three different criteria. First, at least 7 months in the year are designated as "peak IPO months" in Jay Ritter's IPO database. Second, a plot of IPO volume against time visually reveals that these years are the peaks of various IPO cycles. Lastly, we also examine the difference between the annual IPO volume and its five-year moving average. These four years each exceed the moving average by more than $1bn.

also report the pooled results of (i) all years and (ii) peak years only, where just the four peak IPO years (as defined above) are aggregated.

We find that, again contrary to the predictions of the compositional-effect, non-existing analysts (both transfers and new hires) are *more* accurate than existing analysts. The difference is statistically significant in 11 out of 19 individual years, as well as in the pooled tests using all the years and only the peak years, respectively. Further disaggregating the non-existing analysts into new hires and transfers, we find that transfers are significantly *more* accurate than new hires when pooling all the years. However, the difference is not statistically significant when pooling only the four peak IPO years. Overall, we find no evidence that top-tier banks hire new analysts of lower quality/ability during the IPO peak years.

Finally, Table XII presents multivariate analyses of the accuracy of the three types of top-tier non-AAs. We estimate the following regression equation:

$$Error_{i,j,t,n} = \alpha + Type_{i,t}\beta_1 + \gamma' X_{i,j,t,n} + \varepsilon_{i,j,t,n}, \qquad (6)$$

where $Type_{i,t}$ is the dummy variable indicating which subcategory of top-tier, non-AAs analyst i belongs to, and $X_{i,j,t,n}$ is the same vector of controls as before. Panel A reports the estimation results for all years; Panel B reports those for peak IPO years only. In each panel, two specifications are reported. In the first specification, the included *Type* dummy variable is for existing analysts. In the second specification, two *Type* dummy variables are for transfers and new hires, respectively.

Consistent with the uni-variate *t*-tests, we find that existing top-tier, non-AA analysts are *less* accurate than those analysts hired recently by top-tier banks, even after controlling for various firm and forecast characteristics. In the all-year sample, reported in Panel A, this result is statistically significant: Both new hires and transfers are significantly more accurate than existing analysts. While the difference in accuracy becomes non-significant during peak IPO years, as reported in Panel B, the sign remains the same, i.e., the non-existing analysts are still not *less* accurate than the existing ones. All the control variables are significant and have the predicted signs.

To summarize, we find that the proportion of new analysts among the non-AA pool is constant over time, and if anything, they are more, not less, accurate than the existing analysts. These results consistently run counter to the compositional-effect hypothesis that top-tier investment banks resort to hiring a disproportionately large number of lower-quality analysts during peak years, and it is this hiring pattern that drives our main result. Conflict of interest thus remains the most plausible explanation for our main result that top-tier, non-AA analysts become inaccurate during IPO boom years.

7. Conclusion

We examine how personal reputation, conflict of interest, and the interaction of these two forces affect the accuracy of analysts' earnings forecasts. We find that personal reputation is positively related to forecast accuracy. Analysts with the All-American (AA) designation make significantly more accurate forecasts than those without the title. We also find that top-tier-bank analysts are generally more accurate than their lower-tier counterparts. This result derives mainly from non-AA analysts; among AAs, bank status does not affect accuracy. These two results on AA status and bank status both point to a positive pay-performance relation, and hence a reasonable degree of efficiency in the labor market for analysts.

Second, we find that forecast accuracy drops among non-AA analysts employed at top-tier banks in boom years. Since the analysts' expected payoffs from generating underwriting business for their employers by biasing their forecasts are the highest at top-tier banks during peak underwriting periods, this finding provides compelling evidence for the existence of conflict of interest.

Our most interesting finding pertains to the interaction between personal reputation and underwriting pressure. Here we conclude that personal reputation plays an important mitigating role in alleviating the conflict of interest problem. We find that it is only the non-star analysts employed at top-tier investment banks whose forecast accuracy falls during market booms. In contrast, among top-tier-bank analysts, AAs become significantly more accurate during peak underwriting periods. It appears that without a personal reputation at stake, non-AAs have too much to gain and too little to lose by becoming biased in peak underwriting periods. The AAs, however, *do* have a personal reputation to lose, and this concern effectively curbs the degree of bias in their earnings forecasts during hot underwriting markets.

An important caveat is that we limited the scope of analysis in this paper to forecast accuracy. In the real world, however, analysts also make stock recommendations and long-term earnings growth projections. It is thus plausible that AAs behave differently in choosing accuracy of forecasts versus recommendations, and short-run forecasts versus long-run forecasts.

Analyst research has long attracted interest in the academic literature. But never before has this line of research been as relevant as it is today, since the allegation of conflict of interest in sell-side research made front-page headlines and lawmakers scrambled to fix the "Chinese Wall" that is supposed to separate research from banking. Against this backdrop, the angle we take in this paper – examining the role of personal reputation and of bank status, and their interactions with conflict of interest – is timely and relevant.

To the extent that the quality, and not just quantity, of information produced by analysts matters in improving market efficiency, our results suggest that both personal reputation and bank status are useful screening devices for investors to gauge analysts' ability to produce such information. Although conflict of interest does appear to afflict top-tier, non-AA analysts during market booms, overall, top-tier analysts are better than less-select, lower-status-bank analysts. Moreover, the AA designation is found to play an effective mitigating role in the conflict of interest problem at large underwriter banks. Investors can thus benefit from leveraging the disciplinary role of personal reputation by putting more weight on information produced by AA analysts during boom years.

Our results also suggest at least two ways in which overzealous reforms may unwittingly bring in more distortions rather than improvements to the efficiency of the market. For example, the AA election process has recently come under attack as a "popularity contest", and some investment banks have stopped using it as a criterion in determining analysts' performance.[22] This, however, could actually do a disservice to

[22] Eliott Spitzer, among others, has criticized the selection criteria of AAs as subjective. Subsequently, Morgan Stanley announced in 2003 that it would no longer use analysts' *II* rankings to gauge performance. It also stopped providing *II* with photographs of its analysts. *Wall Street Journal* (Eastern Edition). New York, N.Y., Nov 23, 2004. C.5.

investors, because analysts are less incentivized to be accurate once they are deprived of the rewards that come with an AA title.

Similarly, as part of the settlement, the largest banks were required to pay $450 million to purchase and publish third-party-produced research in addition to their own in-house research for the next five years. While this was intended to bring improved transparency and accuracy to Wall Street research, it could potentially distort or create considerable noise to the positive pay-performance relation and thus actually make it harder for investors to distinguish between "good" and "bad' research. Although we cannot pin down where the optimal balance lies, we are compelled to argue, based on our results, that the quest to eliminate all sources of conflict of interest should at least be balanced against the potential risk of reduced market efficiency, of both financial markets and labor markets for analysts.

