Analyzing the Analysts:
Career Concerns and Biased Earnings Forecasts

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Abstract

This paper examines the career concerns of security analysts. We relate long histories of their earnings forecasts to job separations. We find that relatively accurate past forecasts lead to favorable career outcomes such as remaining at or moving up to a high status (large, prestigious) brokerage house. Controlling for accuracy, optimistic forecasts relative to the consensus increase the chances of favorable job separations. Job separations depend much less on accuracy for analysts who cover stocks that are underwritten by their brokerage houses. Such analysts are also much more likely to be rewarded for optimistic forecasts than other analysts. Furthermore, job separations were much less sensitive to accuracy and somewhat more sensitive to optimism during the stock market mania of the late 1990s. These findings suggest that the well-documented analyst forecast optimism bias is likely due to incentives to promote stocks.

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1. Introduction

Many in the financial press call the decade of the nineties the "Age of the Analysts" on Wall Street (see, e.g., Nocera (1997), Cole (2001)). Once relegated to producing boring reports on stocks in the backrooms of brokerages, analysts are now an integral part of Wall Street profit centers. Through media outlets such as CNBC, analysts reach millions of individual investors. At the same time, investment bankers rely on analysts to help them land investment-banking deals. Analysts who are influential among institutional buyers such as mutual fund managers can generate hefty trading commissions for their brokerages.

The growing prominence of these analysts in financial markets has led to heightened scrutiny of their career concerns. This scrutiny has recently reached a peak as well-known e-commerce analysts such as Mary Meeker and Henry Blodget were criticized for maintaining buy ratings on many dot-com stocks even as their once-sky high valuations collapsed. Congressional hearings are underway to consider reforms to protect naïve individual investors who lost money as a result of these over-optimistic recommendations. These hearings to "analyze the analysts", as some in financial press are calling it, are looking into the career concerns of analysts and the conflicts of interest that lead them to compromise the accuracy of their predictions (see Kane (2001)).

A number of regulators and financial economists argue that an analyst's career advancement has little to do with predicting accurately. They cite evidence that analysts' forecasts are typically optimistically biased in that misses tend to be above actual earnings (see Brown, Foster and Noreen (1985), Stickel (1990), Abarbanell (1991), Dreman and Berry (1995), Chopra (1998)). There is also some evidence that an analyst from a brokerage house that has an underwriting relationship with a stock tends to issue more positive predictions than analysts from non-affiliated houses (see Dugar and Nathan (1995), Dechow, Hutton and Sloan (1997), Lin and McNichols (1998), Michaely and Womack (1999)). While there is no direct evidence that these optimistic predictions are due to analyst incentives as opposed to other explanations, surveys indicate that analysts' rewards do depend on their ability to generate investment banking business and trading commissions.[1] These two sources of rewards can lead analysts to compromise the objectivity and accuracy of their predictions by issuing optimistic forecasts so as to promote stocks.[2]

On the other hand, practitioners such as brokerage house research directors counter that the accuracy of analysts' predictions is important for their career prospects. They point out that brokerage houses want analysts who are influential among the buy-side and this influence is ultimately tied to making the right calls. Put another way, even if the compensation of analysts does not depend explicitly on forecast accuracy, to generate investment banking business or trading commissions in the longer run, analysts need to cultivate a reputation for forecasting expertise among the buy-side. Indeed, an analyst's place in the profession depends critically on an annual poll conducted by the magazine *Institutional Investor* of money managers. The top three vote getters in each industry are called *All-Americans* and are highly rewarded for this honor. While some call this poll a "beauty contest" because analysts are known to lobby money managers heavily before the poll, two of the most important criteria for a high ranking in this poll are, nonetheless, perceived expertise in earnings forecasts and stock picking.

Other than such anecdotal evidence from surveys or financial press, there are few systematic studies of the determinants of analyst career concerns. Are analysts punished for inaccurate earnings forecasts? Are they rewarded for issuing optimistic recommendations? How sensitive are rewards to such forecast behaviors and what do these sensitivities vary with? Answers to such questions would help us to understand the career paths and reward systems in place for these important information monitors in the economy. Ultimately, such work might also help us better understand the determinants of analysts' forecasts and determine the appropriate level of regulation of analysts' activities.

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[1] Other explanations for the optimism bias not based on conflicts of interest include cognitive biases (see DeBondt and Thaler (1990), Abarbanell and Bernard (1992), Kahneman and Lovallo (1993)) and selection bias (see McNichols and O'Brien (1997)). Attempting to discriminate among these explanations, Michaely and Womack (1999) surveyed a small sample of about thirty investment professionals on which of these explanations was most plausible. The respondents favored the conflicts of interest explanation.

[2] Investment bankers who are bringing an IPO to market want optimistic forecasts to place the shares at high prices. Stock brokers want optimistic forecasts to get new buyers and hence earn trading commissions since not many institutions or individuals are willing to short.

In this paper, we attempt to measure the career concerns of security analysts using a large panel of information on the brokerage house employment and earnings forecast histories of roughly 12,000 analysts working for 600 brokerage houses between the years of 1983 and 2000. Among brokerage houses, there is a well-defined hierarchy of prestige, with investment banking powerhouses such as Goldman Sachs or Merrill Lynch considered high status and more regional and specialized brokerage houses considered lower status. Although we do not directly observe the compensation of the analysts in our data set, the status of the brokerage house an analyst works for is highly correlated with that compensation. Therefore, movements of analysts across brokerage houses of different status over time allow us to indirectly measure whether an analyst has experienced positive or negative career outcomes. And while analysts perform many tasks, among the most important is generating earnings forecasts.[3] The crux of our analysis is to develop regression specifications to relate analyst job separations to their past earnings forecast behaviors.

We begin our empirical investigation by examining the effect of earnings forecast accuracy on job separations. We find that accuracy is inversely related to the probability of an analyst moving down the brokerage house hierarchy. For instance, extremely inaccurate relative forecasts increase an analyst's chances of experiencing such an unfavorable career outcome by about 62%. We also find that accuracy is positively related to the probability of an analyst moving up the hierarchy. Extremely accurate relative forecasts increase an analyst's chances of experiencing such a favorable outcome by 52%. These effects are economically and statistically significant.[4] This evidence suggests that the labor market does punish the average analyst for inaccurate forecasts, contrary to the some suggestions in the financial press that

[3] One reason is because the buy-side cares about whether a company will make its quarterly earnings forecasts. Another is that analysts can more finely signal their views on stocks with earnings forecasts than with the buy, hold or sell recommendations (see Nocera (1997)).

[4] This evidence is consistent with findings that analysts exert effort in producing earnings forecasts and stock recommendations. These findings include analysts' earnings forecasts being superior to those of mechanical time series models (see, e.g., Elton and Gruber (1972), Brown and Rozeff (1978), Crichfield, Dyckman and Lakonishok (1978)) and recommendations having some investment value in that they seem to predict stock returns in the short-run (see Stickel (1995), Womack (1996)).

analysts' rewards are not tied to accuracy. In other words, analysts cannot be so wildly optimistic as to entirely ignore the tradeoff with accuracy and long-run consequences for their reputation.

However, controlling for accuracy, we find that analysts who issue relatively optimistic forecasts (forecasts greater than the consensus) are more likely to experience favorable job separation outcomes. Analysts who issue a large fraction of optimistic forecasts on the stocks that they follow are 38% less likely to move down the brokerage house hierarchy and 90% more likely to move up the hierarchy. These effects are both economically and statistically significant.

Because few low status brokerage houses do much underwriting, the most plausible interpretation of the finding that analysts at low status houses move up the hierarchy on relatively optimistic forecasts is that they are being rewarded for promoting stocks which presumably helps generate trading commissions. For analysts at high status brokerage houses, underwriting relationships may be an additional source of the optimism bias. Analysts who follow stocks that are underwritten by their brokerage houses may face even steeper incentives to sacrifice accuracy with optimistic predictions than the typical analyst. We find that this is indeed the case. The sensitivity of movements down the hierarchy to forecast accuracy is significantly attenuated for analysts who cover stocks that are underwritten by their brokerage houses, while the sensitivity of movements down the hierarchy to forecast optimism is significantly larger for these analysts.

Additionally, we examine whether these sensitivities differ between the sub-sample periods of 1986-1995 and 1996-2000. We find strong evidence that accuracy matters much less for career concerns in the 1996-2000 than in the earlier period and somewhat weaker evidence that forecast optimism also matters more for career concerns in the later period. These findings are consistent with observations in the financial press that brokerage houses threw whatever concern they had for objectivity in their research out the window in the midst of the stock mania of the late 1990s as the job description for being an analyst became more tied than ever to promoting stocks (see Tully (2001)).

Finally, we consider an alternative measure of career concerns not based on job separations. Brokerage houses have some discretion in assigning analysts to cover certain important stocks. For instance, brokerage houses can allocate their software or Internet analysts to follow Microsoft. We attempt to measure whether accuracy and forecast optimism affect who within a brokerage house is assigned to cover these important stocks that have large market capitalization or large analyst following. The results are qualitatively similar to those using job separations as proxies for career concerns, though the effects are not as economically large. Nonetheless, these findings suggest that internal labor markets (within brokerage houses) also provide some implicit incentives.

Our paper proceeds as follows. In Section 2, we review the related literature and highlight the contributions of our paper in light of existing work. Section 3 describes our data. Section 4 constructs measures of the brokerage house hierarchy and forecast behaviors. We analyze the relationships between our job separation measures and forecast behaviors in Section 5 and consider an alternative measure of career concerns in Section 6. We conclude in Section 7 with answers to some questions of current interest and some directions for future research.

2. Related Literature

In this section, we relate our paper to and briefly discuss our contributions in light of the related literature. While we know much about the properties of analyst forecasts, we know little about how rewards depend on them. There are a few exceptions. Stickel (1992) finds that *All-American* analysts (who are typically better compensated than other analysts) are more accurate earnings forecasters and tend to revise their forecasts more than other analysts, suggesting that accuracy is rewarded. More recently, Mikhail et al. (1999) document that poor relative performance leads to job turnover; however, they do not distinguish between job separations related to movements up or down the brokerage hierarchy.

