The Effectiveness of Reputation as a Disciplinary Mechanism in Sell-Side Research

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We examine whether the quality differentials in earnings forecasts between reputable and nonreputable analysts vary with the severity of conflicts of interest. We measure personal reputation using the Institutional Investor All-American (AA) awards, and bank reputation using Carter-Manaster ranks. While both personal and bank reputation are associated with higher quality forecasts overall, their effectiveness against conflicts of interest differs. The severity of conflicts has a negative and significant effect on the performance of non-AAs at top-tier banks relative to other analysts, while it has a positive and significant effect on the performance of AAs at top-tier banks relative to others. Thus personal reputation is an effective disciplinary device against conflicts of interest, while bank reputation alone is not. (*JEL* G14, G24, G28, D82, J44)

Around the turn of this century, equity markets were red hot, as equity underwriting volumes soared. By 2003, these hot markets were associated with conflict-of-interest scandals involving sell-side research on Wall Street, and some of the "best and brightest" people and firms were charged with violating the public trust by publishing biased research. Individuals, like once-star analyst Jack Grubman, faced multi-million-dollar fines and lifetime bans from the securities industry, and ten of the largest investment banks agreed to pay $1.4 billion in fines through the Global Research Analyst Settlement.

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A large number of studies have documented the presence of conflicts of interest in sell-side Wall Street research.[1] But the fact that analysts and firms of high repute were involved in these scandals is particularly striking, because it is sharply at odds with the theory on the role of reputation. At the personal level, analysts face a trade-off between a loss in long-term reputation and a gain in short-term benefits: by publishing biased research, analysts may lose their standing with investors as valued sources of information and opinion and hence forgo long-term career prospects, while in the short run they may gain substantial underwriting-related compensation. Because analysts with a better reputation have greater long-term benefits to lose, theory predicts that they are more likely to refrain from opportunism in the short run.[2] Similarly, banks face a trade-off between the gains from long-term reputation and short-term opportunism. Theories on repeated games imply that banks have an incentive to build and preserve reputations, and this incentive should help reputable banks better supervise the actions of individual analysts.[3]

In this article, we investigate the apparent disconnect between theory and reality by exploring two questions: (1) Does the trade-off change enough during new issue volume peaks, when the gains from short-run opportunism are much higher than in normal times, to systematically lure reputable analysts and banks into publishing low-quality research? (2) Are personal and bank reputation equally effective in mitigating the conflict of interest problem? By examining these questions, this article bridges two distinct literatures on reputation and on conflicts of interest. Empirically, we examine whether reputable sell-side analysts produce higher quality earnings forecasts relative to nonreputable analysts, and whether their quality differentials vary over time with the severity of the conflicts of interest. Using 1983–2002 U.S. data, we measure personal reputation using the *Institutional Investor* All-American (AA) awards, bank reputation using Carter-Manaster ranks, and the severity of conflicts of interest using the aggregate volume of new equity issues. By analyzing the *time-varying* patterns of analysts' research quality *differentials*, we shed light on the effect of reputation as a disciplinary device against conflicts of interest, the severity of which rises and falls with the level of the overall underwriting market.

Prior literature has examined the static, that is, the time-averaged, relationship between reputation and research quality. On personal reputation, Stickel (1992) shows that AA analysts produce significantly more accurate forecasts

[1] Dugar and Nathan (1995); Michaely and Womack (1999); Dechow, Hutton, and Sloan (2000); and Chan, Karceski, and Lakonishok (2007) document conflicts of interest arising from investment-banking businesses. Cowen, Groysberg, and Healy (2003) and Agrawal and Chen (2004) discuss conflicts of interest caused by trade commissions.

[2] See Benabou and Laroque (1992); Morgan and Stocken (2003); Jackson (2005); and references therein for the theoretical discussions of the trade-offs analysts face and the role of reputation. Sessa (1999) reports that annual compensation for top-ranked analysts in major investment banks can be $1.5 million or more. Eccles and Crane (1988) provide a comprehensive discussion of analyst compensation. For anecdotal reports on All-American analysts' power within an investment bank, see, for instance, Smith and Davis (2005).

[3] For seminal theoretical studies of firm reputation, see Kreps and Wilson (1982); Diamond (1989, 1991); and Chemmanur and Fulghieri (1994). Related empirical work includes Carter and Manaster (1990); Carter, Dark, and Singh (1998); Megginson and Weiss (1991); and Fang (2005).

than other analysts. He concludes that there is a positive relationship between reputation and performance and, hence, pay and performance. Using Australian data, Jackson (2005) finds that more accurate analysts acquire higher future reputations. Fang and Yasuda (2008) find that stocks recommended by AA analysts earn significantly higher excess returns than those recommended by non-AA analysts, particularly in the tech sector. Regarding bank reputation, Cowen, Groysberg, and Healy (2003) find that analysts working at reputable banks make less optimistic forecasts than others. Hong and Kubik (2003) report that both accuracy and optimism positively affect analysts' promotions from nonreputable banks to reputable banks. Fang and Yasuda (2008) find that stocks recommended by analysts working at reputable banks earn significantly higher excess returns than those recommended by analysts working at nonreputable banks.[4]

Beyond documenting a positive correlation between reputation and research quality as in the existing literature, in this article we explicitly investigate whether reputation *mitigates* or *exacerbates* conflicts of interest. If reputation simply captures average "skill," we do not expect the quality differentials between reputable and nonreputable analysts to vary over time with the severity of conflicts of interest. If reputation either mitigates or exacerbates the conflict of interest problem, however, then the quality differentials would vary with the severity of conflicts of interest. Thus, by focusing on the dynamic patterns of analysts' research quality differentials, we address this unexplored empirical question. We further contribute to the literature by examining the effectiveness and limitations of two distinct types of reputation: personal reputation and institutional reputation.

Our findings indicate that, while both personal reputation and bank reputation are associated with higher quality forecasts overall, their effectiveness against conflicts of interest differs. While personal reputation is effective in mitigating conflicts of interest, bank reputation alone is not. We draw these conclusions from three main results.

First, we confirm earlier literature in finding that reputation, both personal and institutional, is positively related to research quality. *Averaged* over the twenty-year experience in our sample, both analysts with AA titles (analysts with personal reputation) and analysts working at more reputable banks (analysts with bank reputation) make significantly more accurate and less positively biased earnings forecasts than other analysts.[5]

Second, we find that the quality of forecasts made by non-AAs working at reputable banks (i.e., analysts with only a bank reputation and not a personal reputation) significantly *worsens* relative to other groups of analysts when conflicts of interest are severe (i.e., when aggregate underwriting volume in the

[4] Also see Jacob, Rock, and Weber (2003) and Barber, Lehavy, and Trueman (2007) for studies comparing analyst research quality of large investment banks and small independent research firms.

[5] All results reported here and below are obtained after various firm, analyst, and forecast characteristics are controlled for.

equity new issues market is high). For example, in a year with average underwriting volume, non-AAs at reputable banks are 6.56%[6] more accurate than non-AAs at nonreputable banks, indicating superior "skill" in normal times. However, in the five years when total new issues volume reached peaks during our sample period, this differential in accuracy drops on average by nearly two-thirds, to only 2.08%. In 2000—the year with the highest equity underwriting volume (in both nominal and real dollars)—the accuracy of non-AAs at reputable banks actually lagged behind that of non-AAs at nonreputable banks. Since the payoff from generating underwriting revenue is largest at the more reputable investment banks (large underwriters) and during peak underwriting periods, this negative time-series relationship between the performance of non-AAs at reputable banks and underwriting volume strongly suggests that bank reputation alone does not effectively mitigate analysts' opportunistic behavior in the presence of conflicts of interest.

In contrast, we find that the relative quality of forecasts made by AA analysts working at reputable banks (analysts with *both* personal and bank reputation) significantly improves in times of high underwriting volumes in the new issues market; that is, top-tier AAs' relative accuracy improves by 11% against top-tier non-AAs and by 7% against lower-tier AAs during peak underwriting years compared to an average year. This statistically and economically significant result suggests that personal reputation is effective in mitigating conflicts of interest.

The remainder of the article is organized as follows. Section 1 outlines our research hypotheses and discusses empirical proxies. Section 2 discusses our data and presents descriptive statistics. Section 3 examines the static relationship between reputation and forecast quality, and Section 4 studies the dynamic relationship between reputation and conflicts of interest. Section 5 concludes.

1. Research Design

1.1 Hypotheses

Does reputation mitigate the conflict of interest problem as suggested by theory, or does it exacerbate the problem as suggested by some recent events? We posit three distinct hypotheses regarding this central question. Prior research suggests that conflicts of interest stem from underwriting-related compensation, which rises during booms in the new issues market.[7] Thus, the severity of conflicts of interest also rises and falls with the underwriting volumes in the new issues market. Whether reputation serves a disciplinary role depends on how the rising conflict of interest in booms alters the long-term/short-term trade-off for different types of analysts. If the rise in short-term profits in hot markets affects

[6] Relative accuracies are reported here as percentages of the average forecast error.

[7] See, for example, Eccles and Crane (1998).

both reputable and nonreputable analysts symmetrically, then the *differential* in research quality between them should remain constant over time. In this case, reputation has no disciplinary effect beyond perhaps being an indicator of ability or skill,[8] and we call this the *reputation-as-ability hypothesis*. If, instead, the temptation of rising short-term profit is disproportionately large for nonreputable analysts—because nonreputable analysts have less to lose from a tainted reputation than reputable analysts—then nonreputable analysts will act more opportunistically during market peaks and their research quality will deteriorate, leading to wider gaps between reputable and nonreputable analysts. We call this the *reputation-as-discipline hypothesis*. Finally, if the rise in short-term profits in hot markets is disproportionately large for reputable analysts, then these analysts will rationally liquidate their reputation capital when it is most profitable to do so, leading their research quality to fall during peak years. We call this the *reputation-liquidation hypothesis*.[9] Which hypothesis is consistent with the facts is an empirical question. By separately examining the three hypotheses for both personal and bank reputation, we further discern whether these two reputation mechanisms function differently.

1.2 Empirical proxies

To test our hypotheses, we need empirical measures of (1) personal and bank reputation, (2) the severity of conflicts of interest, and (3) research quality. Following prior literature, we use the AA title from the *Institutional Investor* magazine as a measure of personal reputation and the Carter-Manaster ranking of investment banks as a proxy for bank reputation. We define the ten banks with the highest Carter-Manaster scores as top-tier banks.

To measure the severity of conflicts of interest, we use market-wide underwriting volume in the equity new issues market (both IPO and SEO).[10] Conflicts of interest are severe when the new issues market is strong and large amounts of underwriting fees are at stake. We use the market-wide (instead of bank-specific) underwriting volume so that all analysts in the cross-section face the same amount of exogenous "pressure" at any given point in time.

Figure 1 plots the total real new equity issuance volume (IPO + SEO) for the overall market, the technology sector, and the nontechnology sector.[11] Interestingly, the peaks of the tech and nontech sectors are different.[12] While the

[8] Our notion of skill is broadly defined, including access to information or network capital.

[9] Note that our empirical approach tests joint hypotheses of (1) time-varying conflicts of interest and (2) the disciplinary role of reputation in mitigating the conflicts.

[10] We also used the market-wide IPO volume as an alternative measure of underwriting activity. Results using this alternative measure are qualitatively and quantitatively similar to those reported in this article.

[11] Issue volumes are in real 1983 dollars. We use the classification in Loughran and Ritter (2004) to distinguish the tech and nontech sectors. See Section 4 for more details.

[12] For example, for nontech stocks, 1996 was a peak year for underwriting activities, with even greater underwriting volumes than in 1999 ($51.4B versus $48.4B in 1983 dollars). For tech stocks, 1999 ($40.6B) and 2000 ($53.3B) were much bigger years than 1996 ($18.5B).

