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Analyst Reputation, Underwriting Pressure 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.

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1. Introduction

Earnings forecasts made by security analysts have been scrutinized by researchers in both the finance and accounting literature for at least a decade. Arguably, however, this strand of research has never been more relevant than today, in the post-bubble era, after shocking cases of conflict of interest involving analyst research have grabbed headlines and implicated numerous once- "star" analysts, and policy makers scrambled to draft new rules and regulations to enforce the integrity of analyst research. In this paper, we re-visit this old topic from a new angle, by asking two relatively unexplored and yet important questions regarding analyst research. We investigate whether the personal reputation of an analyst has a mitigating role in alleviating the potential conflict of interest in terms of producing more accurate forecasts, and to what extent this mitigating role is compromised when the pressure for generating underwriting revenue, which is the very source of the conflict of interest in question, is high.

In an ideal world where moral standard is high, "integrity" alone may suffice to ensure that personal-reputation concerns provide enough discipline to highly-regarded "star" analysts in producing truthful forecasts. Economists, however, tend to be a bit too cynical to accept such a fuzzy term as "integrity" as the theoretical foundation of the disciplinary role of reputation (and in fact, this cynicism seems *ex-post* well-justified by the recent scandals involving a few high-profile analysts). This skepticism notwithstanding, there is still sufficient reason to believe that *ex-ante* personal reputation can play an important role in alleviating the conflict of interest in analyst research. The very simple reason is that analysts' pay is directly related to external reputation indicators such as the powerful *Institutional Investor*'s All-American Analyst ranking, and such ranking is at least in part based on the accuracy of the analyst's forecasts.[2]

So the analyst faces two conflicting incentives, both due to his compensation scheme. On the one hand, his pay is increasing in external reputation, which is in turn increasing in his forecast accuracy. This provides an incentive for him to be as accurate as possible in his forecasts. On the other hand, however, as the press has made well known in recent events, his pay is also increasing in the amount of underwriting revenue that he helps generate for the investment bank that employs him. This, for various reasons, provides an incentive for him to bias upward his earnings forecasts for potential clients, i.e., to exhibit over-optimism to his covered firms. How these two incentives interact is an important and relatively unanswered question and is the focus of this paper.

As mentioned, there is a large existing literature concerning analyst forecast accuracy. Our paper bridges two existing strands of largely unrelated research: the strand that studies the effect of analysts' reputation, and the other that examines the conflict of interest problem.

In an early paper, Stickel (1992) documents that star analysts (those with the *Institutional Investor*'s All-American title) produce more accurate forecasts than their untitled peers. He concludes that there is a positive relationship in reputation and performance, and hence pay and performance. Our study relates to Stickel's work first in providing an update of the results, and more importantly in adding an inter-temporal dimension to the reputation-accuracy relationship. By looking at this relationship through the booms and busts of the underwriting cycle, we provide a richer picture that

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

allows us to infer the interaction between the “reputation effect” and the “underwriting pressure effect”.

The potential conflict of interest in analyst research, though only a recent buzzword in the popular press, has been documented quite extensively in the academic literature. Dugar and Nathan (1995), Lin and McNichols (1998), and Dechow, Hutton and Sloan (2000), among others, find that underwriter-bank or otherwise affiliated analysts are more positively biased than others.[3] In an influential study, Michaely and Womack (1999) look at the trading recommendations made by analysts working at the lead underwriter of an IPO versus those in other banks and find that underwriter analysts are more positively biased and thus their trading recommendations less profitable. The common theme emerging from all this research is that conflict of interest exists. In general, affiliated analysts are more positively biased about their covered firms than unaffiliated analysts; brokerage analysts more biased than research firm analysts[4]; and lead-underwriter analysts more biased than other analysts.

On the question of whether analysts are rewarded for biasing their estimates, Hong and Kubik (2003) find that while analysts are rewarded for being relatively accurate, once controlling for accuracy they are rewarded for optimism as well. They also report that the optimism result is stronger for affiliated analysts.[5]

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[3] 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 particularly among affiliated analysts. Ljungqvist, Marston, and Wilhelm (2003), 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 the firms are more aggressive in recommending the stocks.

[4] See Carleton, Chen and Steiner (1998).

[5] They also report that for analysts who are non All-Americans, scoring in the bottom 10 percent of the

Although outright denial of conflict of interest is hard to stand in light of such mounting evidence, the prior results still beg the question of whether the pervasive optimism is a result of (behavioral) human nature, or, more importantly, the fact that coverage and underwriting decisions are endogenous. After all, studies have found that optimism also exists among non-affiliated and non-broker analysts.[6] That leaves the possibility that banks and analysts tend to cover and (are chosen to) underwrite firms for which they have a favorable view, which then explain why they are more biased.

