Reputation as a Disciplinary Device in Sell-side Research

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Abstract

We study how personal reputation and bank reputation affect conflict of interest in sell-side research using the 1983-2002 U.S. data. There is a significant positive relation between reputation and research quality, both at the personal and at the institutional level. However, we find that relative accuracy of non-All-American analysts who work at reputable banks deteriorates during hot markets. In contrast, relative accuracy of All-Americans does not drop and in some cases increases during hot markets. These results suggest that personal reputation acts as an important disciplinary device in the presence of conflict of interest, whereas bank reputation alone is less effective.

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1. Introduction

Before the current scandals embroiling sell-side research broke out, few would challenge the notion that reputable analysts produced higher-quality research than their non-reputable counterparts. This view, however, has been turned on its head as both prominent analysts and their employer banks have been repeatedly charged with the abuse of public trust. At the individual level, some high-flying analysts such as Henry Blodget of Merrill Lynch and Jack Grubman of Salomon Smith Barney/Citigroup faced not only multi-million-dollar fines but also lifetime bars from the securities industry. At the institutional level, 10 largest investment banks entered into the Global Settlement with the regulators in April 2003, whereby they agreed to pay $1.4 billion in fines for publishing biased research. More than just being a triumph for the regulators, these highly-publicized events shaped a public opinion that sell-side analysts, and especially star analysts and analysts from big investment banks, are untrustworthy.

There is general consensus that conflict of interest does exist in Wall Street research.[2] But should it be more exacerbated for reputable analysts and banks as implied by these current events? Existing theory is mixed on this question. On the one hand, seminal articles such as Kreps and Wilson (1982), Diamond (1989, 1991) and Chemmanur and Fulghieri (1994) suggest that with repeated economic interactions, conflicts of interest between principals and agents may be mitigated by the agents' incentive to build long-term reputations. In the context of interactions between investors and analysts, analysts face a trade-off between a loss in long-term reputation and a gain in

[2] See Dugar et al. (1995) and Dechow et al. (2000), for example, for analyses of analyst affiliation and earnings forecasts. Michaely and Womack (1999), Lin et al. (1998), and Ljungqvist et al. (2005), among others, examine the effect of affiliation on stock recommendations. A fuller review of papers on conflict of interest appears in Section 2.

short-term benefits such as year-end bonuses.[3] The need to preserve his long-term reputation (and its associated benefits) implies that an analyst who has a greater reputation at stake refrains more from opportunistic short-term behavior that would damage his reputation. Likewise, a bank's need to preserve a good reputation may also help it better supervise the actions of individual analysts. On the other hand, this disciplinary role of reputation can break down. Sobel (1985), for instance, shows that in a dynamic game of strategic information transmission, there are situations where it pays an agent to cash in on his reputation. One opportune moment for a stock analyst to cash in his reputation might be during the peak of the internet bubble. Benabou and Laroque (1992) extend Sobel's model and show that when private signals are noisy, agents can hide their lies behind the pretence of honest mistakes. Since dishonesty can go unproven and unpunished for a prolonged period of time, the disciplinary role of reputation is limited.

These opposing theoretical predictions raise the following empirical questions: What is the role of reputation in the presence of conflict of interest? Specifically, did reputable individuals and banks exploit their credibility at the peak of the market to the detriment of investors? Or did they act less opportunistically relative to their non-reputable counterparts? Furthermore, do personal reputation and bank reputation act differently as disciplining devices? We investigate these questions by analyzing relative earnings forecasts quality by sell-side analysts with and without reputation across the peaks and troughs of stock market cycles.[4] By analyzing the *time-varying* patterns of the

[3] Morgan and Stocken (2003) and Jackson (2005), among others, analyze the problems of analysts faced with such trade-offs.
[4] In a related study, Fang and Yasuda (2005b) examine the effect of reputation on investment values of analyst recommendations.

research quality *differential*, we shed light on the effect of reputation on conflict of interest, the severity of which rises and falls with the market.

Several existing studies, notably Stickel (1992) and Jackson (2005), examine the links between personal reputation and forecast quality. Stickel (1992) shows that star analysts (those with the All-American title) produce more accurate forecasts than others. He concludes that there is a positive relation between reputation and performance, and hence pay and performance. Using Australian data, Jackson (2005) also finds that more accurate analysts acquire higher future reputations. An important difference between this paper and these studies is that while the existing papers predominantly address the static relation between reputation and research quality, we examine the dynamic relation, which allows us to shed light on the effect of reputation on conflict of interest. In addition, while these studies focus on personal reputation, we examine both personal reputation and institutional reputation, and the extent to which they work differently.

Existing literature also offers some evidence regarding the relation between bank reputation and research quality. Hong and Kubik (2003), for example, report that both accuracy and optimism have significantly positive effects on promotion of analysts from non-reputable banks to reputable banks. In contrast, Cowen et al. (2003) find that analysts working at reputable banks make less optimistic forecasts than others. Our analysis of bank reputation differs from these work again in that we focus on the time-varying effects of bank reputation.

To our best knowledge, this is the first paper that examines how personal and bank reputation affect conflict of interest in sell-side research. Do reputation mechanisms in place mitigate or exacerbate the conflict of interest problem? In the wake of the recent scandals implicating star analysts and prominent investment banks, examining whether these cases were norms or exceptions is a relevant economic question. In doing so, this paper contributes in a broader sense to the literature on the role and limits of reputation in agency-principal relationships.

The specific questions we set out to answer are the following:

1. Do analysts with personal reputation produce higher- or lower-quality research than their peers without reputation?
2. Do analysts who work for reputable banks produce higher- or lower-quality research than those at lower-status banks?
3. Does the quality of reputable-bank analysts' research deteriorate when conflicts of interest become more acute in hot markets?
4. Does the quality of reputable analysts' research deteriorate when conflicts of interest become more acute in hot markets?

The first two questions address the static relations between reputation and research quality. They are closely related to existing work, and are the starting point of our analysis. Building on these, the latter two questions examine the dynamics in the reputation-quality relations and shed light on the role of reputation in conflict of interest.

To conduct this study, we construct a unique data set consisting of over 750,000 earnings forecasts issued by sell-side analysts for U.S. companies for the period 1983 to 2002. This data merges individual forecast data with firm characteristics, stock prices, analyst and bank reputation measures, and measures of economy-wide underwriting activities. We use the All-American (henceforth AA) designation as the proxy for analyst reputation. Historical AA awards are hand-collected from the *Institutional Investor* (*II*) magazine and the names of the AA analysts from the *II* listings are carefully matched with the analyst names in the translation file provided by I/B/E/S.

Our findings indicate that personal reputation acts as a very important disciplining device in the presence of conflicts of interest, whereas bank-level reputation alone is less effective. We draw these conclusions from three main results. Our first main result is that reputation is positively related to research quality. We find that analysts with the AA titles are significantly more accurate than others, and this effect is particularly strong among analysts working at lower-status banks. Thus, consistent with Stickel (1992) and Jackson (2005), our evidence strongly suggests that objective research quality, as measured by earnings forecast accuracy, is positively related to personal reputation. Similarly, we find that analysts working at reputable banks are more accurate than those employed at non-reputable banks. This is particularly true for non-AAs. This indicates that the positive reputation-quality relation also exists at the institutional level.

Our second main finding is that the accuracy of non-All-American analysts working at reputable banks significantly deteriorates during hot markets. This is true whether they are compared to top-tier-bank AA analysts or to lower-status-bank, non-AA analysts. Since the payoff from generating underwriting revenue is largest at these top-tier investment banks (large underwriters) and during boom underwriting periods, this time-varying pattern strongly supports the view that conflicts of interest distorted information transmission by these analysts. This suggests that bank reputation did not effectively prevent opportunistic behavior of analysts working for them at the height of the boom.

Our third main finding is that the fall in relative accuracy of top-tier-bank analysts in peak years is restricted to analysts without personal reputation. In contrast, the accuracy of analysts with the AA title does ***not*** deteriorate in hot markets. In fact, AA analysts working at top-tier banks become more accurate relative to both their own non-AA colleagues and other AAs at lower-status banks.

These findings suggest that personal reputation works as an effective disciplinary device against the conflict of interest. What does exacerbate conflict of interest is the combination of a ***lack*** of personal reputation ***and*** the lure of large short-term profits. This combination appears to destroy the incentives of non-AA analysts at top-tier banks, as their research quality is compromised during market peaks. Without portable, personal reputation at stake, non-AA analysts working at top-tier banks have little to lose and too much to gain from liquidating the reputation conferred upon them by their employers in hot markets. In contrast, AAs working at the same top-tier banks have more to lose – namely their portable, personal reputation – and this concern, on average, makes them remain more accurate in the peak years.

Interestingly, our results indicate that *bank* reputation is not as effective as personal reputation in curtailing the conflict of interest problem. The fact that top-tier non-AA analysts succumb to conflict of interest at the peak of the market indicates that in the absence of a personal reputation, bank reputation alone did not prevent analysts from opportunistic behavior.

The remainder of the paper is organized as follows. Section 2 describes the incentive components of an analyst's job and the research hypotheses to be tested. Section 3 describes the data and methodology. Section 4 examines the static relation between reputation and forecast accuracy, and 5 studies the dynamic relation between reputation and conflict of interest. Section 6 concludes.

