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Security analysts' career concerns and herding of earnings forecasts

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Several theories of reputation and herd behavior (e.g., Scharfstein and Stein (1990), and Zwiebel (1995)) suggest that herding among agents should vary with career concerns. Our goal is to document whether such a link exists in the labor market for security analysts. We find that inexperienced analysts are more likely to be terminated for inaccurate earnings forecasts than are their more experienced counterparts. Controlling for forecast accuracy, they are also more likely to be terminated for bold forecasts that deviate from the consensus. Consistent with these implicit incentives, we find that inexperienced analysts deviate less from consensus forecasts. Additionally, inexperienced analysts are less likely to issue timely forecasts, and they revise their forecasts more frequently. These findings are broadly consistent with existing career-concern-motivated herding theories.

1. Introduction

■ A number of recent theoretical studies examine the relationship between concern for reputation and herd behavior (e.g., Scharfstein and Stein (1990), Trueman (1994), Zwiebel (1995), and Prendergast and Stole (1996)).[1] These models regard reputation as arising from learning over time about some exogenous characteristic of agents, such as ability, through their observed behavior. Reputation affects agents' decisions when they adjust their behavior to influence the data others use to learn about their ability. Such consideration for reputation or "career concerns" can at times lead agents to ignore private information and copy the actions of others: to herd.

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[1] Other theories of herding, not based on reputation, include Banerjee (1992) and Bikhchandani, Hirshleifer, and Welch (1992).

The idea of an agent managing her reputation because it might affect her wages later in her career dates back to Fama (1980), Lazear and Rosen (1981), and Holmström (1999). These early models of career concerns deal with the decisions of a single agent. The more recent multi-agent models can produce a link between career concerns and herd behavior, suggesting that an agent's propensity to herd might vary over different stages of her professional life. Though these more recent models clarify the varied ways in which concern for reputation can affect herd behavior, there is remarkably little systematic empirical evidence linking herding to career concerns. In this article we try to document whether such an association exists in the labor market for (sell-side) security analysts.

Security analysts are usually employed by brokerage houses to follow firms in an industry and generate information about them such as earnings forecasts and stock recommendations. Their clients are institutional investors or the ''buy side,'' which include mutual funds, hedge funds, and pension funds. An analyst's compensation depends crucially on an annual poll conducted by *Institutional Investors (II)* of the ''buy side.'' Those at the top of the poll are called *II All-Americans.* An analyst has to balance the interests of the ''buy side'' (which generally prefers accurate information on price fluctuations) with the interests of the sales force and investment bankers of the brokerage house (who tend to care about trading commissions and favorable reports for initial public offerings) (see, e.g., Schipper (1991) and Nocera (1997)). While an analyst's compensation includes fees from the trading volume that she generates and the investment banking business that she brings in, her ability to do so in the long run depends in part on her perceived forecasting ability (see, e.g., Stickel (1992) and Trueman (1994)).

Personnel decisions in this labor market attract a great deal of public scrutiny. Analysts with consistently good performances relative to their peers are often touted in the press and sought after by competing brokerage houses, suggesting an important role for past actions and performance on future career prospects.[2] Among the many stories in the financial press, one can often find anecdotal evidence indicating that herding is an economically interesting phenomenon in this profession (see, e.g., Nocera (1997)). Therefore, the labor market for security analysts appears to be an attractive setting in which to measure the effects of career concerns on herding.

Using a large dataset of 8,421 security analysts producing earnings forecasts between 1983 and 1996, we examine how their forecast behavior is influenced by career concerns. We begin by establishing the importance of perceived forecasting ability on analysts' career concerns by estimating the relationship between the likelihood that an analyst is terminated from her job and her past forecast accuracy. An important feature of several of the recent models of reputation-based herding is that future career outcomes are determined not only by past performances but also by past actions, which also serve as signals of the quality of the agent's private information (e.g., perhaps unconventional ones that differ markedly from other agents).[3] Therefore, we also estimate the relationship between the likelihood that an analyst is terminated from her job and her forecast boldness (i.e., forecasts that differ markedly from the consensus), controlling for forecast accuracy. We then discuss how this documented relationship

[2] While the question of why the ''buy side'' values analyst forecasts and recommendations is outside the scope of this article, there is some evidence indicating that analyst forecasts and recommendations may have value in predicting future returns (see, e.g., Stickel (1995) and Womack (1996)).

[3] For instance, in Scharfstein and Stein (1990), ''smart'' agents receive correlated information, while ''dumb'' agents receive uncorrelated noise. Therefore, agents who take unconventional actions are more likely to be inferred by the labor market to be dumb.

between career outcomes and forecast accuracy and boldness might cause forecast behavior to vary systematically across analysts by experience. Finally, we test this prediction by examining how earnings forecasts actually differ between inexperienced (younger) and experienced (older) security analysts.

To preview, we first find that the poorest-performing security analysts (as measured by past forecast accuracy) are the most likely to be terminated and the least likely to be promoted.[4] This relationship between past relative forecast performance and career outcomes is most pronounced for inexperienced analysts. Then, controlling for forecast accuracy, we find that inexperienced analysts are more likely to be terminated and less likely to be promoted when they make relatively bold forecasts than are their older counterparts. We also present some weaker evidence that being bold and bad leads to even worse future career outcomes; however, being bold and good does not significantly improve an analyst's future career prospects.

To the extent that analysts' decisions are influenced by these implicit incentives induced by career concerns, existing herding theories suggest that younger analysts face more career concerns and therefore should take fewer risks in their forecasts. We find that inexperienced analysts do tend to herd more than older analysts in that they forecast closer to the consensus than more experienced analysts do. This finding is robust to a variety of ways of measuring herding. We also find that inexperienced analysts are less likely to produce timely earnings forecasts of firms, and they tend to revise their forecasts more frequently than older analysts do.[5]

Our article is closely related to Lamont (1995). He considers the forecast performance of a subset of GNP (and other macroeconomic) forecasters who are featured in various issues of *Business Week* and similarly finds that older analysts herd less than younger analysts. However, Lamont does not try to distinguish between reputation-based herding theories and alternative nonagency-related stories such as learning by doing. One could, for instance, attribute boldness on the part of older analysts to experience. The more experienced analyst is more confident about her own information and therefore is bolder in her forecasts. She need not be taking into account her career concerns in formulating her forecasts.

Unlike Lamont (1995), we try to better distinguish between herding and alternative explanations by explicitly trying to measure an analyst's career concerns and to link them to their forecasts. This connection between implicit incentives and actions suggests that reputation-based herding theories provide a better account of what is going on in the labor market for analysts than learning-by-doing explanations because learning-by-doing stories have little to say about the relationship between job separation, job performance, and actions.

Our approach and findings are similar to those of Chevalier and Ellison (1999). They document that younger and older mutual fund managers face different implicit incentives. Younger managers are substantially more likely than their older counterparts to be fired for poor performance and for bold actions such as deviating from stated fund objectives. They also observe that younger mutual fund managers are less likely to take such bold actions.

While our results are broadly similar to Chevalier and Ellison (1999), our dataset allows us to estimate more robustly the relationship between career concerns and herding. First, unlike Chevalier and Ellison, we have long histories of the actions and

[4] This finding is similar to Stickel (1992), who documents that analysts at the top of the *Institutional Investor* poll (those at the top of the profession) are more accurate forecasters than other analysts.