Our paper contributes to the conflict of interest literature in two ways. First, we bring in the personal reputation dimension and investigate how it interacts with, and whether is helps containing, the type of conflict of interest induced by the institutional setup that has been extensively studied. Secondly, we attempt to avoid the endogeneity issue by examining more exogenous forces of underwriting pressure, i.e., by looking at the overall, market-level underwriting activities through the peaks and troughs of the underwriting cycles.

Our findings suggest that personal reputation of security analysts is very important in determining forecast accuracy. First, we find that All-American analysts are significantly more accurate than non-All-Americans. This result is stronger among analysts working at lower-status banks. The results suggest a positive relationship between analyst reputation and performance.

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accuracy distribution increases an analyst's chances of moving down the bank hierarchy by 5.2 percentage points. For All-Americans, the effect is only 1.6 percent, suggesting that poor performance matters less for All-Americans. Note this result does *not* imply that All-Americans are less accurate than non-All-Americans at top-tier banks.

[6] 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.

Analysts that work at top-tier banks whose wages are reported to be substantially higher than those at lower-status banks generally perform significantly better than those analysts employed at lower-status banks. This suggests that the overall labor market for analysts is efficient, i.e., there is a positive relationship between pay and performance. This is particularly true of non-All-Americans; in contrast, among All-Americans, there is no significant difference in accuracy among top-tier-bank analysts and lower-status-bank analysts, once various firm and forecast characteristics are controlled for.

Assuming that the payoff from and hence the pressure to generating underwriting business is strongest in top-tier investment banks (large underwriters) and in boom underwriting periods, conflict of interest does seem to exist. Forecast accuracy generally drops among non-All-American analysts employed at top-tier banks in boom years. This finding strongly supports the conflict of interest hypothesis, and is difficult to reconcile with the behavioral hypothesis.

But the most interesting finding of all is that personal reputation does seem to have a mitigating role for the conflict of interest. In peak underwriting years, it is those non-star analysts working at top-tier investment banks whose forecast accuracy is most dramatically compromised. Among analysts working at top-tier banks, star analysts become significantly more accurate relative to their non-star colleagues in peak years. We argue that, without personal reputation at stake, non-star analysts at top-tier banks have all to gain and little to lose by becoming more biased in hot markets. In contrast, star analysts do have a personal reputation and its associated long-term gains to lose. This concern counters the temptation of being overly optimistic and taking the short-term monetary gain (most likely in the form of year-end bonuses), and curbs the degree of bias among these star analysts in peak underwriting years.

The rest of the paper is organized as follows. Section 2 lays out more precisely our hypothesis and testable implications. Section 3 describes data and methodology. Section 4 and 5 presents uni-variate and multivariate analyses, respectively. Section 6 concludes.

2. Analysts' Incentives and the Determinants of Forecast Accuracy

In order to understand the different incentives facing an analyst, it is useful to go over some of the institutional details of how analysts are evaluated and compensated. Ultimately, sell-side analysts are evaluated by buy-side managers. The magazine *Institutional Investor* plays an important role in this process. It undertakes a survey among a large, comprehensive sample of buy-side managers, asking them to evaluate analysts from whom they receive reports along the following four dimensions: stock picking, earnings forecasts, written reports, and overall service.[7] The result of this survey leads to the annual election of the so-called "All-American" analysts (henceforth AA), which is prominently featured in the October issues of the magazine every year. The issue not only lists the award-winning analysts' names, it also publishes analysts' photographs and editorials that highlight the contributions and merits of the winning analysts, *from the buy-side investors' point of view*. In a way, the All-American award is the Oscars of sell-side research.

[7] For the 2002 All-American rankings, the survey was sent to more than 900 institutions. They collected the survey response from more than 3,500 individuals at about 600 firms, which include more than 90 percent of the 100 largest U.S. equity managers. (*Institutional Investor*, October 2002)

In addition to prestige, the AA title is accompanied by monetary reward. Not only do AAs earn substantially higher salary than non-AAs, there is a plethora of anecdotal evidence that AAs, especially veteran, consecutive winners, exercise significant power and clout within the investment banks and among their clients, for the simple reason that their affiliation is coveted by rival banks. But because the evaluation ultimately comes from the buy-side managers, and because buy-side managers are ultimately concerned with the profitability of the analysts' advice, reputation and career concerns suggest that an analyst will want to be as accurate as possible and that an AA analyst will want to stay as accurate as possible.[8]

Furthermore, to the extent that taking up a prominent money-manager position on the buy-side is often considered an ultimate reward for successful sell-side analysts, this long-horizon goal incentivizes the analysts to resist biasing their forecasts upward, because doing so will hurt their relations with investors *ex post*.

However, as the press has made it well-known, analysts are also compensated (mostly in the form of year-end bonuses) for the underwriting business that they help to generate. 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 upward the earnings forecasts, i.e., to exhibit over-optimism for covered companies.