2. Reputation as an Incentive Device for Stock Analysts

Prior literature suggests that at the individual level, there are two main components in an analyst's compensation: performance measures and the ability to generate investment banking revenues.[5] An important performance measure comes from external buy-side managers. The *Institutional Investor* magazine plays an important role in this process. Every year, it conducts a large survey among buy-side managers,[6] asking them to evaluate the analysts along four dimensions: stock-picking, earnings forecasts, written reports, and overall service. The results of this survey lead to the annual election of the AA analysts, which is featured in the October issue of the magazine every year.

Having an AA title is not only professionally prestigious, but also financially rewarding.[7] AAs earn substantially higher salaries than non-AAs and 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 quality and profitability of the analysts' advice, an analyst will likely have an incentive to be as accurate as possible.[8] Moreover, since taking up a money-manager

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[5] See Stickel (1992); and Michaely and Womack (1999). A third component, the analyst's ability to generate trade commissions, has been analyzed by Jackson (2005) and Irvine (2001), among others. We focus on the trade-off between the first two components in this paper.

[6] 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).

[7] Eccles and Crane (1988).

[8] Ljungqvist et. al. (2005) find that analysts make more accurate earnings forecasts for stocks that are highly visible to institutional investors. This is consistent with analysts caring about their perceived research quality by institutional investors. In addition, Hong and Kubik (2003) and Jackson (2005) document that higher past accuracy lead to higher future reputation; Stickel (1992) further reports that

position on the buy-side is often considered an ultimate reward for successful sell-side analysts, this long-horizon goal could further incentivize the analysts to produce high-quality research that benefit investors. However, the actual link between personal reputation and objective research quality remains an empirical issue.[9] While some prior research such as Stickel (1992) and Jackson (2005) has focused on this question, we re-examine the empirical relation to establish the baseline for our analysis.

Analysts are also compensated (mostly in the form of year-end bonuses) for helping their employers generate investment banking business. Effects of sell-side analysts' incentives to generate investment-banking revenues on the quality of their earnings forecasts have been extensively studied in the literature. In fact, there are two different sources of conflicts that the literature refers to. On the one hand, it has been documented that investment-banking affiliation is related to inflated forecasts and recommendations. For example, Dugar et al. (1995) and Dechow et. al. (2000) find that investment-banker analysts are optimistic, relative to non-investment banker analysts, in their earnings forecasts.[10] This "over-optimism" is at heart of the recent conflict-of-interest investigation. On the other hand, it has also been documented that the desire to help corporate managers beat forecasts is associated with analysts' low-balling of earnings. For example, Chan et al. (2003) document that analysts' desire to win investment banking clients may lead to positive earnings surprises, i.e., negative biases,

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lower relative accuracy leads to removal from the AA team. These findings suggest that a non-star analyst is incentivized to be accurate in order to become a star; once a star, an analyst is incentivized to remain accurate.

[9] The extant literature on career concerns and herding provide mixed theoretical predictions as well as empirical evidence. For example, see Welch (2000) and Bernhardt, Campello and Kutsoati (2005).

[10] Michaely and Womack (1999) find that recommendations made by analysts working at IPO lead underwriters are more positively biased. Lin and McNichols (1998) find that lead and co-lead underwriter analsyts' long-term outlooks are more favorable. Also see Ljungqvist, Marston, and Wilhelm (2005).

for some subsets of stocks.[11] Importantly, what can be gleaned from the large body of literature on conflict of interest is that while signs of analysts' biases might depend on the particular nature of the conflicts they face, absolute errors (accuracy) of their forecasts are increasing (decreasing) in the degree of their conflicts.[12] All else equal, a more conflicted analyst will be less accurate.

To capture variation in the degree of conflict, we observe that underwriting-related compensation usually comes as year-end bonuses and is highly variable from year to year. This practice suggests that the degree of conflict of interest varies significantly across the peaks and troughs of the overall underwriting market. Note that this approach departs from the prior literature where the difference between affiliated and non-affiliated analysts around the time of new security issues was used to capture the *cross-sectional* variation in the degree of conflict of interest.[13] However, since we are interested in the effect of conflict on accuracy *holding the analyst constant*, we use the measure of conflict that *varies over time* and examine how it affects the difference between reputable and non-reputable analysts. In short, it is a *difference-in-difference* approach.

Thus, there are two distinct incentives that affect the analysts' earnings forecasts quality at the personal level. There is a (potentially) positive effect on accuracy due to reputation, and a negative effect due to conflict of interest. Our study attempts to shed light on how reputation affects the conflict of interest. In particular, there are three

[11] Also see Abarbanell and Lehavy (2002), Skinner and Sloan (2002), and Bernhardt and Campello (2002) for related analyses.
[12] In theoretical studies, a positive bias is sometimes assumed. See, for example, Morgan and Stocken (2003) and Cheng et al. (2005).
[13] It is well documented that optimism also exists among non-affiliated and non-broker analysts. See, for example, Francis and Philbrick (1993), and Lim (2001). 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. Servaes and Rajan (1997) also find more general overoptimism by analysts following IPOs.

opposing views on this question. According to the ***cynical/regulator view***, reputable analysts liquidated their reputation at the peak of the market by misleading investors and profiting from their lies.[14] This view predicts that relative accuracy of reputable analysts deteriorates in hot markets. In contrast, the ***reputation-as-incentive view*** maintains that analysts with personal reputation find it in their interests to act less opportunistically relative to their non-reputable counterparts at the peak of the markets, since they have more to lose if they get caught red-handed. This view thus predicts that relative accuracy of reputable analysts actually improves in hot markets. Alternatively, it is also plausible that reputation simply captures superior skill/ability of analysts, with no effect on their incentives. According to this ***reputation-as-ability view***, relative accuracy of reputable analysts should remain constant across hot and cold markets. Distinguishing the three possibilities is an important empirical question with significant policy implications; however, to the best of our knowledge, it has not been examined in the literature.

Finally, at the institutional level, an analyst's pay is also determined by the status of the bank he works for. Jobs at reputable banks are higher paid and more prestigious; competition and in the labor market should imply that bank reputation is positively correlated with performance (accuracy).[15] On the other hand, bank reputation is highly positively correlated with their market shares in underwriting markets.[16] This suggests that the lure of year-end bonuses is high at top-tier banks, i.e. conflict of interest, can be particularly high in top-tier banks during market peaks. This further implies that while

[14] This view appears to be anecdotally supported by star analysts who were found to praise some companies publicly by issuing strong-buy stock recommendations, while at the same time express sharply negative opinions of these companies in private emails. However, no large-sample study has been conducted to verify that star analysts are indeed systematically more prone to this type of conflict of interest than non-star analysts.
[15] See Weiss (1980) and Stiglitz (1992) for efficiency-wage models that explain this relationship.
[16] See Carter and Manaster (1990), Carter, Dark, and Singh (1998), Megginson and Weiss (1993), and Fang (2005).

bank reputation may be positively associated with accuracy during normal times, if conflict of interest affects incentives negatively, analysts working at top-tier investment banks can become less accurate during market booms.

Recapitulating the above discussion, we examine the following empirical questions:

1. *Is personal reputation significant in determining analysts' forecast accuracy? Is the effect positive or negative?*

2. *Is bank reputation important in determining analysts' forecast accuracy? Is the effect positive or negative?*

3. *Does accuracy of analysts who work at reputable banks deteriorate during hot markets?*

4. *Does accuracy of analysts with All-American status deteriorate during hot markets?*

The first and second questions provide baseline results for the effect of personal reputation and bank reputation on research quality. The third question addresses the effect of bank reputation on conflict of interest. The last question analogously examines the effect of personal reputation on conflict of interest.

3. Data and Descriptive Statistics

We define the key variable of interest, analysts' 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 year year, and "*n*" orders the forecasts made by analyst *i* for firm *j* for fiscal year *t*. We focus on unsigned forecast errors because, as discussed in Section 2, while the signs of analysts' biases might depend on the particular nature of the conflicts they face, absolute errors (accuracy) of their forecasts are increasing (decreasing) in the degree of their conflicts. Alternatively, absolute errors better reflect the objective quality of earnings forecasts.

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.[17] We use as the scaling factor the book value of equity rather than either the actual EPS or the stock price because of undesirable time-series properties 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. Using book value of equity, which is more stable and less correlated with movements of the financial markets, does not introduce this potential bias.

Information on analysts' fiscal-year-end earnings forecasts is 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.[18] Firm characteristics and stock prices are obtained from Compustat and CRSP.

We hand-collect analysts' AA status from each October issue of the *Institutional Investor* during our sample period. AA analysts' names are carefully matched with analysts' names in the Translation file form I/B/E/S. 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.

[17] Other studies have similarly used scaled errors to address heteroskedasticity. See, for example, Keane and Runkle (1998).

[18] I/B/E/S provides coverage for multiple forecast horizons. Consistent with prior research, and to utilize the most comprehensive data coverage, we focus on fiscal-year-end forecasts only.

We identify as "top-tier" investment banks the ten underwriters with the highest Carter-Manaster ranks in Carter, Dark, and Singh (1998). 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. The Carter-Manaster measure is constructed using tombstone data collected between 1979 and 1991 and is constant over time. We deliberately use this predetermined, time-invariant measure for bank reputation because it reflects the banks' long track record, thus reducing the likelihood that the empirical relation between bank reputation and forecast quality suffers from the reverse-causality whereby it is analysts' biased forecasts that drive changes in bank reputation.