[5] These two findings can be interpreted as being consistent with the forecast boldness result; to do so, however, one would have to stretch existing models a bit, since they do not generally allow for endogenous choice of timing of actions or revision of these actions.

performance of specific analysts. These long histories allow us to better estimate the various relationships involving experience. For instance, whereas Chevalier and Ellison just look at a cross section of managers without controlling for analyst-specific effects, we are able to look at not only the cross section of analysts but also the change in behavior over time for a specific analyst. Additionally, Chevalier and Ellison consider broad-based measures of herding (e.g., differences in the portfolios of different mutual fund managers), whereas we are able to use more precise herding measures such as how different analysts covering a specific stock differ in their forecasts and the timing of those forecasts. These tighter measures are closer in spirit to those discussed in the various herding theories.

The labor markets for professional forecasters and investors have attracted many other herding studies. Articles on herding among forecasters and institutional investors include Lakonishok, Shleifer, and Vishny (1992), Peles (1993), Grinblatt, Titman, and Wermers (1995), Wermers (1995), Falkenstein (1996), Nofsinger and Sias (1996), Wylie (1996), and Cooper, Day, and Lewis (1999). However, the vast majority of these articles do not attempt to relate herding to career concerns. There are some exceptions. Ehrbeck and Waldmann (1996) conclude that empirical patterns in three-month T-bill forecasts do not support simple reputation-driven herding models but are, instead, consistent with behavioral hypotheses. Welch (forthcoming) finds some evidence for herding consistent with informational cascades or career concerns in analyst recommendations on whether to buy or sell stocks. Graham (1999) also finds some evidence consistent with reputation-based models of herding. Kutsoati and Bernhardt (1999) find that relative performance incentives can bias the forecasts of analysts.

The remainder of our article proceeds as follows. Section 2 describes our data and analyzes our measures of job separation, forecast accuracy, and forecast boldness. In Section 3 we present our findings on job separation. We consider our results on herding in Section 4, and Section 5 concludes.

2. Measures of job separation, forecast accuracy, and forecast boldness

■ Our 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 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 1996.[6] During this period, the data consist of the estimates of 8,421 analysts covering 4,527 firms.

We can track the forecast and employment history of each analyst in the I/B/E/S sample. Generally, analysts tend to specialize and cover firms in the same industry. On average, an analyst in I/B/E/S follows 9.5 firms in a year, with a standard deviation of about 4.7 firms. An analyst remains in the sample for a little over four years on average, with a standard deviation of about 3.7 years. Some are in the database only one year (the 10th percentile of the distribution), because either they quickly left the profession, they switched to a firm not covered by I/B/E/S (though this is unlikely, since most brokerage houses submit the forecasts of their analysts to I/B/E/S), or they only started providing earnings forecasts in 1996. However, a number of analysts are in the sample for the entire fourteen-year period. The 90th percentile of the distribution is ten years.

[6] I/B/E/S also distributes the Historical Summary File, which contains more years of data (back to 1976) than the Detail Earnings Estimate History File. However, only the latter contains information on the forecast histories of individual analysts.

With these individual analyst forecast histories, we construct several key variables in this section to gauge how an analyst's forecast behavior affects her career prospects. First, we need a measure of how well an analyst's career is progressing. So we create below several indicators of job separation. According to various theories of career concerns (e.g., Holmström (1999)), job performance is an important variable in determining these job separations. Therefore, we next construct a measure of the past performance of all the analysts in our sample based on their forecast accuracy. Finally, the more recent theories of herding (e.g., Scharfstein and Stein (1990) and Prendergast and Stole (1996)) suggest that the actions of security analysts such as the boldness of their forecasts may also be important in determining job separations. So at the end of this section we discuss how we construct a past forecast boldness measure for each analyst.

□ **Job separation.** We do not actually observe in the I/B/E/S database whether a security analyst has been fired, demoted, or promoted. However, we can determine whether an analyst stops producing earnings forecasts after a certain year or whether the analyst moves to a different brokerage house. Using these two pieces of information, we can create proxies for the career outcomes of analysts.

Since an important component of being a security analyst is producing earnings forecasts and since virtually all analysts submit their forecasts to I/B/E/S, we infer that most analysts who stop producing forecasts have left the profession. The possibility that an analyst may have left for a better job such as mutual fund manager after leaving the I/B/E/S sample is remote, since our analysts are sell-side and not buy-side analysts. Sell-side analysts generally aspire to be *II All-Americans,* while buy-side analysts tend to be promoted to manage mutual funds (see, e.g., Stickel (1992) and Nocera (1997)). Hence, we will say that an analyst is terminated in year $t + 1$ if she made forecasts in year t but stopped producing forecasts sometime in $t + 1$.

We can broaden our definition of a bad career outcome for an analyst by considering the movements of analysts between brokerage houses. One way of determining whether an analyst's move is a promotion or a demotion is to look at the characteristics of the brokerage house to which the analyst moves. Large and prestigious ones will employ many analysts to cover firms in all sectors because they have a larger and more diverse client base than do other brokerage houses. On the other hand, smaller, regional firms that cater to specific clienteles will tend to focus on a particular industry or cross section of stocks and hence will employ fewer analysts.

Our dataset allows us to calculate the number of analysts submitting forecasts for a particular brokerage house in a given year, which is a good proxy for the number of analysts employed by the brokerage house. We create alternative job-separation measures based on the prestige of the brokerage house where an analyst is employed. An analyst has been promoted if she was working for a brokerage house in year t that employed fewer than 25 analysts in the year and moves in year $t + 1$ to a brokerage house that employs at least 25 analysts. Alternatively, an analyst has been demoted if she moved from a high-status brokerage house to a lower-status brokerage house.

In our sample, the mean brokerage house employs about 12 analysts. A brokerage house at the 90th percentile of the distribution employs about 24 analysts, while a brokerage house at the 10th percentile of the distribution employs about 4 analysts. Merrill Lynch employs the most analysts by far, with over 100. Hence the cutoff of 25 analysts seems like a reasonable measure as far as proxying for the more prestigious firms.

□ **Forecast accuracy.** With our measures of job separation, we now turn to constructing indicators of an analyst's past performance to see how it affects her probability of job separation. We first use the I/B/E/S data to construct a yearly performance measure based on an analyst's forecast accuracy. 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*.[7] 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, $A_{j,t}$:

$$forecast\ error_{i,j,t} = |F_{i,j,t} - A_{j,t}|. \tag{1}$$

Because an analyst generally covers more than one firm in a year, we need to aggregate her forecasting accuracy across all the firms she covers. One could compare the average forecast error of an individual analyst to the average forecast error of the other analysts who produce earnings estimates that year. But because analysts cover different firms, even analysts who cover the same industries, this performance measure is problematic because some firms are more difficult than others to predict accurately.