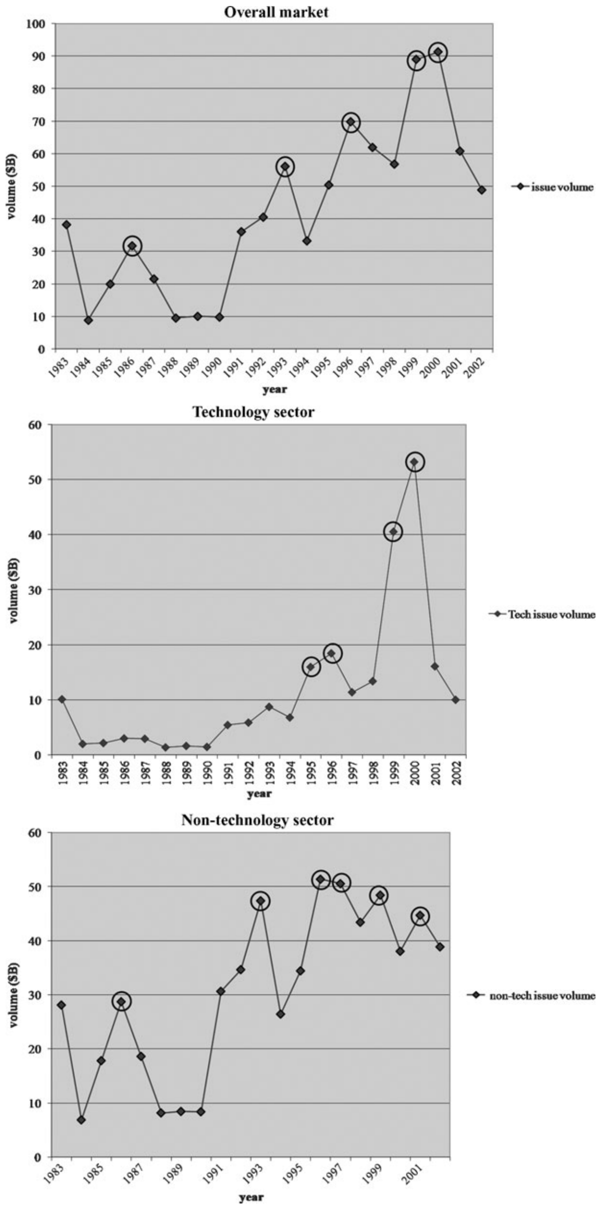

Figure 1
New Issue Activity (1983–2002)
This figure plots the total new issue volume (IPO + SEO) for the overall market, the technology sector, and the nontechnology sector. All new issue volumes are measured in billions of 1983 dollars. Circled years indicated peaks.

stock market bubble at the turn of the century is generally associated with the tech sector (which went through a dramatic boom-to-bust cycle), the nontech sector in fact had a more sustained level of high activity. In our analysis of analyst research quality during peak and nonpeak years below, we define peak years for the overall market to be 1986, 1993, 1996, 1999, and 2000.[13]

We use two empirical measures of analysts' research quality: accuracy (unsigned errors) and bias (signed errors) of their earnings forecasts. We define analysts' forecast accuracy as follows:

$$Error_{i,j,t,n} = \frac{|EPS\ Forecast_{i,j,t,n} - Actual\ EPS\ Reported_{j,t}|}{Book\ Value\ Equity_{j,t-1}}, \tag{1.a}$$

where i indicates an analyst, j indicates a firm, t is the fiscal year, and n orders the forecasts made by analyst i for firm j for fiscal year t. Forecast bias, or signed error, is defined analogously:

$$Bias_{i,j,t,n} = \frac{(EPS\ Forecast_{i,j,t,n} - Actual\ EPS\ Reported_{j,t})}{Book\ Value\ Equity_{j,t-1}}. \tag{1.b}$$

We scale both measures by the firm's book value of equity per share at the previous fiscal year-end to address heteroskedasticity.[14] For a detailed discussion of both unsigned and signed errors as measures of analyst research quality, see Appendix A.1.

2. Data and Descriptive Statistics

We construct a dataset consisting of over eight hundred thousand earnings forecasts issued by sell-side analysts for U.S. companies for the period 1983–2002. Our dataset merges earnings forecast data with firm characteristics, stock prices, analyst and bank reputation measures, and economy-wide underwriting volumes. For detailed descriptions of data sources and variable definitions, see Appendix A.2.

Table 1 contains statistics on the AA election process. Panel A shows the frequency distribution of winning the AA title. There are 10,696 analysts in the I/B/E/S database in our sample period. The 1121 AA analysts thus represent about 10% of all analysts. This indicates that the AA election process is quite competitive. At the same time, once an analyst is elected, it is highly likely that he/she will be elected again: among all AA analysts, 80% win the title multiple times, and the average AA "tenure" is about five years. Panel B presents the

[13] Since market activities were markedly different for the technology and nontechnology sectors, we use sector-level underwriting volumes when we conduct sector-specific analyses. For the tech sector, we define peak years to be 1995, 1996, 1999, and 2000. For the nontech sector, we define peak years to be 1986, 1993, 1996, 1997, 1999, and 2001.

[14] See, for example, Keane and Runkle (1998), for the use of scaled errors to address heteroskedasticity.

Table 1
Summary statistics on AA election

Panel A

Number of times elected as AA	Frequency	Percent
0	9575	89.52%
1	229	2.14%
2	161	1.51%
3	125	1.17%
4	118	1.10%
5	83	0.78%
6	80	0.75%
7	68	0.64%
8	44	0.41%
9	37	0.35%
10	40	0.37%
11	29	0.27%
12	35	0.33%
13	21	0.20%
14	12	0.11%
15	11	0.10%
16	4	0.04%
17	11	0.10%
18	10	0.09%
19	1	0.01%
20	2	0.02%
Total number of analysts	10, 696	100.00%
Number of AAs	1121	10.48%
Number of non-AAs	9575	89.52%

Panel B: Transition matrices

To:	AA	Non-AA	Total
From:			
AA	84.30%	15.70%	100.00%
Non-AA	4.05%	95.95%	100.00%

To:	AA	Non-AA
From:		
New analyst	0.86%	21.76%
AA	77.19%	2.09%
Non-AA	21.94%	76.15%
Total	100.00%	100.00%

This table tabulates the frequency distribution of the number of times an analyst is elected as an AA in the sample.

transition matrix. Once elected, AA analysts tend to be reelected the following year: 77% of the AAs are from last year's AA pool, 22% are from last year's non-AA pool, and less than 1% are completely new analysts. In an unreported probit analysis, we find that AA election is significantly positively related to past accuracy, report frequency, and stock coverage.[15] Combined, these results

[15] Interestingly, Emery and Li (2008) report that past performance is a more important determinant of reelections for existing AAs, while for non-AAs other determinants such as brokerage house size and past star status matter more. Stickel (1992) also finds that lower forecast accuracy increases the probability of deelections for existing AAs.

suggest that AA election is not a result of pure luck but rather a reward for perceived skill (including access to the management of the covered firms) and/or hard work.

A natural question is whether it is easier for analysts at top-tier banks to become AAs. We find that about half of all AAs work at the banks we define as top-tier, and this ratio is stable across years. But, since the ten top-tier banks employ only about 25% of all analysts, the odds of becoming an AA for a top-tier bank analyst is three times as large as that for a lower-tier bank analyst.

Table 2 reports the number of firms, analysts, and forecasts in each year in our merged sample. The number of firms covered peaks at 5475 in 1997 and then declines. While the number of non-AAs more than doubles from 1939 in 1983 to 4442 in 2002, the number of AAs increases by only about 35% from 232 in 1983 to 316 in 2002.[16] The total number of forecasts more than doubles from 52,359 in 1983 to 125,215 in 2002. These figures imply that, on average, each covered firm gets between twenty and thirty reports per year. Interestingly, the ratio of non-AAs to AAs for the head count is always about twice the ratio for the number of reports generated,[17] indicating that AAs tend to issue more forecasts per year than non-AAs.[18]

The last observation is further supported by Table 3, which provides statistics on the working patterns of analysts. It shows that AAs cover more firms and also issue more frequent earnings forecasts per firm than non-AAs. The differences are significant, and the results are consistent with Stickel (1992).

Table 4 compares characteristics of firms covered by AAs and non-AAs. AAs cover significantly larger (by market capitalization) and less risky (by return volatility) firms. AA-covered firms also are more likely to be listed on the NYSE and tend to have higher leverage. Since these systematic differences in firm attributes can affect analysts' forecast quality, we control for them in multivariate analyses.

3. The Static Relationship between Reputation and Forecast Quality

This section examines the overall relationship between reputation, at both the personal level and the bank level, and analyst forecast quality. We do so using the cross-sectional regression approach developed by Fama and MacBeth (1973). Specifically, we first estimate an equation of the form

$$\begin{aligned} \textit{Forecast Quality}_{i,j,n} = {} & \alpha + \textit{Reputation}_i \beta_1 + \ln(\textit{coverage})_i \beta_2 \\ & + \ln(\textit{distance})_{i,j,n} \beta_3 + \textit{Firm Size}_j \beta_4 + \textit{Leverage}_j \beta_5 \\ & + \textit{Volatility}_j \beta_6 + \textit{Firm Fixed Effects}_j \beta_j + \varepsilon_{i,j,n}, \qquad (2) \end{aligned}$$

[16] This number includes all rankings on the AA team, i.e., first place, second place, third place, and runner-up.

[17] For instance, in 1997, the ratio of non-AAs to AAs for the head count was 12.53, but the ratio of non-AAs to AAs for total reports was only 5.93.

[18] Since the election to AA status is announced in October, we identify AA forecasts as those made by an AA from October of the election year to the end of September of the following year.

Table 2
Descriptive statistics for merged sample

		Analysts				Reports			
Fiscal year	Firms	All	Non-AA	AA	Non-AA to AA ratio	All	By non-AA	By AA	Non-AA to AA ratio
1983	2423	2171	1939	232	8.36	52,359	42,082	10,277	4.09
1984	2938	2304	2034	270	7.53	68,354	54,633	13,721	3.98
1985	3287	2407	2190	217	10.09	82,529	70,673	11,856	5.96
1986	3501	2370	2079	291	7.14	81,778	66,240	15,538	4.26
1987	3857	2548	2251	297	7.58	90,565	73,120	17,445	4.19
1988	3965	2464	2160	304	7.11	92,297	74,574	17,723	4.21
1989	3800	2661	2312	349	6.62	87,078	70,155	16,923	4.15
1990	3656	2718	2370	348	6.81	90,457	72,254	18,203	3.97
1991	3562	2410	2062	348	5.93	91,230	70,499	20,731	3.40
1992	3643	2289	1933	356	5.43	91,579	68,997	22,582	3.06
1993	3949	2454	2077	377	5.51	96,176	71,721	24,455	2.93
1994	4323	2831	2457	374	6.57	99,081	75,983	23,098	3.29
1995	4703	3145	2877	268	10.74	107,654	90,710	16,944	5.35
1996	5153	3516	3236	280	11.56	117,244	99,669	17,575	5.67
1997	5475	3951	3659	292	12.53	122,064	104,460	17,604	5.93
1998	5382	4370	4032	338	11.93	136,086	114,824	21,262	5.40
1999	5022	4528	4188	340	12.32	132,786	113,268	19,518	5.80
2000	4550	4687	4356	331	13.16	127,051	108,913	18,138	6.00
2001	3688	4492	4166	326	12.78	126,263	106,190	20,073	5.29
2002	3373	4758	4442	316	14.06	125,215	106,207	19,008	5.59

This table lists summary statistics for the sample. “Firms” is the number of firms covered in the I/B/E/S dataset, 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.

Table 3
Summary statistics for work patterns: AAs versus non-AAs

	Coverage			Average Frequency			Reports		
Year	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

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/she covers. "Reports" is the total number of forecasts an analyst makes during a given period.

for each of the twenty fiscal years in our sample and then test the significance of the coefficients using the empirical distribution of the twenty estimates. In Equation (2), the dependent variable $Forecast\ Quality_{i,j,n}$ is either the accuracy or bias measure defined in Equation (1). The key variable of interest, Reputation_i, is analyst i's AA status in our personal reputation analysis, and the top-tier status of the bank employing the analyst in our bank reputation analysis. Since analysts' personal and bank status may change over time, care has been taken to reflect the correct status at the time the forecast is issued. Among the control variables, $\ln(coverage)_i$ is the natural log of the number of stocks that analyst i covers; $\ln(distance)$ is (the log of) the distance in days between the forecast date and the earnings release date; *Firm Size* is the natural log of the firm's market capitalization of equity at the fiscal year-end in millions of dollars; *Leverage* is the debt/asset ratio at the fiscal year-end; and *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. We use the Huber/White heteroskedasticity-consistent covariance estimators to address heteroskedastic residuals in the cross-section.[19] To capture any unobserved time-invariant firm characteristics, firm-fixed effects are also included in the estimation.

[19] Huber (1967) and White (1980).

Table 4
Comparison of firm characteristics

	Market Cap			Volatility			NYSE			Leverage		
Year	Non-AA	AA	*t*-stat	Non-AA	AA	*t*-stat	Non-AA	AA	*t*-stat	Non-AA	AA	*t*-stat
1983	2936.45	2885.63	0.24	0.019	0.019	1.02	0.88	0.89	−1.71**	0.14	0.15	−1.51
1984	2537.36	2715.20	−0.85	0.018	0.017	3.24***	0.84	0.87	−2.77***	0.13	0.14	−1.51
1985	2859.09	3338.68	−1.51	0.017	0.016	6.47***	0.83	0.88	−4.28***	0.15	0.13	4.00***
1986	3079.64	3605.58	−2.38**	0.018	0.017	3.40***	0.82	0.87	−5.71***	0.16	0.17	−3.01***
1987	3353.19	3992.68	−3.00***	0.020	0.019	5.34***	0.79	0.86	−7.98***	0.16	0.18	−4.99***
1988	3129.82	3718.57	−3.23***	0.021	0.020	3.91***	0.77	0.86	−9.10***	0.16	0.18	−5.36***
1989	3491.34	4203.16	−4.20***	0.017	0.015	9.44***	0.76	0.85	−9.73***	0.17	0.19	−4.94***
1990	3398.54	4447.25	−5.48***	0.019	0.017	7.39***	0.73	0.83	−9.69***	0.17	0.18	−3.56***
1991	4131.00	5276.73	−5.06***	0.022	0.020	8.20***	0.73	0.83	−10.20***	0.17	0.18	−2.94***
1992	4612.96	5614.26	−4.45***	0.022	0.020	9.06***	0.72	0.82	−11.36***	0.15	0.17	−5.51***
1993	4691.82	5763.44	−5.25***	0.021	0.019	11.97***	0.69	0.82	−14.19***	0.15	0.18	−7.31***
1994	4646.25	5524.98	−4.45***	0.022	0.019	14.67***	0.66	0.79	−13.58***	0.15	0.18	−7.68***
1995	5361.20	6274.92	−3.49***	0.021	0.018	12.49***	0.66	0.77	−10.68***	0.16	0.19	−6.28***
1996	5971.99	7826.70	−5.48***	0.023	0.020	12.14***	0.63	0.75	−12.36***	0.16	0.19	−6.32***
1997	7483.70	9999.22	−5.61***	0.024	0.020	18.32***	0.60	0.75	−15.88***	0.17	0.20	−8.68***
1998	9040.75	11, 486.53	−4.70***	0.027	0.024	12.81***	0.58	0.71	−14.31***	0.19	0.22	−7.71***
1999	9612.74	12, 210.70	−2.78***	0.036	0.033	8.85***	0.48	0.63	−13.05***	0.19	0.23	−7.56***
2000	14, 969.93	17, 428.91	−2.83***	0.039	0.035	14.41***	0.54	0.70	−19.20***	0.18	0.22	−12.22***
2001	11, 476.70	15, 643.70	−5.94***	0.037	0.032	19.60***	0.54	0.71	−20.67***	0.18	0.23	−14.46***
2002	8872.30	14, 194.84	−8.75***	0.032	0.027	17.55***	0.55	0.74	−24.08***	0.18	0.23	−13.80***

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 as shares outstanding times the year-end closing price, in 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 computed 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% levels, respectively, based on a two-tailed test.