The paper closest to ours is Hong, Kubik and Solomon (2000) who test herding models along the lines of Scharfstein and Stein (1990), Trueman (1994), Zwiebel (1995) and Prendergast and Stole (1996). Hong et al. (2000) find that young analysts are more likely than their older counterparts to leave the profession for poor forecast accuracy and bold forecasts. Moreover, they find that young analysts are less bold than their older counterparts, consistent with the predictions of reputation-based herding models.[5]

Our paper differs from these studies in a number of important ways. First, we focus on documenting the career concerns of security analysts arising from movements up and down the brokerage house hierarchy. Without data on compensation, these movements are among the best proxies available for career concerns. Such measures of implicit incentives are better than merely looking at how performance affects movements out of the profession because it is difficult to track what happens to an analyst when she leaves the profession. Also, we are able to corroborate our findings with an alternative career concern measure arising from internal labor markets.

Second, our findings on the relationship between job separations and forecast optimism have not been previously documented. These findings strongly suggest that the analyst optimism bias is connected with analyst incentives to promote stocks, especially those underwritten by their brokerage houses. An alternative explanation for the optimism bias is that management shuts out analysts who issue forecasts different than what management wants.[6] It is not clear that this access-to-management story explains the optimism bias since it assumes that managements on average want analysts to be optimistic. On the contrary, many companies want analysts to be pessimistic so that the company can beat earnings forecasts (see Cole (2001)). Nevertheless, as we discuss below, several of our findings run counter to various versions of this story. Hence, our paper improves our understanding of the analyst optimism bias.

3. Data

3.1. Our Sample of Analysts

Our primary data come from the Institutional Brokers Estimate System (I/B/E/S) database. I/B/E/S gathers the earnings forecasts of companies throughout the world from thousands of individual

[5] Chevalier and Ellison (1999) and Lamont (1995) find similar results for mutual fund managers and macroeconomics forecasters, respectively.

security analysts. We use the I/B/E/S Detail Earnings Estimate History File, which contains earnings forecasts of U.S. companies between 1983 and 2000. During this period, the data consist of the estimates of 12,336 analysts, working for 619 different brokerage houses and covering 8,441 firms.

We can track the behavior of individual analysts in the I/B/E/S sample. At each point in time, we can identify the stocks that these analysts follow (*i.e.* the firms they issue earnings forecasts on) and the brokerage houses that employ them. Generally, analysts tend to specialize and cover firms in the same industry. On average, an analyst in I/B/E/S follows about 9.3 firms in a year, with a standard deviation of about 8.3 firms. In addition, we have a comprehensive record of their forecast histories, allowing us to construct past forecast behavior measures (see Section 4 below).

Because we know where an analyst is employed when she issues an earnings forecast, we can measure how many analysts a particular brokerage house employs at each point in time (i.e., the size of the brokerage house). Table 1 provides some summary statistics (for each year in the sample) of the size of brokerage houses. The number of brokerage houses in existence has increased over time, from only 90 in 1983 to over 300 in 2000. Also, the average size of brokerage houses has fallen over time. In 1983, the average size of a brokerage house was about 21 analysts, compared to slightly over 11 in 2000. These numbers reflect the increasing numbers of smaller brokerage houses that specialize on certain industries as opposed to the traditional full service brokerage houses.

The I/B/E/S database does not explicitly record the number of years that an analyst has been working. Because we are interested in how the forecasts and job separations of analysts vary with their experience, we only examine analysts for whom we can calculate the number of years they have been working as analysts. Because our I/B/E/S sample begins in 1983, we know the experience level of all analysts who begin their career after 1983. Therefore, we exclude all left-censored analysts from our subsequent analysis (the samples for Tables 2-12 below exclude all left-censored analysts).

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[6] There is some evidence that the optimism bias exhibits cross-sectional variation related to analyst and company characteristics and that these variations may be consistent with an access-to-management story (see Francis and Philbrick (1993), Das, Levine and Sivaramakrishnan (1998), Lim (2001)).

In this sample, an analyst remains in our I/B/E/S sample for over 4 years on average, with a standard deviation of about 4 years. Some are in the database only one year (the 10th percentile of the distribution), either because they quickly left the profession, they switched to a brokerage houses not covered by I/B/E/S (though this is very unlikely since the vast majority of brokerage houses submit the forecasts of their analysts to I/B/E/S), or they began their career in 2000. However, a number of analysts are in the sample for the entire 17-year period between 1984 and 2000. The 90th percentile of the distribution is 11 years.

3.2. Measures of Brokerage House Status

With these basic facts about security analysts in mind, we next construct measures of the brokerage house hierarchy. There is a discernible ladder of prestige in brokerage houses. At the top of this hierarchy are well-known names such as Goldman Sachs and Merrill Lynch. Such brokerage houses are the elite powerhouses of Wall Street that have large investment banking businesses. They tend to employ many analysts because they do business in all types of industries. At the other end of the spectrum are brokerage houses that specialize in covering specific industries (e.g. high-tech) or types of stocks (e.g. small cap). Such brokerage houses tend to be more numerous and more regional in nature and cater to institutional investors, providing research in exchange for trading commissions from those investors. They tend to be smaller and hire fewer analysts. Security analysts' wages at top-tier brokerage houses are substantially higher than at lower status houses; wages at top-tier houses are highly skewed and can exceed $15 million per year (see, e.g., Nocera (1997), Elkind (2001)).[7] While measures of prestige are somewhat arbitrary, market participants readily agree that only a small number of traditional banking powerhouses such as Goldman Sachs or Merrill Lynch belong in the top tier.

[7] Krigman, Shaw and Womack (2000) describe one reason why prestigious brokerage houses might value influential analysts. They find that firms that switch underwriters after their initial public offerings do so in part because they want to graduate to higher reputation analysts. They strategically buy additional and influential analyst coverage from the new lead underwriter.

In this paper, our primary measure of this brokerage house hierarchy is derived from a brokerage house prestige ranking published by *Institutional Investor* (I.I.). Each year in the October issue of I.I., the ten or so brokerage houses with the most *All-Americans* are listed as "The Leaders". We classify the top ten houses in this annual poll as high status and other brokerage houses as low status for that year. This measure of status is also used in Phillips and Zuckerman (1999), a sociological study that provides additional discussion on various measures of status among brokerage houses.

In addition, we have also re-run all of our analyses using two other measures of status. One of these is based on the size (the number of analysts employed by) of a brokerage house. As mentioned above, the traditional investment banks tend to hire more analysts than smaller, specialized brokerage houses. However, prestige is likely not linear in house size. That is, a brokerage house with 30 analysts is not likely to be significantly more prestigious than one with 25 analysts. Therefore, in this second measure of brokerage house status, we classify the ten biggest brokerage houses each year as the high-status houses and the rest as low-status houses for that year.

The other measure is the well-known Carter-Manaster measure of the investment banking hierarchy using underwriters' relative placements in stock offering "tombstone" announcements (see Carter and Manaster (1990) and Carter, Dark and Singh (1998)). Carter and Manaster (1990) provide descriptions of why an underwriter's reputation is reflected in its position in these announcements and how their rankings (a number between 0 (the least prestigious) and 9 (the most prestigious)) are constructed. We take from the appendix of Carter, Dark and Singh (1998) the average Carter-Manaster ranking for brokerage houses between 1985 and 1991. We label the top ten houses in this ranking as high-status brokerage houses. Because we only use one set of Carter-Manaster rankings over our sample period, this measure of prestige does not vary from year to year.

We list the names of the brokerage houses labeled as high status according to the I.I. ranking in Appendix Table 1. Houses in the top ten of the I.I. ranking that are also in the top ten according to the size ranking are denoted with an asterisk after their names. Appendix Table 2 lists the top 10 houses according to the Carter-Manaster ranking, along with their Carter-Manaster ranking, the number of IPOs on which these rankings are based and the average number of analysts employed by these houses during the sample period. It is easy to see from the two appendix tables that these three rankings are correlated and dominated by well-known and large investment banks.

As a check that our rankings are sensible, we report in Table 2 the percentage of analysts employed by our high status houses each year. These houses in aggregate should not employ the majority of analysts; otherwise, there would be little meaning to being a considered a prestigious house. High status houses under the I.I. ranking employ about 23 percent of the analysts and this figure is relatively stable over the 17-year period. Similar numbers obtain for the other two status measures, with high status houses according to the size ranking employing a somewhat larger fraction of analysts.

Since our three status measures are quite related, it should come as no surprise that our analyses below are robust to the status measure that we use. For brevity, we only report our results for the I.I. ranking and we alert the reader below wherever there are material differences in our findings across different status measures.

3.3 Underwriting Relationships and All American Status of Analysts

Our analyses below rely on two additional sources of data. From the SDC New Issues Database, we obtain information on initial public offerings (IPOs) conducted between 1983 and 2000, including the date of the offering and the name of the lead underwriter. We merge information on these IPOs with our analyst sample. In each year, we can categorize for each stock that an analyst covers whether her brokerage house has an underwriting relationship with that stock. More specifically, if an analyst issues an earnings forecast on a stock in the year to two after its IPO date and in which her brokerage house is the lead underwriter for the IPO, then we define that stock as having an underwriting relationship with the analyst's brokerage house.

We also collect from the October issue of *Institutional Investor* for 1983-2000 the list of the First Team All-Americans. There are on average about sixty such analysts each year. We will simply refer to them as All-Americans. We will be interested in seeing to what extent the relationship between career concerns and forecast behaviors depends on whether an analyst is covering stocks that have underwriting relationships with her brokerage house and on whether an analyst is an All-American.

4. Measures of Job Separations and Forecast Behaviors

In this section, we describe our measures of job separations and forecast behaviors such as accuracy and optimism.

4.1. Measures of Job Separation

We are concerned with movements of analysts between brokerage houses of different status. We create three job separation measures. First, an analyst in the I/B/E/S data is said to have changed brokerage houses in year *t* if she worked for one brokerage house at the beginning of year *t* and at some point during the year moved to a different brokerage house. Second, an analyst is defined as moving to a higher status brokerage house in year *t* if she was working for a low status brokerage house at the beginning of year *t* and moves at some point during that year to a high status brokerage house. Third, an analyst is described as moving to a lower status brokerage house in year *t* if she was working for a high-status brokerage house at the beginning of year *t* and moves at some point during *t* to a low-status brokerage house. Note that if an analyst's house becomes changes status (e.g. moving in or out of the top ten II ranking), that analyst is not considered to have moved to a high status house since the analyst has not experienced a job separation. Because the compensation at top-tier houses tend to be substantially higher than at low status houses, we will regard a movement up the brokerage house hierarchy as a positive career outcome and a movement down the hierarchy as a negative career outcome.

We provide summary statistics of the various job separation measures for all analysts in I/B/E/S in Table 3. As described above, we are most interested in the analysts who leave their brokerage house but stay in the profession. About 14% of analysts each year change brokerage houses, indicating that there is substantial job mobility across brokerage houses. As a fraction of these movers, about 12% are moves up the hierarchy, about 10% are moves down the hierarchy and the remaining 78% are lateral moves. Of these lateral moves, nearly 73% are analysts moving between low status houses.