Instead, we first sort the analysts who cover a firm in a year based on their forecast errors given in (1). We then assign a ranking based on this sorting; the best analyst receives the first rank, 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.[8] Under this relative ranking system, the analyst who produces the most accurate estimate of firm A performs as well as the analyst who 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. This average rank measure might be problematic, however, because the maximum rank an analyst can receive for a firm depends on the number of analysts who cover the firm. Analysts who cover firms that are thinly followed are more likely to have lower average ranks than analysts who 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 who cover that firm. We develop a score measure that adjusts for these differences in coverage. The formula for this score is

$$score_{i,j,t} = 100 - \left[\frac{rank_{i,j,t} - 1}{number\ of\ analysts_{j,t} - 1}\right] \times 100, \tag{2}$$

where *number of analysts*$_{j,t}$ is the number of analysts who cover the firm in a year.[9] 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 illustrate using an example. Table 1 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

[7] Using the most recent forecasts to the cutoff date of July 1st to evaluate the analysts makes sense, since it creates a level playing field for evaluating everyone (see, e.g., Crichfield, Dyckman, and Lakonishok (1978)). Our results are robust to alternative cutoff dates.

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

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

TABLE 1 A Hypothetical Example of a Score Calculation for a Group of Analysts Following a Firm

Analyst	Forecast Error	Rank	Score
1	.12	1	100
2	.25	3	71.4
3	.25	3	71.4
4	.25	3	71.4
5	.38	5	42.9
6	.67	6.5	21.4
7	.67	6.5	21.4
8	.80	8	0

worst analysts receive scores of 100 and 0 respectively. The second through fourth analysts have the same forecast error (as do the sixth and seventh analysts). Therefore, they all receive the same rank of 3, the midpoint of the second through fourth slots (6.5 for the sixth and seventh analysts).

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, this measure would be very noisy for analysts who follow only a couple of firms in a year. Therefore, we create the measure *forecast performance*$_{i,t}$, which is the average of the analyst's forecast scores in year t and the two previous years.[10] Higher overall scores correspond to better analyst performance.

We use these three-year averages primarily because they are less noisy proxies of ability. One way to gauge the goodness of a measure is to simply consider the persistence of the performance scores. In other words, if a measure captures true ability and to the extent that ability is persistent, then the score measure ought to exhibit some persistence over longer periods. In this vein, the serial correlation of three-year averages (calculated using nonoverlapping observations) is about .26, whereas the serial correlation of one-year performance scores is about .1. So the three-year performance measures seem reasonable.

Although we believe this score is a good method of measuring analyst performance, we need to keep in mind some of its peculiarities. First, certain types of analysts are likely to have extreme average scores (both good and bad) regardless of their performance. For instance, analysts who 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, analysts who 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 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

[10] Hence, an analyst must be in at least her third year as an analyst to have a forecast performance measure.

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.

□ **Forecast boldness.** Finally, we construct a measure of an analyst's yearly forecast boldness, using a procedure very similar to our forecast accuracy measure. Let $\overline{F}_{-i,j,t} = 1/n \sum_{m\in -i} F_{m,j,t}$, where $-i$ is the set of all analysts other than analyst i who produce an earnings estimate for stock j in year t, and n is the number of analysts in $-i$. Hence, $\overline{F}_{-i,j,t}$ is a measure of the consensus forecast, the average of most recent forecasts (between January 1st and July 1st) made by all other analysts except analyst i following stock j in year t. Then an intuitive measure of an analyst's boldness is just the absolute value of the difference between $F_{i,j,t}$ and $\overline{F}_{-i,j,t}$:

$$\textit{deviation from consensus}_{i,j,t} = |F_{i,j,t} - \overline{F}_{-i,j,t}|. \tag{3}$$

At this point, we replicate our methodology for constructing the analyst accuracy measure in the previous subsection. That is, for each firm in each year, we rank all the analysts covering a firm by how much they deviate from the consensus forecast of that year (the boldest analyst receives the first rank, the second-boldest the second rank, etc.). From these rankings, we construct for each analyst a boldness score for each of the stocks in her coverage portfolio as in equation (2). Finally, we calculate for each analyst her overall score *forecast boldness*$_{i,t}$, which is the average of the analyst's boldness scores in year t and the previous two years. Higher values correspond to more bold forecasts by the analyst. The same caveats apply in using this measure as in using the forecast accuracy measure. Nonetheless, we think that this score is a reasonable way to capture boldness.

3. The relationship between career outcomes, forecast accuracy, and forecast boldness

■ With our measures of analyst career outcomes and forecast behavior, we first estimate how past forecast performance influences the probability of job separation. The specifications that we consider are motivated from an extensive literature on career concerns (see, e.g., Holmström (1982)) in which the labor market assesses an agent's ability by observing her past performances. Future career outcomes, such as job termination or promotion, depend on these assessments of ability and hence are related to these performance histories.

Some of the more recent models of career concerns and herding suggest that the future career outcomes of analysts depend not just on past forecast accuracy but to some extent on how bold or unconventional their forecasts (actions) were. The reason is that in these herding models, observable actions by agents serve as signals of the quality of an agent's private information. For instance, in Scharfstein and Stein (1990), "smart" agents receive correlated information, while "dumb" agents receive uncorrelated noise. Thus, all else equal, actions that differ markedly from what many other agents do lead the labor market to assess that the agent with the unconventional action is more likely to be "dumb." Alternatively, Prendergast and Stole (1996) model agents as having private information about the precision of the information they possess. A bolder action signals that a young agent knows her information is good and hence younger agents have an incentive to take bold actions. Older agents, however, have an incentive not to change their actions too much from period to period, because when optimal actions are correlated over time, this signals ability to the labor market. Hence, we also try to measure the relationship between job separation and forecast boldness, controlling for forecast accuracy.

□ **Job separation and forecast accuracy.** We begin our analysis of job separation and forecast accuracy by focusing on how analyst forecast accuracy affects the likelihood that an analyst stops producing forecasts and leaves our sample. In this analysis, at any year t, we only include analysts who have at least three years of forecast history, the number of years necessary to allow us to calculate our forecast performance scores defined in the previous section.[11]

In Table 2 we report summary statistics for the various job separation and forecast performance measures defined in Section 2. In this sample, the probability that an analyst leaves the profession in any given year is .158. This is the number of analysts who stopped making forecasts in year $t + 1$ divided by the number who made a forecast in year t (averaged across all the years in our sample). In any given year, the probability that an analyst is promoted is .033. This is calculated as the fraction of analysts who made forecasts in year t and $t + 1$ who began year t in a firm with fewer than 25 analysts and moved in year $t + 1$ to a firm with more than 25 analysts. Calculating as we did for the probability of promotion, the probability of being demoted is .013. Finally, notice that by construction, the average forecast accuracy and boldness measures have means close to 50.

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

$$\textit{job separation}_{i,t+1} = \alpha + \beta_1 \textit{forecast performance indicator}_{i,t} + \epsilon_{i,t+1}, \tag{4}$$

where $\textit{job separation}_{i,t+1}$ is an analyst's career outcome (strictly speaking, whether analyst i is terminated or promoted in year $t + 1$), $\textit{forecast performance indicator}_{i,t}$ is some function of the analyst's past forecast accuracy measured as of year t, and $\epsilon_{i,t+1}$ is an error term. β_1 measures how an analyst's past forecast accuracy affects the probability that she leaves the I/B/E/S sample the next year.

This simple regression 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 performance measure, we noted that analysts who cover firms with thin coverage and analysts who cover few firms are more likely to be in the extremes of forecast performance. If analysts who follow few or thinly covered firms during this window are more or less likely to leave the I/B/E/S sample for reasons other than performance, then we might find a spurious relationship between forecast performance and job separation.

Therefore, we need to control for the type and number of firms that analysts follow during the three-year window we use 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 ($\textit{average coverage dummies}_{i,t}$). 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 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 for this value and include them in the regression specification.