Therefore there are two conflicting incentives that affect the analysts' forecast accuracy. There is the positive incentive due to reputation and the negative incentive due to underwriting pressure. Our study attempts to shed light on how these two opposing

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[8] Stickel (1992) reports evidence that lower relative accuracy leads to removal from the AA team. The finding supports the view that AAs are incentivised to remain accurate.

forces interact in determining forecast accuracy. This is an important empirical question and yet to the best of our knowledge has been relatively overlooked in the literature.

Because of the prominence of the AA designation, using the AA status to proxy personal reputation is a natural choice. In devising empirical proxies for underwriting pressure, we observe that underwriting-related compensation usually comes in the form of year-end bonuses. It is therefore conceivable that the reward from, and hence the short-term temptation to, becoming over-optimistic is highest during underwriting booms and in top-tier investment banks (large underwriters). This suggests that both bank status and underwriting cycle are potential proxies for underwriting pressure.

While the effect of the underwriting boom is clear, the effect of bank status is ambiguous. On the one hand, the lure of year-end bonuses is high at top-tier banks, and perhaps then so is the susceptibility of analysts to conflict of interest. On the other hand, working at a top-tier bank is a higher-paid and more prestigious job, and in the absence of conflict of interest, higher pay should be correlated with better performance (accuracy). Therefore the overall effect of bank status *alone* becomes an empirical question. However, when interacted with underwriting pressure, the bank status variable serves as a very informative inference variable for detecting conflict of interest.

Having laid out our empirical proxies for the opposing forces, we now list the sequence of empirical questions we address in this paper:

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

2. *Is bank status (top-tier vs. lower-status) important in determining the 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 question is concerned with the effect of personal reputation alone. The second and third questions address the effect of underwriting pressure. The last question examines the interaction between the two and allows us to infer whether personal reputation has a mitigating role for the conflict of interest problem.

3. Data and Methodology

3.1. Data Sources and Descriptive Statistics

Our data is compiled from several sources. Details on analyst forecasts, including company, analyst and broker codes, the forecast period, the EPS estimate and the actual EPS eventually reported, are obtained from the I/B/E/S Detailed History file. Since comprehensive data coverage by I/B/E/S starts in 1983, our sample period is from 1983 to 2002. I/B/E/S collects forecasts for multiple horizons; for example, quarter-end, semi-annual, fiscal-year-end, and two-year forecasts are all covered. To ensure consistency in our comparisons, in this study we focus on the fiscal-year-end forecasts only. Not only is this consistent with previous research, but also it is the horizon for which data coverage is most comprehensive over the entire sample period.

Analysts' All-American status is obtained from the October issues of the *Institutional Investor* (*II*) for each year in the sample. For confidentiality reasons, I/B/E/S uses a numeric code to identify each broker and analyst in the details file. The actual names of these entities and personnel are available upon special request in a separate "translation" file. We went through a careful process to match the AA names from the *II* listings with the names in the translation file, and were able to match 1,240 out of the entire sample of 1,376 distinctive AA analysts for the 20 years.

On the premise that the lure of the year-end underwriting-related bonuses (i.e., the potential for conflict of interest) is high in top-tier investment banks and during peak underwriting periods, bank status and underwriting volume can be viewed as two proxies of underwriting pressure. For bank status, we identify the 10 underwriters with the highest (updated) 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 business and are often dubbed "bulge-bracket" firms in the financial press.

To measure underwriting activity, we use the overall market-level IPO volume for a given year. This information is compiled from SDC Platinum. It should be pointed out that using market rather than bank-specific underwriting volume is a deliberate choice. The reason is that potentially there is a high degree of 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. Indeed this is at the very heart of the conflict of interest allegation. It would be difficult to distinguish this possibility from the alternative that it is the underwriting pressure that causes the analysts to be inaccurate in boom years. By using overall market level data, this reverse causality problem is at least partially alleviated.

Our last data source is the Compustat and CRSP databases, where we obtain firm characteristics and stock price information in order to perform some of the multivariate analyses.

Table I tabulates the number of firms, analysts, and forecasts in each year of the sample. The number of firms covered generally grew 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. Majority 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 is relatively stable, increasing from 232 in 1983 to 316 in 2002. 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 get 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 reports generated.[9] In fact, the former is roughly twice as large as the latter. For instance, in 1997, the Non-AA to AA head count ratio is 12.53, but the ratio between reports generated by Non-AAs and those generated by AAs is only 5.93. This indicates that AAs tend to issue more forecasts per year than non-AAs.

Further corroborating this evidence, Table II provides more statistics on the working patterns of AAs versus non-AAs. Statistics in this table shows that AAs provide

[9] 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 to be made by an AA.

coverage to more firms, and for each firm covered, they also issue more frequent earnings forecasts than Non-AAs. The statistics are highly significant for all years. This is consistent with previous evidence in Stickel (1992).