Taking a slightly different look at these job separation patterns, around 2% of analysts that started the year at a low status brokerage house move up to a high status brokerage house in any given year. Around 7% of analysts who started the year at a high status brokerage house move down to a lower status brokerage house in a given year. Hence, these numbers suggest that moving up the brokerage house hierarchy or staying at the top is very competitive. Similar numbers hold for analysts with at least three years of experience.

Our measures of job separations do not take into account the possibility that analysts may have switched houses because of mergers. It is not obvious how to deal with separations due to mergers. Presumably, mergers in which the acquiring house gets to decide on which analysts from the target house to retain are informative and should be considered in our analysis. In any case, we have re-done all of our analysis by defining job separations as only those in which the house from which an analyst leaves at year *t* is also in existence at year *t+1*. All of our results are similar to those presented below when we remove the sub-sample of job separations caused by mergers.

4.2. Measures of Forecast Accuracy

4.2.1. Absolute Accuracy

We begin by constructing a measure of the absolute forecast accuracy of an analyst. We define $F_{i,j,t}$ as the most recent (dollar) earnings per share (EPS) forecast of year-end earnings issued by analyst *i* on stock *j* between January 1st and July 1st of year *t*.[8] Our measure of analyst *i*'s accuracy for firm *j* in year *t* is the absolute difference between her forecast and the actual EPS of the firm, $A_{j,t}$:

$$Forecast\ Error_{i,j,t} = \left| F_{i,j,t} - A_{j,t} \right|. \tag{1}$$

[8] We use the most recent forecasts before the cut-off date of July 1st to evaluate the analysts because we need a common time frame to compare different analysts' forecasts (see, e.g., Crichfield, Dyckman and Lakonishok (1978)). Our results are robust to alternative cut-off dates.

Because an analyst generally covers more than one firm in a year, we need to aggregate this forecasting accuracy measure across all the firms that she covers. The simplest way to do this is to just compute the average forecast error of an analyst for the year. However, this measure would be very noisy for analysts that only follow a couple of firms in a year. Hence we construct:

$$Absolute\,Forecast\,Accuracy_{i,t} = \frac{1}{n}\sum_{j\in J} Forecast\,Error_{i,j,t} \tag{2}$$

where n is the number of different firms that an analyst follows in year t and the two previous years and J is the set of firms the analyst covers. That is, the absolute accuracy measure is an average of the analyst's forecast errors on all the firms she covered over the three previous years. Such a longer averaging period will increase the signal-to-noise ratio of our performance measure.

4.2.2. Relative Accuracy

The average absolute forecast error measure is the simplest way of comparing the forecast accuracy of different analysts; however, because analysts cover different firms, even analysts that cover the same industries, this performance measure is problematic. Some firms are more difficult to accurately predict than other firms. An analyst might have a higher absolute forecast error than another analyst either because the analyst did not perform as well as the other analyst or the firms the analyst follows were more difficult to forecast than the firms of the other analyst.

We construct a relative accuracy measure that accounts for these issues. We first sort the analysts that cover a particular stock in a year based on their forecast error given in equation (2). We then assign a ranking based on this sorting; the best analyst (the one with the lowest forecast error) receives the first rank for that stock, the second best analyst receives the second rank and onward until the worst analyst receives the highest rank. If more than one analyst was equally accurate, we assign all those analysts the midpoint value of the ranks they take up.[9] Under this relative ranking system, the analyst that produces

[9] This means that the ranks need not be integers.

the most accurate estimate of Firm A performs as well as the analyst that produces the best estimate of Firm B, regardless of the actual forecast errors of the analysts for the two firms.

We could just use the average rank of an analyst across all the firms she follows as a measure of her overall accuracy for the year. Analysts with a lower average rank would perform better than other analysts. However, this average rank measure might be problematic because the maximum rank an analyst can receive for a firm depends on the number of analysts that cover the firm. Analysts that cover firms that are thinly followed are more likely to have lower average ranks than analysts that follow firms with high coverage regardless of their forecast accuracy. Therefore, we want to scale an analyst's rank for a firm by the number of analysts that cover that firm. We develop a score measure that adjusts for these differences in coverage. The formula for this score is:

$$Score_{i,j,t} = 100 - \left[\frac{Rank - 1}{Number\ of\ Analysts_{j,t} - 1} \right] \times 100 \tag{3}$$

where $Number\ of\ Analysts_{j,t}$ is the number of analysts who cover the firm in a year.[10] An analyst with the rank of one receives a score of 100; an analyst who is the least accurate (and the only one who is least accurate) receives a score of zero. The median and mean score for a firm in a year is 50.

This score measure might be easier to understand with an example. Table 4 presents the forecast errors of eight hypothetical analysts for a given firm in a year and their scores based on their ranks. The best and worst analysts receive a score of 100 and 0 respectively. The second through fourth analyst have the same forecast error (as does the sixth and seventh analyst); therefore, they all receive the same rank of 3, the midpoint of the second through fourth slot (6.5 for the sixth and seventh analyst).

After we calculate scores for every firm covered by the analyst, we need to compute an overall score that reflects the analyst's recent forecast accuracy. We could just take the average of the analyst's scores for the year; however, as with the absolute accuracy measure, this relative measure would be very noisy for analysts that only follow a couple of firms in a year. Therefore, we create the measure *Relative Forecast Accuracy*$_{i,\,t}$ which is the average of the analyst's forecast scores in year *t* and the two previous years.[11] Higher overall scores correspond to better analyst performance. By construction, the average forecast accuracy measure has a mean close to 50. It has a standard deviation of 7.4.

Although we believe both the absolute and relative forecast accuracy measures are reasonable, we need to keep in mind some of their peculiarities. First, certain types of analysts are likely to have extreme average accuracy measures (both good and bad). For instance, analysts that cover few firms over the three-year period are more likely to be in the extremes. One very good or poor performance on a firm will greatly affect their average score. Also, for the relative measure, analysts that cover thinly followed firms are more likely to be in the extremes. For a given firm, it is easier for an analyst to earn a score near 100 or 0 on their relative performance measures if there are few other analysts covering the firm in a year. We need to keep these things in mind when we move to our empirical work because we want to make sure that we are capturing an analyst's accuracy with this score measure and not the types of firms that she follows.

4.3 Measure of Forecast Optimism

Along with these measures of forecast accuracy, we also construct for each analyst a measure of the extent to which her forecasts are optimistic. One possible measure is to consider a forecast as optimistic if the forecast is above actual earnings. However, such a measure is incomplete in that an analyst with a forecast above actual earnings can actually be relatively the most pessimistic among the forecasters if the other forecasters submit higher forecasts. Hence, a sensible measure ought to take into account the optimism bias of the consensus.

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[10] If only one analyst follows a firm in a given year, a score is not calculated for that firm.

[11] Therefore, an analyst must be in at least her third year as an analyst to have a forecast performance measure. We use these three-year averages primarily because they are less noisy proxies of forecasting expertise.

In each year t and for each stock j that an analyst i follows, we create a dummy variable $I_{i,j,t}$ that equals one if the analyst's forecast is greater than the consensus forecast (which is simply the average of the forecasts submitted by other analysts excluding analyst i) and zero otherwise. The average of these dummy variables across the stocks that the analyst covers gives an optimism score for analyst i in year t. As with the accuracy measures, this relative optimism measure would be very noisy for analysts that only follow a couple of firms in a year. Therefore, we create the measure *Relative Forecast Optimism*$_{i,t}$, which is the average of the analyst's forecast optimism scores in year t and the two previous years. Higher overall scores correspond to more optimistic analyst forecasts.

Summary statistics for this overall score is given in Table 4. Importantly, note that an analyst's relative accuracy score and optimism score are negatively correlated (about -0.18) because the consensus forecast tends to be above actual earnings. So analysts with lots of optimistic forecasts above the consensus will tend be relatively more inaccurate.

5. **Relating Job Separations to Forecast Behaviors**

With these measures of job separations and forecast behaviors in hand, we develop a series of empirical models to measure to what extent past forecast behaviors predict future career outcomes.

5.1. Some Characteristics of Job Separations

Before we develop these regression specifications, we point out a couple of important features of job separations. We begin by examining whether analysts cover the same stocks when they change brokerage houses. It is likely that forecasting the earnings of a company accurately requires non-trivial set-up costs. So, if brokerage houses hire analysts (in part) for their forecast expertise, then analysts ought to follow (issue forecasts) roughly the same set of stocks when they change houses. If there were little overlap in the stocks that they cover when they switch houses, this would suggest that accuracy might have little role in determining job separations.

In Table 5, we calculate the percentage of an analyst's portfolio in year *t* that consists of firms that she was not following in year *t-1*. We then examine whether analysts that change brokerage houses have a bigger change in the firms they follow than analysts that stay with their brokerage house. The findings suggest that there is little difference between the change of the stock portfolios of analysts who leave and those who stay. In the entire sample, the percentage of an analyst's stock portfolio that consists of new firms each year is about 28%; these percentages are almost identical for analysts who stay with their brokerage house and analysts who move to another brokerage house. Hence, it appears that brokerage houses do hire analysts for the skills they have developed in their previous job.

In analyzing the relationship between job separations and forecast accuracy, it is also important to keep in mind that job separations may be related to analyst experience. To see whether this is the case, we look to see whether analysts on average move up the brokerage house hierarchy as they gain experience in Table 6. Using a sample of all analysts who are in the I/B/E/S sample for *n* years, we calculate the percentage of those analysts who worked for a high status brokerage house (as measured using the I.I. ranking) their first year, their second year and all the way to their n^{th} year as analysts.[12] For example, column (4) includes in the sample all analysts who are in the I/B/E/S sample at least five years. In their first year, only 19% of this sub-set of analysts worked for high status brokerage houses. This percentage increases to about 23% in year 5. Therefore, we see a funneling up of analysts from low-status to high-status firms as they age; the same pattern holds for cohorts with different minimum number of years in the sample.[13] In other words, top-tier brokerage houses prefer more seasoned analysts; so it is important in our analysis of job separations and forecast accuracy that we carefully control for experience.

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[12] We examine this subset of analysts to avoid the possibility that differential attrition rates between analysts who work for high and low status brokerage houses drive our results. All of the analysts are in the sub-sample the entire period being examined.