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Figure 1. AA Effect vs. IPO Volume: Top-tier Banks

This figure plots the "AA effect", which is the coefficient on the AA dummy variable from the 20 annual regressions of the equation

$$Error_{i,j,n} = \alpha + AA_i \beta_1 + \gamma' X_{i,j,n} + \varepsilon_{i,j,n} \quad (2)$$

using the top-tier-bank sub-sample. This coefficient is plotted against the underwriting volume.

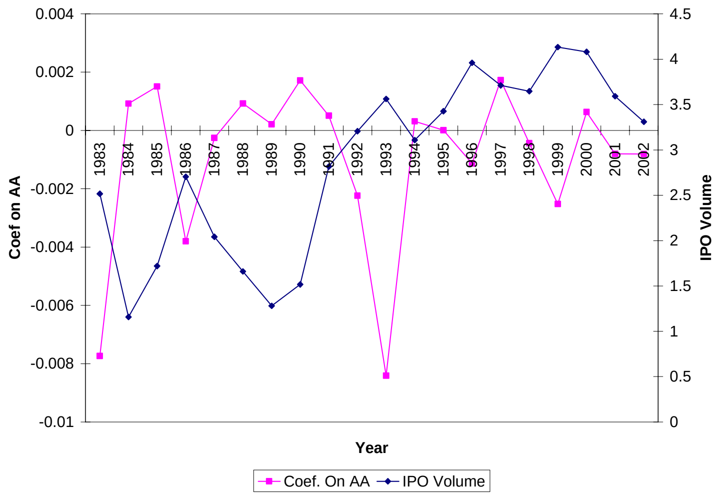

Figure 2. Composition of Top-tier Non-AA Analysts

This figure describes the composition of the pool of top-tier non-AA analysts over the sample period of 1983 to 2002. Each year, the pool of all top-tier non-AAs is subdivided into three categories: Existing, Transfers, and New Hires. An Existing top-tier non-AA is an analyst who is in the top-tier non-AA pool in the current year as well as in the previous year. The remaining top-tier non-AAs are considered Non-Existing, and they are further divided into Transfers and New Hires. A Transfer is a top-tier non-AA analyst in the current year, who was in the forecast database but was not in the top-tier non-AA pool last year. Thus, by definition, he could be a top-tier AA, a lower-status AA, or a lower-status non-AA the previous year. The remaining top-tier non-AAs are considered New Hires. Thus, these are the analysts in the top-tier non-AA pool for the current year but who were not in the forecast database the previous year. The dotted vertical lines indicate peak IPO years.

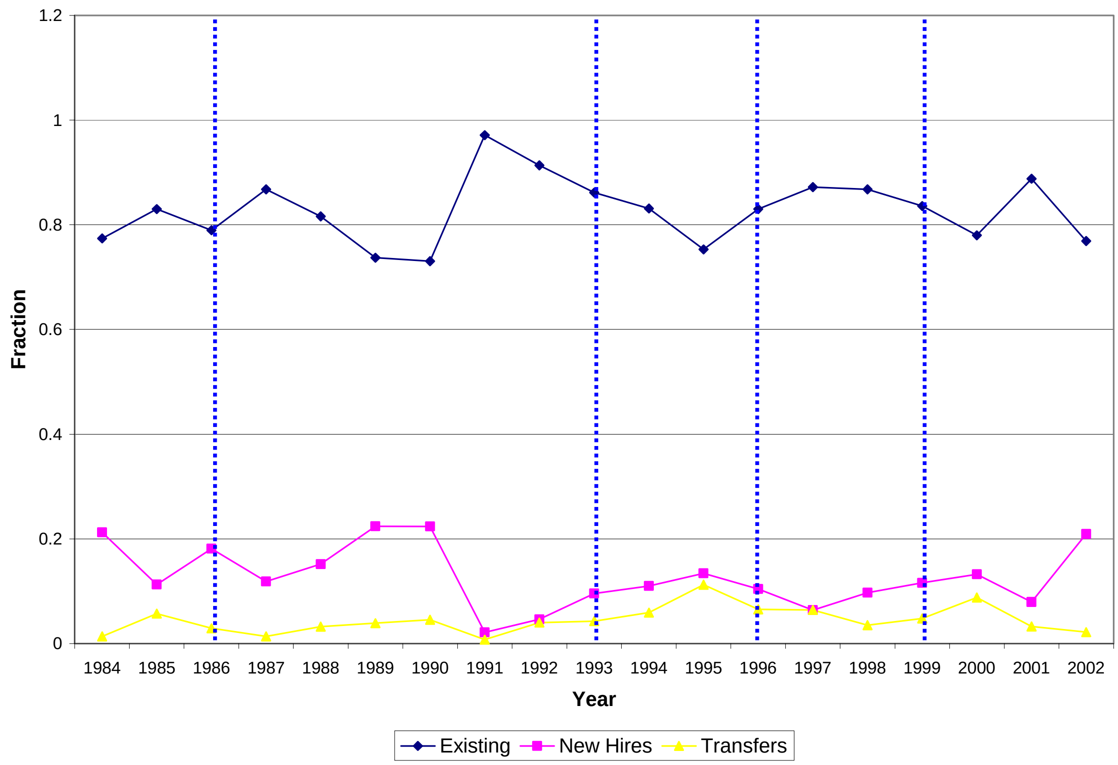

Table I. Sample Descriptive Statistics

This table lists summary statistics for the sample. "Firms" is the number of firms covered in the I/B/E/S data set, computed by the number of distinctive CUSIP codes. "Analysts" is the number of analysts in the sample, counted by distinct analyst codes. "AA" stands for "All-American" analysts. Names of these analysts are obtained from the October issue of the *Institutional Investor* magazine each year, and matched to the names in the I/B/E/S Translation file. "Reports" is the total number of reports (forecasts) issued. Each analyst-firm-estimation date combination is considered a report.

Fiscal Year	Firms	Analysts				Reports			
		All	Non-AA	AA	Non-AA to AA Ratio	All	By Non-AA	By AA	Non-AA to AA Ratio
1983	2,423	2,171	1,939	232	8.36	52,359	42,082	10,277	4.09
1984	2,938	2,304	2,034	270	7.53	68,354	54,633	13,721	3.98
1985	3,287	2,407	2,190	217	10.09	82,529	70,673	11,856	5.96
1986	3,501	2,370	2,079	291	7.14	81,778	66,240	15,538	4.26
1987	3,857	2,548	2,251	297	7.58	90,565	73,120	17,445	4.19
1988	3,965	2,464	2,160	304	7.11	92,297	74,574	17,723	4.21
1989	3,800	2,661	2,312	349	6.62	87,078	70,155	16,923	4.15
1990	3,656	2,718	2,370	348	6.81	90,457	72,254	18,203	3.97
1991	3,562	2,410	2,062	348	5.93	91,230	70,499	20,731	3.40
1992	3,643	2,289	1,933	356	5.43	91,579	68,997	22,582	3.06
1993	3,949	2,454	2,077	377	5.51	96,176	71,721	24,455	2.93
1994	4,323	2,831	2,457	374	6.57	99,081	75,983	23,098	3.29
1995	4,703	3,145	2,877	268	10.74	107,654	90,710	16,944	5.35
1996	5,153	3,516	3,236	280	11.56	117,244	99,669	17,575	5.67
1997	5,475	3,951	3,659	292	12.53	122,064	104,460	17,604	5.93
1998	5,382	4,370	4,032	338	11.93	136,086	114,824	21,262	5.40
1999	5,022	4,528	4,188	340	12.32	132,786	113,268	19,518	5.80
2000	4,550	4,687	4,356	331	13.16	127,051	108,913	18,138	6.00
2001	3,688	4,492	4,166	326	12.78	126,263	106,190	20,073	5.29
2002	3,373	4,758	4,442	316	14.06	125,215	106,207	19,008	5.59