Table 5
Personal reputation and forecast quality

	Panel A: Accuracy (unsigned errors)		Panel B: Bias (signed errors)	
	Estimate	*t*-stat	Estimate	*t*-stat
	A-1: All sectors		B-1: All sectors	
AA	−0.0021	−4.22***	−0.0010	−1.82*
ln(*coverage*)	0.0005	1.44	−0.0002	−0.76
ln(*distance*)	0.0156	10.18***	0.0096	6.95***
Firm Size	−0.0387	−3.33***	−0.0886	−6.55***
Leverage	0.0566	1.72*	0.1198	3.51***
Volatility	−0.3923	−1.528	−1.3695	−4.63***
Constant	0.2406	2.91**	0.6185	6.12***
Firm Fixed Effects	Yes		Yes	
Average R^2	0.76		0.66	
Average N	40,057		40,057	
	A-2: Tech sector		B-2: Tech sector	
AA	−0.0038	−3.30***	−0.0019	−1.67*
Firm Fixed Effects	Yes		Yes	
Average R^2	0.67		0.62	
Average N	8539		8539	
	A-3: Non-tech sector		B-3: Non-tech sector	
AA	−0.0018	−3.15***	−0.0008	−1.06
Firm Fixed Effects	Yes		Yes	
Average R^2	0.77		0.68	
Average N	31,518		31,518	

This table reports average coefficients from Fama-MacBeth regressions of analysts' forecast quality on the analysts' AA status and the other control variables. For each fiscal year, the following regression is estimated:

$$\begin{aligned} \mathit{Forecast\ Quality}_{i,j,n} = {} & \alpha + AA_i\beta_1 + \ln(\mathit{coverage})_i\beta_2 + \ln(\mathit{distance})_{i,j,n}\beta_3 + \mathit{Firm\ Size}_j\beta_4 + \mathit{Leverage}_j\beta_5 \\ & + \mathit{Volatility}_j\beta_6 + \mathit{Firm\ Fixed\ Effects}_j\beta_j + \varepsilon_{i,j,n}. \end{aligned} \tag{2.a}$$

The dependent variable *Forecast Quality*$_{i,j,n}$ is either the unsigned-scaled error (accuracy, panel A) or signed-scaled error (bias, panel B) for analyst *i*'s *n*th forecast for firm *j*'s fiscal year-end EPS. It is the 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 AA on the forecast date and 0 otherwise. The variable ln(*coverage*) is the natural log of the number of stocks that analyst *i* covers. 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 fiscal 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. For brevity, coefficients on the control variables are suppressed for all but the first subpanels (for the all-sector sample); they are always included in the regressions and are qualitatively and quantitatively similar in all specifications. The Huber/White heteroskedasticity-consistent covariance estimators are used. ***, **, and * denote that the coefficient is statistically significantly different from zero at the 1%, 5%, and 10% levels, respectively.

3.1 Personal reputation and forecast quality

Table 5 reports the Fama-MacBeth estimation result of Equation (2) for personal reputation (AA indicator). Panels A and B use forecast accuracy and bias as the dependent variable (forecast quality measure), respectively. In both panels, all-sector results are shown first, followed by results on the tech and nontech sectors.[20] For brevity of presentation, coefficients on the control variables are shown only for the all-sector results; though not reported, they are included and are qualitatively and quantitatively similar in all specifications.

We find that AAs are more accurate than non-AAs: the average coefficient on the AA variable is negative and statistically significant for all sectors, as well as for the tech and nontech samples. Since the average forecast error in our sample is 4.51% (unreported), the coefficient of 0.0021 (for the overall result) indicates that overall AAs are more accurate than non-AAs by 4.47% (0.0021/0.0451) after accounting for various firm, analyst, and forecast characteristics. Thus the AA effect is economically significant.

The control variables have the expected signs. The positive coefficient on ln*(coverage)* (though not significant) indicates that the more stocks an analyst covers, the less accurate his/her forecasts are.[21] The positive coefficient on ln*(distance)* indicates that the earlier the estimates are made, the less accurate they are.[22] The negative coefficient on *Firm Size* suggests that forecast errors are smaller for larger firms, consistent with more readily available information for larger firms. While *Volatility* comes in mostly as insignificant, higher leverage is associated with larger errors, consistent with the notion that leverage reflects cash-flow risk.

Turning to forecast bias (panel B of Table 5), we find that overall AAs are less positively biased than non-AAs. The result is marginally significant for the whole sample and the tech-sector sample, but insignificant for the nontech-sector sample.

In summary, AAs are on average significantly more accurate and somewhat less positively biased than non-AAs, whether they cover tech stocks or nontech stocks.

3.2 Bank reputation and forecast quality

Table 6 reports the estimation results of Equation (2) focusing on bank reputation. We find that bank status is generally associated with higher accuracy (panel A), as indicated by the negative and significant coefficient on the bank status dummy. Specifically, top-tier bank analysts are on average 3.1% (0.0014/0.0451) more accurate than lower-tier analysts for the whole sample. The results are qualitatively and quantitatively similar for the tech and nontech sector subsamples. Top-tier bank analysts are also less positively biased than lower-tier bank analysts (panel B). This result is significant for the all-sector sample and the nontech sector subsample, but insignificant for the tech-sector sample.

[20] We followed the methodology described in Appendix D of Loughran and Ritter (2004) to divide our sample of firms into tech and nontech firms. Tech firms include Internet firms (such as e-commerce firms). We thank Jay Ritter for making the data available on his website.

[21] This is consistent with earlier findings reported in Clement (1999).

[22] Similar results are reported by Kang, O'Brien, and Sivaramakrishnan (1994); Lim (2001); and Bernhardt and Campello (2002), among others. Also, see Ivkovic and Jegadeesh (2004).

Table 6
Bank reputation and forecast quality

	Panel A: Accuracy (unsigned errors)		Panel B: Bias (signed errors)	
	Estimate	*t*-stat	Estimate	*t*-stat
	A-1: All sectors		B-1: All sectors	
Top-tier bank	−0.0014	−3.65***	−0.0013	−2.52**
ln(*coverage*)	0.0003	0.98	−0.0004	−1.14
ln(*distance*)	0.0156	10.20***	0.0096	6.94***
Firm Size	−0.0387	−3.32***	−0.0886	−6.55***
Leverage	0.0565	1.72*	0.1197	3.51***
Volatility	−0.3913	−1.57	−1.3691	−4.63***
Constant	0.2409	2.91***	0.6190	6.13***
Firm Fixed Effects	Yes		Yes	
Average R^2	0.76		0.66	
Average N	40,057		40,057	
	A-2: Tech sector		B-2: Tech sector	
Top-tier bank	−0.0016	−2.54**	0.0000	−0.02
Firm Fixed Effects	Yes		Yes	
Average R^2	0.67		0.62	
Average N	8539		8539	
	A-3: Non-tech sector		B-3: Non-tech sector	
Top-tier bank	−0.0014	−3.19***	−0.0016	−2.69***
Firm Fixed Effects	Yes		Yes	
Average R^2	0.77		0.68	
Average N	31,518		31,518	

This table reports average coefficients from Fama-MacBeth regressions of analysts' forecast quality on the analysts' bank status and the other control variables. For each fiscal year, the following regression is estimated:

$$\begin{aligned} \textit{Forecast Quality}_{i,j,n} &= \alpha + \textit{TopTier}_i\beta_1 + \ln(\textit{coverage})_i\beta_2 + \ln(\textit{distance})_{i,j,n}\beta_3 + \textit{Firm Size}_j\beta_4 \\ &\quad + \textit{Leverage}_j\beta_5 + \textit{Volatility}_j\beta_6 + \textit{Firm Fixed Effects}_j\beta_j + \varepsilon_{i,j,n}. \end{aligned} \tag{2.b}$$

The dependent variable *Forecast Quality*$_{i,j,n}$ is either the unsigned-scaled error (accuracy, panel A) or signed-scaled error (bias, panel B) for analyst i's nth forecast for firm j's fiscal year-end EPS. It is the 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 AA on the forecast date and 0 otherwise. The variable ln(*coverage*) is the natural log of the number of stocks that analyst i covers. 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 fiscal 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. For brevity, coefficients on the control variables are suppressed for all but the first subpanels (for the all-sector sample); they are always included in the regressions and are qualitatively and quantitatively similar in all specifications. The Huber/White heteroskedasticity-consistent covariance estimators are used. ***, **, and * denote that the coefficient is statistically significantly different from zero at the 1%, 5%, and 10% levels, respectively.

In summary, results in this section broadly support a positive link between reputation and the analysts' research quality. This is confirmed at both the personal and bank levels, for both accuracy and bias, and for the tech and nontech-sector stocks. However, a positive correlation between reputation and research quality in the static Fama-MacBeth setting does not prove that reputation mitigates conflicts of interest; it only supports the *reputation-as-ability hypothesis*. The question of whether reputation helps mitigate conflicts of interest must be examined in a dynamic setting, where the severity of conflicts of interest varies over time. We turn to this analysis in the next section.

4. The Dynamic Relationship between Reputation and Forecast Quality

4.1 Does bank reputation mitigate conflicts of interest?

To investigate whether bank reputation mitigates the conflict of interest problem in sell-side research, we estimate a pooled regression using all years of data and include an interaction term between bank reputation and our measure of conflicts of interest. Specifically, we estimate the following equation:

$$\begin{aligned} \mathit{Forecast\ Quality}_{i,j,t,n} = \alpha &+ \mathit{TopTier}_{i,t}\beta_1 + (\mathit{TopTier}^{*}_{i,t}\ \ln(\mathit{UWVolume}_t))\beta_2 \\ &+ \ln(\mathit{Coverage})_{i,t}\beta_3 + \ln(\mathit{distance})_{i,j,t,n}\beta_4 \\ &+ \mathit{Firm\ Size}_{j,t}\beta_5 + \mathit{Leverage}_{j,t}\beta_6 \\ &+ \mathit{Volatility}_{j,t}\beta_7 + \mathit{Year\ Dummies}_t\beta_t \\ &+ \mathit{Firm\ Fixed\ Effects}_j\beta_j + \varepsilon_{i,j,t,n}. \end{aligned} \tag{3}$$

In Equation (3), the key variable of interest is the interaction term between the top-tier dummy and the (log of) market-wide underwriting volume. According to the *reputation-liquidation hypothesis*, analysts at reputable banks liquidate the reputation capital associated with their employers' prestige at the peaks of the new issues market, when large amounts of underwriting-related bonuses are at stake. This hypothesis predicts a positive sign on the interaction term, indicating more inaccurate and biased forecasts for top-tier bank analysts during peak years. In contrast, the *reputation-as-discipline hypothesis* predicts a negative sign, meaning that the performance of analysts working at top-tier banks improves relative to those working at lower-tier banks during peak years. Finally, if bank status simply captures analyst skill, the *reputation-as-ability hypothesis* predicts a zero effect of this interaction term over and above the effect of reputation itself.

Regression results for Equation (3) are reported in Table 7.[23] Panels A and B use forecast accuracy and bias as the dependent variable (forecast quality measure), respectively. In each panel, all-sector results are reported first, followed by the tech and nontech sector results. For each sector, separate regressions are estimated for all analysts, AAs, and non-AAs. For brevity of presentation, coefficients on the control variables are shown only for the all-sector results; they are qualitatively and quantitatively similar in all specifications. For the tech and nontech sector subsamples, we use the sector-specific underwriting volumes rather than the market-wide underwriting volume because the two

[23] We continue to use the Huber/White/sandwich heteroskedasticity-consistent covariance estimator to allow for heteroskedasticity. Firm fixed effects and year fixed effects are also included. We also continue to allow errors to be clustered at the firm level.