For the proxy of conflict, we compile from SDC the market-wide IPO volume for each year in our sample. Again, we deliberately use market-level rather than bank-specific underwriting volume to alleviate a potential reverse-causality problem. A particular bank's surge in the underwriting ranks could be due to genuine (perhaps irrational) bullishness of its analysts. While this generates a negative association between bank-level underwriting volume and accuracy, we cannot infer causality. Since market-level volume is less influenced by individual analysts' actions, but there are more financial rewards (potential bonuses) at stake during overall market booms, a finding of overall underwriting volume being negatively associated with analysts' accuracy is more unambiguously indicative of conflict of interest.

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.[19] 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

[19] 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.

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.

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.

4. Relation between Reputation and Accuracy

In this section we report the empirical relation between analyst and bank reputation and forecast accuracy. We first present the uni-variate analysis results, and then the results of our multivariate analyses.

4.1. Uni-variate Analysis

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.

First, we observe that AAs in general are significantly more accurate than non-AAs. In the overall sample, the pooled test for the all-year sample period shows that AAs are significantly more accurate than non-AAs by 0.40% (4.19%-4.59%). With the average error of 4.51%, this indicates that AAs are more accurate than non-AAs by an economically-significant margin of 8.86% (0.40/4.51). This "AA effect" holds and is even stronger among analysts working in lower-status banks: the difference in accuracy is 0.68% (3.94%-4.62%), which is larger than in the overall sample. In contrast, the "AA effect" is much weaker in the top-tier-bank sub-sample: While AAs are still more accurate than non-AAs, the difference is neither statistically nor economically significant.

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 can be understood as an efficiency-wage result, since analysts at top-tier banks are generally better paid.[20] To the extent that even the non-AAs at top-tier banks are quite skilled, being an AA at top-tier banks is less of a distinction.

Are AAs elected based on past accuracy? While this question is not a central part of our analysis, we nonetheless are interested in re-confirming the findings of the prior literature.[21] We report the results of a simple probit model analysis in Table V. Specifically, we estimate the following equation:

$$\Pr(AA\_Election)_{i,t} = \alpha + \beta_1 AvgError_{i,t-1} + \beta_2 AvgFreq_{i,t-1} + \beta_1 Coverage_{i,t-1} \quad (2)$$

[20] See Weiss (1980) and Stiglitz (1992).
[21] Both Stickel (1992) and Jackson (2005) find a positive relation between past accuracy and analyst reputation.

Thus, we examine how an analyst's probability of being elected as an AA in calendar year t is affected by (i) his average error in year t-1, (ii) his average frequency of updating forecasts in t-1, and (iii) the number of firms that he covers in t-1. We find that election is significantly positively related to past accuracy. Thus AA election appears to correctly identify more accurate analysts. Also, we find that both forecast frequency and breadth of coverage significantly increase an analyst's probability of being elected. Overall, results demonstrate that an analyst's accuracy and diligence is rewarded in the AA election. This is important as the findings support the view that the AA election provides analysts with incentives to be accurate and diligent.

Turning to bank reputation, Table VI reports forecast accuracy of analysts working at top-tier banks and lower-status banks. Interestingly, we see that while in the whole sample (the first vertical panel) and the non-AA sub-sample (the last vertical panel), analysts working in top-tier banks are 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. One might also expect that conditional on AA status, bank status will have a weaker effect, since being an AA is itself an important distinction, and all AAs are highly paid. However, here the evidence indicates that AAs in top-tier banks are *less* accurate than those in lower-status banks, which is a somewhat surprising result. However, the uni-variate test here is not conclusive because we have not yet controlled for various firm and forecast characteristics. Thus we will revisit this topic in the next sub-section where multivariate analyses are reported.

In summary, uni-variate results in this sub-section show that the AA status is unambiguous related to higher accuracy. Bank status is also positively correlated with forecast accuracy, although the relation is somewhat weaker. These results suggest that overall, reputation is positively related to research quality, and that this relation holds both as the personal level and at the institutional level. These results are consistent with efficiency-wage models of labor markets where top-tier banks pay premium wages in order to screen for high-ability analysts, and analysts in turn signal their ability by competing for AA titles and top-tier-bank jobs. In the next sub-section we take our analysis to multivariate settings.

4.2. Multivariate Analysis

4.2.1. The Effect of Personal Reputation on Accuracy

To examine the effect of personal reputation on accuracy in a multivariate setting, we use the cross-sectional regression approach developped by Fama and MacBeth (1973). Specifically, for each of the 20 fiscal years in our sample, we first estimate an equation of the form:

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

and then test the significance of the coefficients using the empirical distribution of the 20 estimates. In equation (3), 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 control for heteroskedastic residuals in the cross-section, we use the Huber/White/sandwich variance estimator for the coefficient estimates. To further capture any unobserved firm-level variables, firm-fixed effects are also included in the estimation.

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.[22] More importantly, the annual estimations produce a time series of the AA coefficients, which 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 simply captures superior ability or skill of reputable analysts, there is little reason to expect the relative accuracy of AA to change over time. This ***reputation-as-ability*** view predicts a flat line if one were to plot the AA coefficient against market volume. If, on the other hand, reputation differentially affects the *incentives* of analysts with and without AA titles, then we would expect this

[22] Our dataset consists of more than 10,000 analysts covering over 4,000 firms during the 20-year period. In addition, 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 over time. The Fama-MacBeth approach is well suited for this complex data structure.

coefficient to vary across time. More specifically, ***the cynical/regulator view*** predicts that relative accuracy of reputable analysts declines in hot markets, i.e., AA coefficient and underwriting-volume varaible co-move pro-cyclically. In contrast, the ***reputation-as-incentive view*** predicts that relative accuracy of reputable analysts increases in hot markets, i.e., AA coefficient and underwriting-volume variable co-move but in a counter-cyclical manner.

Table VII reports the Fama-MacBeth regression results on equation (3). 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 in Stickel (1992) and Jackson (2005), AAs are more accurate than non-AAs: The pooled coefficient on the AA variable is negative and statistically significant. The coefficient of 0.0020 indicates that, with the average error of 0.0451, being an AA improves an analyst's accuracy by 4.43% (0.0020/0.0451), after accounting for various firm and forecast characteristics. Thus the AA effect is economically significant.[23]

All control variables in equation (3) have expected signs. The positive coefficient on ln(distance) indicates that the earlier are the estimates made, the less accurate they are.[24] The negative coefficient on firm size suggests that forecast errors are smaller for larger firms, consistent with information being more readily available for larger firms. Higher volatility in stock prices indicates higher risk, and, as expected, it is associated

[23] 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.

[24] Similar results are reported by Kang (1994), Lim (2001), Bernhardt and Campello (2002), and Ivkovic and Jegadeesh (2004), among others.

with larger forecast errors. Finally, higher leverage is associated with larger errors, consistent with the notion that leverage reflects cash-flow risk.

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, *when averaged over the entire sample period.*[25]

This lack of superior accuracy of top-tier AAs over top-tier non-AAs *in an average year,* however, masks an important inter-temporal pattern. To illustrate, in Figure 1 we plot the time series of AA-coefficients from the 20 top-tier-bank annual regressions against the corresponding underwriting volume in each year. A highly intriging and consistent pattern emerges: Here, we see that the coefficient on AA drops significantly (become more negative) when the underwriting volumes are high, as indicated by the dotted vertical lines in the figure.[26] In fact, the two lines co-move almost in exactly opposite directions.

[25] The fact that the AA vs. non-AA differential is bigger in lower-status banks than in top-tier banks is consistent with multiple, mutually-non-exclusive explanations. First, to the extent that analysts' job market is competitive and efficient, non-AAs as top-tier banks can be a better pool of workers than those at lower-status banks. Thus being an AA at a top-tier bank is less of a distinction. Second, it could be that AAs at top-tier banks are more conflicted than AAs at lower-status banks, reducing their superior accuracy. Lastly, it could be that AAs at top-tier banks may be elected less on the basis of their accuracy and more on other factors than AAs at lower-status banks. Our evidence is not sufficient to rule out these mutually non-exclusive hypotheses.

[26] 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.

These results indicate that while top-tier non-AAs do not under-perform their AA colleagues in an average year, they repeatedly under-perform during peaks of the market. While the fact that they can be as accurate as the AAs on average indicates that these analysts are not devoid of skill, their tendency to under-perform during market peaks indicate that they become succeptible to conflict of interest which becomes accute during market booms. In contrast, AAs' unambiguous superiority during market peaks is strongly consistent with the ***reputation-as-incentive*** view, which suggests that analysts with reputation improve their relative accuracy in hot markets.

While the Fama-MacBeth regression coefficidents provides an intuitive analysis of the inter-temporal pattern of relative performance, we are not able to test statistical significance and make conclusive inferences. We will examine the time-series pattern more rigorously in the next section when we study the effect of reputation on conflict of interest.