Table III compares select firm characteristics for firms covered by AAs versus those covered by non-AAs. Patterns in this table are quite striking. AAs cover firms that are significantly larger in terms of market capitalization (the 1st vertical panel of the table) and less risky in terms of having lower stock return volatility (the 4th vertical panel of the table). In addition, AA covered firms are more likely to be listed on the NYSE (the 3rd vertical panel). Interpreting all three variables as information proxies, it appears that firms covered by AAs have more public information available. Since such firm characteristics could contribute to higher accuracy among AAs, it is therefore important to control for these additional 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 of the table). One the one hand, higher leverage could indicate higher risk; on the other hand, it could also indicate more stable cash-flows and hence higher debt capacity. Therefore a priori it is difficult to determine the effect of leverage on forecast accuracy, and the sign on the leverage variable in a multiple regression becomes an empirical question.

One final remark on Table III is to note that the fraction of firms covered 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 of the table). This indicates that coverage is added, i.e., more information is produced, for smaller firms over time.

3.2. Empirical Methods

The key variable of interest, analyst forecast accuracy, is defined 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 that he covers, "*t*" is the forecast period, i.e. the fiscal year, and "*n*" denotes the nth forecast the analyst issues for the firm for that forecast period.

We use the absolute error rather than the signed error measure because of the following reasons. On one hand, the conflict of interest consideration suggests that analysts are mainly tempted to bias their forecast upward, and thus we should be looking at positive (signed) bias in their forecasts. On the other hand, the personal reputation consideration suggests that analysts want to minimize their absolute errors (both positive and negative). Since we are interested in shedding light on how these two forces interact with each other, we use the absolute errors as the dependent variable that capture both considerations.

We scale this absolute error measure by the book value of equity per share of the firm at the previous fiscal year end. Scaling is crucial because heterogeneity across firms could cause heteroskedasticity in the residual terms of our regression analysis. For example, some firms have much larger earnings per share simply because they have much fewer shares. Comparing un-scaled forecast errors of such firms with un-scaled forecast errors of other firms with more conventional share numbers would be inappropriate. Scaling the absolute error measure with book value of equity per share partly alleviates this heteroskedasticity issue.[10]

Furthermore, we use the book value of equity rather than either the actual EPS reported or the market value of equity for the following reasons. As is well documented in the press and the literature, the recent years have witnessed a dramatic rise in corporate earnings and market stock prices in the late 90's followed by an equally dramatic decline in the post-bubble years of 2001 and 2002. Given this time-series pattern, using either earnings or stock prices as the scaling factor would result in making scaled errors during boom years appear artificially small relative to errors in trough years. Yet the allegation of tainted Wall Street research concerns manipulations of forecasts made during those boom years. Thus we use book value of equity which is much less correlated with booms and troughs of the financial markets.

Our empirical methodology is straightforward. There are two dimensions along which we want to study the forecast-error variable: personal reputation (AA versus Non-AA), and underwriting pressure (top-tier banks versus lower-status banks). We first perform t-tests on the forecast-error variable along each of these two dimensions. Results pertaining to this analysis are presented in the next section. In order to study the interaction between these two dimensions, as well as the inter-temporal variation in underwriting volume, in Section 5 we perform regression analyses on the forecast-error variable.

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[10] Other studies have similarly used scaling to justify homoskedasticity assumption of forecast errors in their analyses. For example, see Keand and Runkle (1999).

4. Uni-variate Analyses

We begin our analysis with a series of uni-variate t-tests. Table IV compares the forecast accuracy of AA analysts versus that of non-AA analysts. Since the AA status is our proxy for personal reputation, this table can be read as the uni-variate tests of the effect of personal reputation. The first vertical panel is the overall comparison; the latter two panels are for the 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, AA analysts are more accurate than non-AA analysts in the whole sample, as well as the lower-status-bank sub-sample. In the whole sample, the t-statistics on the differences between AAs' and non-AAs' average forecast errors are highly significant for twelve out of the twenty years. The pooled test using all twenty years of data 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. The "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 average margin by which the AAs in these banks are more accurate than the non-AAs is 0.68% (3.94%-4.62%), significantly larger than the difference in the overall sample.

Notably, however, the "AA effect" is much weaker in the top-tier-banks 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 could be the case 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 should be stressed that we do not interpret the higher accuracy among AAs as saying that the AA status causes the analysts to be more accurate, even though the comparison is based on forecasts made after the analysts obtain AA status. We only interpret the result as a positive correlation between personal reputation and forecast accuracy. In fact, since election to AA status most likely reflects past forecast accuracy,[11] the result does indicate that at least the AA status does not cause the analyst to lose accuracy.

Table V compares forecast accuracy along the bank-status dimension. The first vertical panel is for the whole sample, and the latter two are for 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 with analyst accuracy in the whole sample and the non-AA sub-sample is expected if the labor market for security analysts is reasonably efficient. Since jobs in top-tier banks are more competitive and higher paid,

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[11] As a preliminary 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, suggesting that past forecast error significantly reduces the chance of being elected as an AA.

analysts working in top-tier banks on average should be more skilled and select workers; to the extend that there is a positive pay-performance relationship, we would expect that top-tier-bank analysts are generally more accurate.