Table 7
Effect of bank reputation on conflict of interest

	Panel A: Accuracy (unsigned errors)						Panel B: Bias (signed errors)					
	All		AA		Non-AA		All		AA		Non-AA	
	Estimate	*t*-stat	Estimate	*t*-stat	Estimate	*t*-stat	Estimate	*t*-stat	Estimate	*t*-stat	Estimate	*t*-stat
	A-l: All sectors						B-l: All sectors					
Top-Tier bank dummy	0.0010	0.27	0.0206	2.67***	−0.0137	−4.11***	−0.0081	−2.15**	−0.0076	−0.91	−0.0055	−1.58
Top Tier ∗ ln(*UWVolume*)	−0.0005	−0.52	−0.0048	−2.44**	0.0031	3.25***	0.0017	1.76*	0.0012	0.55	0.0013	1.29
ln(*coverage*)	0.0005	1.06	−0.0014	−1.18	0.0006	1.29	−0.0010	−2.21**	−0.0032	−2.60***	−0.0006	−1.26
ln(*distance*)	0.0165	63.85***	0.0161	31.29***	0.0165	56.03***	0.0098	36.63***	0.0085	15.99***	0.0101	33.00***
Firm Size	−0.0116	−12.84***	−0.0056	−4.95***	−0.0128	−11.92***	−0.0156	−16.85***	−0.0143	11.58***	−0.0159	−14.42***
Leverage	0.1445	12.11***	0.1574	7.70***	0.1419	10.16***	0.1269	10.39***	0.1428	6.88***	0.1220	8.53***
Volatility	1.3694	14.90***	1.8699	9.24***	1.2868	12.23***	0.8584	9.16***	0.9279	4.47***	0.8621	8.04***
Constant	−0.0213	−3.37***	−0.0611	−6.03***	−0.0124	−1.72*	0.0191	2.94***	0.0294	2.74***	0.0179	2.42***
Year Dummies	Yes		Yes		Yes		Yes		Yes		Yes	
Firm Fixed Effects	Yes		Yes		Yes		Yes		Yes		Yes	
N	801,137		151,916		649,221		801,137		151,916		649,221	
R^2	0.24		0.23		0.25		0.19		0.16		0.20	

(*continued overleaf*)

Table 7
Continued

	Panel A: Accuracy (unsigned errors)						Panel B: Bias (signed errors)					
	All		AA		Non-AA		All		AA		Non-AA	
	Estimate	*t*-stat	Estimate	*t*-stat	Estimate	*t*-stat	Estimate	*t*-stat	Estimate	*t*-stat	Estimate	*t*-stat
	A-2: Tech sector						B-2: Tech sector					
Top-Tier bank dummy	−0.0021	−1.25	−0.0004	−0.15	−0.0007	−0.36	0.0040	2.13**	0.0052	1.61	0.0038	1.62
*Top Tier**ln(*UWVolume*)	0.0001	0.14	0.0010	0.66	−0.0004	−0.41	−0.0023	−2.42**	−0.0027	−1.52	−0.0019	−1.62
N	170,783		24,446		146,337		170,783		24,446		146,337	
R^2	0.26		0.29		0.26		0.16		0.17		0.17	
	A-3: Non-tech sector						B-3: Non-tech sector					
Top-Tier bank dummy	0.0044	0.94	0.0265	2.74***	−0.0162	−4.00***	−0.0113	−2.31**	−0.0121	−1.15	−0.0070	−1.67*
*Top Tier**ln(*UWVolume*)	−0.0015	−1.08	−0.0072	−2.62***	0.0043	3.33***	0.0029	2.04**	0.0025	0.85	0.0019	1.45
N	630,354		127,470		502,884		630,354		127,470		502,884	
R^2	0.24		0.23		0.25		0.20		0.16		0.21	

This table reports the results of regressing forecast quality on bank status, an interaction term between bank status and aggregate underwriting volume, and the control variables. The specification is

$$\begin{aligned} \textit{Forecast Quality}_{i,j,t,n} = {} & \alpha + \textit{TopTier}_{i,t}\beta_1 + (\textit{TopTier}^*_{i,t}\ln(\textit{UWVolume}_t))\beta_2 + \ln(\textit{coverage})_{i,t}\beta_3 + \ln(\textit{distance})_{i,j,t,n}\beta_4 + \textit{Firm Size}_{j,t}\beta_5 + \textit{Leverage}_{j,t}\beta_6 \\ & + \textit{Volatility}_{j,t}\beta_7 + \textit{Year Dummies}_t\beta_t + \textit{Firm Fixed Effects}_j\beta_j + \varepsilon_{i,j,t,n}. \end{aligned} \tag{3}$$

The dependent variable *Forecast Quality*$_{i,j,n}$ is either the unsigned-scaled error (accuracy, panel A) or signed-scaled error (bias, panel B) of analyst *i*'s *n*th forecast for firm *j's* annual EPS for fiscal year *t*. The *TopTier* bank dummy is 1 if analyst *i* works at one of the ten top-tier banks identified in Appendix A.2 on the forecast date, and 0 otherwise. The variable ln(*UWVolume)* is the natural log of the annual total equity issue volume in billions of (1990 real) dollars. The $\textit{TopTier}_{i,t}$* ln($\textit{UWVolume}$)$_t$ is the interaction term. The variable ln(*coverage*) is the natural log of the number of stocks that analyst *i* covers in year *t*. 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 fiscal 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-weighted index) in the 120-day period prior to the forecast date. For brevity, coefficients on the control variables are suppressed for all but the first subpanels (for the all-sector sample); they are always included in the regressions and are qualitatively and quantitatively similar in all specifications. The Huber/White heteroskedasticity-consistent covariance estimators are used. ***, **, and * denote that the coefficient is statistically significantly different from zero at the 1%, 5%, and 10% levels, respectively.

sectors have different boom-and-bust cycles for their equity issuance activities (see Figure 1).

Focusing on accuracy (panel A of Table 7), we find that for the overall sample, the coefficient on the interaction term between bank status and underwriting volume is insignificant. This, however, masks interesting patterns in subsamples: the interaction term is negative and significant for the AA subsample, and positive and significant for the non-AA subsample.

In the non-AA subsample, while the negative and significant coefficient on the top-tier dummy indicates that top-tier non-AAs are significantly more accurate than lower-tier non-AAs *in general*, the positive and significant coefficient on the interaction term indicates that the accuracy of top-tier bank non-AAs *deteriorates* in hot markets relative to that of lower-tier bank non-AAs. This finding—despite having better skills (as they are more accurate in general), top-tier non-AAs *become* less accurate in hot markets—is consistent with the notion that the reputation conferred upon these analysts by their employers' prestige is being liquidated at the peaks of the market.[24] This in turn suggests that bank reputation alone is not effective in curbing conflicts of interest.

What is the economic significance of this result? Holding various firm, analyst, and forecast characteristics constant, in a year with average underwriting volume, top-tier non-AAs are more accurate than lower-tier non-AAs by a margin of 0.003 (or 6.56% of the average error).[25] Similar calculations reveal that during the five peak years, the performance differential between top-tier non-AAs and lower-tier non-AAs narrows to 0.001 (or 2.29% of the average error).[26] Thus, two-thirds (4.27% out of 6.56%) of the superior accuracy achieved by top-tier non-AA analysts vis-à-vis their lower-tier counterparts in normal years disappears in peak years. In 2000, the year with the largest underwriting volume, top-tier non-AAs actually became less accurate than lower-tier non-AAs.[27] This time-varying result is consistent with reputation-liquidation by non-AA analysts working at top-tier banks during market peaks.

In contrast, in the AA subsample, the coefficient on the interaction term is negative and significant. This indicates that top-tier AA analysts become more accurate in hot markets relative to lower-tier AAs. The coefficients imply that

[24] Note that this result is unlikely to be driven by 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 just performance in the past twelve months at a given point in time.

[25] Average log(underwriting volume) = 3.52. Thus, using coefficients from the non-AA estimation in panel A, the total bank-status related differential for the average year is $-0.0137 + (0.0031*3.52) = -0.0030$. Since average error is 4.51%, this represents $(-0.0030) / 0.0451 = -6.56\%$ of the average error.

[26] The average log(underwriting volume) during the peak years = 4.15. Thus, $-0.0137 + (0.0031*4.15) = -0.001$; $(-0.001) / 0.0451 = -2.29\%$. Compared to the average differential in accuracy of -6.56% calculated above, this is a deterioration of $-2.29\% - (-6.56\%) = 4.27\%$, or roughly two-thirds of the differential.

[27] In 2000, log(underwriting volume) = 4.51. The total bank-status related difference is $-0.0137 + (0.0031*4.51) = 0.00009$. The positive figure indicates that in this year top-tier non-AAs become *less* accurate than lower-tier non-AAs.

top-tier AAs' relative accuracy over lower-tier AAs improves by about 11% of the average error (0.0451) between the average underwriting year and 2000, the year with the largest equity underwriting volume; if we consider the five peak years (1986, 1993, 1996, 1999, and 2000) together, the improvement is about 7% of the average error.[28] Both types of analysts here have personal reputation (AA titles) and are differentiated by the reputation of their employer banks. Thus, the result is consistent with the view that, when analysts have a personal reputation, working at reputable banks further reduces the incentive to act opportunistically. Thus, the additional bank reputation conferred upon analysts who already have personal reputation seems to mitigate conflicts of interest.

Sector results (panels A-2 and A-3 for the tech and nontech sectors, respectively, in Table 7) show that the foregoing conclusion largely stems from the nontech sector: here, non-AAs at top-tier banks become less accurate during hot markets (relative to non-AAs at lower-tier banks), while AAs at top-tier banks become more accurate during hot markets (relative to AAs at lower-tier banks). In contrast, in the tech sector, the relative accuracy of top-tier bank analysts over lower-tier bank analysts does *not* vary with the fluctuations in the sector underwriting volume.

Panel B of Table 7 reports the results for bias. First, in the all-sector sample (panel B-1), we find that top-tier bank analysts become marginally more positively biased relative to lower-tier bank analysts during hot markets (significant at 10%). Sector results indicate that this is mainly driven by non-AAs in the nontech sector (panel B-3). Taking this evidence on bias together with the result on accuracy (panel A-3), we find that non-AAs at top-tier banks covering nontech stocks become both less accurate (as shown in panel A-3) and (marginally) more positively biased compared to non-AAs at lower-tier banks during hot markets; that is, their relative performance deteriorates according to both quality measures. AA analysts at top-tier banks, on the other hand, do *not* become more positively biased, while their relative accuracy improves relative to AAs at lower-tier banks (panel A-3).

Summarizing our results on bank reputation as a mitigating device against conflicts of interest, we find diverging results for AAs and non-AAs. Top-tier AAs become more accurate relative to lower-tier AAs during peak underwriting years, while the two groups' forecast bias differential does not vary with the market underwriting volume. In contrast, top-tier non-AAs become significantly less accurate and (marginally) more positively biased compared to

[28] In the average year, the total differential attributable to bank-status among AAs is $0.0206 + (-0.0048^*3.52) = 0.00366$. This is 0.00366 / 0.0451 = 8.12% of the average forecast error of 4.15%. In 2000, the differential is $0.0206 + (-0.0048^*4.51) = -0.00116$. This is −0.00116 / 0.0451 = −2.50% of the average forecast error. Thus, top-tier AAs' relative accuracy over lower-tier AAs improves by −2.50% – 8.12% = −10.62% between the average year and 2000. Similarly, the total difference in all five peak years is $0.0206 + (-0.0048^*4.15) = 0.00064$, which is 1.42% of the average error. 1.42% – 8.12% = −6.70%, meaning that top-tier AAs' relative accuracy over lower-tier AAs improves by 7% between an average year and one of the peak years.

lower-tier non-AAs during peak years.[29] These findings indicate that bank reputation alone is not sufficient to mitigate conflicts of interest: analysts whose sole reputation is conferred on them by their employers (top-tier non-AAs) act in a fashion consistent with the reputation-liquidation hypothesis in market peaks. When personal reputation is present, bank reputation plays a mitigating role.

4.2 Does personal reputation mitigate conflicts of interest?

To investigate the effect of personal reputation on the conflict of interest problem, we estimate a pooled regression analogous to Equation (3):

$$\begin{aligned} \mathit{Forecast\ Quality}_{i,j,t,n} = {} & \alpha + AA_{i,t}\beta_1 + (AA^*_{i,t}\ln(\mathit{UWVolume}_t))\beta_2 \\ & + \ln(\mathit{coverage})_{i,t}\beta_3 + \ln(\mathit{distance})_{i,j,t,n}\beta_4 \\ & + \mathit{Firm\ Size}_{j,t}\beta_5 + \mathit{Leverage}_{j,t}\beta_6 + \mathit{Volatility}_{j,t}\beta_7 \\ & + \mathit{Year\ Dummies}_t\beta_t + \mathit{Firm\ Fixed\ Effects}_j\beta_j \\ & + \varepsilon_{i,j,t,n}, \end{aligned} \tag{4}$$

where the analyst's AA status is interacted with the conflict of interest measure—the aggregate underwriting volume. The *reputation-liquidation hypothesis* again predicts a positive sign on the interaction term, whereas the *reputation-as-discipline hypothesis* and the *reputation-as-ability hypothesis* predict a negative sign and a zero coefficient, respectively.