[13] Because the number of brokerage houses is increasing over time (as shown in Table 1), if analysts just randomly move across brokerage houses, then we would expect that the percentage of analysts who work for high status brokerage houses to decline as they age. Given that we find the opposite, there appears to be strong evidence that analysts on average move up the brokerage house hierarchy as they gain experience.

5.2. Sensitivity of Job Separations to Forecast Accuracy

It is to this analysis that we now turn. At any year *t*, we only include those analysts that have at least three years of forecast history, the number of years necessary to allow us to calculate our forecast accuracy scores defined in the previous section.[14] In Panel B of Table 3, we report summary statistics for the various job separation measures using this sub-sample of analysts with at least three years of experience and the forecast accuracy measures defined in Section 4.2. In any given year, the probability that an analyst moves from a low status to a high status firm is 2.73% and the corresponding number for movers from a high status to a low status firm is 7.77%.

To capture the relationship between job separation and forecast performance, we begin with the following simple probit model specification:

$$Pr\left(Job\ Separation_{i,t+1}\right) = \Phi\left(\alpha + \beta_1 Forecast\ Accuracy\ Indicator_{i,t}\right) \tag{4}$$

where $Job\ Separation_{i,t+1}$ is an analyst's career outcome, (e.g. whether analyst *i* moves from a low status to a high status brokerage house in year *t+1*), and $Forecast\ Accuracy\ Indicator_{i,t}$ is some function of the analyst's past forecast accuracy measured as of year *t*. We are interested in how an analyst's past forecast accuracy affects the probability that she experiences a particular career outcome.

This simple probit specification is incomplete because there are possible biases in the estimation that need to be controlled for carefully. When we described the construction of our analyst forecast accuracy measures, we noted that analysts who cover firms with thin coverage and analysts that cover few firms are more likely to be in the extremes of forecast performance. If analysts that follow few or thinly covered firms during this window are more or less likely to separate from their jobs for reasons other than their performances, then we might find a spurious relationship between forecast performance and job separations.

[14] For instance, at the beginning of 1987, our analysis only includes those analysts that are also in the sample in 1986, 1985 and 1984.

Therefore, we need to control for the type and number of firms that analysts follow during the three-year window that is used to calculate the forecast accuracy measure. First, we condition on the average coverage of the portfolio of firms that the analyst follows those three years to control for the fact that an analyst might be following thinly covered firms ($Average\,Coverage\,Effects_{i,t}$).[15] We also add dummy variables for the number of firms the analyst follows during the three-year window ($Number\ of\ Firms\,Covered\,Effects_{i,t}$). Additionally, we also include indicators for the years of experience of the analyst ($Experience\,Effects_{i,t}$), and a full set of dummies for the brokerage house an analyst works for ($Brokerage\,House\,Effects_{i,t}$) and year dummies ($Year\ Effects_t$).

Our final probit specification is then:

$$Pr\left(Job\,Separation_{i,t+1}\right)=\Phi\begin{pmatrix}\alpha+\beta_1 Forecast\ Accuracy\ Indicator_{i,t}+Average\ Coverage\ Effects_{i,t}\\+\ Number\ of\ Firms\ Covered\ Effects_{i,t}+Experience\,Effects_{i,t}\\+\,Year\ Effects_{t+1}\ +\ Brokerage\,House\,Effects_{i,t}\end{pmatrix} \tag{5}$$

Table 7 presents the results of the estimations of this probit model for the various job separation measures involving movements along the brokerage house hierarchy. In columns (1) and (2), the dependent variable is whether an analyst experiences a movement down the brokerage house hierarchy. In column (1), being in the bottom 10 percent of relative accuracy increases the probability of experiencing this unfavorable outcome by 4.78 percentage points, and this effect is statistically different from zero at the five percent significance level.[16] In any given year, about 7.77% of analysts move down the hierarchy; so, being inaccurate increases an analyst's chances of experiencing such a negative career outcome by about 62%. In column (2), scoring in the top 10% of the performance distribution decreases an analyst's

[15] We could just add this variable linearly to the regression specification, but we are concerned that there might be a more complicated relationship between this average coverage measure and the job separation. Because the values of this variable fall roughly between 0 and 40, we create a series of 40 dummy variables that correspond to increments of one of this value and include those dummies in the regression specification.

[16] The standard errors of these probit estimations are calculated to allow for the correlation of observations of analysts who work for the same brokerage house. All of the standard errors of the regressions presented below are adjusted in this way.

chances of moving down the brokerage house hierarchy by about 2.5 percentage points. Therefore, good past forecasting performance decreases an analyst's chances of experiencing such an unfavorable outcome by about 32%; however, this effect is imprecisely estimated.

In columns (3) and (4), the dependent variable is an indicator for moving up the brokerage hierarchy. Being in the bottom 10% of relative accuracy, in column (3), decreases the probability of moving from a low status to a high status brokerage house by about 1.4 percentage points. On average, about 2.73% of analysts experience such a positive career outcome; therefore, extremely poor past relative forecasting accuracy decreases an analyst's chances of moving up the brokerage prestige hierarchy by about 52%. Column (4) shows that extreme good performance increases an analyst's chances of moving up the hierarchy by about 41%. These results are both economically and statistically significant.

These results indicate that relative accuracy is rewarded. Moreover, it appears that the economic effect of extremely poor accuracy is slightly larger than for extremely good accuracy. We have also considered other ways of specifying an analyst's accuracy score in the probit model, including just specifying the score linearly and expressing an analyst's score with dummies for various deciles of accuracy. The results of these various specifications are all qualitatively similar to our previous results, with the strongest effects occurring at the tails of the accuracy distribution.

In columns (5) through (8) of Table 7, we re-estimate the effect of forecasting performance on job separations using the measure of absolute forecasting accuracy instead of the measure of relative forecasting performance. We find that the various job separation measures do not appear to be as sensitive to the absolute performance measure as to the relative performance measure. For instance, in column (5), we consider the effect of poor absolute forecasting accuracy on movements down the brokerage house hierarchy. The effect is actually of the wrong sign though imprecisely estimated. This difference between using the relative and absolute performance measures is consistent with Mikhail, Walther and Willis (1999), who find that absolute performance has little effect on job turnover. They do not, however, consider the direction of the job movements (i.e. up or down the hierarchy or out of the profession).

It is interesting to note that the conventional wisdom among many practitioners, especially brokerage house directors, is that the quality or breadth of analyst reports that accompany earnings forecasts are as (if not more) important determinants of reputation for forecasting expertise than simple accuracy. Therefore, our findings that simple measures of relative forecast accuracy strongly predict career outcomes are somewhat surprising.

Moreover, these findings have a number of implications for the sizeable literature on whether analysts have forecasting ability. Existing studies are somewhat mixed regarding whether analysts are homogeneous in forecasting ability. O'Brien (1990) compares average forecast accuracy across analysts and industries and finds no systematic differences (see also Butler and Lang (1991)). More recent studies, however, document that there are persistent differences in forecasting ability among analysts (see, e.g., Sinha, Brown and Das (1997), Clement (1998), Mikhail and Walthers (1997)). Others find that analysts at top-tier brokerage houses tend to be more accurate (see, e.g., Jacobs, Lys, and Neale (1999)).

Our findings suggest that in analyzing whether analysts have persistent differences in forecasting performance, one might need to take into account of the biases associated with job separations caused by performance. Our findings also suggest that analysts from top-tier brokerage houses are better forecasters than those from low-status houses in part because top-tier brokerage houses are able to pay more and employ more talented analysts.

5.3. Sensitivity of Job Separations to Forecast Optimism

We next look at the relationship between job separations and forecast optimism. The probit model specification is similar to equation (5):

$$Pr\left(Job\ Separation_{i,t+1}\right) = \Phi\begin{pmatrix} \alpha + \beta_1 Forecast\ Optimism\ Indicator_{i,t} \\ + Relative\ Accuracy\ Effects_{i,t} \\ + Average\ Coverage\ Effects_{i,t} \\ + Number\ of\ Firms\ Covered\ Effects_{i,t} + Experience\ Effects_{i,t} \\ + Year\ Effects_{t+1} + Brokerage\ House\ Effects_{i,t} \end{pmatrix} \quad (6)$$

where $Forecast\ Optimism\ Indicator_{i,t}$ is some function of Relative $Forecast\ Optimism_{i,t}$ and $Relative\ Accuracy\ Effects_{i,t}$ is a set of dummies to control for where the analyst places in the relative accuracy score distribution. The coefficient of interest is β_1, which measures the sensitivity of job separations to relative forecast optimism.

Table 8 presents the estimates of equation (6). In columns (1) and (2), the dependent variable is an indicator for movements down the hierarchy. In column (1), we estimate the effect of being in the top 10% of the relative forecast optimism score distribution decreases an analyst's chances of experiencing an unfavorable career outcome by about 38%. In contrast, being in the bottom 10% of the forecast optimism distribution, in column (2), increases an analyst's chances of experiencing such an unfavorable outcome by about 10%. While both effects are economically interesting, only the result in column (1) is statistically significant from zero.

In columns (3) and (4) of Table 8, the dependent variable is an indicator for movements up the brokerage hierarchy. Being in the top 10% of the optimism distribution raises by 90% an analyst's chances of moving from a low to a high status house, and this effect is statistically significant from zero. Scoring in the bottom 10% of the distribution does decrease the chances of experiencing such a favorable outcome by about 6%, although this effect is not statistically significant from zero.

Note that our findings are not due the forecast optimism score being a proxy for relative accuracy. As we mentioned in Section 4.3, the optimism and accuracy scores are negatively correlated. Yet we find that optimism leads to favorable career outcomes. Hence, the accumulated evidence suggests that controlling for accuracy, the labor market for analysts rewards optimism.

Moreover, it appears that there is an asymmetry: there are significant rewards to extreme relative optimism but not extreme relative pessimism. Broadly speaking, the most plausible interpretation of this finding is that it is relatively optimistic forecasts (those that stand out from the crowd or the consensus) that effectively promote stocks and get new buyers. This in turn means more trading commissions and higher IPO prices. Whether an analyst is somewhat negative or very negative doesn't really matter as much. Taking this discussion a bit further, this asymmetry seems contrary to some versions of the access-to-management story in which management punishes analysts who deviate wildly in the negative direction from the earnings numbers that management feeds them. Also, because few low status brokerage houses do much underwriting, the fact that analysts at low status houses move up the hierarchy on relatively optimistic forecasts suggests that they are being rewarded for promoting stocks in general, and not necessarily just those with underwriting relationships.