However these effects can be weaker or even reversed for AAs, since being an AA is itself an important distinction and to the extent that all AAs are already highly paid, our result indicates that conditional on being an AA, bank-status, or pay, does not increase accuracy. Indeed the fact that AAs in top-tier banks are *less* accurate than those in lower-status banks is consistent with the hypothesis that the culture to reward optimism and in so doing sacrifice accuracy is stronger among top-tier banks, and is a potential source for conflict of interest.[12] However uni-variate results here are not conclusive since it could also simply be that AAs in top-tier banks cover more complex firms. We shall revisit the bank-status effect below in multivariate analyses.

In summary, uni-variate results in this section show that AA analysts are unambiguously more accurate than non-AAs, suggesting that personal reputation is positively correlated with accuracy. The effect of bank status, however, is somewhat ambiguous. While in the whole sample, bank status is positively related to accuracy, the effect does not exist among AAs.

Since the uni-variate results are more descriptive than indicative, in the next section we turn to multivariate analyses that will allow us to make more robust inferences. In particular, we will focus on not only the separate effects of personal

[12] 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 their 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.

reputation and bank status, but also on how these factors interact with underwriting pressure, and thus shed light on the alleged conflict of interest in analyst research, and most importantly on whether personal reputation can mitigate this conflict of interest.

5. Multivariate Analyses

5.1. The Effect of Personal Repuation

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

To address these econometric issues, we adopt the cross-sectional regression approach developped by Fama and MacBeth (1973). In particular, 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. The advantage of this apporach is two-fold. Econometrically, estimating the regression equation annually addresses the potential serial correlation problem in the data. In addition, the time-series pattern of the coefficients on the AA indicator can be itself informative. In particular, the time series allows us to examine how the relative accuracy of the AAs (the coefficient on the AA dummy) changes through the peaks and troughs of the market, which sheds light on the interaction between personal-reputation and underwriting-pressure effects.

In equation (2), the dependant variable $Error_{i,j,n}$ is the scaled error of analyst *i's* *n*th forecast on firm *j*. The key variable of interest is AA_i, which is analyst *i*'s AA status at the time when the forecast is issued. $X_{i,j,n}$ is a list of controls that we consider to 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, firm leverage, and stock return volatility. In addition, to control for any other unobserved, firm-specific characteristics, firm fixed effects are also included in the estimation.[13]

Table VI reports the Fama-MacBeth regression results on equation (2). As in the uni-variate case, the first vertical panel presents the results for the whole sample and the latter two panels are for the top-tier and the lower-status-bank sub-samples, respectively. First, consistent with our uni-variate results and the personal reputation effect documented in Stickel (1992), we find that AAs are more accurate than non-AAs: in the whole sample, the coefficient on the AA dummy is negative and significant.

Also consistent with our uni-variate result, the reputation effect is strong in lower-status banks and weak in top-tier banks. In the top-tier bank sub-sample, although the coefficient on AA is still negative, it is not significantly different from zero. In terms of

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

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 then their non-star colleagues, AAs in top-tier banks are not.

There are two possible explanations. First, it may be that AAs at top-tier banks face more pressure to bias their estimates upward than AAs at lower-status banks. This would support the conflict of interest view. Alternatively, however, it could also be that analyst jobs at top-tier banks are better paid and more competitive, and so on average non-AAs at top-tier banks are more skilled and select workers than those at lower-status banks. As a result, being an AA is less of a distinction among the top-tier-bank sub-sample. This alternative would suggest a degree of efficiency in the analysts' labor market. We will revisit these two non-mutually-exclusive alternatives in the next sub-section where we study the effect of bank status in detail.

All the control variables in equation (2) have predicted signs. The positive coefficients on log(distance) indicates that the further away from the earnings dates are the estimates made, the less accurate they are. The negative coefficients on firm size suggests that average errors are smaller for larger firms, presumably because these firms are better covered and information is more readily available for them. High leverage is associated with larger errors, which suggests that rather than proxying for stable cash flows, high leverage implies high cash-flow risk. Higher volatility in stock prices indicates higher uncertainty, and not surprisingly, it is associated with higher forecast errors. In summary, regression results in this section confirm our findings from the uni-variate tests. We see that personal reputation has a positive effect on forecast accuracy, even after controlling for other variables.

5.2. The Effect of Bank Status

To examine the effect of bank status on analyst forecast accuracy, we use the same cross-sectional regression approach and estimate 20 annual regressions of the form:

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

and then aggregate the results as before. In equation (3), $TopTier_{i,t}$ is the key indicator dummy of interest, which equals 1 if and only if analyst *i* is employed at a top-tier bank at the time of forecast, and $X_{i,j,n}$ is the same set of control variables as before.