Table 8 presents the estimation results of Equation (4). Panels A and B use accuracy and bias as the forecast quality measure (dependent variable), respectively. Examining forecast accuracy (panel A), we first observe that the sign on the key interaction term between AA status and the underwriting volume is negative and significant for the whole sample. This indicates that AAs become more accurate relative to non-AAs during market peaks, which is consistent with the *reputation-as-discipline hypothesis*. Furthermore, subsample regressions show that this overall result is driven entirely by top-tier-bank analysts. This means that the performance by AAs at top-tier banks relative to their non-AA colleagues significantly improves during peak years. Quantitatively, this performance improves by 0.0050, or 11% of the average error, between the average year and the five peak years.[30]

[29] Interestingly, the result on top-tier non-AAs is mainly driven by the nontech sector. In the tech sector, top-tier non-AAs do not become less accurate and/or more biased in peak years. This seems to be at odds with the conventional wisdom that conflicts of interest are more severe in the tumultuous tech sector. But perhaps this is not entirely surprising given that the nontech sector actually experienced a more sustained boom in the late 1990s. It is also possible that the extreme incentives generated by the boom are stronger in the nontech sector because overall there are fewer opportunities to reap huge short-term gains in the nontech sector.

[30] In the average year, the performance gap is 0.0319 + (−0.0079*3.52) = 0.00412. This is 9.14% of the average forecast error (0.045). In the peak years, the gap is 0.0319 + (−0.0079*4.15) = −0.00085, or 1.88% (−0.00085 / 0.0451) of the average forecast error. −1.88% – 9.14% = −11.03%, that is, top-tier AAs' relative accuracy over top-tier non-AAs improves by 11% between an average year and one of the peak years. Similarly, in 2000, 0.0319 + (−0.0079*4.51) = −0.00375. −0.00375/0.0451 = −8.32%. −8.32% – 9.14% = −17.46%. Thus,

Table 8
Effect of analyst reputation on conflict of interest

	Panel A: Accuracy (unsigned errors)						Panel B: Bias (signed errors)					
	All		Top-tier banks		Lower-tier banks		All		Top-tier banks		Lower-tier banks	
	Estimate	*t*-stat	Estimate	*t*-stat	Estimate	*t*-stat	Estimate	*t*-stat	Estimate	t-stat	Estimate	*t*-stat
	A-l: All sectors						B-l: All sectors					
AA	0.0116	2.76***	0.0319	4.87***	−0.0032	−0.67	−0.0051	−1.14	−0.0009	−0.13	−0.0015	−0.29
*AA** ln*(UWVolume)*	−0.0032	−2.99***	−0.0079	−4.62***	0.0002	0.13	0.0009	0.79	−0.0004	−0.24	0.0003	0.19
ln*(coverage)*	0.0006	1.34	−0.0004	−0.57	0.0006	1.11	−0.0009	−1.83**	−0.0006	−0.83	−0.0009	−1.75*
ln*(distance)*	0.0165	63.84***	0.0174	38.97***	0.0161	52.15***	0.0098	36.61***	0.0100	21.43***	0.0097	30.25***
Firm Size	−0.0116	−12.90***	−0.0112	−11.88***	−0.0117	−10.03***	−0.0156	−16.86***	−0.0152	−15.21***	−0.0158	−13.19***
Leverage	0.1445	12.11***	0.1777	9.44***	0.1355	9.15***	0.1269	10.38***	0.1684	8.80***	0.1171	7.72***
Volatility	1.3694	14.89***	1.6371	10.66***	1.2753	12.33***	0.8584	9.16***	0.8491	5.40***	0.8517	8.06***
Constant	−0.0216	−3.41***	−0.0398	−4.71***	−0.0150	−1.89*	0.0183	2.81***	0.0083	0.94	0.0216	2.66**
Year Dummies	Yes		Yes		Yes		Yes		Yes		Yes	
Firm Fixed Effects	Yes		Yes		Yes		Yes		Yes		Yes	
N	801,137		207,586		593,551		801,137		207,586		593,551	
E^2	0.24		0.27		0.25		0.19		0.21		0.19	
	A-2: Tech sector						B-2: Tech sector					
AA	−0.0068	−4.17***	−0.0045	−1.69*	−0.0075	−3.41***	0.0026	1.38	0.0023	0.77	0.0008	0.31
*AA** ln*(UWVolume)*	0.0014	1.65*	0.0013	0.95	0.0009	0.81	−0.0020	−2.03**	−0.0018	−1.09	−0.0011	−0.78
N	170,783		40,026		130,757		170,783		40,026		130,757	
E^2	0.26		0.25		0.26		0.16		0.14		0.17	

(*continued overleaf*)

Table 8
Continued

	Panel A: Accuracy (unsigned errors)						Panel B: Bias (signed errors)					
	All		Top-tier banks		Lower-tier banks		All		Top-tier banks		Lower-tier banks	
	Estimate	*t*-stat	Estimate	*t*-stat	Estimate	*t*-stat	Estimate	*t*-stat	Estimate	t-stat	Estimate	*t*-stat
	A-3: Non-tech sector						B-3: Non-tech sector					
AA	0.0190	3.58***	0.0435	5.42***	0.0005	0.08	−0.0069	−1.23	−0.0008	−0.09	−0.0012	−0.19
AA ∗ ln(*UWVolume*)	−0.0057	−3.74***	−0.0123	−5.26***	−0.0007	−0.41	0.0016	0.97	−0.0006	−0.25	0.0003	0.15
N	630,354		167,560		462,794		630,354		167,560		462,794	
R^2	0.24		0.27		0.24		0.20		0.22		0.20	

This table reports the results of regressing forecast quality on AA status, an interaction term between AA status and aggregate underwriting volume, and the control variables. The specification is

$$\begin{aligned} \textit{Forecast Quality}_{i,j,t,n} = {} & \alpha + AA_{i,t}\beta_1 + (AA^*_{i,t}\ \ln(\textit{UWVolume}_t))\beta_2 + \ln(\textit{coverage})_{i,t}\beta_3 + \ln(\textit{distance})_{i,j,t,n}\beta_4 + \textit{Firm Size}_{j,t}\beta_5 + \textit{Leverage}_{j,t}\beta_6 + \textit{Volatility}_{j,t}\beta_7 \\ & + \textit{Year Dummies}_t\beta_t + \textit{Firm Fixed Effects}_j\beta_j + \varepsilon_{i,j,t,n}. \end{aligned} \tag{3}$$

The dependent variable $\textit{Forecast Quality}_{i.j,n}$ is either the unsigned-scaled error (accuracy, panel A) or signed-scaled error (bias, panel B) of 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 AA on the forecast date, and 0 otherwise. The variable ln(*UWVolume*) is the natural log of the annual total equity issue volume in billions of (1990 real) dollars. The variable *AA** ln(*UWVolume*) is the interaction term. The variable ln(*coverage*) is the natural log of the number of stocks that analyst *i* covers in year *t*. 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 fiscal 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. For brevity, coefficients on the control variables are suppressed for all but the first subpanels (for the all-sector sample); they are always included in the regressions and are qualitatively and quantitatively similar in all specifications. The Huber/White heteroskedasticity-consistent covariance estimators are used. ***, **, and * denote that the coefficient is statistically significantly different from zero at the 1%, 5%, and 10% levels, respectively.

This result and our earlier finding (Table 7), viz., that the accuracy of non-AA analysts at top-tier banks goes down during peak years relative to that of non-AA analysts at lower-tier banks, can be thought of as two sides of the same coin: top-tier non-AAs' research quality deteriorates relative to both comparison groups—top-tier AAs (Table 8) and lower-tier non-AAs (Table 7). In other words, top-tier non-AAs consistently act according to the *reputation-liquidation hypothesis*. In contrast, top-tier AA analysts consistently act according to the *reputation-as-discipline hypothesis*, because their accuracy improves relative to both top-tier non-AA analysts (Table 8) and lower-tier AA analysts (Table 7).

Thus, our findings suggest that while conflicts of interest exist and negatively impact the research quality of analysts, they seem to have the greatest impact on *non-star* analysts working at top-tier banks. We find that non-AA analysts working at top-tier banks become significantly less accurate during boom years of the new issues market, when the attraction of large year-end bonuses and the temptation to liquidate one's reputation for profit is high compared to normal years. In contrast, AAs working at top-tier banks actually become more accurate relative to both their non-AA colleagues at top-tier banks and AAs at lower-tier banks at market peaks. Since AAs are rewarded for their ability to generate business, the temptation for them to liquidate their personal reputation is presumably also strongest during hot markets. Our finding that the performance of AA analysts significantly improves relative to other analysts during market peaks indicates that they are able to resist pressures from conflicts of interest better than other analysts, which is consistent with the notion that personal reputation mitigates conflicts of interest.

Turning to subsector results (panels A-2 and A-3 for the tech and nontech sectors, respectively, of Table 8), we observe that the all-sector result is mostly driven by the nontech sector. For the tech sector, the interaction term between AA status and underwriting volume is generally insignificant (only marginally significant for the all-analyst sample). Thus, in the tech sector, there is no evidence that personal reputation played a strong mitigating role. In contrast, the nontech sector result shows that AA analysts at top-tier banks become significantly more accurate during hot markets (relative to non-AA analysts at top-tier banks), while the relative accuracy of AAs at lower-tier banks does not change in hot markets (relative to non-AAs at lower-tier banks).

Panel B of Table 8 shows the results for the bias (signed errors). Overall, AAs either become less positively biased (in the tech sector) or do not change their level of bias relative to other analysts (in the nontech sector) during boom years. Taken together, the results for both accuracy (panel A) and bias (panel B) broadly support the view that personal reputation either improves research quality in boom years or is associated with higher research quality that is

top-tier AAs' relative accuracy over top-tier non-AAs improves by 17% between the average underwriting year and 2000, the year with the highest underwriting volume.

time-invariant through booms and busts. These results are consistent with the *reputation-as-discipline* and *reputation-as-ability hypotheses* and inconsistent with the *reputation-liquidation hypothesis.*

In summary, our results in this section suggest that, despite some notable and highly publicized exceptions, personal reputation works as an effective disciplinary device against conflicts of interest. What exacerbates conflicts of interest is the combination of a *lack* of personal reputation *and* the lure of large short-term profits. In particular, this combination appears to undermine the research quality of non-AA analysts at top-tier banks. Non-AA analysts working at top-tier banks have little portable, personal reputation to lose and 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, helps them maintain their research quality in the peak years.

4.3 Robustness checks

4.3.1 Composition hypothesis. Our results suggest that, consistent with the *reputation-liquidation hypothesis* with respect to bank reputation, top-tier non-AA analysts become relatively less accurate during hot markets than either AAs at top-tier banks or non-AAs at lower-tier banks. Consistent with the *reputation-as-discipline hypothesis* with respect to personal reputation, AA analysts at top-tier banks become relatively more accurate during boom markets than either non-AAs in their own banks or other AAs in lower-tier banks.

One concern about this interpretation is that our results can be driven by changing compositions of analyst pools over the new issues market cycle. Changing compositions could explain the drop in relative accuracy among non-AAs at top-tier banks during peak years under the following circumstances: (1) top-tier banks hire disproportionately more new analysts during peak years to meet additional demand; (2) new analysts are on average worse forecasters than existing ones; and (3) new analysts are more likely to be non-AAs than AAs. Likewise, if in peak years top-tier banks hire more new AAs who are more accurate than existing ones, then the relative accuracy of the AAs at top-tier banks may increase compared to other groups. These compositional effects challenge the interpretation of our results as due to the mitigating role of reputation.

To investigate this alternative hypothesis, we perform two tests. We first examine the actual composition of three groups of analysts: the top-tier AA pool, the top-tier non-AA pool, and the lower-tier non-AA pool. We then compare the forecast quality of newcomers in each pool relative to the existing members of that pool.

Figure 2 shows the composition of each pool. 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 someone who belonged to the same pool in the previous year. The rest are newcomers, whom we further divide

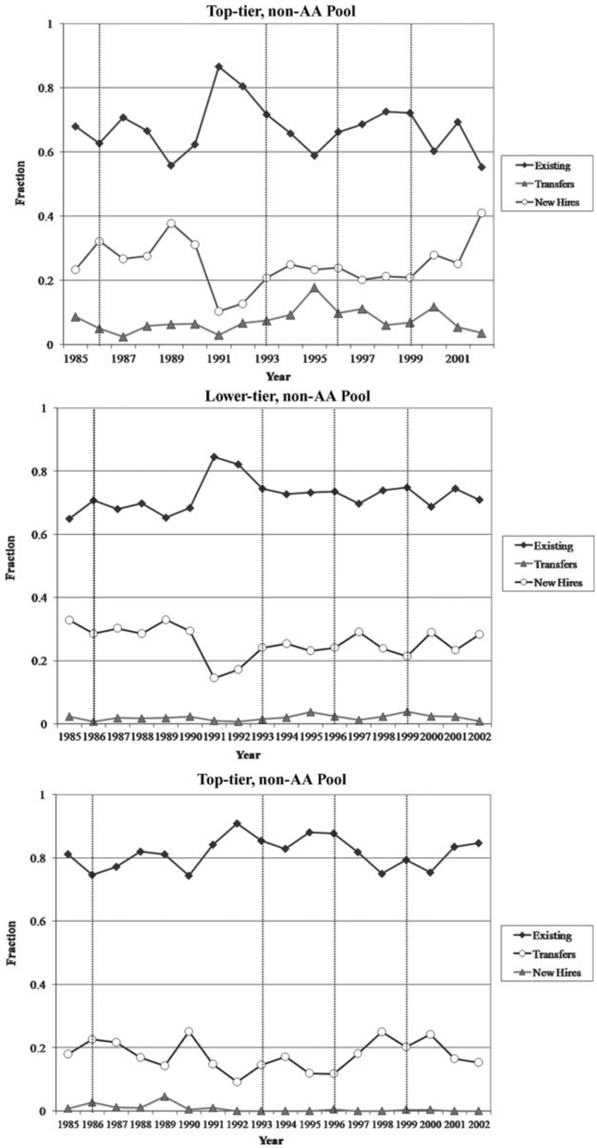

Figure 2
Compositions of Analyst Pools
These figures plot the compositions of three analyst pools: the top-tier non-AA pool, the top-tier AA pool, and the lower-tier non-AA pool. Each analyst pool each year is first divided into existing analysts and newcomers. An existing analyst is in a pool for a given year and was also in the same pool the previous year. The rest are considered newcomers. Newcomers are further divided into transfers and new hires. A transfer is an analyst who did not belong to a particular pool but was in the database in the previous year. The remaining analysts are new to the database and are considered new hires.