Table II. Summary Statistics for Work Patterns – AAs versus Non-AAs

This table reports summary statistics on the work patterns of AA analysts versus non-AA analysts. "Coverage" is the number of distinct firms (CUSIP codes) covered by an analyst. "Average Frequency" is the mean number of forecasts an analyst makes for his covered firm, averaged over the firms that he covers. "Reports" is the total number of forecasts an analyst makes during a given period.

Year	Coverage			Average Frequency			Reports		
	Non-AA	AA	T-Stat	Non-AA	AA	T-Stat	Non-AA	AA	T-Stat
1983	3.97	4.58	-2.64	1.93	2.78	-11.23	8.07	12.57	-7.24
	(1,459)	(218)		(1,459)	(218)		(1,459)	(218)	
1984	3.99	4.90	-3.77	2.29	3.17	-10.66	9.40	15.75	-8.32
	(1,544)	(244)		(1,544)	(244)		(1,544)	(244)	
1985	4.01	4.80	-3.43	2.54	3.40	-8.84	10.83	16.28	-6.18
	(1,631)	(193)		(1,631)	(193)		(1,631)	(193)	
1986	4.44	5.30	-3.41	2.32	3.20	-11.28	10.89	17.23	-7.68
	(1,606)	(276)		(1,606)	(276)		(1,606)	(276)	
1987	4.63	5.72	-4.14	2.37	3.38	-12.56	11.78	19.49	-8.78
	(1,685)	(284)		(1,685)	(284)		(1,685)	(284)	
1988	4.92	6.07	-4.01	2.61	3.54	-11.41	13.75	21.75	-7.79
	(1,621)	(289)		(1,621)	(289)		(1,621)	(289)	
1989	4.97	5.95	-3.14	2.43	3.16	-8.80	13.04	19.60	-6.08
	(1,767)	(331)		(1,767)	(331)		(1,767)	(331)	
1990	4.78	6.10	-4.53	2.65	3.53	-10.11	13.45	21.83	-8.19
	(1,792)	(322)		(1,792)	(322)		(1,792)	(322)	
1991	5.08	6.43	-4.93	3.04	4.02	-10.65	16.04	25.41	-8.60
	(1,563)	(328)		(1,563)	(328)		(1,563)	(328)	
1992	5.38	6.89	-5.42	2.92	3.88	-11.27	16.58	26.89	-9.38
	(1,525)	(342)		(1,525)	(342)		(1,525)	(342)	
1993	5.69	7.33	-4.85	2.78	3.66	-11.01	16.39	27.12	-9.30
	(1,671)	(374)		(1,671)	(374)		(1,671)	(374)	
1994	5.37	7.94	-7.46	2.67	3.37	-9.78	15.21	27.16	-10.59
	(1,959)	(367)		(1,959)	(367)		(1,959)	(367)	
1995	5.56	8.21	-7.86	2.73	3.52	-9.55	16.20	29.37	-10.64
	(2,304)	(262)		(2,304)	(262)		(2,304)	(262)	
1996	5.67	8.55	-9.04	2.77	3.52	-8.37	16.75	30.46	-10.94
	(2,572)	(274)		(2,572)	(274)		(2,572)	(274)	
1997	5.71	9.19	-11.63	2.65	3.36	-9.61	16.40	31.44	-12.75
	(2,995)	(285)		(2,995)	(285)		(2,995)	(285)	
1998	5.75	10.07	-12.96	2.75	3.70	-12.91	17.44	37.50	-15.19
	(3,421)	(332)		(3,421)	(332)		(3,421)	(332)	
1999	4.05	6.71	-10.72	2.42	2.72	-4.91	10.16	18.75	-10.63
	(3,242)	(324)		(3,242)	(324)		(3,242)	(324)	
2000	6.51	11.51	-13.80	2.52	3.31	-11.86	18.05	39.61	-15.78
	(3,851)	(325)		(3,851)	(325)		(3,851)	(325)	
2001	7.15	12.82	-10.70	2.82	4.16	-14.35	23.29	54.78	-14.72
	(3,836)	(319)		(3,836)	(319)		(3,836)	(319)	
2002	6.98	13.87	-16.26	2.79	4.08	-15.01	22.70	57.00	-18.79
	(4,160)	(312)		(4,160)	(312)		(4,160)	(312)	

Table III. Comparison of Firm Characteristics

This table compares select firm characteristic for firms covered by AAs and those covered by non-AAs. "Market Cap" is the firm's market capitalization of equity, computed by shares outstanding times the year-end closing price. The unit of measure is millions of dollars. "Leverage" is the firm's debt to asset ratio, computed as total debt divided by total assets. "NYSE" is an indicator variable equaling 1 if the firm is listed on the NYSE, and 0 otherwise. "Volatility" is the market-model residual return standard deviation, computing using 120 days of returns data prior to each forecast date. If a firm receives multiple forecasts for a year, the volatilities are averaged to arrive at the final volatility measure. ***, **, and * denote that the t-statistic is statistically significantly at the 1, 5, and 10 percent levels, respectively, based on two-tailed test.