4.2.2. The Effect of Bank Reputation on Accuracy

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

$$Error_{i,j,n} = \alpha + TopTier_i \beta_1 + \gamma' X_{i,j,n} + \varepsilon_{i,j,n}, \qquad (4)$$

and then aggregate over the years. In (4), $TopTier_{i,t}$ 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 VIII reports estimation results for Equation (4). 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 VI, this table shows that although bank status is generally associated with higher analyst accuracy (as indicated by the negative and significant coefficient on the bank status dummy), this result comes from non-AA analysts. Conditional on being an AA, however, bank status does not affect accuracy.[27]

The fact that bank status matters more for non-AAs is consistent with both personal and bank reputation being effective screening devices in analysts' labor market. Since jobs at top-tier banks are better paid and more competitive, a positive relation between bank status and accuracy suggests a positive pay-performance relation and efficiency-wages being paid in analysts' labor market. This effect is expected to be weaker among AAs since all AAs are highly paid and select individuals. In other words, if bank status is viewed as a certification of analyst quality, this certification is more informative among non-AAs since these analysts have no other certification.

In Figure 2 we plot the time series of top-tier-bank coefficients from the twenty, non-AA annual regressions against the corresponding underwriting volume in each year. The results are in sharp contrast to the top-tier AA coefficient plot in Figure 1: Here, even though top-tier-bank coefficients are negative in 18 out of the 20 years, the two years in which the coefficients are positive coincide with the peak years. In general, the coefficients co-move with the market cycles in a pro-cyclical manner. That is, even

[27] Here we note that multivariate results here no longer support the previous uni-variate result (Table VI) 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.

though top-tier non-AA analysts are higher-ability analysts than their lower-status-bank counterparts, they appear to lose their accuracy in peak markets, which is consistent with the ***cynical/regulator view***. We will further examine this question in the next section.

In summary, our results in this section broadly support a positive link between reputation and the objective research quality of analysts. This is confirmed both at the personal and the bank level. While this is an intuitive result, it comes almost as a surprise in light of recent scandals which tarnished the image of star analysts and prominent institutions. We leave this section by noting that these results pertaining to levels do not establish reputation as affecting analysts' incentives: they could simply mean that analysts with higher ability/skill are more likely to be elected as AAs and hired by top-tier banks. But with these results as baselines, we can now turn to the questions of our particular interest, namely the effectiveness of reputation as a disciplinary device in the presence of conflicts of interest.

5. Reputation as a Disciplinary Device

5.1. The Effect of Bank Reputation on Conflict of Interest

To investigate how bank reputation affects the conflict of interest problem in sell-side research, we include an interaction term between bank reputation and our measure of conflict of interest – the market-wide IPO underwriting volume. We choose this proxy for conflict of interest because investment-banking-related bonuses are the fattest during market peaks, and hence these are the most opportune moments to cash in on one's reputation. According to the ***cynical/regulator*** view, analysts pick the most profitable time to liquidate the reputation capital associated with their employer banks' prestige, and this occurs at the height of market booms. This view predicts a positive sign on the interaction term. In contrast, to the extent that bank reputation tempers analyst incentives, the ***reputation-as-incentive*** view predicts a negative sign, since according to this view, analysts working at reputable banks act less opportunistically than their non-reputable colleagues in the presence of conflict of interest, ceteris paribus. Alternatively, if bank status simply signals analyst skill/ability, the ***reputation-as-ability*** view predicts a zero effect of this interaction term over and above the effect of reputation itself on accuracy.

Specifically, our model is now a pooled regression using all years of data:

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

In (5), $TopTier_{it}$ 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*(UWVolume)* is the log of the underwriting volume in year t. This variable is interacted with the top-tier dummy to examine the effect of bank reputation on conflict of interest. To allow for heteroskedasticity, the Huber/White/sandwich estimator of variance is used for the coefficient estimates. Firm fixed effects are also included, and we allow errors to be clustered at the firm level.

Regression results for equation (5) are reported in Table IX. As before, the first vertical panel pertains to the whole sample, and the latter two pertain to the AA and non-AA sub-samples, respectively. For the overall sample, the coefficient on the interaction term between bank status and underwriting 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 results are consistent with the ***cynical/regulator view***. The negative and significant coefficient on the top-tier-dummy, on the one hand, is consistent with the baseline results in Section 4.1 and 4.2.2 that top-tier non-AAs are significantly more accurate than lower-status non-AAs in general. The positive and significant coefficient on the interaction term, on the other hand, indicates that the forecast accuracy of top-tier-bank non-AAs drops in hot markets relative to that of lower-status-bank non-AAs. Together these results suggest that, even though top-tier-bank non-AAs are more skilled analysts than lower-status-bank non-AAs, they become more inaccurate in hot markets, consistent with the notion that the reputation conferred upon these analysts by their employers' prestigious status is being liquidated at the peaks of the market.

Note that this result is unlikely to be driven by the reverse causality whereby it is the analysts' forecast bias (either positive or negative) that drives certain banks into the top-tier-bank category. In our data construction, we deliberately employ a time-invariant, Carter-Manaster measure of bank reputation so that the top-tier bank status reflects a long track record of past performance rather than how a given bank did in the past 12 months at a given point in time. Given this, the result for top-tier AAs is more consistent with the ***cynical/regulator view***.

In contrast, results in the AA sub-sample are consistent with the ***reputation-as-incentive view***. In the middle panel of the table, the coefficient on the interaction term is negative and significant. This indicates that top-tier-bank AA analysts become relatively more accurate in hot markets than lower-status-bank AAs. Both types of analysts here have personal reputation (AA titles) and are differentiated by the reputation level of their employer banks. Thus, the result is consistent with the notion that, when analysts have personal reputation of their own, working at reputable banks provides them with further incentive to act less opportunistically, i.e., the additional reputation conferred upon them by their employers' status acts as a further disciplinary device. These contrasting findings for AAs and non-AAs suggest a positive role of personal reputation, which we now turn to in the next subsection.

5.2. The Effect of Personal Reputation on Conflict of Interest

To investigate the effect of personal reputation on the conflict of interest problem in sell-side research, we analogously include an interaction term between the personal reputation dummy and our measure of conflict of interest, namely the underwriting-volume variable. As before, the ***cynical/regulator*** view predicts a positive sign on the interaction term, whereas ***reputation-as-incentive*** view predicts a negative sign, since, according to this view, reputable analysts act less opportunistically than their non-reputable colleagues in the presence of conflict of interest. Alternatively, ***reputation-as-ability*** view predicts a zero effect of this interaction term.

Specifically, we estimate the following regression equation:

$$Error_{i,j,t,n} = \alpha + AA_{i,t}\beta_1 + (AA_{i,t} * \ln(UW\,Volume)_t)\beta_2 + \gamma' X_{i,j,t,n} + \varepsilon_{i,j,t,n} \quad (6)$$

where $AA_{i,t}$ is the reputation dummy, $X_{i,j,t,n}$ is the same vector of controls as before, and $AA_{i,t} * \ln(UWVolume)_t$ is the interaction term between reputation and underwriting volume (which proxies for conflict of interest).

Table X presents the estimation results of Equation (6). 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 the underwriting 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 the ***reputation-as-incentive view***, which states that reputable analysts act less opportunistically than non-reputable colleagues at the height of the boom markets, because they have more to lose if they get caught red-handed.

This result can be thought of as a flip-side of the same coin as our finding in Table IX 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 other words, one group of analysts who are consistently acting according to the ***cynical/regulator view*** are the top-tier, non-AA analysts, whether they are compared to lower-status, non-AA analysts or to top-tier, AA analysts. In contrast, top-tier AA analysts are consistently acting according to the ***reputation-as-incentive view***, whether they are being compared to the top-tier, non-AA analysts or to the lower-status, AA analysts.

What do these results together imply? Our findings suggest that while conflict of interest exists and negatively impacts the research quality of analysts, it seems to have the greatest impact on *non-star* analysts working in top-tier banks. As observed before, the potential attraction of the year-end bonus, and thus the temptation to liquidate one's reputation for profit, are presumably strongest for analysts working at top-tier banks during hot markets. And indeed, we find that analysts working at top-tier banks become significantly less accurate during boom years. Importantly, however, this drop in accuracy is limited to non-AAs. At the height of market peaks, AAs working at top-tier banks actually become more accurate relative to both their own non-AA colleagues in top-tier banks and other AAs at lower-status banks.

Since AAs are financially rewarded for their star status and business-generating ability, their temptation to liquidate their personal reputation is also presumably strongest during hot markets. Our finding that AA analysts in general become significantly more accurate during peaks of the market indicates strongly that personal reputation plays a positive role in mitigating conflict of interest.

Thus, contrary to the (popular) image of a corrupt star analyst, personal reputation works as a powerful disciplinary device against the conflict of interest. What does exacerbate conflict of interest is the combination of a ***lack*** of personal reputation ***and*** the lure of large short-term profits. This combination appears to destroy the incentives of non-AA analysts at top-tier banks, as their research quality is compromised during market peaks. Without portable, personal reputation at stake, non-AA analysts working at top-tier banks have little to lose and too much to gain from liquidating the reputation conferred upon them by their employers in hot markets. In contrast, AAs working at the same top-tier banks have more to lose – namely their portable, personal reputation – and this concern, on average, makes them remain more accurate in the peak years.