Table 9
Relative accuracy of existing analysts versus newcomers: univariate tests

	Existing analysts versus newcomers			Transfers versus new hires		
	Newcomer	Existing	*t*-stat	Transfers	New Hires	*t*-stat
			Panel A: Top-tier non-AAs			
All years	0.0354	0.0429	−7.68***	0.0336	0.0366	−2.05**
Peak years only	0.0386	0.0458	−2.39**	0.0338	0.0406	−1.38
			Panel B: Lower-tier non-AAs			
All years	0.0441	0.0437	0.41	0.039	0.0457	−4.32***
Peak years only	0.056	0.0447	2.72***	0.0390	0.0589	−3.33***
			Panel C: Top-tier AAs			
All years	0.041	0.0407	0.07	0.0403	0.0565	−1.84*
Peak years only	0.0357	0.0349	0.25	0.0337	0.0795	−1.41

This table compares the accuracy of existing analysts and newcomers in three pools of analysts: top-tier non-AAs, lower-tier non-AAs, and top-tier AAs. Results using entire twenty years of data and using only peak years are reported. The peak years are 1983, 1993, 1996, and 1999. Each analyst pool each year is first divided into existing analysts and newcomers. An existing analyst is in a pool for a given year and was also in the same pool the previous year. The rest are considered newcomers. Newcomers are further divided into transfers and new hires. A transfer is an analyst who did not belong to a particular pool but was in the database in the previous year. The remaining analysts are new to the database and are considered new hires. ***, **, and * denote that the coefficient is statistically significantly different from zero at the 1%, 5%, and 10% levels, respectively.

into transfers and new hires. A transfer is an analyst who did not belong to a particular pool but was in the database in the previous year. The remaining analysts are completely new to the database and are considered new hires.

The top-tier non-AA subplot in Figure 2 shows that the fraction of existing analysts hovers around 70%. Existing analysts account for about 75% of the lower-tier non-AA pool and about 80% of the top-tier AA pool over time, with even smaller fluctuations. Importantly, there is no systematic increase or decrease in any of the three pools during the peak years, indicated by the vertical lines in the figure. These patterns suggest that changing analyst composition is unlikely to be a main driver of our previous results.

Next, within each analyst pool, we compare the accuracy of existing analysts and newcomers. Tables 9 and 10 present univariate and multivariate results of this test, respectively. Contrary to the predictions of the composition effect, univariate results in 9 show that, among top-tier non-AAs (panel A), newcomers (both transfers and new hires) are generally *more* accurate than existing analysts. Among newcomers, 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. For the lower-tier non-AAs (panel B), newcomers are in general as accurate as existing

Table 10
Relative accuracy of seasoned and new top-tier non-AAs: multivariate tests

Variable	Estimate	t-stat	Estimate	t-stat
	Panel A: The top-tier non-AA pool			
Existing	0.0046	4.19***	–	–
Transfer	–	–	−0.0055	−2.99***
New Hire	–	–	−0.0043	−3.38***
ln(*coverage*)	−0.0017	−2.61***	−0.0016	−2.46**
ln(*distance*)	0.0159	40.44***	0.0159	40.15***
Firm Size	−0.0124	−13.19***	−0.0124	−13.19***
Leverage	0.0965	9.14***	0.0965	9.14***
Volatility	0.6650	9.18***	0.6648	9.17***
Constant	0.0154	2.41**	0.0208	3.23***
Year Dummies	Yes		Yes	
Firm Fixed Effects	Yes		Yes	
R^2	0.51		0.51	
N	118,224		118,224	
	Panel B: The lower-tier non-AA pool			
Existing	0.0020	2.14**	–	–
Transfer	–	–	−0.0010	−0.36
New hire	–	–	−0.0022	−2.19**
Year Dummies	Yes		Yes	
Firm Fixed Effects	Yes		Yes	
R^2	0.27		0.27	
N	530,945		530,945	
	Panel C: The top-tier AA pool			
Existing	0.0014	0.35	–	–
Transfer	–	–	−0.0020	−0.48
New hire	–	–	0.0118	2.00**
Year Dummies	Yes		Yes	
Firm Fixed Effects	Yes		Yes	
R^2	0.25		0.25	
N	89,335		89,335	

This table reports multivariate tests comparing the accuracy of existing analysts versus newcomers in three pools of analysts: top-tier non-AAs, lower-tier non-AAs, and top-tier AAs. The specification is

$$Error_{i,j,t,n} = \alpha + Type_{i,t}\beta_1 + \ln(coverage)_{i,t}\beta_2 + \ln(distance)_{i,j,t,n}\beta_3 + FirmSize_{j,t}\beta_4 + Leverage_{j,t}\beta_5 + Volatility_{j,t}\beta_6 + YearDummies_t\beta_t + FirmFixedEffects_j\beta_j + \varepsilon_{i,j,t,n}. \quad (5)$$

The dependent variable $Error_{i,j,t,n}$ is the scaled forecast error for analyst i's nth forecast for firm j's annual EPS for fiscal year t. The indicator variable *Type* indicates the subcategory to which an analyst belongs. Each analyst pool each year is first divided into existing analysts and newcomers. An existing analyst is in a pool for a given year and was also in the same pool the previous year. The rest are considered newcomers. Newcomers are further divided into transfers and new hires. A transfer is an analyst who did not belong to a particular pool but was in the database in the previous year. The remaining analysts are new to the database and are considered new hires. The variable ln(*coverage*) is the natural log of the number of stocks that analyst i covers in year t. 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 fiscal 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. The Huber/White heteroskedasticity-consistent covariance estimators are used. ***, **, and * denote that the coefficient is statistically significantly different from zero at the 1%, 5%, and 10% levels, respectively.

analysts.[31] Among top-tier AAs (panel C), there is no discernible difference in accuracy between newcomers and existing analysts in either the whole sample period or in peak years.[32] Thus, neither pool seems to experience changes in

[31] Interestingly, during peak years, newcomers are significantly less accurate than existing analysts. This is consistent with the notion that the labor market becomes less competitive during market peaks.

[32] This indirectly indicates that the AA election standard is relatively stable over time.

average accuracy in the direction predicted by composition effects.[33] These univariate results are confirmed in multivariate analyses reported in Table 10. Specifically, for each analyst pool, we estimate the following regression equations:

$$\begin{aligned} Error_{i,j,t,n} = {} & \alpha + Type_{i,t}\beta_1 + \ln(coverage)_{i,t}\beta_2 + \ln(distance)_{i,j,t,n}\beta_3 \\ & + Firm\ Size_{j,t}\beta_4 + Leverage_{j,t}\beta_5 + Volatility_{j,t}\beta_6 \\ & + YearDummie\ s_t\beta_t + Firm\ Fixed\ Effects_j\beta_j + \varepsilon_{i,j,t,n}, \end{aligned} \quad (5)$$

where $Type_{i,t}$ is a dummy variable indicating whether an analyst is an existing one or a newcomer, and whether he/she is a transfer or a new hire. The set of control variables is the same as before. Panels A, B, and C are for the three analyst pools, respectively.

Consistent with the univariate findings, panels A and B of Table 10 show that, among top-tier non-AAs and lower-tier non-AAs, newcomers—both transfers and new hires—are significantly more accurate than existing analysts. Panel C indicates that among top-tier AAs, existing analysts are more accurate than new hires. These results are exactly opposite to those predicted by the composition hypothesis.[34]

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

4.3.2 Specification with firm-year dummies. In the main specification of the model [Equations (3) and (4)], we control for unobserved (and time-invariant) firm-specific characteristics that affect earnings forecasts by including firm fixed effects. We separately control for unobserved year-specific factors that affect all earnings forecasts made in a given period by including year dummies. In other words, the regression is effectively run on

$$ForecastQuality_{*i,j,t,n} = ForecastQuality_{i,j,t,n} - \bar{y}_j - \bar{y}_t, \quad (6)$$

[33] Results in the lower-tier non-AA pool suggest that the narrowing of quality differential between top-tier non-AAs and lower-tier-non-AAs in peak years cannot be explained by a surge in the hiring of skilled analysts by lower-tier banks in these years. Results in the top-tier-AA pool suggest that the deterioration of top-tier non-AAs' research quality relative to top-tier AAs in peak years cannot be explained by top-tier banks hiring a large number of new, talented AAs in these years.

[34] Our results are consistent with the mixed evidence in the prior literature regarding the relationship of analysts' experience with research quality. While Mikhail, Walther, and Willis (1997) and Clement (1999) report a positive relation, Jacob, Lys, and Neale (1999) find no relation after controlling for analysts' company-specific aptitude in forecasting.

where $ForecastQuality_{i,j,t,n}$ denotes the research quality variables (accuracy or bias) as defined in Equations (1.a) and (1.b), and $\bar{y}_j$ and $\bar{y}_t$ denote its mean for firm j and year t, respectively. One concern with this approach is that AAs may cover stocks that are particularly easy to forecast (and not covered by other analysts) in hot market years, and that this changing coverage drives our results. To check the robustness of our results against this possibility, we consider an alternative approach that includes firm-year dummies. In other words, instead of evaluating relative performance against mean accuracy for firm j, we now evaluate relative performance against mean accuracy for firm j in year t. The regression is effectively run on

$$ForecastQuality_{*i,j,t,n} = ForecastQuality_{i,j,t,n} - \bar{y}_{j,t}, \tag{7}$$

where $\bar{y}_{j,t}$ denotes the mean accuracy (or bias) for firm j in year t.

The framework in Equation (7) alleviates the concern that changing coverage by analysts could drive our result because the relative performance now becomes a function of who else covers firm j in a given year t. However, one implication of the new specification is that observations of firms for which only one forecast was made in year t are effectively excluded from the analysis (absorbed by the firm-year dummy). More broadly, firm-year pairs that are covered by analysts from only one reputation group are also effectively excluded from the analysis insofar as the reputation variables and their interaction terms are concerned.[35] For the reputation dummies and their interaction terms to have explanatory power, we require forecasts for firm j in year t to contain forecasts made by both reputable and nonreputable analysts. To the extent that many firms draw forecasts made by only nonreputable analysts (simply because reputable analysts are fewer in number), we expect the explanatory power of reputation dummies to go down with this specification. However, the specification is useful in checking whether our results are driven by within- or between-firm-year variations.

We reexamine the effect of bank and personal reputation in the dynamic setting by estimating

$$\begin{aligned} ForecastQuality_{i,j,t,n} &= \alpha + TopTier_{i,t}\beta_1 \\ &+ (TopTier^*_{i,t}\ln(UWVolume_t))\beta_2 \\ &+ \ln(coverage)_{i,t}\beta_3 + \ln(distance)_{i,j,t,n}\beta_4 \\ &+ Firm\ Size_{j,t}\beta_5 + Leverage_{j,t}\beta_6 \\ &+ Volatility_{j,t}\beta_7 + Firm - Year\ Dummies_{j,t}\beta_{j,t} + \varepsilon_{i,j,t,n} \end{aligned} \tag{8}$$

[35] Our main specification of separate firm and year dummies also effectively excludes some data points. In that case, firms that are covered by only one reputation group throughout the entire period will be effectively excluded from the analysis insofar as the reputation variables are concerned. However, because it is far more likely for a firm to be covered by only one type of analysts in one year rather than in all twenty years, the loss of data points is far more severe in the firm-year fixed effect specification than in the separate firm-dummy, year-dummy specification.

and

$$\begin{aligned} ForecastQuality_{i,j,t,n} = \alpha &+ AA_{i,t}\beta_1 \\ &+ (AA^*_{i,t}\ln(UWVolume_t))\beta_2 \\ &+ \ln(coverage)_{i,t}\beta_3 + \ln(distance)_{i,j,t,n}\beta_4 \\ &+ Firm\ Size_{j,t}\beta_5 + Leverage_{j,t}\beta_6 \\ &+ Volatility_{j,t}\beta_7 \\ &+ Firm - YearDummies_{j,t}\beta_{j,t} + \varepsilon_{i,j,t,n}. \end{aligned} \quad (9)$$

The results for Equations (8) and (9) are reported in Tables 11 and 12, respectively. Panel A of 11 shows that top-tier analysts become significantly more accurate during hot markets for the whole sample. But this result is statistically insignificant for both subsamples (AAs and non-AAs) and subsectors (tech and nontech). Thus the new specification reveals weak evidence in support of bank reputation as a mitigating force. In Table 12, we find that top-tier AAs become significantly more accurate and less positively biased compared to top-tier non-AAs during market peaks. This result is primarily driven by the nontech sector. Overall, AAs' performance remains strong, even in hot markets.