Year	Market Cap			Leverage			NYSE			Volatility		
	Non-AA	AA	T-Stat	Non-AA	AA	T-Stat	Non-AA	AA	T-Stat	Non-AA	AA	T-Stat
1983	2,936.45	2,885.63	0.24	0.14	0.15	-1.51	0.88	0.89	-1.71**	0.019	0.019	1.02
	(6,328)	(1,023)		(6,320)	(1,023)		(6,329)	(1,023)		(6,329)	(1,023)	
1984	2,537.36	2,715.20	-0.85	0.13	0.14	-1.51	0.84	0.87	-2.77***	0.018	0.017	3.24***
	(6,248)	(1,201)		(6,239)	(1,201)		(6,250)	(1,201)		(6,250)	(1,201)	
1985	2,859.09	3,338.68	-1.51	0.15	0.13	4.00***	0.83	0.88	-4.28***	0.017	0.016	6.47***
	(6,594)	(927)		(6,551)	(926)		(6,594)	(927)		(6,594)	(927)	
1986	3,079.64	3,605.58	-2.38**	0.16	0.17	-3.01***	0.82	0.87	-5.71***	0.018	0.017	3.40***
	(7,192)	(1,465)		(7,171)	(1,464)		(7,193)	(1,465)		(7,193)	(1,465)	
1987	3,353.19	3,992.68	-3.00***	0.16	0.18	-4.99***	0.79	0.86	-7.98***	0.020	0.019	5.34***
	(7,957)	(1,628)		(7,919)	(1,624)		(7,962)	(1,629)		(7,961)	(1,629)	
1988	3,129.82	3,718.57	-3.23***	0.16	0.18	-5.36***	0.77	0.86	-9.10***	0.021	0.020	3.91***
	(8,029)	(1,752)		(7,977)	(1,749)		(8,041)	(1,759)		(8,041)	(1,759)	
1989	3,491.34	4,203.16	-4.20***	0.17	0.19	-4.94***	0.76	0.85	-9.73***	0.017	0.015	9.44***
	(8,879)	(1,977)		(8,787)	(1,970)		(8,903)	(1,983)		(8,903)	(1,983)	
1990	3,398.54	4,447.25	-5.48***	0.17	0.18	-3.56***	0.73	0.83	-9.69***	0.019	0.017	7.39***
	(8,749)	(1,980)		(8,649)	(1,969)		(8,759)	(1,983)		(8,759)	(1,983)	
1991	4,131.00	5,276.73	-5.06***	0.17	0.18	-2.94***	0.73	0.83	-10.20***	0.022	0.020	8.20***
	(8,002)	(2,123)		(7,913)	(2,113)		(8,011)	(2,128)		(8,011)	(2,128)	
1992	4,612.96	5,614.26	-4.45***	0.15	0.17	-5.51***	0.72	0.82	-11.36***	0.022	0.020	9.06***
	(8,178)	(2,355)		(8,006)	(2,334)		(8,187)	(2,360)		(8,187)	(2,360)	
1993	4,691.82	5,763.44	-5.25***	0.15	0.18	-7.31***	0.69	0.82	-14.19***	0.021	0.019	11.97***
	(9,446)	(2,723)		(9,389)	(2,713)		(9,463)	(2,728)		(9,463)	(2,728)	

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Year	Market Cap			Leverage			NYSE			Volatility		
	Non-AA	AA	T-Stat	Non-AA	AA	T-Stat	Non-AA	AA	T-Stat	Non-AA	AA	T-Stat
1994	4,646.25	5,524.98	-4.45***	0.15	0.18	-7.68***	0.66	0.79	-13.58***	0.022	0.019	14.67***
	(10,342)	(2,877)		(10,283)	(2,862)		(10,358)	(2,881)		(10,358)	(2,881)	
1995	5,361.20	6,274.92	-3.49***	0.16	0.19	-6.28***	0.66	0.77	-10.68***	0.021	0.018	12.49***
	(12,439)	(2,094)		(12,327)	(2,077)		(12,498)	(2,101)		(12,498)	(2,101)	
1996	5,971.99	7,826.70	-5.48***	0.16	0.19	-6.32***	0.63	0.75	-12.36***	0.023	0.020	12.14***
	(14,045)	(2,273)		(13,948)	(2,262)		(14,052)	(2,273)		(14,052)	(2,273)	
1997	7,483.70	9,999.22	-5.61***	0.17	0.20	-8.68***	0.60	0.75	-15.88***	0.024	0.020	18.32***
	(16,153)	(2,502)		(16,008)	(2,490)		(16,162)	(2,503)		(16,162)	(2,503)	
1998	9,040.75	11,486.53	-4.70***	0.19	0.22	-7.71***	0.58	0.71	-14.31***	0.027	0.024	12.81***
	(18,293)	(3,127)		(18,214)	(3,118)		(18,331)	(3,128)		(18,331)	(3,128)	
1999	9,612.74	12,210.70	-2.78***	0.19	0.23	-7.56***	0.48	0.63	-13.05***	0.036	0.033	8.85***
	(11,777)	(1,951)		(11,769)	(1,945)		(11,823)	(1,958)		(11,823)	(1,958)	
2000	14,969.93	17,428.91	-2.83***	0.18	0.22	-12.22***	0.54	0.70	-19.20***	0.039	0.035	14.41***
	(21,669)	(3,230)		(21,648)	(3,229)		(21,830)	(3,250)		(21,830)	(3,250)	
2001	11,476.70	15,643.70	-5.94***	0.18	0.23	-14.46***	0.54	0.71	-20.67***	0.037	0.032	19.60***
	(22,609)	(3,426)		(22,534)	(3,422)		(22,888)	(3,464)		(22,888)	(3,464)	
2002	8,872.30	14,194.84	-8.75***	0.18	0.23	-13.80***	0.55	0.74	-24.08***	0.032	0.027	17.55***
	(22,899)	(3,503)		(23,006)	(3,529)		(23,801)	(3,598)		(23,801)	(3,598)	

Table IV. Forecast Errors: AAs vs. Non-AAs

This table compares the forecast errors of the AA analysts to those of the non-AA analysts. Forecast error is calculated as the absolute difference between an analyst's EPS estimate and the actual EPS eventually reported, scaled by the firm's book value of equity per share at the previous fiscal year end. The numbers reported are the scaled errors times 100, so that they are percentage errors. Numbers of observations are in parentheses. The t-statistics are for the differences in the forecast errors. AA is the analyst's All-American status at the time the forecast is issued. ***, **, and * denote that the t-statistic is statistically significantly at the 1, 5, and 10 percent levels, respectively, based on two-tailed test.