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

TABLE 2 Descriptive Statistics of Performance and Job Separation Dataset

	Mean (1)	5th Percentile (2)	Median (3)	95th Percentile (4)
Job separation measures				
Probability that analyst leaves profession	.1583			
Probability that analyst moves to higher-status bank	.0329			
Probability that analyst moves to lower-status bank	.0134			
Analyst performance measures				
Accuracy measure	51.32 (7.71)	38.47	51.64	63.20
Forecast boldness	49.15 (7.01)	38.37	48.86	61.18
Experience	.8269			

Notes: Data from I/B/E/S for the years 1986 to 1995 (8,892 observations). Analysts are included in the sample if they have at least three years of experience. Standard deviations are in parentheses.

We also add dummy variables for the number of firms the analyst follows during the three-year window (*number of firms covered dummies*$_{i,t}$). Additionally, we include dummies for the brokerage house that an analyst works for in year t (*brokerage house effects*$_{i,t}$), indicators for the industry that the analyst follows (*industry effects*$_{i,t}$), and a full set of year dummies (*year effects*$_t$).[12] Our final regression specification is then

$$\begin{aligned} \textit{job separation}_{i,t+1} = \alpha &+ \beta_1 \textit{ forecast performance indicator}_{i,t} \\ &+ \textit{average coverage dummies}_{i,t} \\ &+ \textit{number of firms covered dummies}_{i,t} + \textit{brokerage house effects}_{i,t} \\ &+ \textit{industry efffects}_{i,t} + \textit{year effects}_{t+1} + \epsilon_{i,t+1}. \end{aligned} \tag{5}$$

Table 3 presents the results of these regressions, in which the job separation measure is simply whether an analyst left the sample (i.e., terminated). In column (1), the forecast performance indicator is a dummy variable for an analyst having a forecast accuracy score in year t in the bottom 5% of the forecast performance score distribution. Therefore, β_1 measures the probability that very poor-performing analysts leave the I/B/E/S sample compared to other analysts. The coefficient in column (1) is positive and statistically different from zero, suggesting that analysts at the bottom of the score distribution are slightly less than 4 percentage points (3.8%) more likely to leave the profession than other analysts.[13]

This estimate of 3.8% is not only statistically significant but also economically interesting. To better place the magnitude of this estimate in some perspective, recall

[12] We include these additional effects just to be safe, since it is hard to gauge how forecast performance may be correlated with employment, the industry that an analyst follows, or general time trends (see, e.g., Michaely and Womack (1999)).

[13] For all of the job separation estimates, we present only results from linear probability models. Results from logit and probit models are qualitatively similar.

TABLE 3 The Effect of Past Performance on the Probability That an Analyst Experiences Bad Career Outcomes

	Probability That Analyst Leaves Profession		Probability That Analyst Moves to Higher-Status Bank	
Score Distribution	(1)	(2)	(3)	(4)
0 through 5th percentile	.0376 (.0187)	.0608 (.0200)	−.0208 (.0100)	−.0192 (.0104)
5th through 10th percentile		.0539 (.0198)		−.0171 (.0101)
10th through 25th percentile		.0350 (.0130)		−.0070 (.0070)
25th through 50th percentile		.0272 (.0111)		−.003 (.0054)
50th through 75th percentile		.0170 (.0110)		.0094 (.0058)
Observations	8,892	8,892	7,484	7,484

Notes: The dependent variable in columns (1) and (2) is an indicator that the analyst is not in the I/B/E/S sample the following year. The dependent variable in (3) and (4) is an indicator that the analyst moved to a higher-status investment bank the following year if the analyst did not leave the industry. The regression specification is equation (5). The standard errors are in parentheses and are adjusted to account for the within-analyst correlation of the observations.

that in any given year, an analyst has a 15.8% chance of leaving the sample. A past forecast performance in the bottom 5% increases the probability of termination by 3.8%, or about 24% of the mean.

In column (2), we include a finer set of indicators for an analyst's place in the forecast accuracy score distribution. The coefficients on these indicators measure how much more likely an analyst who scores in that region is to leave the sample than analysts who score above the 75th percentile. Again, analysts at the bottom of the distribution (bottom 5%) are much more likely to leave the profession. As an analyst's score improves, the probability of leaving declines.

We next consider the effect of forecast accuracy on our second job separation measure, job promotion, defined as an analyst moving from a low-status brokerage house (those employing fewer than 25 analysts) to a high-status brokerage house (those employing more than 25 analysts). Column (3) provides the estimate of the effect of being in the bottom 5% of the distribution of forecasting performances on job promotion. The coefficient on past forecast accuracy suggests that poor performance decreases the probability of being promoted by about 2%. In any given year, about 3.3% of analysts are promoted; hence, having a poor past forecasting performance decreases an analyst's chances of being promoted by about 60% of the mean. We re-estimate this relationship in column (4) using a finer set of past forecast accuracy indicators. Once again, we see that as an analyst's forecast performance improves, she increases her probability of being promoted.[14]

Regardless of how we measure career outcomes, our results are broadly consistent with career-concern models of the Holmström (1999) variety in which past poor performance leads to updated assessment of an agent's ability and possible job termination.

[14] We also estimated the relationship between past forecast accuracy and demotions. While the results are of the right sign, there is little statistical significance, since there are very few demotions in any given year (only 1%). We omit these results for brevity.

Such theories are generally ambiguous on how the sensitivity of the termination-performance relationship varies for agents of different experience (see, e.g., Holmström (1999) and Gibbons and Murphy (1992)). Nonetheless, it can be instructive to empirically document how this relationship varies for analysts of different experience to measure the implicit incentives at work in the labor market for security analysts. Therefore, we next consider how the effect of forecast accuracy on job separation varies with an analyst's experience.

Although the I/B/E/S dataset does not record how long an analyst has worked in the profession, we do know how many years an analyst is in the database. We create a dummy variable called $experience_{i,t}$, which takes on a value of one if an analyst at year t has more than three years of past experience in the database and zero if the analyst has only three years of past experience. Recall that analysts need at least three years of past experience to be included in our sample because we need those three past years to calculate our past forecast performance measure. There is a potential problem with this variable for analysts who are in the database in 1983 (the first year of the sample). 1983 could have been their first year as an analyst or their twentieth year. But because we only use observations from the I/B/E/S data from 1986 onward, all analysts who produced forecasts in 1983 will be experienced under our definition no matter how many years they were in the profession before 1983. (Summary statistics for this experience indicator are reported in Table 2.) For example, an analyst with three years of past forecasting performance (1984–1986) who is entering her fourth year in 1987 is considered to be inexperienced or young in 1987. Alternatively, another analyst with past forecasts in 1983 through 1986 is considered experienced in 1987 because she has more than three years of past performance in 1987.

By adding this experience variable and an interaction of it with the forecast performance measure in the regression specification of equation (5), we can estimate how poor performance affects the probability of job separation for experienced and inexperienced analysts separately. The estimates of this regression using the dependent variable of an analyst leaving the I/B/E/S sample are presented in column (1) of Table 4. The coefficient of interest is the interaction term; it is negative and statistically significantly different from zero, indicating that experienced analysts are less likely than their younger counterparts to leave the profession after a poor performance. In fact, the coefficient on the interaction term of poor performance and experience suggests that young analysts face a stiff penalty for poor performance, but poor performance has little effect on the future career outcome of an old analyst. In column (2), we present the estimates of the same regression specification, now using promotion as the job separation measure. Again, we find that the promotion probability of experienced analysts is less sensitive to poor performance than that of inexperienced analysts.