5.4 Sensitivity of Job Separations to Forecast Behavior by Various Analyst Characteristics.

To better understand the relationships between career outcomes and forecast behaviors established above, we next consider how these relationships vary by three analyst characteristics: (1) whether an analyst's brokerage house has an underwriting relationship with the stock that the analyst covers, (2) whether an analyst's forecast takes place before or after 1995, and (3) whether an analyst is an All-American.

We explore these relationships with the following model specification. To examine whether the sensitivity of job separations to forecast performance depends on these characteristics, we estimate the following interaction model:

$$Pr\left(Job\ Separation_{i,t+1}\right) = \Phi\begin{pmatrix} \alpha + \beta_1 Forecast\ Accuracy\ Indicator_{i,t} + \beta_2 Analyst\ Characteristic_{i,t} \\ + \beta_3 Forecast\ Accuracy\ Indicator_{i,t} \times Analyst\ Characteristic_{i,t} \\ + Average\ Coverage\ Dummies_{i,t} \\ + Number\ of\ Firms\ Covered\ Dummies_{i,t} \\ + Experience\ Effects_{i,t} + Year\ Effects_{t+1} + BrokerageHouseEffects_{i,t} \end{pmatrix} \quad (7)$$

where *Analyst Characteristic*$_{i,t}$ is a measure of the analyst characteristic of interest, and the other variables are defined as before. The coefficient of interest is β_3, which measures whether the effect of analyst accuracy on job separations is different for the analysts with the characteristic of interest compared to other analysts.

Similarly, we also examine whether the effect of optimism on job separations varies by these characteristics. Our model specification is:

$$Pr\left(Job\,Separation_{i,t+1}\right)=\begin{pmatrix}\alpha+\beta_1 ForecastOptimismIndicator_{i,t}+\beta_2 AnalystCharacteristic_{i,t}\\ +\beta_3 ForecastOptimismIndicator_{i,t}\times AnalystCharacteristic_{i,t}\\ +Relative\,AccuracyDummies_{i,t}+AverageCoverageDummies_{i,t}\\ +Number\,of\;Firms\,Covered\,Dummies_{i,t}+ExperienceEffects_{i,t}\\ +Year\,Effects_{t+1}+BrokerageHouseEffects_{i,t}\end{pmatrix} \quad (8)$$

The coefficient of interest is again β_3, which measures whether the effect of optimism on moving down the brokerage hierarchy varies by the analyst characteristic of interest. Throughout the analysis below, we will focus on movements down the hierarchy as our measure of job separations.

5.4.1 Sensitivity of Job Separations to Behaviors by Underwriting Relationships

There is substantial anecdotal and survey evidence indicating that underwriting relationships are an especially important reason why analysts exhibit an optimism bias (see Michaely and Womack (1999)). We see if the incentives implicit in job separations are consistent with this prior evidence by estimating equations (7) and (8) where *Analyst Characteristic*$_{i,t}$ is *Percent Underwriting*$_{i,t}$: the percent of an analyst *i*'s portfolio that consists of stocks that the analyst's brokerage house has underwriting relationships with.

Column (1) of Table 9 reports the estimates of equation (7).[17] If an analyst covers no stocks that have an underwriting relationship with her brokerage house, scoring in the bottom 10% of accuracy distribution leads to an increase of the chance of moving down the hierarchy by about 6 percentage points. The coefficient on the interaction term is negative and statistically different than zero at the 10 percent significance level, suggesting that the higher the percentage of stocks an analyst follows that have an underwriting relationship with her brokerage house the lower the effect of poor accuracy on moving down the hierarchy. For an analyst whose portfolio consists of 5% of stocks that have underwriting relationships with her brokerage house, being in the bottom 10% of accuracy only increases her chances of moving down the hierarchy by about 4 percentage points, indicating that underwriting relationships dampen the sensitivity of job separations to accuracy significantly.

Column (2) of Table 9 reports the estimates of equation (8). Conditional on covering no stocks that have underwriting relationships with her brokerage house, being in the top 10% of the optimism score decreases an analyst's chances of moving down the hierarchy by 1.6 percentage points. The interaction term is negative and statistically different from zero, indicating that the effect of optimism on lowering the chances of moving down the hierarchy is greater for analysts who covers stocks with underwriting relationships. For an analyst whose portfolio consists of 5% of stocks that have underwriting relationships with her brokerage house, scoring in the top 10% of accuracy decreases her chances of moving down the hierarchy by an additional 3 percentage points or a total of 4.7 percentage points. Therefore, it appears that underwriting relationships also diminish the sensitivity of adverse job separations to optimism.

5.4.2 Sensitivity of Job Separations to Forecast Behaviors by Different Sample Periods

In addition to looking at how underwriting relationships affect the sensitivities of job separations to accuracy and optimism, it is also interesting to look at how these sensitivities have varied over time.

[17] We have also re-done the regressions in Table 9 by controlling for whether an analyst is an All-American. The results are unchanged.

Many argue that analysts face more pressures to tradeoff accuracy for optimism in the late 1990s as the size of underwriting businesses and the power of institutional money has grown over time.

To see if analysts are rewarded less for accuracy during the period of 1996-2000 as compared to earlier periods, we estimate equation (7) where we let *Analyst Characteristic*$_{i,t}$ be *After 1995 Indicator*$_{i,t}$, which equals 1 if the observation is after 1995 and zero otherwise. Column (1) of Table 10 reports the coefficients of interest where the dependent variable is movements down the hierarchy. Conditional on being in the 1986-1995 period, being in the bottom 10% of accuracy increases an analyst's chances of moving down the hierarchy by 8.1 percentage points. The coefficient on the interaction term is negative and statistically different from zero, suggesting that poor performance mattered less for moving down the hierarchy for analysts in the 1996-2000 period. Being in the bottom 10% of accuracy after 1995 only decreases an analyst's chances of moving down the hierarchy by about 4.6 percentage points. In other words, accuracy matters less for job separations in 1996-2000 than in earlier periods.

To see if the sensitivity of job separations to optimism has increased in recent times, we estimate equation (8) where we again define *Analyst Characteristic*$_{i,t}$ to be the indicator *After 1995 Indicator*$_{i,t}$. Column (2) of Table 10 reports the coefficients of interest where dependent variable is movements down the hierarchy. Conditional on being in the 1986-1995 period, being in the top 10% of optimism decreases an analyst's chances of moving down the hierarchy by only about 2 percentage points. The coefficient on the interaction term is negative, suggesting that optimism matters more for decreasing an analyst's chances of moving down the hierarchy for observations after 1995. Conditional on being in the 1996-2000 period, being in the top 10% of optimism decreases an analyst's chances of moving down the hierarchy by about 4 percentage points. While the effect is economically large, the interaction is only statistically significant from zero at the 14 percent level of significance.

These findings are most consistent with analysts being rewarded for promoting stocks with optimistic forecasts and that whatever self-discipline brokerages had to generate objective forecasts diminished with the stock market boom. These findings run counter to the access-to-management story. Since the early 1990s, it has been common practice for high-tech and blue-chip companies to try to persuade analysts to issue earnings projections that are just slightly below what the company is actually preparing to report (see Cole (2001)). Indeed, rather than optimism mattering less for career concerns as the access-to-management story would suggest, we find that accuracy has mattered less and optimism mattered more for career concerns in the late 1990s. It also appears that the optimism bias has increased in the 1990s (see Dreman and Berry (1995)), contrary to what the access-to-management story would predict.

5.4.3 Sensitivity of Job Separations to Forecast Behaviors by All-American Status

Finally, we examine how the relationships between job separations and forecast behaviors vary depending on whether an analyst is an All American. There are a couple of reasons why the effect of forecast performance might vary depending on whether an analyst is an All-American. First, it may be that accuracy is not rewarded per se. Rather, accuracy is rewarded only to the extent that accuracy is recognized with an All-American award. To the extent that such a certification effect is at work, we would expect that analysts with good past performance but without an All-American award would not experience a significant increase in the chances of remaining at or moving up the hierarchy. Or, it may be that for analysts who are able to achieve an All-American status, accuracy may not matter; All Americans analysts might bring the brokerage house visibility and other forms of recognition, so accuracy may not be the only thing they are evaluated on.

To see if All Americans face different incentives than other analysts, we estimate equation (7) where we define *Analyst Characteristic*$_{i,t}$ to be *All-American*$_{i,t}$ which equals 1 if the analyst was an All-American during the three-year period in which his performance was measured and zero otherwise. Column (1) of Table 11 reports the coefficients of interest. The dependent variable is movements down the hierarchy. For analysts who are not All-Americans, scoring in the bottom 10% of the accuracy distribution increases an analyst's chances of moving down the hierarchy by 5.5 percentage points. The coefficient on the interaction term is negative, suggesting that poor performance matters less All-Americans. Conditional on being an All American, being in the bottom 10% of accuracy only decreases an analyst's chances of moving down the hierarchy by only 2 percentage points. This effect is, however, imprecisely measured.

To see if the sensitivity of job separations to optimism varies for All Americans, we estimate equation (8) where we replace the variable *Analyst Characteristic*$_{i,t}$ by the indicator *All-American*$_{i,t}$. Column (2) of Table 11 reports the coefficients of interest. Conditional on being not an All-American, being in the top 10% of optimism decreases an analyst's chances of moving down the hierarchy by about 2.5 percentage points. The coefficient on the interaction term is negative, suggesting that optimism matters more for decreasing an analyst's chances of moving down the hierarchy for All Americans. While the effect is economically large, it is again imprecisely measured.

6. Relating An Alternative Measure of Career Concerns to Forecast Behaviors

Having considered the effect of forecast behaviors on job separations, we now briefly consider an alternative measure of career concerns related to stock coverage assignments. There are certain important stocks that receive substantial attention from the investment community. An example of such a stock in the high-tech sector is Microsoft. These stocks are very large firms as measured both by market capitalization and the number of analysts that follow them. Receiving an assignment to cover such an important stock is a very favorable career outcome since the expected rewards (trading commissions, investment banking business, visibility) to covering a firm like Microsoft are much greater than covering a small software company.

Different analysts employed at the same brokerage house can potentially cover these important stocks. Typically, the software analyst within the high-tech sector covers Microsoft; however, other analysts within high-tech such as the Internet analyst can follow instead. To the extent that the buy-side cares about forecasting expertise, one would expect that brokerage houses might want to place their most accurate analysts on these important stocks.

We attempt to measure whether such implicit incentives exist within brokerage houses. Our empirical strategy involves tracking the firms that a brokerage house covers and determining whether the performance of the analyst covering the firm affects whether the brokerage house replaces the analyst with another analyst. For each stock that an analyst in a given brokerage house is following in a given year, we examine whether the brokerage house has an analyst (any analyst) following that firm the subsequent year. If no one is following the firm, then the brokerage house has dropped the stock. If someone is following the stock, then the brokerage house is continuing to follow the stock. We only look at instances where a brokerage house continues to follow the stock and ask who is following the stock the subsequent year.