Year	All			Top-Tier Banks			Lower-Status Banks		
	AA	Non-AA	t-stat	AA	Non-AA	t-stat	AA	Non-AA	t-stat
1983	1.42	3.66	-16.96	1.29	3.39	-12.82	1.53	3.75	-10.92
	(394)	(14,821)		(185)	(4,039)		(209)	(10,782)	
1984	3.69	4.04	-1.12	3.84	3.55	1.19	3.55	4.17	-1.56
	(3,337)	(14,278)		(1,565)	(3,045)		(1,772)	(11,233)	
1985	3.74	4.90	-4.65	3.96	3.88	0.35	3.56	5.16	-5.22
	(3,661)	(16,534)		(1,659)	(3,403)		(2,002)	(13,131)	
1986	3.94	4.81	-2.93	3.85	4.95	-1.58	4.03	4.76	-2.18
	(3,739)	(17,645)		(1,881)	(4,354)		(1,858)	(13,291)	
1987	3.73	4.48	-3.86	4.13	3.85	0.86	3.25	4.66	-7.72
	(5,152)	(18,885)		(2,777)	(4,288)		(2,375)	(14,597)	
1988	3.31	3.65	-1.46	3.54	3.70	-0.37	2.96	3.64	-4.24
	(5,843)	(20,871)		(3,492)	(4,354)		(2,351)	(16,517)	
1989	3.70	4.01	-2.78	3.76	4.10	-1.57	3.61	3.99	-2.76
	(6,334)	(21,391)		(3,620)	(4,146)		(2,714)	(17,245)	
1990	7.20	4.67	3.38	8.54	4.52	3.32	5.59	4.71	1.19
	(6,909)	(23,591)		(3,764)	(4,194)		(3,145)	(19,397)	
1991	3.03	3.41	-4.29	3.06	3.30	-1.60	3.00	3.43	-4.06
	(7,730)	(23,605)		(4,263)	(3,755)		(3,467)	(19,850)	
1992	3.00	3.59	-4.91	2.90	3.56	-2.87	3.12	3.60	-3.27
	(8,668)	(23,829)		(4,725)	(3,958)		(3,943)	(19,871)	
1993	2.83	3.22	-3.20	2.83	3.45	-1.84	2.83	3.18	-2.63
	(9,664)	(26,217)		(5,214)	(4,412)		(4,450)	(21,805)	

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Year	All			Top-Tier Banks			Lower-Status Banks		
	AA	Non-AA	t-stat	AA	Non-AA	t-stat	AA	Non-AA	t-stat
1994	3.92	3.76	0.53	4.29	2.85	3.76	3.43	3.95	-1.61
	(9,567)	(26,482)		(5,444)	(4,485)		(4,123)	(21,997)	
1995	4.33	3.81	2.42	4.51	4.01	1.33	4.05	3.77	1.12
	(9,512)	(31,504)		(5,867)	(5,346)		(3,645)	(26,158)	
1996	3.27	3.69	-3.39	3.30	3.83	-2.84	3.23	3.66	-2.63
	(7,046)	(37,372)		(4,400)	(7,339)		(2,646)	(30,033)	
1997	3.23	3.88	-4.24	3.31	3.62	-1.71	3.11	3.95	-3.93
	(8,094)	(42,730)		(4,941)	(8,976)		(3,153)	(33,754)	
1998	3.58	5.29	-11.26	3.36	4.76	-7.16	3.92	5.42	-6.08
	(9,445)	(47,935)		(5,714)	(9,437)		(3,731)	(38,498)	
1999	9.00	9.29	-0.30	8.99	9.61	-0.42	9.02	9.23	-0.14
	(5,586)	(29,611)		(3,428)	(5,026)		(2,158)	(24,585)	
2000	5.67	6.47	-3.58	5.54	6.11	-2.03	5.91	6.54	-1.30
	(11,077)	(58,185)		(7,359)	(9,841)		(3,718)	(48,344)	
2001	5.33	5.12	0.97	5.67	5.52	0.40	4.67	5.05	-1.37
	(14,258)	(73,073)		(9,439)	(11,071)		(4,819)	(62,002)	
2002	3.28	3.31	-0.29	3.26	3.17	0.60	3.31	3.34	-0.18
	(11,480)	(61,912)		(7,035)	(9,387)		(4,445)	(52,525)	
Total	4.19	4.59	-5.74	4.37	4.43	-0.55	3.94	4.62	-7.80
	(147,496)	(630,471)		(86,772)	(114,856)		(60,724)	(515,615)	

Table V. Forecast Errors: Top-tier Bank Analysts vs. Lower-Status Bank Analysts

This table compares forecast errors of analysts in top-tier banks versus those of the analysts at the lower-status banks. Forecast error is calculated as the absolute difference between an analyst's EPS estimate and the actual EPS eventually reported, scaled by the firm's book value of equity per share at the previous fiscal year end. The numbers reported are the actual scaled errors times 100, and therefore are percentage errors. Numbers of observations are in parentheses. The t-statistics are for the differences in forecast errors. Top-Tier banks are taken to be the top 10 underwriters identified in Cater, Dark, and Singh (1998). These top 10 underwriters are: Alex Brown & Sons, Drexel Burham Lambert, First Boston Corporation, Goldman Sachs & Company, Hambrecht & Quist, Merrill Lynch, Morgan Stanley & Company, Paine Webber, Prudential-Bache, and Salmon Brothers. ***, **, * denote that the t-statistic is statistically significantly at the 1, 5, and 10 percent levels, respectively, based on two-tailed test.

Year	All			AA			Non AA		
	Top Tier	Lower Status	t-stat	Top Tier	Lower Status	t-stat	Top Tier	Lower Status	t-stat
1983	3.30	3.71	-3.67***	1.29	1.53	-1.06	3.39	3.75	-3.13***
	(4,224)	(10,991)		(185)	(209)		(4,039)	(10,782)	
1984	3.65	4.09	-1.31	3.84	3.55	1.13	3.55	4.17	-1.60
	(4,610)	(13,005)		(1,565)	(1,772)		(3,045)	(11,233)	
1985	3.91	4.95	-3.97***	3.96	3.56	1.73*	3.88	5.16	-4.23***
	(5,062)	(15,133)		(1,659)	(2,002)		(3,403)	(13,131)	
1986	4.62	4.67	-0.10	3.85	4.03	-0.75	4.95	4.76	0.26
	(6,235)	(15,149)		(1,881)	(1,858)		(4,354)	(13,291)	
1987	3.96	4.47	-2.56***	4.13	3.25	3.00***	3.85	4.66	-3.55***
	(7,065)	(16,972)		(2,777)	(2,375)		(4,288)	(14,597)	
1988	3.63	3.55	0.33	3.54	2.96	1.64*	3.70	3.64	0.22
	(7,846)	(18,868)		(3,492)	(2,351)		(4,354)	(16,517)	
1989	3.94	3.94	-0.01	3.76	3.61	0.83	4.10	3.99	0.59
	(7,766)	(19,959)		(3,620)	(2,714)		(4,146)	(17,245)	
1990	6.42	4.83	2.63***	8.54	5.59	2.12**	4.52	4.71	-0.69
	(7,958)	(22,542)		(3,764)	(3,145)		(4,194)	(19,397)	
1991	3.17	3.36	-1.98**	3.06	3.00	0.53	3.30	3.43	-0.90
	(8,018)	(23,317)		(4,263)	(3,467)		(3,755)	(19,850)	
1992	3.20	3.52	-2.20**	2.90	3.12	-1.52	3.56	3.60	-0.14
	(8,683)	(23,814)		(4,725)	(3,943)		(3,958)	(19,871)	
1993	3.12	3.12	0.00	2.83	2.83	0.01	3.45	3.18	0.86
	(9,626)	(26,255)		(5,214)	(4,450)		(4,412)	(21,805)	