Our results on job separation strongly suggest that perceived forecasting ability is important for analysts' career concerns, since those with extremely poor performance face higher probabilities of job termination and lower probabilities of promotion. Our results on job separation complement those of Mikhail, Walther, and Willis (1999). Using an alternative analyst database called ZACKS, they conclude that, controlling for firm and time-period effects, forecast horizon, industry forecasting experience, and even the profitability of recommendations, an analyst is more likely to move from one brokerage house to another if her forecast accuracy declines relative to her peers. Establishing the importance of perceived forecasting ability in determining future career outcomes is a first step in measuring career concerns and herding in earnings forecasts.

□ **Job separation and forecast boldness.** In addition to past forecast accuracy, herding models suggest that we should also examine how the past forecast boldness of

TABLE 4 The Effect of Past Performance on the Probability That an Analyst Experiences Bad Career Outcomes: By Experience

	Analyst Leaves Profession (1)	Analyst Moves to Higher-Status Bank (2)
Poor performance indicator (bottom 5%)	.1101	−.0546
	(.0409)	(.0134)
Experience	−.0031	−.0086
	(.0111)	(.0072)
Poor performance × experience	−.0894	.0410
	(.0452)	(.0158)
Observations	8,892	7,484

Notes: The dependent variable in the first column is an indicator that the analyst is not in the I/B/E/S sample the following year. The dependent variable in the last column is an indicator that the analyst moved to a higher-status investment bank the following year. The regression specification is equation (5). The standard errors are in parentheses and are adjusted to account for the within-analyst correlation of the observations.

analysts affects the probability of job separation. We use the same regression specification as equation (5), with two modifications. First, we include a dummy variable for the analyst being in the top 5% of the boldness-measure distribution as an indication that the analyst was bold in year t (the measure is $\textit{forecast boldness}_{i,t}$). Then we condition on the analyst's forecast performance by including dummy variables for her position in the forecast performance distribution.[15] Therefore, the coefficient on the boldness indicator measures how, conditional on an analyst's forecast accuracy, past bold forecasts affect the probability that an analyst leaves the I/B/E/S sample.

The estimates of this relationship are given in Table 5. Column (1) presents the estimates of the effect of boldness on job termination for all analysts, and column (2) considers the effect of boldness separately on experienced and inexperienced analysts. We find that boldness does not affect the probability that an analyst leaves the sample; however, in column (2), we observe that although younger analysts with more bold forecasts are more likely to leave the profession, the effect disappears as the analysts age. Being in the top 5% of boldness and being young increases the probability of leaving the sample by 7%.

To get a sense of economic significance, the right benchmark with which to evaluate this estimate is to consider the fraction of young analysts who leave the sample on average. Recall from Table 1 that about 15.8% of analysts leave the sample in any given year. This number can be broken down for young and old, with about 22% of young analysts and 14% of old analysts leaving the sample each year. Hence, being bold and young increases the probability of leaving the sample by about ⅓ of the mean (22% for young analysts). This magnitude is indeed economically interesting.

In column (3) and (4), we replicate the regressions of the first two columns using as a dependent variable whether an analyst is promoted. The coefficients suggest that past forecast boldness has little effect on the probability that analysts are promoted. So, most of the effect of boldness occurs for young and tends to be associated with terminations.

[15] We use the same categories for these dummies as those used in the regressions in columns (2) and (4) of Table 3.

TABLE 5 **The Effect of Past Boldness on the Probability That an Analyst Has a Bad Career Outcome: By Experience**

	Analyst Leaves Sample		Analyst Moves to Higher-Status Bank	
	(1)	(2)	(3)	(4)
Boldness indicator (top 5%)	−.0024 (.0176)	.0729 (.0389)	−.0070 (.0087)	−.0001 (.0267)
Experience		−.0024 (.0111)		.0062 (.0070)
Bold performance × experience		−.0949 (.0436)		.0089 (.0284)
Performance score effects	Yes	Yes	Yes	Yes
Observations	8,892	8,892	7,484	7,484

Notes: The dependent variable in columns (1) and (2) is an indicator that the analyst is not in the I/B/E/S sample the following year. The dependent variable in columns (3) and (4) is an indicator that the analyst moves to a higher-status bank. The regression specification is equation (5). The standard errors are in parentheses and are adjusted to account for the within-analyst correlation of the observations.

These findings indicate that inexperienced and experienced analysts face different implicit incentives. Inexperienced analysts are punished more harshly through termination for relatively poor forecast performance and for relatively bolder forecasts than their more experienced counterparts. These findings provide some support for herding models of the Scharfstein and Stein (1990) variety, in which unconventional actions are punished since they signal that the agents have received uncorrelated noisy signals, indicative of low-ability or dumb agents. Taking the implications of these models more seriously, they suggest the possibility that there can be asymmetries in the effect of boldness on career outcomes depending on whether that boldness corresponds to an accurate forecast performance or an inaccurate forecast performance.

In Table 6, we consider the interaction effect of forecast accuracy and boldness on career outcomes. In column (1), we find that being bold and bad increases the probability of being terminated. While the effect may be economically interesting, there is little statistical power. In column (2), we find that being bold and good, however, has little effect on being terminated. In column (3), we find that most of the effect of being bad and bold is for young analysts. In column (4), we find little difference in being bold and good for young or old analysts. Because the statistical power of these regressions is quite limited, we hesitate to put too much of an interpretation on these findings.

4. Herding results

■ Given the findings of Section 3, reputation-based herding models suggest that analysts may have an incentive to herd with the consensus. Continuing with the Scharfstein and Stein (1990) example, if an agent learns that her private information about an investment opportunity is very different from the information that other agents receive, she learns that it is more likely that she is ‘‘dumb.’’ Because taking the action that her information suggests is optimal would signal to the labor market that her ability is low, the agent ignores her information and herds. Zwiebel (1995) focuses on an alternative motivation for herding, using a model in which taking unconventional action increases the variance of the market's *ex post* assessment of an agent's ability. In his

TABLE 6 The Effect of Past Boldness and Performance on the Probability That an Analyst Has a Bad Career Outcome: By Experience

	(1)	(2)	(3)	(4)
Boldness indicator (top 10%)	.0033 (.0135)	.0047 (.0143)	−.0395 (.0313)	−.0113 (.0289)
Poor performance indicator (bottom 10%)	.0431 (.0166)		.0595 (.0403)	
Good performance indicator (top 10%)		−.0321 (.0125)		−.0180 (.0271)
Experience			−.0212 (.0134)	−.0147 (.0130)
Bold performance × poor performance	.0997 (.1174)		.1854 (.1952)	
Bold performance × good performance		.0131 (.0329)		−.0193 (.0624)
Bold performance × experience			.0538 (.0344)	−.0214 (.0328)
Poor performance × experience			−.0196 (.0434)	
Good performance × experience				−.0198 (.0302)
Bold × poor × experience			−.1363 (.2462)	
Bold × good × experience				.0495 (.0724)
Observations	8,892	8,892	8,892	8,892

Notes: The dependent variable is an indicator that the analyst is not in the I/B/E/S sample the following year. All specifications also include dummies for the average coverage of the firms the analyst follows her previous three years in the sample, a set of dummies for the number of firms the analyst covers those three years, investment bank effects, industry effects, and year effects. The standard errors are in parentheses and are adjusted to account for the within-analyst correlation of the observations.

model, average ability agents prefer the conventional action because it reduces the risk of being fired, while very high and very low ability agents prefer unconventional actions. To the extent that the implicit incentives for bold actions differ for inexperienced and experienced analysts, both these models would predict very different herding outcomes for these two groups.