Interestingly, our results indicate that *bank* reputation is not as effective as *personal* reputation in curtailing the conflict of interest problem. The fact that top-tier non-AA analysts succumb to conflict of interest at the peak of the market indicates that in the absence of a personal reputation, bank reputation alone did not prevent analysts from opportunistic behavior. Bank status only served as an additional incentive device among AA analysts in top-tier banks, as these analysts' accuracy is found to improve relative to even the AAs in lower-status banks.

The difference in the effectiveness of personal reputation and bank reputation as disciplinary devices may be explained by agency problem. For an individual analyst, his personal reputation is human-capital which he has full control rights over and it is portable. Thus there is no incentive-alignment problem in preserving its value. The same analyst faces a much bigger agency problem with regard to the preservation of his employer's reputation, because he does not fully control this reputation capital; he can free-ride and he can also walk out when things turn bad to avoid being tainted by his employer's loss of reputation. High labor mobility may thus make it particularly difficult for non-AA analysts to rationally care about preserving his bank's reputation. The finding that AAs seem to be additionally incentivized by bank reputation can be because as star analysts and hence investment banks' face to the world, AAs' human capital are more closely related to the fortune of their banks. Anecdotal evidence suggests that it is not uncommon for AAs to become Managing Directors or partners of the investment bank they works for. This is only one plausible hypothesis explaining the difference between personal reputation and bank reputation that emerges from our empirical analyses. We leave more in-depth research in this regard to future work.

5.3. Robustness: Composition Effects

Our results in the previous sections suggest that, consistent with the ***cynical/regulator view*** with respect to bank reputation, top-tier, non-AA analysts become relatively less accurate during hot markets, vis-à-vis both AAs at top-tier banks and non-AAs at lower-status banks. Consistent with the ***reputation-as-incentive view*** with respect to personal reputation, top-tier, AA analysts become relatively more accurate during boom markets vis-à-vis both non-AAs in their own banks and other AAs in lower status banks.

Alternatively, can these results be driven by changing compositions of analyst pools? Changing compositions could explain the drop in relative accuracy among non-AAs at top-tier banks during peak years if, for example, the following conditions are true: (i) top-tier banks hire disproportionately more new analysts during peak years to meet additional demand; (ii) new analysts are on average worse forecasters than existing ones; and (iii) new analysts are more likely to be non-AAs than AAs. Likewise, if top-tier banks hire disproportionately more new AAs who are more accurate than others during peak years, then the overall top-tier AA pool's relative accuracy may increase compared to other groups. If these compositional effects are at work, then our results can be driven purely by movements in the analysts' labor market, rather than by any incentive effects due to reputation.

To investigate the composition effect as an alternative hypothesis, we perform two tests. The first test examines the actual composition of all three groups of analysts whose relative performances form the basis of our inferences: the top-tier AA pool, the top-tier non-AA pool, and the lower-status non-AA pool. The second test compares the forecast accuracy of new comers in each pool relative to the existing members of each pool in order to understand how changing compositions might be affecting our results.

Figure 3 shows the composition of each of the three pools. For each analyst pool and each year, we divide the pool into three subsamples: existing, transfers, and new hires. An existing analyst in a pool is an analyst who belongs to the same pool in the previous year. The rest are by definition new comers, and we further divide them into transfers and new hires. A "transfer" is an analyst who does not belong to a particular pool in the previous year, but is in the database in the previous year. The remainders are analysts who are completely new to the database, and are thus considered new hires.

The top-tier non-AA subplot shows that the fraction of this pool that is made up by existing analysts hovers around 70%, and the ratio does not seem to trend up or down over the sample period. The composition of the other two pools are even more stable: existing analysts account for about 80% of the top-tier AA pool over time and 75% of the lower-status non-AA pool over time, and there is no particular trend. Importantly, there is no systematic increase or decrease in any of the three pools during peak years, indicated by the dotted vertical lines in the figure. Based on these results, changing analyst composition is unlikely to be the main drivers of our previous results.

Next, within each analyst pool, we compare the accuracy of existing analysts versus that of new comers. In Table XI and XII we present uni-variate and multivariate results pertaining to this test, respectively. Contrary to the predictions of the composition effect, uni-variate results in Table XI show that, among top-tier non-AAs (Panel A), new comers (both transfers and new hires) are generally *more* accurate than existing analysts. Among new comers, transfers are in turn more accurate than new hires, probably reflecting more work experience. Thus, we find no evidence that top-tier banks hire disproportionately more new and inaccurate analysts during peak years. This means that the relative fall in accuracy among top-tier non-AAs is unlikely to be driven by composition effects; instead, it is more likely to be driven by changing incentives among existing analysts.

But since our inferences are drawn from relative accuracy between analyst pools, to complete this argument, we still need to demonstrate that the other two pools against which the top-tier non-AAs are benchmarked – namely the top-tier AA pool and the lower-status non-AA pool – also do not experience composition-related surges in accuracy. Figure 3 has already shown that neither one of these pools experiences abnormal hiring patterns during market peaks. Examining the accuracy of new comers versus existing analysts in Table XI, we see that for the lower-status non-AAs (Panel B), new comers are in general as accurate as existing analysts. This is again contrary to the prediction of composition effects.[28] Among top-tier AAs, there is no discernable difference in accuracy between new comers and existing analysts in the whole sample period as well as peak years.[29] Thus neither pool seems to experience changes in average accuracy in the direction predicted by composition effects.

These uni-variate results are confirmed in multivariate analyses reported in Table XII. Specifically, for each analyst pool, we estimate the following regression equations:

$$Error_{i,j,t,n} = \alpha + Type_{i,t}\beta_1 + \gamma' X_{i,j,t,n} + \varepsilon_{i,j,t,n}, \qquad (7)$$

[28] Interestingly, during peak years, new comers are significantly less accurate than existing analysts. This is consistent with the notion that the labor market becomes less competitive during market peaks.
[29] This indirectly indicates that the AA election standard is relatively stable over time.

where $Type_{i,t}$ is a dummy variable indicating whether an analyst is an existing one or a new comer, and whether he is a transfer or a new hire. $X_{i,j,t,n}$ is the same vector of controls as before. Panel A, B, and C are for the three analyst pools, respectively.

Consistent with uni-variate findings, we see from Panel A that among top-tier non-AAs, new comers are significantly more accurate than existing ones. This is true for both transfers and new hires. These results go the opposite direction as would be suggested by the composition hypothesis. In the other two analyst pools (Panel B and C), new comers (both transfers and new hires) in general are as accurate as the existing analysts.

In summary, our analyses in this subsection find no evidence that the change in relative accuracy among various analyst pools documented in this paper can be explained by abnormal hiring of new analysts with different skill level from existing analysts. This allows us to rule out composition effects as an alternative explanation to our results, and conclude that the results are driven by changing incentives among existing analysts.

6. Conclusion

In the wake of the scandals embroiling sell-side research, some of the best names on Wall Street have been repeatedly singled out and charged with abuse of public trust. A relevant economic question raised by these recent events is whether reputation, personal and institutional, can mitigate or exacerbate the conflict of interest problem that is widely believed to be present in sell-side research. In other words, it is of interest to know whether the recent events reflect the norm, or isolated cases of breach of reputation.

We tackle this question by investigating a series of empirical questions using a rich sample of U.S. data from 1983 to 2002. Our starting point is first establishing the normal relations between personal and bank reputations and the accuracy of equity analysts' earnings forecasts, which we take as a proxy for the objective quality of an analyst's research. If reputations serve as effective screening devices for analyst quality, then we expect there to be a positive relation between accuracy and both personal reputation and institutional reputation, on average. Building on these static, baseline results, we then go on to examine the time-varying relation between reputation and accuracy. This exercise sheds light on the effect of reputation on conflict of interest because conflict of interest, which manifests itself as investment-banking-related monetary awards, rises and falls with the movement of the overall level of investment-banking activities. When the new issues market is booming, there is more investment-banking-related bonus money at stake, ceteris paribus, and the temptation to forgo research quality is high. By then looking at how analysts with and without personal and bank reputation react to the time-varying forces of conflict of interest, we can infer the effect of reputation on conflict of interest.

Our findings indicate that personal reputation acts as a very important disciplining device in the presence of conflicts of interest, whereas bank-level reputation alone is less effective. These conclusions are drawn from three main findings. First, we find that reputation is positively related to research quality overall. Not only are analysts with AA titles significantly more accurate than others, but so are analysts working at top-tier investment banks as well. This indicates that a positive reputation-quality relation exists at both the personal and the institutional level.

Second, we find that the accuracy of non-All-American analysts (i.e., those without personal reputation) working at top-tier investment banks significantly deteriorates during market peaks. This is true whether they are compared to top-tier-bank AA analysts or to lower-status-bank, non-AA analysts. Since the payoff from generating underwriting revenue is largest at these top-tier investment banks (large underwriters) and during boom underwriting periods, this time-varying pattern strongly supports the view that conflicts of interest distorted information transmission by these analysts.

Third, we find that the fall in relative accuracy of top-tier-bank analysts in peak years is restricted to analysts without personal reputation. The accuracy of analysts with the AA title does ***not*** deteriorate. In fact, AA analysts working at top-tier banks become more accurate relative to both their own non-AA colleagues and other AAs at lower-status banks.