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Year	All			AA			Non AA		
	Top Tier	Lower Status	t-stat	Top Tier	Lower Status	t-stat	Top Tier	Lower Status	t-stat
1994	3.64	3.87	-0.82	4.29	3.43	1.95**	2.85	3.95	-4.61***
	(9,929)	(26,120)		(5,444)	(4,123)		(4,485)	(21,997)	
1995	4.27	3.81	2.25**	4.51	4.05	1.25	4.01	3.77	0.91
	(11,213)	(29,803)		(5,867)	(3,645)		(5,346)	(26,158)	
1996	3.63	3.63	0.03	3.30	3.23	0.35	3.83	3.66	1.20
	(11,739)	(32,679)		(4,400)	(2,646)		(7,339)	(30,033)	
1997	3.51	3.88	-2.55***	3.31	3.11	0.86	3.62	3.95	-2.02***
	(13,917)	(36,907)		(4,941)	(3,153)		(8,976)	(33,754)	
1998	4.23	5.28	-6.39***	3.36	3.92	-2.42**	4.76	5.42	-3.09***
	(15,151)	(42,229)		(5,714)	(3,731)		(9,437)	(38,498)	
1999	9.36	9.21	0.17	8.99	9.02	-0.02	9.61	9.23	0.36
	(8,454)	(26,743)		(3,428)	(2,158)		(5,026)	(24,585)	
2000	5.87	6.50	-3.24***	5.54	5.91	-0.73	6.11	6.54	-1.65*
	(17,200)	(52,062)		(7,359)	(3,718)		(9,841)	(48,344)	
2001	5.59	5.02	2.72***	5.67	4.67	2.76***	5.52	5.05	1.75*
	(20,510)	(66,821)		(9,439)	(4,819)		(11,071)	(62,002)	
2002	3.21	3.33	-1.46	3.26	3.31	-0.24	3.17	3.34	-1.66*
	(16,422)	(56,970)		(7,035)	(4,445)		(9,387)	(52,525)	
All Years	4.40	4.55	-2.34**	4.37	3.94	3.69***	4.43	4.62	-2.58***
	(201,628)	(576,339)		(86,772)	(60,724)		(114,856)	(515,615)	

Table VI: Effect of Analyst Reputation on Forecast Accuracy

This table presents the aggregated result from the 20 annual Fama-MacBeth regressions of analysts' forecast errors on the analysts' AA status and the other control variables. The first vertical panel presents the results of the whole sample. The latter two panels are for the top-tier banks and the lower-status banks, respectively. The reported coefficients are the averages from the 20 annual regressions, and the t-statistics are computed from the empirical distributions of the coefficient estimates.

For each fiscal year from 1983-2002, the following regression is estimated:

$$\begin{aligned} \text{Error}_{i,j,n} &= \alpha + \text{AA}_i\beta_1 + \ln(\text{distance})_{i,j,n}\beta_2 + \text{Firm Size}_j\,\beta_3 + \\ &\text{Leverage}_j\beta_4 + \text{Volatility}_j\beta_5 + \text{Firm Fixed Effects}_j\beta_j + \varepsilon_{i,j,n} \end{aligned} \quad . \tag{2}$$

The dependent variable $Error_{i.j,n}$ is the scaled error for analyst *i*'s *n*th forecast for firm *j*'s fiscal year-end EPS. It is calculated as the absolute difference between his forecast and the actual EPS released, scaled by the book value of equity per share of the firm at the previous fiscal year end.

AA dummy is 1 if analyst *i* is an All-American on the forecast date and 0 otherwise. The variable ln(distance) is the natural log of the difference between the forecast-period-end date and the forecast date in days. Firm size is the natural log of the firm's market capitalization of equity at the calendar-year end in millions of dollars. Leverage is the debt/asset ratio at the fiscal-year end. Volatility is the residual standard deviation of the firm's stock return against the market (CRSP Value-weighted Index) in the 120-day period prior to the forecast date. Firm fixed effects are included in the estimation. The Huber/White/sandwich estimator of variance is used for each of the annual regressions. Average R^2 and numbers of observations from the 20 regressions are reported. ***, **, and * denote that the coefficient is statistically significantly different from zero at the 1, 5, and 10 percent levels, respectively.

	All		**Top-Tier Banks**		**Lower-Status Banks**	
	Estimate	t-stat	Estimate	t-stat	Estimate	t-stat
AA dummy	-0.0020	-3.8154***	-0.0010	-1.5629	-0.0022	-3.7144***
ln(distance)	0.0159	10.1631***	0.0168	8.5423***	0.0156	10.4861***
Firm Size	-0.0390	-3.3571***	-0.0463	-2.9623***	-0.0379	-3.4171***
Leverage	0.0615	1.9093*	0.1325	2.3522**	0.0429	1.1678
Volatility	-0.4193	-1.5652	-0.1765	-0.5767	-0.4912	-1.6111
Firm Fixed Effects	Yes		Yes		Yes	
Average R^2	0.76		0.77		0.78	
Average N	38,620		10,017		28,603	

Table VII: Effect of Bank Status on Analyst Forecast Accuracy

This table presents the aggregated result from the 20 annual Fama-MacBeth regressions of analysts' forecast errors on the bank status and the other control variables. The first vertical panel presents the results of the whole sample. The latter two panels are for the AA and non-AA sub-sample, respectively. The reported coefficients are the averages from the 20 annual regressions, and the t-statistics are computed from the empirical distributions of the coefficient estimates.

For each fiscal year from 1983-2002, the following regression is estimated:

$$\begin{aligned} &\text{Error}_{i,j,n} = \alpha + \text{TopTier}_{i}\beta_1 + \ln(\text{distance})_{i,j,n}\beta_2 + \text{Firm Size}_{j}\beta_3 + \\ &\text{Leverage}_{j}\beta_4 + \text{Volatility}_{j}\beta_6 + \text{Firm Fixed Effects}_{j}\beta_j + \varepsilon_{i,j,n} \end{aligned} . \quad (3)$$

The dependent variable $Error_{i.j,n}$ is the scaled error for analyst *i*'s *n*th forecast for firm *j*'s fiscal year-end EPS. It is calculated as the absolute difference between his forecast and the actual EPS released, scaled by the book value of equity per share of the firm at the previous fiscal year end.

Top-tier bank dummy is 1 if analyst *i* works at one of the 10 top-tier banks as identified in the text pm the forecast date, and 0 otherwise. The variable ln(distance) is the natural log of the difference between the forecast-period-end date and the forecast date in days. Firm size is the natural log of the firm's market capitalization of equity at the calendar-year end in millions of dollars. Leverage is the debt/asset ratio at the fiscal-year end. Volatility is the residual standard deviation of the firm's stock return against the market (CRSP value-weighted index) in the 120-day period prior to the forecast date. Firm fixed effects are included in the estimation. The Huber/White/sandwich estimator of variance is used for each of the annual regressions. Average R^2 and numbers of observations from the 20 regressions are reported. ***, **, and * denote that the coefficient is statistically significantly different from zero at the 1, 5, and 10 percent levels, respectively.