It is interesting that while we still detect significant deterioration of relative performance by top-tier non-AAs against top-tier AAs (in Table 12, pertaining to personal reputation), we no longer detect their deterioration against lower-tier non-AAs (in Table 11, pertaining to bank reputation) under this alternative specification. The significant attenuation of the bank-reputation result likely occurs because lower-tier non-AAs are more likely than any other analyst groups to cover stocks that are covered only by members of the same group, and, as discussed above, such observations are effectively excluded in the analysis of the reputation variables. In unreported calculations, we find that about one-third of all firm-year pairs in our sample are covered only by lower-tier non-AAs, whereas only 1% of all such pairs are covered only by AAs. Similarly, conditional on being covered by at least one AA in a given year, the average number of analysts covering the stock during the year is 14.5, whereas conditional on *not* being covered by any AA analysts, the average number of analysts covering such a stock during that year is only 3.9 (the difference is statistically significant at the 0.01% level).[36] These statistics indicate that far more firm-year pairs are represented only by lower-tier non-AAs than by

[36] These results are consistent with the fact that AAs tend to cover larger and more established firms (see Table 4). This also indicates that AA analysts are leaders rather than followers, and whenever they cover a stock, other analysts flock to it as well. For related literature on career concerns and analyst herding, see, for example, Scharfstein and Stein (1990); Zwiebel (1995); Hong, Kubik, and Solomon (2000); Welch (2000); Bernhardt, Campello, and Kutsoati (2006); Ottaviani and Sorensen (2006); Clark and Subramanian (2005); and Dasgupta and Prat (2006).

Table 11
Effect of bank reputation with firm-year dummies

	Panel A: Accuracy (unsigned errors)						Panel B: Bias (signed errors)					
	All		AA		Non-AA		All		AA		Non-AA	
	Estimate	*t*-stat	Estimate	*t*-stat	Estimate	*t*-stat	Estimate	*t*-stat	Estimate	*t*-stat	Estimate	*t*-stat
					All sectors							
Top-Tier bank dummy	0.0021	1.00	0.0090	1.44	−0.0006	−0.49	−0.0007	−0.26	−0.0148	−1.65*	−0.0002	−0.13
Top-Tier * ln(*UWVolume*)	−0.0010	−1.93*	−0.0022	−1.48	−0.0003	−0.84	−0.0002	−0.37	0.0036	1.65*	−0.0003	−0.61
ln(*coverage*)	0.0005	2.24**	0.0010	1.04	0.0006	2.83***	−0.0004	−1.75*	−0.0005	−0.49	−0.0002	−0.91
ln(*distance*)	0.0166	81.58***	0.0174	38.75***	0.0166	70.53***	0.0102	45.40***	0.0094	18.69***	0.0105	40.40***
Firm Size	−0.0389	−8.92***	−0.0449	−4.10***	−0.0387	−7.98***	−0.0744	−16.99***	−0.0944	−8.57***	−0.0713	−14.67***
Leverage	0.0868	4.60***	0.1472	3.00***	0.0769	3.68***	0.1404	7.32***	0.2309	4.66***	0.1263	5.95***
Volatility	−0.1592	−3.02***	−0.0986	−0.68	−0.1690	−2.99***	−0.6645	−10.56***	−0.9425	−5.78***	−0.6168	−9.03***
Constant	0.2393	7.85***	0.2775	3.57***	0.2366	7.06***	0.5136	16.77***	0.6858	8.74***	0.4844	14.39***
Firm-year Fixed Effects	Yes		Yes		Yes		Yes		Yes		Yes	
N	801,137		151,916		649,221		801,137		151,916		649,221	
R^2	0.7900		0.7572		0.8135		0.7069		0.6230		0.7421	

(*continued overleaf*)

Table 11
Continued

	Panel A: Accuracy (unsigned errors)						Panel B: Bias (signed errors)					
	All		AA		Non-AA		All		AA		Non-AA	
	Estimate	*t*-stat	Estimate	*t*-stat	Estimate	*t*-stat	Estimate	*t*-stat	Estimate	*t*-stat	Estimate	*t*-stat
					Tech sector							
Top-Tier bank dummy	−0.0005	−0.46	0.0022	0.93	−0.0004	−0.29	0.0018	1.23	0.0046	1.55	0.0004	0.24
Top-Tier * ln(*UWVolume*)	−0.0006	−1.16	−0.0009	−0.68	−0.0006	−0.84	−0.0012	−1.61	−0.0016	−0.91	−0.0004	−0.52
N	170,783		24,446		146,337		170,783		24,446		146,337	
R^2	0.7256		0.7496		0.7286		0.6118		0.6527		0.6146	
					Non-tech sector							
Top-Tier bank dummy	0.0024	0.83	0.0093	1.12	−0.0012	−0.8	−0.0022	−0.61	−0.0220	−1.82*	−0.0011	−0.56
Top-Tier * ln(*UWVolume*)	−0.0012	−1.50	−0.0026	−1.15	−0.0002	−0.4	0.0001	0.13	0.0061	1.83*	−0.0001	−0.17
N	630,354		127,470		502,884		630,354		127,470		502,884	
R^2	0.8019		0.7577		0.8337		0.7273		0.6213		0.7733	

This table reports results from regressing forecast quality on bank status, an interaction term between bank status and aggregate underwriting volume, and the control variables. The specification is

$$ForecastQuality_{i,j,t,n} = \alpha + TopTier_{i,t}\beta_1 + (TopTier_{i,t}{}^{*}\ \ln(UWVolume_t))\beta_2 + \ln(coverage)_{i,t}\beta_3 + \ln(distance)_{i,j,t,n}\beta_4 + FirmSize_{j,t}\beta_5 + Leverage_{j,t}\beta_6 + Volatility_{j,t}\beta_7 + Firm{-}Year\,Dummies_{j,t}\beta_{j,t} + \varepsilon_{i,j,t,n}. \quad (8)$$

The dependent variable $ForecastQuality_{i.j,n}$ is either the unsigned-scaled error (accuracy, panel A) or signed-scaled error (bias, panel B) of analyst i's nth forecast for firm j's annual EPS for fiscal year t. The *TopTier* bank dummy is 1 if analyst i works at one of the ten 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 equity issue volume in billions of (1990 real) dollars. The $TopTier_{i,t}{}^{*}$ $\ln(UWVolume)_t$ is the interaction term. The variable ln(*coverage*) is the natural log of the number of stocks that analyst i covers in year t. 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 fiscal 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-weighted index) in the 120-day period prior to the forecast date. For brevity, coefficients on the control variables are suppressed for all but the first subpanels (for the all-sector sample); they are always included in the regressions and are qualitatively and quantitatively similar in all specifications. The Huber/White heteroskedasticity-consistent covariance estimators are used. ***, **, and * denote that the coefficient is statistically significantly different from zero at the 1%, 5%, and 10% levels, respectively.

Table 12
Effects of personal reputation with firm-year dummies

	Panel A: Accuracy (unsigned errors)						Panel B: Bias (signed errors)					
	All		Top-tier banks		Lower-tier banks		All		Top-tier banks		Lower-tier banks	
	Estimate	t-stat	Estimate	t-stat	Estimate	t-stat	Estimate	t-stat	Estimate	t-stat	Estimate	t-stat
					All sectors							
AA	0.0010	0.41	0.0026	1.11	−0.0042	−1.43	0.0051	1.75 *	0.0089	2.73***	0.0077	1.79*
AA * ln(*UWVolume*)	−0.0008	−1.33	−0.0009	−1.31	0.0005	0.76	−0.0017	−2.36**	−0.0024	−2.72***	−0.0023	−2.17**
ln(*coverage*)	0.0007	3.15***	0.0003	0.59	0.0006	2.45**	−0.0002	−0.95	0.0000	−0.06	−0.0004	−1.47
ln(*distance*)	0.0166	81.56***	0.0175	46.15***	0.0164	67.20***	0.0102	45.39***	0.0104	24.64***	0.0101	37.65***
Firm Size	−0.0389	−8.91***	−0.0436	−4.89***	−0.0378	−7.41***	−0.0744	16.99***	−0.0859	−9.59***	−0.0714	−13.95***
Leverage	0.0868	4.60***	0.1905	4.81***	0.0594	2.70***	0.1404	7.32***	0.2501	6.21***	0.1107	4.96***
Volatility	−0.1589	−3.02***	−0.0500	−0.44	−0.2209	−4.33***	−0.6644	−10.55***	−0.6528	−4.97***	−0.7021	−10.71***
Constant	0.2387	7.83***	0.2567	4.17***	0.2353	6.61***	0.5129	16.75***	0.5946	9.60***	0.4919	13.77***
Firm-Year Fixed Effects	Yes		Yes		Yes		Yes		Yes		Yes	
N	801,137		207,586		593,551		801,137		207,586		593,551	
R^2	0.7888		0.7989		0.8176		0.7069		0.7244		0.7345	

(*continued overleaf*)

Table 12
Continued

	Panel A: Accuracy (unsigned errors)						Panel B: Bias (signed errors)					
	All		Top-tier banks		Lower-tier banks		All		Top-tier banks		Lower-tier banks	
	Estimate	*t*-stat	Estimate	*t*-stat	Estimate	*t*-stat	Estimate	*t*-stat	Estimate	*t*-stat	Estimate	*t*-stat
					Tech sector							
AA	−0.0032	−2.68***	−0.0014	−0.66	−0.0053	−3.37***	0.0004	0.30	0.0029	1.04	−0.0032	−1.62
AA * ln(*UWVolume*)	0.0003	0.41	0.0000	−0.01	0.0013	1.44	−0.0008	−0.89	−0.0017	−1.13	0.0011	0.91
N	170,783		40,026		130,757		170,783		40,026		130,757	
R^2	0.7256		0.7643		0.7248		0.6118		0.6871		0.6055	
					Non-tech sector							
AA	0.0026	0.81	0.0047	1.67*	−0.0030	−0.76	0.0069	1.76*	0.0118	2.90***	0.0120	2.04**
AA * ln(*UWVolume*)	−0.0013	−1.49	−0.0016	−1.77*	0.0003	0.27	−0.0025	−2.26**	−0.0035	−2.91***	−0.0038	−2.37**
N	630,354		167,560		462,794		630,354		167,560		462,794	
R^2	0.8019		0.8030		0.8394		0.7273		0.7291		0.7656	

This table reports results from regressing forecast quality on the AA status, an interaction term between AA status and aggregate underwriting volume, and the control variables. The specification is

$$ForecastQuality_{i,j,t,n} = \alpha + AA_{i,t}\beta_1 + (AA_{i,t}{}^{*}\ \ln(UWVolume_t))\beta_2 + \ln(\text{coverage})_{i,t}\beta_3 + \ln(distance)_{i,j,t,n}\beta_4 + FirmSize_{j,t}\beta_5 + Leverage_{j,t}\beta_6 + Volatility_{j,t}\beta_7 + Firm - YearDummies_{t,t}\beta_{t,t_j} + \varepsilon_{i,j,t,n}. \quad (9)$$

The dependent variable $ForecastQuality_{i,j,n}$ is either the unsigned-scaled error (accuracy, panel A) or signed-scaled error (bias, panel B) of 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 AA on the forecast date, and 0 otherwise. The variable ln(*UWVolume*) is the natural log of the annual total equity issue volume in billions of (1990 real) dollars. The variable *AA** ln(*UWVolume*) is the interaction term. The variable ln(*coverage*) is the natural log of the number of stocks that analyst *i* covers in year *t*. 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 fiscal 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. For ease of presentation, coefficients on the control variables are suppressed for all but the first subpanels (for the all-sector sample); they are always included in the regressions and are qualitatively and quantitatively similar in all specifications. The Huber/White heteroskedasticity-consistent covariance estimators are used. ***, **, and * denote that the coefficient is statistically significantly different from zero at the 1%, 5%, and 10% levels, respectively. The Huber/White/sandwich estimator of variance is used for the coefficient estimates.

any other reputation group alone.[37] Effectively, we throw out more forecasts made by lower-tier non-AAs than any other groups with the new specification. Thus, it is not surprising that this specification disproportionately reduces the explanatory power of the bank reputation dummy in the comparison between lower-tier non-AAs and top-tier non-AAs (Table 11). In contrast, our main results concerning the relative performance of top-tier AAs versus top-tier non-AAs remain robust to this specification (Table 12), because top-tier AAs tend to cover stocks that are covered by many other analysts (and the within-firm-year variation is still significant).

In summary, the analysis here shows that AAs cover stocks that are covered by many other analysts, and their relative performance against other analysts improves during hot markets. The alternative specification confirms our main finding and shows that it is not driven by AAs covering thinly covered and "easy-to-forecast" stocks during peak years.

5. Conclusion

In the course of the recent scandals in sell-side research, some of the best names on Wall Street were singled out and charged with abuse of public trust. A relevant economic question is whether reputation, personal and institutional, mitigates or exacerbates the conflict of interest problem that is widely believed to be present in sell-side research. In other words, do reputable analysts act less or more opportunistically when general temptation is at its peak?