Estimation results of Equation (3) are summarized in Table VII. The first vertical panel is for the overall sample, and the latter two are for the AA and non-AA sub-samples, respectively. The key observation from this table is that while in the whole sample and the non-AA sub-sample the coefficient on the top-tier-bank indicator is negative and statistically significant, it is insignificant for the AA sub-sample. These results are largely consistent with the uni-variate results in Table V, and they indicate that although top-tier-bank analysts are generally more accurate than their lower-tier counterparts, this result comes from non-AA analysts. Conditioned 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. The overall positive relationship between bank status and analyst accuracy suggests that the labor market for security analysts is reasonably efficient. Since a job at a top-tier bank is more competitive and better paid, the negative coefficient on bank-status implies a positive relationship between analysts' pay and performance. This indeed holds strongly for the 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 of a distinction among the AA sub-sample. Notably, multivariate results here removes the previous uni-variate result (from 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 most likely due to the fact that AAs in top-tier banks cover different (and presumably more complex) firms than AAs in lower-status banks.

So far we have separately examined the effect of an analyst's personal reputation (Section 5.1) and the effect of his employment affiliation (Section 5.2). Our results are consistent with the notion that both personal reputation and bank status are effective screening devices in analysts' labor market: Analysts with the AA title are more accurate than those without, and analysts working at top-tier banks are more accurate than those in lower-status banks. The analysis, however, is static in nature and does not capture analysts' dynamic incentives that change over the booms and busts of the market cycle. Thus the results do not yet allow us to say much about either conflict of interest or the mitigating role of personal reputation (if any). In the next sub-sections we examine the interactions between the static measures of personal reputation and bank status and the dynamic measures of the underwriting-market environment in order to shed light on these 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 market years. Conflict of interest is expected to be more severe in top-tier banks because these banks have a strong tradition to reward analysts for the generation of underwriting business. But even within top-tier banks, the prospect of a large underwriting-related bonus is particularly high during boom years. Since issuing overly optimistic forecasts helps win underwriting contracts, the temptation to bias forecasts should then be the greatest under the combination of these conditions.

Empirically, 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.[14] In (4), $TopTier_{it}$ is again 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 list of controls, and *ln(IPOVolume)* is the log of 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, then we should expect to see a positive sign on the interaction term, 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-

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

AA sub-samples, respectively. First, we find that in the overall sample the coefficient on the interaction term between bank status and the IPO volume variable is not significantly different from zero. But this masks the interesting pattern revealed in the sub-sample panels: the interaction-term coefficient 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. This indicates that the accuracy of forecasts made by non-AA, top-tier-bank analysts drops in peak IPO years relative to those of non-AA, lower-status-bank analysts. This time-varying result is hard to explain with a behavioral hypothesis alone: 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.[15] Instead, assuming that underwriting pressure is the strongest at top-tier investment banks (large underwriters) *and* during boom new-issues-market years, this result strongly supports the view that conflict of interest exists.

In contrast to the above result, the coefficient on the interaction term is negative and significant for the AA sub-sample. This indicates that, unlike their non-AA peers, 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 status makes these star analysts immune to the conflict of interest

[15] 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 used a time-invariant definition of top-tier banks 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.

problem that seems to adversely affect the accuracy of non-AAs during boom years.[16] We will come back to this point in the next sub-section when we discuss the role of personal reputation in mitigating the potential conflict of interest.

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 most interesting question: how do these two forces interact? Specifically, does reputation have a mitigating role for the conflict of interest problem?

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

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

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 that we are most interested in. Since the previous sub-section shows that top-tier-bank analysts tend to become inaccurate during peak IPO years (which is consistent with conflict of interest), if personal reputation plays a role in mitigating the effects of conflict of interest, then we should expect to see a negative sign on the interaction term. In other words, if personal reputation partially alleviates the conflict of interest problem, then AAs should be relatively more accurate when the rest of the analysts are succumbing to the force of 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 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 to the conflict of interest problem that becomes particularly acute in top-tier banks in boom years.

This result is consistent with our earlier finding (from Table VIII) that accuracy of non-AA, top-tier-bank analysts go down (relative to those of non-AA, lower-status-bank analysts) during peak years. 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 for conflict of interest, then 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 because non-AA, top-tier-bank analysts become inaccurate during boom years, the conditions under which the conflict of interest problem 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 the above finding, we analyze the time-series pattern 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 against the corresponding IPO underwriting volume for the 20 years in our sample. A striking pattern emerges from the top-tier-bank sub-sample. Here we see that when the underwriting volumes are high, the coefficient on AA drops significantly (become more negative). In fact, the two lines almost move in exactly opposite directions. Examining the other plots, we find that this striking pattern is less pronounced in the lower-status-bank sub-sample and the overall sample. This is consistent with our conclusion that AAs become significantly more accurate in top-tier banks relative to their non-star colleagues during peak market 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 over-optimistic are presumably strongest in top-tier banks during hot 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, and in comparison, AAs become significantly more accurate than non-AAs in top-tier banks and during boom years, the conditions under which conflict of interest should be most acute. Since both bank status and market conditions are controlled for and the only variation is whether or not an analyst has an AA designation, we find it compelling to argue that the difference is driven by personal reputation. It seems that without personal reputation at stake, non-AA analysts working at top-tier banks have too little to lose and too much to gain from biasing their estimates in hot markets. By comparison, AAs working at the same top banks have more to lose and this incentive, 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 counter to the popular press depiction of “star” analysts in the recent years roiled in scandals and their reputation tarnished. We argue, based on our findings, that those “fallen” analysts who were also All-Americans were perhaps not representative of the AA group, but rather more representative of those analysts working at top-tier banks at the height of the market boom. And on average, it is those *non-star* analysts working at top-tier investment banks whose forecast accuracy is most dramatically compromised during peak years.