In Section 3 we documented that young and old analysts face very different career concerns (or implicit incentives), with the young having lots of career concerns and the old having few. One of the central messages of career-concern-motivated herding models is that agents facing more career concerns herd more than those facing no career concerns, all else equal. We want to examine whether inexperienced analysts produce different forecasts than their experienced counterparts do, in order to identify the effect of career concerns. Of course, young and old will differ in dimensions other than their career concerns; therefore, we will have to control for as many of these other differences as possible.[16]

[16] See Avery and Chevalier (1999) for an analysis of how herding patterns can differ for young and old as the old accumulate private information about their ability. Also see Prendergast and Stole (1996) for a model in which old agents are more conservative than young agents.

In this section we test the prediction that young analysts will herd more than old analysts by examining how experience affects various measures of analyst forecast herding. First, we present our findings on how experience is related to an analyst's propensity to deviate from consensus earnings estimates. Then we examine the sensitivity of our results to changes in our empirical specification. Next, we measure the effect of experience on alternative measures of herding, such as timeliness of earnings forecasts and the frequency of revisions. Finally, we implement an alternative estimation strategy to measure the effect of experience on herding.

□ **Deviation from consensus and experience.** Our empirical strategy is to relate the measure of how far from the consensus forecast an analyst's forecast is to whether she is inexperienced or experienced, controlling for a number of other factors. In this analysis, we want to control in particular for differences in ability between young and old analysts, using past forecast performance as a proxy. In Section 3 we used a three-year average to measure performance in determining job separation. We found that a young analyst whose forecast performance in her first three years was in the bottom 5% of the forecast performance distribution faced a significantly higher chance of suffering an adverse career outcome in her fourth year. It makes sense, then, that her career concern is greatest during her first three years, and we would expect any effect of career concern on herding to occur sometime during these first three years. Therefore, we want to examine how these young analysts behave during their first three years compared to older analysts. However, we also want to control for differences in the ability of old and young using past forecast performance scores. Therefore, in this section we use only a two-year window to calculate an analyst's past forecast performance score, so we can compare the forecast behavior of young analysts in their third year to the forecast performance of other (older) analysts.[17]

We adopt the following basic specification to measure the relationship between experience and herding:

$$\begin{aligned} &\textit{deviation from consensus}_{i,j,t} \\ &\quad = \alpha + \beta_1 \textit{experience}_{i,t} + \textit{firm}_j \times \textit{year}_t \textit{ effects} + \textit{forecast performance}_{i,t-1} \\ &\qquad + \textit{brokerage house effects}_{i,t} + \epsilon_{i,j,t}, \end{aligned} \tag{6}$$

where the familiar variables are defined as before (i.e., $\textit{deviation from consensus}_{i,j,t}$, $\textit{experience}_{i,t}$, $\textit{forecast performance}_{i,t-1}$, and $\textit{brokerage house effects}_{i,t}$), and $\textit{firm}_j \times \textit{year}_t$ *effects* is a full set of dummies for all firms each year.[18] Row 1 of Table 7 presents the descriptive statistics for the dependent variable in the I/B/E/S sample. The average deviation from consensus over all forecasts is about 18 cents. Row 2 of Table 7 gives the summary statistics for the experience indicator.

The coefficient of interest is β_1, which measures whether experienced analysts deviate more or less from the consensus than other analysts do. Because we include $\textit{firm} \times \textit{year}$ dummies in the regression specification, we are identifying the effect of experience on forecast behavior by comparing the forecasts of all the analysts who follow a particular firm in a year and examining whether inexperienced and experienced

[17] In controlling for forecast performance, we are leaving out of our analysis many young analysts in their first or second years. It is important to note that all our effects only get stronger if we get to sample from these young analysts' forecast behavior.

[18] Note that the unit of observation in this regression is an analyst's forecast of an individual firm in the year.

TABLE 7 Descriptive Statistics of Measures of Herding

	Mean (1)	10th Percentile (2)	50th Percentile (3)	90th Percentile (4)
Deviation from consensus: $\lvert F_{i,j,t} - \overline{F}_{-i,j,t}\rvert$	.17 (.59)	.01	.06	.39
Experience indicator	.85			
Additional performance measures				
Probability that an analyst produces the initial forecast of a firm's earnings	.036			
Revision performance: $R_{i,j,t} - \overline{R}_{-i,j,t}$	.08 (1.01)	−1.00	−.13	1.41

Notes: Data from I/B/E/S for the years 1986 through 1995. 81,127 observations. Standard deviations are in parentheses. See the text for a description of the sample used to calculate the probability that an analyst produces the leading forecast.

analysts deviate from the consensus forecast of that firm differently. For firms that have very low analyst coverage in a year, there might not be much of a consensus for an analyst to possibly deviate from; therefore, at least ten analysts must be following a firm in a year for it to be included in this analysis.[19]

Column (1) of Table 8 presents the results from this regression specification. The coefficient on experience is positive and statistically different from zero, indicating that analysts with tenure are more likely than other analysts to deviate from the herd. The magnitude of the coefficient suggests that experience increases an analyst's deviation from the consensus by over 1.1 cents on average, or about 7% of the mean deviation from consensus (about 18 cents) or 17% of the median deviation from consensus (about 6 cents). This magnitude is certainly economically interesting.

One potential concern with this empirical strategy is that analyst attrition might bias our regression results. For our strategy to produce unbiased estimates, we need to assume that the experienced and inexperienced analysts following the same firm in a year are, conditional on observable variables, on average similar to one another except for their tenure. Then we can attribute any differences we observe in their forecasts to experience. However, we found in the previous section that inexperienced analysts who are bold are more likely than other analysts to leave the profession. This suggests that the average experienced analyst might tend to be less bold than the average inexperienced analyst because the experienced analyst survived this selection process.

Although we cannot easily assess the magnitude of this potential attrition problem, we can estimate the direction of the bias. If older analysts are less bold than their younger counterparts because of this selection issue, then our regression estimates will understate the degree that more experienced analysts than inexperienced analysts deviate from the consensus because of career concerns. Without this attrition, younger bold analysts would have become experienced bold analysts, increasing the average boldness of older analysts and increasing the difference between young and old analysts. Therefore, attrition can be a problem with our empirical strategy, but it probably causes us to underestimate the effect of experience on herding.

At the end of this section we shall consider an alternative estimation strategy in which we measure the relationship between herding and experience by controlling for

[19] We check the sensitivity of our results to this requirement below.