We tackle this question by studying whether the quality differentials in earnings forecasts between reputable and nonreputable analysts vary over time with the severity of conflicts of interest, which rises during peaks of the new issues market when large amounts of underwriting-related compensation are at stake. If reputation only serves as a proxy for research quality and has no additional disciplinary effect, then the quality differentials between reputable and nonreputable analysts should stay constant over time. If reputation mitigates or exacerbates the conflict of interest problem beyond being an indicator of skill, then the quality differentials would vary with the severity of the conflicts of interest.

Our findings suggest that personal reputation is an effective disciplinary device against conflicts of interest, while bank reputation alone is not. Specifically, the accuracy of non-AA analysts working at top-tier investment banks (i.e., analysts with no personal reputation but with a bank reputation conferred upon them) significantly deteriorates relative to top-tier AA analysts or to lower-tier non-AA analysts during market peaks compared to an average year. This result holds whether we measure research quality using forecast accuracy or bias. Since the payoff from generating underwriting revenue is largest at these

[37] Subsequently, most (98%) of the forecasts effectively excluded by the firm-year fixed effects specification are made by lower-tier non-AA analysts. In comparison, 66% of all forecasts in the overall sample are made by lower-tier non-AA analysts.

top-tier investment banks (large underwriters) and during boom underwriting periods, this time-varying pattern strongly supports the view that conflicts of interest distort information transmission by non-AAs at top-tier banks during market peaks. In contrast, AAs working at top-tier banks (i.e., analysts with both a personal and a bank reputation) become more accurate and either less positively biased or exhibit no change in their bias relative to both non-AAs at top-tier banks and AAs at lower-tier banks during hot markets compared to normal times.

The contrasting results on top-tier non-AAs and top-tier AAs suggest that bank reputation is less effective than personal reputation as a disciplinary device against the conflict of interest. Bank reputation serves as an additional disciplinary device only among AA analysts at top-tier banks, as these analysts' accuracy is found to improve even relative to the AAs at lower-tier banks. We conjecture that the difference in the effectiveness of personal reputation and bank reputation as disciplinary devices may be explained by an agency problem. An individual analyst's personal reputation is human capital over which the analyst has full rights, and it is portable. Thus there is no incentive-alignment problem in preserving its value. An analyst faces a much bigger agency problem in preserving the employer's reputation, because the analyst does not fully control this capital; the analyst can free-ride in good times or walk away in bad times. High labor mobility may thus make it particularly difficult for non-AA analysts to rationally care about preserving the bank's reputation especially during market booms. The finding that bank reputation heightens AAs' (but not non-AAs') incentives to produce accurate and less biased research may reflect AAs' stronger personal identification with the bank's future prosperity. Anecdotal evidence suggests that it is not uncommon for AAs to become managing directors or partners of the investment banks they work for. We leave more in-depth research on these questions to future work.

Overall, our conclusion that personal reputation is an effective disciplinary device against the conflicts of interest contradicts the highly publicized image of a corrupt star analyst. Instead, our findings point at a different culprit that exacerbates conflicts of interest: the combination of a *lack* of personal reputation *and* the lure of large short-term profits. This combination appears to undermine the research quality of non-AA analysts at top-tier banks during market peaks. As a result, the positive link between performance and institutional reputation weakens in hot markets, while the link between performance and personal reputation strengthens in such times.

These findings 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.[38] In light of our

[38] Morgan Stanley announced in 2003 that it would no longer use analysts' *Institutional Investor* rankings to gauge performance. It also stopped providing photographs of its analysts to the magazine (*The Wall Street Journal*, 23 November 2004).

finding that not only is the AA title associated with superior research quality on average but this superiority also becomes more pronounced in boom years when conflicts of interest are severe, we argue that this recent move to downweight the AA title in performance evaluation may actually do a disservice to investors, because analysts have fewer incentives to strive for and maintain research quality once they are deprived of the rewards of being a star analyst. Instead, investors might be better served by mandatory disclosures of both institutional and personal reputation measures (as well as past research quality history) so that investors can fully incorporate the time-varying link between research quality and the two types of reputation into their evaluation models. To this end, we support the mandates of the Global Research Analyst Settlement that improve investors' access to these types of information on individual analysts.

Appendix

A.1 Research quality measures

In defining the measures of research quality, we need to take into account two considerations. First, the main allegation in the controversy surrounding the conflicts of interests is that the conflicted analysts were positively biased. This suggests that a signed error may be an appropriate measure of research quality, with lower bias being associated with higher quality. At the same time, the prior literature has often used accuracy, or absolute error, of analyst forecasts as an alternative measure of research quality.[39] This is also consistent with how buy-side managers (and third-party evaluators such as *The Wall Street Journal*) assess analysts' forecast quality.[40] Loh and Mian (2006) recently document that forecast accuracy is positively related to recommendation value, supporting the view that absolute forecast errors capture the objective quality of research.

Furthermore, on the question of how conflicts of interest affect analysts' forecasts, the literature has shown that conflicts can result in both positive and negative biases in forecasts, thus challenging the view that lower bias equals higher quality. Dugar and Nathan (1995) and Dechow, Hutton, and Sloan (2000), for example, find that affiliated analysts, who are more likely to face conflicts of interest, are more optimistic in their earnings forecasts than nonaffiliated analysts. Chan, Karceski, and Lakonishok (2007), among others, document that analysts' desire to win investment banking business leads them to low-ball earnings forecasts shortly before the earnings releases in order to help company managers beat forecasts.[41] The difference between these findings seems to hinge on the forecast horizon examined. Lin and McNichols (1998) document that affiliated analysts do not make more favorable forecasts for current- and subsequent-year earnings than unaffiliated analysts, but they do make significantly more favorable long-term growth forecasts. Thus, conflicts of interest are more likely to result in a positive bias only in relatively long-horizon forecasts.

[39] Others have also examined, among other things, bias in recommendations (e.g., Ljungqvist, Marston, and Wilhelm 2006) and investment values of analysts' stock recommendations (e.g., Fang and Yasuda 2008).

[40] Prior research that uses accuracy as a research quality measure includes, among others, Stickel (1992); Keane and Runkle (1998); Mikhail, Walther, and Willis (1999); and Ljungqvist, Marston, Starks, Wei, and Yan (2007). In both *Institutional Investor*'s AA election and *The Wall Street Journal*'s evaluation of analysts, the criterion related to earnings forecast is the accuracy (i.e., absolute error) of forecasts.

[41] Also see Bernhardt and Campello (2002); Skinner and Sloan (2002); and Abarbanell and Lehavy (2003), for related analyses.

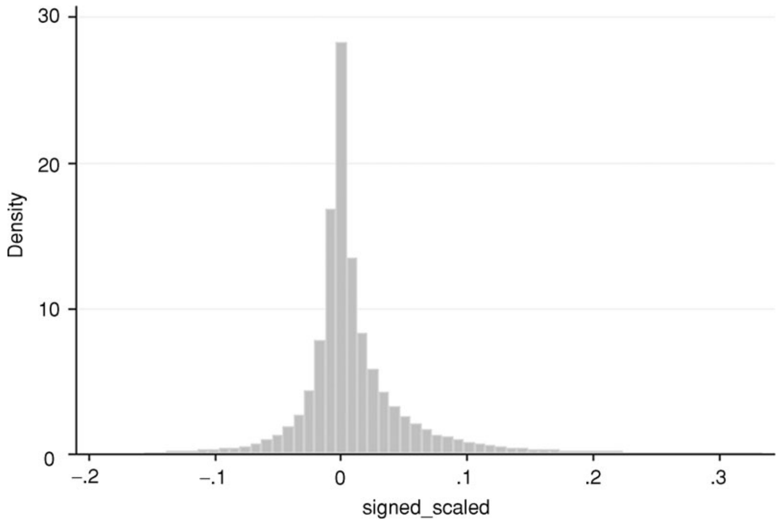

Figure A.1
Histogram of Signed Forecast Errors
This graph plots the signed scaled forecast error, defined as EPS forecast minus actual release EPS scaled by book value of equity.

Table A.1
Bias in early and late forecasts

	N	Fraction positive	*t*-statistic for equality
		Panel A: Frequency test	
Early forecasts	28,842	0.54	34.53
Late forecasts	26,172	0.42	
		Panel B: Magnitude test	
Early forecasts	28,466	0.0184	29.31
Late forecasts	25,798	0.0056	

This table reports results of *t*-tests on whether early forecasts are more positively biased than late forecasts. For each firm and each forecast horizon (fiscal year), late forecasts are defined as those made within 90 days of the release date of actual earnings. Panel A presents the *t*-test result on whether the fractions of positive errors are the same among early forecasts and late forecasts. Panel B presents the *t*-test result on whether the means of signed forecast errors are the same among early forecasts and late forecasts.

Figure A.1 plots a histogram of signed forecast errors in our sample.[42] From this graph, we can clearly see that forecast errors (defined as forecasts minus actual earnings, scaled by book-value of equity) have a positive skew. In unreported tests, the null hypothesis that forecasts are unbiased is overwhelmingly rejected in favor of a positive bias. This positive bias reflects the overall optimism among analysts that has been extensively documented.

Interestingly, consistent with prior evidence, the degree of bias changes over the forecast horizon. Earlier forecasts are generally more positively biased than later forecasts. Table A.1 shows two simple tests illustrating this point. Defining "late" forecasts as those made within ninety days of the earnings release date, we find a positive bias (i.e., the forecast is bigger than the actual released

[42] All forecasts are fiscal year-end forecasts. Details on our data sample are discussed in Section 3 and Appendix A.2.

earnings) in 56% of early forecasts, and in only 42% of late forecasts. In terms of magnitude, while early forecasts have a 1% positive bias on average, late forecasts have only about a 0.55% positive bias. The probability that both measures are equal in the two samples is virtually zero.

Relevant to our study, these features in the data imply that when analysts face a trade-off between truth telling (which enhances their long-term reputation) and manipulation (which is driven by conflicts of interest), forecast errors, as a two-sided deviation from the true earnings, should unambiguously rise with conflicts of interest. In contrast, the relationship between forecast biases and conflicts is less clear-cut, complicated by factors such as the timing of the forecasts.

Given the features of the data reflecting the two-sided sources of conflicts as discussed above, we use both forecast accuracy and bias as alternative measures of forecast quality.

A.2 Data Sources and Variable Definitions

Information on analysts' fiscal year-end earnings forecasts is obtained from the I/B/E/S Detailed History file.[43] Comprehensive data coverage by I/B/E/S starts in 1983; thus, our sample also starts in 1983. Firm characteristics and stock prices are obtained from Compustat and CRSP.

For an observable measure of an analyst's personal reputation, we use the AA designation from the *Institutional Investor* magazine. In this survey, buy-side managers evaluate the analysts along such dimensions as industry knowledge, timely and informative research reports, accuracy of their earnings forecasts, and profitability of their stock recommendations.[44] The survey results lead to the annual election of AA analysts, featured in the magazine's October issues. We hand-collect the AA list from each October issue of the magazine during our sample period and carefully match AA analysts' names with names in the Translation file from I/B/E/S. Out of the entire sample of 1376 distinct AAs over the twenty-year period, we were able to match 1121 analysts with I/B/E/S forecast data.

For an observable measure of bank reputation, 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 Burnham Lambert, First Boston Corporation, Goldman Sachs & Company, Hambrecht & Quist, Merrill Lynch, Morgan Stanley & Company, Paine Webber, Prudential-Bache, and Salomon Brothers. Thus, there are both large underwriters and some small but prestigious names in this top-tier list.

The Carter-Manaster measure is constructed using historical records of banks' tombstone positions between 1979 and 1991.[45] We deliberately use this long-term measure of bank reputation to avoid a potential reverse-causality problem. If we use a short-term measure of bank reputation based on, for instance, the trailing-twelve-month market shares, then a sudden (incorrect) bullishness of analysts at a particular bank could *cause* the bank's rank to rise. While this situation corresponds to a negative relationship between observed bank reputation and analyst accuracy, we cannot correctly infer causality. In contrast, the long-term measure we use is relatively exogenous with respect to the short-term actions of individual analysts at these banks.

Finally, to measure market-level underwriting activity, we compile from SDC the *market-wide* equity underwriting volume for each year in our sample and deflate the figures to arrive at the real underwriting volumes in 1983 dollars. Since market-wide underwriting volume is positively correlated with the aggregate underwriting-related compensation among sell-side analysts, this variable reflects the severity of conflicts of interest faced by *all* analysts.

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[43] 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.

[44] The survey coverage is quite comprehensive. In 2002, for example, the survey was sent to more than nine hundred institutions; over thirty-five hundred individuals from about six hundred firms responded, accounting for more than 90% of the one hundred largest U.S. equity managers (*Institutional Investor,* October 2002).

[45] Tombstones are the announcements of security offerings. The position at which an investment bank's name appears on these documents is seen on Wall Street as an indication of the bank's prestige among the banks in the underwriting syndicate. The Carter-Manaster measure of bank reputation is constructed from these positions.

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Erratum

In "The Effectiveness of Reputation as a Disciplinary Mechanism in Sell-Side Research," by Lily Fang and Ayako Yasuda (v. 22 n. 9 pp. 3735–3777), one of the figures in Figure 2 was mislabeled.

The title for the third figure at the bottom of p. 3760 should read, "Top-tier, AA Pool" rather than "Top-tier, non-AA Pool". The first figure at the top of p. 3760 is already correctly labeled as "Top-tier, non-AA Pool."

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