	All		**AA**		**Non AA**	
	Estimate	t-stat	Estimate	t-stat	Estimate	t-stat
Top-Tier Bank	-0.0016	-4.3575***	0.0001	0.1512	-0.0017	-4.2341***
ln(distance)	0.0159	10.1864***	0.0165	7.7780***	0.0157	10.2168***
Firm Size	-0.0390	-3.3562***	-0.0341	-2.7224***	-0.0408	-3.3136***
Leverage	0.0615	1.9078*	0.1138	2.4510**	0.0558	1.6260
Volatility	-0.4181	-1.5606	-0.0625	-0.1564	-0.4966	-1.7146*
Firm Fixed Effects	Yes		Yes		Yes	
Average R^2	0.76		0.78		0.78	
Average N	38,620		7,334		31,286	

Table VIII: Effect of Bank Status and Underwriting Pressure on Analyst Forecast Accuracy

This table presents the results of the multivariate regression of analysts' scaled forecast errors on the bank status, an interaction term between bank status and (log of) market level IPO volume, and the other control variables. The first vertical panel presents the results of the whole sample. The latter two panels are for the AA and non-AA sub-sample, respectively. The specification is as follows:

$$\begin{aligned}&\text{Error}_{i,j,t,n} = \alpha + \text{TopTier}_{i,t}\beta_1 + (\text{TopTier}_{i,t} * \ln(IPOVolume)_t)\beta_2 + \\ &+ \ln(\text{distance})_{i,j,t,n}\beta_3 + \text{Firm Size}_{j,t}\,\beta_4 + \text{Leverage}_{j,t}\beta_5 + \\ &\text{Volatility}_{j,t}\beta_7 + \text{Year Dummies}_t\beta_{\text{year}} + \text{Firm Fixed Effects}_j\beta_j + \varepsilon_{i,j,t,n}\end{aligned} \tag{4}$$

The dependent variable $Error_{i,j,n,t}$ is the scaled error for analyst *i*'s *n*th forecast for firm *j's* annual EPS for fiscal year *t*. The top-tier bank dummy is 1 if analyst *i* works at one of the 10 top-tier banks as identified in the text on the forecast date, and 0 otherwise. The variable ln(IPOVolume) is the natural log of the annual total IPO issue volume in billions of (1990 real) dollars. The top-tier* ln(IPOVolume) is the interaction term. The variable ln(distance) is the natural log of the difference between the forecast-period-end date and the forecast date in days. Firm size is the natural log of the firm's market capitalization of equity at the calendar-year end in millions of dollars.. Leverage is the debt/asset ratio at the fiscal-year end. Volatility is the residual standard deviation of the firm's stock price against the market (CRSP value-weighed index) in the 120-day period prior to the forecast date. Year dummies refer to dummy variables for calendar years of the estimates. Firm fixed effects are included in the estimation. Point estimates for the year dummies and firm fixed effects are not reported, although they are included in the estimation. ***, **, and * denote that the coefficient is statistically significantly different from zero at the 1, 5, and 10 percent levels, respectively. The Huber/White/sandwich estimator of variance is used for the coefficient estimates.

	All		**AA**		**Non AA**	
	Estimate	t-stat	Estimate	t-stat	Estimate	t-stat
Top-Tier-bank dummy	0.0003	0.13	0.0155	2.92***	-0.0101	-4.50***
Top-Tier * ln(IPOVolume)	-0.0004	-0.49	-0.0043	-2.56**	0.0026	3.19***
ln(distance)	0.0166	61.05***	0.0162	29.99***	0.0166	53.48***
Firm Size	-0.0118	-12.68***	-0.0055	-4.74***	-0.0131	-11.78***
Leverage	0.1474	11.97***	0.1625	7.68***	0.1447	10.02***
Volatility	1.4080	14.83***	1.9222	9.24***	1.3231	12.17***
Constant	-0.0198	-3.12***	-0.0672	-6.39***	-0.0098	-1.36
Year Dummies	Yes		Yes		Yes	
Firm Fixed Effects	Yes		Yes		Yes	
R^2	0.24		0.23		0.25	
N	772,403		146,688		625,715	

Table IX: Effect of Analyst Reputation and Underwriting Pressure on Analyst Forecast Accuracy

This table presents the results of the multivariate regression of analysts' scaled forecast errors on the analysts' AA status, an interaction term between the AA status and (log of) market level IPO volume, and the other control variables. The first vertical panel presents the results of the all sample. The latter two panels are for the top-tier and lower-status banks, respectively. The specification is as follows:

$$\begin{aligned}&\text{Error}_{i,j,t,n} = \alpha + \text{AA}_{i,t}\beta_1 + (\text{AA}_{i,t} * \ln(IPOVolume)_t)\beta_2 \\ &+ \ln(\text{distance})_{i,j,t,n}\beta_3 + \text{Firm Size}_{j,t}\ \beta_4 + \text{Leverage}_{j,t}\beta_5 + \\ &\text{Volatility}_{j,t}\beta_7 + \text{Year}_t\ \beta_{year} + \text{Firm Fixed Effects}_j\beta_j + \varepsilon_{i,j,t,n}\end{aligned} \quad . \tag{5}$$

The dependent variable $Error_{i,j,t,n}$ is the scaled forecast error for analyst *i'*s *n*th forecast for firm *j'*s annual EPS for fiscal year *t.* The AA dummy is 1 if analyst *i* is an All-American on the forecast date, and 0 otherwise. The variable ln(IPOVolume) is the natural log of the annual total IPO issue volume in billion of (1990 real) dollars. The variable AA* ln(IPOVolume) is the interaction term. The variable ln(distance) is the natural log of the difference between the forecast-period-end date and the forecast date in days. Firm size is the natural log of the firm's market capitalization of equity at the calendar-year end in millions of dollars. Leverage is the debt/asset ratio at the fiscal-year end. NYSE-listed dummy is 1 if firm *j* is listed on the New York Stock Exchange, and 0 otherwise. Volatility is the residual standard deviation of the firm's stock return against the market (CRSP value-weighted index) in the 120-day period prior to the forecast date. Year dummies refer to dummy variables for calendar years of the estimates. Firm fixed effects are included in the estimation. Point estimates for the year dummies and firm fixed effects are not reported, although they are included in the estimation. ***, **, and * denote that the coefficient is statistically significantly different from zero at the 1, 5, and 10 percent levels, respectively. The Huber/White/sandwich estimator of variance is used for the coefficient estimates.