If it is a new person, then the brokerage house has rotated a new person onto the stock. This can happen a couple of ways. One, the analyst previously covering the stock could have left the brokerage house (fired or left voluntarily), and therefore the brokerage house had to find a new analyst to follow the stock. Or, the analyst previously covering the stock could have stayed with the brokerage house but for some reason was replaced by another analyst covering this stock.

We are most interested in the rotations in which the analyst who leaves the stock stays with the brokerage house and continues to follow the same industry. Therefore, our sample is all stocks that a brokerage house follows in year t and also year $t+1$ in which the analyst who was covering the stock for the brokerage house in year t is also working for that brokerage house and following the same industry (but not necessarily covering that stock) in year $t+1$. Then, we construct a variable that is an indicator of whether that analyst (the one that was covering the stock in year t) is following the stock for the brokerage house in year $t+1$. We include in this sample only stocks that are followed in year t by an analyst who we have a relative performance score for, where the score is as defined in Section 4.2.2. This leaves us with a sample of 110,077 observations for analysis. The unit of observation is a brokerage house/stock/year cell. In this sample, about 9% of the time, the analyst who was covering the firm in year t did not continue to cover the firm for the brokerage house in year $t+1$.

We want to relate the probability that an analyst stops following a given stock for the brokerage house to the analyst's past forecast accuracy. The regression specification we will use is the following:

$$Pr\left(Analyst\ Stops\ Covering\ Stock_{i,j,k,t}\right) = \Phi\begin{pmatrix} \alpha + \beta_1 ForecastAccuracyIndicator_{i,t} \\ + Average\ Coverage\ Effects_{i,t} \\ + Number\ of\ Firms\ Covered\ Effects_{i,t} \\ + Year\ Effects_t + Brokerage\ House\ Effects_{i,t} \end{pmatrix} \tag{9}$$

Subscript *i* is for the analyst who covers the stock in year *t*. Subscript *j* is for the stock, and subscript *k* is for the brokerage house. $Analyst\ Stops\ Covering\ Stock_{i,j,k,t}$ is an indicator whether analyst *i*, who was covering stock *j* for brokerage house *k* in year *t*, does not follow the stock in year *t+1*. $ForecastAccuracyIndicator_{i,t}$ is some function of an analyst's relative accuracy. This variable is measured using all stocks that an analyst followed in the past three years and therefore measures the general forecasting accuracy over all stocks the analyst follows as opposed to accuracy for just stock *j*. We also add in the usual controls. The coefficient of interest is β_1, which measures whether analysts who perform poorly are more likely to move off a stock than other analysts.

We are most interested in estimating the regression in equation (9) for a sub-sample of only high-profile stocks. We will classify a stock as high profile in two ways. First, we define a stock as being high profile if 20 or more analysts follow the firm. Second, we classify a stock as being high profile if it has a value greater than $5 billion. About 5 percent of firms are classified as high profile using both classifications.

However, we first run the regression in equation (9) using all stocks as a benchmark. There are a couple of reasons for doing so. Because analysts tend to cover more than one stock at any point in time, it might be the case that poor performance will lead to movements out of a stock, regardless of size, because brokerage houses want to curtail the responsibilities of the analyst by decreasing the number of stocks that the analyst follows. Also, it might be that analysts have some discretion over which stocks they cover and for some reason, after poor performances, they might want to drop some stocks from their coverage. Since the high profile stocks are always covered by brokerage houses and are what analysts strive to cover, if we find analysts moving off of high profile stocks after poor performances, then it is likely not to be done voluntarily by analysts. In some sense, what interests us is the difference in magnitudes between β_1 for the sub-sample of high profile stocks and the sample comprising of all stocks.

The results of the regression in equation (9) are presented in Table 12. In columns (1) through (3), we look at the effect of poor performance (bottom 10% of relative accuracy scores) on whether an analyst stops following a stock for all brokerage houses. The first column includes all stocks and is the benchmark case. The coefficient on the poor performance indicator is essentially zero, suggesting that analysts who perform poorly (and do not leave their brokerage house or change the industry that they cover) are not more likely to stop following a given stock than any other analyst. In column (2), we present the regression results only including the analysts who follow the stocks with high analyst coverage. The effect of performing poorly is positive and statistically significant for this group of stocks compared to the entire sample. The size of the coefficient suggests that poor performance increases the probability that an analyst leaves a high-profile stock by over two percentage points, an increase of over 20%. In column (3), we present the regression result only including the analysts who follows stocks with high market caps. The coefficient is again positive and statistically significant for this sub-sample, again providing suggestive evidence that analysts who are following high-profile stocks are more likely to move off of high-profile stocks following extreme poor forecasting performances.

In columns (4) through (6) of Table 12, we look at the effect of good performance (top 10% of relative accuracy scores) on whether an analyst stops following a stock for all brokerage houses. Column (4) presents the effect of good performance on an analyst moving off of any stock. The coefficient on the top performance indicator is essentially zero, suggesting that analysts who perform well are not more likely to stop following a given stock than any other analyst. In column (5), we present the regression results only including the analysts who follow the stocks with high analyst coverage. The effect of performing poorly is negative and imprecisely measured for this group of stocks compared to the entire sample. The size of the coefficient suggests that poor performance decreases the probability that an analyst leaves a high-profile stock by over one-half a percentage point, an increase of about 5%. In column (6), we present the regression result only including the analysts who follows stocks with high market caps. The coefficient is again negative but statistically insignificant. The findings in Table 12 collectively suggest that the internal labor markets within brokerage houses may provide some implicit incentives for security analysts to produce forecast track records by rationing high profile or "plum" stocks to analysts with better track records.

We have also looked at the effect of forecast optimism on the likelihood that an analyst stops following a high profile stock. We find that optimism decreases the likelihood of an analyst stopping coverage on a high profile stock, while pessimism increases this likelihood. These effects of optimism, however, are slightly smaller than the corresponding effects involving accuracy and are imprecisely measured. We omit these findings for the sake of brevity.

While we view these findings as primarily corroborating our results using job separations, the findings are interesting in of themselves in that few studies have simultaneously studied implicit incentives from both external and internal labor markets. In this way, our paper also contributes to the broader literature on career concerns (see Jensen and Murphy (1990), Khorana (1996)). Such panel data are simply not available for most other labor markets.

7. Conclusions

We draw two broad sets of conclusions from our findings. First, it appears that earnings forecast accuracy does matter for analyst career concerns. Second, controlling for accuracy, analysts are rewarded for generating relatively optimistic forecasts. The latter finding is most likely due to analyst incentives rewarding optimistic forecasts presumably because they help promote stocks. Consistent with this view, rewards depend less on accuracy and more on optimism for analysts covering stocks underwritten by their brokerage houses. Also, rewards were less sensitive to accuracy and more sensitive to optimism during the stock market boom of the late nineties.

Our findings also suggest answers to a couple of questions of interest. There is some debate about whether the well-documented analyst optimism bias is due to incentives or non-incentive based explanations. And if it is due to incentives, what do these incentives look like? Answers to these questions are interesting not only from an academic perspective but are also relevant from a policy perspective as current Congressional hearings are debating whether and what types of regulations to impose on brokerage houses.

To the extent that analyst incentives to promote stocks have little to do with optimistic forecasts, regulation aimed at such incentives may be ineffective. Our findings suggest otherwise. Moreover, our findings may be helpful in thinking through regulatory responses. For instance, our findings suggest that the optimism bias is not merely due to underwriting relationships. So, current attention on underwriting relationships as the sole conflict of interest may be premature. Also, the fact that such incentives are implicit as opposed to being written into analyst contracts suggest that information regarding explicit analyst incentives may be of little help in determining the nature of conflicts of interest.

Finally, there are some additional interesting avenues of future research. For instance, it would be interesting to understand how these incentives affected analyst behavior through the various stages of an analyst’s career. Do analysts build up reputation for making the right calls when young and hence building up a following among investors and then cash out on this reputation when old?

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Table 1: Characteristics of Brokerage Houses over Time

The entries are descriptive statistics on brokerage houses in the I/B/E/S database between 1983 and 2000. For each year in the sample, we report the total number of such houses and sample statistics on the size (number of analysts employed) of these houses.

		Number of Analysts Working for a Brokerage House			
Year	Number of Houses	Average	25th Percentile	Median	75th Percentile
1983	90	20.71	7	12	25
1984	108	18.26	7	11	25
1985	126	16.56	5	10	22
1986	136	15.02	4	9	16
1987	142	15.07	5	9	17
1988	153	13.46	5	9	16
1989	171	12.67	4	8	15
1990	174	11.91	4	7	15
1991	178	10.76	4	7	14
1992	166	11.83	4	7	15
1993	196	11.41	3	7	15
1994	196	11.57	3	7	14
1995	211	12.48	3	7	15
1996	223	12.66	3	7	15
1997	278	10.70	2	5	12
1998	317	11.04	2	5	11
1999	314	11.54	1	5	12
2000	305	11.61	1	5	12

Table 2: Percentage of Analysts who Work for High Status Brokerage Houses

The entries are the percentage of all analysts who are categorized as working for high status brokerage houses. For each year in the sample, we report these percentages for each of our three status measures: the I.I., size and Carter-Manaster rankings, respectively.

Year	I.I. Ranking	Size Ranking	Carter-Manaster Ranking
1983	22.87	39.13	18.57
1984	23.66	33.44	20.87
1985	21.34	29.76	19.13
1986	24.91	31.94	23.00
1987	20.99	28.12	22.91
1988	26.44	28.69	23.90
1989	26.35	28.11	24.69
1990	21.78	25.07	20.67
1991	23.19	25.03	19.41
1992	20.66	25.68	19.34
1993	24.84	25.97	20.15
1994	24.74	24.88	19.97
1995	22.78	25.20	19.87
1996	22.81	25.92	19.74
1997	21.37	24.70	19.72
1998	24.49	25.76	19.65
1999	21.79	27.73	19.24
2000	24.81	25.86	18.50

Table 3: Summary Statistics of Analyst Job Separations

Security analysts in the I/B/E/S database in a year are tracked to see if they separate from their employer during the next year. We report the percentage of analysts who experience various types of job separations in a year (averaged over the sample period of 1983-2000). These percentages are calculated using the I.I. rankings to determine brokerage house status. In Panel A, the sample includes all analysts. In Panel B, the sample includes analysts with at least three years of experience.