The contrast between the second and the third finding suggests that personal reputation works as an effective disciplinary device against conflict of interest. What does exacerbate conflict of interest is the combination of a ***lack*** of personal reputation ***and*** the lure of large short-term profits. This combination appears to destroy the incentives of non-AA analysts at top-tier banks, as their research quality is compromised during market peaks. Without portable, personal reputation at stake, non-AA analysts working at top-tier banks have little to lose and too much to gain from liquidating the reputation conferred upon them by their employers in hot markets. In contrast, AAs working at the same top-tier banks have more to lose – namely their portable, personal reputation – and this concern, on average, makes them remain more accurate in the peak years.

Interestingly, our results indicate that *bank* reputation is not as effective as personal reputation in curtailing the conflict of interest problem. The fact that top-tier non-AA analysts succumb to conflict of interest at the peak of the market indicates that in the absence of a personal reputation, bank reputation alone did not prevent analysts from opportunistic behavior. We offer some explanation as to why personal and bank reputation may have different effectiveness as disciplinary devices.

These results have important policy implications. In the wake of the sell-side research scandals, some investment banks have moved away from using the AA title as a criterion in evaluating analysts' performance.[30] However, our results indicate that, while far from being a perfect measure, the AA title is positively related to research quality overall and its usefulness in differentiating relative performance increases dramatically during market peaks. This raises the possibility that the recent move to down-weight personal reputation in performance evaluation may actually do a disservice to investors, because analysts are less incentivized to act right once they are deprived of the rewards of being a star analyst.

At the most basic level, our results also indicate that the empirical link between reputation and research quality over the 20-year period of our sample is a ***positive*** one. This means that the reputation mechanisms at place do a reasonably good job in screening for research ability, overall. While this should be an intuitive result, it comes almost as a surprise in light of recent scandals which tarnished the image of star analysts and prominent institutions. As part of the Global Settlement, the largest banks agreed to

[30] Eliott Spitzer, among others, has criticized the selection criteria of AAs as non-objective. 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.

purchase third-party-generated research using part of the $1.4 billion in fines over the next five years. While this regulatory solution is clearly intended to improve transparency and accuracy in sell-side research, our findings raises the question of whether such measures distort the proper incentive mechanisms in place. Our results indicate that a better approach might be to let investors make informed evaluations of different analyst groups and their time-varying accuracy, while letting the reputation mechanism do its job of incentivizing analysts when and where it can. To achieve this end, another solution implemented in the Global Settlement, which is to improve investors' access to analysts' past research quality and reputation measures, could prove to be an effective tool.

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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

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

using the top-tier-bank sub-sample. This coefficient is plotted against the underwriting volume. The dashed lines indicate four peaks years in IPO volume during our sample period: 1986, 1993, 1996, and 1999.

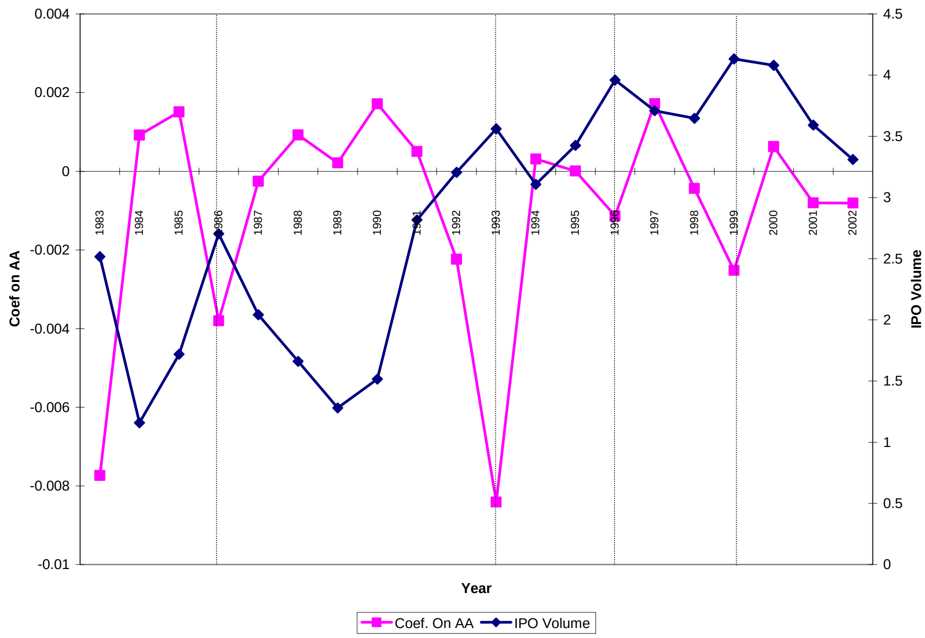

Figure 2. Bank Effect vs. IPO Volume: non-AA analysts

This figure plots the "bank-status effect", which is the coefficient on the top-tier bank dummy from the 20 annual regressions of the equation

$$Error_{i,j,n} = \alpha + TopTier_{i}\beta_1 + \gamma' X_{i,j,n} + \varepsilon_{i,j,n} \qquad (4)$$

The coefficient estimates pertain to the non-AA analyst sub-sample. This coefficient is plotted against the underwriting volume. The dashed lines indicate four peaks years in IPO volume during our sample period: 1986, 1993, 1996, and 1999.

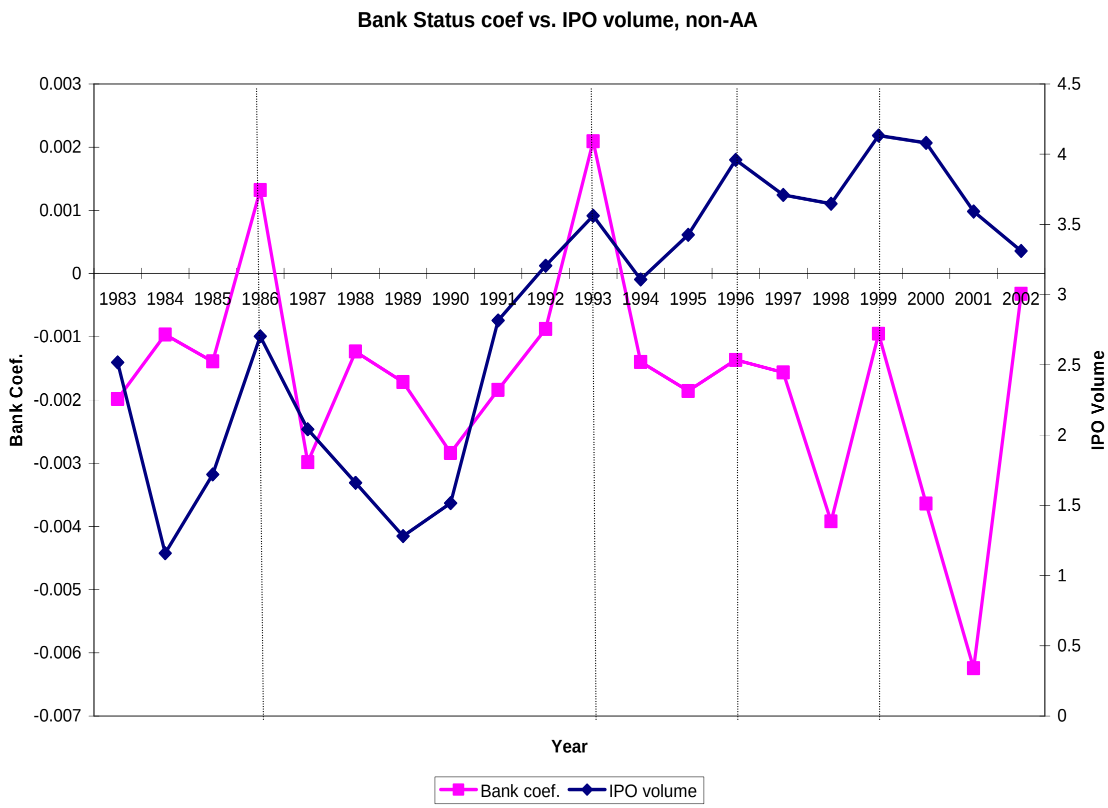

Figure 3. Compositions

These figures plot the compositions of three analyst pools: the top-tier non-AA pool, the top-tier AA pool, and the lower-status non-AA pool, over the sample period of 1983 to 2002. For each analyst pool and each year, we divide analysts into two categories: existing, and new comer. An existing analyst in a pool is an analyst in a pool for a give year and he is also in the same pool the previous year. The rest are considered new comers. We further divide new comers into transfers and new hires. A transfer analyst is an analyst who does not belong to a particular pool in the previous year, but is in the database in the previous year. The remainders are analysts who are completely new to the database, and thus they are considered new hires.