	All		Top-Tier Banks		Lower-Status Banks	
Variable	Estimate	t-stat	Estimate	t-stat	Estimate	t-stat
AA	0.0074	2.57**	0.0225	4.98***	-0.0037	-1.15
AA * ln(IPOVolume)	-0.0026	-2.87***	-0.0068	-4.69***	0.0004	0.37
ln(distance)	0.0166	61.05***	0.0176	37.50***	0.0162	49.79***
Firm Size	-0.0118	-12.73***	-0.0113	-11.77***	-0.0119	-9.89***
Leverage	0.1474	11.97***	0.1823	9.46***	0.1379	9.01***
Volatility	1.4079	14.83***	1.6735	10.59***	1.3146	12.30***
Constant	-0.0198	-3.14***	-0.0400	-4.66***	-0.0133	-1.68*
Year Dummies	Yes		Yes		Yes	
Firm Fixed Effects	Yes		Yes		Yes	
R^2	0.24		0.27		0.25	
N	772,403		200,349		572,054	

Table XI. Relative Accuracy of Existing and Non-Existing Top-tier Non-AAs: Uni-variate Tests

This table reports uni-variate test results for the relative accuracy of different types of top-tier non-AA analysts. Each year, the entire pool of top-tier non-AAs is subdivided into three categories: Existing, Transfers, and New Hires. An Existing top-tier non-AA is an analyst who is in the top-tier non-AA pool in the current year as well as in the previous year. The remaining top-tier non-AAs are considered Non-Existing, and they are further divided into Transfers and New Hires. A Transfer is a top-tier non-AA analyst in the current year, who was in the forecast database but was not in the top-tier non-AA pool last year. The remaining top-tier non-AAs are considered New Hires. Thus, these are the analysts in the top-tier non-AA pool for the current year but who were not in the forecast database the previous year. The average forecast errors in each group, and t-statistics for differences between groups are reported. Peak years consist of 1986, 1993, 1996, and 1999. (See Section 6 for details of how peak years are identified).

Year	Existing versus Non-Existing			Transfers versus New Hires		
	Non- Existing	Existing	t-stat	Transfers	New Hires	t-stat
1984	0.03	0.04	-4.52	0.02	0.03	-1.61
1985	0.03	0.04	-6.06	0.02	0.03	-2.22
1986	0.04	0.05	-0.84	0.05	0.04	0.57
1987	0.03	0.04	-3.93	0.03	0.03	-0.18
1988	0.05	0.03	1.01	0.03	0.05	-1.48
1989	0.04	0.04	-1.00	0.06	0.03	2.85
1990	0.04	0.05	-2.45	0.03	0.04	-2.77
1991	0.03	0.03	0.26	0.02	0.04	-2.96
1992	0.02	0.04	-5.07	0.02	0.02	-1.24
1993	0.02	0.04	-4.64	0.01	0.02	-3.42
1994	0.02	0.03	-3.36	0.03	0.02	0.90
1995	0.03	0.04	-2.09	0.02	0.04	-3.47
1996	0.03	0.04	-5.81	0.03	0.03	0.45
1997	0.03	0.04	-1.25	0.03	0.03	0.54
1998	0.04	0.05	-2.63	0.03	0.04	-1.20
1999	0.07	0.06	0.83	0.05	0.08	-1.55
2000	0.06	0.06	-1.48	0.06	0.06	0.17
2001	0.04	0.06	-4.13	0.04	0.04	-0.20
2002	0.03	0.03	-1.58	0.02	0.03	-2.77
All years	0.04	0.04	-7.67	0.03	0.04	-2.34
Peak Years Only	0.04	0.05	-2.53	0.03	0.04	-1.38

Table XII. Relative Accuracy of Existing and Non-Existing Top-Tier Non-AAs: Multivariate Tests

This table reports multivariate tests results for the relative accuracy of different types of top-tier non-AA analysts. The specification is the following:

$$Error_{i,j,t,n} = \alpha + Type_{i,t}\beta_1 + \ln(\text{distance})_{i,j,t,n}\beta_2 + Firm\ Size_{j,t}\beta_3 + Leverage_{j,t}\beta_4 + Volatility_{j,t}\beta_5 + Year_t\beta_{year} + Firm\ Fixed\ Effects_j\beta_j + \varepsilon_{i,j,t,n}$$

The dependent variable $Error_{i,j,t,n}$ is the scaled forecast error for analyst *i*'s *n*th forecast for firm *j*'s annual EPS for fiscal year *t*. The indicator variable Type indicates which subcategory a top-tier non-AA analyst belongs to. Each year, the entire pool of top-tier non-AAs is subdivided into three categories: Existing, Transfers, and New Hires. An Existing top-tier non-AA is an analyst who is in the top-tier non-AA pool in the current year as well as in the previous year. The remaining top-tier non-AAs are considered Non-Existing, and they are further divided into Transfers and New Hires. A Transfer is a top-tier non-AA analyst in the current year, who was in the forecast database but was not in the top-tier non-AA pool last year. The remaining top-tier non-AAs are considered New Hires. The variable ln(distance) is the natural log of the difference between the forecast-period-end date and the forecast date in days. Firm size is the natural log of the firm's market capitalization of equity at the calendar-year end in millions of dollars. Leverage is the debt/asset ratio at the fiscal-year end. Volatility is the residual standard deviation of the firm's stock return against the market (CRSP value-weighted index) in the 120-day period prior to the forecast date. Year dummies and firm fixed effects are included in the estimation. ***, **, and * denote that the coefficient is statistically significantly different from zero at the 1, 5, and 10 percent levels, respectively. The Huber/White/sandwich estimator of variance is used for the coefficient estimates. Panel B reports the estimation results for peak IPO years only. Peak years consist of 1986, 1993, 1996, and 1999. (See Section 6 for details of how peak years are identified).

Panel A: All Years

Variable	Estimate	t-stat	Estimate	t-stat
Existing	0.0041	3.59***	--	--
Transfer	--	--	-0.0060	-3.13***
New Hire	--	--	-0.0033	-2.61**
ln(distance)	0.0160	38.84***	0.0160	38.49***
Firm Size	-0.0127	-13.14***	-0.0127	-13.13***
Leverage	0.0995	9.18***	0.0995	9.18***
Volatility	0.6682	8.94***	0.6678	8.93***
Constant	0.0142	2.23**	0.0203	3.13***
Year Dummies	Yes		Yes	
Firm Fixed Effects	Yes		Yes	
R^2	0.50		0.50	
N	114058		114058	

Panel B: Peak Years Only				
Variable	Estimate	t-stat	Estimate	t-stat
Existing	0.0024	0.78	--	--
Transfer	--	--	-0.0063	-1.03
New Hire	--	--	-0.0007	-0.22
ln(distance)	0.0132	14.1***	0.0132	14.04***
Firm Size	-0.0196	-5.41***	-0.0196	-5.41***
Leverage	0.0888	3.17***	0.0885	3.17***
Volatility	1.2971	5.15***	1.2987	5.15***
Constant	0.0755	2.86***	0.0776	2.97***
Year Dummies	Yes		Yes	
Firm Fixed Effects	Yes		Yes	
R^2	0.81		0.81	
N	20988		20988	

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