TABLE 8 The Effect of Experience on the Deviation of Analysts' Forecasts from the Consensus

	(1)	(2)	(3)
Experience	.0109 (.0045)	.0110 (.0046)	.0109 (.0046)
Firm × year effects	Yes	Yes	Yes
Brokerage house effects	Yes	Yes	Yes
Month of forecast effects	No	Yes	No
Order of forecast effects	No	No	Yes
Observations	81,127	81,127	81,127

Notes: Standard errors are in parentheses. They are calculated allowing for intra-analyst correlation. The dependent variable is a measure of the deviation of an analyst's forecast from the consensus forecast as described in the text. The regression also includes controls for the analyst's past performance.

analyst fixed effects. Using this estimation strategy, we are identifying the effect of experience on herding by following the behavior of a specific analyst over time. Our results are robust to this alternative estimation strategy.

Another potential concern with our current regression strategy is that we are not controlling for some aspect of how analysts produce forecasts that creates a spurious correlation between experience and deviations from the consensus. One possibility is the timing of when analysts produce forecasts. We only examine the earnings forecasts of a firm that an analyst makes from January through June of the year for which the analyst is producing the earnings estimate. If older analysts tend to make their final forecasts at different times during this six-month window than other analysts do, then our regression results might be finding a timing effect instead of a career-concern effect. For example, experienced analysts might produce earlier final forecasts during this window than inexperienced analysts do. Since more information is available to those who submit forecasts later rather than earlier, the dispersion in forecasts may simply reflect the fact that older analysts had less information when they submitted their forecasts.[20]

We can explore this potential problem by controlling for the timing of an analyst's earnings forecast in our regression specification. First, we can add month effects to our regression equation. That is, we add to the right-hand side a set of dummies for the month in which a forecast is submitted in a given year.[21] Column (2) of Table 8 displays the results of this regression estimation. The effect of experience on deviating from the consensus is almost completely unchanged when this extra set of controls is added.

Also, we can control for the order in which analysts produce their final forecasts for a firm. We create a set of dummy variables that correspond to the order of the final forecast of all the analysts who follow a given firm in a year. We then include these variables as extra controls in our regression specification of equation (6). The results of this estimation are in column (3) of Table 8. Again, the effect of experience on the propensity to herd is not quantitatively changed by the inclusion of these extra controls. Therefore, issues of the timing of forecasts do not appear to be driving our results.

[20] We discuss issues involved in the timing of an analyst's forecasts in much greater depth below.

[21] Since forecasts are included in the sample only if they were made between January 1st and July 1st of the year studied, the month dummies represent the months January through June.

TABLE 9 The Effect of Experience on the Deviation of Analysts' Forecasts from the Consensus: Sensitivity Tests

	Alternative Forecast Date Cutoffs		Alternative Minimum Number of Analysts Needed to Follow Firm	
	October (1)	April (2)	5 Analysts (3)	15 Analysts (4)
Experience	.0103 (.0049)	.0057 (.0033)	.0097 (.0042)	.0076 (.0050)
Firm × year effects	Yes	Yes	Yes	Yes
Brokerage house effects	Yes	Yes	Yes	Yes
Observations	101,098	55,860	98,970	59,958

See notes to Table 8.

Our results strongly suggest that experienced analysts are in fact bolder than their inexperienced counterparts. This finding provides additional support for reputation-based herding models. The linking of implicit incentives and actions also suggests that learning by doing or other nonreputation-based stories might provide a less parsimonious account of the data than the herding models do.

□ **Sensitivity analysis.** Next, we want to make sure that our regression results measuring the effect of experience on deviating from the consensus are robust to different ways of creating the sample of analysts' forecasts. First, we examine our results when we alter the time window used to decide which forecast of a firm an analyst used. In the work above, we used the latest forecast between January 1st and July 1st. There is no particular reason for this July 1st deadline; therefore, we examine whether our results change if we use a deadline of October 1st or April 1st. Column (1) of Table 9 displays the estimates using the later deadline. The effect of experience on deviating from the consensus is smaller but still positive and statistically different from zero. Using the earlier cutoff date (column (2)) produces a slightly smaller effect of tenure on deviations from the consensus, but the qualitative finding that older analysts are less likely to herd does not appear sensitive to the choice of forecast window.

Our final specification test involves examining the sensitivity of our results to our rule on how many analysts need to be following a firm for it to be included in the analysis. In our original specification, at least ten analysts had to cover a firm in a year. In column (3) of Table 9 we include all firms that have at least five analysts producing forecasts in the regression analysis, and column (4) includes only firms with at least fifteen analysts. In both cases, the effect of experience on deviating from the consensus is slightly smaller than our benchmark results, but in general, our results do not appear to be driven by sample-selection restrictions.

Note that our main finding of more herding by young analysts is broadly supportive of Scharfstein and Stein (1990) and Zwiebel (1995) to the extent that we interpret their models as suggesting that agents with more career concerns herd more. Our finding of less herding among old analysts is less favorable for Prendergast and Stole (1996), since they predict jaded and conservative old-timers.[22]

[22] This is not surprising, however, since their model assumes that there is private information about ability for young and old. If we think that the young learn about their abilities along with the labor market, our findings then suggest that there is little private information early in an analyst's career.

Incidentally, we obtain similar results if we measure boldness of forecasts not at the level of a stock but averaged over all the stocks that an analyst covered. In other words, we could give each analyst a boldness measure based on an average of how boldly she forecasts in each of the stocks she covered (i.e., using the forecast boldness scores of Section 2). Such a broad-based herding measure is analogous to that used by Chevalier and Ellison (1999). It is comforting that our results also stand up in this setting.

However, we chose the more refined herding measure of calculating herding by stock not only because it is closer in spirit to existing herding models but also because it affords us alternative measures of herding related to timing and revisions that are not easily addressed using the broad-based herding measure. It is to these alternative measures that we now turn.

□ **Alternative herding measures and experience.** In this section we consider two additional, complementary measures of herding. Certainly, analysts who generate forecasts before other analysts are less likely to be herding than those who issue forecasts after other analysts. In this spirit, our first alternative measure of herding is whether an analyst was the first person to issue an earnings estimate for a stock for a given fiscal year. We assume that an analyst who issues the first forecast of the fiscal year is less likely to be herding than those who do not. For each firm that an analyst follows in a fiscal year, we identify the date of the first forecast for that analyst. We compare that date to the dates of the first forecasts of the other analysts who also follow that stock to determine the analyst who produced the first forecast for the firm's earnings that year.[23] Occasionally, more than one analyst reports the first earnings forecast on the same date. In such circumstances, all analysts who report a forecast that day are considered to be the first mover. Then we can determine the relationship between an analyst's tenure and the probability that she moves first.

We estimate the regression model of equation (6) using as a dependent variable an indicator that the analyst produced the first forecast for the firm instead of our deviation from consensus measure. The coefficient on experience tells us whether experienced security analysts are more or less likely to lead than other analysts. Column (1) of Table 10 displays the results of this regression. The coefficient on experience indicates that older security analysts are more likely than younger analysts to produce the first earnings forecast of a stock. The coefficient on age is statistically significantly different from zero and implies that experience raises the probability of leading by about .6 percentage points. Since the probability that analysts in this I/B/E/S sample produced the first forecast for a firm was 3.6%, this suggests that experience increases the propensity to lead by about 17%.[24]

For our second alternative herding measure, we consider the relationship between an analyst's frequency of revisions of earnings forecasts and experience. Unlike timing of forecasts, frequency of forecast revisions is a more problematic measure of herding. On the one hand, frequently revising a forecast may reflect an analyst changing her mind multiple times to accommodate the opinions of others. Thus we say that such an analyst is herding. Alternatively, if there is sufficient information arrival to warrant

[23] I/B/E/S does not report the date that an analyst produces an earnings forecast; rather, it provides the date that the analyst contacted I/B/E/S about the earnings estimate. We assume that analysts report their forecasts quickly to I/B/E/S as part of their effort to advertise their work.