Figure 3.1. Compositions of the Top-tier, non-AA Pool

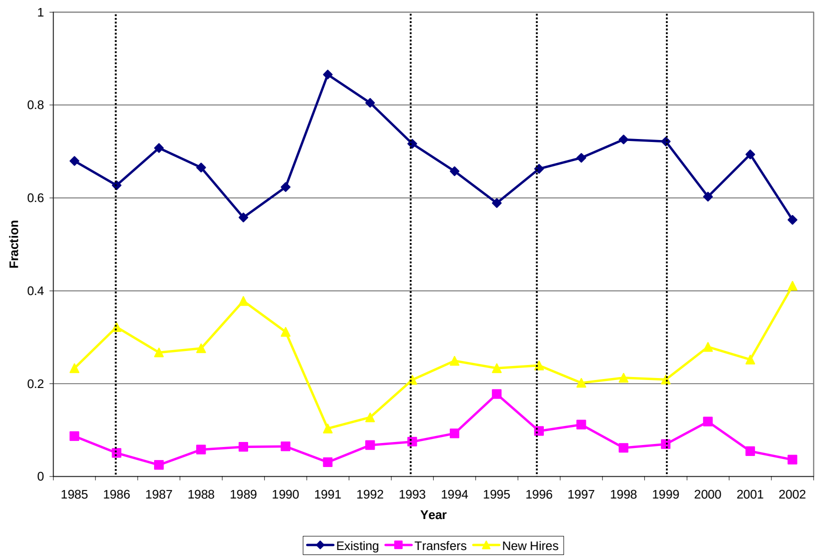

Figure 3.2. Compositions of the Lower-status, non-AA Pool

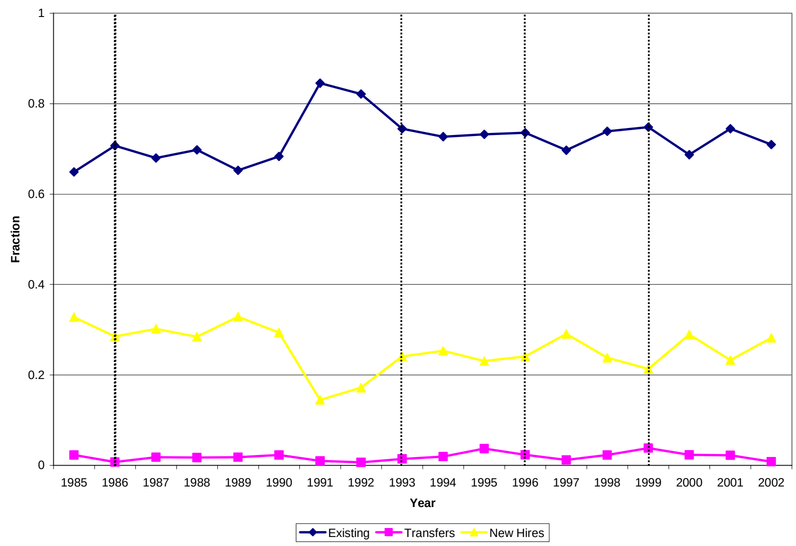

Figure 3.3. Compositions of the Top-tier, AA Pool

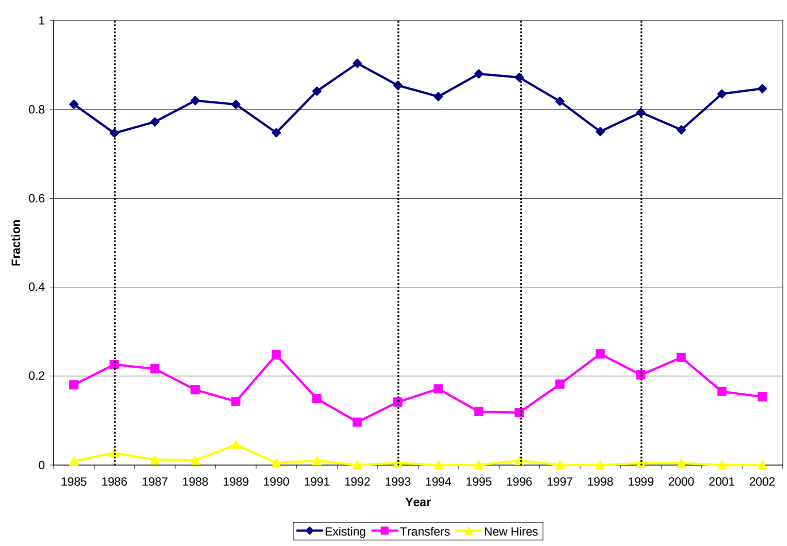

Table I. Sample Descriptive Statistics

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 covered by an analyst. "Average Frequency" is the average number of forecasts an analyst makes for a firm 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
1984	3.99	4.90	-3.77	2.29	3.17	-10.66	9.40	15.75	-8.32
1985	4.01	4.80	-3.43	2.54	3.40	-8.84	10.83	16.28	-6.18
1986	4.44	5.30	-3.41	2.32	3.20	-11.28	10.89	17.23	-7.68
1987	4.63	5.72	-4.14	2.37	3.38	-12.56	11.78	19.49	-8.78
1988	4.92	6.07	-4.01	2.61	3.54	-11.41	13.75	21.75	-7.79
1989	4.97	5.95	-3.14	2.43	3.16	-8.80	13.04	19.60	-6.08
1990	4.78	6.10	-4.53	2.65	3.53	-10.11	13.45	21.83	-8.19
1991	5.08	6.43	-4.93	3.04	4.02	-10.65	16.04	25.41	-8.60
1992	5.38	6.89	-5.42	2.92	3.88	-11.27	16.58	26.89	-9.38
1993	5.69	7.33	-4.85	2.78	3.66	-11.01	16.39	27.12	-9.30
1994	5.37	7.94	-7.46	2.67	3.37	-9.78	15.21	27.16	-10.59
1995	5.56	8.21	-7.86	2.73	3.52	-9.55	16.20	29.37	-10.64
1996	5.67	8.55	-9.04	2.77	3.52	-8.37	16.75	30.46	-10.94
1997	5.71	9.19	-11.63	2.65	3.36	-9.61	16.40	31.44	-12.75
1998	5.75	10.07	-12.96	2.75	3.70	-12.91	17.44	37.50	-15.19
1999	4.05	6.71	-10.72	2.42	2.72	-4.91	10.16	18.75	-10.63
2000	6.51	11.51	-13.80	2.52	3.31	-11.86	18.05	39.61	-15.78
2001	7.15	12.82	-10.70	2.82	4.16	-14.35	23.29	54.78	-14.72
2002	6.98	13.87	-16.26	2.79	4.08	-15.01	22.70	57.00	-18.79

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
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***
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***
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***
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***
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***
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***
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***
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***
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***
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***
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***
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***
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***
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***
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***
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***
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***
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***
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***

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
1984	3.69	4.04	-1.12	3.84	3.55	1.19	3.55	4.17	-1.56
1985	3.74	4.90	-4.65	3.96	3.88	0.35	3.56	5.16	-5.22
1986	3.94	4.81	-2.93	3.85	4.95	-1.58	4.03	4.76	-2.18
1987	3.73	4.48	-3.86	4.13	3.85	0.86	3.25	4.66	-7.72
1988	3.31	3.65	-1.46	3.54	3.70	-0.37	2.96	3.64	-4.24
1989	3.70	4.01	-2.78	3.76	4.10	-1.57	3.61	3.99	-2.76
1990	7.20	4.67	3.38	8.54	4.52	3.32	5.59	4.71	1.19
1991	3.03	3.41	-4.29	3.06	3.30	-1.60	3.00	3.43	-4.06
1992	3.00	3.59	-4.91	2.90	3.56	-2.87	3.12	3.60	-3.27
1993	2.83	3.22	-3.20	2.83	3.45	-1.84	2.83	3.18	-2.63
1994	3.92	3.76	0.53	4.29	2.85	3.76	3.43	3.95	-1.61
1995	4.33	3.81	2.42	4.51	4.01	1.33	4.05	3.77	1.12
1996	3.27	3.69	-3.39	3.30	3.83	-2.84	3.23	3.66	-2.63
1997	3.23	3.88	-4.24	3.31	3.62	-1.71	3.11	3.95	-3.93
1998	3.58	5.29	-11.26	3.36	4.76	-7.16	3.92	5.42	-6.08
1999	9.00	9.29	-0.30	8.99	9.61	-0.42	9.02	9.23	-0.14
2000	5.67	6.47	-3.58	5.54	6.11	-2.03	5.91	6.54	-1.30
2001	5.33	5.12	0.97	5.67	5.52	0.40	4.67	5.05	-1.37
2002	3.28	3.31	-0.29	3.26	3.17	0.60	3.31	3.34	-0.18
All Years	4.19	4.59	-5.74	4.37	4.43	-0.55	3.94	4.62	-7.80

Table V. Probit Estimation Results of AA Election

This table shows the estimation result on a probit model examining the relation between the probability of being elected as an All-American analyst, and three explanatory variables: an analyst's average past forecast error, his past frequency of updating, and his past breadth of coverage. The dependent variable, $AA\_Election_{i,t}$ is 1 if analyst i is elected to AA at t, and 0 otherwise. $AvgError_{i,t-1}$ is the average error of analyst i during election year $t-1$ (averaged over the firms that he covered). $AvgFreq_{i,t-1}$ is the average frequency of forecasts made by analyst i during election year $t-1$ (averaged over the firms that he covered). $Coverage_{i,t-1}$ is the number of firms that analyst i covered during election year $t-1$.