6. Conclusion

We examine how personal reputation, underwriting pressure, and most importantly 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 designation make significantly more accurate forecasts then those without the title.

We find that top-tier-bank analysts are generally more accurate than their lower-tier counterparts. This result derives mainly from non-AA analysts; in contrast, among AAs, those working at top-tier banks are not significantly more accurate than those working in lower-tier banks. The overall result is consistent with the efficiency of the labor market for analysts.

Importantly, we find that forecast accuracy drops among non-AA analysts employed at top-tier banks in boom years. Since the payoff from and the pressure to generate underwriting business is highest in top-tier banks and in peak underwriting periods, this finding provides compelling evidence for the existence of conflict of interest.

Our most interesting finding is regarding the interaction between personal reputation and the underwriting pressure. Here we find evidence that personal reputation does play a mitigating role in alleviating the conflict of interest problem. We find that it is strictly those non-star analysts working at these banks whose forecast accuracy suffers the most during such periods. Among top-tier-bank analysts, AAs become significantly more accurate during peak underwriting periods. We argue that without a personal reputation to lose, non-AAs have too much to gain and too little to lose by becoming over-optimistic in peak underwriting periods. The AAs, however, do have a personal reputation (and its associated long-term career benefits) to lose, and this concern effectively curbs the degree of bias in their earnings forecasts during hot underwriting markets.

Analyst research has historically attracted interest in the academic literature. But never before has this line of research been as relevant as it is today, after conflict of interest in sell-side research has grabbed national headlines and lawmakers scrambled to fix the “Chinese Wall” that is supposed to separate research from banking. The angle we take in this paper – examine the role of personal reputation and its interaction with external underwriting pressure – is timely and relevant against this backdrop.

What we have learned from this exercise suggests that there might be an alternative to the outright separation of investment banking and security research, and this alternative involves efficiently leveraging the disciplinary role of personal reputation. One possibility is to put more weight on personal reputation in determining the analyst compensation. To the extent that information production activities such as analyst research improves market efficiency, and yet such activities would not be undertaken without investment-bank subsidy, this alternative warrants consideration as an efficient way of resolving the conflict of interest problem in the current industrial organization of sell-side research.

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Figure 1. The AA effect vs. Underwriting Volume

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

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

against the underwriting volume. Figure 1.a is for the whole sample, and Figures 1.b and 1.c are for the top-tier and lower-status banks respectively.