Panel A: Entire I/B/E/S sample

% of Analysts who Change Houses each Year: 14.32%

	% of Analysts who Move	% of Analysts Working for Low Status House	% of Analysts Working for High Status House
Analysts who Work for a Low Status House who Move to a High Status House	12.15%	2.20%	
Analysts who Work for a High Status House who Move to a Low Status House	10.25%		7.02%
Analysts who Work for a High Status House who Move to Another High Status House	5.09%		3.49%
Analysts who Work for a Low Status House who Move to Another Low Status House	72.50%	13.13%	

Panel B: Analysts with more than 3 years of experience

% of Analysts who Change Houses each Year: 14.43%

	% of Analysts who Move	% of Analysts Working for Low Status House	% of Analysts Working for High Status House
Analysts who Work for a Low Status House who Move to a High Status House	14.53%	2.73%	
Analysts who Work for a High Status House who Move to a Low Status House	12.54%		7.77%
Analysts who Work for a High Status House who Move to Another High Status House	6.40%		3.97%
Analysts who Work for a Low Status House who Move to Another Low Status House	66.52%	12.52%	

Table 4: Measures of Forecast Performance

Panel A. A Hypothetical Example of a Relative Accuracy Score Calculation

The entries are an example of the forecasts of eight analysts covering a hypothetical firm. The analysts are ranked based on the size of the error of their forecasts, and the relative accuracy score measure of each analyst, described in Section 4.2.2, is calculated.

Analyst	Forecast Error	Rank	Score
1	0.12	1	100
2	0.25	3	71.4
3	0.25	3	71.4
4	0.25	3	71.4
5	0.38	5	42.9
6	0.67	6.5	21.4
7	0.67	6.5	21.4
8	0.80	8	0

Panel B. Summary Statistics of Analyst Performance Measures

The entries are summary statistics of the two analyst forecast performance measures for analysts with at least three years of experience in the I/B/E/S data set.

	Average	10^{th} Percentile	Median	90^{th} Percentile
	(1)	(2)	(3)	(4)
Analyst’s Accuracy Score	50.94 [8.15]	40.77	51.24	60.61
Analyst’s Optimism Score	47.58 [13.51]	30.77	47.62	64.71

Table 5: The Percentage of an Analyst's Portfolio in a Year that Consists of New Stocks

Security analysts in the I/B/E/S database in a year are tracked to see what percentage of stocks they cover in the following year are not stocks they are covering this year. The entries contain the percentage of an analyst's portfolio the following year that are new stocks. We report these percentages for the entire sample and for analysts who experience various types of separations. These percentages are calculated using the I.I. rankings to determine brokerage house status.

	Percentage of Portfolio that is New
Entire Sample	26.18%
Analysts who Leave Brokerage House	27.59%
Analysts who Stay with Brokerage House	25.93%
	Percentage of Portfolio that is New For Analysts who Change Houses
Analysts who Work for Low Status House Who Move to a High Status House	23.73%
Analysts who Work for High Status House Who Move to a Low Status House	24.50%
Analysts who Work for High Status House Who Move to Another High Status House	25.74%
Analysts who Work for Low Status House Who Move to Another Low Status House	29.20%

Table 6: The Percentage of Analysts Who Work for High Status Brokerage Houses by Experience

Security analysts in the I/B/E/S database who start their career between 1983 and 1999 are partitioned into different samples based upon the number of years they are in the I/B/E/S database. The samples include all analysts who are in the I/B/E/S database a minimum number of years. The entries are the percentage of analysts in these samples that work for high status brokerage houses by their experience, where high status is measured using the I.I. ranking.

	Minimum Number of Years Analyst is in Sample								
Years of Experience	2 Years	3 Years	4 Years	5 Years	6 Years	7 Years	8 Years	9 Years	10 Years
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1	18.32	19.42	19.00	18.79	19.01	19.15	20.10	21.51	22.32
2	19.77	20.36	20.23	20.15	20.33	20.66	21.41	22.51	23.88
3		20.89	20.62	20.64	20.91	20.94	21.89	22.79	24.22
4			22.47	21.93	21.57	21.13	22.49	23.36	23.88
5				23.35	22.48	22.83	23.56	24.64	25.26
6					22.48	23.30	23.44	23.93	24.22
7						22.45	24.04	24.50	25.43
8							24.52	25.78	25.78
9								24.93	24.91
10									26.82
Number of Analysts	4907	3212	2274	1623	1210	1060	836	702	578

Table 7: The Effect of Past Accuracy on Job Separations

Security analysts who have at least three prior years of experience are tracked to examine if past forecasting accuracy affects the likelihood that an analyst moves from a high to a low status house (Move Down) or that an analyst moves from a low to a high status house (Move Up). The probit specification is equation (5). In columns (1)-(4), the *Relative Forecast Accuracy* score is used to measure forecasting accuracy. In columns (5)-(8), the *Absolute Forecast Accuracy* score is used to measure forecasting accuracy. Brokerage house status is measured using the I.I. ranking. Standard errors are in parentheses; they are adjusted to account for within-brokerage house correlation of the observations. The entries in the brackets are the marginal probabilities that an analyst with the various accuracy scores experience a job change compared to other analysts. (*Significant at 10 percent level. **Significant at 5 percent level.)

	Relative Forecast Accuracy				*Absolute Forecast Accuracy*			
	Moves Down		Moves Up		Moves Down		Moves Up	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bottom 10% of Accuracy Scores Indicator	.3487** (.1509) [.0478]		-.4563** (.1438) [-.0141]		-.0428 (.1776) [-.0046]		-.2044 (.2190) [-.0085]	
Top 10% of Accuracy Scores Indicator		-.2596 (.1809) [-.0246]		.2053** (.0854) [.0112]		-.0944 (.1690) [-.0100]		.1536 (.1876) [.0086]
Year Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Experience Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Brokerage House Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Firms Covered Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Average Coverage Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Log likelihood	-411.44	-412.86	-403.68	-403.54	-413.07	-413.87	-404.84	-404.91
Observations	1866	1866	6143	6143	1866	1866	6143	6143

Table 8: The Effect of Past Optimism on Analyst Job Separations

Security analysts who have at least three prior years of experience are tracked to examine if past forecasting optimism affects the likelihood that an analyst moves from a high to a low status house (Move Down) or that an analyst moves from a low to a high status house (Move Up). The probit specification is equation (6). The measure of optimism is the *Relative Forecast Optimism* score. Brokerage house status is measured using the I.I. ranking. Standard errors are in parentheses; they are adjusted to account for within-brokerage house correlation of the observations. The entries in the brackets are the marginal probabilities that an analyst with the various optimism scores experience a job change compared to other analysts. (*Significant at 10 percent level. **Significant at 5 percent level.)

	Moves Down		Moves Up	
	(1)	(2)	(3)	(4)
Top 10% of *Relative Forecast Optimism* Scores Indicator	-.4028** (.1425) [-.0297]		.3793** (.1893) [.0247]	
Bottom 10% of *Relative Forecast Optimism* Scores Indicator		.0734 (.1559) [.0076]		-.0401 (.2252) [-.0017]
Relative Forecast Accuracy Effects	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes
Experience Effects	Yes	Yes	Yes	Yes
Brokerage House Effects	Yes	Yes	Yes	Yes
Number of Firms Covered Effects	Yes	Yes	Yes	Yes
Average Coverage Effects	Yes	Yes	Yes	Yes
Log Likelihood	-403.92	-404.89	-392.46	-394.22
Observations	1866	1866	6143	6143

Table 9: The Effect of Accuracy and Optimism on Analyst Job Separations by Underwriter Status

Security analysts who have at least three prior years of experience are tracked to examine if the effects of relative forecasting accuracy and optimism on the likelihood that an analyst moves from a high to a low status house (Move Down) depend on underwriting relationships. The probit specification is equation (7). *Percent Underwriting* is the fraction of an analyst's portfolio of stocks that have underwriting relationships with her brokerage house. Brokerage house status is measured using the I.I. ranking. Standard errors are in parentheses; they are adjusted to account for within-brokerage house correlation of the observations. The entries in the brackets are the marginal probabilities that an analyst with the various accuracy and optimism scores and underwriting relationships experience a move down compared to other analysts. (*Significant at 10 percent level. **Significant at 5 percent level.)

	Moves Down	Moves Down
	(1)	(2)
Bottom 10% of *Relative Forecast Accuracy* Scores Indicator	.4276** (.1579) [.0605]	
Percent Underwriting	.7172 (1.203) [.0745]	1.708 (1.209) [.1597]
Bottom 10% of *Relative Forecast Accuracy* Scores Indicator×*Percent Underwriting*	-4.390* (2.399) [-.4561]	
Top 10% of *Relative Forecast Optimism* Scores Indicator		-.1973 (.2049) [-.0161]
Top 10% of *Relative Forecast Optimism* Scores Indicator×*Percent Underwriting*		-6.737* (3.974) [-.6297]
Relative Forecast Accuracy Effects	No	Yes
Year Effects	Yes	Yes
Experience Effects	Yes	Yes
Brokerage House Effects	Yes	Yes
Number of Firms Covered Effects	Yes	Yes
Average Coverage Effects	Yes	Yes
Log Likelihood	-403.79	-402.77
Observations	1866	1866

Table 10: The Effect of Accuracy and Optimism on Analyst Job Separations by Sub-Periods

Security analysts who have at least three prior years of experience are tracked to examine if the effects of relative forecasting accuracy and optimism on the likelihood that an analyst moves from a high to a low status house (Move Down) differ between 1996-2000 and early periods. The probit specification is equation (7). *After 1995 Indicator* equals one for analysts' forecast issued after 1995. Brokerage house status is measured using the I.I. ranking. Standard errors are in parentheses; they are adjusted to account for within-brokerage house correlation of the observations. The entries in the brackets are the marginal probabilities that an analyst with the various accuracy and optimism scores issuing forecasts in different period experience a move down compared to other analysts. (*Significant at 10 percent level. **Significant at 5 percent level.)