Dependent variable: $AA\_Election_{i,t}$	Coef.	z-stat
$AvgError_{i,t-1}$	-0.2471	-3.29***
$AvgFreq_{i,t-1}$	0.2096	35.64***
$Coverage_{i,t-1}$	0.0210	13.78***
Constant	-1.7851	-80.68***
Pseudo R2	0.05	
N	36,667	

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

This table compares forecast errors of analysts in top-tier banks versus those at the lower-status banks. Forecast error is the absolute difference between an analyst's EPS estimate and the actual EPS 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; thus they are percentage errors. Top-Tier banks are the 10 underwriters with highest rankings in Cater, Dark, and Singh (1998). See Section 3 for details of their identification. ***, **, * 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***
1984	3.65	4.09	-1.31	3.84	3.55	1.13	3.55	4.17	-1.60
1985	3.91	4.95	-3.97***	3.96	3.56	1.73*	3.88	5.16	-4.23***
1986	4.62	4.67	-0.10	3.85	4.03	-0.75	4.95	4.76	0.26
1987	3.96	4.47	-2.56***	4.13	3.25	3.00***	3.85	4.66	-3.55***
1988	3.63	3.55	0.33	3.54	2.96	1.64*	3.70	3.64	0.22
1989	3.94	3.94	-0.01	3.76	3.61	0.83	4.10	3.99	0.59
1990	6.42	4.83	2.63***	8.54	5.59	2.12**	4.52	4.71	-0.69
1991	3.17	3.36	-1.98**	3.06	3.00	0.53	3.30	3.43	-0.90
1992	3.20	3.52	-2.20**	2.90	3.12	-1.52	3.56	3.60	-0.14
1993	3.12	3.12	0.00	2.83	2.83	0.01	3.45	3.18	0.86
1994	3.64	3.87	-0.82	4.29	3.43	1.95**	2.85	3.95	-4.61***
1995	4.27	3.81	2.25**	4.51	4.05	1.25	4.01	3.77	0.91
1996	3.63	3.63	0.03	3.30	3.23	0.35	3.83	3.66	1.20
1997	3.51	3.88	-2.55***	3.31	3.11	0.86	3.62	3.95	-2.02***
1998	4.23	5.28	-6.39***	3.36	3.92	-2.42**	4.76	5.42	-3.09***
1999	9.36	9.21	0.17	8.99	9.02	-0.02	9.61	9.23	0.36
2000	5.87	6.50	-3.24***	5.54	5.91	-0.73	6.11	6.54	-1.65*
2001	5.59	5.02	2.72***	5.67	4.67	2.76***	5.52	5.05	1.75*
2002	3.21	3.33	-1.46	3.26	3.31	-0.24	3.17	3.34	-1.66*
All Years	4.40	4.55	-2.34**	4.37	3.94	3.69***	4.43	4.62	-2.58***

Table VII: 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:

$$\begin{aligned}&\text{Error}_{i,j,n} = \alpha + \text{AA}_i\beta_1 + \ln(\text{distance})_{i,j,n}\beta_2 + \text{Firm Size}_j\,\beta_3 + \\ &\text{Leverage}_j\beta_4 + \text{Volatility}_j\beta_5 + \text{Firm Fixed Effects}_j\beta_j + \varepsilon_{i,j,n}\end{aligned} \quad . \tag{3}$$

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.

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 VIII: Effect of Bank Reputation 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:

$$\begin{aligned}\text{Error}_{i,j,n} &= \alpha + \text{TopTier}_i \beta_1 + \ln(\text{distance})_{i,j,n} \beta_2 + \text{Firm Size}_j \beta_3 + \\ &\text{Leverage}_j \beta_4 + \text{Volatility}_j \beta_6 + \text{Firm Fixed Effects}_j \beta_j + \varepsilon_{i,j,n}\end{aligned} \quad . \tag{4}$$

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	-4.36***	0.0001	0.15	-0.0017	-4.23***
ln(distance)	0.0159	10.19***	0.0165	7.78***	0.0157	10.22***
Firm Size	-0.0390	-3.36***	-0.0341	-2.72***	-0.0408	-3.31***
Leverage	0.0615	1.91*	0.1138	2.45**	0.0558	1.63
Volatility	-0.4181	-1.56	-0.0625	-0.16	-0.4966	-1.71*
Firm Fixed Effects	Yes		Yes		Yes	
Average R^2	0.76		0.78		0.78	
Average N	38,620		7,334		31,286	

Table IX: Effect of Bank Reputation on Conflict of Interest

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:

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

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(UWVolume) is the natural log of the annual total IPO issue volume in billions of (1990 real) dollars. The top-tier* ln(UWVolume) 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(UWVolume)	-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 X: Effect of Analyst Reputation on Conflict of Interest

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:

$$\begin{aligned}&\text{Error}_{i,j,t,n} = \alpha + \text{AA}_{i,t}\beta_1 + (\text{AA}_{i,t} * \ln(\mathit{UW\,Volume})_t)\beta_2 \\ &+ \ln(\text{distance})_{i,j,t,n}\beta_3 + \text{Firm Size}_{j,t}\,\beta_4 + \text{Leverage}_{j,t}\beta_5 + \quad . \\ &\text{Volatility}_{j,t}\beta_7 + \text{Year}_t\,\beta_{year} + \text{Firm Fixed Effects}_j\beta_j + \varepsilon_{i,j,t,n}\end{aligned} \tag{6}$$

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(UWVolume) is the natural log of the annual total IPO issue volume in billion of (1990 real) dollars. The variable AA* ln(UWVolume) 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(UWVolume)	-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 Analysts versus New Comers: Uni-variate Tests

This table reports uni-variate test results comparing the accuracy of existing analysts versus that of new comers in three different pools of analysts. The three panels are for top-tier non-AAs, lower-status non-AAs, and top-tier AAs, respectively. Two test statistics are reported for each group: a pooled t-statistic using the entire twenty years of data, and a second pooled t-statistic using data only from four peak years in terms of underwriting volume. The four peak years are 1983, 1993, 1996, and 1999. For each analyst pool and each year, we divide analysts into two categories: existing, and new comer. An existing analyst in a pool is an analyst in a pool for a give year and he is also in the same pool the previous year. The rest are considered new comers. We further divide new comers into transfers and new hires. A transfer analyst is an analyst who does not belong to a particular pool in the previous year, but is in the database in the previous year. The remainders are analysts who are completely new to the database, and thus they are considered new hires.

	Existing Analysts versus New Comers			Transfers versus New Hires		
	New Comer	Existing	t-stat	Transfers	New Hires	t-stat
Panel A: Top-tier non-AAs						
All years	0.0367	0.0445	-7.67***	0.0347	0.0382	-2.34***
Peak Years Only	0.0396	0.0474	-2.53***	0.0347	0.0416	-1.38
Panel B: Lower-status non-AAs						
All years	0.0462	0.0453	0.92	0.0399	0.0482	-5.03***
Peak Years Only	0.0579	0.0462	2.73***	0.0404	0.0608	-3.32***
Panel C: Top-tier AAs						
All years	0.0426	0.0421	0.11	0.0419	0.0566	-1.64
Peak Years Only	0.0371	0.0361	0.31	0.0353	0.0722	-1.25

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

This table reports multivariate test results comparing the accuracy of existing analysts versus that of new comers in three different pools of analysts. The three panels show results for top-tier non-AAs, lower-status non-AAs, and top-tier AAs, respectively. The specification is the following:

$$Error_{i,j,t,n} = \alpha + Type_{i,t}\beta_1 + \ln(\text{distance})_{i,j,t,n}\beta_2 + Firm\ Size_{j,t}\beta_3 + Leverage_{j,t}\beta_4 + Volatility_{j,t}\beta_5 + Year_t\beta_{year} + Firm\ Fixed\ Effects_j\beta_j + \varepsilon_{i,j,t,n} \quad (7)$$

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. For each analyst pool and each year, we divide analysts into two categories: existing, and new comer. An existing analyst in a pool is an analyst in a pool for a give year and he is also in the same pool the previous year. The rest are considered new comers. We further divide new comers into transfers and new hires. A transfer analyst is an analyst who does not belong to a particular pool in the previous year, but is in the database in the previous year. The remainders are analysts who are completely new to the database, and thus they 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 A: the Top-tier non-AA Pool				
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	114,058		114,058	

Panel B: the Top-tier AA pool				
Variable	Estimate	t-stat	Estimate	t-stat
Existing	0.0012	0.31		
Transfer			-0.0019	-0.45
New Hire			0.0120	2.03**
ln(distance)	0.0173	21.29***	0.0173	21.23***
Firm Size	-0.0060	-3.69***	-0.0060	-3.69***
Leverage	0.0689	5.59***	0.0688	5.57***
Volatility	1.4062	7.11***	1.4052	7.10***
Constant	-0.0402	-3.17***	-0.0381	-2.59***
Year Dummies	Yes		Yes	
Firm Fixed Effects	Yes		Yes	
R^2	0.25		0.25	
N	86,264		86,264	

Panel C: the Lower-status non-AA Pool				
Variable	Estimate	t-stat	Estimate	t-stat
Existing	0.0018	1.72*		
Transfer			-0.0009	-0.32
New Hire			-0.0019	-1.73*
ln(distance)	0.0160	47.70***	0.0160	47.61***
Firm Size	-0.0116	-8.83***	-0.0116	-8.83***
Leverage	0.0776	5.52***	0.0776	5.52***
Volatility	1.0044	9.83***	1.0043	9.83***
Constant	0.0060	0.72	0.0068	0.81
Year Dummies	Yes		Yes	
Firm Fixed Effects	Yes		Yes	
R^2	0.2733		0.2733	
N	511,605		511,605	