Figure 1.a – All Sample

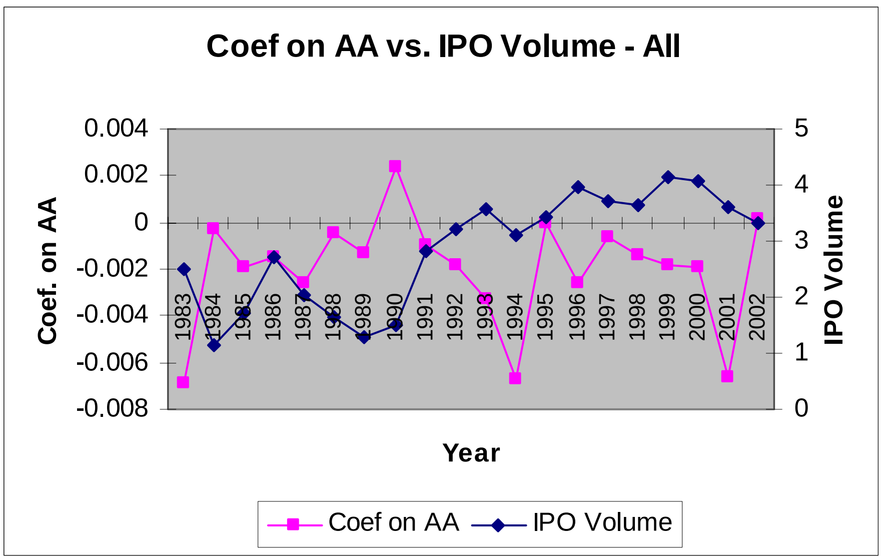

Figure 1.b Top-tier-bank Sub-sample

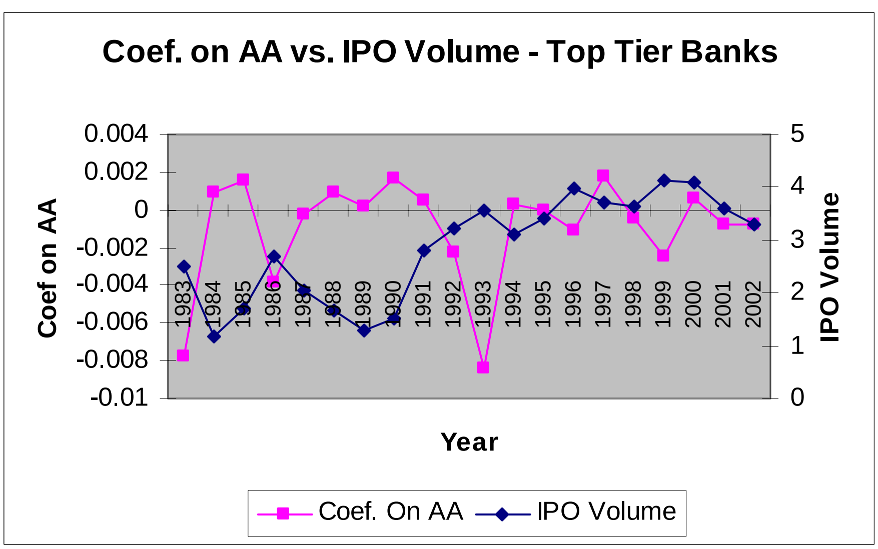

Figure 1.c. Lower-status-bank Sub-sample

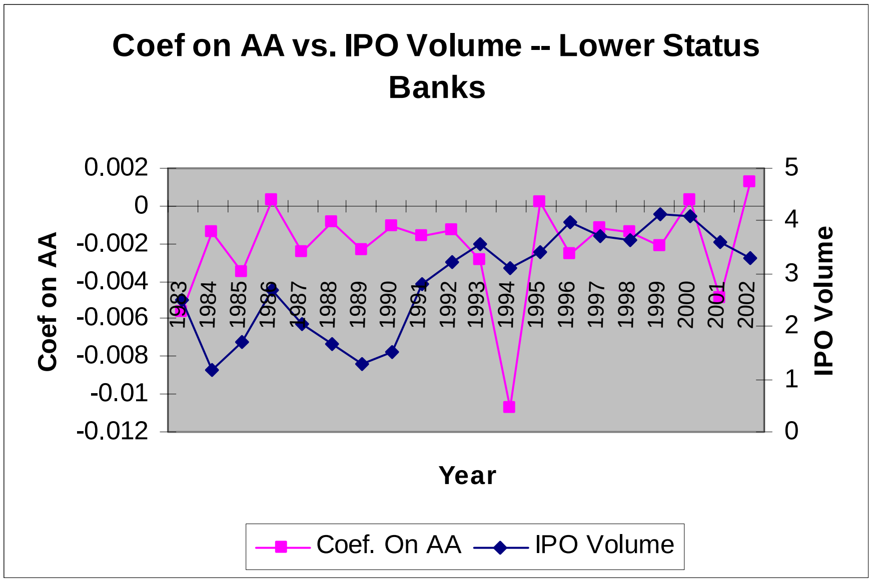

Table I. Sample Descriptive Statistics

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 October issues 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. 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. ***, **, * denotes 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 when the forecast is issued. ***, **, * denotes 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 and Manaster (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. ***, **, * denotes 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 all 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:

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

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 the analyst *i* is an All-American on the forecast date and 0 otherwise. Log 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. 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. ***, **, * denotes 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***
Log 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 all 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:

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

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. Log 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. 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. ***, **, * denotes 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***
Log 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 all sample. The latter two panels are for the AA and non-AA sub-sample, respectively. The specification is as follows:

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

The dependent variable $Error_{i,j,n,t}$ is the scaled error for analyst *i's nth* forecast for firm *j's* annual EPS for fiscal year *t*. 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. Log(IPO volume) is the natural log of the annual total IPO issue volume in $ billions (in real dollars). Top-tier bank dummy * log(IPO volume) is the interaction term. Log(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. 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 though they are included in the estimation. ***, **, * denotes 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 * IPO volume	-0.0004	-0.49	-0.0043	-2.56**	0.0026	3.19***
Log (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:

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

The dependent variable $Error_{i,j,t,n}$ is the scaled forecast error for analyst *i's nth* forecast for firm *j's* annual EPS for fiscal year *t*. AA dummy is 1 if the analyst *i* is an All-American on the forecast date, and 0 otherwise. Log(IPO volume) is the natural log of the annual total IPO issue volume in $ billions (in real dollars). AA dummy * log(IPO volume) is the interaction term. Log(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. 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 otherwise 0. 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 though they are included in the estimation. ***, **, * denotes 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 * log(IPO volume)	-0.0026	-2.87***	-0.0068	-4.69***	0.0004	0.37
Log (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	

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