	Moves Down	Moves Down
	(1)	(2)
Bottom 10% of *Relative Forecast Accuracy* Scores Indicator	.5336** (.1767) [.0813]	
Bottom 10% of *Relative Forecast Accuracy* Indicator×*After 1995 Indicator*	-.4462* (.2529) [-.0353]	
Top 10% of *Relative Forecast Optimism* Scores Indicator		-.2395 (.2075) [-.0196]
Top 10% of *Relative Forecast Optimism* Scores Indicator×*After 1995 Indicator*		-4157 (.2852) [-.0290]
Relative Forecast Accuracy Effects	No	Yes
Year Effects	Yes	Yes
Experience Effects	Yes	Yes
Brokerage House Effects	Yes	Yes
Number of Firms Covered Effects	Yes	Yes
Average Coverage Effects	Yes	Yes
Log Likelihood	-410.32	-403.66
Observations	1866	1866

Table 11: The Effect of Accuracy and Optimism on Analyst Job Separations by All-American Status

Security analysts who have at least three prior years of experience are tracked to examine if the effects of relative forecasting accuracy and optimism on the likelihood that an analyst moves from a high to a low status house (Move Down) depend on whether an analyst is an All American. The probit specification in columns (1) and (2) are equation (7) and (8) respectively. *All-American Indicator* equals one if an analyst became an All American at some point during the three-year window of analyst performance. Brokerage house status is measured using the I.I. ranking. Standard errors are in parentheses; they are adjusted to account for within-brokerage house correlation of the observations. The entries in the brackets are the marginal probabilities that an analyst with the various accuracy and optimism scores and All-American status experience a move down compared to other analysts. (*Significant at 10 percent level. **Significant at 5 percent level.)

	Moves Down	Moves Down
	(1)	(2)
Bottom 10% of *Relative Forecast Accuracy* Scores Indicator	.3947** (.1727) [.0552]	
All-American Indicator	-.0982 (.0732) [-.0103]	-.1776 (.1244) [-.0156]
Bottom 10% of *Relative Forecast Accuracy* Scores Indicator× *All-American Indicator*	-.4437 (.4335) [-.0342]	
Top 10% of *Relative Forecast Optimism* Scores Indicator		-.3215** (.1410) [-.0245]
Top 10% of *Relative Forecast Optimism* Scores Indicator×*All-American Indicator*		-4840 (.4860) [-.0310]
Relative Forecast Accuracy Effects	No	Yes
Year Effects	Yes	Yes
Experience Effects	Yes	Yes
Brokerage House Effects	Yes	Yes
Number of Firms Covered Effects	Yes	Yes
Average Coverage Effects	Yes	Yes
Log Likelihood	-410.42	-403.26
Observations	1866	1866

Table 12: The Effect of Accuracy on Whether a Brokerage House Removes an Analyst from Following a Firm

Brokerage houses that cover a firm for more than one year are tracked to see whether the accuracy of the analyst following the firm influences whether the brokerage house replaces that analyst with another analyst. The probit specification is equation (9). The sample includes all analysts that continue to work for the same brokerage house and cover the same industry as they did the previous year. Regression in (1) measures the effect of poor relative accuracy on whether an analyst is removed from following the firm. Regression in (2) measures the same effect only for analysts that follow firms that are covered by at least 20 other analysts. Regression in (3) measures the same effect for analysts covering firms worth more than $5 billion. Regressions in (4)-(6) are identical to (1)-(3) except that the effect of good relative accuracy is measured. The entries in the brackets are the marginal probabilities that an analyst is removed from following a firm. Standard errors are in parentheses; they are adjusted to account for within-brokerage house correlation of the observations. (*Significant at 10 percent level. **Significant at 5 percent level.)

	Poor Relative Accuracy			Good Relative Accuracy		
	All Stocks	High Coverage	High Value	All Stocks	High Coverage	High Value
	(1)	(2)	(3)	(4)	(5)	(6)
Indicator for Poor Past Performance (Bottom 10% of Distribution)	.0321 (.0434) [.0042]	.1732** (.0849) [.0245]	.1568* (.0826) [.0213]			
Indicator for Good Past Performance (Top 10% of Distribution)				.0003 (.0416) [.0000]	-.0518 (.0830) [-.0064]	-.0239 (.0822) [-.0029]
Year Effects	Yes	Yes	Yes	Yes	Yes	Yes
Experience Effects	Yes	Yes	Yes	Yes	Yes	Yes
Brokerage House Effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of Firms Covered Effects	Yes	Yes	Yes	Yes	Yes	Yes
Average Coverage Effects	Yes	Yes	Yes	Yes	Yes	Yes
Log Likelihood	-24,291.37	-6607.70	-7003.31	-24,292.32	-6614.45	-7009.86
Observations	110,077	34,028	26,439	110,077	34,028	26,439

Appendix Table 1: High Status Brokerage Houses using I.I. Ranking by Year

A list of the top ten rated brokerage houses by *Institutional Investor* (I.I.) between 1983 and 2000 and the number of analysts they employ. An asterisk is after a brokerage house that is also one of the ten biggest that year.

Year	Brokerage House	Analysts	Year	Brokerage House	Analysts	Year	Brokerage House	Analysts
1983	Dean Witter*	52	1984	Dean Witter*	52	1985	Dean Witter	40
	DLJ	35		DLJ	34		DLJ	34
	Drexel Burham Lambert	37		Drexel Burham Lambert	38		Drexel Burham Lambert	43
	First Boston	34		First Boston	37		First Boston	35
	Goldman Sachs	33		Goldman Sachs	31		Goldman Sachs	35
	Kidder Peabody	39		Kidder Peabody*	42		Kidder Peabody*	48
	Merrill Lynch*	91		Merrill Lynch*	92		Merrill Lynch*	95
	Morgan Stanley	21		Morgan Stanley*	45		Morgan Stanley	26
	Paine Webber*	44		Paine Webber*	50		Paine Webber*	49
	Smith Barney	39		Smith Barney*	44		Salomon Brothers	39
1986	DLJ	31	1987	DLJ	29	1988	DLJ	30
	Drexel Burham Lambert*	52		Drexel Burham Lambert*	53		Drexel Burham Lambert*	57
	First Boston	42		First Boston	47		First Boston*	48
	Goldman Sachs*	48		Goldman Sachs	46		Goldman Sachs	37
	Kidder Peabody*	45		Merrill Lynch*	102		Merrill Lynch*	98
	Merrill Lynch*	104		Morgan Stanley	37		Paine Webber*	49
	Morgan Stanley*	52		Paine Webber*	54		Prudential-Bache*	44
	Paine Webber*	51		Prudential-Bache	33		Salomon Brothers*	55
	Salomon Brothers	37		Salomon Brothers*	59		Shearson Lehman*	81
	Smith Barney	45		Smith Barney	46		Smith Barney	42
1989	DLJ	29	1990	DLJ	33	1991	DLJ	29
	Drexel Burham Lambert*	66		First Boston*	45		First Boston*	38
	First Boston*	51		Goldman Sachs*	48		Goldman Sachs*	50
	Goldman Sachs	41		Kidder Peabody*	45		Kidder Peabody	35
	Merrill Lynch*	88		Merrill Lynch*	84		Lehman Brothers*	58
	Morgan Stanley	41		Morgan Stanley	33		Merrill Lynch*	79
	Paine Webber*	90		Paine Webber*	52		Morgan Stanley*	35
	Prudential-Bache*	42		Prudential-Bache	35		Paine Webber*	42
	Salomon Brothers*	50		Shearson Lehman*	67		Prudential	34
	Shearson Lehman*	72		Smith Barney*	41		Smith Barney*	42
1992	DLJ*	39	1993	DLJ	43	1994	DLJ	34
	First Boston	36		CS First Boston*	45		CS First Boston*	42
	Goldman Sachs*	46		Goldman Sachs*	56		Goldman Sachs*	49
	Kidder Peabody	39		Lehman Brothers*	72		Lehman Brothers*	65
	Lehman Brothers*	66		Merrill Lynch*	101		Merrill Lynch*	110
	Merrill Lynch*	77		Morgan Stanley*	47		Morgan Stanley*	55
	Morgan Stanley*	44		Paine Webber*	50		Paine Webber	46
	Paine Webber*	43		Prudential	38		Prudential	34
	Prudential	36		Salomon Brothers*	49		Salomon Brothers*	56
	Smith Barney*	44		Smith Barney*	50		Smith Barney*	64
1995	DLJ	42	1996	Bear Stearns*	59	1997	Bear Stearns*	56
	CS First Boston*	53		DLJ	47		DLJ	52
	Goldman Sachs*	53		Goldman Sachs*	65		Goldman Sachs*	66
	Merrill Lynch*	127		Merrill Lynch*	136		Lehman Brothers*	77
	Morgan Stanley*	63		Morgan Stanley*	73		Merrill Lynch*	163
	Paine Webber*	50		Paine Webber	43		Morgan Stanley*	71
	Prudential	42		Prudential	41		Paine Webber	46
	Salomon Brothers*	61		Salomon Brothers*	67		Salomon Brothers*	67
	Sanford Bernstein	17		Sanford Bernstein	20		Sanford Bernstein	22
	Smith Barney*	86		Smith Barney*	88		Smith Barney*	83
1998	Bear Stearns*	73	1999	Bear Stearns*	76	2000	Bear Stearns*	71
	DLJ*	61		DLJ*	80		DLJ*	78
	First Boston*	93		First Boston*	109		First Boston*	111
	Goldman Sachs*	73		Goldman Sachs*	87		Goldman Sachs*	94
	J.P. Morgan*	64		J.P. Morgan*	76		J.P. Morgan	69
	Lehman Brothers*	61		Lehman Brothers	68		Lehman Brothers*	75
	Merrill Lynch*	157		Merrill Lynch*	170		Merrill Lynch*	200
	Morgan Stanley*	89		Morgan Stanley*	92		Morgan Stanley*	95
	Paine Webber	41		Paine Webber	45		Paine Webber	49
	Salomon Smith Barney*	124		Salomon Smith Barney*	108		Salomon Smith Barney*	100

Appendix Table 2: High Status Brokerage Houses using Carter-Manaster Ranking

An alphabetical list of the ten brokerage houses classified as high status using the Carter-Manaster rankings of Carter, Dark and Singh (1998). The number of IPOs of these brokerage houses between 1985 and 1991 is listed as well as the Carter-Manaster rank of the brokerage house based on those IPOs and the average number of analysts who work for the brokerage house in a year between 1985 and 1991.

	Carter-Manaster Ranks	Number of IPOs	Average Number of Analysts
Alex Brown & Sons	8.88	107	35
Drexel Burham Lambert	8.83	114	47
First Boston Corporation	9.00	53	44
Goldman Sachs & Company	9.00	85	44
Hambrecht & Quist	9.00	44	21
Merrill Lynch	8.88	145	93
Morgan Stanley & Company	8.88	73	36
Paine Webber	8.75	63	55
Prudential-Bache	8.75	84	34
Salomon Brothers	9.00	47	51