[24] We worry that this regression might simply be capturing that the young have not followed that firm very long. Similar results hold if we estimate the regression including only observations in which the analyst has followed the particular firm at least two years.

TABLE 10 The Effect of Experience on Other Aspects of Analyst Forecast Behavior

	Probability Analyst Produces First Forecast (1)	Number of Revisions of Forecast (2)
Experience	.0063	−.0210
	(.0025)	(.0109)
Firm × year effects	Yes	Yes
Brokerage house effects	Yes	Yes
Observations	81,127	56,753

Notes: The dependent variable in column (1) is an indicator that the analyst was the first to produce a forecast of the earnings for the firm. The dependent variable in column (2) is the number of revisions an analyst makes of a forecast. See the text for a description of the samples used to estimate both regressions. Robust standard errors are in parentheses. They are calculated allowing for intra-analyst correlation.

changing of opinions, then frequently revising one's forecasts need not be herding at all. So, interpreting this second measure depends on the model of herding that one has in mind. With these caveats, it is nonetheless interesting to consider how this alternative herding measure varies with experience.

We can construct a revision measure by considering the number of times in a year an analyst revises her earnings forecast of a company and comparing that to how often other analysts covering the same stock in the year revise their estimates. Specifically, we count the number of times that an analyst revises her earnings estimate between January 1st and July 1st and compare that to the average number of revisions of other analysts covering that stock over the same period:

$$\textit{revision performance}_{i,j,t} = R_{i,j,t} - \frac{1}{n}\sum_{m\in -i} R_{-i,j,t}, \tag{7}$$

where $R_{i,j,t}$ is the number of times analyst i revises her earnings estimate of stock j in year t during the fiscal year before July 1st. We subtract by the average number of times that other analysts revise their forecasts to control for heterogeneity across stocks in the propensity of analysts to revise earnings estimates. The last row of Table 7 displays the descriptive statistics of this measure for analysts in the I/B/E/S sample.

Now we estimate the regression model of equation (6) using as a dependent variable the number of revisions that an analyst made less the average number of revisions made by others following the stock in the same fiscal year.[25] The coefficient on experience measures whether older analysts revise their estimates more often or less often than younger analysts do.

Column (2) of Table 10 presents the estimates for the regression model with the revision performance dependent variable. The coefficient on tenure is negative and statistically significantly different from zero, suggesting that more experienced analysts are less likely to update their forecasts. The magnitude of the coefficient on experience

[25] We only include an observation if the analyst was following the firm during the entire time between January and July of the year being examined. Therefore, the sample size of the regression is smaller than for the regressions using the other herding measures.

in column (2) implies that becoming experienced lowers the revision performance measure by .021.

We can interpret these latter two findings as being consistent with our forecast boldness results. But we do not want to push too hard on this interpretation, since few of the existing models allow for endogenous timing of actions (such as who leads with the first forecast). Most of the models assume a specific sequence of timing of actions. An exception in this regard is Grenadier (1999), who considers an informational-cascades model in which agents can endogenously choose the timing of their actions.

□ **Alternative estimation strategy.** As we mentioned above, we worry about various types of biases to our estimation of the relationship between experience and herding. While we believe that our current method for measuring this relationship is probably a conservative one, it would also be interesting to consider an alternative estimation strategy by including analyst fixed effects in the regression specification. That is, instead of including $firm_j \times year_t$ *effects* in the regression specification in equation (6), we include $analyst_i \times firm_j$ *effects*. Therefore, we are identifying the effect of experience on herding by following an analyst covering a specific firm over time and examining whether her forecast behavior is different when she is young and when she is older.

It is important to note the potential attrition bias of this identification strategy. For instance, imagine that an analyst can exhibit one of two forecasting/herding patterns with tenure: (1) timid when young and bold when old or (2) bold when young and timid when old. To the extent that young, bold analysts are fired, then we would tend to observe only analysts who were timid when young and bold when old, since the young bold analysts would drop out of the sample. So even if there is not a correlation between experience and herding, we might be biased to finding it when following the behavior of an analyst over time.

We pursue this strategy in Table 11. Column (1) reports the effect of tenure on the first herding measure, deviation from the consensus. In this regression, we control for analyst-by-stock effects as well as brokerage-house effects. The coefficient of tenure on deviation from consensus is positive, indicating that analysts herd less with job tenure or experience. This finding is consistent with our earlier findings. Column (2) reports the effect of tenure on the second herding measure, whether an analyst is the first to issue an earnings forecast. Using the same set of controls as in column (1), we

TABLE 11 The Effect of Tenure on Various Measures of Herding Using an Alternative Estimation Strategy

	Deviation from Consensus (1)	Probability Analyst Produces First Forecast (2)	Number of Revisions of Forecast (3)
Experience	.0142	.0102	−.0225
	(.0071)	(.0031)	(.0139)
Analyst × firm effects	Yes	Yes	Yes
Brokerage house effects	Yes	Yes	Yes
Observations	81,127	81,127	56,753

Notes: Robust standard errors are in parentheses. They are calculated allowing for intra-analyst correlation. The regression also includes controls for the analyst's past performance.

find that experience tends to lead to more timely forecasts. In column (3), we report the coefficient of tenure on the number of revisions that an analyst makes. Here, we find that tenure leads to fewer revisions. Both of these findings are also consistent with those documented using the first estimation strategy.

An important finding of this article is not just the linking of career concerns to herding by identifying the differential career concerns and subsequent herding behavior of young and old analysts, but also how we estimated this relationship. The fact that our dataset allows us to track an analyst over long periods is important insofar as it gives added comfort to the results in Section 4.

5. Conclusion

■ In this article we examine the link between career concerns and herding in the labor market for security analysts. We first document the implicit incentives faced by security analysts by analyzing the determinants of job separation. We find that younger and older analysts in fact face different incentives, with younger analysts punished more harshly for poor forecasting performance and forecast boldness. The fact that boldness of forecasts affects future career outcomes, controlling for forecast accuracy, is consistent with several theories of career concerns and herding. Also consistent with theories of career concerns and herding, we find that younger analysts in fact herd more than their more experienced counterparts; that is, younger analysts forecast closer to the consensus forecast. Additionally, we find that older analysts are also more likely to issue timely forecasts and to revise their earnings forecasts less than their younger counterparts do.

Importantly, these results are robust to a variety of measures of job separation, forecast performance, and herding, as well as to a number of ways of identifying the effect of experience on herding. These results jointly suggest that herding theories offer a richer and more compelling account of the herding and experience patterns in this labor market than alternative stories, such as learning by doing, that do not consider the shape of the implicit incentives (career concerns) faced by security analysts.

Now that we have produced evidence for a link between career concerns and herding, a natural next step is to distinguish between the various existing models of career concerns and herding. We have taken a small step in this direction by pointing out the asymmetries in the termination-performance relationship for analysts that may be able to distinguish between the different models. However, much more needs to be done to flesh out the various theories. We leave these issues for future research.

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